



Article scientifique

Article

2006

Accepted version

Open Access

This is an author manuscript post-peer-reviewing (accepted version) of the original publication. The layout of the published version may differ .

The American Welfare State in Comparative Perspective: Reflections on
Alberto Alesina and Edward L. Glaeser, Fighting Poverty in the US and
Europe

Pontusson, Harry Jonas

How to cite

PONTUSSON, Harry Jonas. The American Welfare State in Comparative Perspective: Reflections on Alberto Alesina and Edward L. Glaeser, Fighting Poverty in the US and Europe. In: Perspectives on politics, 2006, vol. 4, n° 02. doi: 10.1017/S1537592706060233

This publication URL: <https://archive-ouverte.unige.ch/unige:41003>

Publication DOI: [10.1017/S1537592706060233](https://doi.org/10.1017/S1537592706060233)

The American Welfare State in Comparative Perspective: Reflections on Alberto Alesina and Edward L. Glaeser, *Fighting Poverty in the US and Europe*

Jonas Pontusson

Fighting Poverty in the US and Europe: A World of Difference. By Alberto Alesina and Edward L. Glaeser. Oxford: Oxford University Press, 2004. 262 pages. \$55.00 cloth¹

Alberto Alesina and Edward Glaeser's recent book, *Fighting Poverty in the US and Europe*, exemplifies the recent incursion of economists into the domains of political science and sociology. In thinking about welfare states, economists have traditionally been interested in their effects on the distribution of income and, above all, their implications for efficiency and growth.² Alesina and Glaeser instead set out to explain why "Americans are much less willing to redistribute from the rich to the poor than Europeans" (2) or, in other words, why the American welfare state is so small by comparison to European welfare states. This, then, is a book about American exceptionalism in the realm of social policy, but Alesina and Glaeser's discussion also addresses the general problem of accounting for cross-national variation in the public provision of social welfare. Their project is to provide an account of the exceptional nature of the American welfare state that is consistent with and sheds light on differences among other welfare states as well. This makes for an audacious book that deserves critical scrutiny.

According to *The Economist's* laudatory review, Alesina and Glaeser "are doing what the best in their profession do well these days: seeking to explain society not merely with conventional economic tools but with analysis of institutions, geography and social behaviour."³ The absence

of formal models makes Alesina and Glaeser's book readily accessible to a wide audience, but also makes one wonder whether there is anything distinctively "economist" about the book.⁴ From the perspective of contemporary political science, the tenor of *Fighting Poverty* is surprisingly old-fashioned in the sense that the book probes inductively for causes of American exceptionalism in a manner reminiscent of "historical-comparative macro sociology." Another striking feature of the book is the absence of any sustained engagement with the existing literature on comparative welfare state development. One searches in vain for references to Esping-Andersen, Hicks, Huber and Stephens, Pierson, or Swank, to mention only a few recent contributors to this literature.

For the most part, I shall resist the temptation to complain about Alesina and Glaeser's neglect of the comparative welfare state literature. My essay begins by summarizing the main arguments and evidence presented by Alesina and Glaeser and then engages in a critical discussion of both their approach to comparative social inquiry and the core claims of their account of American exceptionalism in the realm of social policy. I argue that Alesina and Glaeser's quest to nail down "first causes" is something of a distraction and that their use of comparative analysis to bolster their account of American exceptionalism is not entirely consistent. More specifically, Alesina and Glaeser's claim that proportional representation promotes redistribution lacks micro-foundations and their discussion exaggerates both the racial-ethnic homogeneity of European societies and the role of socialist parties in twentieth-century European politics. As the comparative welfare state literature suggests, the absence of socialism alone does not provide a satisfactory explanation of why the U.S. lacks a

Jonas Pontusson is Professor of Politics at Princeton University (jpontuss@Princeton.EDU). This essay was written while I was a Visiting Scholar at the Russell Sage Foundation. I am most grateful to the foundation for its support. For comments on previous drafts, I am grateful to Ira Katznelson, Mary O'Sullivan, and Kenneth Sokoloff.

European-style welfare state. I conclude that greater attention to historical dynamics represents a potentially fruitful way to advance our understanding of cross-national variation in the scope of redistributive social programs.

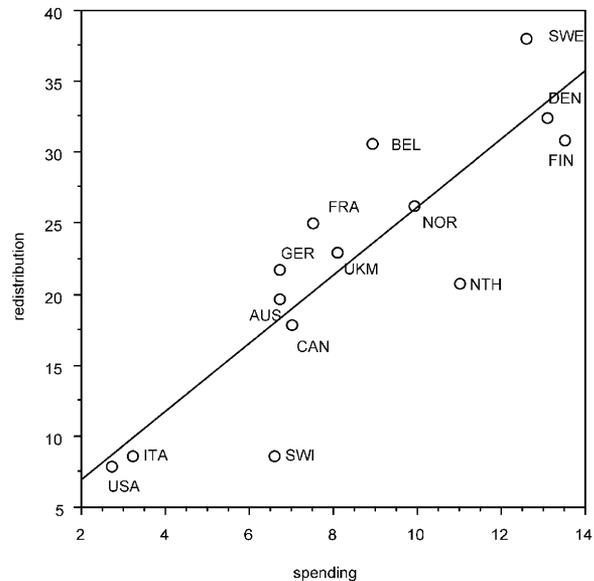
What Is to Be Explained?

A couple of points of clarification are in order before we delve into Alesina and Glaeser’s account of American exceptionalism. To begin with, it should be noted that the title of their book is somewhat misleading, for this is not a book about government policies specifically designed to alleviate poverty. Rather, Alesina and Glaeser are interested in any and all policies that affect the distribution of income. In other words, their objective is to explain why the U.S. government redistributes less than European governments do. The second point is potentially more significant: though Alesina and Glaeser present evidence on the redistributive effects of taxes and income transfers in a handful of countries (21–38), their efforts to test different causal arguments on a cross-national basis rely on government spending on social transfers, expressed in percent of gross domestic product (GDP), as a proxy for redistribution. Thus their empirical analysis actually addresses the question of why the U.S. government spends less on social transfers than European governments do.

Some readers are likely to object to the way Alesina and Glaeser conflate the question of the extent of redistribution with the question of the size of the welfare state. Much of the existing literature emphasizes that redistribution is but one—and possibly not the most important—motivation behind the growth of social spending in advanced industrial countries over the twentieth century. It is commonplace to argue that social programs provide insurance against various social risks and that a wide range of range of social groups—farmers and high-income workers as well as low-income workers—have an interest in this kind of insurance.⁵ In a related vein, Esping-Andersen’s classic treatise, *The Three Worlds of Welfare Capitalism*, argues that the key differences among advanced welfare states have to do with the way the provision of social benefits is organized rather than the amount of money governments spend on the provision of social benefits.⁶ The literature inspired by Esping-Andersen typically holds that the universalistic welfare states of Scandinavia are more redistributive, though not necessarily larger, than the insurance-based continental welfare states of continental Europe.

To my mind, the distinction between insurance and redistribution as separate motives behind public provision of social welfare has often been overdone. All available evidence indicates that low-income workers are more exposed to risk than high-income workers.⁷ Therefore, even social insurance programs that provide income-differentiated benefits are bound to have important redistributive effects. As figure 1 illustrates, the extent of redistribution and the size

Figure 1
Redistributive effects of income transfers in late 1990s versus government spending on income transfers in percent of GDP in 1995.



Note: Redistribution is here defined as the percentage change in Gini coefficients from “net market income” to “disposable income,” with Gini coefficients calculated for households headed by people between the ages 25 and 59 (standard LIS equivalence scale). The social spending figures do not include spending that specifically targets the elderly. Measures of redistribution that include the elderly are difficult to interpret; in countries with generous public pension systems, people tend to save very little for their retirement and therefore many elderly have virtually no “market income.” For each country, the observations of redistribution are the most recent available from the Luxembourg Income Study. The Swiss figures refer to 1992 (redistribution) and 1990 (spending).

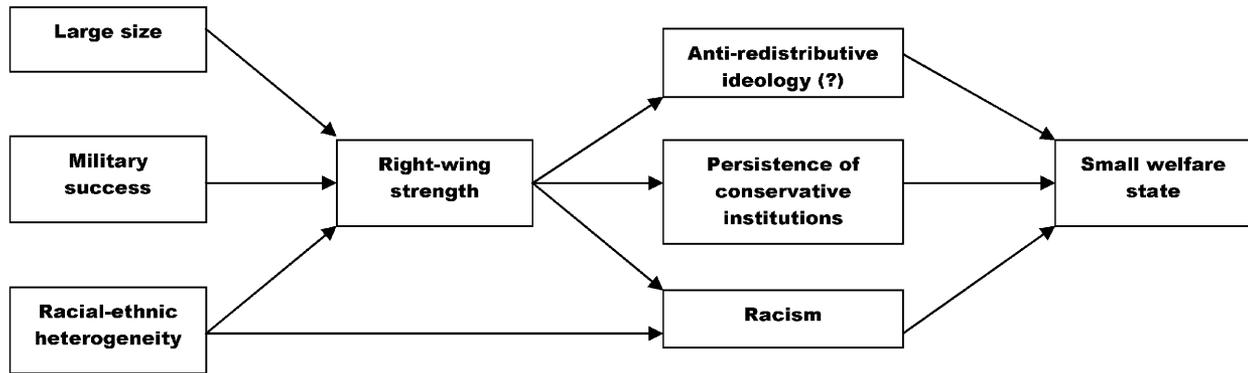
Source: Pontusson 2005 (Figure 7.2).

of the welfare state, measured by public spending on income transfers in percent of GDP, are in fact closely correlated on a cross-national basis.⁸ Based on this evidence, Alesina and Glaeser would seem to be on quite solid ground in using social spending as a proxy for redistribution and in suggesting that the politics of the welfare state can largely be conceived as the politics of redistribution.

Alesina and Glaeser’s Account of American Exceptionalism

Figure 2 summarizes the core causal claims of Alesina and Glaeser’s book in schematic form. Alesina and Glaeser develop this account of American exceptionalism through a process of backward induction. They begin by considering a range of arguments that could conceivably explain cross-national variation in the size of the welfare state. They reject some of these arguments as being irrelevant to the contrast between the U.S. and Europe while they treat others as partial explanations, identifying proximate causes

Figure 2
Alesina and Glaeser's account of American exceptionalism



that call for further explanation, at a deeper level of causality.

At the very outset, Alesina and Glaeser reject the relevance of two “economic” explanations of public welfare provision for understanding why the U.S. is different from Europe. The median-voter theorem postulates that demand for redistribution increases with market inequality. By any and all measures, however, the distribution of income before taxes and transfers is more unequal in the U.S. than in any other long-standing member of the Organization for Economic Cooperation and Development (OECD). Similarly, Alesina and Glaeser reject the relevance of international openness. While it is true that the U.S. economy is larger and less exposed to international competition than all European economies, standard macroeconomic measures show that the American economy is also more volatile than European economies. Hence the proposition that openness generates demand for social protection by increasing economic insecurity does not seem relevant to understanding American exceptionalism.⁹

As Alesina and Glaeser note early on, and document at some length in a later chapter, the difference between Europe and the U.S. in the scope of redistribution, measured by social spending, corresponds to sharply different popular views about poverty and opportunities for upward mobility. While 60 percent of EU citizens subscribe to the view that the poor are trapped in poverty, only 29 percent of Americans share this view. Conversely, 60 percent of Americans believe that the poor are lazy, as compared to 26 percent of EU citizens (184). Not surprisingly, Americans who believe that the poor are trapped are much more likely to support public welfare provision than those who believe that the poor are lazy. Alesina and Glaeser also show that the percentage of the population who believe that luck determines income correlates closely with the percentage of GDP devoted to social spending across nineteen OECD countries (187) and, similarly, that beliefs about poverty correlate with the generosity of welfare benefits across states within the U.S. (190).

Popular beliefs about poverty and opportunities for upward mobility might be invoked to explain differences in public policy, but this line of argument obviously raises the question why American attitudes are so different from European attitudes. Alesina and Glaeser argue quite vehemently that these differences do not correspond to objective economic realities (61–68 and again 191–195). Actual differences in income mobility between Europe and the U.S. are much smaller than popular beliefs would lead us to expect. Based on one study comparing income mobility in Germany and the U.S., Alesina and Glaeser suggest that the American poor may, in fact, be more trapped than their European counterparts. Also, low-income households work at least as hard in the U.S. as in most European countries. As for the possibility that popular beliefs are the product of divergent historical experiences, Alesina and Glaeser assert that “most available evidence supports the view that nineteenth-century America was no more and possibly less mobile than twentieth-century America” (65) and cite several studies showing that income mobility in selected American cities was not hugely greater than income mobility in comparable European cities in the nineteenth century.¹⁰

Alesina and Glaeser thus arrive at the proposition with which most political scientists and sociologists begin, namely that “differences in beliefs about the economy have more to do with politics than with economics” (197). With “the right” serving as a shorthand expression for political actors opposed to redistribution, Alesina and Glaeser proceed to argue that “ideology is more of an effect of the political success of the right than a cause of that success” (185). The “Marxist-inspired” view of the poor as “locked into poverty through no fault of their own” came to prevail in Europe because of “the ability of socialists to dominate political discourse and the schools” (208). In the U.S. as well as Europe, “politicians used rhetoric, symbols and schooling to propagate their own world view. . . . The difference between the United States and Europe is that in Europe, the left won, and then used

the tools of power to impose their ideology on the nation” (210–211).

Alesina and Glaeser’s discussion of ideology lends itself to two alternative interpretations. Some of this discussion suggests that beliefs about poverty and opportunities for upward mobility are more or less irrelevant to the question of why the U.S. redistributes less than European countries do. Alternatively, Alesina and Glaeser might be read as saying that such beliefs constitute an important causal mechanism linking partisan politics to public policy outcomes. Their ambiguity on this score is difficult to resolve, but it is clear that Alesina and Glaeser assign much greater causal weight to two other proximate causes of American exceptionalism in the realm of social policy: political institutions and racial-ethnic heterogeneity.

Alesina and Glaeser’s discussion of political institutions rehearses several conventional arguments. Like many students of American political development, Alesina and Glaeser identify federalism as an obstacle to the expansion of the American welfare state, arguing not only that state-level decision-making restricts the scope for redistribution across states, but also that tax competition represents an important constraint on redistribution within states (89). They also point to the Senate and the judiciary as institutions providing conservative forces opposed to redistribution with political leverage. The emphasis that Alesina and Glaeser put on electoral rules represents a more distinctive feature of their discussion of institutions, motivated first and foremost by the observation that there exists a very strong correlation between the proportionality of the electoral system and levels of social spending across OECD countries (see 86). Bringing the association between proportional representation and social spending to the fore represents an important contribution to the comparative welfare state literature, to which I shall return.

In several passages, Alesina and Glaeser refer to ethnicity, language and religion as divisive forces in the history of the American labor movement, but their main argument about racial-ethnic heterogeneity as a source of American exceptionalism focuses on “racial hatred” as the basis for political mobilization against redistributive policies. The core of this argument is captured by the following observation: “when there are significant numbers of minorities among the poor, then the majority population can be roused against transferring money to people who are different from themselves” (134). In support of this thesis, Alesina and Glaeser demonstrate that there exists a strong correlation between racial fractionalization and social spending across a sample of fifty-three countries (141–142) and also that there exists a strong correlation between blacks as a percentage of the population and the generosity of welfare benefits across American states (147).¹¹ In addition, Alesina and Glaeser present survey results indicating that race and racial attitudes are important determinants of support for redistributive policy in the U.S. Finally, they describe the role

that racism has historically played in the mobilization of American voters against the expansion of the welfare state.

Controlling for levels of economic development, Alesina and Glaeser estimate that the cross-national association between electoral proportionality and social spending accounts for roughly half of the difference between the U.S. and the average spending level in Western Europe (87). A similar analysis regressing spending on racial fractionalization and economic development yields results indicating that racial fractionalization accounts for 40–60 percent of the difference in social spending between the U.S. and Western Europe (145). In short, political institutions and racial heterogeneity matter more or less equally and jointly provide a more or less complete account of why the U.S. redistributes less than Europe, apparently leaving “ideology” with no independent role to play.

In a passage downplaying the role of ideology, Alesina and Glaeser refer to “institutions” and “heterogeneity” as “the root causes of right-wing political success” in the U.S. (185). Elsewhere, however, Alesina and Glaeser argue forcefully that institutions should *not* be seen as “innate, exogenous first causes.” Instead, institutions should be seen as “flexible” and “ultimately the result of deeper, and perhaps more permanent differences between countries” (95).¹² Their reasoning here is clarified by the observation that “the role of U.S. courts . . . is due to an explicit institutional design, so one cannot take it as an ‘explanation’ . . . this ‘explanation’ simply begs the question of why the United States chose these kinds of checks and balances and attributed this role to the courts” (93).

Why, then, did the U.S. end up with political institutions that have been biased against welfare-state expansion? As Alesina and Glaeser point out (97, 219), the institutional structures of nineteenth-century European states were even less hospitable to redistribution, so the real question becomes, why did democratization usher in constitutional change in Europe but not in the U.S.? Alesina and Glaeser’s answer to this question is strikingly simple: socialist parties and labor unions were more powerful in Europe than in the U.S. and effectively forced the introduction of proportional representation and other institutional changes in the early part of the twentieth century. In their own words, “the political power of socialist and labor groups in the early part of the twentieth century are [sic] the primary cause of proportional representation in Europe” (107). Leaving aside, for the time being, the accuracy of Alesina and Glaeser’s account of the introduction of proportional representation (PR), this clearly amounts to yet another reformulation of the question rather than a final answer. The new question becomes, why were labor movements and Left parties more successful in Europe than the U.S.?

Though less adamant, Alesina and Glaeser’s brief discussion of the “endogeneity of racial hatred” (177–180) serves to qualify the extent to which racial fractionalization by itself explains the limited scope of redistribution

in the U.S. This discussion not only concedes that racist attitudes became much less prevalent in the U.S. over the last quarter of the twentieth century, but stresses that attitudinal changes have been accompanied by behavioral changes, most notably a sharp decline in residential segregation. Noting that this decline of racial segregation followed seven decades of “hardening racial attitudes in northern cities,” Alesina and Glaeser speak of “a relatively fluid process where racist attitudes ebb and flow over time” (178). Why then does racial fractionalization remain closely correlated with lower levels of welfare provision across countries and across American states? Alesina and Glaeser’s solution to this puzzle is to argue that heterogeneity creates “the potential for hatred,” but hatred itself is “the outcome of a political equilibrium where politicians supply hate when hatred is a complement to their policies” (179). Again, “right-wing political success” becomes a determinant of racism, which no longer appears terribly different from other forms of ideology. Still, racial heterogeneity remains an important exogenous variable in Alesina and Glaeser’s account, altering the probability of success of race-based political appeals.

In Alesina and Glaeser’s account, the proximate causes of American exceptionalism in the realm of social policy—political institutions, racial hatred and (perhaps) popular beliefs about opportunities for upward mobility—all lead to the question of why conservatives and market-liberals have dominated American politics while socialists and other progressives have dominated European politics. The brevity and speculative nature of Alesina and Glaeser’s treatment of this question is disappointing, given its centrality to the book as a whole. Essentially, Alesina and Glaeser identify three basic reasons for U.S.-European differences in the relative influence of “left-wing” and “right-wing” political forces. First, the geographic size of the U.S. made it impossible to organize an effective nationwide labor movement. In Western Europe, especially the smaller countries, early labor protests were successful in forcing constitutional change “because the seats of power were so close to the masses of industrial workers” (112). The second reason has to do with military conflicts and pertains to the contrast between the U.S. and the larger European countries. In France, Germany, and Italy, Alesina and Glaeser argue, labor movements were only successful in forcing constitutional change in the context of military setbacks (or outright defeats) that undermined the repressive capacities of the state. The fact that the U.S. has never lost a major war or experienced foreign invasion thus looms large in their account of American exceptionalism. Finally, Alesina and Glaeser again invoke racial and ethnic divisions as an explanation of the failure of the labor movement to project political influence at the national level. As illustrated by figure 2, Alesina and Glaeser thus end up with geography, military success, and racial-ethnic heterogeneity as the ultimate causes of American exceptionalism.

How Much Should We Worry about First Causes?

The quest to nail down first causes constitutes an important feature of Alesina and Glaeser’s approach to social science. Alesina and Glaeser are not only concerned, as all social scientists should be, with making sure that their explanatory variables are exogenous in the sense of being conceptually and temporally prior to the outcome; they also seek to get to the start of each causal chain, i.e., to identify explanatory variables about which we would not need or want to ask why the cases differ on this variable. Most of us would presumably be satisfied with treating geography—specifically, in this case, country size—as given or “innate,” but it is far less obvious why we should be satisfied with treating racial-ethnic heterogeneity or military success in this fashion. In part, the racial-ethnic makeup of different countries is, of course, a result of their immigration policies. To be sure, we must stop somewhere, to avoid infinite regress, but where to stop becomes a matter of scholarly judgment. So long as the causal argument is not circular, there is considerable room for scholars to make different judgments on this score. By the same token, such judgments need to be explicitly justified.

For the sake of argument, let us agree that country size, military success and racial-ethnic heterogeneity constitute reasonable first causes of American exceptionalism. The question then becomes whether these three variables constitute an exhaustive list of relevant first causes, operating through the balance of political power? Louis Hartz’s followers would surely want to add the absence of feudalism to the list.¹³ As Alesina and Glaeser seem to concede (e.g., 191), economic affluence would also seem to be a feature distinguishing the U.S. from Western Europe in the nineteenth century. Alesina and Glaeser make very little effort to persuade us that they have identified the right first causes or, in other words, that their first causes are more important than other potential first causes.

There is undoubtedly some truth to the claims that the large size of the U.S. and the heterogeneity of its population have affected the ability of the American labor movement to influence national politics. However, it is equally clear that country size and racial or ethnic heterogeneity leave a great deal of variation in labor power unexplained from a comparative perspective. Australia is a huge country with a dispersed population and multiple centers of economic and political power, much like the U.S. Yet, as Alex Hicks points out, “across the first four decades of the [twentieth] century, the Australian Labour Party was far and away the most electorally well-supported socialist or labor party in the world, averaging over 43 percent of the vote in 16 elections.”¹⁴ More importantly, there are multiple pathways whereby country size might explain variation in the mobilizational capacity of labor movements. Alesina and Glaeser’s discussion of country size focuses on

the ability of labor movements to effect constitutional change through disruptive behavior. Just as plausibly, country size might affect labor power through economies of scale in union organizing.¹⁵

Most social scientists these days would agree, I think, that explanation involves specifying the causal mechanisms as well as identifying exogenous causal variables. Reading Alesina and Glaeser's book one sometimes has the impression that their quest to nail down first causes gets in the way of building micro foundations for their arguments about electoral systems and race-based political appeals. In the end, it is the intervening variables in Figure 2—conservative political power, institutional design, and racism—and their causal effects that constitute the core of Alesina and Glaeser's account of American exceptionalism. The critical reader should ask, for example, whether she buys their analysis of the relationship between Left power, proportional representation, and redistribution. The question of whether (or how) country size affects labor power does not really further our understanding of the associations among these variables.

Paired Comparison or “Large-N” Comparative Analysis?

Alesina and Glaeser engage in two different types of comparative analysis. On the one hand, they juxtapose the U.S. to Western Europe or, in other words, use a more or less stylized “European experience” as a foil to shed light on the unique features of the American experience. On the other hand, they engage in more broad-based comparative analyses in which the U.S. constitutes but one of many data points and “Europe” dissolves into some 15–20 different data points. The latter type of comparison is meant to complement the former. Quite rightly, Alesina and Glaeser aspire to develop an explanation of American exceptionalism that is consistent with broader patterns of cross-national variation in the extent of redistribution. To overcome the limitations of paired comparison, they also use their “large N” analyses to generate estimates of the relative importance of different variables for explaining the gap between social spending in the U.S. and the average level of social spending in Western Europe.

The combination of different comparative strategies is an attractive yet problematic feature of *Fighting Poverty*. Students of comparative European politics will be troubled by passages in which Alesina and Glaeser seem to treat “Europe” as if it were country or represented a single historical experience rather than being an abstraction deployed for heuristic purposes. More importantly, the way Alesina and Glaeser deploy large-N comparisons to bolster their account of American exceptionalism is not entirely consistent.

Comparing various measures of macro-economic volatility for the U.S. and the E.U. leads Alesina and Glaeser

to reject the idea that welfare-state expansion might be seen as a response to societal demands generated by exposure to the world economy (70). The authors make no further effort to probe the associations between openness, volatility and welfare spending across OECD countries. In this and other instances, the paired comparison of the U.S. and Europe serves as a screening device: no matter how important openness and vulnerability might be from a broader comparative perspective, these variables drop out of Alesina and Glaeser's discussion because they do not shed any light on American exceptionalism vis-à-vis Europe. Elsewhere, Alesina and Glaeser suggest that explanations of American exceptionalism should be evaluated, in part, based on whether or not they also explain variation among European countries. In downplaying the significance of income mobility as a source of American exceptionalism, for example, they observe that “the data within Europe do not seem to suggest a pattern where less mobility is associated with a stronger welfare state” (67).

Alesina and Glaeser never apply the idea that “within-Europe comparison” might be used to assess the relative merits of plausible explanations of why the U.S. is different from Europe to their argument about racial fractionalization. Even a cursory glance at the scatterplot they present in support of the claim that racial fractionalization is a major determinant of cross-national variation in redistribution reveals that “within-Europe comparison” does *not* support this claim (141). While the European countries included in this scatterplot all have low scores on Alesina and Glaeser's measure of racial fractionalization, they cover a very wide range on their measure of redistribution. In multiple regression, racial fractionalization remains strongly associated with less redistribution when Alesina and Glaeser control for GDP per capita, but it is clear that the results of this analysis depend on the inclusion of some thirty developing (non-OECD) countries.¹⁶ In the name of maximizing variation on independent and dependent variables, one might well argue that it is a good thing that Alesina and Glaeser's comparative analysis goes beyond the usual OECD suspects. The only problem with this retort is that the association between proportional representation and redistributive spending that Alesina and Glaeser also claim to have established depends crucially, as they themselves point out (85–86), on the analysis being restricted to OECD countries. Considered as a general theory of why some governments redistribute more than others, Alesina and Glaeser's two-pronged account does not appear to hold up to systematic comparative analysis.

Proportional Representation, Left Power and Redistribution

Alesina and Glaeser's explanation of the close association between proportional representation and the size of the welfare state across OECD countries might usefully be

juxtaposed to the explanation proposed by Torben Iversen and David Soskice in a recent paper.¹⁷ For Iversen and Soskice, PR promotes redistribution because it favors government by leftist parties or, more precisely, left-of-center coalition governments. The basic intuition of their formal model is as follows. Low-income and middle-income groups have a common interest in the provision of social benefits financed by taxing high-income groups. However, middle-income groups prefer the policies that a party representing high-income groups would pursue (no taxes, no benefits) over the policies that a party representing low-income groups would pursue (taxes on both middle and high-income groups, with benefits restricted to low-income groups). In a majoritarian electoral system, middle-income voters must choose between one party that seeks to form a coalition of low-income and middle-income voters and another party that seeks to form a coalition of high-income and middle-income voters. Given that either party might in fact pursue the preferences of their core constituency once it has been elected, middle-income voters will tend to play it safe and vote for the party of the Right. The logic of coalition formation changes under proportional representation, which allows for one or more parties directly representing the preferences of middle-income groups. Government participation by such parties ensures that Left-of-Center governments do not veer to the Left and thus broaden electoral support for redistributive politics.

For Iversen and Soskice, then, the political success of Left parties is a result of PR. For Alesina and Glaeser, by contrast, PR is a result of the success of Left parties and other pro-redistribution political forces, such as unions. In their account, PR was typically imposed by left-wing forces or, in some cases (notably Sweden), introduced by conservatives to preempt left-wing mobilization. At first glance, Alesina and Glaeser's class-power account of the introduction of PR in Western Europe will strike many political scientists as odd or, at a minimum, overly simplistic. In particular, many readers are likely to object that Alesina and Glaeser's rendition of this history (97–107) entirely neglects mobilization by ethnic-regional minorities and elite efforts to accommodate these minorities. On the other hand, most conventional accounts recognize that the introduction of PR was part of the process whereby the suffrage was extended, that working-class mobilization was a crucial component of this process, and that conservatives or liberals (possibly both) looked to PR as a means to mitigate the effects of universal suffrage.¹⁸ Andrew McLaren Carstairs' judicious interpretation of the partisan politics of the introduction of PR in Western Europe deserves to be quoted at some length:

Whenever electoral reform through PR gained recognition as a practical possibility, the social democrats and socialists tended at first to be in favour of such reform. As the prospect, or achievement, of universal suffrage rendered it increasingly likely that the social democrats would achieve the status of a major or domi-

nant party, it was bourgeois and non-socialist parties which inclined towards a reform of the electoral system, and the social democrats turned against it. The timing of this change in attitude was seldom such as to prevent the reforms from being carried out, and never succeeded in reversing the adoption of a proportional system.¹⁹

As Carstairs and others make clear, universal suffrage and parliamentary government were the primary objectives of European socialists in the first decades of the twentieth century. PR was never an end itself, but rather conceived as a means to gain more power. Moreover, socialists were rarely, if ever, in a position to impose their preferences regarding electoral rules against the wishes of a united front of non-socialist parties. This said, I hasten to point out that Glaeser and Alesina's historical account of the introduction of PR is actually quite nuanced, recognizing strategic behavior by various political actors, and that their argument does not necessarily involve the claim that socialists legislated PR by themselves. The crucial proposition is that PR was more likely to be adopted where unions and socialists were strong. This is certainly a plausible proposition.

Alesina and Glaeser's discussion of proportional representation raises two questions for their overall account of why some governments redistribute more than others. The first question concerns the status of PR as intervening variable in the causal chain that they trace out. If PR can be treated as an institutional design chosen by political actors committed to redistribution, does PR really have a causal role to play? Why would we not attribute greater redistribution directly to the greater influence of left-wing forces in Europe? Put differently, doesn't Alesina and Glaeser's argument amount to saying that the association between PR and redistribution is in fact spurious?

Alesina and Glaeser clearly believe that PR does have a causal role to play, but they are surprisingly vague regarding the causal mechanisms that lead from PR to redistribution. The following passage is the closest they come to articulating a theory about the link between PR and redistribution: "in majoritarian systems characterized by geographically based electoral districts in which each district chooses one representative, the elected government favors spending programs that can be geographically targeted. Proportional electoral systems, in contrast, favor spending on universal programs" (82). So far as I can tell, this is an argument about *types of social spending* rather than an argument about *levels of social spending*. There is no obvious reason why the majoritarian bias towards geographic targeting would necessarily produce less spending. And whether geographically targeted spending is less redistributive than "universalistic" spending, including income-differentiated social insurance in the Bismarckian tradition, would seem to be contingent on many factors—in other words, an open question.

The second question raised by Alesina and Glaeser discussion of PR is this: Why did apparently ascendant

left-wing political forces choose this type of electoral arrangement? Alesina and Glaeser's answer is interesting (and remarkably consistent with the above quote from Carstairs). Proportional representation, they argue (106),

has generally been the policy of socialists who thought it would strengthen their hand. Presumably, this belief came about because at the moment of constitutional reform, the socialists were still a minority party. Perhaps labor unions knew that they could always count on a core of single issue voters, and these voters would confer success in proportional representation systems.

Along the same lines, Alesina and Glaeser argue that the combination of small electoral districts and geographic concentration of the working class made it possible for the British labor movement to influence politics despite first-past-the-post elections. In the U.K., uniquely, "the labor movement found it relatively easy to ride to power within the established constitution" (112). Again, the proposition that socialists imposed PR does not quite capture Alesina and Glaeser's argument. Rather, PR turns out to be the product of a particular historical conjuncture in which socialists and labor groups had the power to disrupt, but did not yet constitute an electoral majority. This formulation generates a new question: Why didn't continental socialists follow the British example and become advocates of majoritarian elections once they were in a stronger electoral position? One might well argue that electoral rules became increasingly difficult to change over time, but if Alesina and Glaeser were to adopt this line of argument they would need to qualify some of their assertions about the malleability of political institutions.

Is Europe Really So Homogenous?

Alesina and Glaeser are surely right in emphasizing the historical role of racism as an obstacle to progressive politics in the U.S. and I have no reason to contest the proposition that a high degree of racial "fractionalization" was a precondition for racism to assume such an important role. Still, I am inclined to think that Alesina and Glaeser exaggerate the extent to which the European labor movements and welfare states are rooted in social solidarities formed among people with demographic characteristics that enable them see each other as being alike rather than belonging to different racial, ethnic, religious or linguistic groups. Though cognizant of the negative political implications of racial-ethnic heterogeneity, Alesina and Glaeser seem to partake in the American myth of the U.S. as being a uniquely diverse "immigrant nation."

As far as recent immigration is concerned, the U.S. record is not nearly as distinctive as many Americans seem to believe. According to the OECD, foreign-born inhabitants accounted for about 11 percent of the U.S. population in 2001. This figure is significantly higher than the figures for Finland (3 percent), Denmark (6 percent), Nor-

way (7 percent) and Belgium (8 percent), but the U.S. falls in the same range as Germany (9 percent), France (10 percent), the Netherlands (10 percent), Austria (11 percent) and Sweden (12 percent). By this measure, Australia (23 percent), New Zealand (20 percent), Switzerland (20 percent) and Canada (18 percent) stand out as the "immigrant nations" of the OECD.²⁰ Arguably, the political implications of immigration depend crucially on the racial or ethnic composition of immigrant populations. The majority of immigrants to Sweden in the 1960s were Finns, who could be rather easily integrated into Swedish society. Since the 1970s, however, the bulk of immigrants to West European countries have come from outside Western Europe, boosting racial diversity as well as linguistic and religious diversity.

What primarily distinguishes the U.S. from Western Europe is not the extent or character of recent immigration, but rather its long history of immigration and, of course, its history of slavery. Alesina and Glaeser emphasize that immigration to Europe is recent and point to the rise of right-wing populism (Le Pen, Haider, and the rest) as an indication that increased heterogeneity may have the same effects on the politics of redistribution in Europe as it has historically had in the U.S. (175–177). In their words, "if the European welfare state gets rolled back in the near future, it is likely that anti-immigrant rhetoric will be used" (166). Changes in the ethnic composition of European societies have undoubtedly played a role in the rise of right-wing populism, but so has mass unemployment and rising economic inequality. The uneven spread of right-wing populism across Europe and its ups and downs within particular countries also deserve to be noted. Most importantly, we ought to keep in mind that immigration to Western Europe has persisted over the last 30–40 years and that much of the impetus behind recent cost-cutting and structural reforms of European welfare states has come from organized business and mainstream conservatives.

Many European countries have a long history of ethnic, regional, linguistic, and religious divisions—in other words, "historic minorities." One might perhaps argue that race is a particularly salient cleavage because racial minorities look different from the majority, but this argument is too facile. People in general, and perhaps Europeans in particular, seem to be very good at recognizing "difference" and, as Alesina and Glaeser concede, there is no obvious reason why opponents of redistribution could not capitalize on non-racial cleavages that cut across class cleavages. Historically, such cleavages have indeed divided some European labor movements, but the politics of accommodation among different regional/ethnic/religious communities also seem to have contributed to the post-war expansion of some European welfare states.

As Alesina and Glaeser recognize, Belgium is a deeply fractured nation and yet has one of the largest welfare

states in Europe. Quite plausibly, Alesina and Glaeser explain this apparent puzzle by pointing out that the Flemish majority has historically been poorer than the Walloon minority (171–172). Consistent with their discussion of the American experience, Alesina and Glaeser's treatment of the Belgian case clarifies that it is not heterogeneity per se that represents an obstacle to redistributive politics but rather the concentration of poverty among ethnic or racial minorities. This strikes me as a very useful clarification, but it raises two general issues for Alesina and Glaeser. First, their effort to demonstrate that social spending correlates with racial and ethnic fractionalization across countries becomes less relevant, since their fractionalization indices do not capture the theoretically important variable.²¹ Secondly, the endogeneity problem again raises its ugly head. For the purpose of explaining cross-national variation in welfare-related government policies, it seems quite reasonable to treat racial or ethnic heterogeneity as an exogenous variable. However, it is far less clear that the concentration of poverty among racial or ethnic minorities can be treated in this manner, for various aspects of government policy clearly play an important role in determining the relative economic position of minorities.

Returning to the American case, a recent paper by Woojin Lee and John Roemer proposes a different interpretation of the association between racism and lack of redistribution than that advanced by Alesina and Glaeser.²² As indicated above, Alesina and Glaeser attribute this association to what Lee and Roemer refer to as an “anti-solidarity effect:” voters oppose redistributive transfers to minorities whom they perceive as undeserving. Lee and Roemer argue that the association between racism and lack of redistribution might also be due to what they call a “policy bundle effect.” In this scenario, voters who desire redistribution nevertheless vote for the anti-redistributive party (the Republicans) because they support that party's position on race issues. According to Lee and Roemer's estimations, both effects are in play and matter roughly equally.

From a comparative perspective, and particularly from the perspective of Alesina and Glaeser's account of American exceptionalism, the interesting thing about Lee and Roemer's paper is that it suggests that the political consequences of racism are related to electoral rules. Needless to say perhaps, the “policy bundle effect” arises because American voters must choose between one party that is pro-redistribution and also supports racial minorities and another party that is anti-redistribution and opposed to affirmative action and other pro-minority measures. In PR systems, it is possible, at least in principle, to have parties that bundle these issues differently. Indeed, it is not terribly far-fetched to interpret the programs of some right-wing populist politicians in Europe as being pro-redistribution as well as anti-immigrant.

What Do We Learn from the Comparative Welfare State Literature?

In emphasizing redistribution from the rich to the poor as the core activity of modern welfare states and explaining the growth of welfare spending in terms of the influence of labor movements and the political success of socialist parties, Alesina and Glaeser's analysis resembles the class power model of welfare state development advanced by Korpi and other reformist Marxists in the 1970s.²³ The class power model remains influential, but the comparative welfare state literature has evolved since the 1970s. In dialogue with other theoretical traditions, scholars inclined to assign importance to working-class mobilization (myself included) have taken on board a number of other considerations and developed more nuanced accounts. It should also be noted that the class power model was designed in the 1970s to explain variation in the scope of redistribution across European countries and, more broadly, across OECD countries. Applying similar reasoning to the problem of explaining American exceptionalism, Alesina and Glaeser end up conveying a rather odd picture of socialism as the hegemonic political force across Western Europe in the postwar era or, indeed, for most of the twentieth century.

What, then, are some of the insights of the comparative welfare state literature that need to be taken into account in developing a general account of why some governments redistribute more than others? Many recent studies adopt an historical-institutionalist perspective or seek to combine institutionalist and class-power explanations. As we have seen, institutions also play a role in Alesina and Glaeser's discussion, but at least some of their formulations treat institutions as simply temporary arrangements—rules of the political game—chosen by dominant political actors to maintain their power. By contrast, much of the comparative literature emphasizes how institutions shape the preferences and strategies of political actors, the resilience of institutions in the face of globalization and other pressures and, perhaps most importantly, the path-dependent character of institutional change.²⁴

More specifically, this literature brings out the significance of institutional arrangements associated with tripartite corporatism. Perhaps because their discussion focuses on the U.S. case, Alesina and Glaeser appear to be entirely oblivious to the role of corporatism in the development of modern welfare states. This in turn affects the way that they think about the relationship between labor movements and the welfare state. For Alesina and Glaeser, it seems, labor movements influence the political process either through disruptive behavior (general strikes triggering constitutional reforms) or through electoral mobilization. In the comparative literature focusing on European experiences, unions instead appear as constructive participants in the development and administration of social policy.²⁵

The comparative literature also brings out the role of other political actors in the postwar development of European welfare states. Many authors emphasize the historical importance of Catholic social doctrine and, in particular, the role of Christian Democratic parties as agents of welfare-state expansion on the continent.²⁶ In a similar vein, Peter Baldwin's revisionist account argues that centrist (non-denominational) parties representing farmers and the urban middle class forced Scandinavian Social Democrats to adopt more ambitious social policies in the 1940s.²⁷ Most recently, several important studies suggest that employers have often welcomed social legislation and played a facilitating role in its implementation.²⁸

Simply put, the comparative literature teaches us that cross-class alliances played a crucial role in the postwar development of European welfare states. Where labor unions and socialist parties were strong, welfare states assumed more redistributive forms, but European countries developed extensive systems of social welfare provision even in the absence of strong unions and socialist government. The politics of the welfare state are considerably more complex than Alesina and Glaeser would have us believe and the absence of socialism alone does not provide a satisfactory account of American exceptionalism in the realm of social policy.

An Alternative Approach to History

To observe that the determinants of welfare state development are more complex than Alesina and Glaeser's stylized account of American exceptionalism suggests is not a particularly satisfying conclusion. Additional variables must be taken into account, but we still need some kind of theoretical framework that orders these variables and yields specific propositions about how and under what circumstances they matter. For all its limitations, Alesina and Glaeser's book represents an advance on recent quantitative analyses of OECD-wide patterns of social spending in this regard. By way of conclusion, I want to suggest that paying more attention to historical dynamics represents a potentially fruitful way to move the comparative welfare state literature forward.

Like many other contributors to the comparative welfare state literature, Alesina and Glaeser explain contemporary differences in public welfare provision across countries in terms of variables that date back to the nineteenth century, if not earlier. By 1920, at the latest, the configuration of demographics, institutions, and political power that explains why the American welfare state is smaller and less redistributive than European welfare states was already in place. This approach may be adequate for the purpose of explaining the exceptional nature of the American case, but it becomes problematic from a broader comparative perspective, for there is a lot of reshuffling of country rankings on the dependent variable that happens in the intervening period. Table 1 illustrates this point by

Table 1
Government Spending on Social Transfers in Percent of GDP, 1930, 1960, and 1990.

	1930	1960	1990
Leaders	GER 4.8	GER 18.1	SWE 30.8
	IRE 3.7	AUT 15.9	DEN 29.3
	DEN 3.1	FRA 13.4	NTH 27.6
	FIN 3.0	BEL 13.1	BEL 26.9
		ITA 13.1	FRA 26.6
Middle (median marked by double line)	SWE 2.6	DEN 12.3	FIN 24.8
	NZL 2.4	NTH 11.7	NOR 24.7
	NOR 2.4	SWE 10.8	AUT 24.1
	UKM 2.2	NZL 10.4	ITA 23.3
	AUS 2.1	UKM 10.2	GER 22.8
	AUT 1.2	CAN 9.1	NZL 21.9
	SWI 1.2	FIN 8.8	UKM 19.5
	FRA 1.1	IRE 8.7	IRE 18.1
	NTH 1.0	NOR 7.9	CAN 18.6
		AUS 7.4	SWI 17.9
	USA 7.3		
Laggards	BEL .6	SWI 4.9	AUS 14.2
	USA .6	JPN 4.1	USA 13.4
	CAN .3		JPN 11.2
	JPN .2		
	ITA .1		

Sources: 1930 and 1960 figures from Lindert 2004, 12–13; 1990 figures from OECD 2004b.

reporting data on government spending on social transfers, in percent of GDP, for 1930, 1960, and 1990.

Table 1 suggests a number of interesting comparative observations. Over the course of the twentieth century, Japan stands out as the welfare-state laggard *par excellence*. The U.S. was a laggard in 1930 and 1990, but less obviously so in 1960. Germany had developed the largest welfare state by 1930 and retained this leadership position in 1960, but the German welfare state grew at a relatively sluggish rate from 1960 to 1990. The conservative or Christian democratic welfare states of continental Europe expanded more rapidly than other welfare states from 1930 to 1960 and held all five top positions in 1960. Over the subsequent 30 years, we observe a marked divergence among continental welfare states: social spending expanded rapidly in France, Belgium, the Netherlands, and Switzerland, but grew relatively slowly in Austria and Italy, as well as Germany. Most notably perhaps, it was not until the 1960s and 1970s that the Nordic countries emerged as leaders in the OECD social spending league.

The trajectories of the Anglo-Saxon countries, characterized as liberal welfare states in the existing literature, is also noteworthy, especially because all but one of these countries (Ireland) had winner-take-all elections throughout the twentieth century (or at least, in the case of New

Zealand, through the 1980s). Among the six Anglo-Saxon countries, only Canada and the U.S. were welfare-state laggards in 1930. By a significant margin, social spending exceeded the median for the eighteen countries included in table 1 in Ireland, New Zealand, the U.K., and Australia at this time. The relative decline of the liberal welfare states, considered as a group, began in the period 1930–60 and accelerated in the period 1960–90.

Imagine the same scholar writing an essay on why some governments redistribute more than others in the early 1930s, the early 1960s, and the early 1990s—a social scientist employing methods akin to those of Alesina and Glaeser. It is safe to assume, I think, that three essays would have arrived at quite different conclusions. Changes in the causal dynamics of welfare state development represents a challenge that the welfare state literature has yet to tackle in any sustained way.²⁹ Historical dynamics also represent an opportunity to test competing explanations and to refine our causal arguments. Prominently featured in the existing literature, more or less time-invariant institutional variables (electoral rules, constitutional veto points, etc.) obviously cannot by themselves explain why some welfare states expanded more rapidly than others during specific periods, but they may well have played an important role in conjunction with other variables. I do not have any simple explanation to propose for the patterns shown in table 1, but my hunch is that greater attention to historical dynamics would bring two themes to the fore of the theoretical agenda of comparative welfare state research: production regimes in a changing world economy and the mobilization of new social forces.

Notes

1 All parenthetical citations in the text refer to the reviewed book.

2 E.g., Tanzi and Schuknecht 2000.

3 *The Economist*, March 11, 2004.

4 A companion paper for Brookings presents a “brief formal model” from which several of the arguments in Alesina and Glaeser’s book are derived (Alesina, Glaeser, and Sacerdote 2001, 203–208).

5 Moene and Wallerstein’s 2001 model of social spending posits that demand for insurance actually increases with income. Insurance motives also figure prominently in recent work that emphasizes employers’ preferences in the realm of social policy; e.g., Estevez-Abe, Iversen, and Soskice 2001 and Mares 2003.

6 Esping-Andersen 1990.

7 E.g., see Cusack, Iversen, and Rehm 2005.

8 Consistent with the evidence presented in figure 1 Huber and Stephens 2001 (ch. 4) stress the redistributive character of “Christian democratic welfare states.”

9 The openness argument is articulated most forcefully by Rodrik 1997, while the median-voter argument is articulated by, among others, Meltzer and Richard 1981. Needless to say, perhaps, neither Rodrik nor Meltzer and Richard are specifically concerned with the question of American exceptionalism.

10 It deserves to be noted that all economic historians do not agree with these claims. Ferrie’s 2005 analysis of census data indicates that there was much greater occupational and geographic mobility in the U.S. than in the U.K. between 1850 and 1880 and that both forms of mobility have declined in the U.S. since the beginning of the twentieth century.

11 Alesina and Glaeser also report cross-national correlations between ethnic and linguistic fractionalization and welfare spending (142–143). Their fractionalization indices measure the probability that two individuals, drawn at random from the population, will be members of different racial, ethnic, or linguistic groups.

12 Alesina and Glaeser waffle a bit on exactly how “flexible” institutions are. Their favorite formulation seems to be that institutions are “quite flexible” (132), but they also speak of the “extreme mutability of political institutions” (129). The ambiguity of the terms “flexibility” and “mutability” in this context should also be noted: these terms could be taken to mean that the same institutions can serve different political purposes, but Alesina and Glaeser seem to be concerned with one set of institutional rules being replaced by another, as in the case of reforming electoral systems.

13 Alesina and Glaeser note that European consciousness of social class “was surely abetted by the vestiges of feudalism that still remained in Europe through World War I” (208), but do not cite Hartz’s 1955 treatise.

14 Hicks 1999, 98.

15 Wallerstein 1989 develops this argument.

16 Latin American and Caribbean countries account for the majority of these non-OECD countries, but the analysis also includes some Southeast Asian countries. Alesina and Glaeser never provide any explanation of why these particular countries (and not others) are included.

17 Iversen and Soskice 2005.

18 E.g., see Katzenstein 1985, 150–156.

19 Carstairs 1980, 215.

20 OECD 2004a, 307–308. Note that the cited figures for Belgium, Germany, and Switzerland refer to immigrants (non-citizens) rather than “foreign born.”

21 Implicitly recognizing this objection, Alesina and Glaeser suggest that racial fractionalization is more salient than ethnic fractionalization because racial groups are more likely to be differentiated by income (140).

- 22 Lee and Roemer 2004.
 23 Korpi 1978, Stephens 1979.
 24 E.g., Immergut 1992, Pierson 2001, Swank 2002.
 25 Hicks 1999, Swank 2002, Wilensky 2002.
 26 Esping-Andersen 1990, van Kersbergen 1995, Huber and Stephens 2001.
 27 Baldwin 1990.
 28 Swenson, 2002, Mares 2003.
 29 Hicks 1999 comes closest to the approach that I am advocating here.

References

- Alesina, Alberto, Edward Glaeser and Bruce Sacerdote. 2001. Why doesn't the United States have a European-style welfare state? *Brookings Papers on Economic Activity*, 2001(2): 187–254.
- Baldwin, Peter. 1990. *The Politics of Social Solidarity*. Cambridge: Cambridge University Press.
- Carstairs, Andrew McLaren. 1980. *A Short History of Electoral Systems in Western Europe*. London: George Allen & Unwin.
- Cusack, Thomas, Torben Iversen, and Philipp Rehm. 2005. "Risks at work: The demand and supply sides of redistribution." Presented at the conference on Democracy, Inequality and Representation, Syracuse University, May 6–7.
- Esping-Andersen, Gøsta. 1990. *Three Worlds of Welfare Capitalism*. Princeton: Princeton University Press.
- Estevez-Abe, Margarita, Torben Iversen, and David Soskice. 2001. Social protection and the formation of skills. In *Varieties of Capitalism*, eds. Peter Hall and David Soskice. Oxford: Oxford University Press.
- Ferrie, Joseph. 2005. History lessons: The end of American exceptionalism? Mobility in the United States since 1850. *Journal of Economic Perspectives* 19(3): 199–215.
- Hartz, Louis. 1955. *The Liberal Tradition in America*. New York: Harcourt Brace.
- Hicks, Alexander. 1999. *Social Democracy and Welfare Capitalism*. Ithaca, NY: Cornell University Press.
- Huber, Evelyne, and John Stephens. 2001. *Development and Crisis of the Welfare State*. Chicago: University of Chicago Press.
- Immergut, Ellen. 1992. *Health Politics*. Cambridge: Cambridge University Press.
- Iversen, Torben, and David Soskice. 2005. "Electoral institutions, parties and the politics of class." Presented at the conference on Democracy, Inequality and Representation, Syracuse University, May 6–7.
- Katzenstein, Peter. 1985. *Small States in World Markets*. Ithaca, NY: Cornell University Press.
- Korpi, Walter. 1978. *The Working Class in Welfare Capitalism*. London: Routledge.
- Lee, Woojin, and John Roemer. 2004. "Racism and redistribution in the United States." Presented at the Workshop on Wealth and Power in the Post-Industrial Age, Yale University, January 29–30.
- Lindert, Peter. 2004. *Growing Public*. Cambridge: Cambridge University Press.
- Mares, Isabella. 2003. *The Politics of Social Risk*. Cambridge: Cambridge University Press.
- Meltzer, Allan, and Scott Richard. 1981. A rational theory of the size of government. *Journal of Political Economy* 89 (5): 914–27.
- Moene, Karl Ove, and Micahel Wallerstein. 2001. Inequality, social insurance and redistribution. *American Political Science Review* 95 (4): 859–74.
- OECD. 2004a. *Trends in International Migration*.
 ———. 2004b. Social expenditure database. www.oecd.org/els/social/expenditure.
- Pierson, Paul, ed. 2001. *The New Politics of the Welfare State*. New York: Oxford University Press.
- Pontusson, Jonas. 2005. *Inequality and Prosperity: Social Europe versus Liberal America*. Ithaca: Cornell University Press.
- Rodrik, Dani. 1997. *Has Globalization Gone Too Far?* Washington, DC: Institute for International Economics.
- Stephens, John. 1979. *The Transition from Capitalism to Socialism*. London: MacMillan.
- Swank, Duane. 2002. *Global Capital, Political Institutions and Policy Change in Developed Welfare States*. Cambridge: Cambridge University Press.
- Swenson, Peter. 2002. *Capitalist against Markets*. Oxford: Oxford University Press.
- Tanzi, Vito, and Ludger Schuknecht. 2000. *Public Spending in the 20th Century*. Cambridge: Cambridge University Press.
- Van Kersbergen, Kees. 1995. *Social Capitalism*. London: Routledge.
- Wallerstein, Michael. 1989. Union organization in advanced industrial democracies. *American Political Science Review* 83(2): 483–501.
- Wilensky, Harold. 2002. *Rich Democracies*. Berkeley: University of California Press.