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# Democracy, Justice, and Their Institutions

Utilizing thin criteria, the *World Bank Development Report* for 1997 reported that “[i]n 1974 only thirty-nine countries—one in every four worldwide—were independent democracies. Today, 117 countries—nearly two of every three—use open elections to choose their leaders.” Concern about the scope and nature of democracy, especially about its relationship with liberalism, and about its stability have been central to political science, especially American political science, since its inception. The essays in this part that consider these issues confront enduring problems of democracy, justice, and heterogeneity and delve into the institutional components of democracy, ranging from legislatures to the organization of civil society.

Ian Shapiro discusses “The State of Democratic Theory.” He begins with an examination of the main contending views of democratic purposes, those which involve the search for a common good and those which attend to the legitimate management of power relations. He argues that the latter understanding of the enterprise makes better sense but that it is compatible with an appropriately stripped down view of the former. His attention then shifts to mechanisms for advancing democracy. Starting with the debates among Schumpeter and his critics, he argues that wholesale rejections of Schumpeterian democracy are unpersuasive, concluding that the more fruitful path is to explore ways to make competitive democracy work better, expand its reach beyond governmental institutions, and supplement it with other institutional devices. This leads him to a consideration of the literature on the extent to which electoral systems can operate as engineering devices to promote competitive democracy in societies usually thought antithetical to it. He concludes that there is no good reason to suppose any society inherently incapable of democracy but argues for an incremental approach given the dearth of reliable knowledge about the adaptability of politicized identities to the requirements of competitive democracy. Existing scholarship reveals that we do know something about the economic preconditions for viable democracy. But we are mainly in the dark, he in-

sists, about the cultural and institutional factors that influence democracy's viability, and too little is known about which democratic institutional arrangements are best. If Schumpeterian democracy needs supplementing, then questions arise: Who should do the supplementing and what should it be? Because there are no perfect decision rules, a purely procedural scheme like competitive majority rule can produce self-defeating results. Partly for this reason, some democratic theorists argue for theories of substantive democracy against which the results of procedures can be evaluated. Shapiro notes, however, that we confront a proliferation of such theories, yet lack a compelling way to choose among them. To deal with this conundrum, he argues that the best course is for courts or other second-guessing institutions to play a reactive, escape-valve role to limit the perverse consequences of democratic procedures.

"Justice" by Jeremy Waldron inquires about the present state of discussion in legal and political theory. By "justice," he means social or distributive justice; that is, the evaluation of social policy and institutional arrangements on the basis of the way they affect the distribution of scarce resources and opportunities among individuals in society. He claims that the term "justice" does not take in the whole of liberal political philosophy, and argues that recent theoretical discussions ostensibly about justice have tended to drift away from that subject and become meditations on political morality in general or public reason in a multicultural society. This has left a significant gap in our philosophical discussion of issues about wealth, poverty, and distribution, a gap that economic theorists who are relatively uninterested in distributive issues have not been slow to fill. If political philosophy in the 1970s and early 1980s was dominated by a fierce and focused debate about social justice occasioned by the publication of John Rawls's first book in 1971, the past ten years or so have seen a blurring of that focus under the influence of Rawls's later work, and a diversion of the fierce energies that *A Theory of Justice* aroused into a safer and less consequential discussion of the nature of political argument in a multicultural society. He argues for a return to concerns with social justice, especially given the growing global concerns with poverty and inequality.

Romand Coles's "Pluralization and Radical Democracy: Recent Developments in Critical Theory and Postmodernism" illuminates some of the most important debates and developments in recent critical theory and postmodernism. These types of theories are diverse in their philosophical perspectives, subject matters, styles of theorizing, political implications, and sometimes even the conversations of which they understand themselves to be a part. Coles briefly orients the discussion around some of the critiques of political liberalism that have emerged from theorists associated with these positions. He addresses several of the philosophical differences and political stakes between Habermasian critical theory and various postmodernisms, showing that a wide range of positions exists within each theoretical school and noting that some of the most promising work emerges

by drawing on insights from both critical theory and postmodernism. Central to this essay is a cluster of questions concerning the nature and scope of democratic dialogue and power; the way theorists interpret the role of bodily affect, disposition, and perception in democratic engagement; and the way responses to the former are entwined with theorists' approaches to political questions concerning practices of rights, coalition politics, order, responsibility, and our relation to political institutions. The essay's discussion moves from political liberalism to critical theory to postmodernism. Coles maintains that this order should not be taken to suggest the endorsement of an uncomplicated linear progression of value from one to the next.

From these more abstract, theoretical debates, we turn to an examination of the literature on a variety of political institutions in democracies. Gerald Gamm and John Huber examine research on one of the fundamental institutions of democracy, "Legislatures as Political Institutions: Beyond the Contemporary Congress." They argue that for the bulk of political scientists today, the study of legislatures is the study of the U.S. Congress. Although this generalization fairly approximates the contemporary field, this was not the state of the discipline at the turn of the last century. Their paper's main objective is to understand the implications for legislative studies of the dominance of work on the contemporary Congress. The most obvious ramification is the extent to which congressional scholarship has served as a model of positivist, rigorous, scientific research for the more general study of legislatures, and the extent to which congressional scholarship has sustained the discipline's interest in institutions during an era otherwise dominated by behavioral work.

They also contend that the breadth of research on Congress—drawing on participant observation, roll-call analyses, interviews, rational-choice theory, and even history—has demonstrated the importance of embracing multiple approaches to analysis. But the primacy of congressional research has also exacted costs, primarily in biases that are created in the types of questions scholars pose and in the types of answers they provide to these questions. Important avenues for legislative research, for example, include efforts to explain the choice of institutional arrangements or to analyze the impact of institutions on the behavior of individuals, other political institutions, or policy. Gamm and Huber conclude by arguing that recent research happily has begun to move beyond some of the biases inherent in a Congress-centered approach.

Turning to the developing world, "The Great Transformation in the Study of Politics in Developing Countries" by Barbara Geddes looks at the impact of democratization. She argues that students of developing countries in the year 2000 were like geographers in 1520: the known world began changing about 25 years before, and we are still trying to figure it out. Just as most observers were explaining the fragility of democracy in developing countries, democratization began its recent global sweep. In a second equally unexpected development, many governments began to

abandon state interventionist economic policies in favor of greater market orientation. These events have led to a reevaluation of the theoretical underpinnings of the study of comparative development, affecting what we consider worthy of study, the set of stylized facts we accept as more-or-less true, our basic understanding of the economics of development, the theoretical approaches we use to explain politics, and the research methods we favor. She claims that these events may interact with changes in the methods of studying these developing countries to produce mutually reinforcing progress in our understanding of political and economic development.

Kathleen Thelen examines a key relationship for democracy in "The Political Economy of Business and Labor in the Developed Democracies." She underscores a central development in the literature that has driven a broad reorientation in the study of labor, social democracy, and the welfare state by directing attention to the contribution of employer interests and strategies in shaping the political economy of labor in the advanced industrial countries. Thelen explores the distinctive strengths and weaknesses of two distinct strands in this new literature, claiming that neither can stand alone and that insights generated by each are incomplete, even misleading, to the extent that scholars fail to incorporate the insights of the other. More broadly, she illustrates the explanatory power of an historical-institutional framework by underscoring the weaknesses of research strategies that emphasize one component—the historical or the institutional—to the neglect of the other.

IAN SHAPIRO

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## *The State of Democratic Theory*

My aim here is to assess the current state of democratic theory. Such an undertaking needs a yardstick, two of which suggest themselves. One is normative, implied when we ask how persuasive the theories are that seek to justify democracy as a system of government. The other is explanatory, prompted by asking how successful the theories are that try to account for the dynamics of democratic systems. Normative and explanatory theories of democracy grow out of literatures that proceed, for the most part, on separate tracks, largely uninformed by one another. This is unfortunate, partly because speculation about what ought to be is likely to be more useful when informed by relevant knowledge of what is and what is feasible, and partly because explanatory theory too easily becomes banal and method driven when isolated from the pressing normative concerns that have fueled worldwide interest in democracy in recent decades. Accordingly, I take an integrative tack, focusing on what we should expect of democracy and on how those expectations might best be realized in practice.

Sharpening this focus, one inevitably confronts dissensus on both issues. The present essay is organized around these disagreements. I begin with a discussion of the main contending views of democratic purposes, those involving the search for a common good and those that attend to the legitimate management of power relations. I make the case that the latter understanding of the enterprise makes better sense but that it is compatible with an appropriately stripped down view of the former. In the second section, my attention shifts to mechanisms for advancing democracy. I start with the debates among Schumpeter and his critics, dividing the latter into two groups: those who think his competitive democracy desirable but insufficient and those who think it undesirable. I find wholesale rejections of Schumpeterianism to be unpersuasive, concluding that the more fruitful path is to explore ways to make competitive democracy work better, expand its reach beyond governmentalist institutions, and supplement it with other institutional devices. This leads to a consideration of the literature on the extent to which electoral systems can operate as engineering devices to pro-

mote competitive democracy in societies usually thought antithetical to it in the third section. Here I conclude there is no good reason to suppose any society inherently incapable of democracy, but I argue for an incremental approach given the dearth of reliable knowledge about the adaptability of politicized identities to the requirements of competitive democracy.

Even if democracy might in principle operate anywhere, it becomes plain from the literature on its durability, examined in the fourth section, that this does not mean democracy is easily instituted, or, once installed, destined to survive. These, too, are subjects about which empirically well supported generalizations are hard to come by. As with the literatures on democratic purposes and electoral engineering, the literatures on getting and keeping democracy prompt the thought that the state of democratic theory is a bit like the State of Wyoming: large, windy, and mainly empty. The sprawling scholarship reveals that we do know something about the economic preconditions for viable democracy, but, notwithstanding the confident assertions of various commentators, we are mainly in the dark about the cultural and institutional factors that influence democracy's viability. Prudence suggests that although it is wise to try to inculcate support for democracy among those who operate it, it is far from clear how important this is or how to achieve it.

If Schumpeterian democracy needs supplementation, the questions arise: who should do the supplementing and what should it be? Debates about these issues are taken up next. Because there are no perfect decision rules, a purely procedural scheme like competitive majority rule can produce self-defeating results. Most obviously, majorities can use their power to undermine democratic freedoms by abolishing opposition, undermining future political competition. But democratic procedures can have perverse consequences in a host of subtler ways as well. Partly for this reason, some democratic theorists argue for theories of substantive democracy against which the results of procedures can be evaluated. As I note in the fifth section, however, we confront a proliferation of substantive theories, yet lack a compelling way to choose among them. As a result, I agree with those who propose a middle-ground approach, in which courts or other second-guessing institutions should play a reactive, escape-valve role in limiting the perverse consequences of democratic procedures. This is followed by a discussion of alternative ways for courts to behave in democratic systems, leading to the conclusion that their legitimacy will likely vary with the degree to which they act in democracy-sustaining ways.

## ■ | Conflicting Views of Democratic Purposes

The idea that democracy does or should converge on the common good finds its locus classicus in Rousseau's *Social Contract*, and in particular in

his contention that decision procedures should converge on a general will that embodies the common good. Rousseau (1968, [1762] 72) famously, if vaguely, characterized this by saying that we start with “the sum of individual desires,” subtract “the plusses and minuses which cancel each other out,” then “the sum of the difference is the general will.” Attempts to make sense of this formulation have spawned two literatures: an aggregative one, geared to finding out just how we are supposed to do the relevant math, and a deliberative one, concerned with getting people to converge on the common good where this is understood more robustly than totting up exogenously fixed preferences.

### AGGREGATIVE VERSUS DELIBERATIVE CONCEPTIONS OF THE COMMON GOOD

Much twentieth-century literature on aggregation has focused on the difficulties of achieving it coherently. Its proponents accept that the goal of democratic decision procedures should be to discover something like a general will, referred to in the modern idiom as a social welfare function. Following Arrow (1951), they often notice that even moderate disagreement frustrates its discovery and conclude that democracy is impossible as a result. The possibility of voting cycles (results moving among outcomes depending on the order of voting) means that any given outcome may be an artifact of the decision procedure or who controls the agenda, not anything that might meaningfully be identified as the popular will. The result has been a large technical literature about the relative merits of different decision rules as aggregative procedures, and the constraints that must be imposed on preferences to avoid the possibility of cyclical majorities. These literatures have been the subject of many reviews and will not be discussed here.<sup>1</sup> Suffice it to note that if the purpose of democracy is to arrive at social welfare functions, in many circumstances this may be elusive.<sup>2</sup>

The deliberative view is concerned with transforming preferences

1. The most comprehensive and accessible, if somewhat dated, review is Mueller 1989, ch. 6. See also Shapiro 1996, ch. 2 and Przeworski 1999.

2. Here we should distinguish the observation that cyclical preferences in the population make outcomes of democratic decision rules *arbitrary*, in the sense that had things been voted on in a different order some other proposal would have won, from the claim that they are *manipulable*, in the sense that some agenda setter determines the result. Despite the claims of some commentators to the contrary, there is little evidence of successful manipulation of this kind in democratic politics (see Green and Shapiro 1994, ch. 7; Mackie 2000). This distinction matters because there may be reasons for according legitimacy to results that are arbitrary but not manipulated, in the relevant senses, from the perspective of the power management view discussed below. It should, however, be noted that even the claim that results are arbitrary depends on the proposition that cyclical preferences are likely in large populations. For a contrarian argument, see Tangian 2000 17, 337–65.

rather than aggregating them. It is not really Rousseauist (Rousseau had no faith in deliberation as a useful political device). However, it owes something to his injunction that people should not vote their individual preferences but rather their perceptions of what is good for the society as a whole.<sup>3</sup> The goal is to move us “beyond adversary democracy” (Mansbridge 1980). Deliberative remedies are put forward in response to various maladies that are perceived as pervading contemporary democracy. Poor quality of decision making, sound-bite politics, low levels of participation, declining legitimacy of government, and ignorant citizens are among the more frequently mentioned. The idea is that if we can get away from the soap opera of electoral one-upmanship, more thoughtful and effective political choices will result.<sup>4</sup> Deliberative forums can range from town meetings, to designated deliberation times, to citizen juries and “deliberative polls”—randomly selected groups who become better informed about particular issues and render decisions as to what should be done (Fishkin 1991). On some accounts such entities should inform existing processes, on others they should replace them en route to instituting a more robust participatory politics. The unifying impulse motivating these proposals is that people will modify their perceptions of what society should do in the course of discussing this with others. The point of democratic participation is more to manufacture the common good than to discover it on this account.<sup>5</sup> The assumption is that if people talk for long enough in the right circumstances, they will agree more often and this is a good thing.

Both propositions can be challenged. Deliberation can bring differences to the surface, widening divisions rather than narrowing them. This is what Marxists hoped would result from consciousness-raising: it would lead workers to discover their interests to be irreconcilably at odds with those of employers, assisting in the transformation of the proletariat from a class in itself to a revolutionary class for itself. In the event, these hopes proved naive. The general point remains, however, that there is no obvious reason to think deliberation will bring people together, even if they hope it will and want it to. A couple with a distant but not-collapsing marriage might begin therapy with a mutual commitment to settling long-standing differences and learning to accommodate one another better on matters

3. For Rousseau, voting was a means of disciplining private interest by getting people to focus on what is best for society as a whole. As he put it: “When a law is proposed in the people’s assembly, what is asked of them is not precisely whether they approve the proposition or reject it, but whether it is in conformity with the general will which is theirs; each, by casting his vote, gives an opinion on this question” (1968, [1762] 153).

4. For one influential argument, see Gutmann and Thompson 1996. For criticism, see Macedo 1999.

5. Indeed, deliberative theorists sometimes write as if the activity of searching for the common good is itself the common good. See Shapiro 1996, ch. 4 for discussion.

that cannot be resolved. Once honest exchange gets underway, however, they might unearth new irreconcilable differences, with the effect that the relationship worsens and perhaps even falls apart in acrimony. Deliberation can reasonably be expected to shed light on human interaction, but this may reveal hidden differences as well as hidden possibilities for convergence. It all depends on what the underlying interests, values, and preferences at stake are.<sup>6</sup>

Even when agreement is achievable through deliberation, it is not always desirable. People may not want to settle some disagreements. They may derive satisfaction from differentiating themselves from one another, as contemporary theorists of difference suppose. Alternatively, they may perceive consensus as leading to mediocrity, as Mill and Tocqueville worried. It is the competition of ideas—argument rather than deliberation—that such theorists thought vital to liberty, and, as I observe below, institutional analogs of this thought lie at the core of many contemporary democratic commitments. Suffice it to conclude here that deliberation need not lead to agreement, and when it does this may not be advantageous.

## MANAGING POWER RELATIONS

The deliberative view is also criticized on the grounds that its goal of fostering agreement rests on overly sanguine assumptions about power. One person's consensus is another's hegemony, and although uncoerced agreement may be feasible in some ideal world or "speech situation" (Habermas 1979, 1984), in the actual world power relations suffuse virtually all human interaction. The most trenchant versions of this critique, most recently associated with Michel Foucault (1972, 1977, 1980) but with progenitors as different as Plato, Hobbes, Marx, Mosca, and Michels, can be taken to imply that democratic control of power relations is impossible: power relations evolve, displacing one another over time, but they are never eliminated. Collective life remains power and domination all the way down.<sup>7</sup>

The ubiquitous view overreaches in two respects: by failing to discriminate among different ways in which power is exercised and by equating the valid observation that power suffuses all collective life with the implausible claim that all collective life is reducible to power relations. To recognize power as ubiquitous is not to concede that all power is alike or that some ways of living with it might not be better than others. And to say that power relations suffuse activities as various as workplace, family, and church is not to deny that things go on in all these areas other than exer-

6. See Sunstein 2000 for a discussion of empirical conditions under which deliberation leads to a divergence rather than a convergence of opinion.

7. See Laclau and Mouffe 1985 and Hayward 2000 for illustrations and discussions of this view.

cises of power. Producing goods and services may often, and perhaps inevitably, involve power relations, as may pursuing intimacy, affection, education, and spiritual fulfillment. But these activities are not themselves power relations.

Important challenges for democratic theorists revolve around devising mechanisms to govern the power dimensions of human interaction as well as possible, while minimizing interference with the other activities in which people engage. Rather than see democracy as a device for discovering or manufacturing the common good, democracy can be understood as a device for managing the power dimensions of activities people engage in as they pursue their own—individual or shared—conceptions of the good. Democracy is a subordinate or conditioning good on this account, and the creative challenge is to find ways to structure power relations democratically while limiting interference with the superordinate goods people pursue.

At a minimum this involves making decision making more inclusive of those who are affected by the results and creating avenues for meaningful—if loyal—opposition by those who are adversely affected by prevailing decisions. Because there are no perfect decision rules, a degree of imposition attaches to all collective decisions. Those who lose may reasonably aspire, therefore, to achieve a different result in the future. This suggests that opposition rights are important for democratic politics independently of the value of inclusive participation, though they have been relatively underattended to by democratic theorists.<sup>8</sup> To the extent that this commitment rests on an idea of the common good, it is perhaps best characterized as Machiavelli did when he identified it as that which those with an interest in avoiding domination share in common.<sup>9</sup>

Conceiving of democracy as a system for structuring power relations offers several advantages. First, it poses normative questions about democracy in a “compared to what?” framework, since democracy is now judged not by the either/or question whether it produces social welfare functions or leads to agreement but rather by how well it enables people to manage power relations measured by the yardsticks of promoting inclusive participation and minimizing domination. By its terms this is a comparative question. Second, the power-centered approach invites us to avoid another kind of binary thinking—about democracy itself. Ways of managing power relations can be more or less democratic. On this conception, relevant questions concern how much democracy is possible or desirable in a given

8. Exceptions are Dahl (1971), Burt (1992), Shapiro (1999), Schiffrin (1999), and Pettit (2000).

9. See his consideration in *The Discourses* of the Roman argument that the common people should be made the guardians of freedom because, unlike the aristocracy whose desire is to dominate, their desire is not to be dominated (1970, 1: 5 [c. 1517]).

situation. Particularly when democratizing power relations comes at a price in terms of other goods, these are important questions to ask. It is one of the singular contributions of Dahl's idea of polyarchy that it turns questions about democracy into more-or-less questions rather than whether-or-not questions.<sup>10</sup> Third, the power-centered approach brings the normative literature on democratic theory into confrontation with the empirical political science literature on democracy. Democratic theorists have often paid too little attention to this literature, giving their speculations a less than worldly quality and leading most others to ignore them as a result.

Even when theorists take up the first two questions, inattention to empirical practice can mar the results. For instance, Buchanan and Tullock (1990) answer the "how much democracy?" question by noting that its benefits must be traded off against other valuable ways of spending one's time. Requiring high levels of agreement enables people to protect their interests, they say, but this takes time that could be spent on other activities. So they come up with a sliding scale: the more important an issue to you, the more likely you are to preserve a veto power by insisting on unanimity rule or something close to it. For things that matter less, however, it will make better sense to run the risk that you will lose on any given vote, accept majority rule, and reduce decision-making costs. Democracy is thus best suited to issues of moderate importance on their account. Issues of high importance should be insulated from democracy, while issues of low importance might best be delegated to administrators. Yet Buchanan and Tullock assume, without adducing any evidence, that it is the standard panoply of libertarian protections that people most value, so it is these that are to be insulated from change by supermajorities or even unanimity rule. If we query that assumption, their substantive claims are all thrown into doubt.

The costs of failing to attend to actual politics run deeper, as can be seen if we focus more carefully on the "compared to what?" question. Even if we knew what people regard as the most important questions (which includes the heroic assumption that they all agree on this), why assume that they would want to insulate these from democracy by insisting on supermajorities or unanimity rule? This can only make sense if we start, as Buchanan and Tullock do, from the fiction of no collective action in a pre-

10. Dahl identifies eight elements that measure the degree to which the conditions of polyarchy are met. These eight conditions deal with four periods: voting, where votes by members of the political system must have equal weight, and the choice with the most individual votes wins; prevoting, where members have equal chances of presenting alternatives and information about alternatives; postvoting, where those leaders or policies that won the vote displace those with less votes and the orders of elected officials are followed; and the interelection period, where decisions are subordinate to those made during elections, for example, an interim senator will be replaced by the senator who wins the next election (1956, 71–76, 84–89).

political condition, and then ask what decision rule people would choose in order to minimize the likelihood that their preferences would be frustrated in the future to the extent that they agree to depart from that condition. But, as Barry (1990), Rae (1975), and others have noted, if we jettison this unrealism there is no reason to regard unanimity rule as the appropriate default option, since it privileges the status quo.<sup>11</sup> In the real world of ongoing politics, if I assume that I am as likely to oppose a given policy as to support it regardless of whether or not it is the status quo, then majority rule or something close to it is the logical rule to prefer. At a minimum, we would want to know who benefits from, and who is harmed by, the status quo, before rendering a judgment on the desirability of privileging it against change.<sup>12</sup>

The “compared to what?” perspective suggests a more realistic question: not “whether-or-not collective action?” but rather “what sort of collective action?” That is, we consider the power dimensions of different collective action regimes vis-à-vis one another. Perhaps due to their proclivity for thinking in social contract idiom, libertarian commentators often write as if not having collective action is a coherent option in societies that nonetheless have private property, enforcement of contracts, and the standard panoply of negative freedoms. As Holmes and Sunstein (1999) have recently reminded us, however, these are costly institutions that stand in need of effective collective enforcement. The libertarian constitutional scheme is a collective action regime maintained by the state, and disproportionately financed by implicit taxes on those who would prefer an alternative regime.

The literature on deliberative democracy is likewise vulnerable. Apart from the conceptual difficulties already mentioned, inattention to “compared to what?” questions often makes deliberation look better than it should. For instance, advocates of deliberative democracy such as Gutmann and Thompson (1996) and Ackerman and Fishkin (2000) argue for the merits of deliberation by pointing out how little of it there is in contemporary politics dominated by superficial television campaigns and political advertising. But sound-bite politics and media driven campaigns may well result principally from the powerful American antipathy toward publicly financed elections and the concomitant influence of private money in politics. This would presumably remain in a world of expanded deliberative institutions, given the Supreme Court’s 1976 decision that reg-

11. See Barry 1990, Rae 1969, 40–56, and M. Taylor 1969, 228–31. When the number of voters is odd, the optimal decision rule is majority rule,  $n$  over 2, plus  $\frac{1}{2}$ ; when  $n$  is even, the optimal decision rule is either majority rule ( $n$  over 2 plus 1) or majority rule minus 1 (simply  $n$  over 2). Generally see Mueller 1989, 96–111.

12. Even if we accept the contractualist metaphor the logic of Buchanan and Tullock’s defense of unanimity rule can be shown to break down once time and externalities are taken into account (see Rae 1975, 1270–94).

ulating political expenditures is an unconstitutional interference with free speech.<sup>13</sup>

Any credible defense of deliberative democracy in the U.S. context would have to show how deliberative institutions would be any less corrupted by those with the resources to control agendas and bias decision making than existing institutions are and that it would merit its cost. Consider, for instance, the proposal for a “deliberation day,” to be held a week before national elections, in which everyone would be paid \$150 to show up at their local school or community center to deliberate. According to its proponents (Ackerman and Fishkin 2000; 29) this would cost \$15 billion a year in public funds, not to mention the indirect costs to the economy. It is hard to see what benefit would result from so vast an expenditure once candidates have been selected, platforms chosen, interest groups deployed, and campaign funds spent. By contrast, \$15 billion a year spent to support fledgling third parties or publicly financed elections might attenuate many of the pathologies that lead people to call for more deliberation.<sup>14</sup>

A fourth advantage of the power-centered approach is that it offers a tractable perspective on long-standing conundrums about the relations between democracy and citizenship. Democratic theory is often said to be impotent when confronted with questions about its own scope. It depends on a decision rule, usually some variant of majority rule, but this assumes that the question “majority of whom?” has been settled. If this is not done democratically, however, in what sense are the results that flow from democratic decision making genuinely democratic? Thus Shapiro and Hacker-Cordón observe that “a chicken-and-egg problem lurks at democracy’s core. Questions relating to boundaries and membership seem in an important sense prior to democratic decision-making, yet paradoxically they cry out for democratic resolution” (1999b, 1).

If democracy is about managing power relations, it becomes unnecessary to think of questions about citizenship as different from questions about any other superordinate good that is conditioned by democratic con-

13. In *Buckley v. Valeo* 424 U.S. 1 (1976) the Court held, inter alia, that although Congress may regulate financial contributions to political parties or candidates, it cannot otherwise regulate private expenditures on political speech. The Court has since allowed some minor constraints on corporate expenditures in *Austin v. Michigan State Chamber of Commerce*, 110 S.Ct. 1391 (1990), but for all practical purposes the *Buckley* rule makes it impossible to limit privately funded political advertising.

14. Ackerman and Fishkin insist that “it is a big mistake to view the annualized cost of \$15 billion through the lens of standard cost-benefit analysis” on the grounds that its “large” benefits “cannot be reckoned on the same scale as other elements in the cost-benefit equation” (2000, 29). Even if we were to concede that the benefits could coherently be declared to be large at the same time as they are said to be incommensurable with their costs, their claim ignores the point stressed here: that its benefits surely should be weighed against other ways in which such a sum could be spent to enhance U.S. democracy.

straints. The claim to a democratic say in collective decisions, whether or not one is a citizen, appropriately rests on the causal principle of having a pertinent affected interest. The rallying cry of the American revolutionaries, after all, was “No taxation without representation!” not “No taxation without citizenship!” There might be good reasons for restricting citizenship, but this does not mean that noncitizens should be denied rights to vote on matters that affect their pertinent interests, as when a decision is taken to deny the children of illegal aliens access to the California public schools<sup>15</sup> or when “guest workers” in foreign countries claim a say in the laws that govern them (see Barbieri 1998). The causally based view has been invoked in a number of recent arguments aimed at decentering citizenship as decisive in determining rights of democratic participation, and replacing it with systems of overlapping jurisdiction in which different groups are sovereign over different classes of decisions, as is occurring in the governance of the European Union. The operative thought here is that the appropriate demos should be settled decision by decision, not people by people.<sup>16</sup>

Difficulties are, of course, bound to arise in settling conflicting claims about whose pertinent interests are affected by a given decision. Controversial as this might often be, arguments about who has a legitimate claim to citizenship are scarcely less so (see R. Smith 1997). Moreover, there is instructive experience with arguments about affected interests in other arenas. In dealing with tort actions, for instance, courts develop rules for deciding who should have standing to sue, for sorting genuine from frivolous claims, and for distinguishing weaker from stronger allegations to have been adversely affected by an action. The comparison illustrates that institutional mechanisms can be developed to assess and manage conflicting claims about how pertinent interests are affected. They may be imperfect mechanisms, but they should be evaluated by reference to the other imperfect mechanisms of collective decision making that actually prevail in the world, not by comparison with an ideal that prevails nowhere.<sup>17</sup>

## ■ | Schumpeterian Competition

The most influential twentieth-century approach to the democratic management of power relations is Schumpeter’s argument (1942) in *Capitalism*,

15. This was passed as Proposition 187 by a majority of 59 to 41 percent in a California ballot initiative in November 1994 and subsequently struck down in federal court as violating the constitutional right to education regardless of immigration status and because immigration law is a federal rather than a state matter.

16. See Pogge 1994b, Antholis 1993, and Wendt 1994. For other arguments that decisions about membership should not be seen as anterior to democratic decision making, see Shapiro and Hacker-Cordón 1999b, esp. chs. 6, 10, 12, 15.

17. For elaboration and defense of his claim, see Shapiro 1999, 31–39.

*Socialism and Democracy*. It reduces to a double claim: (1) that structured competition for power is preferable both to Hobbesian anarchy and to the power monopoly that Hobbes saw as the logical response to it and (2) that the choices among anarchy, monopoly, and competition are the only meaningful possibilities. Both of Schumpeter's claims were innovative, and, although they have drawn heavy critical fire, neither has been driven from the field.

#### ADVANTAGES AND LIMITATIONS OF STRUCTURED COMPETITION

Schumpeter's view is often said to be conservative. There is substance to this, but focusing too quickly on it obscures the radical dimensions of his argument. He was radical first in seeing an alternative to the idea that Western political theorists had taken over more or less self-consciously from Hobbes: that power is a natural monopoly. Conventional liberals, Marxists, and elite theorists all held variants of this view. On the surface republicans took a more complex view that power is divisible and can be controlled by competition among the political branches. However, as Dahl (1956, 30–32) showed long ago, Madison's account was long on rhetoric—"ambition will be made to counteract ambition"—(Hamilton, Madison, Jay 1966 [1788], 160) and short on explaining how such mechanisms would actually work. *The Federalist* solution was mainly a matter of engineering institutional sclerosis to make all government action difficult and so protect the interests of landed elites. In this it was not qualitatively different from standard liberal justifications of bicameralism, strong constitutionalism, and other types of institutional veto that have often been put forward to limit democracy (see Riker 1982, 13: 101–16; Riker and Weingast 1988; Holmes 1995).

Schumpeter's account was a radical departure in that he thought that rather than succumb to power (Hobbes) or hem it in (all these others), it could be controlled by being turned into an object of electoral competition. The authors of *The Federalist* had distinguished incentives from constraints in the pursuit of power; Schumpeter actually delivered on an incentives-based account.<sup>18</sup> Whereas constraints are geared to limiting politicians' power via rules (such as separation of powers or other constitutional limitations), incentives link what politicians find strategically beneficial to the demands of competitive politics.

18. Thus in *Federalist No. 48* Madison insists that "mere demarcation on parchment of the constitutional limits of the several departments, is not a sufficient guard against those encroachments which lead to a tyrannical concentration of all the powers in the same hands." Such "exterior provisions" are inadequate and must be supplemented, as he elaborates in *Federalist No. 51*, by giving "those who administer each department the necessary constitutional means *and personal motives* to resist encroachments of the others" (Hamilton, Madison, Jay 1966 [1788], 150–51, 159–60, emphasis added).

Once parties are modeled on firms trying to maximize votes as analogs of profits, then leaders can be seen as disciplined by the demands of competition. Attempts by Downs (1957) and others to turn this into a predictive theory of electoral competition have been less than successful (see Green and Shapiro 1994, ch. 7), but as a normative theory Schumpeter's account broke new ground. From his perspective, the value of competition is twofold: it disciplines leaders with the threat of losing power in the same way that firms are disciplined by the threat of bankruptcy, and it gives would-be leaders the incentive to be responsive to more voters than their competitors are. The—always problematic—theory of representative government is thus replaced by a political analog of consumer sovereignty.

Schumpeterian democracy is often denoted “minimal” for one of two reasons: its exclusive focus is on (usually national) political institutions narrowly defined, and Schumpeter's definition of democracy is by reference to competition for power. However, nothing inherent in Schumpeter's reasoning thus limits it. To say that social arrangements other than national political institutions should be democratized is not to deny that Schumpeterian tools might be useful in that endeavor, as Shapiro (1999, chs. 4–7) has illustrated. Nor is it to assert that national political institutions could not benefit from Schumpeterian reform even if other reforms might also be deemed necessary. Notice, too, that even in Schumpeter's formulation the sense in which competitive democracy is minimal can be exaggerated. The competitive requirement has been read by modern Schumpeterians like Huntington (1991) to mean that a polity is not democratic unless governments have at least twice given up power following electoral defeat, which would arguably have ruled out the United States until 1840, Japan and India for much of the twentieth century, and most of the so-called third wave democracies in the ex-Communist countries and sub-Saharan Africa. Minimal, in short, does not mean negligible.

Conceding this, it is less than clear that electoral competition is much of a disciplinary system given the high rates at which incumbents are often reelected (see Lowenstein 1995, 653–67). But again there is the response: compared to what? The discipline of the electoral constraint might seem modest when compared to an ideal that prevails nowhere, but achieving it would be a substantial gain for the billions who live in countries where it is lacking (see Przeworski 1999, 43–50).

The sense in which Schumpeterianism produces responsive government is also constrained. In theory at least, the standard Left criticism of markets—that they reward those with greater resources—does not apply. One-person, one-vote is a resource equalizer that is widely seen as a non-negotiable requirement of democracy, despite occasional defenses of markets in votes on efficiency or intensity grounds (see Buchanan and Tullock 1962, 125–26, 132–42). The difficulty in practice is that, particularly in the United States, politicians compete in the first instance for campaign contributions and only secondarily for votes. Perhaps there would be decisive

voter support for confiscatory taxes on estates worth over \$10 million, but no party proposes this. Indeed, in June 2000 the U.S. Congress gave strong bipartisan support to a bill that would abolish the existing estate tax—paid by only the wealthiest 2½ percent of Americans.<sup>19</sup> It seems likely that politicians avoid taxing the wealthy for fear of the funds that would be channeled to their electoral opponents if they sought to do so. Empirical study of such claims is inherently difficult, but it seems reasonable to suppose that the proposals politicians offer are heavily shaped by the agendas of campaign contributors; why else would they contribute? Add to this the fact that the small number of major parties means that what we really get is oligopolistic competition, and it becomes clear that the sense in which parties are as attentive to voters as firms in competitive markets are to consumers is highly attenuated.

These powerful objections are aimed not at the idea of political competition but rather at the ways in which the system is imperfectly competitive. Disproportionate power of campaign contributors could be reduced (proposals for reform abound),<sup>20</sup> and reforms could be instituted to increase the number of parties, facilitating more competition. Indeed, it is remarkable that public interest litigants, activists, and political commentators (not to mention political theorists) do not argue for attempts to use antitrust laws to attack the existing duopoly. If competition for power is the life blood of democracy, then the search for bipartisan consensus (and the ideal of deliberative agreement that lies behind it) is really anticompetitive collusion in restraint of democracy. Why is it that people do not challenge legislation that has bipartisan backing or other forms of bipartisan agreement on *these* grounds? It is far from clear that there are fewer meritorious reasons to break up the Democratic and Republican Parties than there are to break up AT&T and Microsoft.<sup>21</sup>

There are legal obstacles to antitrust action against political parties but also untested legal possibilities. For instance, the Supreme Court's Noerr-Pennington doctrine rules out applying antitrust laws to "valid governmental action, as opposed to private action."<sup>22</sup> But this does not speak to activities by political parties. Moreover, although the Sherman Act has

19. The Death Tax Elimination Act of 2000, or House Resolution 519, was passed by a vote of 279 to 136 on 9 June 2000. The Senate voted for it by 59 to 39 on 14 July 2000—Bastille Day! It was subsequently vetoed by President Clinton. However the estate tax was subsequently reduced and slated for repeal in 2009 as part of President George Bush's comprehensive tax cut enacted in 2001.

20. See Ackerman 1993 and Ayres 2000 for examples.

21. The Progressives did advance a version of this critique (see Epstein 1986, 17–71). The lone voice in the contemporary literature seems to be Wittman's (1973).

22. *Eastern R.R. President's Conf. v. Noerr Motor Freight*, 365 U.S. 127 (1961), 136.

generally been held not to apply to noneconomic entities such as labor unions, exceptions are made when a conspiracy is alleged between such an entity and a business to injure the interests of another business or when the agreement sought does not encompass a "legitimate union interest."<sup>23</sup> Analogously, activities by political parties might not be exempted if they allied with corporate contributors to promote anticompetitive practices or could otherwise be shown to be seeking agreements with one another which went beyond "legitimate party interests."

The constitutional obstacles to applying antitrust principles to politics are rooted in the right of petition and the "ability of the people to make their wishes known to their representatives."<sup>24</sup> But the rationale for this type of political exemption does not go to forms of collusion that undermine the process of free political expression itself, which parties engage in by maintaining prohibitive costs to entry, agreeing to exclude minor parties from political debates, and related practices. Because the Sherman Act has been held to apply only to business combinations<sup>25</sup> and to organizations that have commercial objectives,<sup>26</sup> antitrust regulation of such behavior might require additional lawmaking. It is hard, for obvious reasons, to envision legislators enacting such laws, but it is less difficult to think of political antitrust measures being adopted as a result of ballot initiatives.

Arguments about the merits of party proliferation (usually to be achieved via proportional representation) are sometimes advanced on the quite different ground that this would lead to fairer (read: more representative) outcomes. Notice that such arguments can be oversold. Proportional representation may lead to more representative electoral outcomes by offering voters a broader array of parties, but it need not lead to more representative governments. Frequently we see this in Israel when small extremist parties needed for any viable governing coalition exert disproportionate influence on government policy, leading to highly unrepresentative government. Nonetheless, trying to ensure that the parties competing with one another are more representative of the electorate is a challenge that can in principle be taken up within the Schumpeterian framework, and

23. *Connell Constr. Co. v. Plumbers & Steamfitters Local Union No. 100*, 483 F. 2d 1154, 1164 (5th Cir. App. 1973); see also *Local Union No. 189, Amalgamated Meat Cutters v. Jewel Tea Co.*, 381 U.S. 676.

24. *Noerr*, 356 U.S. 137, 138. Thus the court rejected a claim by the State of Missouri that the National Organization of Women had violated the Sherman Act by organizing a conference boycott in states that had not ratified the Equal Rights Amendment, holding that the participants were engaging in legitimate forms of political organizing rather than undermining commercial competitors (*Missouri v. National Organization of Women, Inc.*, 467 F. Supp. 289, 304 [1979], cert. denied 449 U.S. 842 [1980]).

25. *Parker v. Brown* 317 U.S. 341, 351 (1943).

26. *Klor's Inc. v. Broadway-Hale Stores, Inc.*, 359 U.S. 213 n. 7, and *Apex Hosiery Co. v. Leader*, 310 U.S. 469, 493 n. 5.

there is some reason to think that, on average, proportional representation leads to policies that are closer to the preferences of the median voter than does competitive alternation in power (see Rae 1967, 1995; Powell 2000).

### ALTERNATIVES TO SCHUMPETERIANISM?

Pressed sufficiently far, however, the emphasis on representativeness turns into a rejection of the competitive ideal. This becomes evident if we suppose that a government could represent all interests optimally<sup>27</sup> and ask who the opposition would then represent? Sometimes the implicit ideal of those who emphasize representativeness is agreement: if all groups are fairly represented, then they can negotiate an outcome that all can accept, making opposition politics unnecessary. This fallacious reasoning has already been explored in my discussion of Buchanan and Tullock; it need not detain us here.

A different defense of proportionality and consensus democracy rests on the belief that disagreements in some societies are so profound that promoting competitive politics amounts to pouring gasoline on conflictual fire. Proponents of this view generally have ethnically and racially divided societies in mind, and they think of the relevant identities as both primordialist and overdetermining: given for all time and more important than all other matters. Unless there are enough crosscutting cleavages of interest to institutionalize uncertainty about future outcomes, losers have no reason for allegiance to the system. Minorities will expect to lose on every issue.<sup>28</sup> When elections amount to an ethnic or racial census, minorities would do better to reach for their guns, try to secede, or otherwise defect from the political process (see Shapiro 1996, chs. 4, 7).

Such thinking gives rise to consociationalism. Its injunction is to devise systems of minority vetoes or other mechanisms to force leaders of different groups to work out a *modus vivendi* and govern as a “cartel of elites” (Lijphart 1969, 213–15, 222). Here the appeal to consensus is not based on

27. Achieving such optimality is elusive in practice, as the architects of the McGovern-Frazier reforms to achieve pure proportionality within the Democratic Party learned when it became apparent that there are more pertinent interests than could be represented proportionately (see Ranney 1975). It may also be elusive in theory (see Rae et al. 1981).

28. If A is opposed to B on issue 1, but knows that in the future he is likely to be allied with B in opposition to C on issue 2, then A has an incentive to moderate his present conflict with B as much as possible and cultivate her as a future ally. If, on the other hand, A and B know that they are likely to be on opposite sides of the fence on every issue, then there are no incentives favoring moderation, and if one of them calculates that he will always be in the minority, he has no self-interested reason to be committed to a democratic order. The pluralists generalized this, arguing that only when cleavages are pluralistic or crosscutting will a democratic order be stable (see Miller 1993, 734–47).

fairness but rather on avoiding civil war. If the primordialists are right, competitive democracy is impossible in such circumstances and consociational accommodation is the best we can hope for. If they are wrong, however, their remedy might sustain—or even produce—the malady to which it is alleged to respond.<sup>29</sup> Perhaps consociational arrangements (such as those embodied in the Dayton accords in the Yugoslavian conflict) are required to end ethnic civil wars, but this does not mean they supply a viable basis for democracy. Critics of primordialism are quick to emphasize that consociational institutions can manufacture or exacerbate ethnic division. In their view, ethnic, racial, and other group-based antipathies are neither natural nor necessary. Politicized divisions might have developed differently than they have, and they can change (see Vail 1989). These writers seldom get into the technicalities of how they believe this might be accomplished, but they think it plausible to suppose that politicized identities could develop that differ radically from those presently prevailing in the world. In particular, people might come to accept, perhaps even celebrate, differences that today are sources of mutual hatred (see Jung 2000, ch. 9).

Conceding that politicized identities change and are socially constructed does not, however, generate the conclusion that they are infinitely malleable. It does not even entail that those identities that might not have been mobilized politically but nonetheless have been can subsequently be demobilized. This is more than the problem of getting the toothpaste back into the tube. The extent to which things are alterable may not vary with the degree to which they are socially constructed at all. Many features of the natural world, ranging from the temperature of our bath water to the genetic structure of our beings, can be altered by conscious human design. Socially constructed phenomena, by contrast, often defy all efforts at conscious human control. Markets are human constructions, yet we may be unable to regulate them so as to operate at full employment without inflation for long periods of time. Ethnic hatred might concededly be learned behavior and hence by its terms socially constructed, yet we may have no idea how to prevent its being reproduced in the next generation. Proponents of social constructionism leap too quickly from that idea to the assumption of alterability; at best the two are contingently related.

An intermediate and more plausible account might run as follows. Human beings are shaped by context and circumstance, but they are also constrained by their inherited constitutions. These constitutions may themselves evolve, but at a given time and place they limit the possibilities of social reconstruction. Human psychology is always malleable but never infinitely so, and certain ways of shaping it are likely to be more effective

29. As Courtney Jung and I have argued is the case in regard to South Africa (see Jung and Shapiro 1995; Shapiro and Jung 1996). More generally, Horowitz (2000) points out that consociationalism has been singularly unsuccessful as a device for managing ethnic conflict.

than others in any given situation. The interesting questions concern what the limits to this malleability are and which forms of social reconstruction are likely to be more satisfying and effective than others. The difficulty for democratic institutional designers is that these are empirical questions about which there is not much accumulated knowledge in the social sciences. As a result, it seems wise to work at the margins and to think about institutional redesign rather than *tabula rasa* design. Identities are fixed to some—usually unknown—degree, but they also adapt to circumstances, incentives, and institutional rules. The goal should be to reshape such constraints, where possible, so that at the margins identities evolve in ways that are more, rather than less, hospitable to democratic politics. From this perspective the critique of consociationalism is that, to the degree that politicized identities are malleable, it tends to reproduce the wrong ones.

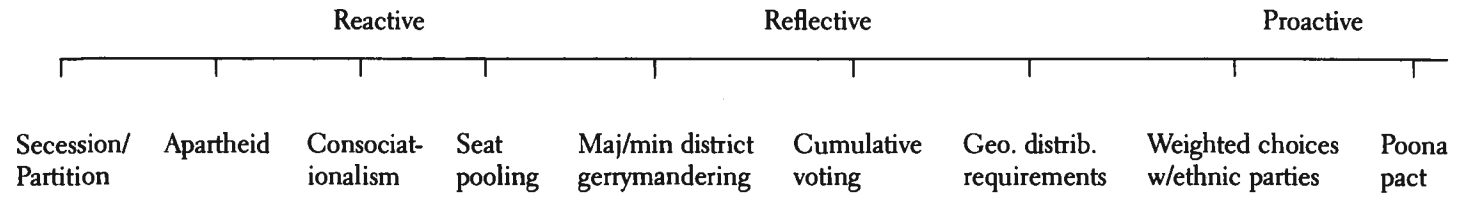
## ■ | Electoral Incentives and Multiplying Cleavages

Electoral systems are potential instruments for undermining ethnic conflict in the service of promoting competitive politics, but, given what has just been said, it is unclear how effective they can be. Assuming opinion to be at least partly mobilized and shaped from above, a logical place to start is the incentives facing candidates for office. In a Schumpeterian spirit the goal should be to avoid encouraging aspiring leaders to foment group-based hatred as they seek power. From this perspective we can array electoral systems on an ethnic engineering continuum, ranging from *reactive* systems that cater to ethnic difference, through *reflective* systems that are neutral with respect to existing preferences, to *proactive* systems that seek to alter them in ways that promote competitive democracy (see figure 1).

Secession and partition anchor the continuum's reactive pole. Next to it come apartheid and consociationalism (the former imposed by the stronger party, the latter sanctified by some kind of elite agreement), where the aspiration is to achieve functional partition within a unified polity. Further along are systems that engineer around ethnic differences to produce diversity in legislatures, as is the case with gerrymandering to create majority minority districts in the American South. These reactive responses all take ethnic difference as given, hoping to work around it. Toward the center of the continuum we come to reflective responses: those that are sensitive to ethnic difference but neutral in the sense of being biased neither in favor of it nor against it. The various cumulative voting schemes discussed by Lani Guinier's fit this description.<sup>30</sup> Here the principle is to give each

30. For Guinier's proposals, see Guinier 1991, 1077–154, 1994a, 109–37). On the battle over her confirmation as assistant attorney general for civil rights, which she lost for her advocacy of this scheme, see Guinier 1994b. Her fate suggests a criterion, in addition to representative fairness, for evaluating proposed decision rules: whether they can be widely understood and perceived as democratic.

**Figure I | Ethnic Engineering Continuum**



voter as many votes as there are seats. If a state is to have eight congressional representatives, every voter gets eight votes that can be cast however they wish: all for one candidate or spread among several. If there are intense ethnic preferences, members of a particular group can cast all eight votes for "their" representative; if not, not. Unlike racial gerrymandering and consociationalism, reflective schemes respond to ethnic preferences without doing anything to produce or reinforce them. As a result, they avoid the critique of reactive systems that they promote balkanization. Yet by the same token cumulative voting does nothing to ameliorate or undermine potentially polarizing forms of aspirational difference where these are present.

For engineered responses aimed at reducing such conflicts, we move to the proactive part of the continuum: arrangements that supply would-be leaders with incentives to avoid mobilizing support in ways that exacerbate cultural competition and to devise, instead, ideologies that can appeal across the divisions of relevant groups. Hence Donald Horowitz's contention that, when group-based antipathies are strong, electoral systems are needed that give elites incentives to compete for votes among politicized groups other than their own, and so promote accommodation rather than exclusionary politics (1991, 155, 1985). He describes a successful example of this kind from Malaysia, in which Malay and Chinese politicians were forced to rely in part on votes delivered by politicians belonging to the other ethnic group. The votes would not have been forthcoming "unless leaders could portray the candidates as moderate on issues of concern to the group that was delivering its votes across ethnic lines." In such situations, which Horowitz identifies as having operated for considerable periods (and then failed) in countries as different as Lebanon, Sri Lanka, and Nigeria, compromises at the top of a coalition are reinforced by electoral incentives at the bottom (Horowitz 1985, 154–55).

Another possible device is geographical distribution requirements, such as the Nigerian formula for presidential elections employed in 1979 and 1983, in which the winning candidate had to get both the largest number of votes and at least 25 percent of the vote in two-thirds of the then-nineteen states of the Nigerian Federation. This type of system would not work in countries like South Africa, however, given the territorial dispersion of politicized groups. In such circumstances, the two most-promising candidates are proportional representation, utilizing the single transferable vote system, and an alternative vote rule, listing more than one ordered preference but declaring elected only candidates who receive a majority, rather than a plurality, of votes. Both systems require politicians to cater to voters' choices other than their first preferences, assuming heterogeneous constituencies, so that the politicians' incentives work in the appropriate moderating directions. This will be further accentuated by the alternative vote system, assuming that parties proliferate (Horowitz 1985, 184, 166, 187–96). In many circumstances such vote-pooling systems are more likely

to achieve interethnic political cooperation than consociational arrangements or systems, whether first past the post or proportional, that merely require seat pooling by politicians in coalition governments. As reactive systems, they do nothing to moderate group antipathies. On the contrary, they give politicians incentives to maximize their *ex ante* bargaining position by increasing what economists might describe as their group's reservation price for cooperation.

Proactive incentives to avoid appealing to intergroup antipathies will not always work. Parties might proliferate within politicized groups in ways that undermine this dimension of the logic behind weighted vote schemes.<sup>31</sup> Moreover, some of the worst of what often (misleadingly) gets labeled interethnic violence is actually intraethnic violence that results when different parties seek to mobilize support from the same ethnic group. Much of the South African violence that erupted in the eastern part of the country after 1984 resulted when the United Democratic Front (representing the then illegal African National Congress [ANC]) was formed and challenged Inkatha Freedom Party (IFP) support among Zulus there, and some of the worst violence among white nationalists resulted from comparable competition for the white nationalist vote. There are limits to the degree that intraethnic competition of this sort can be ameliorated by weighted vote mechanisms. If parties have incentives to mobilize support in more than one ethnic constituency, they should avoid campaigning as ethnic parties any more than they have to. In practice, however, parties like the IFP—whose *raison d'être* is ethnic—may have little scope to campaign on any other basis. Accordingly, they may resist—perhaps violently—any inroads into their “traditional” sources of support. They can only play a zero-sum ethnic game.

When relying on the logic of cross-group mobilization does not lead to ethnic accommodation, it may be possible to move further along the continuum and become more explicitly proactive, as in the 1931 Poona Pact in India. It requires that Untouchables be the representative in 148 designated constituencies, a number corresponding roughly to their proportion in the population (Van Parijs 1996, 111–12). This both ensures that the specified number of Untouchables become parliamentary representatives and gives aspirants for office an incentive to seek support from all sectors of heterogeneous constituencies, not merely “their own” ethnic group. (Untouchables are not prohibited from running elsewhere, but, as geographically dispersed minorities in all constituencies, they are seldom elected.<sup>32</sup>) Attractive as such solutions can be in some circumstances, they involve manifestly paternalistic institutional design that is unlikely to win legitimacy unless there is widespread acknowledgment that a minority has been

31. For elaboration of these and related difficulties confronting Horowitz's proposals, see Shapiro 1993, 145–7.

32. In 1996 they occupied 3 out of a total of 400 (Van Parijs 1996, 112).

unjustly treated over a long time and that it will not otherwise be represented.<sup>33</sup> Even then, such proposals will likely be attacked on many of the same grounds as are reverse discrimination and affirmative action. They can also be expected to provoke the charge, if from a different ideological quarter, that those competing for the designated minority spots will lack the incentive to represent the relevant minority interests. Rather, the temptation will be to try to outperform the competition as Uncle Toms.

The further institutional designers try to move along the continuum toward explicit proactive systems that force integration in exclusionary and racist societies, the more they will learn about how much redesign of ethnic antipathy is feasible in them. At present the only statement that can be made with much confidence is that there is no particular reason to think any society inherently incapable of Schumpeterian electoral competition. As the Indian and Japanese examples underscore, even societies with profoundly inegalitarian cultures and undemocratic histories have adapted to the demands of democratic politics in ways that many would have insisted was impossible before the fact. South Africa may turn out to be another such case in the making, though the jury must remain out until ANC hegemony faces a serious challenge.

## ■ | Democracy's Durability

This is not to say that competitive democracy is easily established or, once established, easily sustained. Generations of scholars have theorized about the conditions that give rise to democracy. Tocqueville (1966 [1832]) alleged it to be the product of egalitarian mores. Seymour Martin Lipsett contends it is a by-product of modernization (1959, 69–105). For Barrington Moore the critical factor is the emergence of a bourgeoisie (1966, 413–32), while Rueschemeyer, Stephens, and Stephens (1992) argue that the presence of an organized working class is decisive. It now seems clear that there is no single path to democracy and, therefore, no generalization to be had about which conditions give rise to democratic transitions. Democracy can result from decades of gradual evolution (Britain and the United States), imitation (India), cascades (much of Eastern Europe in 1989), collapses (Russia after 1991), imposition from above (Spain and Brazil), revolutions (Portugal and Argentina), negotiated settlements (Poland, Bolivia, Nicaragua, and South Africa), or external imposition (Japan and West Germany) (see Przeworski 1991, ch. 1; Huntington 1991, ch. 1; Shapiro 1996, ch. 4). Perhaps there are other possibilities. No matter how democracies come into being, however, it may be that they are more

33. According to Nagel, a comparable solution operates with respect to four seats reserved for New Zealand's Maoris, who are also geographically dispersed (1993, 11).

likely to survive and thrive in some circumstances than others. Here the literature takes three tacks: institutional, economic, and cultural.

The institutional literature grows out of Linz's contention (1978, 1994) that parliamentary systems are more stable than presidential ones. He argued that presidential systems tend toward polarization both in the political culture and between presidents and congress, which they lack the institutional mechanisms to alleviate. Parliamentary systems, by contrast, were said to be more stable and better able to deal with leadership crises. Linz's view has been challenged by Shugart and Carey, who differentiate among more and less stable presidentialisms (1992, ch. 3), and Mainwaring and Shugart, who suggest that weak or "reactive" presidential systems, such as that in the United States, can be as stable as parliamentary ones (1997a, 12–55). Subsequent scholarship suggests that the arrangements that matter most may have less to do with whether or not presidentialism and more to do with other institutional features. For instance, a substantial presence of the presidential party in the assembly, favorable conditions for coalition politics, and centralized executive authority in the government may contribute more to stability than parliamentary institutional arrangements. This may account, in Latin America for example, for the differences between the more stable and governable countries like Argentina, Chile, Colombia, and Uruguay and less stable ones such as Ecuador, Peru, and contemporary Venezuela (see Foweraker 1998, 665–70; Cheibub and Limongi 2000).

The state of the art in the economic literature is Przeworski et al. 2000, ch. 2, which explores the impact of economic development on the stability of democratic regimes. The authors find that although economic development does not predict the installation of democracy, there is a strong relationship between economic development (in particular the level of per capita income) and the survival of democratic regimes. Democracies appear never to die in wealthy countries, whereas democracies in poor countries are fragile, exceedingly so when per capita incomes fall below \$2,000 (1975 dollars). When per capita income falls below this threshold, democracies have a one in ten chance of collapsing within a year. Between per capita incomes of \$2,001 and \$5,000 this ratio falls to one in sixteen. Above \$6,055 annual per capita income, democracies, once established, appear to last indefinitely. Moreover, poor democracies are more likely to survive when governments succeed in generating development and avoiding economic crises (Przeworski et al. 2000, 106–17).

The cultural literature is more difficult to pin down. In some countries governments stage coups rather than give up power when they are voted out of office, yet no defeated U.S. president contemplates sending the tanks down Pennsylvania Avenue. Pressed for an explanation, many appeal to beliefs, ideology, or some other cultural variable. Intuitively plausible as this might be, it is hard to study systematically, partly because people mean

so many different things by culture and partly because cultural explanations tend to be residual explanations.

Huntington contends that commitment to democratic values on the part of political elites is necessary for democracy to endure (1991, 36–7). This plausible conjecture may help explain the (otherwise puzzling) endurance of Indian democracy against the odds. Indian elites were often trained in Oxford and Cambridge during the colonial period, and may have imbibed commitments to democracy from the English. It was not true, by contrast, of African political elites; this perhaps has something to do with why democracies did not generally survive in British ex-colonies there. Perhaps institutional variables account for the difference, however. The British engaged in direct rule in India, whereas indirect rule through local surrogates was the African norm (see Mamdani 1996). As the successful installation of democracy in Japan and the Federal Republic of Germany after World War II might suggest, democracy can be imposed on countries where it has no successful track record so long as there is direct control until democratic institutions take root. Detracting from this account and again suggesting the importance of culture and beliefs is the U.S. example, where democracy survived despite British reliance on indirect rule. Institutional, cultural, economic, and other variables probably all play their parts. Unfortunately, the available data does not lend itself to the kind of large-*n* multivariate analysis which would be required to get a systematic grip on their relative importance.

As a conceptual matter, commentators in the rational choice tradition have contended that worrying about the democratic commitments of citizens is either unnecessary or pointless in a democracy. For instance, Przeworski defines a democracy as a system of spontaneous or self-reinforcing compliance that operates successfully only when self-interested players who fail to get their way calculate that it is to their advantage to accept defeat and wait for the next chance to prevail within the rules, rather than destroy the system. When the system works, normative commitments to democracy, while sometimes present, are “not necessary to generate compliance with democratic outcomes” (1991, 19–34). The strategic calculation, by anyone who has the power to destroy the system that it is in their interest not to do so is sufficient, and likely necessary as well, for the system to survive. Otherwise the “commitment problem,” as game theorists since Thomas Schelling (1960) have labeled it, cannot be solved. If Przeworski is right, trying to induce normative commitments to democracy in elites is a waste of time. In the circumstances where they are needed to prevent breakdown they will probably not produce that result, and, where breakdown does not threaten, they are redundant. Just as incentives matter more than constraints in this scheme, they also matter more than culture and beliefs.

Przeworski himself notes the existence of a counterexample to his discussion of necessity, however, and there appear to be other instances where

groups whose instrumental interests are harmed by democratic processes have nonetheless supported them (see Shapiro 1996, ch. 4). In subsequent work, Przeworski has acknowledged that to date there has not been a theoretical solution to the commitment problem that relies exclusively on self-interested spontaneous compliance (1999, 25–31). As an empirical matter, this logic would leave unexplained the compliance of many political losers who have no reasonable prospect of winning in the future. Jimmy Carter and George Bush are two recent U.S. illustrations, but this practice goes back two centuries in the United States to John Adams's acquiescence in his defeat by Jefferson and the Republican Party in 1800.<sup>34</sup>

It is, in any case, too simple to say that a certain structure of preferences (such as one that the pluralists referred to as a system of crosscutting cleavages) will lead to self-sustaining democratic institutions while others will not. Preferences are not primordial givens; they are shaped, partly by education and acculturation and partly in response to institutional arrangements as we have seen. As a result, it seems wise to try to structure things so that people will reflect on their goals from the standpoint of the reasonable demands of others and be prepared to modify the ways in which they pursue them so as not to undermine democracy. This means that losers must come to accept the legitimacy of present defeats and sometimes even try to play constructive roles in implementing policies they oppose, while winners should appreciate the wisdom of not exploiting every dimension of their present strategic advantage. They should see the wisdom of tolerating—even valuing—continuing opposition, even if this limits the degree to which their goals can be maximized in a given situation. In short, it is prudent to assume that if democracy is to survive, people will have to be persuaded to value it for more than its short-term instrumental benefits.<sup>35</sup>

Another strand of scholarship on the role of culture in democratic stability focuses on mass rather than elite beliefs. Some research suggests that mass beliefs about democracy may play a role in its durability, but the effect does not seem to be strong and it operates in conjunction with numerous other variables.<sup>36</sup> There is also literature, centered on Putnam (1993a,

34. Hardin describes Adams's acceptance of defeat as "perhaps the most important single action by anyone under the U.S. Constitution in its first decades . . . that made the nascent American democracy meaningful in a way that must be at the core of any sensible definition of democracy" (1999, 136).

35. This suggests that Wollheim's paradox (which turns on the possibility of tension between what an individual wants and how he ought to view that preference in the event that he does not prevail through procedures of democratic decision that he accepts as legitimate [1962, 77–87]) should be thought of more as a problem of political socialization than as a philosophical paradox.

36. An empirical study suggesting that no single variable is decisive is Berg-Schlosser and De Meur 1994, 253–80. For an analysis suggesting that some aspects of political culture matter more than others for sustaining democracy, see Muller and Seligson 1994, 635–52.

1993b, 2000), which attends to mass political culture in a different way. Here the suggestion is not that mass beliefs *about democracy* are important but rather that it is membership and, above all, trust in local associations that makes democracy durable over the long haul. Putnam's thesis grew out of a study of Italy in which he argued that effective government and institutional success were contingent on the vitality of the civic community. Putnam found that those regions of Italy that had an ongoing tradition of civic engagement also had a higher level of institutional success than those regions without civic participation, despite exhibiting identical institutional structures.

For civic engagement to flourish, community members have to trust in the reciprocity of those around them and have the ability and resources to utilize social networks. Putnam deployed the term *generalized reciprocity* to connote a social understanding that one's efforts to participate and protect the common good will be reciprocated by others, known or unknown. However, Putnam distinguished two different types of networks: horizontal, the organization of individuals of equal status and resources, and vertical, the organization that brings together those of unequal status in relations of dependence or hierarchy. It is horizontal networks, which usually emerge from community participation, that he held to be critical in generating the social capital needed for institutional success. Indeed, vertical networks, for example, the Catholic Church, feudal landholdings, and clientalism, cannot cultivate the social trust he deemed essential, because the inferior and superior will experience different outcomes from the same moment of cooperation (Putnam 1993a, 173–75). On Putnam's account, horizontal civic participation, like an upward spiral, brings about the greater trust, networks, and norms that make generalized reciprocity, and hence institutional success, possible.

In a like spirit, Putnam (2000) argues that in the contemporary United States the erosion of local community participation undermines democratic participation and with it stability and governability. In the first 65 years of the twentieth century, participation in political groups, formal social clubs, and informal clubs like bowling leagues or bridge clubs was steadily increasing. After the mid-1960s, however, it began to decline. Putnam charts and then seeks to explain this waning tradition of community socializing and political participation, blaming it on numerous factors ranging from suburbanization to the mass media, particularly television, as well as demographic changes: as the older and involved generation dies out, the younger generations of baby boomers and generation Xers do not fill in the ranks in social, political, or philanthropic organizations. Nor are they interested in the informal social networks enjoyed by the older generation. The result, on Putnam's telling, is atrophy of the social networks that support the generalized reciprocity that is essential to effective democratic institutions.

Suggestive as this might be, Putnam's argument has drawn heavy critical fire, both from historians of Italy and students of contemporary U.S.

democracy (see Goldberg 1996; Sabetti 1996; Levi 1996; Gobetti 1996; Ladd 1999, 25–119). As a theoretical matter, it is difficult to see why strong local attachments and trust within local civic groups should be expected to translate into trust of democratic political institutions. Rousseau argued long ago allegiance to “sectional societies” is more likely to undermine than reinforce commitment to the collective good (1968 [1762] 150ff). Levi makes a similar point in relation to Putnam: we might more plausibly expect intensive trust in local civic associations to breed distrust of government rather than trust, as it does among militia groups for example (1996, 45–56). A possible line of response might be that it is large publicly committed civic institutions that are important for democracy, but it is hard to differentiate the Boy Scouts from the Hitler Youth on that count. At present, as with the literature on the allegedly deleterious effects of “divided societies,” it is difficult to see a compelling case, conceptual or empirical, that low levels of civic trust are subversive of democracy.

## ■ | Supplementing Schumpeterianism

Although few today would endorse the idea of single party democracy, many democratic theorists are uncomfortable with the reduction of democratic politics to the procedures of competitive pluralism. Sometimes this is motivated by the recognition, since Arrow (1951), that there are no perfect procedural decision rules. Sometimes it flows from the view that democracy involves multiple commitments that can conflict. The question then becomes: what additional necessary conditions should be required? Here the challenge revolves around the reality that a substantive standard seems needed to decide when procedures have failed or relevant conflicts have occurred and to determine what would count as an adequate supplement or remedy. Yet there are as many theories of substantive democracy as there are substantive democratic theorists, and it seems unlikely that any one will win the philosophical high ground.<sup>37</sup> This suggests either an arbitrary choice among them, or the resort once more to procedures, and so a chicken-and-egg problem.

Some have responded to this conundrum by trying to work out a middle ground between procedural and substantive views, developing quasi-substantive constraints on democratic procedures. For instance, Burt (1992) contends that democracy involves foundational commitments to both majority rule and nondomination, with the ever-present potential for conflict when majorities make decisions that lead to domination. Judicial review is warranted when such conflicts arise, he argues, and courts should not conduct it in ways that assume they know how the conflict should be resolved.

37. See Ely 1980, Beitz 1988, and Habermas 1994 for examples. For further discussion of this point, see Shapiro 1996, ch. 2.

Rather they should declare that the domination that has emerged from the democratic process is unacceptable and insist that the parties try anew to find an accommodation. Thus in contrast to what many have seen as the altogether too timid approach taken by the U.S. Supreme Court in the school desegregation cases of the 1950s and after, on Burt's view the Court took the right stand. In *Brown v. Board of Education* (347 U.S. 483 [1954]) the justices declared the doctrine of separate but equal to be an unconstitutional violation of equal protection, but they did not describe schooling conditions that would be acceptable. Rather, they turned the problem back to southern state legislatures, requiring them to fashion acceptable remedies themselves.<sup>38</sup> These remedies came before the court as a result of subsequent litigation, were evaluated when they did, and were often found to be wanting (Burt 1992, 271–310). But the Court avoided designing the remedy itself, and thus the charge that it was usurping the legislative function.

Ruth Ginsburg has made a powerful case that when courts move beyond a reactive role, they undermine their legitimacy in a democracy. Although she thinks that it is sometimes necessary for the court to step ahead of the political process to achieve reforms that the Constitution requires, if it gets too far ahead it can produce a backlash and provoke charges that it is overreaching its appropriate place in a democratic constitutional order (1993, 30–38).<sup>39</sup> She and Burt both think that the sort of approach adopted by Justice Harry Blackmun in *Roe v. Wade* (410 U.S. 113 [1973]) exemplifies this danger. In contrast to the *Brown* approach, in *Roe* the Court did a good deal more than strike down a Texas abortion statute. The majority opinion laid out a detailed test to determine the conditions under which any abortion statute could be expected to pass muster. In effect, Justice Blackmun authored a federal abortion statute of his own. As Ginsburg put it, the Court “invited no dialogue with legislators. Instead, it seemed entirely to remove the ball from the legislators’ court” by wiping out virtually every form of abortion regulation then in existence (1993, 32).

On the Ginsburg-Burt view, the sweeping holding in *Roe* diminished the Court's democratic legitimacy at the same time as it polarized opinion about abortion and put paid to various schemes to liberalize abortion laws that were underway in different states. Between 1967 and 1973 statutes were passed in nineteen states liberalizing the permissible grounds for abortion. Many feminists had been dissatisfied with the pace and extent of this reform. This is why they mounted the campaign that resulted in *Roe*. Burt concedes that in 1973 it was “not clear whether the recently enacted state laws signified the beginning of a national trend toward abolishing all abortion restrictions or even whether in the so-called liberalized states, the

38. *Brown v. Board of Education II* 349 U.S. 294 (1955).

39. See also “Nomination of Ruth Bader Ginsburg to be an Associate Justice of the United States Supreme Court: Report Together with Additional Views,” Executive Report, 103-6-93-1, U.S. Senate 1993.

new enactments would significantly increase access to abortion for anyone." Nonetheless, he points out that "the abortion issue was openly, avidly, controverted in a substantial number of public forums, and unlike the regimen extant as recently as 1967, it was no longer clear who was winning the battle" (Burt 1992, 348). Following the *Brown* model, the Court might have struck down the Texas abortion statute in *Roe* and remanded the matter for further action in the Texas state legislature, thereby setting limits on what legislatures might do in the matter of regulating abortion without involving the Court directly in designing that regulation. This would have left space for democratic resolution of the conflict, ensuring the survival of the right to abortion while at the same time preserving the legitimacy of the Court's role in a democracy (Burt 1992, 349–52).<sup>40</sup>

A comparable view is developed by Shapiro (1999), who argues for suspicion of hierarchical outcomes on the grounds that they limit the capacity for effective opposition which democracy requires. Hierarchies are often legitimate, and when they are created in democratic ways, they have a particularly strong claim on our allegiance. Yet hierarchies have propensities to atrophy into systems of domination, necessitating institutional constraints that shift burdens of proof to those who would defend them. Sometimes the appropriate policing mechanisms are courts, conceived on the Ginsburg-Burt model, sometimes they are other second-guessing institutions. Whatever the mechanism, the goal is to give participants in a democratic order incentives to find ways to pursue their goals that maximize inclusive participation in decision making at the same time as they minimize domination. As with the Ginsburg-Burt view, there is no presumption that a third-party institutional designer knows what the right answer should be. Rather, the thought is to try to structure things so that the players themselves will have incentives to discover, even invent, ways to avoid perverse consequences of democratic procedures and to minimize the tensions between democracy and the pursuit of other goods.

Some will find this too minimal a role for courts in protecting important freedoms, plumping instead for a liberal constitutionalism that constrains democracy rather than this democratic constitutionalism that seeks only to make it operate more effectively (see Rawls 1971; Ackerman 1980; Dworkin 1986). At least since Tocqueville's time it has been common to

40. The Ginsburg-Burt approach was eventually adopted by the Supreme Court in *Planned Parenthood of Pennsylvania v. Casey*, 112 S.Ct. 2791 (1992). By affirming the existence of a woman's fundamental constitutional right to an abortion, recognizing the legitimacy of the state's interest in potential life, and insisting that states may not pursue the vindication of that interest in a manner that is unduly burdensome to women, the Court set some basic parameters within which legislatures must fashion abortion regulations. This approach was reaffirmed in *Stenberg v. Carhart*, 120 S.Ct. 2597 (2000), when the Court held that a Nebraska statute outlawing partial birth abortions imposes an undue burden on women. These issues are taken up further in the editor's introduction to Shapiro 2001.

worry that democracy is a threat to political freedom. But, as Dahl (2002, ch. 4) has recently reminded us, Tocqueville was wrong. In the century and a half since he wrote, political freedoms have turned out to be safer in democracies than in nondemocracies by a considerable margin. Almost half a century earlier Dahl had registered skepticism that, even within democracies, constitutional courts could be shown to have a positive effect on the degree to which individual freedoms are respected (1956, 105–12). Subsequent scholarship has shown his skepticism to be well founded (see Dahl 1989, 188–92; Tushnet 1999; Hirschl 1999). Indeed, there are reasons for thinking that the popularity of independent courts in new democracies may have more in common with the popularity of independent banks than with the protection of individual freedoms. They can operate as devices to signal foreign investors and international economic institutions that the capacity of elected officials to engage in redistributive policies or interfere with property rights will be limited. That is, they may be devices for limiting domestic political opposition to unpopular policies by taking them off the table (Hirschl 2000).

From a different quarter it might be objected that the middle-ground views I have surveyed are in fact implicitly substantive. If courts or other second-guessing agencies intervene in the results of democratic procedures declaring them to be unacceptable on democratic grounds, there must be a theory of substantive democracy, however implicit, by reference to which such judgments are rendered. Embracing this conclusion involves too quick a dismissal of the potential for middle-ground views. Their motivating intuition is that people can reasonably find things unacceptable even when they cannot articulate an account of what would be acceptable. During the 1970s in South Africa many people had no doubt that apartheid violated essential principles of democratic governance, but few—if any—of them could have spelled out a consistent theory of democratic representation or even said how they would resolve the various conundrums about representation that have arisen in the post-apartheid South Africa. They were against domination in Machiavelli's sense mentioned in the first section, even if they could not have explained what they favored. This is dramatic but not atypical; in many ways human beings are reactive adaptive creatures. They reject what is unacceptable and shy away from what fails, assuming that it must be possible to come up with something better. Often this is more of a regulative ideal than an implicit theory, and sometimes the hope will prove vain. But not always. The middle-ground views rest on the supposition that often enough human ingenuity can rise to the challenge thrown up by the failures of democratic procedures and that loading the dice to facilitate that outcome is a better democratic solution than the going alternatives.<sup>41</sup>

41. A different variant of the middle-ground views is put forward by Pettit (1997, 2000).

## ■ | Concluding Comments

It is conventional, though partly misleading, to distinguish power management views of democratic purposes from those geared to the search for a common good. Aggregative and deliberative views of the common good run into serious difficulties as we saw in the first section, where I argued that the alternative focus on democracy as a means for legitimate management of power relations offers distinct advantages. This does, however, rest on a conception of the common good, if a comparatively thin or subordinate one. I paraphrased Machiavelli by suggesting that it embodies what those with an interest in avoiding domination share in common. It requires rights both of inclusive participation and of loyal opposition: freedom to participate in decisions that affect you and to oppose and try to change outcomes you reject by working through the system.

My discussion of the best means for securing democratic rights of participation and opposition was organized around an examination of the critics and defenders of Schumpeterian democracy in the second section. There I argued that the Schumpeterian conception is flawed for two reasons: democratic polities are often not competitive in ways that the model assumes and Schumpeterian's exclusive focus on governmental institutions is unsatisfying given the ubiquity of power relations in other realms of social life. Powerful as these criticisms are, I suggested that the appropriate response is not to reject the Schumpeterian ideal outright but rather to find ways of making politics more genuinely competitive and of extending democratic constraints to nongovernmentalist arenas of collective action. The dangers of rejecting the idea of political competition were seen in my discussion of consociationalism: it can lead to a self-fulfilling prophecy that democracy is impossible or to an embrace of the implausible proposition that institutionalized consensus is an appropriate vehicle for democracy. I did not discuss the extension of democracy into nongovernmental domains of social life, a subject I have explored extensively elsewhere (see Shapiro 1999).

If there is no good reason to assume democracy is impossible in some parts of the world, this does not mean it is possible in all circumstances, as I noted. Entrenched antipathies might be difficult to overcome in the relevant ways, dictators might have vicelike grips on power, or other factors might make democratic innovation unlikely. Yet transitions to democracy can occur in improbable settings (most recently Nigeria in 1999–2000), and, when they do occur, this can be by one of a several possible routes. More consequential is whether democracy, no matter how instituted, is likely to survive. On this there is surprisingly little accumulated knowledge in political science. The absence of severe poverty and the presence of economic growth seem to help, but the literatures on which institutional and cultural factors are more or less conducive to democratic political stability remain very much works in progress.

In the last section I turned to the critique of Schumpeterianism that, as a purely procedural view, it can generate perverse results. Obvious instances are when majorities vote to disenfranchise minorities or otherwise render their opposition ineffective. Responding to the weakness of pure proceduralism with theories of substantive democracy creates difficulties of its own, the most striking's being the need to choose among them without resort once more to proceduralism. However, I suggested that a fertile area of democratic theory is being developed by a group of quasi-substantive theorists who are attending to institutional devices geared to limiting democracy's propensity to produce outcomes at variance with its constitutive ethos. I focused particularly on their accounts of the appropriate role for constitutional courts. Unlike liberal constitutionalists, for whom such courts exist to protect liberal values from the vicissitudes of democratic politics, theorists in this group argue that we should limit courts to protecting democracy from itself. Intervention is legitimate only when the democratic process threatens to break down or when acting on the democratic imperative undermines another.<sup>42</sup> Even then, on these views, if courts are to build and maintain their legitimacy in democracies, they should always intervene in democracy-sustaining ways.

42. One example that suggests itself from the discussion of oligopolistic competition and collusion in the second section would be for U.S. courts to entertain public interest antitrust actions against political parties.

## *Justice*

What is the present state of discussion in legal and political theory so far as the topic of justice is concerned? By *justice*, I mean social or distributive justice: the evaluation of social policy and institutional arrangements on the basis of the way they affect the distribution of scarce resources and opportunities among individuals in society. It will be quite important to my argument in this chapter that the term *justice* does not take in the whole of liberal political philosophy. One point that I shall emphasize is that recent theoretical discussions ostensibly about justice have tended to drift away from that subject and become meditations on political morality generally, or public reason in a multicultural society. This has left a significant gap in our philosophical discussion of issues about wealth, poverty, and distribution, a gap that economic theorists who are relatively uninterested in distributive issues have not been slow to fill.

If political philosophy in the 1970s and early 1980s was dominated by a fierce and focused debate about social justice occasioned by the publication of John Rawls's first book (1971), the past ten years or so have seen a blurring of that focus under the influence of Rawls's later work (1980, 1985, 1996), and a diversion of the fierce energies that *A Theory of Justice* aroused into a safer and less consequential discussion of the nature of political argument in a multicultural society.

Social justice is about equality, property, need, desert, and opportunity. It is about structures that govern people's access in society to the main sources of well-being: wealth, income, employment, education, enterprise, health care, and so on. The debate inspired by *A Theory of Justice* raged around such propositions as:

Social and economic inequalities are to be arranged so that they are . . . to the greatest benefit of the least advantaged. (Rawls 1971, 83)

[U]ndeserved inequalities call for redress; and since inequalities of birth and natural endowment are undeserved, these inequalities are somehow to be compensated for. (Rawls 1971, 100)

[T]he difference principle represents, in effect, an agreement to regard the distribution of natural talents as a common asset. (Rawls 1971, 101)

From Rawls's critics, these positions elicited equally robust responses: "Taxation of earnings from labor is on a par with forced labor" (Nozick 1974, 169), "[D]istributionist theories cannot be theories of justice" (Mack 1976, 145), and "'Social justice' can be given a meaning only in a directed or 'command' economy" (Hayek 1976, 62). Great progress was made in the analytic understanding of these and related issues. The role of maximin, the logic of historical entitlement, the relation between desert and respect for persons, the currency of equality (resources, well-being, capabilities), the argument for Rawls's "difference principle" over radical equality claims (on the one hand) and over claims of need (on the other), the relation between justice and liberty—these were just a few of the topics whose discussion flourished in the decade or so following the appearance of Rawls's magisterial book. Some of that discussion was focused on particular questions of policy—the defense (or critique) of welfare rights, for example, or redistributive taxation or the discussion of affirmative action.

From a theoretical point of view, however, the most encouraging feature of the discussion initiated by Rawls was the elaboration of various model-theoretic devices—the original position, the veil of ignorance, the notion of lexical priority (as opposed to a priority expressed through the metaphor of weight), and the distinctions between pure, perfect, and imperfect procedural justice—not to mention the revival and more rigorous use of existing theoretical constructs like the difference between classical and average utilitarianism, the distinction between the right and the good, and the idea of a Lockean proviso. All of this meant that when philosophers of justice turned their attention to issues of public policy, they were able, in some cases at least, to offer a distinctive contribution qua philosophers. They did not simply convert the new normative spirit in political theory into an opportunity for political advocacy.

That's where we were in the 1970s and 1980s. And some of that discussion persists: certainly, the theoretical apparatus that was introduced in Rawls's conception (and in the debates surrounding it) has become a permanent part of the theoretical landscape. It is not beyond question; nothing in our theoretical landscape, however venerable, is beyond question. But as far as one can tell, this model-theoretic apparatus is the most enduring part of Rawls's legacy, even if the particular claims that Rawls embodied in that framework are opposed or rejected by many theorists.<sup>1</sup>

Something happened, however, in the late 1980s to turn the mainstream of liberal discussion away from the hard detail of issues about

1. Indeed theoretical interest in the use of these tools may have been heightened by the recent publication of Rawls's article-length writings (Rawls 1999a) and of a restatement (Rawls 2001) of the main themes of Rawls 1971.

justice. John Rawls (especially in Rawls 1996) continued to contribute powerful model-theoretic ideas—overlapping consensus, for example, and the burdens of judgment—but the debate they evoked was no longer oriented specifically to issues of social justice. Beginning with his John Dewey Lectures at Columbia, Rawls began paying particular attention to the issue of how one might argue politically in a society whose members held disparate and opposed ethical, philosophic, and religious conceptions.<sup>2</sup> Indeed, he seemed to be rejecting a universalist approach in favor of one that was connected with the particular difficulties faced by our own society:

[W]e are not trying to find a conception of justice suitable for all societies regardless of their particular social or historical circumstance. . . . We look to ourselves and to our future and reflect upon our disputes since, let's say, the Declaration of Independence. How far the conclusions we reach are of interest in a wider context is a separate question. (1980, 518–19)

This makes it sound as though a theory of justice is expected to capture only the particularity of U.S. thought and practice. It resonated with a theme emphasized by Walzer (1983): that a well-ordered society is a society true to its own understandings; or, if it is to be reproached as unjust, it is to be reproached as having fallen away from values that already have a purchase in the life and practice of its members. Critics quickly established of course that the Walzerian position would work only if one could identify and privilege a set of understandings relevant to justice that could be construed definitely as “ours” for the purpose of this sort of reflection on what it would mean for “our” society to be well ordered. And arguably that condition is not given (see Dworkin 1985a).

A well-ordered society is defined in Rawlsian theory as one in which citizens can justify their shared arrangements to one another (Rawls 1971, 453 ff.). But most people live in multicultural societies whose members hold a variety of beliefs about the building blocks of social justification—things like autonomy, God, human nature, relations between the sexes, the importance of reason, and the meaning of life. So what if a pious Muslim asks a secular materialist to justify (say) the toleration of pornography? What vocabulary should they use in the ensuing discussion? Or what if an economist asks a Catholic theologian to justify anti-euthanasia laws? What sort of dialogue or conversation would certify a society like this as well ordered? It is no good saying, “Well, put them behind the veil of ignorance, and see what they come up with.” The veil of ignorance is itself a way of modeling ideas about fairness, and the problem posed by pluralism is that

2. This had been raised also in earlier discussions about liberal neutrality in Dworkin 1978 and in Ackerman 1980.

fairness may be understood quite differently (or may not figure prominently at all) in various traditions.

In his second book, *Political Liberalism*, Rawls (1996) insisted that public justifications in a well-ordered society must in some sense stand above or apart from the religious, cultural, and philosophical issues that divide the citizens. A person does not show another the requisite respect if he responds to requests for justification in terms that he knows the other cannot accept. So a Left liberal like me may not say, for example, to a Social Darwinian that even the feeblest person is entitled to our compassion because he is created in the image of God. I must find some way of putting my point about equality that can be affirmed even by people who do not share my religious convictions. The principles of justice, the assumptions (like basic equality) that they rest on, and the model-theoretic conceptions (such as the original position) that we use to elaborate them—all these must be made intelligible to and must be capable of commanding the allegiance of people whose deepest convictions are not reflected in their own terms in the content of social justification.

I do not intend to examine in detail the ideas like overlapping consensus, public reason, and political liberalism, which Rawls uses to answer this challenge. This is not because they are unimportant; it is rather because they are not specific to the issue of justice (which is what I am commissioned to study in this chapter). Since 1980 much of what Rawls has written and much of what is most hotly debated in his work, though presented under the label “justice,” is in fact general political philosophy—dealing either with general metatheoretical questions about objectivity, universalism, discourse ethics, and the need for shared frameworks or—when he deigns to descend to first-level concerns—with issues about toleration, constitutionalism, democracy, and civil rights, rather than with issues specific to social justice (the issues that dominated the first wave of discussion generated by Rawls 1971) such as distribution, desert, needs, opportunities, and markets, and the rival claims of equality, fairness, and economic efficiency. Of course the detailed elaboration of Rawls’s political liberalism is not irrelevant to justice (and I shall say something in a moment about its specific applicability in that regard); but most of his discussion since 1980 has proceeded on a much more general front.<sup>3</sup>

Something similar may be said, too, about the debate between liberals and communitarians that was sparked by Michael Sandel’s critique (1982) of Rawls’s book. Though Sandel’s work was initially focused quite closely on the detail of Rawls 1971, it did raise much more general issues about social and ethical ontology, issues whose discussion has taken us into fertile and fascinating territory—but territory miles away from the site of contesta-

3. This is true also of discussions between Rawls and Jurgen Habermas, and their respective disciples (see Habermas 1998a; Rawls 1995).

tion about social justice, where the claims of the Rawlsian liberals and the Sandelian communitarians were initially staked.

This tendency of discussions of justice to open up into much broader philosophical discussions has been well known since Aristotle's time. Not only is "justice . . . often thought to be the greatest of virtues," said Aristotle, but "proverbially 'in justice is every virtue comprehended'"—or, at least, every virtue that relates to one's dealings with other people. Still, Aristotle said, it is also worth "investigating . . . the justice which is a *part* of virtue," that is specifically those fairness-related virtues which have to do with the regulation of "graspingness," the regulation of people's tendency to grab what is more than their share under various conditions and circumstances (1954, 106–12).

Admittedly, it was an important thesis of Rawls's first book that social justice, even in the narrower of the two senses that Aristotle distinguished, is to be understood in relation to the impact on individuals of the whole basic structure of social institutions taken as a single scheme, so that one cannot reach a verdict about the justice of (say) the taxation system, without considering its relation to the welfare system, the education system, and the system of liberties that enables individuals to make something of the resources and opportunities assigned to them (1971, 7). That meant that even a discussion dedicated specifically to social justice would have to consider issues like the nature and priority of liberty and the relation between justice and the constitution. And so reflecting on how to argue about justice would involve reflecting on how to argue about these topics as well.

Even so, it does not follow that social justice is merely a matter of reflecting on the way to argue about basic liberties. Even under Rawls 1996, a discussion of social justice should still involve an attempt to grapple with detailed issues about economy, equality, efficiency, opportunity and fairness under the specific constraints of political liberalism. And, in my view, *that* is what people working within the present Rawlsian paradigm seem to be neglecting: they are much more interested in discussing the constraints of political liberalism itself—and the troubling issues that are most immediately raised as we attempt to elaborate those constraints (e.g., issues about religious freedom, cultural accommodation, and so on)—than in pursuing the discussion of social justice under those auspices.

I don't underestimate the difficulty of dealing with social justice issues within the framework of political liberalism. These are not issues that can be dealt with by a strategy of vagueness or evasion—putting about a set of anodyne formulas that mean all things to all people. A theory of social justice has hard, critical work to do, on Rawls's original account: it has to settle complex questions about freedom, equality, and opportunity, and it has to hold its own against rival conceptions (against Nozick 1974, for example, or against utilitarian or efficiency-based approaches). If there is a fault in Rawls's later work, it is a tendency to lose sight of the difficulty of the so-

cial and economic argumentation in *A Theory of Justice*. The actual examples of overlapping consensus for a pluralist society provided in *Political Liberalism* are laughably easy by comparison. Both Kantians and non-Kantians might favor democracy, Rawls says; and both Christians and secularists may well oppose slavery (Rawls 1996, 122–25). The hard part comes when we try to establish an overlapping consensus among (say) Christian fundamentalists, Hindus, secular humanists, scientific determinists, and members of the dot-com generation on the definition of *equal opportunity*, the use of economic incentives, and the distinction between liberty and the worth of liberty.

Just to give a taste of the difficulty, consider the problem of the relevance of desert to basic social entitlement. This was central in social justice discussions in the 1970s and 1980s, and various approaches to it informed people's views about market success, the problem of the undeserving poor, and so on. Now, it was not hard to see that insistence on a strong theory of desert might mean that a theory of justice would have to buy into social and religious controversies about virtue (cf. Dworkin 1978, 138; Hayek 1976, 73 ff.). But it was much more difficult to know what to *do* with that point or what would be a fair or a neutral way to move on from it. Does one simply reject desert in this sphere (and the whole view of the person that goes with desert), or does one try to develop a thin theory of desert or to modify the assumptions, for example, about freedom and background responsibility for character, that deserving is sometimes thought to presuppose? Can we imagine an overlapping consensus on problems like *that* between (say) the Protestant work ethic, the notion of apostolic poverty, and ideas of the fundamental solidarity of community?

It is easy to despair of answering questions like this under the conditions that Rawls's later work has emphasized. It would be a pity, however, if Rawls's followers were to abandon his most challenging theorems on social justice simply because they do not fit the template of "public reason" which he has now made central to his theory (Rawls 1993, 212 ff.). Certainly, it would be a shame to do so without considering what becomes of the primacy of justice in a liberal theory if we are forced to admit that some liberal values (like toleration and basic liberties) can be elaborated in overlapping consensus while others (like economic equality) cannot.

I should add that issues about desert and social justice have not been wholly neglected in the most recent discussion. In a recent book, David Miller has argued (against the claims made in Rawls 1971) that we do possess "a coherent concept of desert that is sufficiently independent of our existing institutions for it to serve as a critical weapon in the armory of social justice," and Miller insists that theories of social justice would lose touch with popular opinion if they pretended to ignore it (1999, 155). Samuel Scheffler also observes that liberal political philosophy has often been viewed as carelessly repudiating ordinary beliefs about the responsibility of the individual for his or her own actions in relation to social justice (1992,

299; cf. Rawls 1971, 103–4). “The widespread reluctance among political philosophers to defend a robust notion of preinstitutional desert” is traceable, Scheffler suggests, to the assumption of a naturalistic worldview, which alone seems (to liberals, certainly not to their opponents) to stand above the ferment of religious and cultural controversy between various worldviews in a pluralistic society (1992, 309). It seems we cannot take desert and responsibility as seriously as ordinary intuitions require without abandoning this stance of philosophical neutrality. Scheffler offers no comfortable solution to this difficulty, but his willingness to raise it represents an important mapping of the concerns of political liberalism onto issues specific to social justice.

While Miller and Scheffler confront questions about the neutrality of the terms in which society evaluates distributions, other theorists have asked about the terms in which individuals state their own preferences and conceive of their own interests, so far as issues of justice are concerned. The work of Amartya Sen (1982) and Ronald Dworkin (1981a, 1981b, 2000) on equality invites us to consider what it would be to treat as equals, for the purposes of justice, persons with quite different sorts of aims and aspirations for their lives. One person may be willing to put up with a great deal of pain for the sake of some athletic achievement; another may be an aesthete with exquisitely expensive tastes. Is there a neutral metric of justice or equality under whose auspices we can define a fair distribution of resources to these individuals?

Equality of utility seems unfair to the athlete, who values something other than happiness and freedom from pain; but equality of wealth or income seems unfair to the person with expensive tastes, who may get much less satisfaction than others from a given stock of resources. Again, one individual may prefer a life of leisure, another a prudent and productive life. Or one individual prefers working communally with others, while another prefers a small holding of his own. Is there a fair basis on which we can say what all these people are entitled to *ab initio* (let alone in the context of an ongoing scheme of social and economic cooperation)? Dworkin has developed an interesting set of model theoretic devices—an initial auction of resources, packaged in different ways, and a hypothetical insurance market to deal with issues of choice and affliction—as a basis for thinking through such questions under fairly challenging assumptions about the incommensurability of individuals’ aims and circumstances (1981a).<sup>4</sup>

Philippe Van Parijs (1995) has attempted to do something similar for

4. In the preface to Dworkin 2000, there is an indication that Dworkin intends to develop the philosophical basis of his 1981 and 1981a more systematically in a book. But as it stands, Dworkin 2000 is not such a systematic exposition. In the meantime, Rakowski 1991 is the major book-length elaboration of a position like Dworkin’s.

our discussion of freedom; his book *Real Freedom for All* argues that a free society is required to offer a basic income to everyone, irrespective of what they propose to do with it or how they intend to live. Of course, no society does this or does it directly, and most people's sustenance is not predicated on any such starting point. Nevertheless Van Parijs argues that the distinctions we do draw between individuals for the purposes of assessing justice and entitlements—the distinction for example between the idle and the productive—have no legitimacy unless they presuppose such an initial distribution or are conditioned by what such a presupposition would distributively entail. Without that condition a claim that the lazy are not entitled to subsistence would have to presuppose the rest of us have the right to pick and choose who we exclude from the resources available in society, and that, Van Parijs (1995) argues, is indefensible.

The works just referred to exhibit exactly the virtues that are called for but not displayed in Rawls's later work. They address problems associated with the diversity and incommensurability of people's comprehensive conceptions of what makes life worth living. But they do so without losing sight of the central issues of justice. By contrast, recent work ostensibly on justice by Brian Barry, which also pays specific attention to the problem of developing a theory in a way that is fair or impartial to rival religious, cultural, and philosophical conceptions, is almost completely schematic so far as the detail of social justice is concerned. Barry has produced two volumes of a projected three (or perhaps four) volume *Treatise on Social Justice*. The first volume (1989b) developed a most valuable contrast between theories of justice that proceed from the premise that justice must be to everyone's advantage and theories of justice that see no need for such a premise. The second volume (1995) was supposed to set out the implications of Barry's own view—a view of the latter type—for the distribution of benefits and burdens in a society (1989b, xiii). It turns out, however, that most of Barry 1995 is devoted to an exploration of what it would mean for a theory of justice to be impartial as between rival conceptions of the good, and that largely schematic discussion is illustrated by examples (e.g., about constitutional guarantees of religious freedom) drawn from outside the specific area of social justice. Let me say again that I don't for a moment doubt the importance of discussions like this (or that of Scanlon 1998, with which Barry has great sympathy). The issue of how to argue and justify normative positions in political philosophy in a pluralistic society is massively important. But though that discussion is a necessary preliminary for theorizing about social justice, it is not itself a substitute for it. And one worries a little when one sees the task of detailed argumentation about justice being indefinitely postponed.

How much of this has to do with changes in political circumstances? There can be no doubt that the changes of the 1980s and 1990s have put

liberal egalitarianism on the defensive. It is less clear, what effect this has had on discussions of justice in academic political philosophy. F. A. Hayek once wrote:

I have come to feel strongly that the greatest service I can still render to my fellow men would be that I could make the speakers and writers among them thoroughly ashamed ever again to employ the term “social justice.” (1976, 97)

But the disconnect between Hayek’s endeavors and mainstream political philosophy was shown by the fact of his laboring under the impression that he and John Rawls were in general agreement on these matters.<sup>5</sup> At any rate, the political success of Hayekian and other New Right ideas certainly has not stifled discussions of social justice (though it may have contributed somewhat to the embarrassed postponement of detailed discussion that I was complaining about at the end of the previous section).

The events of 1989 to 1991 have perhaps made it harder to sustain straight-faced worries about whether liberal justice can be made appealing to the followers of Karl Marx. People don’t spend as much time these days as they used to pretending that there is an important and troubling line of argument (as opposed to a few throwaway lines) in pamphlets like *The Critique of the Gotha Program* (Marx 1977 [1875]) which modern theories of justice need to address, nor do they invest the energy as they used to invest (certainly in the Britain of my acquaintance in the late 1970s and 1980s) in the thankless task of trying to convince Marxists that liberal work on justice should not be dismissed as just another bourgeois trick (cf. Husami 1978; Buchanan 1982).

Perhaps what the new political mood *has* produced is a philosophical environment in which it is easier for philosophers to pose tough questions about the motivation to be just and about the basis of the demand that each of us should submit the prosperity of our families to the demands of social justice. Such questions no longer require us to imagine a Thrasymachus breaking into our discussion from the outside like a wild beast (Plato 1995, 16). Instead we can ask quite calmly, as Thomas Nagel (1991) has asked in an influential book, whether there is not a level of sacrifice that it is unreasonable for the state to impose upon the well-off, for the benefit of the poor, in the interests of justice. Is there not a standpoint, Nagel asks—the standpoint of one’s personal concerns and affections—from which the egalitarian demands of impartial justice can be made to

5. Cf. Hayek 1976, xiii. “Though the first impression of readers may be different, Rawls’s statement which I quote later in this volume (1976, 100) seems to me to show that we agree on what is to me the essential point.” The quoted passage from Rawls to which he refers (and with which he agrees) specifies the design of the basic structure as the important problem of justice. See also the discussion of this in Waldron 1993a, 29–31.

seem unreasonable? And if they do seem unreasonable from the personal point of view of, say, a prosperous middle-class American, isn't that an important fact that moral and political philosophy must reckon with, rather than just a sordid clash between justice and amoral self-interest (in which of course justice *ought* to prevail?) Doesn't it require us to rethink our conceptions of justice from the beginning, and to call in question our initial sense of what justice is entitled to demand?

Of course, the issue of how demanding morality may reasonably be is a hardy perennial (see, e.g., Scheffler 1982): it has flared up in recent years in debates about yuppie ethics (cf. Baron 1987; H. Shue 1988) initiated, for example, by the consequentialist rigorism of Shelly Kagan's book *The Limits of Morality* (1989) and by some provocations published in the popular press by Peter Singer (1999). But its direct application to claims about justice—and the shameless assumption that justice may have to yield to the demands by some of us for a good life, even if such a life is not available to all—does seem a remarkable feature of the recent philosophical debate.

The demandingness of justice is an issue that the Rawlsians had hoped to finesse, by a combination of strategies. First, Rawls had argued in *A Theory of Justice* that the two principles of justice as fairness, applying as they did to the basic structure of society, did not make any particular demands on individuals, except that they accept and support the basic structure of just institutions (1971, 6–10, 73–78, 93–101). Within that structure, individuals were conceived as free to act from self-interested or partial motivations if they wanted to. However, this thesis has been heavily attacked in recent work by G. A. Cohen (1997, 2000). Cohen argues that “justice requires an ethos governing daily choice which goes beyond one of obedience to just rules” (2000, 136), for, as Okin (1989) and others have argued, the basic structure cannot be understood as something independent of what people do in their daily lives. Just institutions cannot therefore be expected to survive in the face of powerful popular sentiment about the unreasonableness of the demands of justice on individual lives.

Rawls had also attempted to finesse the point by presenting his theory of justice as a conception which works in the interests of everyone: “In justice as fairness society is interpreted as a cooperative venture for mutual advantage” (1971, 84) He said that “everyone may expect to improve his situation if all comply with these principles, at least in comparison with what his prospects would be in the absence of any agreement” (1971, 296). Brian Barry has done political philosophy a major service by dissecting and refuting this claim. He shows that Rawls makes no more than a half-hearted effort to argue for a mutual advantage interpretation, that is, to show that there are none who might do better for themselves in a situation where no one observed Rawlsian principles than in a situation where everyone did (1989b, 71–74, 241–54). The most Rawls can show is that the difference principle does not worsen anyone's prospects relative to a baseline

of simple equality. But simple equality is not what we have at present; and Rawls fails to show—he does not even try to show, nor could he show—that the readers of his book (let alone those who are too busy managing their wealth to bother with books about justice) would do as well as they are doing at the moment, if his two principles were to be put into effect. In response to an imagined complaint from a well-endowed person who would have less (than he has at present) under the difference principle, Rawls writes:

It is clear that the well-being of each depends on a scheme of social cooperation without which no one could have a satisfactory life. . . . The difference principle . . . seems to be a fair basis on which those better endowed, or more fortunate in their social circumstances, could expect others to collaborate with them when some workable arrangement is a necessary condition of the good of all. (1971, 103)

But this is just a gesture. It is not argued for; Barry shows that there is no basis in Rawls's model for arguing for it; and the logic of the original position is against it, inasmuch as that conception requires each party to bargain without any awareness of what he could hope to achieve for himself by trusting to his own force or cunning.<sup>6</sup>

Let me add that this is not a point *against* Rawls: a mutual advantage condition on theories of justice is quite implausible, if the baseline from which advantage is to be calculated refers to existing holdings in the world. It's just a point about truth in advertising. But it does mean that the Rawlsian enterprise has to face up to the problems posed by the gulf between justice and the partial interests of individuals.

Many of these themes play out in complex and difficult forms in the discussion of global justice. In 1971, Rawls had assumed that theoretical discussions of social justice could be confined, for the time being, to the case of "the basic structure of society conceived for the time being as a closed system isolated from other societies" (8). But obviously there are issues that require us to loosen this restriction. The framework for international trade and the global economy, the existence of borders and immigration restrictions, and the regulation of transnational environmental effects—all these raise questions of justice that cannot be properly addressed within the confines of Rawls's assumption. There is now a growing body of work about global justice (see, e.g., Barry 1989a; Pogge 1994b). Some of it attempts to develop and apply the Rawlsian framework to this wider context (cf. Pogge

6. Barry has also done sterling work in regard to some confused claims made about the relation between justice and mutual advantage by David Gauthier, in behalf of his theory of justice in Gauthier 1986 (see Gauthier 1998, 120 ff.; Barry 1998, 218–24).

1989); others pursue the subject more directly (e.g., Jones 1999). Perhaps not surprisingly, in view of our discussion in section 1, John Rawls's ostensible contribution (1999a) to this discussion is mostly not about global justice at all but about liberal approaches to international relations and human rights issues.

Much elementary work remains to be done in this area. First, theorists of global justice need to address the question of the extent to which talk about justice presupposes an agency or an institution (like the state) capable of doing justice. If it does, then the application of global justice theory is confined either to the work of global institutions or to those aspects of particular countries' institutions—like their immigration policies, trade preferences, environmental regulations, and so on—which have an extra-territorial impact. I suspect it is a mistake to confine the discussion in this way. In 1976, F. A. Hayek tried to convince us that we could not talk about the justice or injustice of market outcomes because these outcomes are not “the result of a deliberate allocation” by the state (64). We were not convinced: though we accepted that the use of an evaluative concept like justice must be restricted to arrangements that were in some sense subject to human control, we did not accept that its use was restricted to outcomes that were deliberately contrived, let alone those that were deliberately contrived by the state or a statelike institution (see Plant 1984, 3–5).

Well, the same applies in the global context. Certainly the disparity of access that people in different countries have to natural resources and to established frameworks of economic interaction is not a natural fact (like the weather); and even if it were, it is not a natural fact that humans can do nothing about (nothing to make it better, nothing to make it worse). Even accepting that it is an inadvertent resultant of millions of human decisions, we are required nevertheless to assess it at the bar of justice, because those decisions *could* be regulated within frameworks that are different from those within which they are currently regulated. There are also things that large numbers of people in the world could do to change the way they are regulated, despite the fact that there is no world government. So there is a question to be answered about the justice of our persisting with the current framework (or with its present trajectory of change).

These points are important in combating a tendency to think about global justice in terms of what distinct societies, whose members happen to be endowed with different levels of wealth and resources, owe to one another. The image that is sometimes used is that of two island societies, which have only recently become aware of each others's existence, and one is much poorer than the other: do the residents of the wealthier island owe anything to the residents of the poorer island if the former managed to secure their greater wealth without any interaction with the latter (cf. Nozick 1974, 185–86)? But that is nothing like the situation in the real world. First of all, there have been in fact millennia of interaction between peoples and of movement of peoples back and forth across the face of the earth. Sec-

ond, the main obstacles to poor people's access to resources such as minerals, fertile land, water, and so on, are human made (border fences, immigration control, etc.), not natural.

Once there is a physical possibility that that a person (or a whole people), P, might use a given resource, then the claim that that resource belongs rightfully to A and that A is entitled to exclude P, must be defended in a way that takes proper account of P's interests. It is not enough for A to show that her claim is defensible vis-à-vis the interests of her immediate neighbors B and C. Once P appears on the horizon, A's claim must be defensible to P as well. Otherwise there is no legitimacy to A's claim that P ought to leave her holding alone, and nothing but force or lies to back it up. From this point of view, it is not *global* justice theory that requires special justification. What requires special justification is the restriction of the scope of a theory of justice to the members of a particular society; and even if such justifications can be made out, they are likely to imply that any claims established by such restricted theorizing are purely pro tem or in personam, holding only between the individuals whose interests have been considered, without prejudice to the interests or claims of outsiders.

The point can be stated even more starkly with regard to borders and immigration controls, whose effect is to exclude outsiders not so much from access to natural resources as from participation in the established local or national economy in a given territory. Morally, it is quite inappropriate to concoct a defense of the justice of immigration controls with reference only to the interests of people on one side of the fence. Such controls have an impact on would-be migrants as well as existing residents of the country of immigration, and an assessment of their justice requires an assessment and comparison of those impacts (Carens 1995b). This is not a matter of taste, with cosmopolitans swinging one way, and communitarians the other. It is a matter of the elementary logic of justice, and its neglect I think—even in academic debates about immigration—bears witness once again to the fact, which we discussed in the previous section, that the primacy of justice (over, say, self-interest or national interest) is no longer uncontested.

Turning now from the clash between justice and particular interest to issues within the theory of justice itself, there has been an intriguing fallout from the events of the past ten years: a resuscitation of what amounts to a conception of justice based on aggregate utility.

This is not what one would have expected from the debates of the 1970s. One might have thought that the triumph of markets and capitalism after 1989 would have produced by the end of the century an owl of Minerva taking flight in Nozickian garb. It was, after all, Nozick 1974 that sharpened the debate about justice, rights, private property, and equality in the 1970s, taking the arguments of the Right from the repetition of tired, spittle-laden slogans about liberty into the realm of systematic and analyti-

cally respectable accounts of self-ownership and historical entitlement. For those of us on the Left, it was Robert Nozick (rather than, say, F. A. Hayek or James Buchanan) who undermined our confidence in simple formulas of equality and who convinced us—for example, through the famous Wilt Chamberlain example—that “no end-state principle or distributional patterned principle of justice can be continuously realized without continuous interference with people’s lives” (1974, 160–64).

But Nozick did his work almost too well to be much use to the triumphant Right after 1989. He argued honorably. He was never prepared to say that his historical entitlement critique of equality amounted to a defense of existing market institutions, nor was he willing to pretend that a Lockean defense of property of the kind he outlined could go any distance toward legitimizing contemporary disparities of wealth in (say) the United States. On the contrary he thought it undeniable that contemporary holdings would be condemned as unjust by any remotely plausible conception of historical entitlement. (The point of the argument in Chapters 7 and 8 of Nozick 1974 was that egalitarians were condemning the existing distribution for the wrong reason—for example, simply as unequal—rather than on account of the violence, fraud, expropriation, ethnic cleansing, state corruption, and so on, involved in the history of most significant holdings of property in the United States.) Once actual historical injustice was established, then the burden fell on the part of his conception dealing with the rectification of injustice. And for Nozick, it was an open question whether the actual operation of (say) Rawls’s difference principle might not approximate the operation of a plausible process for rectifying historic injustice. That, he said, would involve addressing some quite difficult questions about time, counterfactuals, and second-best principles:<sup>7</sup>

These issues are very complex and are best left to a full treatment of the principle of rectification. In the absence of such a treatment applied to a particular society, one *cannot* use the analysis and theory presented here to condemn any particular scheme of transfer payments, unless it is clear that no considerations of rectification of injustice could apply to justify it. (1974, 231; Nozick’s emphasis)

This was not the sort of thing defenders of free markets and opponents of welfare wanted to hear in the 1990s.

Instead what interested them was the consequentialist defense of market institutions. Whatever the case with Lockean entitlement (and who really wanted to open *that* can of worms?), market structures operating on a set of holdings simply accepted as given (or, by some other stratagem, put largely beyond question) showed themselves able to generate immense prosperity for a surprisingly broad sector of society. Thinking people of

7. I attempted to grapple with some of these conundrums in Waldron 1992.

course knew that inequality would escalate if market economics was given its head; but they reckoned that so long as the general atmosphere of consumer prosperity buoyed most people's hopes, those who sounded alarms about a greater and greater amount of wealth and power being concentrated in fewer and fewer hands could be made to sound like spoilsports (or Soviet-era dinosaurs).

In relation to this pragmatic dismissal of distributive concerns, the Rawlsians and Nozickians might in principle make common cause. Both denied that this implicit utilitarianism treated individuals with the right sort of respect; both rejected the proposition that prosperity for most could justify poverty for a few (cf. Rawls 1971, 19 ff; Nozick 1974, 33). Both were prepared to argue that the implicit utilitarianism of the market-based approach was an inappropriate ground for thinking about justice.

They would have had their work cut out, of course. Utilitarians did not simply lie down in the face of the original Rawlsian critique. Early respondents to *A Theory of Justice* took the argument back to Rawls, maintaining that the best bet for parties in Rawls's original position would be the principle of average utility and that Rawls's argument in favor of a maximin approach was simply not convincing (Harsanyi 1975). Others insisted that the philosophical critique of utilitarianism was founded on a misapprehension about the utilitarian conception of the place of rules in moral life and that the critique underestimated the resources of *indirect utilitarianism* (Hare 1981). Still others denied the aspersion cast on utilitarianism by Rawls (1971, 26–27) and others, that it failed to take seriously the distinction between persons:

[S]ince there is no fact of "separateness" that anyone has overlooked, no delusion that a group of persons is one super-person, the protest that utilitarians overlook separateness amounts to no more than the claim that one ought not to transfer the model of intrapersonal trades to interpersonal trades. It is an expression of one view about equal respect, and so not a reason for choosing it. (Griffin 1986, 169–70)

In the philosophical study of justice, the worry about utilitarianism was always that it seemed insufficiently interested in distributive issues: its interest seemed to be riveted in the aggregate or average bottom line of the social distribution, not the matrix of distributive outcomes for individuals. But the eye to the aggregate or average outcome need not be the result of lack of concern about distributive matters; on the contrary, there is always some aggregate element even in the most individualized theory of justice. Thus, in the passage from which my last quotation was drawn, the moral philosopher James Griffin made the following point:

[M]erging people's interests into a single moral judgment by maximizing them is a distributive principle. It is a view, right or wrong, about when sacri-

ficing one person for another is justified. It is just a modern muddle to contrast sharply distributive and aggregative principles, as if an aggregative principle could not also be fully deliberately distributive. . . . Similarly, no plausible principle of distribution—think, for instance, of Rawls’s Difference Principle—could be purely distributive, without some maximizing tendency, as if reducing everyone to the same level of misery could satisfy it. Every plausible principle of equality is based on the thought that everyone matters and matters equally, and to stress only the formal features of distribution is to recall the *equally* but to forget the *matters*. Even a principle of a minimum acceptable level of welfare has, if not a maximizing, at least a quantitative element. And the principle of utility, too, represents another conception of the distribution that equal respect for persons requires. (1986, 168–69, Griffin’s emphasis)

Something like the Griffin approach—that is, careful presentation and defense of utilitarianism as a *fair* and *just* basis for making hard choices between individual utilities in society—is evident, too, in contractarian defenses of average utilitarianism.<sup>8</sup>

Unfortunately, however, very few utilitarians have devoted this level of care to the distributive issue. And Griffin himself acknowledges that where aggregative measures are actually used—for example, in economic analysis—the casual blurring or sidelining of the distributive issue is often quite striking: “It crops up commonly in regarding, as economists often do, an aggregative principle as a principle of ‘efficiency’ and other principles as ones of fairness” (Griffin 1986, 168). Thus it is often hard to tell whether those who defend the operation of a market economy as efficient, for example, intend that to be an evaluation of its justice (or an evaluation responsive to concerns about its justice).

Casual confusions of this kind are particularly evident in what has become known as the “economic analysis of law” (EAL). I want to spend some time with this approach, because as things stand it represents one of the most powerful tools used in the academy to deflect intellectual interest from distributive issues.

We may begin with the article of faith, accepted uncritically by most economists, that interpersonal comparisons of utility are meaningless and that accordingly neither economists nor any other social scientists could pronounce (on welfarist grounds) either for or against policy proposals that involve balancing costs to some against benefits to others. When this began to be accepted in the early decades of the twentieth century, its impact on debates about social justice was mainly to discredit the traditional utilitarian argument for equality of income, based on the diminishing marginal

8. Here I am thinking of the argument in Harsanyi 1975. But I also have in mind the case for the selection of utilitarian principles in the original position, sympathetically presented (but later argued against) in Rawls 1971, 161 ff., in a section Rawls entitles “The Reasoning Leading to the Principle of Average Utility.”

utility of money; see Cooter and Rappoport 1984, for a superb account. Inevitably, however, policy scientists became restless under the force of the implied principle of restraint—"No comment on any policy proposals other than Pareto-improvements"—and various strategies were evolved for evaluating proposals that seemed likely to leave some people in society worse off. One strategy was to ask whether the winners (under a policy that involved costs as well as benefits) might gain enough to be able to compensate the losers (by the latter's own lights) and still be better off. This was the famous Kaldor-Hicks criterion (see Coleman 1992, 167), which promised to certify almost anything a classical utilitarian would approve of as a (potential) Pareto improvement.

As far as anyone can tell, this (or something like it) is the evaluative notion appealed to in modern law-and-economics discussions of wealth-maximization.<sup>9</sup> The EAL is committed to the principle that disputed rights and resources should be allocated to those who value them most. This after all is what voluntary market transactions do. When I buy your peach for a dollar, I lose a dollar and you lose a peach; but I gain something I value more highly than my dollar and you gain something you value more highly than your peach. Thus the transaction increases the amount of human value accruing from the possession and use of peaches (and dollars) on both sides (cf. Posner 2001, 98).

Consider now a more complicated transaction. Imagine that your peach trees are dying because the water you might use to irrigate them is being used instead by my factory. And suppose that with this water my factory produces more for me than your orchard produces for you; indeed, it produces so much more for me that I could pay you all you might earn from an irrigated orchard and still have plenty left over for myself. Obviously, then, the amount of human value accruing from the use of this water is greater if it is used in my factory than in your orchard. And that would be the market outcome, at least in an ideal world: if I had a legal right to the water, you would not be able to offer me enough to divert it to your orchard; and if you had the legal right to the water, I would be able to buy that right from you and use it in my factory. Value would be maximized in either case. The only additional real-world question is: how costly will the process of bargaining be, and how much of the net gain will be eaten up in lawyers' fees, negotiating time, and the costs of drawing up the appropriate contracts or conveyances? According to the EAL, the law should be such that the process is as costless as possible. In the example we are considering, in which the facts about relative profitability are known, an initial legal assignment of the water right to the factory owner will minimize the time and trouble of transacting. So if a court ever faces the ques-

9. Certainly, it is the version appealed to in Richard Posner's canonical account (1980, 487). Here I follow the analyses of Coleman 1982 and Dworkin 1995.

tion about where the water right should be assigned, *that* should be the basis on which it answers the question.

Now, patently, this question, “What can the law do to facilitate market outcomes?”—asked in this way—does not address issues of distributive justice. And one would have thought that those issues were important in our example. After all, it makes a huge difference to the individual wealth and well-being of the respective parties (under the efficient outcome) where the water right is initially located. If it is initially located with the orchardist, then even though the factory owner eventually gets the water, the orchardist will end up with at least as much money as he would have received from his irrigated crop; but if (as the model suggests) the court assigns the water right initially to the factory owner, then the orchardist will end up with nothing, or nothing but the value of parched land and dead trees. True, the factory owner will have enough from her profits to compensate the orchardist. But it is no part of the wealth maximization model that this compensation should actually be paid. Any insistence that it should be paid is at best a distraction, according to EAL, and at worst a recipe for multiplying transaction costs.

This carelessness about distributive outcomes has two sources: the so-called Coase theorem, foundational to EAL, which states that the initial assignment of entitlements—and a fortiori distributive outcomes which are just a function of initial entitlements—is irrelevant to the maximization of wealth (Coase 1960), and the assimilation of a Kaldor-Hicks improvement to a Pareto improvement. Both points should be the focus of criticism. First, the initial distribution of entitlements makes no difference to the pursuit of efficient outcomes even if it is fallacious to infer that efficiency *ought* to be pursued without regard to the distribution of entitlements. (And note that the fallacy obtains even in a world of costless transactions.) The initial (and the eventual) allocation of entitlements may matter for reasons that have nothing to do with efficiency; it may matter for reasons of justice.

Second, the legal imposition of a Kaldor-Hicks improvement (as between the orchardist and the factory owner) has none of the normative respectability of a Pareto improvement (e.g., as between the person with the peach and the person with the dollar). When we bypass the need for a voluntary transaction in the imposition of a Kaldor-Hicks improvement, we are not *in any way* honoring the preferences of the losing party. Hypothetically, the orchardist might prefer a certain sum  $c$  of compensation to keeping his right to the water; but if  $c$  is not actually to be paid, then the orchardist has no relevant preference at all (except to keep the water right) and that is not honored in the least by transferring the right to someone who would pay  $c$  if he had to but does not. To oppose a Kaldor-Hicks imposition in the name of justice is therefore not to oppose justice to human welfare, nor is it to oppose justice to people’s revealed preferences, nor is it to oppose justice to a

scheme that makes everyone better off. It is rather to insist on the importance of respecting actual individuals with their actual preferences in the actual world; and it is to oppose the imposition on individuals of actual losses for which nothing but hypothetical compensation is envisaged.

All this was pointed out decades ago (Coleman 1982). But the leading advocate of wealth maximization, Richard Posner, has evidently failed to learn the lesson. In his recent writing, he is willing to concede that distributive issues like inequality may pose certain costs of their own—for example, in social stability—which the advocate of wealth maximization would do well to take into account (2001, 102). But this is not a way of taking distributive issues seriously: such issues are important in themselves, and important in regard to respect for individual persons. They are not just something to be factored into a sort of aggregative social pragmatism.

A somewhat different line that economic scholars take is to say that if issues of distributive justice and equality are important, they should be handled by the legislature and should not contaminate the economic reasoning of courts. This is a spectacularly ill-conceived response. It is not an accident or a nuisance that distributive issues often arise in lawsuits. Even people who agree cheerfully about what counts as an improvement may disagree bitterly and even violently about how the benefits of any given improvement should be distributed. And it is typical (some would say definitive) of lawsuits that they embody such questions. Plaintiffs and defendants are not like public interest lobbyists in a legislature, putting forward rival solutions in the name of efficiency to some problem of common concern. They are desperately concerned precisely with the distributive features of a given solution, because their individual lives, fortunes, and liberty may hang in the balance.

Recent jurisprudence has emphasized that parties come to court not as lobbyists with various bright ideas for legal reform and clarification but—at least in their own eyes—as right bearers: each party thinks she is *entitled* to the outcome for which she is rooting. Ronald Dworkin has made much of that in his legal theory (1977, 82 ff.); but subsequent discussion has oriented the point mainly to what Dworkin says about right answers: each party thinks the law already justifies the outcome he seeks, and Dworkin believes jurisprudence should attempt to make sense of that thought. But the same point may also be oriented toward the distributive issue: plaintiffs and defendants approach litigation in the spirit that nothing matters more than the distributive question of who (in particular) ends up with what.<sup>10</sup> I don't just mean they are greedy and self-interested. For each

10. I think this is what Dworkin is getting at with his distinction between legal principles and legal policies: when a legal principle is at stake in litigation, the distributive issue between plaintiff and defendant is an instance of something that goes to the very heart of the justification of the provision in question (1977, 90–100).

of them, their position is a matter of what is legally right: plaintiff insists that he, in particular, is legally entitled to some benefit or resource or compensation, and defendant insists that he is not. To say that courts should try and finesse the distributive issue is to ignore the fact that which way a certain benefit goes on the distributional matrix is, almost invariably, what the lawsuit is exactly about.

The very best economic analysis in law concedes all this, and acknowledges too that legal issues cannot be solved without a basis of thinking about utility that takes distributive issues seriously. Thus a recent and important long article by Louis Kaplow and Steven Shavell called “Fairness Versus Welfare” says that legal analysis requires a social welfare function affording some defensible basis for aggregating welfare across individuals, and that fundamental conceptions of justice are appropriately deployed in designing and critiquing such a function (2001, 985–89). The principles of fairness that they criticize are not to be identified with such conceptions but rather with moralistic ideas that are independent of welfare judgements, even those that already take distributive considerations appropriately into account (Kaplow and Shavell 2001, 989). Indeed, they cite Rawls 1971 as a source of insight on the fundamental question of distribution, and they in common with others among the more scrupulous practitioners of the EAL acknowledge that in any sensible division of intellectual labor, philosophy will be assigned frontline responsibility for developing these conceptions. Now, whether that is intended sincerely or not by the economists, and whether they intend—as Kaplow and Shavell actually do—to take notice of what emerges from the philosophical discussion of justice is an open question. Mostly it has been my aim in this essay to emphasize the importance of that assignment, so far as the theorizing about justice is concerned. The economists are not going to develop theories of just distribution; at best they will expect the political philosophers to do it. So if political philosophers do not give their attention to this topic (because they would rather discuss more fashionable topics like public reason in a pluralist society), there is a danger that nobody will and the economists will be able to wander off happily confirmed in their suspicion that there is, after all, nothing—or nothing rigorous—to be said on this subject.

# *Pluralization and Radical Democracy: Recent Developments in Critical Theory and Postmodernism*

## ■ | Introduction<sup>1</sup>

This essay aims to illuminate some of the most important debates and developments in recent critical theory and postmodernism. These theories are highly diverse in their philosophical perspectives, subject matters, theoretical styles, and political implications. I initiate my discussion with critiques of political liberalism in the hope of soliciting readers who are relatively unfamiliar with this body of theory (as well as those immersed in it) to enter into reflections and dialogues they might otherwise avoid.<sup>2</sup> I begin with Habermasian criticisms, as these diverge least from many themes in political liberalism. From there, the perspectives discussed increasingly deepen and broaden the terrain of contestation, and the focus shifts to the philosophical differences and political stakes between critical theory and various postmodernisms. Central questions concern the nature and scope of democratic dialogue and power; the way theorists interpret the role of bodily affect, disposition, and perception in democratic engagement; the interpretation of important sites, objects, and practices of power and radical democratic contestation; and how responses to the questions are en-

1. I wish to thank Jane Bennett, Kimberley Curtis, Bonnie Honig, Ira Katznelson, and an anonymous reviewer for their helpful comments on and criticisms of earlier drafts of this essay; and Tania Roy and Matt Diamond for their vital research assistance.

2. I do not wish to suggest that responding to political liberalism has been the central focus of critical theory and postmodernism. The latter have sought to expand our sense of the terrain of politics and possibilities for action through various engagements that have often had much more to do with exploring directions suggested by Foucault, Derrida, Adorno, Butler, Deleuze and Guattari, Arendt, Heidegger, Nietzsche, and others, in the face of ongoing political struggles concerning race, gender, sexuality, ecology, economic equality, postcoloniality, radical democratic community power, technology, prisons, nationalism, education, identity, coalition, and so forth.

twined with approaches to rights, coalition politics, order, responsibility, and political institutions. Surveying a range of positions, I suggest that some of the most promising work draws on insights from—as well as tensions between—both critical theory and postmodernism.

One of the most important contributions of many of the works at hand is to elaborate, in numerous ways and across many domains, more receptive and generous modes of dialogical political contestation than one finds in most liberal theory. The textual strategy that follows seeks to embody these insights. If much of the theorizing under discussion is often blithely dismissed or ignored by other perspectives, we must resist the impulse to return the favor.

In what follows, I pursue fewer dialogues in more depth, rather than try to cover exhaustively but thinly the vast terrain invoked by the terms *critical theory* and *postmodernism*. There are costs of omission that accompany this strategy, but it is, I think, the most promising way to enact the substantive insights suggested. My claim is that the most compelling insights are emerging precisely in the *tensions* between different positions. These only come to light through patient explorations that disclose the complexities of blindness and insight in the contestations that follow. While both critical theory and postmodernism have made great advances in recent years, the engagements between them have often had the character of ships passing in the night—or polemic. If it is at the agonal intersections between them that one finds what is most promising, the only way to elaborate this insight is by dwelling at these points with a care that might have some power to persuade those not already convinced. The mode of this dwelling is as important as the sites themselves. By enacting an alternative practice of engagement, one might not only discern things missed by more constrained optics, but also possibly shed another kind of light on both certain persistent weaknesses in some of the perspectives at hand and more hopeful paths.

## ■ | Political Liberalism and Its Limits

Liberalism has many faces.<sup>3</sup> Yet the mode of liberalism that has the greatest constructive influence on political theory, and the mode which others feel most compelled to critically address, is political liberalism as articulated foremost by John Rawls (1996). At the heart of Rawlsian liberalism is a notion of reciprocity rooted in the idea of society as a system of cooperation between free and equal citizens. Political life thus construed is to be

3. There are numerous contemporary exponents of liberalism whose theorizing has been forged partly through engagements with critical theory and postmodernism. Among the most remarkable, one would have to include Flathman (1992, 1998), Kateb (1992), and Moon (1993).

guided and limited by conceptions of freedom, equality, and fairness that constitute a “public reason” presiding over constitutional questions and all matters concerning fundamental questions of justice. Such reason requires that citizens keep their moral, theological, and philosophical differences out of political life when basic matters are at stake, unless “in due course public reasons, given by a reasonable political conception, are sufficient to support whatever the comprehensive doctrines are introduced to support” (Rawls 1996, li–lii). Public reason’s directions and limitations “provide the *common currency* of discussion and a deeper basis for explaining the meaning and implications of the principles and policies each group endorses” (1996, 165, my emphasis). Formulating deliberation as exchange of agreed on terms of linguistic currency, Gutmann and Thompson write: “the primary job of reciprocity is to regulate public reason, the terms in which citizens justify to one another their claims regarding all other goods.” The good given and received is that we all make claims on terms that we can accept in principle (1996, 55).

If, in decades past, the common currency of public reason was given more transcendental grounding, since the 1980s, political liberalism has increasingly understood itself as the expression of the deepest commitments of the tradition of liberal democratic societies. Political liberalism seeks to avoid aggressive articulations of historical contingency and irony (like Rorty 1989), proclaiming instead a neutrality toward such philosophical worldviews. Yet the teleological story Rawls (1996) now tells of the emergence of political liberalism—from a *modus vivendi* among those warring over comprehensive doctrines to a constitutional consensus to a deepening “overlapping consensus”—is replete with contingencies of power and worldviews.

That said, political liberalism understands itself as the deepest expression of this tradition and claims to articulate the terms that ought to singularly ground political legitimacy. Political liberals acknowledge reasonable pluralism within the political liberal family. Yet all who lie beyond this family—from libertarians to Arendtian radical democrats to those religious traditions that understand themselves politically—are, insofar as they enter the contestations of political life in ways that exceed the limits defined by political liberalism, making claims that are morally (not legally) illegitimate from the Rawlsian position.

Accompanying this constraint on difference in the public sphere is a rather restrained affirmation of broad-based political engagement. Many political liberals now urge that “forums for deliberation should abound” (Gutmann and Thompson 1996, 37; Fishkin 1991), within the limits of public reason. Yet much political liberalism is infused with the proceduralism that Rawls articulates when he writes that the advantage of the least well-off (articulated by the difference principle) should be inscribed in public law and not apply “to particular transactions of distribution, nor to decisions of individuals and associations, but rather to the institutional

background against which these transactions and decisions take place” (1996, 283). Secure in the knowledge that elsewhere in the social system the necessary corrections to preserve justice are being made: “individuals and associations are then left free to advance their ends more effectively within the framework of the basic structure” (269).

While political liberal negotiations of the balance between proceduralism and deliberative forums vary, their affirmations of deliberation are generally framed in terms of sites of political discussion designed to inform the determination and administration of proper policies. Rarely does one find discussions of possible transfigurations of everyday political, social, and economic life that might increase political dialogue and engagement or much support for contemporary social movements that often enact such engagements. Civil society is conceived more as a site for nurturing support and consultation than as a location of pluralizing insurgent mobilizations that cultivate social forces aimed at transforming the practices and mores of liberal polities.

## ■ | Critical Theory Contestations

To make this assignment manageable, I limit my present discussion of critical theory to those theories associated with the Frankfurt School.<sup>4</sup> I first trace the account of critical theory given by Jurgen Habermas, its leading contemporary figure. Then I work through its difficulties and increasingly discuss those who draw on early critical theory to engage themes proximate to some of those discussed in the following section on postmodernism.

In the dominant Habermasian account (e.g., Habermas 1987; Benhabib 1986; Honneth 1991), critical theory begins in the 1930s with the efforts of Theodor Adorno, Max Horkheimer, Herbert Marcuse, and Walter Benjamin to challenge an “instrumental reason” that banishes reflection on human ends and focuses exclusively on the rationality of efficient means. Entwined with instrumentality is the systematic organization of politics, society, and the culture industry in ways that integrate humans and nonhuman nature into a totality of ever-increased productivity that disproportionately empowers the few and assimilates virtually everyone into a one-dimensional society.

According to Habermas, these theorists so exaggerated their criticism that they undermined their foundations and misapprehended modern political, social, and ethical developments. Reducing rationality to instrumental reason, they missed how enlightenment theory and political practice *also* unleashed a “communicative rationality” that pointed toward a more just and defensible order of things. Entangled in nihilism, Adorno lost sight of hopeful developments in modernity, like universalist ethics,

4. For overviews of the Frankfurt School, see Jay (1973) and Held (1980).

constitutional democratic politics, and democratic social movements. While Habermas criticizes the “colonization” of these developments by market and bureaucratic systems, he argues that this colonization has not eroded the foundations of communicative rationality or overridden their contemporary sociological and political manifestations.

Habermasians engage political liberalism on four related fronts. They challenge Rawlsians on:

The question of normative foundations

The static and reifying implications of these foundations

The extent to which differences are normatively precluded in political discourse

Political liberalism’s limited understanding of democratic practices

Questions concerning normative foundations are central to the engagement between Habermas and Rawls (Rawls 1996; Habermas 1998a; see also Baynes 1992; Chambers 1996). Initially, Habermas (1990) criticized Rawlsian liberalism for grounding its normative position in monological subjectivity and enlightened self-interest. Morality thus founded is implicated in modes of self-assertion that ultimately undermine universalist goals and harbors ahistorical assumptions that block critical reflection on how power is inscribed in definitions of need, the good, and subjectivity itself. For Habermas, selves originate in and are sustained through communicative action. Moral theory seeking to secure the best aspects of enlightenment deontological universalism—while jettisoning the worst—should ground itself in the communicative structures and pragmatic presuppositions of everyday lifeworld interaction.

Habermas argues that Rawls’s political turn toward a hermeneutic position claiming to express the intuitions of a liberal democratic tradition still harbors some of the old problems and introduces new ones. Habermas discerns static tendencies, as the original position still frames the pursuit of the universal in an overly “monological fashion” by “imposing a common perspective on the parties . . . through informational constraints . . . [that] neutralize the multiplicity of particular interpretive perspectives from the outset. [In contrast] discourse ethics . . . views the moral point of view as embodied in an intersubjective praxis . . . which enjoins . . . an idealizing *enlargement* of . . . interpretive perspectives,” through “mutual criticism of the appropriateness of the languages in terms of which situations and needs are interpreted. In the course of *successively* undertaken abstractions, the core of generalizable interests can then emerge step by step” (1998a, 57–58).

For Habermasians, normative philosophy should be oriented toward clarifying the “demanding presuppositions of the ‘public use of reason’” and argumentative practice (1998a, 59). The “principle of universalization (U),” presupposed in everyday communication, states: “All affected can ac-

cept the consequences and the side effects its *general* observance can be anticipated to have for the satisfaction of *everyone's* interests. . . ." (1990, 65). This position frames all substantive claims in a manner that solicits the discursive participation of all affected to determine or revise their validity. It "does not bracket the pluralism of . . . worldviews at the outset" (1998a, 59) but rather "includes the other" insofar as "needs and wants are interpreted in light of cultural values," in a process where worldviews must be engaged and criticized in actual public discourses in light of (U) (1990, 76–78). Habermas maintains the deontological priority of justice over the good but argues that the former is substantively discernable only as the visions of the latter are openly employed and filtered through rational discourses.

Habermas argues that with Rawls's hermeneutical turn toward the liberal democratic tradition, "practical reason is robbed of its moral core and is deflated to a reasonableness that becomes dependent on moral truths justified otherwise" (1998a, 82–83). Rawls's liberal intuitions both rely for their moral force on "a lucky convergence" of particular doctrines, and simultaneously exclude these doctrines when they overstep the limits of liberal intuitions as Rawls discerns them. This combination of reliance and exclusion leads to a moral deficit that manifests itself in a paucity of potentially context transcending arguments that can be offered when the "neutralizing" dimensions of political liberalism face the "others" who exceed the boundaries Rawls ascribes to the democratic tradition. Rawls's may be closer to Rorty's contingency than he intends.

Habermas's quasi-transcendental discourse ethics and his foregrounding of actual participation and "the inclusion of the other" have important political implications. In contrast both to liberal models of democracy based exclusively on affirmations of formal rights, and communitarian models focused on the material conditions and public autonomy required for freedom, discourse democracy focuses on a "*proceduralist understanding of law* that is centered on the procedural conditions of the democratic process" (1998b, 18, 1996; Rosenfeld and Arato 1998). Popular sovereignty and human rights are seen as "co-original," "complimentary," and "nourished from the same root." Habermas seeks to ensure private rights *and* public autonomy by emphasizing "a public of citizens who participate in political communication in order to articulate their wants and needs, to give voice to their violated interests, and . . . to clarify and settle the contested standards and criteria according to which equals are treated equally and unequals treated unequally" (1998b, 244).

Habermasian visions of the public sphere have shifted away from an earlier embrace of a radically democratic socialist totality, and most critical theorists now argue that state bureaucracies and market systems "can no longer be transformed democratically from within . . . without damaging their proper systemic logic and therewith their ability to function" and coordinate action in an immensely complex society (Habermas 1992, 444;

Cohen and Arato 1994; Rosenfeld and Arato 1998). Hence, instead of subjecting states and markets to direct radical democratic governance, a “separation of powers” is necessary, in which state, economy, and a heterogeneous civil society are each granted a degree of autonomy. The task of radical democracy is to “erect a democratic dam” against state and economic colonization of the lifeworld and to make these systems more responsive to the communicatively generated guidance of the practically oriented demands of the lifeworld.

During the 1980s, Habermas stressed the irreducibly conflictual dimensions of the relationship between civil society and states and market systems. Systems figure primarily as colonizing powers that public spheres must endlessly resist and attempt to sensitize to the demands of the lifeworld. More recently, critical theorists tend to identify more particular conflicts (rather than a general theory of conflict) and emphasize that democratic steering is an efficacious (and therefore legitimacy-bestowing) aspect of modern societies, especially during times of crisis.

Discursive democracy involves the delicate interplay between informal public spheres (associations, social movements, etc.) and formal publics (representative institutions). The former provide a “context of discovery” which, free of procedural regulations, is far more “wild” and “unrestricted,” ranging “more widely and expressively” (Habermas 1996, 307–8). Here struggles around need, recognition, and identity can be pursued over broader and more uncertain terrain. Gradually, these struggles can gain attention and “be taken up by responsible political authorities, put on the parliamentary agenda, discussed, and if need be, worked into legislative proposals and binding decisions” (Habermas 1996, 314). In Habermas’s view, the flows of communication and influence are often one directional, such that informal public spheres are eclipsed and distorted by political economic power. Yet in times of crisis these flows can and do change direction in ways that allow the informal realms of civil society to become increasingly influential. Concerning issues like feminism, ecology, poverty, and the arms race, informal public spheres have shown they can have considerable influence on national political discussion and policy. Though often weak and discontinuous, this influence illustrates that discursive democracy is not just an ideal, but an ideal with significant efficacy in the practices of constitutional democracies. In marked contrast to political liberalism, the insurgent aspect of informal public spheres plays an elemental role in the ongoing practice and development of democracy.<sup>5</sup>

In contrast to more radical democratic formulations, however, these public spheres are repeatedly urged to exercise “intelligent self-restraint” and “leave intact the modes of operation internal to functional systems and other highly organized spheres of action” (Habermas 1996, 372); they must

5. For provocative discussions of these issues, see the wide range of analyses in Bohman and Rehg (1997).

limit themselves to “mild forms of indirect steering” (Habermas 1998b, 19). The best that democratic citizens can do is seek to enhance their capacities for such steering through policies and practices that aim—along with every development of state protections of private rights and every extension of state powers to provide entitlements requisite for public autonomy—to enhance the capacities of “*affected parties themselves* to conduct public discourses” in civil society (24).

Just as Habermasian democracy is more insurgent and contestatory than political liberalism but less so than many articulations of radical democracy, it occupies a middle ground concerning questions of difference and democracy. Though Habermas criticizes political liberalism for excessively limiting the pluralism of worldviews, he himself is not immune to this charge. As Rawls argues, Habermas’s “immodest quasi-transcendental position aims to be too comprehensive,” offering “a general account of meaning, reference, and truth or validity both for theoretical reason and for several forms of practical reason.” All religious and metaphysical views in excess of the Habermasian account thus appear to “lack any logical force of their own” and often criticized summarily “without taking much time to argue against them in detail; rather, he lays them aside—or occasionally dismisses them” (1996, 376–78). Brian Shaw, a sympathetic critic, traces Habermas’s “regrettable mixture of indifference and contempt for religious belief” (1999, 634) and suggests that religious discourses cut across and disrupt Habermas’s overly tidy Kantian distinctions between truth, morality, and aesthetic modes of validity.

Once again, the deepest philosophical moves have important political implications. Habermas comprehends modernity in part as a process of “pluralization of the lifeworld.” In this context, the best way to reduce potential conflicts is through universalistic law and deontological discourse that limits the political influence of ethical visions of the good through an ongoing testing process that evaluates their general acceptability in light of (U) (Habermas 1998a, 42). The (U) is said to act like “a knife that makes razor-sharp cuts between evaluative statements and strictly normative ones, between the good and the just” (Habermas 1990, 104).

But how sharp is this razor and how does it work? If value orientations are “enlisted” for their descriptions, information, and interpretations in normative discourses, are they not inextricable from the normative claims that result? Discourses aimed at universalization would put pressure on particular visions of the good, but the latter would always profoundly influence and pressure the former as well, so the logics and meanings of justice and visions of the good would never be entirely separable. If this is true, then we should expect that normative discourses will not always facilitate consensual resolutions to conflicts among those of different traditions and worldviews (Coles 1997; McCarthy 1998; Bernstein 1998; Moon 1995). Moral claims would not necessarily rise above conflicts over the good but rather often remain deeply entangled with them. Given Habermasian ex-

pectations for the consensual effects of normative discourse, Rawls seems right when he charges: "Habermas's description of the procedure of reasoning and argument . . . is . . . incomplete. It is not clear what forms of argument may be used, yet these importantly determine the outcome. Are we to think . . . that each person's interest [and evaluations] are to be given equal consideration? What are the relevant interests? Or are all interests to be counted?" (1996, 430). If Habermas and others have flagged the impositional and unreciprocal aspects of Rawlsian public reason, Rawls in turn identifies a certain formalistic deficit in the Habermasian alternative.

While Habermas's position has moved in directions that are more inclusive of the other, significant problems remain. In Habermas's hands, the deontological razor, however illusory it may be, cuts into the world in a manner that radically privileges the position of "normative discourse" in relation to the pluralistic worldviews it claims to engage. As Bernstein argues, it does so by means of the "violently distortive fiction" that ethical traditions develop exclusively with an eye to the realization of their own particularist goods and values. This fiction distorts, because many traditions are oriented equally toward "universalistic demands and obligations." Maintaining the sharp separation *elevates* discourse morality above all modes of evaluative theory and practice, by denying *both* the historical entanglements of all universalistic claims *and* the transcendence of many traditionalist positions (Bernstein 1998, 301, 1993; Shaw 1999; see also Butler 1996, 2000). He rhetorically *lowers* traditionalist and other evaluative positions by casting their horizons in terms that are radically particularist, thereby conjuring them as inherently oblivious and violent toward their others unless enrolled in and limited by the strivings of discourse ethics. In question is not Habermas's claim that traditions encroach on each other, sometimes violently. Rather it is whether discourse ethics is as free from this encroachment as Habermas claims and whether traditions are as inherently devoid of potent and distinct resources for transcending their limits, listening to the claims of others, and so forth, as Habermas suggests. Insofar as neither argument proves to be true, we are faced with different contending historical projections of the universal that would have to engage in dialogue and negotiation with one another with less probability of consensual resolution—but also less deaf oblivion—than critical theory would generally have us believe.

To better address these difficulties, Benhabib "emphasizes and even radicalizes those aspects of a discourse ethic which are universalistic without being rationalistic, which seek understanding among human beings while considering the consensus of all to be a counterfactual illusion, and which are sensitive to differences of identity, needs and modes of reasoning. . . ." (Benhabib 1992, 8). Attentive to context and "concrete others," she seeks to bring universals and particulars into creative tension with one another in ways that more intelligently transfigure the boundaries between the good and the just, and public and private (1986, 1992).

Though Benhabib's writing is often rich, synthetic, and probing, perhaps her proximity to Habermas's paradigm leaves her at times more enmeshed in some of his problems than she wishes to be. Hence, in response to I.M. Young's suggestion (1997) that "greeting, storytelling, and rhetoric" ought to be given a more prominent place in theories of public dialogue, Benhabib writes: they "cannot become the public language of institutions and legislatures in a democracy for the following reason: to attain legitimacy, democratic institutions require the articulation of the bases of their actions and policies in discursive language that appeals to commonly shared and accepted public reasons. . . . Young's attempt to transform the language of the rule of law into a more partial, affective, and situated mode of communication would have the consequence of inducing arbitrariness," as rhetoric sways without reason, and stories remain local and sometimes inaccessible to others, and so on (1996b, 83).

At the moment, I am less concerned with Young's position (which is more nuanced than Benhabib would suggest) than Benhabib's response. This is because what one witnesses, perhaps, is that when pushed, a certain set of fundamental Habermasian orientations tend to reassert themselves and overdetermine the response, in spite of Benhabib's insightful efforts to open them in hopeful directions. Hence, while Benhabib writes toward engagements that would recognize people as having a "concrete history, identity, and affective-emotional constitution" (1992, 159), she quickly rejects narrative and affect—at least in the political discourses within decision-making institutions—when it is shown how these frequently introduce problematic contingencies and nonidentity into political discourse. Perhaps positions more capable of sustaining the sensitivities for which Benhabib calls would stand at a greater distance from the Habermasian paradigm that significantly engenders them?

Habermas's own rhetoric appears to play no small role in pushing toward a different star than those that often guide Benhabib's promising efforts. Accompanying his more rhetorical than reasoned rendering of visions of the good and his rhetorically exaggerated sense of capacity of discourse ethics to disentangle critical theory from both, many of Habermas's texts engender an insistent and sometimes almost singular striving for consensus (Coles 1992b). Consensual striving is often rhetorically disclosed in association with terms like *restore* and *reach*, which tend to shift our attention away from questioning the damages that might *also be imposed* in the *forging* of many moral and political agreements. In this light, ongoing agon related to strong commitments to particular modes of seeing and doing is summarily shunned. Tarrying between nonidentical positions is seen merely as a barrier to "carrying on the world's business." And criticizing the idea that the purpose, legitimacy, and function of ethicopolitical discourse is wholly related to the *achievement* of universalizable norms in the midst of political conflict is seen as inching toward treason.

Nowhere is this clearer than in the recent encounter between Haber-

mas and one of his most loyal interpreters, Thomas McCarthy (1998), who presents a sharp set of arguments about the entwinement of the good and the just, similar to those presented above. He concludes that, given the messy historicity of normative claims, sometimes consensus will be forged, yet sometimes disagreement will appear to be intractable. Many questions may not even ideally have “one right answer.” Thus he questions Habermas’s overly “cognitive” and “consensus-seeking” understanding of political discourse and legitimacy. Beyond a minimal consensus concerning “the basic rights and principles of the democratic constitutional state . . . we would need an account of democratic public life that is decidedly less centered on rational consensus than [Habermas’s] is” (1998, 125–26). McCarthy remains a strong proponent of discursive democracy, yet suggests that the advantages of the latter should be framed less in terms of the movement toward consensus—though these advantages do sometimes occur, can be important, and should be sought—and more in terms of the role discourse democracy plays in establishing and maintaining relations that are free from violence. In combination, discourse and majority decision making facilitate cooperative action even when there is disagreement, by inscribing the tentativeness of human understanding and relations in practices that allow minorities an open space to contest majority decisions and possibly persuade people to change policy. Beyond these practical benefits, discourse democracy has cognitive benefits, even when these exceed the framework of “movements toward consensus.” In pluralistic societies, the benefits of discursive contestation often consist in weeding out errors and ideology. Moreover, such practices can help us discover and cultivate “mutual accommodation,” “respect,” “tolerance,” and “reasonable disagreement.” McCarthy notes there are tensions between these advantages and theories of dialogue supposing the ultimate existence of one right answer to most normative questions. Interestingly, however, Habermas can hear in McCarthy’s arguments only an “anti-universalism” that slides dangerously and in spite of itself toward Carl Schmitt’s decisionism (1998c). Habermas (1987) has made these claims repeatedly about postmodernists and early critical theorists, but when he levels them at McCarthy, questions arise about how the possibilities, hopes, and fears engendered by the Habermasian rhetorical disclosure of the world might tend to contribute to a significant deafness precisely in the name of a particular rendering of inclusion.

Bernstein argues, relatedly, that *in addition to* the procedural side of democracy, emphasized by Habermas, democratic practices capable of resisting the “garbage in, garbage out” phenomena require a richly developed ethos of dispositions, including “a willingness to listen,” “respecting the views of minorities,” and the “courage to change one’s mind” (1998, 291; see also 1993). With MacIntyre, Bernstein accents these “internal goods” of democratic practices.

Wellmer (1991, 1998) further develops these themes. Greatly influenced by the theory of communicative action, Wellmer seeks to avoid Habermas's "objectivist misreadings" (which overemphasize an imaginary consensus end point) and embraces instead a "weak localizing" interpretation that emphasizes a dialogical openness and movement beyond limits that resist constraints of power and dogma, while recognizing that we are—as finite beings within a particular horizon—always constrained by history, contingency, and an "obscurity and opacity [that] are continually being renewed." Dialogue pluralizes and further opens horizons as often as it brings about convergence and fusion. Truth appears partially at best, and our ability to grasp it is forever having to prove itself anew in the movement of dialogue, practices, and applications that confirm it or call it into question. What is "striking" about our "ability to understand" is "its orientation toward the future" (Wellmer 1998, 234).

Habermas (and those closest to him) agree, but there is a difference. Habermas's embrace of a fallibilism that will "not rule out" the possibility of new transforming experiences is far too weak for Wellmer. Greatly informed by Adorno's sense of the "traces of contingency, opaqueness, and violence in linguistic meaning" and social life (1998, 233), Wellmer calls us beyond fallibilism to urge that cultivating radical receptivity and greater capacities for engaging the uncanniness of the future is crucial for the pursuit of truth, freedom, and justice. Wellmer pushes dialogical ethics and politics in fascinating directions that seek to resist the "compulsion to identify," by constructing constellations and "syntheses that will provide new stimuli for communication" and a "logic of 'nonreifying' argumentation." In the midst of a world structured around often odious modes of power, the capacities of finite beings for radical dialogue and pluralizing democratic politics requires repeated provocations that help open us to that which is defined as "the meaningless, the taboo, the socially excluded and heterogeneous aspects of their experience" (1998, 167).

Wellmer argues that these closures are very often anchored and perpetuated at the affective level. Hence Adorno's aesthetic theory becomes a particularly important site for cultivating our capacities for more generous engagements with excluded heterogeneity. The sublime in art is, from an "energetic" standpoint, "shocking"; it bursts into our interior space and "generates a tremor, a vertigo, loosening the confines of the experiencing ego" (163). It aids the paradoxical work—so important to a more generous democratic politics—of communicating the uncommunicable, and thereby allows us dialogically to experiment with modes of seeing, being, and becoming that have been marginalized and condemned. One could think here of modes of sexuality; abnormal ways of reflecting, carrying one's body, or expressing oneself in public; public presencing of religious corporate bodies and practices; spiritual celebrations; shocking personal and collective modes of registering socially unacknowledged suffering; rad-

ical solidarities with “illegal aliens”; taboo relationships toward death and dying; and wildly sympathetic relationships with nonhuman beings—for starters.

Wellmer argues not that democratic politics should be replaced by aesthetics but rather that, because power works significantly at levels other than that of linguistic communication, democratic discourses and struggles must *also* and importantly experiment with modes of expression and receptivity better able to engage these levels. We must bring them partly to the surface, expose them from new vantage points of vision and feeling that loosen the virtually automatic sway they often exercise on us, and open them to critical reflection and modification (1998, 167, 163).

In the United States, a resurgence of interest in Adorno parallels and extends many of these ideas. Rather than a theorist of nihilistic aesthetic subjectivism, Adorno emerges as a theorist with major insights for a dialogical democratic ethic and politics of a more “receptive generosity” (Coles 1997), which resists closures in Habermasian critical theory through unending engagements with that which remains nonidentical—beyond the limits of the dominant orders (Dallmayr 1992; Morris 2001; Phelan 1993; Schoolman 2001). In the more philosophical and ethical registers, what distinguishes such theories is an accent on the epistemological and ethical centrality of cultivating responsiveness to nonidentity, the importance of engaging aesthetic and affective sensibilities in these efforts, and an understanding of democratic dialogue and engagement that is more affirmative of agonism, paradox, and contingency.

For Adorno, all identities—concepts, selves, groups, communities, nations—fail to exhaust the materiality that they both emerge from and seek to make coherent and intelligible. Nonidentical remainders are engendered ever-again in our efforts to grasp and give form to selves, communities, and the world. Adorno’s point is not that all our efforts here are for naught or that they all inevitably share the same degree of blindness and violence. We can do better or worse—epistemologically and ethically—as we negotiate questions of identity. Yet even under the best conditions, Adorno suggests, we can never entirely leave blindness and damage behind us—and this truth grows in magnitude the more we attempt to deny it. The upshot of this insight of “negative dialectics” is that the most hopeful dialogical practices are those suspended between the tensional pulls of reconciliation between self and others and an intransigent resistance to all claims that pure reconciliation does or even can exist; those suspended between efforts toward consensus and ongoing incitements toward agonistic dissent. Both harmony and agon, when taken as primary ethical and political aims, tend to engender blindness and damage toward others—the former by concealing the damages of its claims to and yearnings for reconciliation, the latter by blinding itself to and sometimes fostering the violence that becomes visible through our yearnings for reconciliation (Coles 1997). Like many of Adorno’s oppositions, the poles of this tension that

might enliven more generous and receptive political dialogue, “keep faith with their own substance through their opposites” (1973, 15) and they “are linked by criticizing one another not by compromising” (61). This insight—this tension-dwelling—holds significant promise for moving beyond the mutually deaf oppositions that have become entrenched in encounters between some consensualist critical theorists and some agonal postmodernists.

In terms of political economy, the directions initiated by more difference-affirming critical theorists are often entwined with challenges to the dichotomous character of Habermas’s “lifeworld system” paradigm, and they tend toward more radical democratic and insurgent efforts to transform state power and markets.<sup>6</sup> As the costs of overemphasizing consensus come to light, so too do the weaknesses of overvaluing coordination in administrative and economic life. The claim is not that these are not crucial aims but rather that they must be situated, pursued, and limited in relation with other aspirations that cultivate democratic capacities and practices for dialogically elaborating modes of cooperation *and* ongoing possibilities for people on the undersides of power to contest these modes in radical democratic fashion. These efforts are also pursued by post-Marxists informed by themes of contingency, pluralization, and agon developed by numerous critical theorists and postmodernists.<sup>7</sup>

## ■ | Pluralizing and Radicalizing the Democratic Responsibility

Proximate themes are explored by numerous theorists frequently associated with postmodernism. Postmodernism is a highly contested concept, and many who are labeled *postmodern* resist the term. In fact, within political theory in the 1990s, *postmodern* perhaps coheres most in its frequent function as a term of dismissal.<sup>8</sup> In the hands of many of its critics, postmodernism names a knot of noxious themes: nihilism, anarchy, singular obsessions with difference, myopic identity politics, deconstruction re-

6. For a sample of more radically democratic arguments in this vein, see McCarthy 1991, ch. 6, Forbath 1998, and Coles 1997.

7. For example, Bowles and Gintis (1986), Laclau and Mouffe (1985), Reinhardt (1997), and Corlett (1998). Post-Marxist theorists such as these draw significantly on postmodern themes in the context of working through the theoretical and practical problems of western Marxism. Laclau and Mouffe have been especially influential during the past decade.

8. For liberal critiques of postmodernism, see Spragens 2000 and Moon 1993. For Straussian criticism, see Rosen 1987 and Zuckert 1996. For communitarian criticism, see Taylor 1984. MacIntyre (1990) criticizes from the vantage point of “tradition.” Moderate “New Left” criticism finds expression in Todd Gitlin 1995 and Rorty 1998.

duced to destruction, genealogy reduced to war, anti-Enlightenment, anti-universalism, and the abolition of normative foundations.

What is remarkable (but often unremarked), is that if you round up a list of usual postmodern suspects—say, Judith Butler, William Connolly, Iris Young, Jacques Derrida, Michael Shapiro, and Shane Phelan—for all their important differences, none will ascribe to this knot, and most recognize it as a noose and have repeatedly resisted it.

To provoke some distance from these dismissive stereotypes, consider first the nuances articulated by a few writers concerning their relationship to different aspects of modernity. Foucault writes of enlightenment as “a form of reflection within which I have tried to work” (1986, 96) and a dialogical “critique and permanent creation of ourselves in our autonomy” (1984, 44). Derrida writes: “It is a logic, logic itself, that I do not wish to criticize here. I would even be ready to subscribe to it, but with one hand only, for I keep another to write or look for something else, perhaps outside Europe” (1992, 69). He articulates and provocatively explores the tensional mixture of debt and genealogical suspicion in relation to dominant understandings of democracy, universalism, consensus, and numerous other themes and practices. Connolly argues: “We need new improvisations today, those that rework rather than eliminate secular, liberal practices of majority rule, minorities, progress, dissent, rights, sympathy, tolerance, and creative dissidence” (1999, 96). Far from being an antiuniversalist, Judith Butler writes that “the task that cultural difference sets for us is that articulation of universality through a difficult labor of translation” not governed by the a priori assumption that “a Kantian can be found in every culture” (1996, 52).

These positions express what Derrida calls a “double contradictory law”—the idea that justice, ethics, and promising democratic practice, call us to work within the difficult tensions of a double obligation: that of dialectically extending the most compelling moments of the inheritances of reason and justice *and* that of cultivating a more radical receptivity toward that which has been marginalized, excluded, colonized, or never imagined by these inheritances—or perhaps that which hasn’t yet emerged. The blindness and violence that accompany our finitude make *both* of these efforts necessary. Our judgement is likely best—or least bad—when developed in the tensions between the orientations of the best practices so far and the cultivation of a radical yearning to receptively engage challenges from other perspectives, practices, and struggles.

This double relationship is not a weakening of some purportedly more radical position that would wholly reject “modernity” in the name of “the new.” As Derrida says, thinking of the political disasters of the twentieth century, “we know the ‘new’ only too well.” Often what happens under this name is “the worst” (1992, 18). We need to dialectically protect and extend within any historical legacy those institutions, practices, and knowledges that prepare us to engage others receptively and to resist the worst. But we

know too, he suggests, of the endlessly renewed processes and pressures within our logos and order that exclude and wage war against others, otherness, and other possibilities *as such* in the name of fending off “the worst.” Hence, we must cultivate a stance that would dispose us toward a radical receptivity that “anticipates the unanticipatable.” Ethical-political positions that would resist evil and participate in opening a more generous future would be those that cultivate judgement and will in the midst of the tensions of this double contradictory law (Derrida 1992, 1994; Critchley 1992, 1998; Beardsworth 1996).

Connolly’s writing exemplifies the kind of rich political theory that emerges from a “bivalent tension” (1999, 71) and offers a sense of how such theorizing often moves back and forth between ontological, affective, ethical, and political registers. Deeply informed by Foucault’s genealogies of power, Connolly develops genealogies of the territorial politics of the nation-state, the drug and culture wars, capital punishment, the functioning of the secular in contemporary liberalism, and constructions of race. Yet he is powerfully aware that genealogy’s strategies of disclosure, detachment, and escape from hitherto invisible modes of power, while crucial, are insufficient for political theory. It is also necessary to cultivate one’s ethical and political commitments.

The work of projecting a “generous ethical sensibility grounded in appreciation of the fugitive abundance of being” (1995, xiii) is tacit in Connolly’s writing in 1991 and becomes increasingly central in his writings in 1995 and 1999. With a retrospective glance, Connolly argues both that all political orders have been implicated in substantial blindness and unacknowledged violence and that many of those people and modes of being which were excluded have struggled against these confines in ways that eventually enacted freer and more just and democratic possibilities for co-existence. Hence he prospectively wagers that current and future orders do and will similarly harbor unacknowledged exclusions; many of these excluded modes of being and becoming will likely harbor transformative possibilities for justice, freedom, and flourishing that exceed the terms and practices that currently legitimate their exclusion; and working against the grain of the closures we all almost certainly harbor in varying degrees toward peoples and modes of being that challenge the orders with which we identify, we would do well to cultivate a more radical “critical responsiveness” toward others and still inchoate otherness within and around us.

Connolly’s stance is both indebted to prevalent liberal democratic theory and very critical of it.<sup>9</sup> On the one hand, Rawlsians are “superb at acknowledging the justice of newly acknowledged claims and constituencies once the politics of becoming has carried their voices within range of [their] hearing” (1999, 68). The political liberal “moral code” of justice, including aspects of its conceptions of liberty, equality, reciprocity, tolerance, rights,

9. For a related critique, see Honig 1993.

democratic cooperation, and distribution, is often important in contemporary efforts to protect, inform, and empower movements for justice among diverse currently recognized groups. Yet while it is “indispensable to social regulation, judgment, and coordination, it is also too crude, blunt, and blind an authority to carry out these functions sensitively . . . particularly when new and surprising modes of suffering are encountered” (59).

Contemporary pluralism comes up far short due to the way it assumes the posture of a privileged and relatively static and definitive code. First, Connolly claims, it often blindly and dogmatically resists newly emergent struggles to register hitherto excluded groups and ways of being (e.g., illegal aliens, nonhuman beings, future unborn generations, and those who would bring “doctrines” into the struggles of public life to contest political liberalism). Second, even when it finally acknowledges the legitimacy of claims brought forth by some social movements, it tends to assimilate these claims to its own categories, remaining obstinately deaf to and unchanged by the challenges posed to its formulations. Many struggles around sexuality, for example, don’t simply aim at inclusion in the current definitions of reciprocity and personhood but seek also to transform them, as they foreground and “challenge the visceral experience of exclusive sensual naturalness upon which heterosexual identity had been based . . . and “elevate a new dimension of being into the rubric of personhood itself” (68).

These challenges would involve not only changes in heterosexual identity but also—by drawing to the fore the visceral, affective, and pre- and unconscious dimensions of people’s response to the world—challenge the cognitive rationalist assumptions that underpin many liberal (and Habermasian) formulations of just deliberative exchange, reciprocity, tolerance, and fairness. Transformations in these terms might, then, radically alter the understandings, sensibilities, and practices with which we aim at justice. Dialogues and struggles around justice would thus often have to engage in a micropolitics that seeks to transfigure the affective registers of our lives and social order. These alterations around specific relations like race and sexuality could cumulatively enhance our capacities for broader engagements in a more generous “politics of becoming” that stretches strenuously beyond itself to hear and be transfigured by others who are—and by otherness that is—barely audible to us.

Numerous theorists influenced by various strands of postmodernism develop related lines of analysis. Drawing on Levinas and Derrida, Michael Shapiro (1997) develops an “ethics of encountering otherness,” in the context of his efforts to unmap the “violent cartographies” of colonial encounters. These structures of visibility and invisibility—and related practices—remain powerful today, he argues, and they are perpetuated by the nation-state-oriented geopolitical map of “security studies” that conceals the extensive violence toward peoples excluded from this map. Iris Young (1997) develops an ethics of communication through the lens of “asymmetrical reciprocity,” in which listening and questioning come to play

prominent roles in helping us live more generously in the midst of deep differences and the deafness of myriad social practices.

Melissa Orlie (1994) draws on Nietzsche and Arendt to cultivate a political perspectivism that incorporates a plurality of embodied views in order more thoughtfully to respond to the will to power that necessarily attends our embeddedness in the world. Stephen White (1991), drawing on critical theory and postmodernism, articulates a notion of responsibility that negotiates between a responsibility to act and a responsibility to otherness. Through a Levinasian reading of Derrida, Simon Critchley (1992, 1998) cultivates an ethics that accents the blindness and violence enmeshed in even the best ethicopolitical projects.

Jane Bennett (1996) challenges the way aesthetics has been marginalized from many central modern “code only” ethics and argues that Foucault’s aesthetics of the self is integral to enhancing “the range of possibility in perception, enactment, and responsiveness to others.” Bennett shares the aim of an ethics more responsive to otherness, but she is wary of efforts like Critchley’s that she thinks overly accent the importance of “disenchantment.” She (2001) argues for an ethical sensibility that cultivates “enchantment” as crucial to inspiring and fashioning care for otherness.

Resonating with some recent critical theory informed by Adorno, these theorists argue that the conditions and practices of democracy and justice hinge vitally on our ability to respond to others beyond our horizons of clear vision and comfort. And this in turn requires arts of the self and micropolitical engagements aimed at transfiguring how we perceive and affectively respond to the world. Struggles played out on these registers then interact with macropolitical economic struggles concerning, for example, family law, medical benefits, agricultural practices, redistributive programs, immigration policy, urban design, environmental practices, relations of production, military practices, and health care.

Despite many commonalities, there are important differences among these theorists on numerous fronts, such as how much constructive ethical and political work is desirable, how to strike the balance between opening and structure, the degrees to which they accent the tragic and the playful, the ways they emphasize learning and struggle in democratic contestation, and their proximity and distance to different modes of everyday politics. Rather than sketch many differences, I explore in some depth differences between Connolly and Young to offer a sense of some of these stakes.<sup>10</sup>

10. Among the developments in recent critical theory I omit in order to work more carefully with less, of particular importance is the scholarship on Hannah Arendt. Much of this work engages debates proximate to those above. Hence, one finds Benhabib’s more consensual reading (1996a), Honig’s agonistic Arendt (1993), Villa’s agonistic accent (1996) tempered by Arendt’s reading of Kant’s theory of judgement. Numerous contributions in Honig 1993 explore tensions around difference, bringing Arendt to bear on issues of gender, sexuality, race, and so forth

In an effort to bolster a “politics of difference,” Young (1990) very insightfully explores ways in which power functions at the affective level. On her reading, explicit racism, homophobia, ableism, and other modes of power that earlier operated in conscious and visible forms have significantly receded, as “conscious acceptance” has increasingly gained sway in many public spheres. At the same time, however, “unconscious aversions” remain deeply lodged and in some cases intensify, in manners that often mark, degrade, and devalue numerous groups.

Drawing on Julia Kristeva (1977) and others, Young argues compellingly that the formation of identity is entwined with “abjection,” which produces and maintains the border that separates and defines it. “Abjection is the feeling of loathing and disgust that the subject has in encountering certain matter, images, and fantasies” (1990, 143)—that cross or threaten the borders by which selves and associated groups emerge and are sustained. These threats illuminate the contingency, arbitrariness, and always compromised existence of borders—the radical proximity of the dangerousness just on the other side of the vulnerable edge that is the condition of one’s identity.

Young argues that abjection is historically linked to group differences and structural power. However, “even if abjection is a result of any subject’s construction, nothing in the subject’s formation makes group loathing necessary” (1990, 145). Once fabricated, group abjection becomes an insidious force undergirding social relations (influencing our sense of attractiveness, intelligence, competence, maturity, respectability, and thereby influencing hiring, promotion, voting, racial profiling, punishment, etc.). Yet its link with groups and power may be severed or at least greatly attenuated, by working to acknowledge heterogeneity within the groups with which one identifies and by working in a variety of settings to transform the unconscious aversions between groups. Young “does not wish formally to regulate” aesthetic and affective expression and judgment, “because the dangers to liberty are too great. . . . The injunction to ‘be just’ in such matters amounts to no more and no less than a call to bring these phenomena of practical consciousness and unconsciousness under discussion, that is, to *politicize* them . . . fostering politicized cultural discussion, and making forums and media available for alternative cultural experiment and play.” Yet, depending on what she means when she refers to “a kind of social therapy” and “consciousness raising” (1990, 152–53), a host of questions begin to emerge—including those from Foucaultians concerned about the modes of power lodged in many forms of therapy.

These questions are linked to others concerning institutions and prac-

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Questions of democracy and civil society are explored in Issac 1998; themes of narrative and politics in post-Archimedean times in Disch 1994; the relation between ontology, aesthetics, and ethics in Arendt’s thought as an alternative to the enclave politics of late modernity in Curtis 1999.

tices of pluralization and coalition. Young has tended to focus on a politics of coalition structured around various kinds of institutional modes of group representation, caucuses of oppressed groups, reserved seats, group veto power, and so on. More often than Connolly, she accents themes of constructive political and social organization. He too affirms social movements and coalition politics, but he tends to gesture, with the Deleuzian term *assemblages* (Deleuze and Guattari 1987), toward looser, more multiplicitous, and perhaps more dynamic sets of alliances, saying less about institutional forms and requisites. One should not exaggerate these differences; and neither is deaf to the concerns of the other (contrary to their exchange in Young 1992 and Connolly 1993). Yet they are not insignificant. I suspect that Connolly eschews “therapy” and avoids endorsing some of the above mentioned institutions of group representation in part because his work emphasizes themes that accent questions concerning the ways some such institutions might reify groups and pose a barrier to future possibilities of critical responsiveness, by concentrating power around some insurgent relational groupings at the expense of others. To gain deeper insight into the political differences and further illustrate how the work in each different register has implications for the others, we must return to Young’s and Connolly’s thoughts on identity and exclusion.

As Stephen White (2000) reminds us, there are no seamless arguments from these ontological-ethical reflections to very particular political practices and institutions. There are only tendential workings to and fro between questions and concerns that emerge at the various registers of political theory and political practice. Yet the political differences at hand are, I suspect, partly prefigured by the philosophical reflections under discussion. For Young, abjection may be unavoidable in selves, but it is not necessarily linked to groups. While group-oriented abjection is central now, she accents a political horizon of the coexistence of differences beyond exclusion. There would still be opacity, ambiguity, and distance, but group relations would not, for the most part, be implicated in power and marginalization. Her ethical-ontological reflections foreground the idea that group differences are “often compatible and might enrich everyone’s understanding when they are expressed” and deemphasize the likely reemergence of “othering” (1990, 189).

For Connolly, however, while difference beyond exclusion is an important *point* in the constellation of his ethical-political yearnings, his horizon is constituted by the *tension* between this idea and a sense that significant exclusionary compulsions will continue to accompany every (including group) identity. This latter sense is rooted in interpretations of history, the precariousness of the human condition, and resurgent drives to security. It suggests that identities, which are always significantly related to social relations and power-laden recognitions among groups, will endlessly tend to (re)engender pressures toward marginalization, abjection, blindness, and unacknowledged violence.

We can and should resist these tendencies through multiple ethical and political practices; and coexistence can be less violent, richer, and ethically transformative. Yet these possibilities wane when we cease to attend to the tragic dimension that always likely accompanies them. Negative dialectics, receptive generosity, and critical responsiveness solicit a vigilant and ever-renewed witness not only (with Young) to the rich possibilities beyond each relational identity but also to the seemingly ever-renewed energies of exclusion (which Young downplays). Having suggested these connections, it is important to note that they are indeterminate and malleable, as evidenced by Young's recent shifts (2000, ch. 4)—in the face of empirical criticisms—toward more “fluid” and “pluralizing” modes of group representation.

Shifting the angle of interrogation, the danger risked by genealogical positions like Connolly's is that their suspicions concerning power lurking in all relational identities and practices, as well as their (related) yearning for a futural and inchoate responsiveness, might tend to inhibit the development of needed constructive theorizing concerning political mobilizations, practices, and institutions. Since the mid-1990s, Connolly and several of the other theorists are moving in directions that articulate the tensions between dialectical construction and genealogical responsiveness in promising ways. But these developments in critical theory and postmodernism are still very young, and the difficult work of entwining genealogy and construction is just beginning to emerge. The extent to which what currently seems promising might also generate significant barriers to needed developments remains to be tested in coming efforts at theoretical articulation and ethical-political practice. Constructively, Young (2000) is making increasingly incisive contributions to questions of representation and institutional design. This is especially true in her most recent discussions of local participation, region governance structures, and multidimensional global democracy, where possibilities for democratic “inclusion” are explored in a manner that is both radically transformative and institutionally specific.

## ■ | Politics of the Double Gesture between Critical Theory and Postmodernism

Further light is shed on the political promise of the ontological-ethical “double contradictory law” or “bivalent tension,” by interrogating the contestation between two theorists who are simultaneously very insightful and limited by some of the dangers that reside in Habermasian critical theory and genealogy. Kenneth Baynes, a Habermasian, criticizes Wendy Brown, a radical democratic genealogist, for constructing what he believes is a critique of rights that concludes “that the language of rights is likely to do

more harm than good, that rights codify and reinscribe the very powers they were designed to confront, and thus that radicals and progressives should think twice before including rights within their emancipatory projects" (2000, 451). Baynes thinks Brown's skepticism toward rights continues Marx's critique of how (particularly proprietary) rights can depoliticize and naturalize material inequalities and conflicts of civil society, construct an illusion of liberty, equality, and community in the state, and ideologically disguise the complicity between state and capitalist power. Moreover, inspired by Foucault, Brown argues that contemporary rights discourses often "converge with the disciplinary production of identities seeking them": rights become "disciplinary modalities of power producing the very subjects whose rights become a method of administering them" (2000, 118).

Baynes's alternative account (1992), drawing on Habermas and LeFort, emphasizes how democratic practices and rights are cogenerative and "mutually suppose one another." The paradoxes surrounding rights are part of a constructive dialectic of deepening and broadening democratization. By creating "new modes of access to the public sphere," by recognizing, protecting, and sometimes even soliciting myriad associational possibilities, and by continually "outrunning themselves" such that each right creates a basis for new rights—rights enhance freedom, equality, and practices of *discursive democracy* engaged in interpreting their scope, meaning, application, and so on.

Brown claims that Baynes misreads her. She offers not a totalizing critique of rights but rather a critique articulated specifically in relation to the politics of identity and injury. Baynes ignores the strong dialectical twist in Brown's argument that rights "may be most effective to the degree that they remain empty of specific content . . . articulate a political universal that, as an ideal or a vision, operates as a critique of status quo inequalities and hence as an incitement to address those inequalities politically rather than legally" (2000, 469). Driven by the Habermasian underestimation of both the extent to which power operates through the production of subjects and the need for a politics that contests these productions,<sup>11</sup> Baynes's positive dialectic "soothes" modern "anxieties about critique," but it marginalizes the genealogical investigations and democratic struggles that might empower more emancipatory democratic projects. Democracy gets reduced to the "agenda of elected officials."

*Ships passing in the night.* Brown is less dismissive and more dialectical than Baynes suggests, and Baynes is more cognizant of power and political contestations beyond parliamentary discourse than Brown recognizes. More importantly, their projects—and critical theory and genealogy more generally—might mutually inform each other far more than they do. Brown's genealogies might radicalize the critical theorists' democracy-rights dialectic in ways that more explicitly and reflectively address the pol-

11. For this argument, see also Cruikshank 1999 and Dumm 1987.

itics of subject formation and the ways some articulations of rights risk undesirable complicities with it. Genealogies of power and complicity might move critical theorists toward more robust contestations of often-unrecognized modes of power in civil society (including markets). In turn, this radicalized dialectic might enhance Brown's political critique in manners that are more specific, complex, and nuanced concerning the mobile relations between "empty" and "specified" rights in contemporary struggles, which often contain many of the dynamic, broadening, and politicizing impulses suggested by Baynes, even as they often harbor dangers suggested by Brown.

I am *not* suggesting that the dialectical critical theory and genealogy can or ought simply merge. They cannot and ought not. Each performs works of affirmation and critique that should remain irreducible to the other. Genealogy must strive beyond the constraints of even the most radical dialectic. And dialectical theory, to develop affirmative energy and direction, requires more explicit horizons.

Yet mutual irreducibility need not mean dismissive deafness. Might not Baynes's one-sidedness be entwined with a faith in humans' capacities for a highly consensual and rational communicative dialectic that leads him often to avoid—and become anxious in the face of—some of the deepest considerations of power, blindness, and contingency that might call this faith into question? Might not Brown's equally resolute, maybe even anxious, avoidance of some of Baynes's insights be engendered by a tacit faith in a genealogical vision that too singularly identifies human freedom with incessant unmasking and too often perceives the identities of selves, groups, and practices through a lens that reduces them too much to an effect of disciplinary power?

Alternative ontological-ethical articulations of a bivalent sensibility and double obligation might better prefigure the work of an analogous double gesture in more directly political registers: a progressive politics that develops *both* textured critical dialectical ideals and institutions *and* a genealogical and deconstructive responsiveness that exceeds these ideals and exposes them to their limits. Presently, this might imply the development of a radical dialectic of rights and democracy *and* genealogical inquiries into their limits and insidious complicities with power. Each pole should be transfiguratively informed, invigorated, and chastened by the other. Careful judgment is enhanced by these discrepant impulses and the lively paradoxes, tensions, and partial mediations between them. Theorists will weight and mediate these poles and tensions differently, and therefore the agon between critical theorists and radical democratic genealogists will continue. But it would be more edifying if each side listened to and worked more with insights cultivated by the other.

Is this a kind of "reflective equilibrium" (Rawls 1971) for radicals, between discourse ethics and genealogy, such that the movement to and fro between different levels of inquiry gradually adjusts each so that they might

form a coherent framework? Efforts to interrogate incoherence *are* an indispensable *part* of political theory. Yet the double gestures under discussion, including the reciprocal adjustments and interrogations between double gestures at different registers of theorizing, exceed this singular imperative. Instead, the aim should be to figure each register of inquiry and the relations between them so that they *also* solicit a certain *disequilibrium* that enlivens our judgment in the midst of essential but conflictive impulses and insights. Moreover, instead of aiming exclusively to formulate a position that is *defensible* (*defensive*), the disequilibriums of the double gesture are constructed, in no small part, in order to enhance our receptive capacities and our vulnerability to other voices. The ethical vice of unreflective deafness is the congenital defect of every imagined equilibrium.

The political promises of these double gestures are apparent in considerations of democracy above, beneath, and across the boundaries of the nation-state. Habermas has recently argued that the protection of individuals requires the protection of their culturally specific “intersubjective life contexts.” This implies giving serious attention to the demands of minority cultures within states for political autonomy, rights, and benefits. Above the state, he theorizes postnational civil societies “composed of interest groups, nongovernmental organizations, and citizen initiatives and movements” that address transnational problems and generate shared political cultures, in relation to which institutions of regional integration and associated political parties are beginning to develop (1998a, 153).

He formulates provocative institutional designs for “cosmopolitan democracy” that might, prodded along by social movements, move beyond the fetishization of state and interstate relations. Hence, for example, he advocates a second chamber in the U.N. General Assembly, in which “peoples would be represented . . . not by their governments but by directly elected representatives. Countries that refuse to permit deputies to be elected by democratic procedures (giving special consideration to their national minorities) could be represented in the interim by nongovernmental organizations appointed by the World Parliament itself as the representatives of oppressed populations” (1998a, 187). He argues for a World Court that can initiate prosecutions and is no longer restricted to considering relations between states. Young’s (2000) vision of multidimensional and multilayered democratic “global regulatory regimes” adds an important set of tasks to this general agenda. Habermas senses the limits of proposals framed in analogy to national constitutions, and calls for “more institutional imagination” (2000, 188).

Recent works by genealogists such as Michael Shapiro (1997), who relentlessly exposes and works beyond “violent cartographies” of our statist political imaginations, and Bonnie Honig’s explorations (2001) of the relationships between democracy and foreigners, might significantly spur institutional and noninstitutional imaginations. Searching for democratic practices that might better resist xenophobia, Honig explores the idea of

the “*taking* foreigner”: “an honorific democratic practice—that of demanding or, better yet, simply enacting the redistribution of those powers, rights, and privileges that define a community and order it hierarchically. Here the iconic taking foreigner puts foreignness to work on behalf of democracy by modeling forms of agency that are transgressive, but (and therefore) possessed of potentially inaugural powers. Carried by agencies of foreignness, this revalued taking stretches the boundaries of citizenship and seems to call for a rethinking of democracy as also a cosmopolitan and not just a nation-centered set of solidarities, practices, and institutions” (2001, 8). Note how this might complicate the borders of minority as well as majority cultures.

Such analyses might stretch the imagination further beyond the limits of sovereignty than Habermas has yet to venture. In combination with insights by Tully (1995), whose discussion of constitutionalism is informed by indigenous peoples, common law, postmodernism, and feminism, we might be drawn to explore dissipations and pluralizations of sovereignty that work with and beyond liberal democratic developments. Liberals like Pogge (1994b) and Kuper (2000), parallel to Habermas, argue that vertically dispersed powers of sovereignty in nested territorial units can better accommodate various allegiances and facilitate the exercise and protection of person’s liberal rights. Beyond Pogge, however, Kuper suggests that liberals supplement territorial conceptions with the further dispersal of sovereignty horizontally according to functional requirements and tasks (see also Dryzek 1990, 1996). “Our practical task is to gradually pluralize the global basic structure by creating a variety of forms of democratically responsive, semiautonomous legal authority. . . .” (Kuper 2000, 666).

More genealogical cosmopolitanism(s) (in conjunction with some of the best postcolonial theorizing [Mignolo 2000]), by accenting the importance of vulnerability and responsiveness toward others and otherness within and beyond the democratically institutionalized matrix of groups, scales, and issues, might further disperse and push democratic theory and practice in ways that further register the *dissonant dynamic* (and thus *temporal*) struggles and suppressed solidarities within any “now,” no matter how territorially, culturally, and functionally variegated its structure may be. In a sense, even Kuper’s functional dispersion remains too territorial/sovereign, insofar as it projects a relatively static spatial matrix onto the realm of “issue areas.” He takes the territorial model and simply turns it at a right angle to imagine horizontally as well as vertically dispersed structures of sovereignty. This is crucial and has much to offer.

Yet his spatial metaphors may reinforce too great a drive toward consolidation and integration in institutional designs. A more receptive generosity and agonistic democracy seeks often to question (and sometimes to resist) this, in part by rearticulating a more dynamic and less structured democratic situation that is perhaps better evoked by a nonlinear and tensional democratic *temporality* among beings and events moving and strug-

gling according to many distinct and often dissonant drummers (Tully 1995). These temporalities variously overlap, reinforce, encroach on, and resist one another, such that even under the best conditions at any moment, the negotiations, agreements, institutions, and directions that are forged will likely fail to respond well to many questions, peoples, and admirable alternative possibilities. The aim is to infuse the presence of all theories and practices with a sense of absence. As Derrida puts it, "*What is proper to a culture is to not be identical to itself. There is no cultural identity without this difference with itself*" (1992, 14).

Democratic practices inflected by the best in deconstruction approach this temporal dispersion and suppression in each moment, culture, and institution as a solicitation, against whatever odds, to reopen their structures of present responsiveness to fresh negotiations with those clamoring within and beyond the problematic constraints, productions, and gates which have arisen. They seek not only to maximize responsiveness in institutional designs (though they contribute to this too, by repeatedly rendering unresponsiveness more visible) but also to cultivate a capacious sense for, and yearning to receptively engage, those to whom our responsiveness has as yet failed to engage. More concretely, this means expanding our capacities to form and engage groups, movements, and nascent practices that cut across and seek to radically reform (or sometimes even abolish) the jurisdictional "spaces" of established institutions. It means enhancing our capacities to affirm that democracy is in no small way the dynamic messy coexistence of practices and institutional spaces that contest as well as complement one another.

Yet how might this ethos articulate itself in relation to institutional designs? In addition to "inclusion," toward what constructive democratic practices might social movements, indigenous peoples, marginalized traditions, and emergent groupings aim in order to enhance capacities for responsiveness to alterity? How might various constituencies aim to intensify the transfigurative exposure of institutions to that which lies beyond their limit? Let me briefly suggest three possibilities that begin to emerge when we begin to think and act in the tensions between critical theory and genealogy.

First, we might aim to design practices that seek to make people situated in dominant institutions experience these institutions and themselves as being "otherness to be included," rather than fortifying an unquestioned sense that they and these institutions unproblematically occupy the central position of "the includers." Consider public hearings, for example. The most usual formats for inclusion provide an opportunity for individuals and associations to listen to officials and provide comments in settings designed and presided over by representatives of the institution involved. Struggles from more genealogical perspectives, or "border thinking" perspectives emerging in numerous postcolonial sites (Mignolo 2000), might seek to transform institutions in ways that required representative bodies regularly

to attend public meetings designed by and presided over by the most-marginalized groups in those groups' chosen public spaces. This is prefigured by the meetings currently conducted by grassroots urban coalitions associated with the Industrial Areas Foundation, where public officials meet the social movement on its terms, times, and terrain. What if such practices of engagement between governing institutions and the least well-off were required at regular intervals, so that, for example, representative bodies had to meet with indigenous peoples in places, times, and modes chosen by the latter?

Second, we might aim to design variegated institutions in ways that have more space for tensions between different jurisdictions, scales, groups, and issue regimes to be given a hearing and negotiated. This would render more indeterminate the partial sovereignties and inside/outside boundaries of institutions, in ways that would provide more openings for groups relatively disempowered by given institutional jurisdictions to gain voice and power. Instead of imagining variegated institutions as nested within an overly (neo-Hegelian) systematic framework where domains are clearly demarcated and each layer fits neatly within limits governed by layers higher up, sites might be designed where more interactive powers might emerge. Young's design, wherein "regional government sets a framework for inter-local negotiation, conflict resolution, and cooperation whose issues are on the local, as well as regional, public agenda" (2000, 234), provides an example that might kindle our imaginations of other interinstitutional relations that facilitate engagements between public bodies that are not included in one another.

Third, in conjunction with engagements stemming from sensibilities and practices like those sketched above, institutions might be designed in ways that enhance their capacities *in relation to the capacities of others* for ongoing dynamic transfiguration in response to those whom they disempower at any given point in time. The ever-challenging and essentially translucent aim here is to orient this process of transformation in ways that secure rights and practices of currently established freedom and justice to protect against the bad, while opening them to changes that increase their responsiveness and accountability to those they poorly address.

These modes and institutions of responsiveness may sometimes pose serious threats to smooth coordination. But responsiveness may also often disclose unwonted solidarities that can enhance coordination. In any case, smooth coordination may not always be the most ethical or politically desirable goal. It certainly is *not* when it comes at the expense of justice; and justice and responsiveness are tightly entwined. What the most promising theorists under discussion seek to cultivate, above all else, are our democratic capacities to be more receptive and generous in relation to the *questions* concerning the damages and suppressed possibilities typically concealed by the dominant paradigms of political inquiry.

# *Legislatures as Political Institutions: Beyond the Contemporary Congress*<sup>1</sup>

For the bulk of political scientists today, the study of legislatures is the study of the U.S. Congress. Other legislatures do exist, of course. The U.S. states have legislatures. The U.S. cities have legislatures. National, provincial, and local governments throughout the world have parliaments, representative assemblies, and legislatures. Even Europe—and, on rare occasions, the world assembled as the United Nations—has a legislature. But the scholarly world of legislative studies is, overwhelmingly, a world that studies the U.S. Congress. And the study of Congress tends to be the study of the postwar House of Representatives.

Although this generalization fairly approximates the contemporary field, it was not the state of the discipline at the turn of the last century. In that earlier time, when the study of legislatures similarly flourished and enjoyed comparably high stature within the broader discipline, scholars studied various national legislatures. Moreover, studies of Congress were bicameral, historical, and grounded in comparisons with other nations as well as the U.S. states. Wilson (1885) and Lowell (1902) examined the U.S. Congress through the prism of the British House of Commons, and Lowell analyzed data that extended back to the time of the Civil War. Follett (1896) and Fuller (1909) studied the House Speakership by examining its historical development, and each scholar compared the office to antecedents in Britain and the American colonies. Similarly, McConachie (1898), Alexander (1916), and Harlow (1917), in their studies of rules and legislative organization, collected their evidence from state legislatures, other countries, and congressional history.

This paper attempts to assess the state of legislative studies in our own

1. The first draft of this paper was presented at the Annual Meeting of the American Political Science Association, August 31–September 2, 2000, Washington, D.C. We are grateful for helpful comments from Alison Alter, Randy Calvert, John Carey, Gary Cox, Daniel Diermeier, Barbara Geddes, Ira Katznelson, Gerhard Loewenberg, Helen Milner, Paul Pierson, and Ken Shepsle. We are also grateful to Joel Andersen for his assistance in surveying the journal literature.

time. Unlike many such essays, however, our primary purpose is neither to provide a comprehensive review of the vast literature nor to summarize all recent advances and debates in the field. This daunting task has been admirably accomplished by others, either in broad surveys or in more-focused essays on particular areas of legislative research.<sup>2</sup> Instead, the main objective of this essay is to understand the implications for legislative studies of the dominance of work on the contemporary Congress. The most obvious implication, we believe, is the extent to which congressional scholarship has served as a model of positivist, rigorous, scientific research for the more general study of legislatures—and the extent to which congressional scholarship has sustained the discipline’s interest in institutions during an era otherwise dominated by behavioral work. Also, we contend, the breadth of research on Congress—drawing on participant observation, roll call analyses, interviews, rational-choice theory, and even history—has demonstrated the importance of embracing multiple approaches to analysis.

But the primacy of congressional research has also exacted costs, primarily in biases that are created in the types of questions scholars pose and in the types of answers they provide to these questions. Important avenues for legislative research, for example, include efforts to explain the choice of institutional arrangements or to analyze the impact of institutions on the behavior of individuals, other political institutions, or policy. Although research on Congress has often promoted these goals, we believe it has also frustrated them. In this essay, we discuss how this has occurred and how recent research has begun to move beyond some of the biases inherent in a Congress-centered approach.

## ■ | The Dominance of Congress in Legislative Studies

Legislative scholars have for some time pointed to the fact that the modern study of legislatures has become inexplicably intertwined with the study of the U.S. Congress. Thus Mezey argues that “most of the legislative literature is firmly rooted in time and place, and for much of the subfield the place is Washington, D.C.” (1993, 356). He argues that this leaves legislative research “theoretically impoverished” (357), identifying a need for more comparative work. In Hedlund’s 1983 review essay on the organizational attributes of legislatures, he finds that the overwhelming emphasis is

2. For an excellent overview of the literature, see Mezey 1993. *Legislative Studies Quarterly* has recently been publishing a series of useful survey articles, such as Rasch 2000; S. S. Smith 2000, and Hibbing 1999. These and other essays are collected in the volume edited by Loewenberg, Squire, and Kiewiet (forthcoming). For reviews of the positive theory of congressional institutions, see Shepsle and Weingast 1995. For a review of much of the literature on the European Union, see Moser 1999b.

on Congress and he calls for increased research on subnational and foreign legislatures (358). And when *Legislative Studies Quarterly* was launched in 1976, Malcolm Jewell, in the Editor's Introduction, complained that "the growing wealth of information and analysis about the U.S. Congress has not been matched by comparable studies of U.S. state legislatures" (1976, 2). Consequently, a central reason for the creation of this journal was to encourage research on assemblies other than Congress.

Despite these calls for change, recent history shows a field that remains firmly rooted in the study of Congress. One way to see this dominance empirically is to look at the recipients of prizes that are given in the field of legislative studies. Since 1988, when it established the Fenno Prize and the Congressional Quarterly Press Award, the Legislative Studies Section of the American Political Science Association has awarded prizes to nine books and to ten papers, designating them the best work of their kind in "legislative studies." Without exception, every one of these nineteen books and papers has been about the U.S. Congress—and, laying aside formal theory, Congress only.<sup>3</sup>

Indeed, this Congress can itself be a rather narrow creature. In the world of legislative studies, Congress tends to be a one-house legislature that came into existence in 1947. Of the nine books that have won the Fenno Prize, just one emphasizes the Senate (Sinclair 1989) and none draws extensively on pre-1947 history. The ten award-winning papers include just two Senate papers and one historical paper. The field of legislative studies is anchored firmly in the study of the postwar House of Representatives.

The history of these two awards neatly summarizes the dominance of congressional research in the modern study of legislatures. Examining all the articles published over the last few years in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics*, we identified a total of 110 empirical articles on legislatures (not including another 23 articles that presented formal models of legislatures without significant empirical content).<sup>4</sup> Of these 110 articles, 94 were solely about a U.S. legislature. Just 16 of these articles incorporated a legislature outside the United States—and even some of these 16 included Congress in their analysis. Of the 94 U.S. articles, 71 looked only at Congress, and 55 looked only at the contemporary Congress. (Surprisingly, at least to us, the journal literature contained more articles on U.S. states—23—than on all non-U.S. legislatures combined.)

*Legislative Studies Quarterly* demonstrates a similar bias toward Congress. Of 87 articles published since 1995 (not including 8 formal models),

3. For a list of award winners, see [www.apsanet.org/about/sections/pastwinners/section3.cfm](http://www.apsanet.org/about/sections/pastwinners/section3.cfm).

4. For this analysis, we looked at the *American Political Science Review* since 1993 and the two other journals since 1996.

65 looked only at the United States.<sup>5</sup> Of the 65 U.S. articles, 39 examined the contemporary Congress, 4 drew on congressional history, and 22 analyzed state legislatures. The remaining 22 articles were either cross-national or focused solely on a non-U.S. legislature. Although less pronounced than the bias demonstrated in the awards presented by the Legislative Studies Section and in the first-tier general journals, the publication record of *Legislative Studies Quarterly* confirms the dominance of congressional scholarship in legislative studies. Even in this journal, established in part to emphasize the study of legislatures other than the U.S. Congress, fully half of all scholarship is devoted to this single national legislature.

## ■ | The Evolution of Modern Legislative Studies

The modern study of legislatures traces its roots to the post–World War II era. In the 1950s and 1960s, even as the behavioral revolution transformed our understanding of voting behavior, several political scientists worked to develop a parallel body of scientific work on institutions. While some scholars studied the presidency, the courts, local governments, bureaucracies, or non-U.S. institutions, the largest body of work focused on the U.S. Congress. Between 1957 and 1961, White published *Citadel* (1957), Truman published *The Congressional Party* (1959), Matthews published his article on Senate folkways (1959) as well as *U.S. Senators and Their World* (1960), and Huitt published his three seminal articles on Senate norms and leadership in the *American Political Science Review* (1957, 1961a, b). This literature was surprisingly cohesive. Apart from Truman 1959, all of it described and analyzed the postwar Senate, all of it emphasized the centrality of norms to the Senate, and all of it drew on sociological theory.

The emphasis on the Senate proved to be short lived. With the important exception of Ripley (1969a, b), legislative scholars in the 1960s and 1970s turned their attention to the House of Representatives. In his 1962 article and in *The Power of the Purse* (1966), Fenno examined the internal operation of the House Appropriations Committee. In this work, with its focus on norms and its sociological framework, Fenno was clearly influenced by the earlier Senate studies. Fenno's research on the House occurred within a rich scholarly context. A major set of scholars in the 1960s and 1970s—including Ripley (1967), Jones (1970), Manley (1970), and Peabody (1976)—also analyzed the postwar Congress, with special attention to the House.

Although emphasizing the contemporary Congress, most of this work was infused with an appreciation for history. Indeed, a surprising number

5. Joel Andersen collected these data from *Legislative Studies Quarterly*. We are grateful to him for his research assistance.

of articles and monographs published in these years emphasized historical change or reconstructed an episode or norm from the congressional past. Much of this work was by historians—such as Galloway (1962), Donald (1965), Rothman (1966), Livermore (1966), Silbey (1967), Patterson (1967), Merrill and Merrill (1971), and Benedict (1973)—produced in an age when historians still valued the study of elite political institutions. But political scientists also produced a significant scholarship in congressional history—including Jones (1968), Abram and Cooper (1968), Cooper (1962, 1970), D. Price (1971, 1975, 1977), Polsby (1968), Polsby, Gallaher, and Rundquist (1969), Ripley (1969a), and D. W. Brady (1972, 1973), Brady and Althoff (1974), and Cooper and Brady (1981a, b).

Legislative scholars in this era were self-conscious in their aspirations to develop a positivist study of the field. Thus Fenno situated himself among scholars whose “main thrust is descriptive rather than reformist” (1966, xvii). And Huitt and Peabody, in characterizing the landmark Study of Congress project, insisted that “reform is not the target of this research project.” Rather, they hoped that the various books produced by this project would simply “tell interested people as much as possible about how Congress works” (Ripley 1969a, v–vi). Through the 1970s, this positivist mission was pursued through a scholarship that was thickly descriptive and often grounded in the tools of sociology. Fenno (1962) and Polsby (1968), exemplars of this literature, explicitly grounded their studies in social theory and organization theory.

Two books, Fenno’s *Congressmen in Committees* (1973) and Mayhew’s *Congress: The Electoral Connection* (1974b), marked a major reorientation in the study of Congress. Together, Fenno and Mayhew demonstrated the effectiveness of studying Congress with an approach that borrowed more from economics than from sociology, an approach drawn from the growing field of rational-choice theory. “How to study legislative behavior is a question that does not yield a consensual answer among political scientists,” Mayhew (1974b, 1, 5) wrote in the introduction to his book. “Mostly through personal experience on Capitol Hill, I have become convinced that scrutiny of purposive behavior offers the best route to an understanding of legislatures—or at least of the United States Congress.” Fenno, in a long footnote, recognized that his new emphasis on “goal seeking by the members” represented a sharp break from his earlier work, where he drew on “the literature created by people studying social systems” (1973, xvii n. 1). Famously, Fenno argued that legislators were driven by one of three major goals: “re-election, influence within the House, and good public policy” (1). Mayhew, of course, argued that the reelection goal was itself sufficient to explain the broad contours of member behavior.<sup>6</sup>

6. Mayhew looked beyond the reelection goal, though, in explaining the internal organization of Congress. Leaders and prospective leaders are also motivated by institutional prestige and power, according to Mayhew (1974b, 145–46).

In the late 1970s and 1980s, growing numbers of rational-choice theorists examined questions relating to legislative organization. But their work, often technical and abstract, remained inaccessible to many students of actual legislatures. At the same time, congressional scholars increasingly followed Fenno's and Mayhew's models of study, analyzing the implications of rational, individual actors, rather than describing the workings of collective social systems. Shepsle and Weingast (1987), explicitly utilizing the tools of rational-choice theory to address a central question in congressional organization, demonstrated the extent to which work in formal theory had converged with work on Congress.

Although they were writing during the heyday of congressional reform, when the basic institutions of Congress were undergoing historic change, scholars generally described a Congress dominated by strong, autonomous committees, where party leadership was irrelevant. With the advantage of hindsight, we can see that this depiction was quickly becoming anachronistic in the 1970s. Yet this was Mayhew's classic depiction of the institution in his 1974 book, where he emphasized the centrality of committees and noted that parties "are more useful for what they are not than for what they are" (1974b, 97). Six years before, in his article on the "institutionalization" of the House, Polsby (1968) had described an institution that was fully mature. Fenno's work (1973) on the committee system began with assumptions about legislator goals, which were pursued against a fixed institutional setting. And Fenno's exploration (1978) of "home style," where he analyzed the behavior of members in their districts, took the institutional environment as a given. A decade later, Shepsle and Weingast (1987) were still navigating this same institutional world.

In 1989 Shepsle acknowledged that this "textbook Congress"—the Congress of weak party leaders and strong committee chairs—had begun to come apart in the 1960s, though its image had continued to linger in scholarship for many years after. Many scholars, of course, had recognized the change as it was occurring. Analyzing the majoritarian revolt against Rules Committee chair Howard W. Smith in the early 1960s, Jones (1968) called attention to an early skirmish in the battle for reform. Many congressional scholars described the reforms as they were implemented in the 1970s, and others began to assess the impact of institutional change on legislators' actions. Sinclair (1989), Smith (1989), Rohde (1991), and Aldrich (1995) all compared the congressional worlds before and after reform to explain changes in member behavior. They sought also to explain why the institution had changed. A similar body of scholarship emerged after Republicans gained control of the House and Senate in 1995. In a short essay, Fenno (1997) examined why House Republicans experienced problems in governing when they moved into the majority. And, in a fascinating study of a southern congressional district that he had originally visited in the early 1970s, Fenno (2000) demonstrated how changes in the media and legislative careers affect a member's district style.

The transformation of the House and Senate in the 1970s and 1990s forced scholars to recognize that the institution was not static, that stylized facts were plastic. Changes in the institution itself led to changes in scholarship, as Rohde (1991) and Cox and McCubbins (1993) developed new theories to account for the prominence of parties in congressional decision making. As congressional scholars reckoned with the historic shifts of the late twentieth century, historical scholarship attracted new interest. Poole and Rosenthal's sweeping 1997 study of ideological shifts in Congress is grounded in an exhaustive study of roll call votes, and their database has become a rich mine for other scholars.

But most scholarship on the Congress has remained ahistorical. Affected more by the rational-choice revolution than by any revolution in subcommittee rights, closed rules, or filibusters, most congressional scholars have continued to study short periods of time in which the institution itself could be held constant. The study of Congress has become increasingly scientific, with the generation of explicit propositions to test, the creation of new data sets for testing these propositions, and the utilization of sophisticated methods to adjudicate substantive debates. We see this, to take one example, in studies of individual and committee preferences, where scholars have developed an impressive array of arguments about how to use (or not use) roll call voting data to measure preferences. We also see it in studies of legislative procedures, the relative influence of parties and exogenous preferences in shaping voting behavior, and the impact of partisanship on leadership and elections.

For some, the upshot of this evolution is that congressional studies have become too specialized. Mezey, for instance, objects that with "each article on each topic, the questions seem to get narrower, the issues more arcane, and findings less relevant to all but the small group of scholars pursuing that specific line of research" (1993, 356). For others, of course, this narrowness is the hallmark of a mature subfield engaged in rigorous normal science. From this perspective, the narrowness of many research projects is inevitable if one really wants to be able to distinguish with confidence the validity of competing empirical claims.

It seems odd in some ways that mature normal science would emerge primarily through the study of a single legislature. After all, if the applicability of legislative theories is limited to a specific legislative arena (e.g., the House) during specific periods (e.g., the postwar period), then it seems doubtful that these theories can be deemed in any sense general. But it is precisely the goal of a mature normal science to produce general theories that are not sharply constrained by time or space. This is the concern that dominates Mezey's review of legislative studies. He points out that "one can no more have a theory of legislatures that applies only to the United States Congress than a theory of relativity that applies only to Chicago" (1993, 357). He thus argues for increased comparative and historical work that will generate theories that in fact apply across political systems or across time.

Regardless of how one evaluates the costs and benefits of mature normal science, the evolution of congressional studies stands in sharp contrast to legislative research on other countries, which clung to a much older approach. Until the 1980s, students of non-U.S. legislatures typically worked on developing typologies of parliaments according to the various roles and functions that parliaments played across political systems and on coding individual parliaments according to where they fit in the various typologies. Work in this tradition was typically motivated by the normative question of whether “parliament matters”—that is, whether the “institution of parliament” influences the outcome of the legislative process.

This focus on parliamentary functions went back at least as far as Bagehot (1966 [1867]), who argued famously that the English Parliament’s purpose is not to influence policy but rather to elect the executive, keep it informed about constituent interests, and keep the public informed about public policy.<sup>7</sup> Since Bagehot, a similar functionalist orientation often dominated comparative research on legislatures. Numerous efforts have been made, for example, to refine and improve the typologies of parliamentary roles (e.g., Blondel 1973; Wahlke et al. 1962; Packenham 1970; Polsby 1975; Mezey 1979; Sisson 1973) or to describe parliamentary structures in efforts to define such roles (e.g., Lees and Shaw 1979).

This mode of comparative legislative research still prospers. Recent work has attempted to sort out how and under what circumstances various parliaments play significant roles in policymaking (e.g., Olson and Mezey 1991; Arter 1984; Damgaard 1992; Liebert and Cotta 1990; Norton 1993). There has also been considerable effort to understand how members of parliaments perceive their own roles (e.g., Cain, Ferejohn, and Fiorina 1987; Searing 1994; Park 1988).

One notable feature of almost all of the typological research cited above is the absence of testable, causal arguments.<sup>8</sup> Instead, the research has a highly descriptive quality, with the primary goal being to interpret the actual role that legislatures or legislators are playing in different political systems. Mezey goes so far as to argue that this body of research has “yielded nothing in the way of testable propositions that would help to explain the differences and similarities in legislative performance that we observe cross-nationally” (1993, 351). Perhaps it is in this regard that the congressional literature stands in such sharp contrast to other legislative research. And this may help explain why so many of the prizes and publications in leading journals are devoted to works that aim explicitly to develop

7. The famous work by Jennings (1957) makes a similar argument.

8. The obvious exception is some of the work on individual roles, where efforts are made to deduce specific factors that affect individual perceptions of their roles (e.g., Searing 1994; Cain, Ferejohn, and Fiorina 1987).

original theoretical arguments or test such arguments in a sophisticated way.

## ■ | Borrowing Arguments: The Impact of Congress on Comparative Studies

By this standard, considerable progress has been made in the comparative study of legislatures over the last ten years. In particular, the rigorous mode of congressional research, where causal arguments about legislative behavior are derived and tested, has changed legislative research on other countries by altering not only the basic methodology of research but also the types of questions that are posed and the types of answers that are offered. Perhaps most notably, following the development of the spatial model in studies of American politics, many comparative studies have moved away from their earlier functionalist orientation and toward an effort to develop and test causal arguments about legislative organization and behavior. Theoretical arguments originally developed in the study of Congress have been borrowed by scholars studying other political systems.

The distributive politics theory in the Congress literature, for example, has led to a series of arguments about the agenda power of committees and the composition of committees (e.g., review essays in Shepsle and Weingast's 1995 edited volume). Drawing on this same basic theoretical framework, Laver and Shepsle (1990, 1996) develop and test an original argument about how ministerial portfolios are allocated in parliamentary democracies. Their theory leads us to expect that the policy preferences of ministers will influence in predictable ways the types of portfolios that ministers receive during government formation. Similarly, Huber (1992, 1996a) draws partially on distributive theory to argue that the "package vote" in France is used to preserve agreements across political parties in the governing majority on distributive issues.

Issues of proposal and veto power within the House have also been central in the Congress literature. This type of theoretical work, which draws on a framework originally developed in Romer and Rosenthal 1978 (see the review in Rosenthal 1990), examines how the distribution of proposal and veto power in the policymaking process influences political behavior and policy outcomes. Research on Congress has applied this approach in a variety of contexts, including the use of restrictive amendment procedures (e.g., Krehbiel 1991; Dion and Huber 1996), the institutional origins of committee power (Shepsle and Weingast 1987), the use of executive vetoes (Cameron 2000), and the impact of divided government on policy change (e.g., Krehbiel 1998; Howell et al. 2000).

Recently, this approach to legislative theorizing has been applied in a

variety of contexts outside the Congress. Tsebelis (1995, 1999) utilizes the framework to develop and test the argument that as the number of partisan and institutional veto players increases in a political system, the propensity for major policy changes will decrease.<sup>9</sup> A related “veto players” argument is developed by Bawn (1999b), who shows that spending levels in different German ministries are influenced by the level of conflict across parties in coalitions and between the Bundestag and the Bundesrat. The framework has also been used by Tsebelis (1994), Moser (1997), Steunenberg (1994), and Crombez (1996) to develop theoretical debates about the circumstances under which the European Parliament can influence policies in the European Union. Huber (1996b) uses the framework to argue that the existence of a confidence procedure in parliamentary democracies gives prime ministers proposal power and relegates assemblies to veto players, which is the opposite of what we find in many presidential systems, where legislatures propose and presidents veto. Rasch (2000) draws on the framework to study strategic voting behavior in parliamentary settings. Vanberg (1998) uses the framework to examine the ability of the parliamentary minority to use threats of constitutional review of legislation to extract policy concessions. And Baldez and Carey (1999) invoke the framework to argue that the rules for budgeting in Chile have a systematic impact on spending levels.

The Baron and Ferejohn bargaining models (e.g., 1989a, b), originally developed to study legislative procedure in the U.S. Congress, have also been imported by scholars studying noncongressional settings. In the Baron and Ferejohn framework, bargaining outcomes are shaped in large part by the preferences legislative actors have to reach agreement quickly rather than slowly (i.e., legislative actors discount the utility of future payments). This framework allows them to argue, for example, that closed amendment rules can be more efficient than open amendment rules because they facilitate quicker agreement. This same insight has been adopted in the comparative context by Tsebelis and Money (1995, 1997), who study bicameralism. They argue that the power of the upper houses in a political system will often depend on their powers of delay. Upper houses can use such power to extract concessions from lower houses (and cabinets), which may have superior agenda power but also often prefer quick action that is made possible only by the acquiescence of the upper house. Similarly, Baron (1993) uses the framework to study the type of outcomes that should emerge from coalition bargaining processes, and Baron (1998) uses the framework to study cabinet bargaining after coalition formation is complete.

Principal-agent theory represents another area where theoretical efforts to explain behavior in the U.S. Congress have been imported by oth-

9. See also Tsebelis 1994 on the European parliament, Bawn 1999b on bicameral bargaining in Germany, and Heller 1997 on budget politics in Italy.

ers. Kiewiet and McCubbins (1991) were the first to develop this framework to study Congress, arguing (among other things) that institutional structures allow congressional control over budget outcomes and that committee assignments and committee behavior are largely controlled by political parties. This same framework was adopted by Ramseyer and Rosenbluth (1993), whose use of the principal-agent framework to reinterpret Japanese legislative politics has clear antecedents in the application of principal-agent theory to the study of Congress. More recently, it has been used by Strom (2000) to interpret the chain of delegation in parliamentary systems for voters to bureaucrats. And the principal-agent framework has played a central role in Steunenberg's work on delegation in the European Union (Steunenberg 1994).

We find, then, over the past ten years an impressive and growing list of examples where a Congress-centered theoretical approach has been exported, with positive results, to the study of other settings. To be sure, it remains a small subset of legislative studies. But it nonetheless represents a dramatic change over what came before. In evaluating the body of comparative scholarship that borrows from congressional insights, we offer several observations.

First, many of the applications of congressional research do not occur in studies of legislatures only. Scholars of other systems may have previously dismissed the relevance of congressional theories because legislatures in other systems often seem to play a peripheral role in the policy process. But many scholars drawing on congressional theories have applied them not to parliaments but to cabinets in parliamentary systems. In our view, this extension of Congress-centered theory to the study of cabinets does not remove the research from the domain of legislative studies. On the contrary, since Bagehot, scholars have recognized that the cabinet is in many respects a supercommittee of the legislature and that activity in the cabinet thus lies squarely in the legislative domain. It is therefore quite natural to view the politics of cabinet government as "legislative politics" in parliamentary systems.

Second, none of these examples is characterized by an automatic and unaltered application of congressional approaches to another setting. Instead, each of these studies of non-U.S. settings makes modifications and theoretical extensions to the ideas first published in the congressional literature, resulting in arguments that are tailored to their new applications. That said, these studies have a theoretical lineage that can be traced back to the study of Congress. Methodologically, these studies share with previous congressional scholarship a preoccupation with developing empirically refutable claims about legislative behavior. In this respect, those who have borrowed from Congress are clearly making it possible to advance beyond the limitations of previous typological research in non-U.S. settings.

Third, the influence of congressional research in other settings has two dimensions. One is simply that we now have impressive new theories

of “other places.” The Ramseyer and Rosenbluth study of Japan, for example, turns on its head the conventional wisdom about the power of the Japanese bureaucracy. Tsebelis and Money draw attention to what were previously dismissed as irrelevant upper chambers. Bawn gives us evidence of when the Bundesrat shapes policy outcomes in Germany. Laver and Shepsle provide us with a clear deductive theory about how coalitions distribute agenda control. And the numerous papers on the European Parliament have clearly enriched our understanding of how policy is made in the European Union. These are but a few examples of how using a framework born in the study of Congress has helped us to understand other times and places.

The second dimension concerns the generality of our theories. Since this research has revealed that many of the theoretical arguments developed with an eye toward Congress are not parochial in their ultimate scope, it is now possible to draw connections between phenomena traditionally viewed as quite distinct. We can now see connections between the dynamics of committee assignments in Congress and the assignment of portfolios in parliamentary systems. The research cited above also demonstrates similarities between policymaking during coalition government in parliamentary systems and policymaking during divided government in the United States. The roles played by political parties in solving agency problems have many similarities in Japan and the United States. And the importance of agenda procedures for facilitating distributive agreements is similar in France and the United States.

Fourth, the rational-choice approach to research on the contemporary Congress has had an indirect effect on our understanding of legislative institutions in other settings. One of the main benefits of the congressional mode of research is that it requires scholars to examine carefully the subtle details of institutional design, focusing in particular on agenda institutions—the rules for making and approving policy proposals. When this analytic approach has been exported to other settings, scholars have often discovered that information about relevant institutional details has never been collected. Consequently, the export of the Congress mode of research to other settings has forced scholars to undertake theoretically informed empirical research on the nature of legislative institutions. In attempting to study portfolio distribution, for example, Laver and Shepsle could not use existing research to identify the agenda powers of cabinet ministers, a deficiency in the existing literature that they responded to with their 1994 edited volume. The chapters in Döring 1995b provide an array of institutional data, the collection of which was motivated almost entirely by theoretical frameworks developed in studies of Congress. And the chapters in Strom, Müeller, and Bergman (forthcoming) typescript provide a carefully collected new data set on the institutional arrangements that govern coalition formation and dissolution.

Finally, it is worth noting that while studies of Congress have exercised

considerable influence on the study of other legislatures, the study of other legislatures has had almost no discernible impact on studies of Congress. Probably the only significant exception relates to Cox and McCubbins's *Legislative Leviathan* (1993), concerning the role of party in the House of Representatives. Many of the theoretical ideas in that work about the incentives of individual representatives to delegate authority to party leaders were first developed in Cox's *The Efficient Secret* (1987), a study of the emergence of party discipline in the British House of Commons in the nineteenth century.

## ■ | Research Biases

Research on Congress has clearly enriched legislative studies over the last decade, through its impact on how other assemblies have been studied. But this influence has brought with it some clear biases in how legislative scholars pose and address research questions. Most important, congressional scholarship has tended to emphasize the individual behavior of legislators, rather than policy outcomes.

This emphasis on individual behavior has theoretical roots. As Mayhew (1974b) and rational-choice scholars have emphasized, legislators can be examined as autonomous reelection seekers, acting independently of party organizations. Individual legislators, especially in the U.S. context, can be held personally accountable by voters. But empirical, rather than theoretical, considerations probably explain the overwhelming emphasis by congressional scholars on individual behavior. In contrast to comparative studies of multiple legislatures, studies of the contemporary Congress tend to be studies in which the institution is held constant and dramatic changes in policy outcomes are rare. What varies most in a single legislature is the behavior of individuals, and the availability of roll call and electoral data facilitate this research bias. Comparative scholars, in contrast, can more easily study aggregate outcomes across legislatures than individual behavior within legislatures.

Logically, of course, we could imagine comparative studies of individual behavior or studies of the contemporary Congress that emphasize policy outcomes. Both literatures exist. Regulatory policy, budget policy, and pork-barrel politics have all been extensively studied by congressional scholars. But, as we report below, research on policy outcomes represents a small part of the work on Congress. Overwhelmingly, current congressional research focuses on individual action.

Given the dominance of congressional literature, the entire subfield of legislative studies is shaped by a perspective that emphasizes the individual. There are striking differences between those studies which focus on Congress and those which focus on other settings. More generally, we

argue, scholarship on any single legislature is fundamentally different from scholarship that is either cross-sectional or longitudinal in design. Historical and comparative studies of legislatures allow scholars to ask questions about the design and impact of institutions that cannot be examined in studies of the contemporary Congress, where the institutional context is exogenous. In historical and comparative scholarship, institutional variation could be internal to the legislature—such as variation between legislatures in degrees of professionalism, in rules, or in the nature of committee systems—or the variation could be external to the legislature—including differences between separation-of-powers and parliamentary systems, or variation in electoral rules. In research on the contemporary Congress, in contrast, institutions are generally held constant.

Surveying recent literature published in the three leading journals, we found that work on the contemporary Congress is dominated by studies of individual behavior. Some form of individual behavior was the dependent variable in 33 of the 55 articles on the contemporary Congress that we identified in recent issues of *APSR*, *AJPS*, and *JOP*. Voting behavior is by far the most common dependent variable in the literature. Recent efforts to explain and characterize patterns of roll call voting include, for example, Box-Steffensmeier, Arnold, and Zorn 1997, Herrick, Moore, and Hibbing 1994, Fett 1994, Krehbiel 2000, Rothenberg and Sanders 2000, Hero and Tolbert 1995, Norton 1999, Hutchings 1999, Gartzke and Wrighton 1998, Vega and Firestone 1995, Grofman, Griffin, and Berry 1995, Overby and Cosgrove 1996, Clucas 1997, Maltzman 1998, Bailey and Brady 1998, Hood and Morris 1998, Swers 1998, Forgette and Sala 1999, Groseclose, Levitt, and Snyder 1999, Snyder and Groseclose 2000, Hager and Talbert 2000, and Nokken 2000. Congressional scholars analyze many other forms of individual behavior as well—including the decision to retire (e.g., Kiewiet and Zeng 1993; A. Gerber 1996; Moore and Hibbing 1998; Hall and van Houweling 1995; Groseclose and Krehbiel 1994), the decision to cosponsor bills (Kessler and Krehbiel 1996; Krehbiel 1995; Schiller 1995), the decision to discharge bills from committee (Kessler and Krehbiel 1996; Binder, Lawrence, and Maltzman 1999; Krehbiel 1995, 1999), the decision to hold hearings (Diermeier and Feddersen 2000), the behavior of party leaders (Sinclair 1999), and a vast amount of research on the behavior of legislators in committees.

In addition to studies of individual behavior, another large body of congressional research examines the internal organization of legislatures, accounting for another ten articles in our sample. Research on the composition and role of committees (including debates regarding the relationship of committees to the floor and tests for committee outliers), legislative procedures, and agenda-setting powers all fall into this category.

Literature on other single legislatures—including research on a single state legislature or a single non-U.S. national legislature—share the biases of literature on the contemporary Congress. Of twelve articles that discuss

a single legislature (not including articles on Congress), three examine individual behavior and another three examine legislative organization. Thus Kathlene (1994) examines gender differences in legislator behavior at hearings, using data from Colorado; Ames (1995a) studies voting in Brazil; Hamm and Harmel (1993) examine the role of the Speaker in Texas; O'Brien and Luehrmann (1998) study institutionalization in China; and Remington and Smith (1998b) analyze organizational features of the Russian Duma.

Policy outcomes, which are only rarely examined in the context of Congress, are much more prevalent in comparative studies of legislatures. Of the 55 articles on the contemporary Congress, just four analyze policy outcomes, including Lee (1998, 2000), Carsey and Rundquist (1999), and Jones, True, and Baumgartner (1997). However, cross-state and cross-national projects often seek to explain differences in budget levels, welfare priorities, public goods programs, and the independence of central banks and judiciaries. Of the 27 cross-legislative studies in our sample, 14 focus on policy outcomes. Among studies of state legislatures, this includes Alt and Lowry 1994 and Abney and Lauth 1997, which analyze budgetary issues; Skocpol et al. 1993, which examines the origins of mothers' pensions; and M. A. Smith 1997, which investigates the policy consequences of partisan turnover. Although less common than in cross-legislative contexts, historical work on Congress also raises important policy questions, including studies of divided government in the United States (e.g., Cameron 2000; Mayhew 1991; Edwards, Barrett, and Peake 1997; Binder 1999; Coleman 1999), the adoption of constitutional amendments and the admission of U.S. states (e.g., Stewart and Weingast 1992; King and Ellis 1996; Crook and Hibbing 1997; Wirls 1999), and the policy implications of electoral realignments (e.g., Sinclair 1977, 1978, 1982; Brady 1985, 1988). Only rarely do comparative studies of legislatures emphasize individual behavior or questions of internal organization.

## ■ | The Standard Template for Congressional Research on Institutions

Within the rational-choice, institutionalist framework that is central to research on the contemporary Congress, the template for the development of new theoretical arguments about legislatures is easily recognizable. Scholars make assumptions about the goals, abilities, and information of the relevant participants (usually members of Congress and the individuals with whom these members interact, such as bureaucrats, the president, voters, or interest groups). They might assume, for example, that members are motivated by their policy preferences, by the desire for reelection, or by the desire for internal advancement within Congress. Scholars will also define

the relevant institutional context. They might care, for example, about the relevant rules for setting an agenda, about special prerogatives of committees, or about the prerogatives of particular members holding leadership positions. Finally, they will derive testable hypotheses about how strategic politicians will behave given their preferences and the institutional context.

Charles Cameron's study (2000) of the presidential veto is an excellent recent example of this mode of theorizing. Cameron wants to understand how bargaining between the U.S. president and Congress unfolds given that Congress can propose bills, the president can veto them, and supermajorities can override a veto. He therefore develops a signaling model that makes straightforward assumptions about how the institutional details of the U.S. presidential veto and the congressional power to override the veto interact to structure bargaining between the president and Congress. He also makes assumptions about preferences and information, assuming that the participants care about policy outcomes. Finally, Cameron develops hypotheses about when vetoes should occur and about the nature of policy outcomes under various configurations of preferences.

Of particular interest to Cameron is the impact of divided government on bargaining. He finds that the probability of vetoes and veto threats is relatively high during divided government on important bills. More important, his theory predicts and his evidence confirms that policy is more moderate during divided government than during unified government. The analytic and empirical rigor of the work, along with the substantive contributions it makes to our understanding of policy outcomes, makes this recent book a path-breaking study of executive-legislative relations in the United States and a model for legislative scholarship generally.

There is a great deal of influential work on Congress that proceeds in similar fashion. Like Cameron, some of this research develops an explicit formal model, but much of it does not. All such studies, however, have in common that they examine strategic legislative behavior where the institutional setting is fixed. Prominent examples in the last ten years of books on Congress in this mode include Epstein and O'Halloran 1999, Krehbiel 1991, 1998, Bianco 1994, Kiewiet and McCubbins 1991, Cox and McCubbins 1993, Shipan 1997, Schiller 2000, and R. L. Hall 1996.

Beyond congressional studies, a similar approach has often been adopted. Examples include Huber 1996a on France, Londregan 2000 on Chile, Remington and Smith 1998b on Russia, and Ramseyer and Rosenbluth 1993 on Japan. And though Laver and Shepsle's study (1996) of portfolio distribution and Tsebelis and Money's study (1997) of bicameralism are cross-national in scope, their mode of theorizing follows the lead of congressional scholars in holding the institutional setting constant.

Holding the institutional setting fixed creates some significant advantages for scholars and is crucial for answering certain types of important questions. Cameron's book, for example, helps us to understand how di-

vided government affects legislative behavior and policy outcomes in the United States. The other research cited above has a similar theoretical foundation, with the goal being to explain variation that exists in behavior and outcomes within a fixed institutional setting. Legislative theories of this sort are not only admirable in their analytic elegance and empirical rigor; they are also obviously substantively important. It is, after all, interesting to understand the factors that influence variation in policy outcomes within the U.S. government.

It is also the case, however, that when institutions are treated as constants, it is impossible to explore effectively several important issues about legislative institutions. The first concerns the origin of legislative structure. The second concerns the impact of institutions on individual behavior and legislative outputs. In what follows, we describe the nature of the biases created in our understanding of these two issues, and we discuss recent efforts to avoid these biases by generating theories where the institutional context varies.

## ■ | Origins of Legislative Institutions

Research on the contemporary Congress does offer interesting theories of institutional *origins*. In this vein, scholars make claims about fundamental problems that legislators must address. They then develop and test theoretical claims about how the problem is resolved when particular institutional arrangements exist. If the tests support the theory, then there is an implicit or explicit claim that the institutions exist to solve the particular problem—or that the legislature has organized itself in a particular way in response to the problem. The problem with this approach is that theories developed in this fashion, however plausible they might be, have a functionalist flavor that renders them essentially untestable as arguments about why legislators organize themselves in the ways that they do.

The example of delegation to committees helps to clarify how the Congress literature develops theories of institutional origins, and where the drawbacks lie with respect to this mode of theorizing. Scholars have widely recognized that committees often receive special privileges. Majorities in legislatures, for example, often give special agenda control to committees, to take but one example. But why would a majority give up power to a minority in the legislature? What are the origins of these institutional arrangements?

One prominent answer to this question is the informational theory developed chiefly by Gilligan and Krehbiel (1987, 1989) and Krehbiel (1991). Their argument is simple and compelling. They assume that an informational problem exists: legislative majorities do not always know which policy will achieve the desired outcome when a new issue arises. Majori-

ties therefore need expertise, which is costly. Who in the legislature will be willing to pay these costs, and what will induce such members to share their information? The answer in informational theories involves commitment to committees. If a majority grants special procedural prerogatives, such as restrictive amendment procedures, to committee members, then committee members can often be induced to pay the costs of specialization (because their policy proposals will be privileged in floor voting). These procedural protections also make it possible for the committee members to share their information with the floor (because the procedural protections make it difficult for the floor to turn around and use the information against the committee members). Thus, by committing to give up certain rights to a minority, the majority can benefit. This is particularly true when the committee membership is itself controlled by members of the floor. The informational theory is rich with empirical predictions about such things as committee composition and the utilization of restrictive procedures. In his book, Krehbiel (1991) tests many of these predictions and, finding support, argues for an informational perspective on legislative organization.

This mode of argument about institutional origins is hardly unique to the informational perspective. In fact, almost any research on Congress—or on any other single legislature—that speaks to the question of why legislatures are organized the way they are takes a similar tack. According to the distributive approach, to take another example, procedural advantages are also given to committees as commitment devices. But the starting assumption of this perspective is not an informational problem but rather a distributive one—majority rule decision making on more than one issue at a time is inherently unstable (because no policy exists that is preferred by a majority to all other policies). Special privileges for committees are therefore created to solve the problem of majority rule instability and to make possible log rolls or other distributive agreements (e.g., Shepsle 1979; Shepsle and Weingast 1981; Weingast and Marshall 1988; Baron and Ferejohn 1989a). Distributive theories thus argue for a distributive perspective on legislative organization.

Partisan explanations for committee deference, like the other two approaches, have tended to take the institutional context for granted. In arguing that parties constitute a form of “legislative cartel,” Cox and McCubbins (1993) suggest that individual legislators have depended on party organization throughout the postwar era and that the continuities in this arrangement overwhelm any changes. Indeed, a central argument in their book is the contention that the seniority system, even in its heyday, never constituted a genuine alternative to partisan decision making in the House. Rohde (1991), in proposing the concept of “conditional party government,” makes the case that the influence of party organization on individual behavior waxes and wanes with party homogeneity. In developing this argument, Rohde explicitly contrasts the textbook Congress of the 1950s

and 1960s with the postreform Congress. Yet, Rohde (and, in recent work, Aldrich and Rohde 2001) builds his general model out of the material of a single legislature, in this case the majoritarian House.

This mode of theorizing is not limited to the general study of organizational forms. It is also used to explain the choice of specific institutions in specific places at specific moments. In their historical study of institutional creation by the English parliament following the Glorious Revolution, North and Weingast (1989) argue that parliamentary institutions were designed largely in response to commitment problems faced by members of Parliament themselves. At this particular historical juncture, Parliament had substantial control over the design of new legislative and executive institutions, and they could have decimated the crown's power. But this did not occur. Instead, Parliament—fearing its own inability to constrain itself from creating pork-barrel projects, and thus fearing its own inability to secure the rights of property holders (who would be taxed to pay for such projects)—designed institutions that allowed the crown to propose spending while Parliament could approve it. This form of institutional commitment, North and Weingast argue, not only prevented Parliament from abusing its own powers but also insured the crown with considerable income—enough to go to war with France from 1689 to 1697.

In arguments about institutional origins of this form, as in many functionalist arguments, the consequence of the institutional form is said to be its cause (Stinchcombe 1968). Informational theory argues that delegation to committees exists because it solves informational problems. Distributive theory argues that delegation to committees exists to solve distributive problems. Partisan theory argues that delegation to committees occurs to cement the dominance of party leaders over legislative affairs. And, in a different context, North and Weingast argue that delegation of limited powers to the crown occurred after the Glorious Revolution to solve a commitment problem faced by members of Parliament. These theories about institutional origins differ in an important way from many functionalist arguments in sociology because they posit a clear and explicit microfoundation for behavior and choice. But, like sociological functionalism, the theories, as theories of institutional origins, are largely untestable.

In research that focuses on a single case—usually the postwar U.S. Congress, but, as we indicate, also in arenas like the seventeenth-century English Parliament—the central cause of the institutional arrangements being studied does not vary. Distributive theories, for example, assume that distributive problems exist, while informational theories assume that policy uncertainty exists. Therefore, the theories do not attempt to explain how legislative organization varies as a function of these problems.

There are clear advantages to this approach. These theories focus on problems that are so fundamental to legislative politics that it is unlikely the problems would be irrelevant to institutional design in any political system. Indeed, the rigorous and sustained treatment of competing explana-

tions for committee deference represent perhaps the most significant contributions to legislative studies in the last twenty years. Because the research questions are tightly focused and because the institutional context is held constant, questions of committee deference in Congress have forced scholars to build their cases with ever-increasing theoretical, analytical, and empirical rigor.

On the other hand, there are drawbacks associated with theorizing about institutional origins in this way. First, there is clearly variation across political systems as well as over time in the United States regarding the form and degree of delegation to committees—variation that obviously cannot be explained by variation in distributive conflict or the need for information. Do legislatures that do not delegate substantial autonomy to committee, as in many parliamentary systems, fail to solve their distributive and informational problems? Are distributive and informational problems nonexistent in such countries? Second, delegation to committees does not represent the only way to address these problems. Diermeier (1995), for example, makes a compelling case that seniority systems can encourage specialization. Laver and Shepsle (1990, 1996) and Austen-Smith and Banks (1990) describe the ways that cabinet institutions solve distributive problems in parliamentary systems, and Huber (1996a, b) describes the role of confidence motions in this context. Thus, it seems doubtful that informational or distributive problems are a sufficient condition for observing delegation to committees.

Finally, careful historical research on the choice of particular institutions often reveals the unintended consequences that result from the selection of particular organizational forms. In Cooper's (1970) study of committee origins in Congress, he discovers a strong bias by Jeffersonian Republicans against standing committees. Imbued with an ethos that demanded chamberwide discussion of any issue, Republicans preferred to do their work in the committee of the whole and restricted committees to the work of writing legislation that the larger body had discussed. Yet, as Cooper shows, these same Republicans ultimately turned to committees, first as bulwarks against executive encroachment and later as agents to perform work that the executive was leaving undone. Binder (1997) shows, too, the extent to which short-term calculations override long-term goals. The suppression of minority rights, which distinguishes the House from the Senate, arose not from any overarching plan or ideology but from transient partisan considerations. And the triumph of unlimited debate in the Senate, Binder contends, is the logical consequence of an obscure early decision to remove the "previous question" rule from the Senate's rules. Understanding institutional origins, in short, requires recognizing the contingency and circumstances of decisions. Path dependence often overwhelms rational choice. This insight, gleaned from history, can often be neglected in an approach to institutional origins that draws data only from a single contemporary legislature.

Unlike the great body of research on the modern Congress, comparative and historical legislative studies provide a means for enriching legislative theories of institutional origins by allowing for a wider variety of theoretical explanation. In particular, these studies permit the development of theories where the broad institutional setting influences the legislative institutions that are chosen, with different settings leading to different institutional arrangements. Proceeding this way, the institutional arrangements in a particular chamber, such as the structure of a committee system, are not viewed as functionalist solutions to universal problems (like the need for information or the need to make collective decisions in multidimensional policy spaces). Instead, they are viewed as optimal solutions to the unique political setting in which the legislators find themselves. And this approach to theorizing about the origin of legislative institutions—unlike arguments drawn from the contemporary Congress or rooted in path-dependent historical accounts—has the additional advantage of being directly testable. If such theories are correct, then legislative institutions should vary in systematic ways across systems according to the institutional arrangements in that system.

A good example of this approach to legislative theory building comes from a recent formal model developed by Diermeier and Myerson (1999), who address a question central to the development of informational and distributive theory: Why should legislative chambers delegate special prerogatives to a minority of their members, as they do when they create strong committees or the need for supermajorities? Although the question is similar to that in previous studies of Congress, the theoretical approach is quite different because it is explicitly comparative. Instead of posing a problem that might be viewed as largely universal to legislative decision making, they develop a bargaining model that examines the broader institutional context in which legislators find themselves.

Diermeier and Myerson begin with the assumption that there is variation across political systems in the extent to which a particular legislative chamber must bargain with other political actors before new legislation can be adopted. Some countries, for example, have bicameral legislatures, both of which must agree on a bill before it can become law. And some countries have a separation of executive and legislative powers, with executive approval of legislative proposals necessary before these proposals can take effect. Diermeier and Myerson want to understand how variation in the political context of this sort affects incentives for a legislative chamber to create “hurdle factors”—institutional arrangements within the legislative chamber that create additional hurdles that must be passed before a proposal can be approved by that chamber. As noted, examples of hurdles include strong committees with agenda power. In such a chamber, the committee must approve a bill before the floor can consider it.

The main claim of their theory is straightforward. The more that the broad institutional context requires a chamber to bargain with other insti-

tutional actors outside the chamber (like presidents or another chamber), the more incentives that chamber will have to create internal hurdles. This is true because such internal hurdles give the chamber more leverage in its negotiations with outside actors. If a president, for example, must satisfy only one actor in a chamber (such as the median member, or a strong leader to whom all power is delegated), he will be able to extract more concessions than if he has to satisfy multiple agents in the chamber (such as the floor majority and a strong committee with gatekeeping power).

Diermeier and Myerson's model thus demonstrates how a comparative approach enriches theorizing about the origin of legislative institutions. From the perspective of their theory, the legislative institutions in the U.S. Congress, which give substantial power to minorities within each chamber, are due not to a need for information, to resolve distributive conflict, or to ensure partisan control over outcomes. Rather, they are a function of the considerable division of power that exists in the American political system. Their general argument, it is useful to underline, is directly testable by looking at how the number of hurdle factors in legislatures is related to the number of institutional actors that must agree on policy change. Though to our knowledge the argument has not been tested directly, Diermeier and Myerson note that their main empirical claim is consistent with the stylized fact that committees are stronger in presidential than parliamentary systems (e.g., Lees and Shaw 1979). The significance of their model comes in its powerful assertion that the most basic rules of the game—in this case, U.S. checks and balances—matter more to committee strength than any of the features examined through the perspective afforded by any study of the contemporary Congress alone.

Other recent scholarship has developed theories of institutional origins using the comparative approach. Compared to Diermeier and Myerson's formal model, much of this research is less explicit with respect to theory but has the advantage of containing an empirical component. Carey and Shugart's 1998 edited volume on decree authority, for example, examines incentives to create and utilize institutional arrangements that permit executives to perform legislative functions. They consider both the creation of *constitutional decree authority*, where the constitution delineates situations where the executive can make law by decree, independent of legislative action, and *legislative decree authority*, where the assembly votes laws that delegate authority to the executive to make laws by decree. In the coeditors' theoretical and empirical chapters (Carey and Shugart 1998a, b), they argue that constitutions will be more likely to include constitutional decree authority if the individual expected to control the executive is responsible for making the constitutions, if it is easy for legislators to change the constitutions, and if a strong veto does not exist. It is useful to note that consistent with the comparative approach, none of these hypotheses about institutional origins could be tested in a single country study. Carey and Shugart also argue that delegated decree authority will be

most likely to occur when a strong veto exists, the issue is urgent, the judiciary is independent of the executive, and a legislative majority supports the executive. Some of these arguments could be tested within a single country—and are by various authors who contribute country studies to the edited volume—but other arguments are explicitly comparative, such as the features of the judicial system and the nature of the executive veto, and thus cannot be tested in a single country (because the relevant explanatory variables are constant within a system). These hypotheses are tested by Carey and Shugart (1998b) by compiling cross-national information from their case studies.

We have discussed these two examples in some detail to illustrate the types of arguments that can emerge about institutional origins from a comparative perspective. But there are many other recent works that exist in this vein. Some of these are empirical and focus on non-U.S. settings. Cox, Rosenbluth, and Thies (2000), for example, show that institutional variation that exists between the upper- and lower-house electoral rules in Japan affect the propensity of Japanese deputies to join party factions. Huber (2000) shows that incentives of parliamentary majorities to create automatic budgeting procedures are influenced by the level of cabinet turnover. And Bernhard (1998) shows that incentives of legislative majorities to delegate fiscal policymaking to independent central banks is influenced by the committee structure in the legislature.

Other arguments in this vein are abstract formal models. Diermeier and Feddersen (1998) show that the level of party discipline within a legislature should be influenced by the nature of legislative-executive relations. Carruba and Volden (2000) develop a model where the voting rule in a legislature should vary with the size of the legislature. Laver (1999) argues that party unity will be a function of the institutional features of coalition agreements in parliamentary systems, as well as how institutions affect the cost of losing office. And Diermeier (1995) develops a model where decisions for legislators to specialize are influenced by expected rates of turnover in the legislature.

Similar arguments have been developed in the American politics literature, often using historical or cross-state data to provide variation. Cox and Katz (1999) argue that the redistricting plans adopted by state legislative majorities are influenced by whether there are partisan or bipartisan institutional arrangements governing the redistricting process. In another context, Hamm and Harmel (1993) argue that the way that the minority organizes itself in the Texas assembly depends on the size of the minority.

Not surprisingly, historical work has played a central role in investigating the origins of legislative institutions. Some historical studies focus on short periods of time. Although they cannot offer a comparative perspective on the effects of institutional change, such studies do provide insights into the historical roots of modern institutions as well as fresh laboratories for testing legislative theories. Thus Gamm and Shepsle (1989), Jenkins

(1998), and Canon and Stewart (2001) have all contributed to debates regarding the origin of standing committees by examining their development in Congress in the early nineteenth century. Jillson and Wilson (1994) study how the structure of the Continental Congress frustrated effective decision making in the 1770s. Katz and Sala (1996) argue that the introduction of the Australian ballot influenced the creation of committee property rights in the U.S. House of Representatives. Other scholars—King and Ellis (1996), Crook and Hibbing (1997), Wirls (1999)—have scrutinized the adoption of the Seventeenth Amendment, which established direct election of senators, analyzing the conditions that lead legislators to change their own institutions. And Theriault (2001) examined the Pendleton Act and other legislation that shifted the incentive structure for career-seeking legislators. Skowronek (1982), Skocpol (1992), Sanders (1999), James (2000), and Carpenter (2001), in their excellent studies of U.S. national policy in the late nineteenth and early twentieth centuries, all demonstrate the relevance of Congress to broad political changes.

Other historical work, which examines longer periods of time, can analyze the sources of institutional change or the consequences of new institutions for individual behavior. Cooper and Brady (1981a), in their study of congressional leadership, provide an important early model for this work. This form of scholarship also includes studies of the congressional reforms of the 1970s, including changes in floor activity and committee structure. It includes Schickler's (2001) major new study of institutional innovation in Congress as well as the landmark books by Binder (1997) and Dion (1997), which examine the circumstances under which a legislature changes its rules to limit minority rights. Binder and Smith (1997), in their study of the filibuster, also contribute to the literature on institutional change. Stewart (1989) examines how individual preferences shaped the reorganization of budget politics in the House between 1865 and 1921. Swift (1996) addresses the early transformation of the Senate into a meaningful legislative body. And Camm and Smith (2000, forthcoming a, b), in reconstructing the emergence of Senate party leadership through the nineteenth and twentieth centuries, find that short-term partisan concerns contribute to long-standing shifts in party organization.

The advantage of comparative and longitudinal analysis is clear. In examining legislatures through a cross-sectional or historical lens, scholars can discern how institutions emerge and change. They can do this because comparative and historical analysis affords variance in institutional forms. Taking advantage of this variance is not straightforward. In comparative work, the difficulty comes in controlling for the array of structural features that may differentiate national legislatures. But the difficulty may be even greater in historical work, even in work that examines long stretches of time. To the extent that the U.S. Congress has changed radically over time, so has the rest of the political system—which makes the nineteenth-century Congress as foreign as the Bundestag to students of the contempo-

rary Congress. But, in other respects, institutional variance is quite limited in historical studies. Compared to variations between foreign legislatures, the basic outlines of U.S. congressional organization have changed little since the early nineteenth century, when the Speaker emerged as a party leader and standing committees assumed a leading role in the legislative process.

But this problem is most pronounced in studies of the contemporary Congress, where scholars generally hold the institutional context fixed. The irony, of course, is that scholars studying the contemporary Congress have contributed more than any other set of legislative scholars to our understanding of the importance of rules, agendas, committees, and leadership. Any serious student of legislatures properly cuts their teeth on Fenno, Mayhew, and all their progeny. All we suggest is the continued need to extend the analytical rigor and positivist orientation of this work to arenas where the institutional context is not fixed. While such research venues clearly present their own thorny difficulties of conceptualization and measurement, the recent literature also shows that they open avenues for understanding the origins of legislative institutions that are closed in studies of the contemporary Congress.

## ■ | Institutions and Their Impact

A second limitation associated with congressional research concerns the development of theories about the *impact* of organizational structure. A central objective of legislative studies is not only to examine the origins of institutions but also to analyze their effect on individual behavior. As in the study of institutional genesis, the bulk of research on the modern Congress holds the institutional context fixed, and thus provides very little insight into this important issue. After all, when institutional arrangements do not vary in one's research, it is impossible to make testable claims about how these arrangements affect behavior.

Research on the contemporary Congress has provided a wealth of impressive institutional theory, but the theories have been of a particular type, with important implications for where we find the biggest holes in our theories of how legislative institutions influence behavior. In particular, rather than developing theories of how, for example, institution *x* affects some output *y*, we have a preponderance of theories of the form: *given* institution *x* exists, how does some other variable *z* affect *y*? Cameron's book (2000) on legislative-executive relations, for example, cannot offer insights into how the veto affects legislative bargaining outcomes because there is no variation in the existence of the veto in American national politics. Instead, Cameron's work asks: Given the particular institutional features of the veto, how do variables like divided government and the importance of

legislation affect legislative bargaining outcomes? Similarly, Laver and Shepsle (1996) ask: Given the institutional arrangements that shape government policymaking in cabinets, how do the spatial preferences of political parties affect the allocation of portfolios? And research on committee assignments in Congress asks how the spatial preferences of individual members affect their assignment to committees, given the agenda institutions that exist in Congress—or, in R. L. Hall's 1996 study, the ways in which legislators participate in Congress, taking for granted the particular rules and institutions of the contemporary House.

In all of these influential studies, since the institutional setting is fixed, it does not and cannot serve as an explanatory variable. A vast body of research on the contemporary Congress has this characteristic. In demonstrating the power and elegance of a theory grounded in the idea that legislators are single-minded seekers of reelection, Mayhew (1974b) took for granted the institutional world of the textbook Congress. This Congress was crucial to Mayhew's argument, since the committee system and the weakness of party reinforced the individuality of career-minded legislators. Despite the centrality of this institution to Mayhew's argument, the demise of the textbook Congress did nothing to shake the influence of Mayhew's theory of legislator behavior. On the contrary, legislative scholarship continues to build on Mayhew's insight and approach.

Since the institutional context is fixed in most Congress research, we obviously do not gain direct insights from congressional studies about how the presence or absence of particular institutions affects the behavior of individual legislators. Instead, we typically develop and test arguments that focus on factors outside the legislative institution itself. Overby and Cosgrove (1996), for example, emphasize the impact of majority-minority districts on voting behavior in the House. Maltzman and Sigelman (1996), in their analysis of one-minute speeches, focus on policy and electoral goals of individual members as their independent variables. In other studies of member behavior, Alan Gerber (1996) emphasizes race and Kiewiet and Zeng (1993) emphasize age as predictors for retirements; Moscardelli, Haspel, and Wike (1998) emphasize ideology and Bailey and Brady (1998) emphasize constituency characteristics as predictors of votes; Cox and Magar (1999) emphasize majority status as a predictor of PAC contributions; Kessler and Krehbiel (1996) argue that individual attributes like an individual's tenure in the House, electoral vulnerability, and individual preferences affect decisions to cosponsor bills; Box-Steffensmeier, Arnold, and Zorn (1997) argue that voting behavior is a function of a representative's constituency characteristics, interest group links, their institutional position within the legislature, party affiliation, and ideology; Rothenberg and Sanders (2000) examine how voting is affected by individual and constituency characteristics, as well as the individual's intention to resign at the next election; and Schiller (1995) looks at how individual characteristics, including an individual's positions within the Senate, affect cosponsorship strategies.

The same bias in the nature of explanatory variables exists in the handful of Congress studies that examine outputs, like policy outcomes. Perhaps the most well developed variable in this context is preference conflict within legislative settings. Thus the literature on divided government, an excellent example of research that focuses on policy, examines how preference conflicts (i.e., divided government) affect policy outcomes (Cameron 2000; Mayhew 1991), the form of delegation to agencies (Epstein and O'Halloran 1999), the approval of presidential appointments (McCarty and Razaghian 1999), and the incidence of budgetary conflict (Clarke 1998).

Historical and comparative approaches, in contrast, have the potential to pose questions of the form: "How does institutional arrangement *x* affect legislative behavior or output *y*?" Such research has a fairly long tradition in studies of parliamentary government, one rooted in research on cabinet formation and dissolution. Strom (1984, 1990), for example, argues that particular legislative institutions (related primarily to committee systems) affect the propensity of minority governments to form. Strom, Budge, and Laver (1994) examine how legislative institutions affect coalition formation more generally, and de Winter (1995) examines how the government's control of the agenda affects the duration of coalition formation processes. Other scholars have examined legislative outputs in non-U.S. settings. Baldez and Carey (1999), for example, examine how rules for making budgets affect budget deficits in Latin America, Thies's (1998) comparison of Japan and the United States examines how committee structures affect the pace of policy change, Huber (1998) examines how turnover in the cabinet affects health care cost containment, and Döring (1995a) examines how the government's control of the agenda affects legislative outputs. Morgenstern (2000) examines how variation in electoral laws affect voting unity in legislatures. And, in a recent formal model, McCarty (2000) explores how variation in a president's veto power affects distributive politics.

Much of the best work in this tradition is on the U.S. states. Thus Francis and Kenny (1997) analyze the impact of term limits on legislative tenure, Abney and Lauth (1997) examine the effect of the line-item veto on budget restraint, and Fiorina (1994) and Squire (1998) investigate the impact of legislative professionalization. Finally, Elisabeth Gerber (1996, 1999) examines the impact of citizen initiatives on policy outcomes. Each of these works helps us to understand how the institutional context in which legislators find themselves affects the choices that legislatures make.

## ■ | Discussion

Legislative studies is one of the oldest and liveliest subfields in political science. In the late nineteenth century, early leaders of the discipline—from

Wilson to Bryce to Lowell—regarded legislatures as the central political institutions in a democratic regime. Early scholarship was normative and reformist. Wilson, for example, wrote about Congress in part to propose that the U.S. national legislature model itself more after the British House of Commons, with vigorous debates that educated the public. Normative concerns have never disappeared entirely: the scholarly obsession with Congress is surely due in part to a broad affection and respect for the institution.

But since the renaissance of legislative studies in the 1950s and 1960s, the field has shifted its focus. Scholarship on legislatures is judged today primarily by its contributions to science and theory, less on its relevance to reform efforts. The consequence is a subfield that has yielded a massive theoretical and empirical literature with all the hallmarks of mature normal science. This has led to some of the liveliest debates in the discipline. It has also supported a broad range of research methods and offered leadership in the development of new theoretical and methodological tools. In our view, it is indisputable that the rigor now characterizing the study of foreign legislatures and U.S. state legislatures is due primarily to the continuing debates and literature on Congress. Thus, by the measure of its influence on legislative studies specifically and the study of political science generally, congressional scholarship has been a remarkable success.

A central argument of this paper, however, is that the predominance of Congress carries with it some clear costs. One obvious drawback is that the more we study the contemporary Congress to the exclusion of other legislatures, the more we know about one specific legislature and the less we gain in our understanding of other specific legislatures and of the nature of legislatures generally. Sound theories of legislatures depend on careful studies of an array of institutions. This problem, of course, can be remedied (and is being remedied) by simply studying other legislatures.

Another drawback is less evident. As we argue, the emphasis on Congress—especially studies that focus on specific periods when the institutional context does not change—leads scholars to pose different questions and offer different theories than can be developed from a comparative or historical perspective. We have focused in particular on how the “Congress template” makes it virtually impossible to develop and test theories of the origins of institutions or theories about how the presence of particular institutional arrangements affects behavior and choice.

The bias of most existing legislative research away from these types of institutional questions is not simply an issue for arcane debates among social scientists. The bias also limits the ability of political scientists to address some of the central normative concerns of the present. In particular, as the recent wave of democratization has occurred, there has been a crucial need for answers to questions about institutional design. Institutional engineers across a wide range of countries and political settings want to know, “What type of institutional arrangements should we adopt in our

legislatures, and what will be the consequences of adopting them?" Even debates in the U.S. Congress over campaign finance reform, term limits, and rules reforms would benefit from comparative research. Legislative scholars have discovered themselves ill equipped to answer these questions because we lack well-developed theories of legislative institutions that have been devised and tested across time and space.

In our view, then, it is not just the natural progression of normal science but, more importantly, the very real needs of practitioners around the world that are orienting the research frontier of legislative studies today toward questions that comparativists of all sorts tend to ask—questions that investigate the origins of institutions and the impact of institutions on behavior. Legislative studies is thus poised to enter an extraordinarily fruitful marriage of positive method with normative concerns, one that will solidify the scientific quality of research, while making it possible to influence the design of the democratic institutional arrangements around the world.

# *The Great Transformation in the Study of Politics in Developing Countries*

Students of developing countries in the year 2000 were like geographers in 1520: the known world had begun changing about 25 years earlier, and we were still trying to figure it out. Just when most eyes were focused on explaining the rise of authoritarianism in developing countries, democratization began its current sweep through much of the world. More human beings currently live under democratic governance than at any previous time in recorded history. In a second equally unexpected development, many governments began to abandon state interventionist economic policies in favor of greater market orientation. On top of everything else, the Soviet empire collapsed and, with it, the only coherent competitor to capitalist economic organization. Though scholars greeted most of these changes with delight, few predicted them (cf. Fleron and Hoffman 1993; Remmer 1995; Kalyvas 1999).<sup>1</sup>

These real world events have led to a wholesale reevaluation of the theoretical underpinnings of the study of comparative development. This reevaluation has affected what we consider worthy of study, the set of stylized facts we accept as more or less true, our basic understanding of the economics of development, the theoretical approaches we use to explain politics, and the research methods we favor. The study of developing countries has changed almost as much during the last 25 years as the world we study.

In this essay I have three goals: to describe the political and economic transitions that have taken place, to note the effects these changes have

1. A number of scholars analyzed the fragilities and contradictions within one or another kind of authoritarianism. See, for example, O'Donnell 1978, 1979 for analyses of the potentially destabilizing tensions between alliance partners within bureaucratic-authoritarian regimes. See Kalyvas 1999 for a summary of the many descriptions of the weaknesses and dysfunctional aspects of communist regimes. No one to my knowledge, however, expected or predicted the sweep of democratization and economic reform across much of the developing world that we have witnessed.

had on theories and methods used in the study of developing countries, and to review some of the most important recent research on politics and political economy in developing countries. I shall not try to provide a comprehensive review of new work but instead to highlight interesting examples of what I consider some of the most promising directions for new research.

## ■ | Then and Now: Transitions

At the beginning of 1974, identified by Huntington (1991) as the start of the “third wave” of democratization, dictatorships of one kind or another governed 82 countries.<sup>2</sup> Only 19 dictatorships still survived at the end of 2000. During these years, 95 authoritarian regimes disintegrated (some countries endured more than one dictatorship during the period). These transitions have resulted in 54 surviving democracies, some quite flawed but many stable and broadly competitive; 13 democracies that lasted only a short time before being overthrown in their turn; and 45 new authoritarian regimes, 17 of which lasted into the new millennium.

When authoritarian governments ruled most developing countries, few political scientists interested in these countries paid much attention to the development of theories of democratic politics. Most focused instead on other questions and debated different theoretical arguments. Although some scholars attempted to extend corporatist or pluralist concepts to authoritarian settings, most focused their attention on explaining the transition to authoritarianism, the relationships between authoritarian governments and elite economic interests, and the link between international economic forces and third world authoritarianism.<sup>3</sup> Those interested in developing countries thus paid little attention to the new theories being developed for the study of democratic politics in the United States and Western Europe. The behavioralist “revolution” bypassed most of those working on developing and Communist countries for the simple reason that the primary forms of evidence used by behavioralists, survey and voting data, simply did not exist. Few in the field saw the relevance of rational-choice theorization of party and legislative politics in settings in which

2. Figures here and elsewhere are drawn from a data set collected by the author that includes all authoritarian regimes except monarchies lasting three years or more, in existence at any time since 1946, in countries with a million or more inhabitants. See Geddes 1999b for more details about the data set.

3. See Stepan 1978, Schmitter 1973, and various essays in Malloy 1977, for efforts to use the corporatist framework to illuminate the relationship between authoritarian governments and major interest groups, especially labor. See especially Skilling 1966, Skilling and Griffiths 1971, and Hough 1974 for the extension and adaptation of interest group theories to explain intragovernment conflict over policy choice in the Soviet Union.

parties, if they existed, faced no competition and legislatures, if they existed, rubber-stamped decisions made elsewhere.<sup>4</sup>

Instead, many of those interested in developing countries focused their attention on a prior and arguably more urgent question, explaining the democratic breakdowns that had occurred in numerous places during the sixties and seventies. Authoritarianism and political inequality were blamed on the effects of international capitalism (Cardoso 1973a; Evans 1979). Transitions to authoritarianism in some of the more industrialized developing countries were attributed to the need to accumulate capital more rapidly as countries industrialized. In one of the most influential arguments, O'Donnell (1973) reasoned that as dependent countries moved from the early stage of industrialization, when products were simple and capital needs modest, to a more advanced stage, when more intense capital accumulation would be required in order to produce more sophisticated products, conflict would intensify between owners and workers. Governments would face a trade-off between acceding to workers' demands and pursuing rapid growth. They would no longer be able to do both at once. As this conflict intensified, upper- and middle-class citizens would withdraw their support for democratic politics, thus producing an elective affinity between authoritarianism and advancing industrialization. In other words, although economic development is generally expected to increase the likelihood of democracy, it was argued that in late-developing countries it would have the opposite effect.<sup>5</sup>

As democratization has spread, students of developing and ex-Communist countries have not rejected old ideas but rather have simply put them aside like clothes that don't fit any more. The old arguments did not aim to explain many of the subjects that now seem interesting, such as the evolution of party systems in new democracies, the development of new legislative institutions, public opinion and voting behavior in new democracies, judicial reform and other efforts to initiate the rule of law, or the effect of political institutions in a multitude of policy domains. So many analysts have turned away from them in order to embrace the theories available in mainstream political science as the bases for explaining politics in new democracies. The research being done by these analysts

4. There are some obvious and extremely well-known exceptions to this statement, including Robert Bates, David Laitin, and Samuel Popkin, but they are exceptions. The barrage of criticism to which especially Popkin (1979) and Bates (1981) were subjected is an indication of how out of step they were with dominant ideas at the time.

5. While most developing countries remained authoritarian, political scientists interested in them read a literature not read by other political scientists, engaged in theoretical debates not followed by others in the profession, and believed arguments inconsistent with the standard theories and stylized facts of most of the discipline. See Hagopian 2000 for a useful review of the literature on political development.

contributes to the general political science literature on these subjects. Among these scholars, the state, conceived as an actor, has receded in theoretical importance, as analysis focuses on the institutionally structured choice processes of politicians whose actions determine what the state does. We now focus on the effects of particular political institutions on state decision making, trying to explain policy decisions, choice of leadership, and institutional change, rather than treating the state as a black box, unitary actor, or embodiment of elite interests.<sup>6</sup>

As the same kinds of subjects become politically important and available for study in developing and developed countries, the training of graduate students focuses less on building specific country expertise and more on tools and theories considered of general use. When the study of developing countries first began to attract substantial numbers of political scientists during the fifties, very little reliable information about the countries was available in libraries or databases. A norm developed that each graduate student should spend at least a year doing research in one country, since such immersion was the only way to acquire reasonably accurate information about how politics and other things in the country really worked. As added benefits, most who thus immersed themselves acquired fluency in the language, made lasting friends in the academic community, and developed an understanding and appreciation of the arguments and theories current among intellectuals in the country. This type of research can be thought of as a vast fact-finding mission conducted by numerous independent scholars, many of whom published books with names like *Politics and Policy in Country X, 1964–73*. These studies remain the best, sometimes the only, easily accessible sources of information about many events in developing countries.

This research strategy, though probably necessary in the circumstances, hindered theory testing. Scholars who had to invest very heavily in language training and fieldwork simply did not know enough about enough other countries to subject knowledge claims to rigorous reality checks. Moreover, the kinds of public data archives that would have allowed empirical tests of arguments in the absence of deep individual knowledge did not exist. As more and better quality data have become publicly available from a much larger number of countries, however, training norms have begun to change. Intensive fieldwork remains a necessary component of research in the comparative field, but we now recognize the usefulness of combining country-specific expertise with training in the use of tools for testing arguments and enough knowledge about the rest of the

6. In the field of international relations, where it often makes sense to treat states as unitary actors, the state continues to be a central concept. It also remains an important concept in sociology. Political scientists, however, tend to disaggregate the state into its constituent decision-making components when trying to explain particular domestic outcomes.

world to give the analyst perspective. Area specialists are gradually assimilating into the mainstream of political science.

## ■ | Then and Now: Economic Development

From the end of World War II until 1980, developing countries grew robustly on average. Except for those in sub-Saharan Africa, developing countries achieved more rapid growth on average than industrialized countries. Nevertheless, the income gap between rich and poor countries increased, large numbers of people remained desperately poor, and most developing countries suffered recurrent economic crises and bouts of severe inflation.<sup>7</sup>

Because students of developing areas have always focused as much on economic development as on more narrowly political outcomes, ideas current among economists have always influenced us. For several decades after World War II, a special branch of economics, development economics, was devoted to the study of the economies of industrializing countries. It was never a major branch of the economics discipline, but it shaped the views of most political scientists studying developing countries. This subfield of economics stressed the limited or unequal benefits of international trade, market failures, and uneven development (e.g., Hirschman 1958; Nurkse 1961; Fishlow 1978). Development economists generally supported state intervention in developing economies to overcome market failures and quicken the pace of industrialization.<sup>8</sup> The subfield tended to be less technical than other branches of economics; this made it more accessible to intellectuals from other disciplines.

A more radical challenge to neoclassical economics, the structuralist school that arose in Latin America, also influenced political scientists working on developing countries. I spell out the structuralist interpretation here in more detail than might seem warranted so that readers unfamiliar with this set of ideas can contrast them with the current orthodoxy and thus understand just how great the intellectual shift has been.

7. To understand this widening gap, one must think through the arithmetic. If a rich country has a GDP equal to \$10,000 per person and is growing at 2 percent per year, at the end of the year its GDP per person will be \$10,200. If a poor country has a GDP equal to \$1,000 per person and is growing at 10 percent per year, at the end of the year it will have a GDP of \$1,100 per person. The gap at the beginning of the year is \$9,000, but the gap at the end of the year is \$9,100, even though the poor country is growing five times as fast in per capita terms. Thus an increasing income gap does not imply slow growth by the poorer country.

8. See Deepak Lal 1997 for a detailed discussion and critique of the theoretical ideas central to development economics and a review of the evidence inconsistent with these ideas.

Central to the structuralist critique of neoclassical economics is the claim that the distribution of the gains from trade between developed and developing economies disadvantages the developing (Prébisch 1950; Singer 1950; Myrdahl 1958). Inflation, a worsening problem in developing countries from the late forties to the eighties, stems largely from two phenomena, according to structuralists: reliance on the export of primary products to earn foreign exchange and the persistence of a dual economy, that is, stark differences in development between urban and rural areas. Reliance on primary product exports, according to the structuralists, causes inflation and balance of payments problems both because of deteriorating terms of trade and because primary products experience very wide and rapid international price swings. Structuralists blame the persistence of these problems, despite rapid industrial growth, on the prevalence of foreign corporations in their economies (Sunkel 1973; Cardoso 1973a, 146–48).

In short, structuralists developed a plausible and logically coherent argument that attributed development problems to the consequences of integration into the international capitalist economy and to exploitation by foreign economic and political interests. Dependency theory, which dominated much academic thinking about development during the sixties and seventies, relied on the structuralist critique of neoclassical economics for economic theory and interpretations. Dependency theorists hypothesized two broad causes of economic difficulties: impersonal economic forces arising from the dependent country's position in the international economy and the influence of transnational corporations and their domestic allies' pursuit of their own interests in opposition to national interests (e.g., dos Santos 1970; Frank 1967, 1970; Cardoso 1973b; Brown 1963; Baran 1957; Evans 1979, 19–38; Leys 1974, 8–18).

Structuralist economic theories provided a well-developed set of arguments about why some of the stylized facts of economics apparently did not hold in industrializing countries: although investment normally causes growth, foreign investment, it was claimed, did not; or, if it did, the growth was uneven and caused distorted development. Although trade normally benefits all parties, developing countries, it was claimed, get a smaller share of the gains from trade than do the more advanced countries; although the gap between rich and poor has narrowed in most countries as they have developed, it was claimed that in late-developing countries industrialization increased inequality. These ideas influenced policymakers in developing countries as well as academics studying them.

Ideas about the causes of development difficulties have changed in response to the “inconvenient facts” observers have had to confront during the last 25 years.<sup>9</sup> Beginning sometime between the late seventies and late

9. Max Weber's term (1958).

eighties, growth faltered in nearly all developing countries outside of Asia.<sup>10</sup> Inflation worsened, in some countries reaching astonishing heights. As governments struggled to respond to the crises of the eighties, the policies proposed by structuralist economists worsened conditions where they were tried (Rodrik 1994, 81–82). Although the greatly increased cost of international borrowing after the second oil shock in 1978–79 precipitated the crises, it was twenty years of state interventionist economic policies that had left developing countries so utterly vulnerable to a rise in world interest rates.

Initially, policymakers in most developing countries, as well as the academics who advised and studied them, railed against the high interest rates and resisted interpretations that put some of the blame for the economic crisis on the shoulders of developing country policies. Over a period of about ten years, however, the neoclassical explanation of the crisis was gradually accepted. This explanation placed the blame for economic difficulties on the import-substitution industrialization (ISI) policies most developing countries had pursued from the fifties through the seventies. As Rodrik (1992) has noted, it is not protectionism, which we usually think of as the hallmark of ISI, that led to the unsustainable deficits characteristic of the ISI strategy but rather the other policies typically bundled with protectionism.<sup>11</sup> In most countries, the development strategies initiated to foment industrialization had made use of overvalued exchange rates to shift resources out of the export sector (dominated by agriculture and mining) and into the manufacturing sector. Over the years, overvalued exchange rates had the unintended consequence of discouraging exports and artificially cheapening, and thus encouraging, imports (except of products protected by tariffs). Recurrent and worsening balance of payments crises resulted. Some countries, notably, Argentina under Perón and several African countries, used an even blunter instrument, marketing boards, to capture the surplus generated by agricultural exports. Farmers were required to sell to marketing boards at below market prices, and the government reaped the profits gained from its position as monopoly middleman. These profits were then used for state investment and subsidies to private manufacturing firms. Needless to say, marketing boards also discouraged legal exports.

Industrialization strategies also led to increasing budget deficits in most countries as governments built infrastructure, invested in basic industries, and hired masses of public employees both to staff these enterprises and to reward political supporters but failed to raise taxes to pay for it all. Budget deficits led to recurrent problems with inflation. Potential investors

10. Most Asian developing countries managed their economies with remarkable success until 1997.

11. See Rodriguez and Rodrik 2001 for a review of the evidence on the effects of trade liberalization.

faced with repeated bouts of inflation and unpredictable policy interventions showed a distressing tendency to put their money elsewhere.

The budget and trade deficits generated by the policy strategy were sustainable for a remarkably long time because of inflows of foreign capital, in the form of investment, debt, and aid. When the excess capital in the hands of oil exporters after the first oil shock in the midseventies caused the price of money, that is, the interest rate, to drop in the midseventies, nearly all developing countries borrowed heavily. Had the interest rate remained low, they probably could have repaid their loans, but it did not. After the second oil shock, when developed countries raised their own interest rates in order to reduce domestic demand for oil, developing countries suddenly found themselves in an untenable situation. Most continued borrowing more and more at higher rates to cover the interest payments coming due during the first couple of years, but then Mexico declared itself unable to meet its obligations in 1982 and the debt crisis began. Suddenly, most lending to developing countries stopped. Countries that had relied on inflows of foreign capital to sustain the import-substitution industrialization policy strategy for as much as twenty-five years could no longer do so.

## ■ | Academic and Policy Responses to the Real World

Governments in most countries initially resisted the policy prescriptions of neoclassical economists. They tried various heterodox policy packages, but these invariably worsened inflation. Meanwhile, Chile, which had made the transition to market-oriented economic policies during the seventies under the repressive auspices of General Augusto Pinochet, although initially blasted by the debt crisis like the rest of the developing world, recovered very quickly. By 1985, Chile's economy had resumed steady growth. Chile was the most unambiguous example of successful market-oriented policies but not the only one. Taiwan, South Korea, and Singapore had grown very rapidly following market-friendly policy strategies for a decade or two prior to the debt crisis. Their experience was frequently cited by those advocating market-oriented policies as evidence of the success of this strategy (e.g., Little 1979; Lal 1997), but other analysts saw them as cases of successful state intervention (Amsden 1989; Wade 1990; Haggard 1990), and consequently their experience was not taken as definitive evidence in favor of orthodox policies. Two former basket cases, Bolivia beginning in 1985 and Ghana beginning in 1983, also began to make modest economic progress under market-oriented reforms, as did a few other infrequently studied small countries. By the late eighties, the evidence was piling up.

The so-called Washington consensus has grown out of the contrast between experiences with heterodox policy packages proposed by economists

influenced by the ideas of the structuralist school and experiences with more market-oriented policies (cf. Rodrik 1994). This is not to say that the market-oriented policy package has been successful everywhere it has been tried or that it has benefited everyone in the countries that have adopted it. It has done neither. But it is the only policy strategy that has been successful in any respect, and thus has been accepted, in the same spirit in which Winston Churchill embraced democracy, as a bad system but better than all the others.

With the disastrous economic failure of both the state-socialist economies and most of the developing mixed economies during the eighties and early nineties, development economics has all but disappeared as a special branch of economics with its own distinctive set of arguments and ideas, as has the structuralist school. Writing about developing and ex-communist economies is now dominated by the ideas of mainstream neoclassical economics, as is the advice given their policymakers by economists (e.g., Sachs 1986, 1993; Sachs, Tornell, and Velasco 1995; Dornbusch 1992; Rodrik 1992, 1994, 1996; Aslund 1995). Although some think the pendulum has swung too far in the direction of unfettered markets, scarcely any observers of developing countries have sustained the uncritical attitudes toward state intervention in the economy that once prevailed. Where once slow, distorted, and unequal growth were attributed to the machinations of self-interested dominant classes and the exploitation inherent in the international division of labor, now these problems are rather routinely viewed as unforeseen consequences of state interventionist development policies. In short, where once structuralists, dependencistas, and those influenced by them relied on a set of distinctive ideas for understanding obstacles to development in late industrializers, now we explain slow growth in developing countries using the same set of ideas about fiscal responsibility, a predictable policy environment, secure property rights, and investment in human capital that would explain it anywhere else.

The wide acceptance that standard economic theories apply with as much force in developing countries as elsewhere has of course entailed the abandonment of the ideas associated with dependency theory and the structuralist school in economics. Observers clung for a decade or two longer to certain unexamined ideas about the costs and benefits of economic reform to different groups, but these also are now being abandoned. Nearly all political scientists who wrote about market-oriented reform during the eighties and early nineties, before very much experience with reform had accumulated, emphasized the unpopularity of reform, especially with the organized working class and urban popular sector (e.g., Haggard and Kaufman 1992, 1995; Przeworski 1991). Since the reforms were expected to be unpopular, many in the early nineties still believed that authoritarian governments could best initiate them. If democratization had occurred before economic reform, then the most-promising political situation for initiating reforms, it was implicitly argued, would be that which

most closely resembled authoritarianism, a strong insulated executive unconstrained by the legislature (Haggard 1990; Haggard and Kaufman 1995).

Analysts were led astray by a failure to anticipate the high costs of failure to reform and the broad popularity of reforms that brought down very high inflation. It took a long time to correct this misunderstanding. Reforms under repressive governments in Chile and Mexico were examined repeatedly; the radical reform in newly democratic Bolivia got less attention. Almost no one checked to see whether politicians who initiated market-oriented reforms were really more likely to be defeated at the polls, as analysts expected.<sup>12</sup> It took a surprisingly long time for most observers to notice that the most democratic East European countries (Poland, Hungary, the Czech Republic, Slovenia) reformed their economies the fastest. As evidence of the progress of economic reform in various countries has trickled in, however, these last vestiges of the belief that different theoretical laws rule political and economic processes in developing countries are being undermined.

As mainstream theories have become widely accepted, methodologies associated with neoclassical economics and the subfield of American politics in political science have also become more common in the study of both politics and economics in developing countries. We are seeing a move away from the kind of small-*n* inductive studies that had previously been standard. Deductive arguments play a larger role in explanations, as political scientists increasingly draw insights from work by economists. As more political scientists have come to accept basic economic arguments about the causes of growth, fruitful interactions between economic theories and political scientists' real-world knowledge have become more frequent.

Large-*n* quantitative studies of the causes of growth, coups, democracy, and other "big questions" have also become more persuasive (e.g., Barro 1991, 1997; Londregan and Poole 1990, 1996; Przeworski et al. 2000). Although quantitative studies have their own set of characteristic weaknesses (the old "garbage in, garbage out" problem), recent ones have taken a big stride forward from what was once the standard practice, drawing general conclusions from the repeated observation of a few well-known cases while ignoring the rest.

These intellectual transitions have followed the real-world transitions to democracy and markets in various regions. Economic reforms have been undertaken, though by no means completed, in nearly every country in the world, and the study of economic development, like policymaking,

12. Notable exceptions are Joan Nelson (1992) who gathered information on a number of less-studied smaller countries that challenged the standard ideas and Carlos Gervasoni (1999), who showed that the initiators of economic reforms in Latin America were more likely to be elected.

is now dominated by standard economic ideas. Changes in the study of politics have occurred more slowly and discontinuously. In the regions of the world in which democratization has gone furthest, Latin America and eastern Europe, young area specialists are most fully assimilated to mainstream disciplinary norms, not only with regard to the theoretical shoulders on which they stand, but also with regard to the quantitative and formal methodologies they use. Democratization is more recent and much less thorough in Africa and Asia, and scholarship on these areas has moved less fully into the mainstream. Scholarship on the Middle East remains more isolated from many of the currents of contemporary political science, just as rulers of these countries have resisted being swept up by the “third wave.”<sup>13</sup> Theories drawn from the mainstream can be adapted to authoritarian contexts, but data limitations still constrain what can be done.

### ■ | The Study of Developing Countries in the Next Decade

The real-world transformation during the last twenty-five years has placed three main subjects on the research agenda for scholars who work on developing and ex-Communist countries: democratic politics in situations of institutional flux and fluid political loyalties, the politics of economic policy making in partly reformed economies, and the politics of ethnic mobilization.<sup>14</sup> The second is an obvious descendant of earlier work on economic policymaking. The first and third have come to the forefront of the agenda as a result of the transformations. Several very influential scholars began working on ethnic politics well before it came to the top of the field’s agenda (e.g., Young 1976; Horowitz 1985; Laitin 1986), but as ethnicity has become a political flashpoint in ever more regions of the world, the topic has moved from the margins of research on developing countries to the center. The study of democratic politics had an even more marginal place in comparative development than ethnicity prior to the eighties. Even though there were a number of long-lived democracies among developing countries before the “third wave,” the study of politics in them was

13. There are of course some impressive exceptions in Middle East scholarship, for example, Lust-Okar 2001, Lust-Okar and Jamal 1999 and Posusney 2001. And a number of younger scholars are making efforts to rejoin the broader political science community.

14. I include ex-Communist countries in the developing country category here. Before the collapse of Communism, there may have been a good reason to treat them as a separate category, but that reason no longer exists. Currently, even the most developed parts of eastern Europe are going through the same processes and facing many of the same problems as developing countries in Latin America and Asia. It therefore seems to make theoretical sense to lump them in this category, at least for now.

left to country specialists. Mainstream theories of comparative development did not include systematic analyses of democratic political processes.

These three issues are now at the top of the agenda. I shall make no attempt at a comprehensive review here. Instead, I want to highlight types and examples of work I consider especially promising.

### POLITICS IN NEW DEMOCRACIES

The institutionalist approach to posttransition politics simply takes the transitions as given, thus bypassing the cul-de-sac of unresolvable arguments about consolidation, and seeks to analyze current political processes. This work borrows and, where needed, revises theories used originally for explaining U.S. and West European democratic politics. One of the advantages of this approach is that it avoids the value-laden arguments about what consolidated democracy should be and how far some competitive but flawed real political systems deviate from whichever definition is chosen. Instead, analysts draw expectations about the effects of particular political practices from theories developed in the study of democratic politics elsewhere; if outcomes in new democracies differ from expectations, existing theories must be modified. This more-detailed and theoretically informed examination of political processes results in a more accurate assessment of exactly how and why the differences affect outcomes, if they do.

Standard theories of democratic politics begin with the assumption that citizens vote, if they do, for candidates they expect to favor the policies and provide the individual benefits and services most advantageous to themselves. Politicians are assumed to seek the continuation and enhancement of their political careers.<sup>15</sup> Democracy gives those who seek political careers an incentive to respond to the citizen interests that will help to keep them in office. The institutionalist approach to the study of politics examines the effects of formal institutions and rules on the strategic behavior of citizens as they pursue their goal of improving their own welfare and politicians as they pursue their goal of remaining in office. Institutions affect whom citizens will seriously consider voting for, which citizens politicians will be most responsive to, what kind of campaign strategies politicians will choose, how many parties will develop, how disciplined the parties will be, whether politicians and parties will focus on offering policy goods or individual services and benefits, whether effective legislative institutions will arise, whether gridlock will develop between the president and the legislature, how strong the status quo bias in policy will be, and, many believe, the likelihood that democracy will survive.

Perhaps the best-known finding associated with this approach is that

15. Some authors add sincere policy preferences to the set of primary goals sought by politicians, but none leave out the goal of continuing in office (see Aldrich and Rohde 2001; Ames 1987).

two-party systems tend to arise in countries with first-past-the-post electoral systems. In such systems, citizens are reluctant to waste their votes on small parties that have almost no chance of being elected, and politicians, knowing that citizens feel this way, are reluctant to waste their efforts forming new parties or running as third party candidates. This finding (Duverger's law) and some others about the effects of features of the electoral system on the party system have been well established by West Europeanists.

The effects of other democratic institutional variations, however, are less well understood. Of special salience in developing countries are a number of issues mostly irrelevant in western Europe and hence not yet much studied: the effects of institutional legacies imposed by outgoing authoritarian governments; the search for the institutional contributors to the perpetuation of high levels of personalism, clientelism, and corruption in open competitive democratic systems; and the need to build a theoretical understanding of presidentialism, both the effect of institutional variations in presidential powers on presidential-legislative bargaining over policy, and the effect of institutional variations in the way presidents are elected on the development of party systems.

During transitions to democracy, the outgoing authoritarian government often tries to negotiate or impose conditions that will protect its members and allies from future prosecution and from disadvantageous policy changes. Many, for example, have granted themselves amnesties for human rights violations. A number have also tried to tinker with the electoral rules in order to prevent future electoral victories by parties they distrust. The Alianza Popular Revolucionaria Americana (APRA) party in Peru, for example, was outlawed most of the time from the thirties to the midsixties, and the Peronist Party in Argentina was not allowed to compete in elections most of the time from 1958 to 1974. As O'Donnell (1973) has shown, simply outlawing popular parties is an unsuccessful institutional manipulation almost guaranteed to backfire. Most of the institutional manipulations initiated by authoritarian governments are similarly unsophisticated and have little long-term significance because new democratic governments have strong incentives to change them. The Pinochet government in Chile, however, imposed a number of mutually reinforcing institutional changes that have proved difficult to change. Some of these have had long-lasting effects, and understanding these effects accurately is necessary in order to understand contemporary politics in Chile.

Among the institutional legacies of the Pinochet dictatorship in Chile is a proportional electoral system with district magnitude equal to 2. The Pinochet government initiated this revision of the traditional high-magnitude Chilean electoral system for the explicit purpose of reducing party fragmentation and advantaging the second-largest party in the political system, which was expected to be conservative. Much ink has been spilled over the limits on representation and competition, and thus on the full realization of democracy, entailed in this institutional manipulation.

Most observers expected it to change what had been a vibrant multiparty system with a left, right, and center much like that in France into a much more centrist system in which left-leaning citizens would have no representation (e.g., Scully 1994; Rabkin 1996; Guzmán 1993). Some of the effects of the new system are easy to observe: as expected, conservative parties, the second-largest bloc, are overrepresented in the legislature; although the number of parties has not declined as expected, they have coalesced into two stable blocs offering joint lists so as not to let competition among them defeat their own side.

The effect of the new system on the representation of underlying citizen opinion, however, is not easy to observe with the naked eye. To figure it out requires a spatial model of politics borrowed from mainstream political science. By modeling Chile's system, open-list proportional representation with district magnitude equal to 2, Magar, Rosenblum, and Samuels (1998) are able to show that in districts with a low probability that either bloc will win both seats (that is, about 80 percent of the districts), candidates will cluster at the median of each bloc, not the district median. In other words, the system actually creates incentives for the election of candidates somewhat spread out in the ideological space, not clustered at the center as most observers had expected. Both left and right citizen opinion is represented in congress. We now have a much more accurate understanding of the effect of this particular legacy of authoritarianism on the representation of citizen interests.

The Magar et al. study (1998) exemplifies a strong trend in the study of politics in new democracies. Analysts use theories and models developed in the study of politics in established democracies to figure out the effects of the particular features of new democratic systems that have not been fully understood by more casual observers. New democracies are experimenting with many institutional innovations. Scholars trying to figure out the effects of these innovations rely on the same theories that have structured our understanding of democratic politics in the older democracies for decades, adapting them to new conditions as needed. Empirical testing of arguments is becoming standard. With such sturdy theoretical shoulders to stand on, progress in working out the effects of electoral rules on Latin American and East European party systems, campaign strategies, and legislative behavior has been quite rapid.

Scholars writing about numerous different countries have produced an impressive body of literature on the effects of the institutional variation in new democracies, in the process adding considerably to preexisting literature.<sup>16</sup> Most new democracies have presidential or semipresidential systems. Presidential elections create centripetal incentives in party systems in the same way that single-member legislative districts do.<sup>17</sup> Where legisla-

16. For a very useful summary, see Carey 1998.

17. But see Filippov, Ordeshook, and Shvetsova 1999 for a more nuanced view.

tors are elected by proportional representation, as they are in most new democracies, the electoral system is pulled in both directions, toward a two-party centrism by presidential elections and toward more ideologically dispersed multipartism by PR legislative elections.

Work on the Latin American presidential systems has discovered that rules that affect the size of presidential coattails determine which pull is stronger. Where presidential and legislative elections occur at the same time, presidential coattails are strong and parties that cannot compete for the presidency often fade away; two-party systems tend to emerge. Where elections for different offices occur on different schedules, parties that have no hope of winning presidential elections can nevertheless continue to do well in legislative and municipal elections, and thus survive (Shugart 1995; Shugart and Carey 1992). Presidential runoffs also appear to encourage the persistence of small parties. Rather than forming preelection coalitions, small parties enter the first round in order to establish their bargaining power as coalition partners for the second round. Moreover, legislative elections occur at the same time as the first round of the presidential election (if they are concurrent), which means that small parties run in them as well. For these reasons, party fragmentation tends to be greater in countries with presidential runoffs.<sup>18</sup> In parliamentary systems, district magnitude has the greatest effect on party fragmentation, but in presidential systems, district magnitude has less effect than run-offs and concurrent election schedules (Jones 1995).

The effects of a number of electoral rules have been pretty thoroughly worked out. These include, besides the effect of presidential runoffs and different election schedules on party fragmentation noted above: the effect of preference voting, term limits, and running multiple lists under the same party label on party discipline in the legislature and candidate campaign strategies (e.g., Ames 1995a, 1995b, 2001; Carey 1996; Taylor 1992; Archer and Shugart 1997; Cox and Shugart 1995; Morgenstern 1999). These are the nuts and bolts of democratic politics, and a great deal of progress has been made in figuring them out. With the institutional basics understood, it then becomes possible to estimate the effects of important societal characteristics such as ethnic heterogeneity with reasonable confidence (Ordeshook and Shvetsova 1994).

In some new democracies, parties seem to be ideologically incoherent, legislative decision making is obstructed by disorganization, legislative and interbranch alliances are held together by pork rather than shared policy goals, and campaigns focus on the distribution of individual gifts in-

18. There has been some controversy over the effects of runoffs, but the balance of the evidence at this point supports the claim that they encourage party fragmentation. It is very hard to disentangle this question empirically because runoffs have generally been initiated in countries with fragmented party systems, so it is hard to judge whether the runoffs are cause or effect.

stead of policy promises. Other new democracies, however, function much as long-established European democracies do; parties are ideologically cohesive and disciplined, interbranch relations depend on party loyalties, and campaigns focus on party programs rather than individual candidates. Political systems of the first type are sometimes called candidate centered, and those of the second type, party centered. Explaining these differences ranks high on the research agenda for institutionalists.

Some progress has been made in identifying the political institutions that perpetuate candidate-centered systems. Most has been written about electoral rules that result in intraparty competition. These include the open list in PR systems, in which citizens may vote for individual candidates and list order is determined by the number of votes received; *sublemas*, multiple lists run under the same party label; and the single nontransferable vote, in which parties field multiple candidates in multi-member districts, citizens vote for one individual candidate, and seats are awarded on the basis of votes to individual candidates. Carey and Shugart (1995) have suggested that four features of ballot structure and rules that guide the translation of votes into seats determine where political systems fall on the continuum between candidate and party centered. Other observers have noted the additional influence of governors' deleterious effects on national party discipline in federal systems (Samuels forthcoming; Mainwaring 1997) and the importance of noninstitutional factors such as the preferences of the very poor and uneducated for immediate individual goods rather than promises of poorly understood policy changes that may never materialize (Desposato 2001).

The first steps have also been taken toward figuring out some more complex institutional issues. Scholars are beginning to build an understanding of presidential powers and the relationship between presidents and legislatures. These studies begin from the premise that the probability of democratic breakdown is increased by conflict between the president and the legislature.<sup>19</sup> Scholars have explored two factors that might contribute to potential conflict or stalemate. The first is divided or minority government. Conflict or stalemate is obviously less likely if the president's party has a majority in the legislature, so analysts pursuing this line of thought emphasize the electoral rules that increase fragmentation in the party system, which in turn increases the likelihood of minority presidents.

The second involves the president's constitutional powers to set the legislative agenda, veto legislation, and issue decrees, that is, the president's power to pursue his own agenda even without legislative support. The implicit idea here is that stalemate is less likely if the president can do much of what he wants without legislative support. Carey and Shugart (1998a)

19. Although it is widely believed that minority presidents and divided government increase the likelihood of democratic breakdown, Cheibub (2002) shows that they have no effect on democratic survival.

have proposed an index to measure these presidential powers, though they have not shown what effects they have. A recent discussion of presidential powers by Mainwaring and Shugart (1997a) attempts to combine the constitutional powers emphasized by Carey and Shugart in other work with what they call partisan powers, meaning essentially the amount of support the president has in the legislature. This addition brings the notion of presidential powers closer to what we think of intuitively as strong presidents. At this point, these arguments have not gone very far either in terms of the- orization of the relationship between presidents and legislatures or in terms of showing clear empirical effects of different arrangements. I have not yet seen spatial models and the veto players argument used to elucidate these arguments in a systematic way, but since this is an obvious thing to do, I am sure it will be done.<sup>20</sup>

Legislatures in developing and ex-Communist presidential systems have traditionally received much less attention than presidents because analysts have considered them less influential. Legislatures are beginning to be taken more seriously, however, and some pathbreaking work has recently appeared. John Londregan's (2000) study of the Chilean Senate is the first to offer an in-depth analysis of legislative committees in a contemporary Latin American legislature.<sup>21</sup> Tom Remington and Steve Smith (1995; 1998a, b) have done a series of studies that draw on the literature on the U.S. Congress to analyze the Russian Duma and its relationship to the president. Legislative studies are on the research frontier for those working on new democracies.

The work noted above focuses on the effects of political institutions, but such analyses always lead back to the prior question: What caused the institutions in the first place? The period of transition has been a good time to investigate this question because a large number of countries have chosen new democratic institutions or modified old ones. Scholars analyzing these choices have shown that in both Latin America and eastern Europe new political institutions have been chosen to further the electoral interests of those who served on the roundtables, legislatures, and constituent assemblies that picked them (e.g., Frye 1997; Remington and Smith 1996;

20. Spatial models and veto players arguments have been used to explain specific processes in specific countries. For example, Lisa Baldez and John Carey (1999) model the Chilean budget process using a veto players model. But no one working on developing countries has used these tools to work out the full logic of how agenda setting, decrees, and vetoes, all of which vary quite a bit in detail among countries, work.

21. Ames (1987) analyzes the role of committees in the Brazilian Chamber of Deputies between 1946 and 1964. Several other scholars studied legislatures in various countries during the seventies (Agor 1971; Hoskin, Leal, and Kline 1976; Pakenham 1970), but contemporary studies drawing on the very sophisticated U.S. congressional literature have barely begun to appear.

Colomer 1997; Geddes 1995, 1996; Benoit and Schiemann 2001; Luong 2000; Bernhard 1997).

The convergence to mainstream theories and methodologies for analyzing politics in democratic developing countries is occurring because a very large body of theory on democratic politics exists, and those who are making use of it can see that it gives them leverage for understanding a good deal of what is going on. Like any other innovation, it is spreading because it helps users to do what they want to do, which is to explain various aspects of democratic politics. Once the countries that analysts were interested in democratized and politics became more orderly and transparent, useful theories were available for understanding them. Voting behavior could be observed and analyzed, and it became possible to do surveys.<sup>22</sup> Democratization opened up niches for certain kinds of work, and scholars, many of them young and well trained, moved into them.

### THE STUDY OF ECONOMIC POLICY AND DEVELOPMENT

In the study of economic policy and development, economists (e.g., Alesina and Drazen 1991; Fernandez and Rodrik 1991; Rodriguez and Rodrik 2001; Aslund 1995; Barro 1997) lately have done much of the most influential work. Let me single out just one that has changed the way I think about the politics of economic reform. Dani Rodrik (1994) models the effect of trade liberalization on welfare. He shows that only during periods of high inflation and deep crisis do the welfare benefits of trade reform outweigh its redistributive effects. This is a clear and concise explanation for one of the repeated findings to arise in inductive studies of reform efforts: reforms that bring down very high inflation face little opposition, even if they also impose costs on some groups, and the politicians who initiate them tend to be reelected. Examples include the radical Menem reforms in Argentina, where inflation had reached about 3,000 percent before the initiation of liberalization; the equally radical reforms initiated in Bolivia in 1985, where inflation had risen to about 40,000 percent (the finance minister in charge during reforms was later elected president); the Fujimori reforms in Peru, where inflation had topped out at about 7,500 percent; and the more moderate reforms that brought inflation down from about 2,000 percent in Brazil in 1995 (where, as in Bolivia, the finance minister credited with initiating the reforms was later elected president).<sup>23</sup>

I discuss this article because it has the characteristics one would hope

22. See Fleron 1996 for a review of much of the recent work on public opinion in ex-Communist countries.

23. Inflation figures from the International Monetary Fund, *International Financial Statistics Yearbook*, 1999, 127.

to find in a model. It is clear and easy to understand. It provides a persuasive, theoretically grounded explanation for a set of observations many who study economic reform had noticed. We already knew in a loose kind of way that when things got really bad people who might otherwise have been expected to oppose reform would support it. Various ad hoc reasons for this have been suggested, but Rodrik's argument is at once connected logically to a strong body of economic theory and entirely plausible. Once I had read it, I put this aspect of reform onto the very small shelf where I store phenomena successfully explained.

Political scientists doing exciting work on economic policy have tended to draw basic insights from Douglass North (1981, 1985, 1989a, b, 1990), who attributes the persistence of economically inefficient institutions to their usefulness to rulers. This perspective helps explain the initiation and persistence of obviously dysfunctional laws and institutions. It can be extended to explain a great deal of behavior in the incompletely reformed economies characteristic of developing and ex-Communist countries (e.g., Frye 2000; Shleifer and Vishny 1998). Resistance to completing reforms in Russia comes, as shown by Hellman (1998), from those who have grown rich from partial reforms and their allies in government, not from those hurt by reforms. One of the reasons corruption is hard to end is that it funds political campaigns and raises the incomes of officials and politicians (Shleifer and Vishny 1993; Geddes 1999a). State ownership of enterprises that could be more efficiently run by private owners allows politicians to reward supporters with jobs, postpone politically dangerous mass layoffs, and supply subsidized fuel and raw materials to favored private firms. The politicians and officials who can trade these resources for political support tend to obstruct privatization (Geddes 1999a).

One of the legacies of authoritarianism in many developing and all ex-Communist countries is an overcentralization of decision making, useful to rulers for maintaining control but economically inefficient. As countries democratized, subnational levels of government in many gained control of more decision-making and revenue-extracting power than they had had before. The World Bank and other international agencies and nongovernmental organizations supported the impulse toward decentralization, believing it would contribute to more participatory politics and also more efficient allocation of scarce resources. A substantial body of theory in economics and political science supported these beliefs (e.g., Tiebout 1956; Weingast 1995). Brazilianists and Russian specialists, however, not to mention Yugoslav specialists, tend to take a much more jaundiced view of decentralism and federalism (Mainwaring 1997; Samuels forthcoming; Pleština 1992; Treisman 2000a, b; Andrews and Stoner-Weiss 1995; Ordeshook 1996). The debate about whether federalism is good or bad in general probably cannot be resolved. Instead, it seems far more fruitful to try to figure out how it works in different contexts and issue areas.

Dan Treisman's (1999b; see also Shleifer and Treisman 2000) analysis

of the effects of Russian federalism on taxation and the initiation of economic reforms takes some important steps toward doing this. Using the intuitions provided by common-pool models, Treisman argues that revenue sharing creates incentives for provincial political leaders to shirk collecting taxes and to collude with firm managers to hide resources. Since the amount of revenue provinces get is not tied to the amount they collect, provincial leaders have no reason to put effort into tax collection. In fact, they can keep more revenues at home, either for their personal use or to spend responding to constituents' needs, if they collude with businesspeople to underreport their assets and then split the taxes saved.

Treisman's argument explains why Russian tax revenues have fallen, which in turn explains a good deal about the inability of the central government to pursue a coherent reform policy and provide an adequate social safety net. The argument will not generalize to all federal systems because the details of revenue collection and sharing differ across systems, and the consequences would in any case be less pernicious in countries in which the provincial populations see themselves as less different and feel less alienated from the center than in Russia (Treisman 1999a). Jones, Sanguinetti, and Tommasi (2000) use a similar model for analyzing the Argentine federal system, thus demonstrating the usefulness of the approach beyond the specifics of the Russian case.

## THE POLITICIZATION OF ETHNICITY

The central theoretical difficulty for scholars who seek to understand the politicization of ethnicity is that ethnic identity is changeable and malleable, as are the interests and attitudes any particular identity implies. This means that the interesting questions have to do with how, when, and why people identify with one of their ethnic options rather than another, perceive their grievances as caused by their ethnicity, feel threatened by other ethnic groups, and decide which among their options for action and inaction to pursue in order to protect themselves or improve their situation (Brubaker 1992 and 1996; Marx 1998). In the face of this analytically difficult substantive problem, observers have noted that ethnic identity is constructed, and described ways it has been constructed in various settings. This work has forced the old idea of ethnic identity as built on unchanging primordial loyalties into the dustbin of history. Explaining how, when, and why ethnicity is constructed dominates the current research agenda for students of ethnic politics.

Questions dealing with ethnicity and identity have been addressed from many different intellectual perspectives. I focus here on those that seek to explain the political consequences of ethnic identity: primarily, political mobilization, peaceful coexistence, violence, assimilation, and secession. Most of the time people with differing ethnic identities live side by side peacefully whether they feel hate, fear, envy, contempt, admiration, or

liking for each other. Explanations for the kinds of mobilization, violence, and attempts to secede that call ethnicity to our attention have to explain what causes the usual state of peaceful coexistence to break down (Fearon and Laitin 1996, 2000).

Even though people feel ethnic loyalties intensely and can be motivated by them to incredible and seemingly irrational acts of heroism or cruelty, scholars have successfully used systematic arguments to explain some behavior motivated by ethnicity. Several authors have suggested arguments for why political entrepreneurs mobilize support within ethnic groups, rather than, for example, economic interest groups (Bates 1983; Laitin 1995). Fearon (1998c) has shown that the inability of new states to credibly commit to protecting the future rights of a minority can make it rational for the minority to fight to secede, even though both groups would have been better off if the commitment could have been made and violence avoided. Posner's study (1998) of the choice among options in Zambians' ethnic repertoires shows the same kind of instrumental reaction to circumstances. He shows that voters preferred candidates from their own (small) tribes when their choices were limited to district-level candidates all from the same party. In multiparty elections, however, both before and after the single-party interlude, voters chose candidates from their (larger) linguistic groups without regard for tribe within a linguistic group. In other words, if tribal candidates could win, voters preferred them, but when a broader support base was needed in order to win elections, voters' loyalties moved up to the next higher level in the hierarchy of potential identities, language group.

David Laitin's wise book (1998a) about Russians stranded in non-Russian republics after the collapse of the Soviet Union stands out in the current literature on ethnicity. Laitin uses a tipping model to gain insight into the choices of individuals about whether to learn the titular language. The tipping model illuminates the contingent nature of decisions about whether to assimilate, fight, or flee. Tipping models help us to understand sudden mass changes in behavior, such as the transition from quiescence to mass mobilization during the collapse of Communism in Romania and East Germany (cf. Przeworski 1986).<sup>24</sup> Laitin also explains the behavior of potential ethnic leaders. He links the strategies of potential nationalist elites to the availability of career opportunities at the center of a multiethnic state to explain their decisions about whether to mobilize ethnic opposition to the center. These arguments, in interaction with information about the economic and demographic context in the various post-Soviet republics, explain why Russians in Estonia are learning Estonian and trying to assimilate while Russians in Kazakhstan refuse to learn Kazakh and keep their suitcases packed.

24. Lohmann (1994) suggests an alternative way of thinking about the popular mobilization in East Germany and other similar phenomena.

Laitin's rationalist arguments are embedded in a very thorough empirical investigation into how real people perceive their situations in four ex-Soviet nations. Arguments in the book are supported by survey research, in-depth observation and interviews with a small number of families, and content analysis of newspapers. The result of these multiple forms of evidence is an extremely persuasive interpretation of events.

Laitin's work shows that although people feel ethnic loyalties deeply, they also respond rationally to the situations they face in everyday life as they define and redefine their ethnic identities. In my judgment, this book exemplifies the best work currently being done on ethnicity, in that it uses some of the tools provided by economic models to help understand otherwise puzzling or seemingly irrational actions and that it also explores in depth and detail how people perceive their situations. Several different kinds of empirical detail support the arguments and interpretations made in the book.

Although the study of the effects of ethnicity remains very diverse and research methods within the field remain quite contested, here too we see that models drawn from economics are being used in some of the most influential studies, and rigorous tests of arguments in at least a few.

## ■ | Methodological Convergence

Changes have begun in the way students of developing countries do research, propose knowledge claims, and decide whether to believe the knowledge claims proposed by others. The changes have occurred in at least three important areas: our response to economic models of politics; the quality, amount, and availability of data relevant to the interests of scholars working on developing countries, along with our level of statistical skills; and standards for what constitutes persuasive evidence. The second two are related but not as closely as might at first appear.

The implications of the simplest economic models, such as the collective action problem, seem to have percolated throughout the comparative field. Where once analysts working in the tradition of comparative historical sociology might have been accused of failing to understand the implications of the logic of collective action, that is no longer true (cf. Thelen 1999). Most economic models have much narrower potential areas of application, of course, but that is not the only reason they have spread less far. Many comparativists find abstract models that so grossly simplify reality intellectually uncongenial. Nevertheless, the models are spreading and becoming more influential because they are levers and pulleys for the imagination. Just as you can lift more with a lever than with your bare hands, you can sometimes figure out more about a situation if you have a

few simple models to apply to it than if you have to rely only on your untutored intuitions.

It may be useful to divide models into two categories: those, like the collective action problem, which lay out a logic that applies to and may illuminate many situations and those which aim to explain a particular real-world outcome. Besides the collective action problem, the first category contains a number of other logical explications of simple interactions among individuals, such as tipping games, chicken, battle-of-the-sexes, Arrow's paradox, and divide-the-dollar. These models are not in themselves falsifiable; they are logical constructions that may or may not be useful for interpreting particular situations. The common-pool logic used by Treisman to help explain tax problems in Russia is an example of the usefulness of this kind of model, as is the tipping model used by Laitin to illuminate sudden changes in language acquisition.

The second category contains the many models that purport to explain specific patterns of outcomes and are thus, in principle if not always in practice, falsifiable. Like other arguments, they can be right or wrong, and they need to be supported by evidence to be persuasive. Because they are simple, logically consistent, and based on explicit assumptions, they can sometimes illuminate a relationship that had not been obvious before. Such is the case with the Rodrik (1994) model of the relationship between high inflation and the effects of trade liberalization on citizen welfare described above. It adds crispness and clarity to the explanation of an empirical regularity many observers had noticed. We would not, however, take it seriously if it did not conform to empirical evidence.

Both kinds of models are being incorporated into studies by comparativists. The first kind has been used as a central analytic tool in explanations of civil-military relations (Hunter 1997), administrative reform (Geddes 1994), peasant rebellion (Popkin 1979), authoritarian breakdown (Geddes 1999b), and regime transition (Colomer 1995; Przeworski 1992). These kinds of models also play a subsidiary role in a very large number of studies. Collective action problems, for example, explain why peasants fail to organize effectively in Bates' study (1981) of African agricultural policy and why concentrated industries get more of what they want in the Latin American policy process in Frieden's study (1991a) of the effects of the debt crisis.

The second kind of model has the same status as any other proposed argument; we should believe it if evidence supports it. Most of our understanding of the effects of different kinds of economic policies now derives from economic models of the second kind, whether we have individually read them or not.

In discussing the increasing use of economic models by scholars interested in developing countries, I do not mean to suggest that they have become standard or commonplace. They have simply become more common and familiar than they used to be, and I think many of us rely on

them for our understanding of some subjects even without being aware of the source of our insights.<sup>25</sup>

Large-*n* studies have also begun to play a greater role in the comparative development field as data have improved in quality, relevance, and accessibility. Quantitative studies can be used both to test theories and for inductive explorations. I will deal with theory testing below when I discuss standards of evidence. Here I focus on quantitative studies with an inductive thrust. At one end of the continuum from inductive to theory testing are broad quantitative studies that seek to discover the causes of some outcome by trying out indicators of as many potential causes as possible in statistical models. All inductive studies are guided to a considerable extent by the theories and speculations available in the literature, but at the most inductive end of the continuum analysts appear quite theoretically agnostic and eclectic; they throw everything they can think of into a regression or some other statistical model and see what patterns seem to emerge. Some recent large-*n* studies of growth and effective governance have these characteristics (e.g., Acemoglu, Johnson, and Robinson 2000; LaPorta et al. 1999), as do some studies of why coups occur (Londregan and Poole 1990, 1996) and what causes democracy (Barro 1999).

Other studies, which fall between the inductive and theory-testing ends of the continuum, aim to find out if some particular potential cause really has the effects that have been suggested. Michael Ross's study (forthcoming) of the effect of oil and other natural resources on the likelihood of authoritarian government is a good example of this kind of work. It shows that reliance on oil exports increases the probability of authoritarian government; the finding is persuasive because Ross shows that it does not depend on any particular measurement or specification decision. Another such study is Bienen, Londregan, and van de Walle's (1995) of the effect of ethnic heterogeneity on leadership turnover in Africa. They show that ethnic heterogeneity does not increase the fragility of African governments. Gasiorowski and Power (1998) use a large-*n* study to test the argument that parliamentary systems are more stable than presidential, and find that among developing countries, they are not.

None of these studies tests a particular theory, although in all cases more than one causal mechanism connecting purported cause and effect has been suggested in the literature. Instead, these studies aim to establish whether a correlation suggested in the literature really exists between a particular potential cause and an outcome, while remaining somewhat agnostic about why the correlation might exist. These studies occupy a midpoint on the induction-theory-testing continuum because they confirm (or fail to confirm) empirical regularities without trying to confirm particular theo-

25. See Levi 2000 for an insightful and much more thorough review of the use of economic models in the comparative field than is possible here.

retical explanations of the empirical regularity. Where they fail to confirm a hypothesized relationship, as in the Gasiorowski and Power and Bienen, Londregan, and van de Walle studies, the results cast doubt on all the arguments that have suggested that such a relationship should be expected to exist. Where the analyst shows that the relationship does indeed exist, however, the task of explaining why it exists remains on the research agenda. These kinds of large-*n* studies have become much more important in the field of comparative development as a means of discovering and confirming empirical regularities, but they should not be interpreted as statistical expressions of theories. They are empirical investigations—like case studies but more rigorous and structured. Once patterns have been established empirically, theories can be proposed to account for them. Then the implications of the theories can themselves be tested.

Large-*n* studies had a bad reputation among comparativists for a long time because of the poor quality of most of the data available and because of the feeling that most of the quantitative work being done was mechanistic and insensitive to the real meaning of the indicators thrown into regressions. These issues remain challenges with which those doing large-*n* cross-national studies must contend. Some of the data sets available off the shelf have well-known and obvious flaws that should, but do not always, give analysts pause when they decide to use them. Sometimes cases are left out of analyses that the reader suspects would overturn results if included. Sometimes model specifications determine results. Many large-*n* studies are unpersuasive because we suspect that if the author had included other cases or a different time period, had specified the model more appropriately, or had used an unbiased indicator for some variable, the results would have been different (see Levine and Renelt 1992). The best large-*n* studies do add to our store of knowledge, however, and I think everyone in the field nowadays accepts this.

What counts as adequate evidence to support an argument and, more broadly, the importance of systematic empirical confirmation of arguments remain contested in the comparative development field. On the side influenced by the political science mainstream, it is expected that arguments will be tested using observations chosen in ways that do not bias results, that measurements and classifications of key concepts will be explicit, that the author's argument will be confronted with rival arguments in the literature, and that some means will be found for holding constant other factors known to affect the relationship of interest but not themselves considered interesting. These conditions can be met through the thoughtful use of statistics, but quantitative methods are neither necessary nor sufficient to achieve them. Careful qualitative research designs can also do so. Those who disagree with these mainstream-influenced methodological norms either do not see their scholarly task as including the systematic presentation of evidence or show evidence that, however otherwise convincing, fails to conform to one or more of the expectations listed above.

The line dividing the two sides on this issue crosscuts the main cleavage between more traditional comparativists and those who borrow models from economics and theories from the study of American politics. Those who question the constraints imposed by the norms of evidence that characterize much of the rest of political science include historical institutionalists and comparative historical sociologists, on the one side, and economic modelers in both economics and political science, on the other.

Historical institutionalists and comparative historical sociologists (e.g., Rueschemeyer, Stephens, and Stephens 1992; Yashar 1997; Collier and Collier 1991) place a high value on the importance of supporting evidence.<sup>26</sup> Their studies usually describe with great care the processes and interactions leading up to the event or change of interest. Among practitioners, these descriptions are considered solid evidence, especially useful for illuminating causal processes and sequences. Many historical institutionalists see conventional norms about case selection and the number of observations needed to test arguments as limiting their ability to focus on the processes and mechanisms central to understanding. They defend several methodological strategies that many nonpractitioners see as undermining the logic of inference: selection of cases on the basis of outcomes (Collier and Mahoney 1996), the use of small-*n* research designs without regard for the number of causes being examined (Mahoney 2000b), and the idea that path-dependent processes cannot be explained by standard causal models (Abbott 1988; Mahoney 2000a).

The disagreement about research norms that separates historical institutionalists and comparative historical sociologists from those more influenced by the political science mainstream is often articulated as a disagreement about the value of small-*n* versus large-*n* research designs (e.g., Lieberson 1991), but number of cases is not the only, and possibly not the most important, issue.<sup>27</sup> A small-*n* study in which the cases are chosen to reduce the likelihood of introducing bias, concepts are defined in concrete ways that leave the classification of cases unproblematic, and rival arguments are taken seriously can be much more persuasive than a large-*n* study using a truncated sample and indicators that fail to capture the meaning of concepts or turn rival arguments into strawmen. If, however,

26. For exemplars of the historical-institutional approach, see those cited in Thelen 1999 and Thelen and Steinmo 1992.

27. See Mahoney 2000b for a careful elucidation and defense of small-*n* research designs, especially in comparative historical sociology. It is noteworthy, however, that the methods Mahoney suggests would be most useful for increasing the persuasiveness of small-*n* research strategies, the method of concomitant variation (i.e., ordinal measurement of enough cases so that simple statistical tests can be carried out) and pattern matching (the testing of implications of the argument at a lower level of analysis), both increase the *n*. The number of observations (*n*) used in any particular test of an argument need not be the same as the number of ordinary language cases, often countries, that are the main focus of a study.

small-*n* studies claim to test arguments using evidence that comes from the same cases from which the arguments were induced, use unclear criteria for classifying into categories, and ignore rival arguments or turn them into strawmen, then many will find them unpersuasive.

In contrast to historical institutionalists, modelers, whether in economics or political science, often seem to reject the idea that arguments need to be confirmed empirically. Arguments are frequently published with just a couple of glancing references to real-world events to supply plausibility. And there is a substantial group of consumers of these arguments who seem to find economic models so inherently plausible that they need no empirical confirmation. There are of course some models that are so fruitful in terms of the jolt they give the imagination that empirical confirmation has little or nothing to do with their value. Most models, however, are simply arguments written down in mathematical form. The producers and consumers of these models often seem to find them persuasive without empirical confirmation, but, as with the evidentiary standards of historical institutionalists, those from outside often do not.<sup>28</sup>

Usually these two groups have little to do with each other, but Bates et al. (1998) seem to be trying to bridge the gap by embedding economic models in careful descriptions of events under the rubric of “analytic narratives.” Bates et al. do not of course reject testing arguments, but they do downplay its importance. They note that many economic models cannot be tested directly but give little attention to the possibility or desirability of testing the implications of arguments that cannot themselves be tested. Instead, they suggest that narratives demonstrating how models work in one or a few particular instances supply persuasive evidence of the usefulness of the models for explaining outcomes.

In other parts of the comparative development field, however, standards about what constitutes persuasive evidence are slowly converging to the mainstream. In the study of democratic political processes, more and more arguments are being tested carefully. Barry Ames (1987, 2001) deserves special mention as an exemplar and forerunner of this trend. Early work on the effects of political institutions, including my own, tended to be supported by rather spotty evidence, and much still does (e.g., Shugart 1998; Mainwaring and Shugart 1997b). Analysts have gradually begun to produce more empirically thorough support for their arguments, however. Mark Jones’ investigation (1995) of the institutional causes of party fragmentation and minority government in Latin America, John Carey’s analysis (1996) of the effects of term limits, and John Londregan’s analysis (2000) of the Chilean Senate are good examples in the Latin Americanist

28. Green and Shapiro (1994) have articulated the best-known critique. Even though many of their specific claims are incorrect, they have identified a central weakness of rational choice (of which economic models are a subset): that many arguments are not tested empirically.

field. Remington and Smith's studies (1995, 1998b) of the Russian Duma set a high standard for research on ex-Communist countries.

In the study of economic development, there is something of a bifurcation between models and empirical work. Many of the models have not been tested, and many of the empirical studies do not seek to test particular models. Greater interaction between models and data remains on the research agenda in studies of the causes of economic policymaking and growth. Data are readily available for testing many of the models of economic policy and growth, however, and the study of economic development by political scientists seems to be moving rather rapidly in the direction of using large-*n* studies both to explore relationships and to test arguments.

Most empirical work on identity and ethnicity has been descriptive or interpretive, while most models and rational-choice arguments have not been supported by systematic arrays of evidence. This situation is beginning to change, however, as demonstrated in the work by Laitin (1998a) and Posner (1998) described above.

## ■ | Conclusion

As substantial parts of the developing and ex-Communist world have converged on the political and economic systems previously characteristic only of the most industrialized countries, the academic field of comparative development has begun to converge on the theories and methodologies originally developed to study the politics of those same industrialized countries. The standard arguments of neoclassical economics have replaced structuralist and dependency-influenced explanations of development and growth. The study of political processes in new democracies has blossomed, fertilized by theories and methods previously honed in the study of the older democracies. Approaches to the study of ethnicity remain eclectic, but a few scholars in this area too have begun collecting systematic data and making use of economic models as analytic tools.

This convergence is uneven and incomplete, depending on the degree of real-world convergence in the region being studied. This unevenness is especially notable for those whose research focuses on politics. For analysts interested in Latin America, eastern Europe, and democratic developing countries in other parts of the world, the sudden access to a set of fruitful and well-developed theories and methodologies has opened up exciting research frontiers in multiple areas, and high quality new research is pouring out. Understanding is growing rapidly about how new democratic institutions are chosen and changed; the effects of new democratic political institutions on emerging party systems, legislative behavior, and bargaining between the executive and the legislature; and what shapes the opinions and voting behavior of citizens in new democracies.

The intellectual situation for those interested in areas that have not democratized, however, seems to me more difficult. They can adopt the fruitful approaches of the mainstream, and a few have done so (e.g., Manion 1996a, b; Lust-Okar 2001), but they cannot stand on the shoulders of several decades of democratic theory in the way others can. Moreover, their access to good quality data remains limited. Fewer scholars are now studying the kinds of governments they study, and the scarcity of useful theories of day-to-day authoritarian politics remains as severe as ever, perhaps more so, as many observers impressed by the spread of democracy seem to have forgotten that many of the world's people still live under autocracy.

# *The Political Economy of Business and Labor in the Developed Democracies<sup>1</sup>*

## ■ | Introduction

The past decade has witnessed important innovations in the study of the political economy of business and labor in the developed democracies. For many years the literature in this area was organized around the concept of democratic corporatism. This concept was closely associated with a few northern European countries that seemed to provide a model of how strong and encompassing unions—backed up by labor-sympathetic governments—could mold political-economic institutions to sustain a highly successful social-democratic variant of capitalism that featured low unemployment, universalistic welfare benefits, and an egalitarian income distribution. However, the concept also served as the key point of reference within a general analytic framework for the study of labor across the developed democracies.

Beginning in the 1980s, changes in the political and economic context brought new strains to labor relations across the advanced industrial world. More turbulent international markets and a resurgence of neoliberal ideology complicated politics as usual in the classic democratic-corporatist countries, but more importantly in the present context, these developments also revealed important shortcomings in the analytic framework that had developed around the concept of corporatism. The literature in this area had yielded important insights by drawing attention to the way

1. I thank James Alt, David Collier, Chris Howell, John Huber, Ira Katznelson, Cathie Jo Martin, Helen Milner, Paul Pierson, Jonas Pontusson, Theda Skocpol, David Soskice, Sven Steinmo, and Peter Swenson for comments.

that institutional arrangements mediated common international pressures faced by the advanced industrial countries in the 1970s and early 1980s. Yet the framework seemed singularly ill suited to understanding the *dynamics of change* in these political economies in subsequent decades, when the nature of the problems they faced shifted and the institutions themselves were part of what was getting renegotiated.

This chapter assesses the state of the discipline in the political economy of business and labor in the developed democracies. It does so by addressing a key development in the literature that has reoriented the study of labor, social democracy, and the welfare state—prompting a reinterpretation of the results of a great deal of previous work while setting important agendas for the future as well. Whereas the traditional corporatism literature had focused most of its attention on *labor* (the structure and strategies of unions), a newer body of work has redirected our attention to the contribution of *employer* interests and strategies in shaping the political economy of labor in the developed democracies.

The literature that “brings capital back in” (Swenson 1991) comes in two broad variants. One highly influential stream emphasizes the role of employers as crucial partners in *cross-class coalitions* that preside over the genesis and then contribute to the reproduction of key political-economic institutions, including but not limited to wage-bargaining institutions. This body of work takes issue with the corporatism literature’s almost exclusive focus on union organization and strength as the most important constitutive force in shaping institutional and political outcomes. It focuses instead on employer interests—and especially on conflicts of interest among capitalists—in terms of preferred political and institutional outcomes. In this literature, cross-class alliances that bring together segments of capital and of labor are what generate and sustain key institutional outcomes and policies.

A second literature, which emerged at about the same time, similarly drew attention to the role of employers but, in this case, employers were brought back in through the concept of *employer coordination*. Like the first, the second literature rejects the corporatism literature’s heavy emphasis on the strength and organization of labor, but in this case replaces that with an analysis of cross-national differences in the capacity of employers to coordinate among themselves. Here again, the focus is on employer interests. However, in this body of work the interests of firms in their relations with labor flow from their embeddedness in a broader set of interlocking institutional arenas that define and sustain distinctive trajectories of capitalist development, or “varieties of capitalism” (Hall and Soskice 2001b).

These two perspectives are almost always treated as complementary, and in many ways they are. Both have certainly played an enormous role in reorienting labor scholarship in such a way as to highlight the constitutive power of employers and employer organizations in shaping political-economic institutions and outcomes. Each perspective has its own

following, and there are a number of scholars who are producing excellent work at the intersection of the two. However, on closer examination we can see that the central analytic tools that anchor each of these approaches—in the first case, the notion of cross-class coalitions and in the second case, the notion of employer coordination—represent very different kinds of variables. The literature on cross-class coalitions draws attention to the *political settlement* on which institutions rest and emphasizes how, at critical moments in history, dominant players in the political economy forge institutions that further their material interests (see also Zysman 1983). By contrast, the literature on varieties of capitalism understands institutions in terms of the *coordinating functions* they perform and emphasizes how different institutional configurations drive firm preferences rather than the other way around.

This chapter explores the contributions—and the distinctive strengths and weaknesses—of these two strands of the literature. I argue that neither perspective can stand alone and indeed, that the insights generated by each one are incomplete and even misleading to the extent that scholars fail to situate these within a framework that incorporates the insights of the other. The coalitional perspective emphasizes agency and politics (winners and losers), but it often downplays or fails to problematize the role of institutions in influencing which kinds of coalitions emerge and prevail. The second perspective, by contrast, emphasizes the role of institutions in shaping employer preferences, but its emphasis on the functional contributions of particular institutions to firm strategies often obscures the *political* foundations on which these institutions rest and which lie behind their reproduction.

Viewed from the vantage point of the broader methodological themes running through this volume, the argument elaborated in the pages below illustrates the explanatory power of an historical-institutional framework (Hall and Taylor 1996; Pierson and Skocpol this volume; Thelen 1999; Thelen and Steinmo 1992). It does so by underscoring the weaknesses of research strategies that emphasize one component—the historical or the institutional—to the neglect of the other. Specifically, the literature on cross-class coalitions—while often appropriately *historical* (especially in the sense of situating employer strategies within a particular historical context)—is frequently insufficiently *institutional* in the sense of taking account of how employer preferences themselves have been molded by features of the political-institutional environment. Conversely, the literature on varieties of capitalism is deeply attuned to the impact of *institutions*—indeed the whole perspective is organized around understanding the way that institutional arrangements shape firm strategies and preferences—but insufficiently *historical* in the way that it conceives of these institutions and neglectful of the political (as opposed to efficiency-based) dynamics behind their reproduction.

Moreover, both perspectives have been criticized by some for “bend-

ing the stick too far in the opposite direction,” that is, overemphasizing the role of employers and losing sight of traditional class (as opposed to cross-class) conflicts and distributional struggles (see, esp. Iversen and Pontusson 2000, 31; Pontusson 2000; Howell 2001). My thesis here is that a successful synthesis of these two perspectives hinges on the self-conscious incorporation of a stronger temporal dimension into the analysis (cf. Pierson and Skocpol, this volume). As soon as we do this, class conflict and the constitutive power of labor are brought back out of the shadows. Sustained attention to the way in which policy feedback sets the context within which successive contests over particular innovations are fought will reveal how the strength of unions and the influence of their political allies shape both the kinds of employers that labor unions face and the interests and coalitions they are likely to pursue.

The chapter proceeds in four steps. First, I address the literature on democratic corporatism from the 1970s and early 1980s, and demonstrate how recent developments—both theoretical and empirical—pose a challenge to some of the analytic conventions on which that literature was based. Second, I look at some of the scholarship that has reoriented the debate on labor politics by reinserting employer interests and show how each of the two strands of thought sketched out above has provided insights into developments in industrial and labor relations over the last decades that appeared anomalous against the backdrop of the previous corporatism literature. Third, I make the case for a synthetic framework that blends together the emphasis on agency and power that is more fully elaborated in coalitional analyses with the logic of alternative institutional frameworks that figures so prominently in the second, varieties of capitalism, perspective. I use two brief empirical examples to illustrate the analytic leverage that can be gained through an historical-institutional approach that incorporates insights from both literatures. A fourth section concludes.

## ■ | The Democratic Corporatism Literature

In the 1970s and early 1980s the study of labor in the developed democracies was dominated by the literature on democratic corporatism.<sup>2</sup> This term was used to refer to a situation in which highly organized business and labor associations were incorporated into policymaking networks, thus facilitating negotiated trade-offs across various economic and social policies. Corporatism in this sense applied best to a rather narrow set of mostly northern European countries that were characterized by centralized labor movements linked to strong social democratic parties. The convention was

2. For an extended discussion and critique see Thelen 1994, on which this section draws.

to think of corporatist arrangements as having been put in place by strong labor movements,<sup>3</sup> and many analysts took the degree of labor's centralization as a basis for measuring the "degree of corporatism" across the whole range of advanced industrial societies (Cameron 1984; Schmitter 1981; Wilensky 1976). The "most corporatist" countries were characterized by highly developed welfare states, centralized and egalitarian wage bargaining institutions, and strong labor participation in managing economic and industrial adjustment processes. The "least corporatist" countries, by contrast, were characterized by less developed and less egalitarian welfare states, high levels of inequality, and contentious and largely counterproductive relations between unions and employers in the face of adjustment pressures.

A large literature analyzed the structure and logic of corporatist decision making and examined the link between such arrangements and positive economic performance in the 1970s and 1980s (e.g., Cameron 1984; Crepaz 1992; Moene and Wallerstein 1999). However, starting in the 1980s, traditional bargaining and welfare state institutions throughout the advanced industrial world came under increasing stress due to new fiscal constraints and heightened competitive pressures associated with globalization. These trends shifted the focus of labor scholarship from the past successes of corporatist arrangements to contemporary strains and the reconfiguration of industrial relations and other labor institutions (A. Martin 2000; Pontusson and Swenson 1996).

However, contemporary trends fit very uneasily within the traditional corporatist framework. Centralized wage bargaining in some—but not all—of the classic corporatist countries has been undergoing profound change (Lange, Wallerstein, and Golden 1995). Meanwhile, traditionally noncorporatist countries such as Spain and Italy have seen a *recentralization* of bargaining (Locke and Baccaro 1996; Perez 2000; Regini 1997; Regini and Regalia 1997; Royo 2000). The old taken-for-granted link between bargaining centralization and labor strength seems curiously out of sync with these developments. Strong labor movements in countries like Sweden have been unable to defend centralized bargaining arrangements, while concertation has been emerging in countries where the corporatism literature would tell us that unions were too weak and fragmented to force it (Perez 2000, 441).

These developments seemed vexing within the traditional corporatism framework, and together, they revealed two broad problems with the analytic conventions that underpinned labor scholarship based on that framework. First, much of the traditional literature on corporatism systematically ignored the constitutive role of employers and employers' organizations in forging and sustaining the institutional arrangements governing labor poli-

3. An exception is Katzenstein (1985).

tics.<sup>4</sup> Recent work has shown that employer coordination is at least as important as the centralization of unions in achieving many of the economic outcomes associated with corporatism (C. J. Martin 2000; Soskice 1991). Moreover, and as events in Sweden and Denmark in the 1980s forcefully demonstrated, the fate of corporatist arrangements hinges crucially on employer strategies: where employers are intent on decentralization, even the strongest labor movements cannot resist.

Second, the corporatism literature of the 1970s and 1980s tended to focus a great deal of attention on wage bargaining and to view collective bargaining institutions in relative isolation from other related institutional spheres. Although corporatist bargaining was concerned with negotiating *policy* trade-offs involving (for example) monetary and social policies, the *institutional* interconnections that linked wage bargaining to other political-economic arenas—including financial institutions, vocational education and training institutions, and welfare state institutions—went largely unexplored.<sup>5</sup>

By contrast, a good deal of the contemporary labor literature is precisely concerned with examining the interactions between various institutional realms, with the interface between financial institutions and collective bargaining structures figuring especially prominently.<sup>6</sup> Related, but not identical to this, is a growing concern with the broad issue of *institutional complementarities*, which goes beyond the question of how policies generated in one arena (e.g., by central banks) affect choices and strategies in other arenas (e.g., collective bargaining). Studies of institutional complementarities, both historical and contemporary, explore the ways in which the existence of certain kinds of institutions in one realm (e.g., industrial relations) may promote the development of complementary institutions in other realms (e.g., vocational training or welfare state institutions) (Ebbinghaus and Manow 2001; Hall and Soskice 2001a; Huber and Stephens 2001; Streeck 2001; Swenson forthcoming).

A number of recent works have challenged the analytic foundations of the traditional literature. The two strands of theorizing discussed below move in important ways beyond the conventions associated with the classic model of democratic corporatism, and each of them contributes key elements for an

4. It is symptomatic that almost all rankings of degrees of corporatism were based on the organizational characteristics of the labor movement (e.g., Cameron 1984; Schmitter 1981).

5. However, Wolfgang Streeck was already dealing with these issues in work that ran mostly parallel to that taking place within the dominant corporatism framework. His contributions will be discussed subsequently, as they played a major role in the reorientation of labor scholarship generally.

6. See, for example, Hall 1994; the contributions by Franzese and Hall and Iversen in Iversen, Pontusson, and Soskice 2000; Soskice 1990b; Soskice 1997; and Soskice and Iversen 1998.

alternative analytic framework for understanding the political economy of advanced capitalism. The next two sections sketch out those features of the literatures under review that constitute important building blocks in such a framework: the role of employers in cross-class coalitions, and the institutional framework within which employer preferences emerge and are articulated.

## ■ | Employers in Cross-Class Coalitions

As noted above, the conventional wisdom held that corporatism was fundamentally a product of labor strength, as highly centralized union confederations were able to use their political and market power to “push back” capital and institutionalize a prominent role for labor in the political economy. However, when in the 1980s employers in some countries turned their sights on corporatist bargaining institutions, it suddenly became clear that these arrangements had always rested on the active or tacit support of capital.

Peter Swenson’s work has been enormously important in highlighting employers’ constitutive, not passive, role in the genesis of centralized bargaining institutions in Sweden, which was long considered the paradigmatic case of corporatism and labor strength (see also Fulcher 1991). Swenson’s work took issue with conventional accounts of the origins of the Swedish model, highlighting how some employers, acting in their own interest, actively orchestrated the centralization of bargaining that became the basis for labor’s strength and incorporation into corporatist institutions in the postwar period (Swenson 1989). Building on work by Carpenter (1972), Ulman (1955), and others (Bowman 1985; Jackson 1984), Swenson underscored how under certain market conditions employers may find it to their advantage to promote unions and to invest them with considerable power in regulating labor markets.<sup>7</sup>

Swenson’s work also invokes the notion of cross-class coalitions developed by Gourevitch and others, and uses it in a nuanced way.<sup>8</sup> Whereas a

7. Bowman showed that employers often actively promote union regulation in sectors characterized by fierce product-market competition. He works out a general argument in game theoretic terms, and illustrates it with the example of the bituminous coal industry in the United States at the turn of the last century. In this case, employers promoted unionization and collective bargaining as a means for controlling competition among themselves by standardizing labor costs, and they went so far as to endorse and support strikes by the union to bring defectors back into line (Bowman 1985, 58–59).

8. Gourevitch’s analysis (1986) of the construction of various political economies is organized around the concept of cross-class coalitions, and the general idea is employed in some classics in political economy such as Polanyi 1944. More recent applications include Luebbert 1991 and Rogowski 1987a. I thank Peter Swenson for assistance in tracing the roots of the concept.

good deal of traditional labor research treated the centralization of bargaining in Sweden as the result of maneuvering between labor and capital as “blocs,” Swenson’s analysis traced the process back to a cross-class alliance between employers in industries exposed to international competition and workers in low-pay sectors. In a first phase of centralization (the consolidation of the union confederation over industry-level unions in the 1930s and 1940s), the metalworkers’ union joined forces with metal employers to rein in their colleagues in higher-pay sectors (for example, construction) that were sheltered from international competition. In a second phase, which ushered in centralized wage bargaining in the 1950s, “metalworkers fell out of the coalition but were pressured into acquiescence with a combination of lockout threats by employers and threats of government intervention by Social Democrats” (Swenson 1989, 228). For Denmark, too, Swenson argued that organized employers “wanted and aggressively promoted the centralization of industrial relations” (1991, 515), and “coerced” unions into this. In both cases, employers not unions are seen as the driving force behind centralization, though they achieved their ends by seeking and promoting strategic alliances with segments of the working class who had their own reasons for going along. In both cases as well, the innovators were powerfully abetted by Social Democratic Parties that intervened at crucial junctures in ways that shored up the alliance for centralization at the expense of its opponents on both sides of the class divide (Swenson 1991, 525–26).

In the meantime, a large and growing body of scholarship has revisited historically crucial turning points in the development of particular political economies, with an eye toward identifying the role played by employers in the genesis of institutions and policies that have traditionally been seen as the product of labor strength. Philip Manow, for example, has produced a reinterpretation of the genesis of *Modell Deutschland* that specifically rests on an analysis of cross-class alliances (Manow 2000). Manow examines the so-called Bremen Agreement of 1956 that first established in Germany the now-familiar system of coordinated industry-level bargaining under the leadership of the metalworkers’ union. Whereas the centralization of bargaining in Sweden reflected a cross-class alliance across sectors, Manow’s analysis of Germany points to an intraindustry deal within the metalworking sector between export-oriented and domestically oriented firms. The deal was consolidated in part by a trade-off that exchanged a system of wage distribution preferred by large firms in heavy industry for welfare policies that allowed employers in export-oriented manufacturing to adjust employment to sudden shifts in demand (Manow 2000).

In a similar vein, Isabela Mares has written a fascinating account of the genesis of unemployment insurance systems in France and Germany in the pre- and interwar period (Mares 2000). Mares rejects labor-centered explanations of the origins of unemployment insurance, particularly their neglect of the interests of employers and their role in political outcomes.

As she puts it, “Without investigating empirically the preferences of firms, most studies *assume* that employers were opposed to the introduction of all social policies and infer that the introduction of any social policy reflects the victory of trade unions and their political allies over a business community forced into retreat” (Mares 2000, 226). In the case of Germany, Mares finds evidence of significant conflicts of interest between small and large firms with respect to preferred outcomes. Large firms in intensely competitive and volatile markets vigorously resisted the introduction of a union-run Ghent system. However, they supported a contributory insurance system over the existing means-tested (flat-rate) one as the best way to off-load the costs of unemployment and to preserve the investment they had made in their workers’ skills.

Mares follows the lead of Swenson not just in highlighting the role of employers in these outcomes, but specifically in highlighting the strategic alliances that produced them. Where she breaks with Swenson is in her account of the process through which the relevant alliance was forged. Swenson sees cross-class coalitions as coming together, frequently though not necessarily always, on the basis of a “prestrategic convergence of interests of unions and employers for a particular social or labor market policy” (Mares 2000, 228). Mares contrasts her own view with this, arguing that her strategic alliances come together *during negotiations*, and are often facilitated by policy entrepreneurs who help to produce compromises that represent the second-best preferences of key actors.

## IMPLICATIONS FOR CONTEMPORARY LABOR POLITICS

Although many of the theoretical innovations associated with cross-class coalitional analysis were rooted in detailed historical analyses of particular countries, the insights these studies have yielded have been extremely useful for making sense of recent broad trends in labor politics. The last two decades have been characterized by heightened pressures for flexibility in industrial relations institutions across the advanced industrial world, but the results have varied cross-nationally. Convergence theories predicting a uniform slide into deregulation have not been borne out (Berger and Dore 1996; Boyer and Hollingsworth 1997; Ferner and Hyman 1998; Wallerstein, Golden, and Lange 1997; Zysman 1996). But theories that attributed the resilience of traditional bargaining institutions in some countries to successful union defense do not provide much purchase, either, on observed cross-national patterns of stability and change (e.g. H. Katz 1993; Turner 1998). The evaporation of employer support for traditional bargaining arrangements has precipitated their breakdown in some strong-labor countries (including Sweden), while employers’ continued willingness to negotiate compromises within existing institutions in other cases (e.g., Germany) has contributed to greater resiliency despite overall lower labor strength.

The search for explanations for these anomalous developments in contemporary industrial relations institutions has been very much guided by insights gleaned from the literature on cross-class alliances surveyed above. Iversen (1996), for example, argues that bargaining decentralization in Sweden and Denmark was a product of a cross-class realignment, set in motion by employers in export-oriented sectors. These firms relied on skilled labor and found themselves constrained by union solidaristic wage policies that narrowed wage differentials, especially those between skilled and unskilled workers (see also Pontusson and Swenson 1996). Firms in such industries led the drive for decentralization and they were abetted in their project by skilled workers who had also come to resent wage policies that benefited unskilled (also public sector) workers (Garrett and Way 2000; Mahon 1991).

Employers' interests also figure prominently in a number of studies that explain the opposite phenomenon—the resurgence of centralized bargaining in traditionally weak-labor–noncorporatist countries such as Italy, Spain, and Portugal. Perez, for example, argues that renewed concertation in Italy and Spain is in no way a function of labor strength; rather, bargaining centralization in both countries was orchestrated by employers and facilitated by governments that provided incentives that brought labor on board (Perez 2000). Richard Locke and Lucio Baccaro's analysis of industrial relations reforms in Italy in the 1990s similarly cites employer interests and conflicts within labor as decisive to ultimate outcomes. Specifically, export-oriented employers in Italy were a driving force behind the move toward industry-level bargaining, accompanied by changes in the rules for shop-floor representation that strengthened the so-called most representative unions against growing challenges by new and often more militant local organizations (Locke and Baccaro 1996, 20, 24).

## CONTRIBUTIONS AND CORRECTIVES

The literature on cross-class coalitions has made an enormous contribution to the study of labor politics in advanced capitalism and many of the core insights that it has produced appear to have become internalized by labor scholars on a broad scale. The convention of viewing cross-national differences in wage bargaining, welfare-state, and other institutions to variation in the strength and centralization of unions has by and large been replaced by a concern with uncovering the role that employers have played in generating and sustaining such institutions.

Ongoing work has also revealed some gaps and blind spots, however. A good deal of the early work in this vein was organized around a refutation of a particularly strong version of the power resource model that saw centralized bargaining and other institutions associated with social democracy and corporatism as the product of labor strength. With this as the foil, it is

perhaps not surprising that many analyses have been devoted to uncovering the heretofore-unappreciated contribution of (segments of) capital to such outcomes. However, many of them have paid scant attention to the *political-institutional* sources of divergent employer interests, and they have also failed to explore the structural configurations that privilege the emergence and triumph of some cross-class alliances over others.

Coalitional analyses provide an important corrective to studies that simply assume that outcomes and policies that benefit labor must have been opposed by employers (see especially Mares 2000, 226). Through intensive archival research, labor scholars have found that many putatively prolabor institutional and policy innovations were congruent with the interests of at least some segments of capital. However, as Hacker and Pierson (2000) have pointed out, establishing that business interests drove these developments requires more than demonstrating that ultimate outcomes were congruent with business interests (the old truism that correlation does not establish causation). In fact, Hacker and Pierson argue that it even requires more than documenting that employers voiced their support for particular policies, for employers may merely have been selecting the least bad from a menu of options not of their own choosing.

Invoking insights from studies of agenda setting, Hacker and Pierson point out that the crucial issue in most situations is not the final choice between particular policy options but rather the prior question of whose preferred range of options form the choice set from which actors are allowed to select. The example they offer responds to revisionist accounts of the history of the U.S. welfare state: “Scholars have spent countless hours in archives trying to chart the behind-the-scenes role of business leaders in the formulation of the Social Security Act—all the while skipping over the obvious point that *the overwhelming majority of employers would have preferred no legislation at all*” (2000, 12, Hacker and Pierson’s emphasis).<sup>9</sup> This argument resonates with an important insight from the work of institutional sociologists such as Dobbin (1994) who argue that in order to understand cross-national differences in outcomes, it is necessary to pay attention not just to a particular decision node (even the final one) but rather to the *range of options* that are entertained in the first place, which are often completely different cross-nationally.

The coalitional approach also focuses a great deal of attention on the winning coalition and often glides over the institutional factors that shape the kinds of coalitions that are likely to emerge and prevail. As Iversen and

9. This same point comes out in Fulcher’s analysis (1991) of the centralization of bargaining in Sweden. In that case, employers’ choice of confederal bargaining took place in a context where unified and centralized industry-level unions existed and where therefore decentralized bargaining would have left individual firms confronting powerful unions on their own—a much worse scenario.

Pontusson point out, “some other alliance is always possible, and there is always some uncertainty about the prospects of success and the actual payoffs of any particular alliance” (Iversen and Pontusson 2000, 32). For example, in the analysis of bargaining centralization in Denmark and Sweden, Swenson demonstrates without a doubt that some segments of capital were in on the winning coalition. But what is not really problematized in any sustained way is the prior question of why the interests of some capitalists prevailed over others in internal organizational struggles, that is, how it came to be that peak employer associations brought their full weight—including massive lockouts—to bear on behalf of the centralizers and against the opponents of centralization, who were members of the same associations. Swenson writes of “dominant” employer groups, but the source of their dominance is not established apart from the fact that they ultimately prevailed.

It may be that a certain indeterminacy is inherent in cross-class coalitional approaches. After all, there is no point in undertaking a coalitional analysis unless the outcome could have been different had that coalition not prevailed. This indeterminacy has been addressed by some of the more recent work, which stresses the role of policy entrepreneurs or state reformers in brokering the coalitions that tip the outcomes in one direction or another. We see this in Mares’s analysis of unemployment insurance in Weimar Germany, where political entrepreneurs were key players in forging a coalition based on the second-best preferences of some unions and employers associations. In Swenson’s analysis (1997) of the United States, too, policy entrepreneurs are crucial actors in “arranging the alliance” between segments of labor and capital around social policy legislation in the New Deal. The argument is that policy reformers acted on signals sent by prominent capitalists (that they would accept and indeed in some ways welcome certain forms of social insurance) and proceeded to craft legislation around these firms’ interest in policies aimed at eliminating cutthroat competition and stabilizing consumer demand.

Emphasizing the role of policy entrepreneurs or reformers in many ways simply reinforces the idea that very different outcomes were possible, and indeed some of these analyses explicitly embrace a highly contingent view of key choice points. Such a perspective thus places a great deal of emphasis on agency, sometimes quite appropriately. However, in doing so, these formulations tend to obscure the *broader institutional arrangements* within which these agents are maneuvering and which often bias outcomes at various junctures by rendering certain coalitions more likely than others or by magnifying the voices of some segments of labor and capital over those of others. This is where the second literature noted above comes in.

## ■ | Varieties of Capitalism: Bringing Institutions Back In

At around the same time that these authors were analyzing the role of cross-class alliances in the origins of corporatism in the centralized systems of northern Europe, another strand of research was emerging which also took issue with the labor centeredness of the corporatism literature. This body of work also brought capital back in, though on somewhat different terms from the coalitional analyses surveyed above. Some of the key works are by David Soskice, and the central variable that emerged from his work was that of employer coordination. Beginning with several important articles in the early 1990s, Soskice argued that the capacity of employers to coordinate among themselves—not labor strength and centralization—was the key to understanding the macroeconomic outcomes traditionally associated with corporatism. Soskice's original reinterpretation of corporatism did not so much reject the corporatism literature of the 1970s as it did suggest that this model no longer captured the key elements behind economic success in the context of more volatile international markets since the 1980s (Soskice 1990a, 171).

In this and subsequent work, Soskice developed a general theoretical framework for understanding divergent trajectories and degrees of success in what he called “coordinated” versus “noncoordinated” (or “liberal”) market economies.<sup>10</sup> The core of the distinction goes back to differences in the capacity of employers to coordinate among themselves in order to create the conditions that can both secure wage restraint and encourage firm-based innovation and adaptation to rapidly changing markets. In an important article in 1990, Soskice (1990b) offered a critique of the influential Calmfors-Driffill model that had associated positive economic outcomes with either highly centralized or very decentralized wage-setting institutions. A major part of Soskice's critique of the Calmfors-Driffill model involved the reclassification of a number of countries (including Switzerland, Japan, Germany, the Netherlands, and parts of Italy) where the level of union centralization did not accurately reflect the degree of wage coordination that *employers* were able to achieve (sometimes informally) in these economies (1990b, 42). In a way, Soskice substituted the concept of employer coordination for the corporatism literature's emphasis on union centralization. He argued that when countries were correctly classified—according to levels of business coordination not union centralization—the coordinated systems actually still enjoyed a comparative advantage over uncoordinated market economies in both wage restraint and innovation (1990a, 191).

10. The nomenclature has gone through several iterations, but despite some changes in labels, the countries understood to belong to each of these broad types have remained the same. Thus, coordinated market economies include Norway, Sweden, Japan, Germany, Switzerland, and Austria, while noncoordinated, or liberal, market economies comprise the Anglo-Saxon countries—Britain, the United States, Ireland, Canada, and Australia.

Importantly for present purposes, Soskice views employer interests with respect to wage bargaining and other labor relations institutions (works councils, for example) in terms of the embeddedness of firms within a broader cluster of political economic institutions including financial institutions, bank-industry linkages, vocational training systems, and more recently, welfare state institutions (see also Ebbinghaus and Manow 2001). He stresses strong complementarities among the various institutions that make up each of the two types of political economy (esp. 1999, 109). Thus, for Soskice, coordinated market economies are characterized by institutions that: (1) ensure companies long-term financing, (2) assign unions a role in maintaining cooperative industrial relations within the company and in coordinated wage bargaining across companies, (3) encourage serious investments on the part of companies in initial vocational training for youth, and (4) sustain cooperation across companies in the development of technology and in standard setting (1999, 106–7). Uncoordinated market economies, by contrast, feature (1) financial systems based on short-term financing but also high risk taking, (2) more deregulated labor markets and more adversarial industrial relations, (3) education and training systems that emphasize general education over strong initial vocational training, and (4) limited coordination (indeed, intense market competition) among companies in areas such as research and development and standard setting (110).

In the early versions of the framework, the comparisons between the two types of economies were mostly invidious (the idea being that the former produced superior economic performance). However, more recent work with Peter Hall has fleshed out the alternative logic (including strengths as well as weaknesses) of the liberal or uncoordinated model. What emerges is a picture in which different institutional configurations generate firm strategies based on differences in comparative institutional advantage. Thus, “the institutional frameworks of liberal market economies [such as the United States and Britain] provide companies with better capacities for radical innovation, while those of coordinated market economies [e.g., Germany, Japan] provide superior capacities for incremental innovation” (Hall and Soskice 2001b, 41). As a result, each is associated with specialization in the production of a different set of goods.

Soskice’s original formulations were very much influenced by a larger literature—much of it more sociological in orientation—that drew similar distinctions between what Wolfgang Streeck calls liberal and socially embedded political economies (Streeck and Yamamura forthcoming).<sup>11</sup> An

11. In fact, a great deal of contemporary theorizing in this vein can be traced back to Streeck’s pioneering work on the German model of diversified quality production (especially Streeck 1991). In that body of work, Streeck challenged neoclassical economic theory by showing how social institutions that systematically interfered with the free play of market forces could produce a distinctive type of capitalism, and one that can in fact be uniquely successful in the marketplace.

important volume by Boyer and Hollingsworth (1997) also pointed to distinctive national “social systems of production” that are defined by a set of complementary and mutually reinforcing institutional arrangements that together support different types of firm strategies in international markets. In fact, this whole line of thought was popularized in a book by a French businessperson and economist, Michel Albert (1993), who draws a distinction between what he calls the Anglo-Saxon and the Rhineland versions of capitalism.

All of these works share a great deal in common. The core institutional features they point to are nearly indistinguishable. The countries they place in each of the two polar categories (however labeled) are also virtually identical (with Germany anchoring the coordinated market economies and the United States cited as the paradigmatic liberal market economy). All of these scholars also have trouble sorting countries such as France that seem not to fit either ideal type very well. What distinguishes Soskice’s version of the argument, however, (and this is the reason I focus on it in the present context) is his emphasis on employer coordination as *the* key and defining feature that separates the two types of systems (1999, especially 126, 130).

In Soskice’s version, economies based on different capacities for business coordination support distinctive types of firm strategies in the market, and employer preferences in terms of labor relations institutions flow from the contribution that these institutions can be expected to make to these strategies. Thus, for example, employers in coordinated market economies support centralized wage-setting institutions as a means to control competition among themselves in labor markets (reduce poaching) and protect firm investments in worker skills. Similarly, strong works councils come out in this literature as important mechanisms to encourage long-term and trustful relations between workers and employers, which is important for the success of firm strategies based on flexibility and high-quality production. As with the literature discussed above, this emphasis thus puts an entirely different spin on institutions that we typically associate with labor strength.

## IMPLICATIONS FOR CONTEMPORARY LABOR POLITICS

The varieties of capitalism approach has provided important insights into trajectories of change within and divergence across countries in wage bargaining and other institutions of crucial importance to labor politics. Hall and Soskice (2001a), especially, have argued that globalization pressures are if anything likely to enhance differences between the two types of economies rather than drive convergence. Because firms in the two types of systems are invested in different types of strategies, they and their governments will want to strengthen the particular mechanisms on which their comparative national advantage is based. This means that firms in lib-

eral market economies such as the United States will have an interest in intensifying market pressures (deregulation), while those in coordinated market economies such as Germany will seek to retain previously built coordinating capacities in the face of new challenges. Although these systems are not invincible, they thus represent self-reinforcing equilibria.

This perspective finds strong corroboration in the conclusions drawn by two important recent volumes that embrace as an organizing theme the broad distinction between liberal and coordinated market economies developed by Soskice (Iversen, Pontusson, and Soskice 2000; Kitschelt et al. 1999). Synthetic chapters in each book—drawing on the combined research of the contributors—paint strikingly similar pictures of overall trajectories of change. The editors of both volumes conclude that recent developments point toward convergence within and divergence between coordinated and liberal market economies along a number of important dimensions (Iversen and Pontusson 2000, 3; Kitschelt et al. 1999, 444). If one looks specifically at wage bargaining institutions, the results are very similar (Thelen 2001). In liberal market economies such as the United States and the United Kingdom, the trend in industrial relations institutions has been strongly deregulatory—including the collapse of multiemployer or pattern bargaining, and an overall decline in the number of workers covered by collective agreements of any sort. These changes have been more dramatic in the United Kingdom than in the United States (given previously higher levels of unionism in Britain), but the dominant trends in both have been toward an escape from all forms of union regulation or, where this is impossible or impractical, a strong preference for dealing with unions that are cut off from national-level bargaining structures and strategies.

The trajectory of change in industrial relations institutions in the coordinated market economies has been quite different, although here too there have been substantial strains and, in some cases, important institutional reconfigurations. The breakdown of peak-level confederal bargaining in Sweden has received the most attention, but contrary to some of the more breathless predictions from the early 1980s of a slide into full deregulation, wage bargaining has reequilibrated at a level that very much resembles the German model of coordinated, multiindustrial negotiations (and led by the export sector which now negotiates more or less as a bloc). The changes in Italian bargaining structures, described above, move in the same direction—not toward deregulation but toward coordinated industry-led bargaining—albeit from a rather different starting point. Similarly, in Germany, employers' continuing interest in a high degree of coordination has played a role in shoring up traditional bargaining arrangements there, despite high unemployment and lower union membership than in the traditionally corporatist countries (Thelen 2000b).<sup>12</sup>

12. This entire argument, along with a survey of developments in wage bargaining institutions across a range of countries, is developed in Thelen 2001.

The results of more detailed case studies, moreover, seem to confirm that the broad patterns that we observe cross-nationally do in fact fit the logic of the overall argument. It does appear that the stability of relatively centralized bargaining in some countries is not simply a function of successful union defense against employer efforts to deregulate labor relations but goes back in some large measure to employers' own continuing interest in these institutions. The high premium that employers still place on coordination goes back partly (as before) to an interest in wage restraint, but it is also related to the extreme vulnerability of employers to industrial strife. If anything, this vulnerability has grown in recent years, as competition in international markets now increasingly depends on the ability of a firm to deliver at high quality and on a just-in-time basis (Thelen and Kume 1999; Thelen and van Wijnbergen 2000).

### CONTRIBUTIONS AND CORRECTIVES

It seems clear that there is much to be gained from analyzing labor relations as one important subsystem within a broader institutional configuration that is sustained at least in part by the coordinating capacity of employers. The question is, is anything lost? Soskice's view of institutions has a distinctly utilitarian cast, and emphasizes how institutions solve various collective action problems in ways that redound to the benefit of all—in this case, of all firms.<sup>13</sup> Thus, for example, in the coordinated market economies, strong unions and centralized bargaining are characterized mostly as resources for (and not so much constraints on) firms that need to control labor costs, secure workers with suitable skills, and maintain cooperative relations with their workforce (e.g., Soskice 1996, 4–9). The kinds of corporate strategies that firms pursue in such political economies “require” industrial relations systems that equalize wages by skill levels (to discourage poaching), link bargaining across industries (to ensure wage restraint), and give workers incentives to maintain a high degree of flexibility on the shop floor (Soskice 1996, 1999).

What is obscured, however, in characterizations of labor institutions based on their functional or efficiency effects are the questions of power and political conflict that drove the development of these institutions in the first place, and the political settlement on which they are premised. This becomes clear if we juxtapose Streeck's and Soskice's different perspectives on institutions such as centralized bargaining and strong works councils. In Streeck's view, such institutions were manifestations of labor strength, and their existence, in his felicitous formulation, both “forced and facilitated” a shift toward firm strategies based on high quality, high skill, and high value-added production. Soskice's formulations, by contrast,

13. In this, Soskice's view shares similarities with a broader rational-choice literature on institutions (e.g., Weingast 1998; Weingast and Marshall 1988).

tend to deemphasize the power (constraint) side of institutions and to emphasize employer interests over union strength as the key to the existence and reproduction of such arrangements.

As the literature surveyed above shows, however, institutional arrangements governing relations between workers and employers are the product of political struggles in which there are winners and losers on both sides of the class divide.<sup>14</sup> To be fair, it should be noted that Soskice is not concerned with the *origins* of the institutions which characterize coordinated market economies and which distinguish them from liberal market economies but rather with the kinds of incentives that they present to firms as they adapt to currently changing market conditions. However, if it is true that institutions reflect political settlements, then the reproduction of those institutions cannot rest solely on the functional contribution they make to firm strategies. It must rest also, at least in part, on the reproduction of the political settlement on which they are founded. Since firms within a single national political economy participate in different segments of the international market (which may reward or punish different types and levels of coordination and where competitive conditions change over time), shifts in the international context should constantly introduce new tensions into even the most stable arrangements.

More generally, the fact that firms need particular kinds of institutional supports for certain kinds of strategies does not mean that they will get them. Solutions to various coordinating problems do not just need to be found, they have to prevail politically. In this regard, what is most striking about the rational-choice literature on coordination and collective action is that most of it rejects the straight utilitarian logic that is the backbone of the varieties of capitalism framework (see, especially, Bates 1988a). Instead, scholars in this framework have begun to invoke explicitly various cultural and political variables (Bates, de Figueiredo, and Weingast 1998; Greif 1998; Levi 1998a; Ostrom 1999).

Moreover, as an empirical matter we know that there are significant tensions at work behind many of the institutions around which the varieties of capitalism literature is organized. But these tensions are difficult to analyze within that framework, which tends to build from a stylized, composite picture of employer interests. In this literature, employer coordination is a national-level—and more or less binary—variable; this is to say that countries are coded as either *having* or *lacking* institutions to facilitate

14. I should note that more recent collaboration with Peter Hall goes beyond previous formulations that focus exclusively on efficiency effects to emphasize how institutions also facilitate deliberation (Hall and Soskice 2001, 11–12). This is important, for as Hall and Soskice note, strategic interactions often take the form of multilevel games in which there is no single, unique equilibrium and in which the capacity to achieve cooperation depends on the quality of deliberative institutions. The capacity for deliberation is clearly important, but it is not the same thing as power, which is what I want to stress here.

nonmarket coordination. From this coding then flow different (though again highly aggregated) notions of employer interest—in the case of coordinated market economies, an interest in maintaining coordination and, in the case of liberal market economies, an interest in further deregulation.

However, as the coalitional perspective emphasizes, different segments of capital have different interests, and conflicts among them are likely to be constantly activated by changing international conditions. In an ideal-typical free market, institutions that outlive their functions would be abandoned, and firms whose interests are no longer well served by the dominant institutions might just disappear. However, in the world of politics, firms, sectors, and established institutional interests will likely put up a fight. Only by disaggregating employer interests will we be able to analyze the political struggles among them and, in so doing, gain insights into the kinds of forces that can undermine as well as stabilize preexisting arrangements, for example, as the preferences of key coalition members regarding optimal institutions change in response to new market challenges (Pontusson and Swenson 1996) or as exogenous changes empower new actors whose stake in the old system is more tenuous or even nonexistent (Iversen 1996).

My point for the varieties of capitalism literature, then, is the mirror image of the point about coalitional analysis. The varieties of capitalism literature are extremely powerful and persuasive in laying out the institutional logic within which employers conceive of their interests and formulate their strategies. However, the weakness of the approach is that it tends to portray these institutions in an ahistorical way, neglecting especially the question of the political as opposed to the utilitarian or market logic on which these institutions rest. Among other things, an emphasis on the political settlement and coalitional alignments behind these institutions will yield insights into the (cross-nationally different) *fault lines* that we can expect to emerge in different systems in the context of putatively common international strains.

## ■ | Toward a Synthesis of Cross-Class Alliances and Varieties of Capitalism

The discussion above already foreshadows my conclusion, which is that each of the two perspectives I have outlined—one emphasizing political maneuvering and cross-class coalitions, the other focusing on institutional logics and developmental trajectories—works best when brought into closer dialogue with the other. From the political coalitionists we can take the important insight that the institutional arrangements that make up a political economy, including industrial relations and welfare state institutions, represent and rest on a particular alignment of interests and there-

fore balance of power within and across classes in capitalist societies. From the institutionalists we take the idea that these institutions cannot be reconfigured at will, not least because they are linked into broader configurations that influence the kinds of interests and strategies that both labor and employers are likely to pursue.

However, beyond simply plucking the best insights from each, the synthesis I have in mind brings these elements together in the context of an historical-institutional framework that incorporates a strong temporal dimension (Pierson and Skocpol, this volume). The coalitional perspective, while appropriately historical (especially in the sense of contextualizing the interests and preferences of key actors), is frequently insufficiently attentive to the institutional constraints under which these interests are formulated and pursued. Conversely, the varieties of capitalism literature, while deeply attuned to the impact of institutions, is often insufficiently historical and in particular strangely silent both on the political origins of these institutions and on the political processes through which they are reproduced (but also sometimes reconfigured) over time.

Explicit attention to institutional legacies and policy feedback can address both weaknesses by clarifying the ways in which interests and institutions interact over time (Skocpol 1992; Weir 1992b). Such a perspective, moreover, brings the constitutive role of labor and of partisan politics—which tend to recede into the background in both literatures—back into clear focus. Situating individual moments of innovation in a broader temporal and institutional framework will reveal, namely, that the choice sets that employers face at any given moment are profoundly conditioned by the historically evolved position and power (or weakness) of organized labor and its political allies.

To illustrate these points, I offer two brief empirical examples, one coming out of the literature on welfare state development, the other from the literature on trends in collective bargaining institutions. Both examples incorporate core insights from the literature surveyed above that “brings capital back in,” but they situate employer interests and strategies within a broader framework that captures the impact of policy and institutional legacies that otherwise often escape notice. Both of them serve as a corrective to coalitional analyses that zero in on specific choice points but often miss the structural features that define the key actors and the strategic options they face, and to varieties-of-capitalism analyses that tend to emphasize the economic logic of particular institutional arrangements but neglect the political coalitions and dynamics on which they are based. Both examples illuminate important elements of path dependence in the genesis and evolution of key institutions that mediate relations between labor and capital in advanced industrial societies.

## PATH DEPENDENCE IN THE EVOLUTION OF THE WELFARE STATE

Recent work on the development of the welfare state in several advanced industrial countries has benefited from insights generated by the literatures discussed above. Work in this area is more likely than before to highlight the role of employer interests in the genesis of social policies that were conventionally associated with labor strength.<sup>15</sup> To stay with a familiar example, Swenson provides striking archival evidence that shows that Swedish employers in the early 1950s—far from opposing universalistic welfare policies—actively supported them (though they were careful to let labor take the credit) (Swenson forthcoming). Swenson shows how policies such as comprehensive health insurance and generous sick pay provided solutions to specific pathologies that had developed in the Swedish labor market as a consequence of centralized bargaining arrangements. Specifically, centralization brought with it both wage restraint and wage compression, which in turn caused labor market scarcity, especially in times of buoyant demand and growth. In such a context, firms had turned to other measures—generous plant-based social benefits—to attract and retain scarce labor, thus unleashing “unhealthy” competition among firms that existing institutions (governing wages) were unable to control. Universal welfare policies in the 1950s restabilized centralized labor market governance by dampening the resulting escalation of plant-based policies.

Swenson correctly asserts that these policies cannot be seen as a product of labor power *against* employers if employers themselves had their own reasons for embracing them, and this serves as a powerful corrective to much of the received wisdom. However, if we situate events in Sweden in the 1950s in a broader institutional and temporal framework, what is equally striking is all the options that employers might have much preferred but that never made it to the agenda. This is the gist of an alternative account offered by Evelyne Huber and John D. Stephens (2001), which is premised not on a rejection of the coalitional perspective so much as its incorporation into a framework that places what Pierson and Skocpol (this volume) refer to as “period effects” and “temporal boundary conditions” at the center of the analysis.<sup>16</sup>

15. Although, as C. Martin (1995) has pointed out, this work harks back in some ways to a previous body of scholarship in the corporate liberal tradition that examined business support for social initiatives as functional to the long-term interests of capital (e.g., Kolko 1977; Weinstein 1968).

16. To be clear, I think that Swenson and Huber and Stephens would not have much to disagree about when it comes to the interpretation of particular policy episodes. The difference, rather, is in what each type of analysis highlights. Swenson’s theory clearly emphasizes employer interests or preferences, while Huber and Stephens are concerned with the broad structural factors that shape these preferences both cross-nationally and over time. Swenson alludes to the way in which

In their study of welfare state development across the advanced industrial world—an analysis that blends quantitative analysis of eighteen developed democracies with qualitative case studies stretching over several decades within individual countries—Huber and Stephens draw attention to the impact of broad structural conditions that shape the goals and strategies of key actors (here, especially, of employers). Analyses based on single country cases or on smaller slices of time, they argue, will often privilege agency and choice in their explanations of policy outcomes, because the structural constraints remain in the background, “unchanging and therefore invisible” (2001, 8–9). In such cases, “researchers are likely to attribute more causal importance to the preferences and strategies of [specific] actors than warranted, or at least they lose sight of the way in which the constraints of the larger power distribution and the institutional context shape the preferences and strategies of these actors to begin with” (33). In short, and as Cathie Jo Martin’s work has also demonstrated, beyond pointing to the role of employers, the key is to understand the *sources* of business preferences, and addressing this issue requires attention not just to a firm’s market environment but to the political landscape employers face as well (C. J. Martin 1999, 2000).

The political-coalitional approach discussed above is sometimes presented as an alternative to institutional analysis, which—it is argued—does not take interests seriously enough.<sup>17</sup> However, Huber and Stephens’ analysis reminds us of the ways in which these interests are themselves mediated by broad structural factors (they emphasize state structure, the party political landscape, and market structures) within which contests among competing societal interests are played out (also C. J. Martin 1999). The welfare discussion in Sweden in the 1950s unfolded in a context in which a number of institutions were already in place, including centralized bargaining and strong and encompassing unions backed up by a Social Democratic party that had been in power continuously for the previous twenty years. Under these conditions, options that were very much on the agenda elsewhere—e.g., based on internal labor markets and strong company-based career ladders, as prevailed in the United States and Japan—were simply no longer viable in Sweden (Huber and Stephens 2001, 29–30). In other words, and as the varieties of capitalism perspective would underscore, what Swedish employers wanted in the 1950s was very much shaped by the kinds of strategies that these firms were (by that time) pursuing and that were supported by a number of complementary institutions that made

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employer strategies are influenced by “established organizational practice, or economic, cultural, political and legal conditions” (Swenson forthcoming), but these are not issues that he pursues at a theoretical level (as Huber and Stephens do).

17. This is the approach, for example, of Swenson’s critique of Skocpol (in Swenson forthcoming).

certain courses of action more feasible and took others off the agenda entirely.

This example represents a classic case of path dependence (Pierson 2000a), in which the effects of previous turning points become virtually invisible as once-viable alternative developmental trajectories grow increasingly remote over time. Huber and Stephens articulate at a more general level the way in which the legacies of past policies shape the goals, expectations, and preferences of key actors, and specifically in this case, employer strategies in welfare state development. Among the four mechanisms they cite, two stand out as particularly important. First, structural conditions affect the *range of options* that are available to employers in different countries and at different junctures. As pointed out above, if we focus very narrowly on the expressed policy preferences of key actors over alternatives A and B in a given debate, we may miss the much more important fact that X, Y, and Z are not on the menu at all (Hacker 2000; Hacker and Pierson 2000). Huber and Stephens' strategy for recovering the impact of these structural variables relies on counterfactual reasoning backed up by over-time analysis and comparisons between countries in which market conditions were broadly similar but state or partisan structures were markedly different.

Second, and at a deeper level, past policies affect what Huber and Stephens call the *distribution of preferences* by which they mean the universe of actors who are around to express their interests in the context of particular (later) policy debates. This is a point that I associate very closely with the work by Wolfgang Streeck mentioned above (1997, esp. 199), and one that Huber and Stephens invoke and deploy in a new way. The basic point is that "national frameworks of social policy contribute to distinctive competitive environments in which some types of employer activities are rewarded and others are not—and in which some types of firms survive and others do not" (Hacker and Pierson 2000, 15). In the case of Sweden, decades of centralized bargaining, wage compression, and social democratic hegemony set in motion changes that, over time, completely reconfigured the political-economic and industrial landscape. These factors "progressively eliminated low-wage sectors and thus eliminated one source of opposition, low-wage employers, to the further pursuit of [a high-wage, high-social-wage] path" (Huber and Stephens 2001, 30). Here we have a good illustration of Pierson and Skocpol's argument that policies often produce distinctive sets of winners and losers, and in at least some cases the losers in one round may simply not survive to fight the next (Pierson and Skocpol this volume).

In sum, although coalitional accounts are sometimes framed as alternatives to institutional analyses, it seems much more fruitful to think of them as natural complements. Capitalist interests are clearly important, but the *source and distribution* of these interests must be sought, as the

institutionalists would argue, in the logic of the structural framework in which they find themselves. Moreover, once we situate employer interests within a broader temporal and institutional framework, the constitutive power of labor reemerges from the shadows. Attending to the ways in which the political and economic organization of labor shapes the strategic context employers confront is a necessary antidote to some of the more breathless claims about business interests in the construction of industrial relations and welfare state institutions. Despite the now widely acknowledged role of employers, broad cross-national research continues to show that a well-organized and politically connected labor movement is the *sine qua non* of strong and strongly egalitarian welfare states and other outcomes associated with democratic corporatism (Bradley et al. 2001; Hicks 1999).<sup>18</sup>

### THE POLITICS OF COORDINATION: STABILITY AND CHANGE IN INDUSTRIAL RELATIONS INSTITUTIONS

Just as coalitional analyses are incomplete unless embedded in an analysis of features of the institutional context that shape the interests, strategies, and alliance opportunities of the key actors, so too would the varieties of capitalism literature benefit from more sustained attention to the *political processes and dynamics* that sustain but also sometimes disrupt the institutional arrangements to which they have so usefully drawn attention. To my mind, this is nowhere more important than in that literature's treatment of the singularly important notion of employer coordination. Much of the literature in this area treats such coordination as a condition that some countries exhibit and others lack, when in reality coordination is a political process, and an outcome that often has to be actively sustained and nurtured. While the varieties of capitalism literature has given us powerful tools for understanding differences in the logic of firm strategies in coordinated versus noncoordinated market economies, the use of employer coordination as a more or less binary, national-level variable and the language of equilibrium tend to blend out important aspects of the political settlement on which such coordination is based in individual countries.

Recent changes in wage bargaining can serve to illustrate. As noted above, collective bargaining institutions in the coordinated market economies have seen significant changes over the past twenty years. While these changes have often fallen short of a complete breakdown of coordination, they have often resulted in important changes in the character and level of coordination. This can be enormously important, for as Iversen and Pontusson (2000) point it, it matters a great deal to many (especially distribu-

18. Swank's work (2001a, b) attributes differences in the resilience of the welfare state in the face of globalization to the differing capacities of its main constituencies, of which labor is not the only one but the most consistent.

tional) outcomes whether employer coordination is achieved at the national level (as in Sweden in the 1970s) or at the industry level (as in Germany) or through more informal relations between large firms (as in Japan). To make sense of many of the recent changes in collective bargaining institutions in the advanced industrial countries requires that we pay attention to the political processes through which coordination is forged, maintained, and also sometimes renegotiated.

This point can be made clear with reference to the German system of industrial relations, which most cross-national studies have coded as a paragon of stability (e.g., Wallerstein and Golden 1997) and continued functionality. Consistent with the logic of the varieties of capitalism literature, the resiliency of traditional bargaining institutions in Germany in the face of many new strains since the 1980s rests in some large measure on employers' continuing interest in wage restraint and labor peace (Thelen 2000b; Thelen and Kume 1999). These are two outcomes that the German system has in the past generated with some consistency and with which, apparently, many German employers seem to be quite unwilling to part.

However, beneath the veneer of formal stability and continued coordination, there are substantial changes afoot. Without going into all the details, a growing chasm has been developing among employers who are differently situated both within the traditional collective bargaining system and in the market.<sup>19</sup> There is a sizable core of firms which remain deeply committed to the current system of coordinated multiemployer bargaining and which are willing (and able) to pay almost any price to preserve social peace. But this group exists alongside other firms (less powerful within the employers associations) which have been pressing for reforms within the traditional structures and which, in the absence of such reforms, have—individually—simply been opting out of the system altogether.

As in the previous case of welfare state institutions, attention to the feedback effects of previous policies and arrangements is critical for understanding the current situation, for in Germany the *very same forces* that are shoring up centralized bargaining institutions in the short run appear to be undermining these structures in the long run. Specifically, the German system traditionally rested on a particular kind of accommodation between the country's large export firms and its sizable sector of small- and medium-sized enterprises. The core of the deal was one in which the large firms typically bore the burden of industrial conflict in order to secure moderate wage settlements that small firms could afford—and which typically did not come close to exhausting the ability of large firms to pay. In the last twenty years changes in international markets have disrupted this basic deal, as the core firms in the German system—locked as they are into strategies based on high quality and reliability which render them ex-

19. For a complete analysis, see Thelen and Kume 1999 and Thelen and van Wijnbergen 2000.

tremely vulnerable to industrial strife—have become increasingly conflict averse. This situation has led to a series of industry settlements in recent years that come much closer than ever before to exhausting the ability of the strongest firms to pay, thus prompting a hemorrhaging of the employers association, as weaker firms opt out of the industry-level contracts altogether. The effects of such bargaining rounds feed back in deeply paradoxical ways—stabilizing the system in a formal sense (by allowing the unions to defeat demands by some employers for formal decentralization), while at the very same time undermining the deeper foundations on which it rests (because central bargains cover a shrinking number of firms).

The point of this story in the present context is to underscore the fact that employer coordination is a dynamic political outcome and that it is not necessarily self-sustaining. The varieties of capitalism literature tends to attribute the stability of institutional arrangements in coordinated market economies like Germany to the benefits of coordination (to employers generally) and the ability of leaders of business associations to recognize, protect, and nurture them (Soskice 1999). But the German case makes clear that the feedback effects generated by these institutional arrangements interact with changes in the external environment in ways that introduce new strains (or render old ones salient in a new way). As the coalitional perspective underscores, these arrangements rest on a particular balance of power within key employer associations and between these and unions. The continued viability of the institutions is not just a question of their efficiency effects but also the reproduction of the political settlement on which they are based. Thus, against the sometimes somewhat functionalist tint to some of the varieties of capitalism literature, it seems clear that there is nothing to guarantee that German employers will succeed in reconstituting their organizations on the basis of a new coalition or internal balance of power, despite the fact that their failure to do so might well be against their individual and collective interests.

This brings us, again, back to the issue of union power. The German industrial relations system has been invoked in the varieties of capitalism literature as a prime example of the importance of employer coordination (*rather than* union strength) to centralized bargaining arrangements. German unions have always organized a much smaller percentage of the national workforce than the classic corporatist countries of northern Europe.<sup>20</sup> However, what has compensated for the relative weakness of German unions in the past is a very high degree of organization on the employer side (Katzenstein 1987; Thelen 1991, ch. 1).<sup>21</sup>

20. At its peak, unionization in Germany was around 35 percent, a far cry from Sweden's 80+ percent organization rate, for example.

21. Since contracts signed by Germany's employer associations are binding on all member firms, a very large majority of German workers have traditionally been covered by union contracts despite modest unionization levels.

Contemporary developments in Germany now reveal the extent to which employer coordination cannot substitute for and in many ways depends on union strength and organization. One of the problems today stems from the fact that union presence is concentrated in precisely those core firms which are willing and able to pay a high premium for labor peace. One sees in Germany today the outlines of an emerging alternative cross-class coalition based on alliances of labor and capital within the large-firm sector but potentially increasingly delinked from small- and medium-sized firms. In fact, more intense cooperation between unions and employers within the large-firm sector appears to be contributing to a perverse dynamic that itself undermines the very institutions that both are trying to preserve (Thelen and Kume 1999, especially 498). Whatever the ultimate outcome in Germany, the general theoretical point is the same. Employer coordination does not just happen; it has to be managed and sometimes renegotiated in the face of new challenges. Moreover, it seems that in the German case employer coordination cannot substitute for union strength; it may be more precise to say that the former depends crucially on the latter.

## ■ | Conclusion

The conclusions of this paper are familiar even if they bear repeated emphasis. As always, the analysis of institutions and of political dynamics go hand in hand. Cross-class coalitional analysis makes most sense when the political processes that lead to the success of particular coalitions over others are situated within a broader institutional and temporal context that takes account of the way institutions forged at one juncture create constituencies, facilitate the articulation of some interests, disarticulate other groups, and affect how various actors view their own interests, also in relation to others. These are the kinds of factors that are often difficult to capture if one focuses narrowly on particular policy episodes. Yet the impact of the broader structural variables is in fact often decisive at these junctures, making some coalitions and some courses of action seem more reasonable, or more possible, than others.

Conversely, however, the kinds of institutional arrangements to which the varieties of capitalism literature has directed our attention must be seen not so much as static or self-reinforcing characteristics of different political-economic systems but as the outcome of past and also ongoing political processes. In particular, the institutions that generate and sustain a high degree of coordination among employers in the coordinated market economies, I have suggested, do not sustain themselves; employer coordination has to be nurtured and, occasionally, even renegotiated entirely. To point to the functions of institutions is not enough, for in order to persist institutional and other outcomes have to prevail not just on efficiency grounds but in the political arena as well.