PART IX

# DEMOCRACY AND CAPITALISM

CHAPTER 33

# CAPITALISM AND DEMOCRACY

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## 1 Introduction

A question permeates much comparative political economy from the classics to contemporary scholarship: how it is possible to combine capitalism with democracy? The former produces stark inequalities in the distribution of property and income, while the latter divides power in a manner that is in principle egalitarian (one person, one vote). So why don't the poor soak the rich? And if they do, how can capitalism be a viable as an economic system?

The answer to the first question depends a great deal on how economic interests are aggregated into public policies. We know from Arrow's impossibility theorem that getting a well-behaved 'social welfare function' when there are multiple dimensions and no dictator is, well, impossible (Arrow 1951). In the case of distributive politics the policy space is inherently multidimensional since there are as many dimensions as there are agents fighting for a piece of the pie. Distributive politics under democratic rules—who gets how much, including whether the poor soak the rich—is therefore anything but straightforward. Like the proverbial elephant in the corner of the room that everyone ignores, most of the existing political economy literature on democracy has skirted the issue. But it cannot be ignored. It is fundamental to how we understand distributive politics under democracy.

Answering the second question requires an understanding of how economic agents respond to the democratic pressures for redistribution. If the state undermines the market, as commonly assumed, how can we explain the economic success of countries that spend well over half their gross domestic products on social protection and redistribution? If the welfare state is built on the shoulders of an unwilling capitalist class, should we not expect capitalists to shun productive investment, stage coups, or move their money abroad? Yet the welfare state has not collapsed, democracy is spreading, and globalization has not resulted in convergence around laissez-faire capitalism. If we want to understand how democracy and capitalism coexist, therefore, we need a model of capitalism that goes beyond a simple dichotomy between state and market.

This essay discusses three different approaches to the study of democratic redistribution, and then considers the recent literature on capitalism as an economic system and how economic and democratic institutions may relate to each other. The first approach assumes that democratic politics is structured around a single left–right redistributive dimension. The central issue in this literature is how democratic politics affects who sets public policies, and much of the debate centers on the question of partisanship: Does 'Who Governs' matter, and if so, in what ways? This is a key question for political economy because it goes to the heart of whether democratic politics makes a difference: Do the poor ever get a chance to try to soak the rich, and how successful are they when they do? In section 3 I discuss some plausible answers.

The main weakness of this approach is that it largely ignores the question of what happens when several political agents compete in a multidimensional distributive space (the elephant in the corner). It is hard to understand why politicians should limit themselves to pursuing redistribution in a single predetermined policy dimension, and when they do not, opportunities to form distributive coalitions abound. The work that puts coalitional politics at the center of the analysis, which I discuss in section 4, paints a richer and more realistic picture of the politics of redistribution. But the cost may be theoretical intractability, and much of the coalitional literature falls into the trap of post hoc description. Description, no matter how accurate, will not produce explanation. At the end of the section I discuss two recent attempts to move beyond such description.

The third approach explains distributive politics as a function of the specific design of democratic institutions—including electoral rules and federalism. The strategy here is to replace ad hoc model assumptions, such as unidimensionality, with ones that are rooted in careful observation of actual institutional designs. This approach moves beyond the partisan literature by explicitly considering how economic preferences are aggregated into policies, at the same time as it avoids the chaotic world of unconstrained coalitional politics. As I argue in section 5, this combination has produced a vibrant research program that helps answer key questions such as when the poor are more likely to soak the rich.

The modern study of capitalism as an economic system has also taken an institutionalist turn, building on transaction costs economics rather than neoclassical models. The 'varieties of capitalism' approach, in particular, illuminates the relationship between redistributive politics and economic performance and helps explain why there is no necessary contradiction between state and market. The work also helps make sense of the observed institutional diversity of modern capitalism, and why such diversity persists in the face of global market integration. But the tradition has thus far produced few insights into the relationship between economic and political institutions, and it has little to say about the political origins of economic institutions—focusing instead on economic-organizational efficiency as a cause.

The lack of a theory of institutional origins also haunts the literature on political institutions. As Riker (1980) argued many years ago, institutions are 'congealed tastes' which themselves have to be explained. Some of the latest literature on democracy and capitalism seeks to endogenize institutions, including the institution of democracy itself, by modeling these as a function of class interests. This brings us right back to the elephant in the corner because without institutional constraints, the issues of multidimensionality and preference aggregation re-emerge. In the concluding section I suggest that there is a new structuralist turn in political economy, where the parameters for our models of institutional design are derived from the specific historical conditions that shaped capitalism in different parts of the world. But I start with a brief discussion of some of the most important precursors for the contemporary literature.

## 2 Precursors for the Contemporary Literature

In a seminal article, Przeworski and Wallerstein (1982) give a simple answer to the question of why the poor don't soak the rich. Any attempt at radical redistribution, or socialism, they argue, would be met by massive disinvestment and possibly violence by the upper classes. So even if the poor would ultimately be better off in a system where private property rights were suspended (itself a big if, of course), the 'valley of transition' would dissuade any rational government with a limited time horizon from attempting it. Conversely, the rich might consent to democracy and redistribution because the costs of repression or the threat of revolution would otherwise be too high. 'Class compromise,' in other words, could be an equilibrium. This model wiped out the notion in the Marxist literature that capitalism could only survive if the lower classes were repressed or misinformed.

Class compromise has survived as a central concept (e.g. Swenson 1991; Garrett 1998; Acemoglu and Robinson 2005), but conceptualizing capitalist democracy as a class compromise does not itself take us very far in explaining the variance in policies and outcomes across countries. Although it is easy to think that democracy—as a particular form of government—and capitalism—as a particular type of economic system—would produce similar policies and outcomes, one of the most striking facts about capitalist democracies is the enormous cross-national variance in inequality, social spending, redistribution, and the structure of social protection. A full-time Norwegian worker in the top decile of the income distribution, for example, earns about twice as much as someone in the bottom decile, whereas in the United States this ratio is well over four (based on 2000 data from the OECD). The extent to which democratic governments redistribute also varies to a surprising degree. According to data from the Luxembourg Income Study the reduction in the poverty rate in the United States as a result of taxation and transfers was 13 per cent in 1994 whereas the comparable figure for Sweden was 82 per cent.

There are two standard approaches to explaining this variance, which frame much of the current debate (even as the literature has moved beyond the original formulations). One is Meltzer and Richard's (1981) model of redistribution, which has been the workhorse in the political economy for two decades (see also Romer 1975). The model is built on the intuitively simple idea that since the median voter tends to have below-average income (assuming a typical right-skewed distribution of income) he or she has an interest in redistribution. With a proportional tax and flat rate benefit, and assuming that there are efficiency costs of taxation, Downs's median voter theorem can be applied to predict the extent of redistribution. The equilibrium is reached when the benefit to the median voter of additional spending is exactly outweighed by the efficiency costs of such spending. This implies two key comparative statics: spending is higher (*a*) the greater the skew in the distribution of income, and (*b*) the greater the number of poor people who vote.

The latter suggests that an expansion of the franchise to the poor, or higher voter turnout among the poor, will shift the decisive voter to the left and therefore raise support for redistribution. Assuming that the median voter's policy preference is implemented, democratization will therefore lead to redistribution. There is some support for this proposition (see Rodrik 1999 on democracy and Franzese 2002, ch. 2 on turnout), although the evidence is contested (see Ross 2005).

The first implication—that inegalitarian societies redistribute more than egalitarian ones—has been soundly rejected by the data (see Bénabou 1996; Perotti 1996; Lindert 1996; Alesina and Glaeser 2004; Moene and Wallerstein 2001). Indeed, the pattern among democracies appears to be precisely the opposite. As noted in the example above, a country with a flat income structure such as Sweden redistributes much more than a country like the USA with a very inegalitarian distribution of income. Sometimes referred to as the 'Robin Hood paradox,' this is a puzzle that informs much contemporary scholarship.

The other main approach to the study of capitalism and democracy focuses on the role of political power, especially the organizational and political strength of labor. If capitalism is about class conflict, then the organization and relative political strength of classes should affect policies and economic outcomes. There are two variants of the approach. *Power resources theory* focuses on the size and structure of the welfare state, explaining it as a function of the historical strength of the political left, mediated by alliances with the middle classes (Korpi 1983, 1989; Esping-Andersen 1990; Huber and Stephens 2001). *Neo-corporatist theory* focuses on the organization of labor and its relationship to the state—especially the degree of centralization of unions and their incorporation into public decision-making processes (Schmitter 1979; Goldthorpe 1984; Katzenstein 1985).

Both variants have come under attack for not paying sufficient attention to the role of employers. Research by Swenson (2002) and Mares (2003), for example, suggests that employers did not simply oppose social policies, but in fact played a proactive role in the early formation of such policies. Also, if the welfare state is built on the shoulders of employers, we should expect investment and economic performance to suffer. But the remarkable fact is that there is no observed relationship between government spending, investment, and national income across advanced democracies (Lindert 1996). Or if there is one, it is so weak that it does not appear to have imposed much of a constraint on governments' ability to spend and regulate labor markets. The neo-corporatist variant is more satisfactory in this respect because it suggests how encompassing unions may choose wage restraint, which leads to higher profits and investment. But this cannot be the whole story since corporatist arrangements were dismantled in the 1980s, often led by export-oriented employers who presumably care deeply about wage restraint (Pontusson and Swenson 1996; Iversen 1996).

A more fundamental question is why conflict should be organized around class and not, say, around sector or occupational group. When people make investments in specific assets, which may be physical or human capital, their interests will be tied up with those investments rather than the collective interest broader class to which they belong (Frieden 1991; Iversen 2005). There is also no systematic account of how distributive conflict between different groups of wage earners gets worked out politically. Dividing a pie invites the formation of redistributive coalitions, and such coalitions cannot be modeled as simply a function of interests. This is clearly also a problem for the Meltzer–Richard model where the median voter is assumed to be king.

## 3 Democracy and Partisanship

Median voter models are very simple to use, but as the Robin Hood paradox suggests, they do not provide much leverage on explaining the observed variance in redistributive politics. Power resources theory points to one potential source of such variation that has been subject to much research: government partisanship. If center-left governments *simultaneously* promote pre-fisc income equality and redistribution, partisanship may not only explain distributive outcomes but solve the Robin Hood paradox.[1] If partisanship is important in explaining distributive outcomes, we would expect equality and redistribution to go hand in hand.

Partisanship may also explain why corporatist institutions are not always conducive to good economic performance. In Lange and Garrett's (1985) well-known model of economic growth, 'encompassing' unions that organize all or most workers are not likely to restrain wages if right partisan governments are in power that are not

[1] Governments may affect the primary distribution of income through public education (Boix 1998), through the facilitation corporatist bargains that promote wage equality (Cameron 1984; Katzenstein 1985; Garrett 1998), or through minimum wage legislation.

attentive to the long-term interests of labor. As I discuss in section 5, this idea of 'congruence' between policies and institutions is an important topic for contemporary models of capitalist institutions.

For partisanship to matter, the median voter theorem must be systematically violated, so there must be some explanation for why this should be the case. It is by no means obvious. Although Downs only applied his argument to majoritarian two-party systems, the median voter theorem also applies to multi-party systems where the median legislator can make take-it-or-leave-it proposals. Since no majority can be formed without the support of the median legislator, those proposals will become the government policy. In simple unidimensional models of government formation the government always includes the party with the median legislator and does not even need a majority to govern since no viable alternatives can be formed (Laver and Schofield 1990). Yet, the comparative evidence seems to imply that partisanship matters (see, for example, Hicks and Swank 1992; Iversen 1998; Huber and Stephens 2001; Cusack 1997; Allan and Scruggs 2004; Kelly 2005; Kwon and Pontusson 2005).

One explanation is suggested by Wittman's (1973) model of probabilistic voting.[2] If two parties represent constituencies with distinct interests on any set of issues, and if they face uncertainty about the election outcome, the platforms that maximize the implementation of the parties' preferred policies will be away from the median. Since their expected utility is the product of the probability of winning times the proximity of policies to parties' ideal point, parties trade off a lower probability of winning for a policy that is closer to their preference. The Wittman model has found wide application in the study of two-party systems, where one of its attractive features is that it can handle multidimensional spaces (more on this below).

Another explanation for partisanship is that political parties, to be electorally successful, have to appeal to core constituents who provide the money and activists required to run effective electoral campaigns (Hibbs 1977; Schlesinger 1984; Kitschelt 1994; Aldrich 1993, 1995). Aldrich (1983) has formalized this idea in a Downsian model with party activists in which party leaders exchange policy influence to relatively extreme core constituents for unpaid work during campaigns.[3] The logic is illustrated by the American primary system where successful presidential candidates first have to win the support of the parties' core constituents before they can contest the general election. In the general election they have an incentive to moderate their image to appeal to the median voter, but since they were chosen as candidates on different platforms, the perception among voters of real policy differences is accurate.

Aldrich's amended Downsian model raises a critical issue of commitment in politics—an issue that is also important for understanding partisanship. If the winning electoral platform in an election is the median voter preference, but candidates

[2] For a more exhaustive discussion of the different possibilities in two-party systems, see Grofman 2004.

[3] An interesting model along these lines has been developed by McGann 2002. In the McGann model voters sort themselves into parties based on their platforms, and the platforms of parties are determined by their members (the median members to be precise). Needless to say, this set-up produces policy divergence.

represent partisan constituencies, how can their commitment to the median voter be credible? Downs largely skirted this issue by assuming that party platforms had to be consistent over time, but it is now standard to assume that such commitments cannot be credible (Persson and Tabellini 1999, 2000). In modern political macroeconomics, for example, governments have a short-term incentive before elections to make the economy look better by using inflationary policies, even as such policies are unsustainable and have adverse economic effects (Alesina, Cohen, and Roubini 1992; Franzese 2002; Adolph 2005; Clark 2003).[4] This creates room for partisan politics.[5]

'Citizen-candidates' models takes this idea to its logical conclusion by assuming that candidates cannot commit to anything other than their own preferred policies (Osborne and Slivinski 1996; Besley and Coate 1997). With two candidates and costs of running, the equilibrium is away from the median voter because otherwise one citizen would not find it worthwhile to enter the race (why run if someone is already representing you?). With strategic voting this divergence can be quite large because voters may not want to switch from an existing candidate to a more moderate entrant in the fear that this may cause the least preferred candidate to win.

Turning from party competition to government formation, new bargaining models also do not bear out the idea that the median legislator can dictate policy. If there is real bargaining taking place, Rubinstein bargaining theory essentially implies that parties will split their policy differences. The threat to break off negotiations and initiate bargaining with another party cannot be used by the median party to get its way. If there are *any* costs of switching (which may simply be the cost of a delay), the new bargaining partner has no incentive to offer a bargain that is better than the original *minus* the cost of switching.[6] As long as parties have different policy preferences, as in the citizen-candidate model, governments will therefore be away from the median.

But while there are compelling reasons why partisanship matters, a critical issue that has largely been skirted is why some countries are dominated by center-left governments and others by center-right, governments. In the absence of such dominance we may get partisan political business cycles, as argued by Alesina and others (Alesina, Cohen, and Roubini 1992), but partisanship could no longer explain persistent cross-national differences in policies and outcomes. Nor could partisanship serve as a credible commitment mechanism as it does in the Lange–Garrett model. In fact, government partisanship *does* vary significantly across democracies (see Powell 2002; Iversen and Soskice 2002), and much of the evidence for the importance of partisanship is cross-national.[7]

[4] Stokes 1999 offers an interesting alternative in which parties appeal to the short-term interests of voters in the election only to pursue longer-term group interests after the election.

[5] There is a very large literature on the political economy of macroeconomics that is not discussed here. Much of this literature originates with Hibbs's seminal 1977 article and is discussed by Hibbs in his contribution to this volume.

[6] This can be inferred from the 'outside option principle' in bargaining theory (Osborne and Rubinstein 1994, 128), although the only proof I am aware of is in Iversen and Soskice 2005.

[7] Power resource theory implies that this is due to differences in the strength of the labor movement. But unionization, or the size of the industrial working class, are poor predictors of partisanship.

Rather surprisingly, most of the literature also fails to distinguish between the preferences of parties and the preferences of voters. Observed policy differences between left and right governments could be due to either. There are methodological fixes to this problem—such as comparing the ideological composition of the government to that of the legislature, or focusing on 'natural experiments' where the outcome approximates a random assignment of the partisan 'treatment' (say, in very close elections)—but we also need a theory of voter preferences. Since voters have an incentive to be 'rationally ignorant,' as argued by Downs many years ago, it is not really satisfactory to assume that parties simply reflect the interests of citizens. Partisan models must also explain how interests are defined and become common knowledge.

## 4 Multiple Dimensions and Coalitional Politics

As noted in the introduction, distributive politics is inherently multidimensional because a pie can be divided along as many dimensions as there are political agents vying for a piece. It is therefore hard to understand why politicians should constrain themselves to contest a single policy instrument such as the proportional tax/flat rate benefit in the Meltzer–Richard model. And when alternative tax-benefit schedules are considered the results change. In Snyder and Kramer (1988), for example, the choice is over different—linear and non-linear—tax schedules, and the majority choice is no longer redistribution that benefits the poor. The Snyder–Kramer model is itself restricted to one dimension (because it limits the choice to single-parameter schedules subject to an exogenously given revenue target), but it demonstrates the sensitivity of models to the tax-benefit assumptions they make.[8]

One of the first to recognize the importance of multidimensional distributive politics was Esping-Andersen (1990). He distinguishes three different 'worlds' of welfare capitalism—each associated with a distinct tax-benefit structure. In the most redistributive (social democratic) type, progressive taxation is coupled with flat rate benefits; in the 'liberal' type means-tested benefits are targeted to the poor; while in the 'conservative' type benefits are tied to income and occupation. Esping-Andersen makes a plausible (and interesting) argument that the structure of benefits is associated with, and perhaps causes, different social divisions and political patterns: the poor against the middle class in the means tested, insiders versus outsiders in the conservative, and public against private sector in the social democratic. To explain redistributive politics, political economy therefore has to endogenize the structure of benefits.

8 The winner in the Snyder–Kramer model is the middle class—a result that is echoed in some multidimensional models discussed below.

Clearly this task can be accomplished neither with a median voter model, nor with a simple left–right partisan model. Building on the majestic work of Moore (1971), Esping-Andersen instead suggests that the answer lies in historically unique class coalitions. Red-green coalitions in Scandinavia forced socialist parties (which lacked stable majorities) to accept universalism. State-corporatist coalitions in continental Europe were forged by autocrats like Bismarck to stem the rise of the labor movement. In countries such as Britain and the USA, where the state and the left were both relatively weak, social issues were essentially dealt with through an extension of the old poor laws, allying the middle class with higher income groups.

Echoing the recent literature on path dependence (Pierson 2000), Esping-Andersen then suggests that the structure of the benefit system re-produces the political support for each type. But neither the origins of the three worlds, nor their stability, can be said to be *explained* since there is no argument to preclude alternative outcomes. For example, why would it not be possible for liberal welfare states to expand redistribution towards the middle class? Or why does the middle class not try to exclude the poor from sharing in the generous benefits of the social democratic model? Or why can outsiders not offer a deal to a subset of insiders in the conservative model that would cause it to break up? Without any explicit theory, much of Esping-Andersen's analysis comes across as post hoc description.

One could make a similar charge against Lowi's (1964) account of public policy-making. Like Esping-Andersen's work, however, it provides a convincing account of the numerous forms that distributive politics can take. Specifically, Lowi distinguishes between distributive, regulatory, and redistributive politics. Distributive politics refers to a situation where narrowly defined groups or constituencies, such as congressional districts, seek to maximize their share of appropriations. Since most of the cost can be externalized, everyone pushes for more spending and no one has a sufficiently strong incentive to prevent others from doing the same: 'I agree to spend on your project if you agree to spend on mine.' The result is what Olson terms 'distributive coalitions,' characterized by excessive and wasteful spending.

Unlike the quiet, behind the scenes logrolling of distributive politics, regulatory politics pits losers against winners as some are advantaged and others disadvantaged by public policies (public procurement decisions, licensing, and other regulatory decisions). Redistributive politics of the Meltzer–Richard variety is also contentious, but the divisions are across class instead of sector. In a sense, therefore, the nature of public policy (distributive, regulatory, or redistributive) can be predicted by the policy area. Yet, it is hard to see how 'policy area' can be treated as a truly exogenous variable any more than the benefit structure in Esping-Andersen's story can. Sure, we know that the New Deal involved class politics and major redistribution, and it clearly contrasts to Lowi's other types, but to move beyond post hoc description needs a theory of why distributive politics takes on particular forms at particular times.

Dixit and Londregan's (1996, 1998) model of transfer spending takes us one step in this direction. Assuming probabilistic voting, if two parties do not know individual voter preferences but do know the distribution of such preferences by groups, and if loyalty to different parties varies across groups, vote-maximizing parties will concentrate transfers on the groups with the most 'swing' voters. The principle is to distribute transfers so that the marginal vote gain for each dollar spent is exactly the same across all groups. Not surprisingly, groups that have a lot of swing votes will be advantaged because the returns (in terms of votes) of investing in these groups are higher than in other groups. With the additional assumption that loyalists are concentrated among the rich and the poor (those with 'extreme' preferences), the implication is that middle class will receive most of the transfers [9]—a result that is known as 'Director's Law'.

An assumption that is not explained is why parties cannot compete for the loyalty of ideologically committed voters. For example, if African-Americans are loyal to the Democrats because of the party's position on affirmative action (or for any other reason for that matter), then there is no reason Republicans cannot appeal to these voters by adopting a more pro-affirmative action platform. Yet, if loyalty is endogenous to party strategies we are right back into strange world of multidimensional politics without a core. The same is true if we drop the two-party assumption since the 'divide the pie' game then does not predict any stable coalition. What is lost by adopting restrictive model assumptions is therefore the ability to consider more complex patterns of coalition formation, and that limits the usefulness of the model for comparative purposes.

Modeling multidimensional coalitional politics is at the center of several new attempts to understand distribution in democracies. In Roemer's (1997, 2001) model, people have intrinsic preferences on some ascriptive dimension such as race or religion, in addition to preferences over redistribution. If the redistributive dimension was the only one that mattered, the analysis would essentially collapse to a Meltzer–Richard model. When a second dimension is introduced, however, the right party can appeal to poor religious or racist voters, and the left party is forced to respond by attracting more wealthy anticlerical or anti-racist voters. As this 'exchange' of voters takes place, the two constituencies will tend to become more similar in terms of income. The original pro-welfare coalition is thus broken apart by appeals to commonalities on another, non-economic, dimension. As Riker (1986) recognized informally many years ago, the (re-)bundling of issues is a critical component of coalitional politics, and it helps explain why the poor don't soak the rich.[10]

Alesina and Glaeser (2004) make a related argument for why racial politics may undermine redistribution. If people feel altruistic only towards people of their own race, they will not redistribute to a minority that constitutes a disproportionate share of the poor. Of course, if solidarity with the poor is a 'taste' then we need a theory of why people acquire this taste, and Alesina and Glaeser go on to argue that elites that

9 Not *necessarily* all because if utility is declining in income, and hence transfers, marginal vote gain of giving to groups with many swing voters will also be declining.

10 The model is in fact more complicated because, once again, there is no equilibrium with majority voting in a multidimensional space. Roemer solves this problem in two alternative ways. In the first formulation one party gets to select its platform before the other party, producing a Stackelberg equilibrium. In another, different factions of both parties must all agree to the policy platform, and this reduces the feasible policy space to a single point.

oppose redistribution can use the race card to undermine support for redistribution. This is similar to the logic in Roemer's model.

Austen-Smith and Wallerstein (2003) provide a quite different story about the importance of race. In their model people have 'race-blind' preferences and are simply trying to maximize their net income. Yet the mere existence of a second dimension (here affirmative action) can cause a legislative coalition in favor of redistribution to break up. The reason (loosely speaking) is that the rich in the majority can offer a bargain to the minority that strengthens affirmative action but reduces redistribution to the poor. Of course, other coalitions are also feasible, but none that generate as much redistribution as bargaining over a single redistributive policy.

The Roemer and Austen-Smith models are both very complex, but they suggest that countries with a higher dimensionality of the policy space also tend to have less redistribution. Przeworski and Sprague (1986) proposed a similar idea when they argued that left policies would become less prominent as party competition became more influenced by non-economic issues. Yet, to my knowledge no systematic comparative test of the effect of multidimensionality on redistribution has been carried out. Alesina and Glaeser present cross-national correlations between spending and racial, ethnic, and religious heterogeneity, but this is an area ripe for empirical research.

## 5 Democratic Institutions

As we have seen, some of the literature has dealt with the complexity of democratic politics by using highly simplifying model assumptions. The Meltzer–Richard model is a prominent example. Such simplifications lead to clear and deductively valid inferences, but they often come at a considerable cost in terms of realism. Those who believe that this cost is too great have turned to history and 'thick description.' Much of this work, such as Esping-Andersen's influential book on welfare capitalism, underscores the importance of coalitional politics and leads to compelling accounts of cross-national differences in policy regimes. Yet, post hoc description is not explanation and the few attempts to model the complexity of coalitional politics, while promising, are themselves complex and have so far produced little comparative research.

In the view of many scholars, focusing on the role of institutions strikes an attractive middle ground. Instead of ad hoc model assumptions, the constraints on political behavior are derived from observed characteristics of political and economic institutions, and instead of post hoc descriptions of behavior, outcomes are predicted from the interaction of purposeful behavior and institutional constraints.[11] The approach has been highly successful in explaining cross-national differences in economic policies and outcomes.

[11] 'Institutions' defined broadly as the 'rules of the game' (North 1990).

Other contributions to this volume go into considerable detail on the role of particular institutions. Here I pick some prominent examples that focus on either democratic institutions (this section) or economic institutions (the next section).[12] These examples are meant to be illustrative of the institutionalist approach, not an exhaustive discussion of the literature. I begin with a discussion of the role of the electoral system because it is a feature of democracies that varies a great deal *and* covaries with government spending, redistribution, and income equality (Persson and Tabellini 2005). This covariation has become the focus of intense scrutiny in recent work in comparative political economy.

In a path-breaking article, Carey and Shugart (1995) propose one way to understand the effects of electoral rules. They argue that the electoral system shapes the incentives of politicians either to toe the party line or to use their influence over public policies to cultivate a personal following. Confirming a long-standing intuition among students of political parties, the incentives for politicians to campaign on a broad party platform depends on the ability of parties to control politicians' re-election chances. The best-known means to accomplish such control is a closed party list system where a candidate's rank on the list determines that candidate's likelihood of re-election. By contrast, in systems where candidates are chosen through primaries, such as elections to the US Congress, political parties cannot directly control who gets on the ballot and politicians have a stronger incentive to pursue their own agendas or location-specific interests.

Carey and Shugart do not fully spell out the implications of their argument for economic policy, but an obvious hypothesis is that systems that encourage politicians to cultivate a personal following will lead to a targeting of public money to local projects and narrowly defined groups. An example is the pre-1994 Japanese system where candidates from the same party competed against each other for a single non-transferable vote, and therefore produced strong incentives for politicians to spend on their districts while ignoring the public interest. The consequence was a highly fragmented fiscal policy (Cox and Thies 1998; Rosenbluth and Thies 2001). This resonates with Lowi's notion of distributive politics, but in the Carey–Shugart framework the nature of the policy-making process is a function of the political system, which varies across time and space. Put another way, Carey and Shugart help explain differences in countries with respect to their use of policies across Lowi's categories.

In Persson and Tabellini's (1999, 2000) account, the critical institutional feature is the electoral formula. In majoritarian systems, they argue, if middle-class swing voters are concentrated in particular districts, parties have an incentive to completely ignore other districts that are leaning one way or the other ideologically. These districts are 'safe' and therefore not worth fighting over. Similarly to Dixit and Londregan's model, money therefore flows to swing votes in middle-class districts. In single-district PR systems, by contrast, there are no safe districts so politicians cannot ignore the loss of support among other groups if all transfers are concentrated on the middle class.

[12] For a comprehensive review of the 'new' institutionalist literature see Hall and Taylor 1996.

The result is greater dispersion of spending across classes (or more spending on broad public goods).

PR systems, however, tend to spend more on both transfers and public goods. To explain this Persson and Tabellini point to a 'second-order effect' of PR, namely that PR systems tend to have more parties and be ruled by multi-party governments (see Ordeshook and Shvetsova 1994; and Neto and Cox 1997 for the evidence). If each party wants to spend on its own group (so that the space is multidimensional) this can lead to a common-pool problem with excessive spending as a result (see also Bawn and Rosenbluth 2002; Crepaz 1998). Again, Lowi's distributive politics has been linked to an institutional feature that varies across countries.

Electoral systems also appear to be systematically related to class politics. Iversen and Soskice (2002) argue that when parties representing different classes have to form coalitions to govern, as is typically the case under PR, the center and left have an incentive to get together to soak the rich. This is not true in two-party majoritarian systems where parties are coalitions of classes. In this situation, middle-class voters can be soaked by the poor if the center-left party deviates from a median voter platform. Assuming that the right cannot engage in regressive redistribution, incomplete platform commitment therefore puts the median voter at risk and the center-left at an electoral disadvantage. The implication is that partisanship and redistribution, class politics in Lowi's scheme, systematically covary with electoral system.

Another democratic institution that has generated intense scholarly scrutiny is federalism. Much of the research in this area originates with Brennan and Buchanan's (1980) argument that competition between local governments for mobile sources of revenue undermines the ability of predatory governments to impose excessive taxation. Coupled with the potential ability of states to secede, which restricts the ability of central governments to exploit member states, federalism may also constitute a credible commitment to property rights—what Weingast (1995) calls 'market-preserving federalism.' Viewed from the left, this logic suggests that federalism may undermine the welfare state and lead to under-provision of social welfare or a 'race to the bottom' (Pierson 1995).

A related argument is that federalism makes it harder to pass new legislation because it has to be ratified in two legislative assemblies. The implication is a status quo bias, which is sometimes argued to have slowed down the expansion of the welfare state (Huber, Ragin, and Stephens 1993). But while federalism does appear to be associated with smaller governments, there is in fact a striking amount of variance across federalist states (Obinger, Leibfried, and Castles 2005). Swiss and US federalism seems to be linked to low spending, but this is not true of German or Austrian federalism.

To account for this variation, Rodden (2003) has proposed to distinguish between federalist systems with different fiscal institutions. If local spending is locally financed, tax competition puts a damper on spending, but if local spending is financed through central or intergovernmental grants local politicians have little incentive to contain spending. This argument applies more generally to political systems where there is a division of labor between local governments and the center (a unitary system like Sweden, for example, exhibits a lot of local policy autonomy).

Revenue-sharing may be seen as a source of common-pool problems (or Lowi's distributive politics), or it may be seen as a method to reduce the power of those with mobile assets and empowering governments to pursue redistribution. Whatever the normative perspective, if there are two different types of federalism, one with a 'soft' and one with a 'hard' budget constraint, then a key issue is why some governments have adopted hard budget constraints while others have not (Wibbels 2003; Wildasin, this volume). Alternatively, there is a wealth of interesting variation in revenue-sharing between local governments and states that seem critical to fiscal policy, yet is not treated as an object of explanation. There *is* a literature on fiscal discipline and its consequences (see von Hagen and Harden 1995 and the von Hagen chapter in this volume), but budgetary rules are for the most part treated as exogenous.

By anchoring model assumptions in the rich details of actual political institutions, the new institutionalist literature enables the coupling of formal reasoning with the realism of inductive research. It reduces the indeterminism of democratic policy-making and suggests promising ways to endogenize partisanship, coalition formation, and styles of policy-making. Our understanding of fiscal policies and distributive outcomes has been greatly advanced in the process. But by highlighting the critical importance of institutional detail, one cannot help but wonder if the real task is not the explanation of the institutions themselves. I return to this question below. But first I turn to another successful branch of institutionalism that focuses on modern capitalism as an economic system.

## 6 Varieties of Capitalism

It is common to portray democratic capitalism as a system where markets allocate income according to efficiency while governments redistribute income according to political demand. This suggests a convenient intellectual division of labor between economists and political scientists, but it is based on a neoclassical view of the economy that few today believe. Instead, the dominant approach to the study of capitalism as an economic system builds on new institutional economics and is known as the 'varieties of capitalism' (or VoC) approach (Hall and Soskice 2001). Just as democracy has been shown to divide into institutional subspecies, so has capitalism. As I discuss at the end of this section, there is in fact a close empirical association between political and economic institutions, although the reasons for this association are not well understood.

The VoC approach assumes that economic institutions are designed to help firms and other economic agents make the best use of their productive assets (Hall and Soskice 2001). As argued by Williamson (1985), North (1990), and others, when an economy is characterized by heavy investment in co-specific assets, economic agents are exposed to risks that make market exchange problematic.[13] A precondition for such an economy to work efficiently is therefore a dense network of institutions that provide information, offer insurance against risk, and permit continuous and impartial enforcement of complex contracts. In the absence of such institutions, exchange is possible only at a small scale in local trading communities where repeated face-to-face interactions enable reputational enforcement of contracts. At a larger scale, or with a greater division of labor, markets that are left to their own devices will either be accompanied by costly and continuous haggling, or be restricted to exchanges of very homogeneous goods.

Another central feature of the VoC approach is the idea that an institution has to be understood in relation to other institutions. Institutional complementarity means that the effectiveness of one institution depends on the design of another. A precursor for this idea is Lange and Garrett's (1985) congruence model that I discussed above. The VoC approach takes the idea much further and argues that *all* major institutions of capitalism are complementary to each other: the industrial relations system, the financial and corporate governance system, the training system, and the innovation system. For example, if firms make investments in their workers' skills, unions gain hold-up power that can be levied against the firms. This necessitates an industrial relations system where such hold-up power can be managed. Conversely, workers will be reluctant to acquire firm-specific skills unless firms can make credible long-term commitments, which require a financial system that provide access to 'patient' capital, and a corporate governance system where workers are given influence, and so on.

Because of these institutional complementarities, one is not likely to find every logically conceivable combination of institutions in the real world. In fact, Soskice (1999) makes the claim that there are only two dominant types: one called liberal market economies (LMEs) and another called coordinated market economies (CMEs). Each is characterized by the extent to which institutions protect and encourage investment in assets that assist firms in pursuing particular product market strategies. In CMEs where firms and workers have invested heavily in assets that are specific to particular companies, industries, or jobs, institutions are designed to protect those investments.[14] In LMEs where such institutional protection is missing or weak, market competition encourages economic agents to make investments in general assets since, in the absence of protection, mobility is the best insurance against risks. This does not eliminate specific assets, but it will reduce their relative importance.

The VoC argument suggests a very different explanation for the welfare state than power resources theory. Mares (2003), for example, argues that companies and industries that are highly exposed to risk will favor a social insurance system where cost and

[13] Polanyi 1944 is an important precursor for many of these arguments.

[14] For example, unionized workers with specific skills possess very considerable potential hold-up power over firms, and firms will be unlikely to rely on workers with specific skills unless individual unions are constrained through an inter-firm collective bargaining system. In turn there is strong evidence that such collective bargaining systems lead to a compression of wages (Rueda and Pontusson 2000; Wallerstein 1999).

risk are shared, leading employers to push universalistic unemployment and accident insurance. Although low-risk firms will oppose such spending, it is remarkable that universalism has been promoted by groups of employers since the literature associates it so closely with policies imposed on employers by unions and left governments. Estevez-Abe, Iversen, and Soskice (2001) and Iversen (2005) further argue that social protection (including job protection, unemployment benefits, income protection, and a host of related policies such as public retraining programs and industry subsidies) encourages workers to acquire specific skills, which in turn enhances the ability of firms to compete in certain international market segments.[15] The welfare state is thus linked to the economy in a manner that creates beneficial complementaries. This may help explain the lack of evidence for the deleterious effects of social spending on growth, and why globalization has not spelled the end to the welfare state.

A mostly unexplored topic in the VoC literature is the relationship between economic and political institutions. It is striking, for example, that the distinction between LMEs and CMEs is almost perfectly collinear with the distinction between PR and majoritarian electoral systems. One possible explanation, which goes back to Katzenstein's (1985) work on corporatism, is that PR promotes the representation of specialized interests in the legislature and its committees. At least this would be true if parties have incentives to accommodate each other's specific interests.[16] Majoritarian systems, by contrast, encourage parties to elect strong leaders in order to convince the median voter that they are not beholden to special interests. Such 'leadership parties' are consequently not conducive to the protection of specialized interests and therefore encourage economic agents to make investments in more portable assets (say, college degrees as opposed to extensive vocational training).

Another reason for the coupling of electoral and economic systems may be that PR serves as a credible commitment to social protection because of its effect on class coalitions and redistribution. A high level of insurance will encourage investment in risky assets and hence support a particular type of firm.[17] This is merely a conjecture that awaits careful empirical corroboration. In particular, it will need to be shown that the correlation between electoral systems and production regimes is not a historical accident, but the result of a deliberate design of political institutions by representatives of particular economic interests.

[15] The skill argument may also account for some of the variation in labor market segregation between the sexes. Women appear to find it much harder than men to enter into specific skills jobs, and this may have to do with the differential ability of men and women to commit to uninterrupted careers (Estevez-Abe 1999; Iversen and Rosenbluth 2005).

[16] This logic may build on Laver and Shesple's (1996) model of government formation, since policies of multiparty governments in their theory reflect the distinct interests of the participating parties. Parties representing agriculture dominate the agricultural ministry, parties representing industry the industrial ministry, etc. It does require, however, that parties representing significant specific interests are not permanently excluded from policy influence.

[17] Although redistribution and insurance are analytically distinct, in practice they are closely related. The unemployed, the sick, the old, and those with low pre-fisc income more generally, will support redistribution and benefit from it. But for those who are currently employed, healthy, young, and enjoying a high income such redistribution is insurance against many of the vagaries of the capitalist economy.

The question of institutional origins, of course, is a matter that concerns the entire institutionalist approach to political economy. The more successful political economy is in explaining economic policies and outcomes with reference to the institutional design, the more pressing it is to explain why one design was chosen rather than another (Thelen 1999; Pierson 2000). But the question then is how we can approach this task without being overwhelmed by the complexity of institution-free politics. In the concluding section I ask whether the answer may lie in a structuralist approach.

## 7 Conclusion: Towards a New Structuralism?

A decade ago nearly all comparative political economists would have called themselves institutionalists. Today an increasing number of scholars are convinced that only way forward is by going back—back to the origins of institutions and the conditions that gave rise to them. The questions that are being asked by these scholars are fundamental: under which structural-economic conditions do autocracies move to democracy (Acemoglu and Robinson 2005; Boix 2003)? How do institutions emerge that will protect property right and produce prosperity (North and Weingast 1989). What are the origins of modern skill systems (Thelen 2004)? What accounts for differences in the structure of social programs (Mares 2003; Swenson 2002). What are the origins of federalism (Wibbels 2003) and electoral institutions (Boix 1999; Cusack, Iversen, and Soskice 2004)?

In a recent paper, Rogowski and MacRay (2003) conjecture that many of the institutional effects that have been documented in painstaking detail by decades of institutional research are in fact epiphenomenal to the structural conditions and interests that gave rise to them. If this is true, it puts a premium on understanding the 'pre-strategic' policy preferences of agents and the circumstances that determine them. But if we make this move we need to understand that this could bring us head to head with the elephant in the corner: the chaos of institution-free politics. If we take this path, is there a way to avoid post hoc description or ad hoc modeling?

Take the example of Acemoglu and Robinson's (2005) theory of democratic transitions. This is one of the most sophisticated attempts to model democracy, yet it relies heavily on the simplifying assumptions of the Meltzer–Richard model. Understandably, Acemoglu and Robinson replace the lack of well-defined institutional constraints with the safe familiarity of a model that is easy to understand, but may not be entirely appropriate for the task at hand.

There is however the outline of an alternative in Acemoglu and Robinson's book that I will loosely refer to as 'new structuralism' (to distinguish it from Marxist structuralism, structural functionalism, and other previous uses of the term). In their model, inequality and the size of the middle class powerfully shape the incentives that political agents have to compromise. A sizeable middle class essentially serves as a buffer against radical demands for redistribution under democracy. Or to put the matter in more general terms: the structure of the pre-democratic economy places constraints on political agents that may help to explain their behavior.

This idea of rooting actors in the structure of the capitalist economy can clearly be taken further. Rogowski (1989), who has made one of the most influential structural arguments to date, focuses on variation in trade exposure and relative factor endowments. Others imply that political interests are defined not only by factor endowments, but by the specificity of these factors (e.g. Frieden 1991; Alt et al. 1996; Iversen and Soskice 2001). In a different context, Mares (2003) emphasizes the size of firms and their exposure to risk when explaining preferences over social policy. Swenson (2002) similarly traces employer-driven social policy initiatives to product market conditions. In explaining modern skill and production systems, Thelen (2004) argues that the strength of the guild system at the dawn of the industrial revolution powerfully shaped subsequent developments.

These examples do not add up to a single coherent approach to the study of institutional design. The point is simply that as we move 'behind' the institutions to explain their genesis, an important task will be to identify the key structural attributes of capitalist economies, and the agents that populate them, so that they can serve as parameters in our models of institutional design and change. Similarly to the institutionalist project, the success of the new structuralism will depend on combining carefully identified historical constraints with rigorous theorizing. If this can be done, we may be able to leave the elephant in peace for a while longer.

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