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Bringing the Welfare State Back In: The Promise (and Perils) of the New Social Welfare History

The welfare state—the complex of policies that, in one form or another, all rich democracies have adopted to ameliorate destitution and provide valued social goods and services—is an increasingly central subject in the study of American history and politics. The past five years have unleashed a veritable tidal wave of books on the topic, including, from historians, Alice Kessler-Harris's *In Pursuit of Equity* and Michael Katz's *The Price of Citizenship*, and, from political scientists, Robert Lieberman's *Shifting the Color Line* and Peter Swenson's *Capitalists Against Markets*.[1] Journals ranging from the *American Historical Review* to *Political Science Quarterly* (and, with less regularity, even the *American Political Science Review*) now routinely feature analyses of U.S. social policy. And going back just a few years more, the mid-1990s saw the publication of several influential works on the subject, notably, Paul Pierson's *Dismantling the Welfare State?* and Theda Skocpol's *Protecting Soldiers and Mothers*, each of which won major book prizes in political science.[2] If any moment deserves to be seen as a heady time for writing on the American welfare state, this is it.

As natural as this state of affairs has come to be seen, it was not always so. Writing in 1991, the historian Edward Berkowitz lamented that the American welfare state "commands little attention from

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today's students, who view it as a confusing, highly technical, and dry subject that cannot compete with the exploits, often heroic, of the blacks and women who have emerged as major figures in the history classroom over the course of the last twenty years."[3] Although Berkowitz was writing of historians, his complaint applied more broadly. Indeed, what is striking in retrospect—not to mention, in light of the huge share of the economy the welfare state represents, even in its famously stingy U.S. incarnation—is precisely how *few* major works concerned themselves with the American welfare state in the years before Berkowitz's words were penned. Today, however, Berkowitz's complaint rings anachronistic: no current observer would say that students of American public affairs bypass social welfare policy.

The explanation for this reversal is at once simple and complex. The simple reason concerns the world outside the academy. Once protected by a real, if uneasy, postwar consensus, the welfare state came under increasing political and economic strain in the 1980s and 1990s. In the process, it has also become the kind of hot, front-page news topic that academics, for all their avowed eschewal of flashy subjects, love to wade into. At the same time, the tenor of policy debates grew sharply more conservative, making it ever harder to accept the once-common conceit that the United States was simply a "laggard" on a universal path toward expanded state responsibility. What master narrative replaced this well-worn story line was, of course, anything but obvious. But at the very least, the welfare state suddenly seemed open to fresh approaches.

The less obvious reason for increased interest in the welfare state concerns a set of shifts within the academic world itself. The birth of policy history as a self-conscious field, the increasing prominence of the political science subfield of "American political development," the rise of "institutional political history," and the increasing sophistication of scholarship on gender and race—all were critical spurs to the explosion of interest in welfare state development. But perhaps most important, scholars simply woke up to a fact so obvious that it was frequently overlooked: the welfare state is a central element of modern society and politics. The seminal trigger for this new wave of analysis was Gøsta Esping-Andersen's landmark sociological study, *The Three Worlds of Welfare Capitalism*.[4] Esping-Andersen replaced the common unilinear conception of welfare state development with a hugely influential threefold typology that contrasted the "social-democratic" welfare states of Scandinavia with the "conservative" model of welfare provision in Continental Europe and the "liberal" model found in Britain and the United States. Much as John Rawls's *A Theory of Justice* revitalized American political theory, *The Three Worlds of Welfare Capitalism* provided a major impetus for criticism, praise, and refinement of arguments about the welfare state both old and new.

And yet a curious thing has happened to the welfare state on its way from the periphery to the center of scholarly concern. Historians and political scientists are now writing about the American welfare state, but they are not really all that concerned with the welfare state as such. For most, instead, the welfare state has become a convenient window into some larger relation of power—between blacks and whites, between women and men, between capital and labor. Nor, indeed, are most scholars really writing about *the* welfare state. Some concern themselves with public assistance for the poor, others with social insurance programs like Social Security, still others with labor policies, such as rules governing unions. An increasing number, in fact, are interested in policies well *beyond* the typical conception of the welfare state, such as tax policies and workplace benefits. In short, the near-perfect silence on the welfare state that once reigned has given way not to a single or harmonious tune, but to a cacophony of sometimes discordant notes that occasionally threatens to drown out the very subject of the melody.

This is perhaps not so surprising. The very diversity of the welfare state as a subject and the sheer sweep of U.S. social welfare history guarantee that scholars will pursue myriad research avenues. The question is whether these diverse inquiries are also leading to a more general picture of American welfare state development, or simply making more complex and foreboding the topography that has to be traversed. The argument of this essay is that while work on the American welfare state has dramatically improved our understanding of U.S. social policy development, there is a real risk that the stories that emerge will read like "one damn thing after another"—study piled upon study, fact upon fact, without adequate integration, explanation, or advancement.

The positive message, however, is the one to emphasize up front: studies of the American welfare state have revolutionized our understanding of American history and politics—and, indeed, have a good claim to represent the strongest area of scholarship in history and political science more generally. I begin the article, therefore, with a review of the amazingly rich and fertile avenues of inquiry that students of the American welfare state have pursued in the last decade or so. Collectively as well as individually, these recent works testify to the tremendous progress in understanding U.S. social policy that has marked the past decade—a conclusion that I want to stress far more than any specific qualms I have about recent work. Given how much of value has been written, in fact, my review will of necessity be highly selective. Yet there can be little dispute that each of the areas I take up—race and the welfare state, gender and social policy, the role of business, and the interplay of public and private benefits—has witnessed signal advances that, together, offer a far broader and deeper view of the American welfare state.

Nonetheless, all is not perfect in the world of welfare state research, and two concerns in particular emerge from my survey. The first is that a number of major lines of research are developing in almost complete isolation from one another. We have arguments about race and gender, about the role of business, about the interplay of private and public social benefits. These claims are not always mutually exclusive, to be sure. But they are not all wholly compatible either; nor are they all equally applicable to all areas of U.S. social policy. More important, because they are rarely set against each other or mapped onto policy areas different from those they were developed to explain, it is nearly impossible to adjudicate among or synthesize the competing claims they make.

This situation leads to the second general weakness of recent scholarship: a notable absence of explanatory integration. Not only are whole veins of research proceeding in largely unreflective independence from each other, but, in addition, even mid-level propositions that could connect these disparate research enterprises remain largely undeveloped. This is all the more striking because recent scholarship tends to exhibit characteristic explanatory faults that are remarkably similar across different areas of inquiry. In what follows, I argue that four such weaknesses—confusion of intentions and effects; hazy and slippery conceptions of power; failure to pursue relevant cross-national, cross-policy, or historical comparisons; and ahistorical styles of argumentation (even in deeply historical research)—are particularly common and debilitating. And I argue that each of these deficiencies could be at least partially rectified by refashioning arguments and approaches around the issue of *social policy development*—how policies, as both outcomes and causes of crucial social processes, evolve over long periods of time.

It goes without saying that these deficiencies are by no means unique to social welfare history. And lest I be misinterpreted, my message is *not* that historians should share the same interests or methods as political scientists, or that political scientists should discard their theories and dive into the archives (though I encourage diving in once in a while). My premise—as uncontroversial to historians, I hope, as it is to political scientists—is that the goal of research on the American welfare state is to better understand its character, causes, and consequences, and that attention to what other scholars are arguing and to basic rules of evidence and inference are necessary to achieve that goal. For reasons I shall outline, I believe this aim can best be achieved by constructing general claims about processes of long-term social policy development. But my core claim is simply that students of the American welfare state ignore what others are writing and how they can convince others of their views at their peril—and, more important, at the peril of a deeper and broader understanding of U.S. history and politics.

Revisiting Race

Students of American history have long recognized race as a distinctive "American dilemma," in the famous words of Gunnar Myrdal. For good reason: while the U.S. historical experience is certainly not unique, slavery and segregation have left enduring scars on the American body politic and produced stubbornly persistent patterns of black exclusion and disadvantage within American society. Today, few would deny that race has been a leading subtext of welfare state debates since at least the New Deal, or that it plays an important, if often covert, role in current discussions of social policy.

Recent scholarship on race and the welfare state, however, moves beyond these uncontroversial observations to map out exactly *how* race matters in social policy. Race and racism, we learn, informed the original design of many programs and continues to shape public perceptions of them. Moreover, because racial disadvantage is embedded in the larger political economy that these programs seek to shape, race enters into social policy even when it is not on the minds of citizens or elites. Not only, then, do perceptions of racial difference undermine the "social solidarity" that is the cement of the welfare state; equally important, many features of the world that social policies seek to change are "race-laden," in the words of political scientist Robert Lieberman, and hence ostensibly race-neutral policies may have deeply racialized effects.[6]

The new scholarship on race has made major contributions to our understanding of American social welfare history. Yet when it comes to placing race in the context of other forces shaping U.S. social policy, it tends to falter. Few scholars, of course, are so bold as to claim that race is the motor force of American welfare state development. But in their emphases and their arguments, they generally echo the argument of Michael K. Brown that "the problems of race, on the one hand, and the failure to create broadly inclusive social policies for all Americans, on the other, have become entwined."[7] Martin Gilens, in *Why Americans Hate Welfare*, argues, for example, that distrust of welfare reflects the twin beliefs of white Americans that "most people who receive welfare are black" and that "blacks are less committed to the work ethic than are other Americans."[8] While Gilens's point is restricted to welfare (that is, public assistance for the poor), the general tenor of the new work on race and social policy is that welfare is the leading case for a basic relationship: the welfare state and debates about it are explicable first and foremost through the lens of racial analysis.

In pointing toward this more ambitious claim, the new scholarship on race risks running aground on two opposed shoals. On the one hand, relatively straightforward arguments about how racist beliefs inform the formation and evolution of social programs are clear in their mechanisms and in their implications about what supportive evidence should look like. Yet they are also limited in their reach, for many areas of the American welfare state do not appear racialized in the sense of being motivated by explicitly racist intentions. On the other hand, the claim that social policies are "race-laden" because they intersect with larger features of American society marked by racial hierarchy has considerable—indeed near-total—reach, but the political mechanisms it highlights are diffuse and quite problematic as subjects of empirical inquiry. Ironically, in fact, the broadest of such claims are quite similar in their observational implications to the discussions of dissenting scholars who have argued that what is notable about American social policy development is the general *absence* of explicit attention to race.[9] If race is everything—hidden, all-encompassing, unchanging—then it risks being nothing, too.

Robert Lieberman's generally exemplary work on the role of race in the development of the American welfare state is a case in point.[10] At times, Lieberman emphasizes the racist motives that led to key policy decisions in the 1930s and after (for example, the initial exclusion of agricultural and domestic workers from the old-age insurance program created in the Social Security Act). Yet at other points, Lieberman seems content merely to show the huge racial implications of seemingly race-neutral policies—an important point but one with a very different meaning for our understanding of U.S. social welfare history. It is not that both arguments cannot be true. Yet one suspects that the back-and-forth dance between the two reflects a desire to "prove" that race is consequential as much as it is an attempt to provide a compelling argument for when and why it matters (and sometimes does not matter) in U.S. social policy debates.

Grasping Gender

While race has long been a central theme in the study of the American welfare state, gender, surprisingly, has not. This despite the fact that women have played a large and obvious role in the development of U.S. social policy, as well as representing the chief beneficiaries of its major antipoverty programs. This absence of attention can no doubt be chalked up in part to the blinders of traditionally male-dominated and -oriented historical research. Yet this explanation is incomplete. Long after gender was a major focus of work in American history, the welfare state was mostly viewed through the lens of male wage-earners and their struggle for expanded social protection.

To understand this, it helps to recognize that the major theoretical current in welfare state scholarship until recently drew from Marx in emphasizing class struggle as the root cause of welfare-state building. Social policies, on this view, were primarily means of "decommodification," a way of freeing workers from wage dependence by providing them with income when they are unable to engage in well-paid work.[11] Like much labor-oriented theorizing, women, if they entered into the analysis at all, were subsumed within the larger category of "worker"—a move that ignored the extent to which women's relationship to the labor market differed from men's and the degree to which ostensibly self-supporting male workers were supported by female domestic work.

This blinkered perspective is no longer tenable. In welfare state research, "feminist" scholarship has had a major impact over the last decade or so, and a good deal of it has concerned the American experience.[12] Symptomatic of the shift is Skocpol's *Protecting Soldiers and Mothers*—which, while controversial within feminist scholarly circles, details the role played by women's groups and reformers at the turn of the last century in promoting what she calls a "maternalist" vision of the welfare state oriented around state protection for women and children.[13] Against Skocpol's interpretation, other scholars have emphasized the repressive elements of the maternalist vision, while a growing body of writing has reinterpreted the post-1930s development of the welfare state in light of the taken-for-granted subordinate position of women.[14] Recent work has emphasized, for example, that many social insurance and employment programs initially excluded female workers, focused on risks and needs distinctive to men, and were built on the assumption that women would remain home to support male breadwinners.[15]

In comparative research, too, gender has become a central frame of reference.[16] Welfare states do not merely "decommodify," this new comparative work argues. They can also "defamilialize," lessening the extent to which women are required to remain home and care for children by providing public day care and structuring policies in gender-neutral ways. Put simply, welfare states not only affect citizens' place and power in the economy; they also affect their place and power in the household—and, indeed, it is at the nexus of these two realms that women's distinctive role, and dilemmas, lies.

The success of feminist scholarship in reorienting existing theories and suggesting new historical interpretations cannot be gainsaid. Nonetheless, this work has also suffered from a number of common weakness, many of which it shares with recent scholarship on race. The first is that the singular emphasis on gender, like the singular emphasis on race, tends to occlude other forces that shape policy and politics, and to limit analysis to certain corners of the social welfare field—in this case, again, overwhelmingly poverty relief. As with work on race, feminist scholars are also often less than clear whether they are talking about sexist beliefs held by citizens and elites or about the impact of ostensibly gender-neutral policies in a world marked by vast gender inequalities, or both. Indeed, far more than recent research on race, feminist scholars face the challenge of interpreting *absence*, for what is striking in many early social policy debates is precisely how little was said distinguishing women and men. This contrasts with the clear, repeated, and often breathtakingly crude and biased references to race in many of the same political debates.

Capturing Capitalists

Work on gender challenges the laborist perspective for its alleged sins of omission. New writings on the role of business, by contrast, tackle it for its alleged sins of commission. The essence of these works' critique is that previous scholarship has overstated the antimony of interests between capitalists and labor and, in doing so, missed the strong capitalist bases of support for domestic social reform.[18]

This argument has two main variants, which are not mutually exclusive. One says that businesses want social programs to impose costs on competitors—for example, by requiring that all firms pay for benefits that they already provide.[19] The other says that businesses want social programs to off-load their costs onto the public fisc—for example, by socializing the cost of risks to which they are particularly susceptible.[20] Both variants argue, however, that some (but, crucially, not all) businesses want generous social programs.[21] To be sure, organized labor demands social programs, too. But their success hinges on the emergence of "cross-class alliances" with capitalists.[22] Only when the bourgeoisie are on board does the proletariat get what it wants.

The recent sweeping work of Peter Swenson, *Capitalists Against Markets*, which compares the fate of social reforms in the United States during the 1930s and in Sweden immediately after World War II, exemplifies, while deepening, the new business-power thesis. Swenson argues that during the Great Depression a significant segment of the business community was at least latently supportive of new social insurance programs that would cripple their low-wage, low-price domestic competitors. The original turn in Swenson's argument is not so much his identification of a capitalist interest in reform, but his attempt to tease out the bases of capitalist influence. Swenson argues that neither the so-called instrumental power of business (its lobbying prowess and resources) nor its "structural" power (its control over investment and jobs, about which politicians care regardless of whether business organizes to press for policy change) were crucial.[23] Rather, it was New Dealers' anticipation of long-term capitalist support for—and fear of long-term capitalist opposition to—domestic social reforms that, Swenson argues, represents the primary means by which the largely unexpressed pro-reform sentiments of the business community shaped the making of 1930s social policy.[24]

As this brief summary indicates, there is more than a whiff of the New Left to Swenson's provocative thesis. Yet unlike earlier New Left scholars who argued that Progressive Era and New Deal social reforms were essentially conservative creatures of business interests, Swenson and those who make related claims do not believe that the progressive ambitions of social reformers were hijacked by corporate America.[25] They want to argue instead that underlying business interests were largely consistent with what reformers wanted. This, of course, raises the issue of how one demonstrates *influence*. If reformers want what business wants, that could evidence influence or simply preference congruence. And indeed, in much of the recent literature, Swenson's contribution included, surprisingly scant and circumstantial evidence is offered that reformers actually responded to actual or anticipated business power in crafting their proposals.

No less serious, for all the close attention to historical detail that characterizes recent business-power accounts, these works are often at their core notably ahistorical. Swenson, for example, uses large employers' eventual acceptance of the Social Security Act as an important piece of evidence in favor of his thesis that the act was initially consistent with their interests. But, of course, the eventual response to the New Deal is hardly an accurate gauge of initial interests. Once legislation is in place, after all, employers may simply believe they cannot realistically overturn it, or the policy may in fact change what employers want by altering market conditions, reshaping the population of employers, or encouraging new conceptions of business interests.

Similarly, many works that stress employers' influence tend to begin the story when reform gets on the agenda, then trace the direct interventions of business on specific policy choices. But this "snapshot" approach to the role of business makes it nearly impossible to judge the true power of employers, because it leaves unanswered the profound question of whether the policy terrain on which business operates at any particular moment is tilted in its favor or against it.[26] In broader historical relief, for example, what is arguably most striking about social policymaking during the New Deal is the marked *weakness* of employers relative to the position that they enjoyed in the decentralized political economy of the prior decades.[27] Nonetheless, the renewed emphasis on the role of business does powerfully call into question the traditional assumption that capitalists are merely recalcitrant stumbling blocks on the road to social reform.

Heralding the Hidden

In at least one respect, new work on business emulates older theories of the welfare state—and that is in its emphasis on public spending programs like Social Security. In this, the business-power literature is of a piece with nearly everything that has been written on the welfare state. While scholars often note the importance of taxation and policy tools besides direct social spending, studies of the welfare state are, almost without exception, studies of social spending, with little attention paid either to tax policy (including the actual provision of benefits through the tax code) or to the wide range of "publicly subsidized and regulated private social benefits," such as private, employment-based health insurance, that tax policy usually helps underwrite.[28]

On one level, this conflation of social policy and public spending is understandable. Much of what welfare states do, after all, is spend—as much as two-fifths of GDP in some Nordic countries. But on another level, it is unexpected, for taxation and the role of the private sector have probably been the most consistently explosive issues in welfare state development. It is also surprising because one of the most influential theoretical writings on the welfare state—Richard Titmuss's famous *Essays on the "Welfare State"*—placed tax policy (which he termed "fiscal welfare") and private social benefits (which he called "occupational welfare") on par with spending as a means of achieving social welfare ends.[29] Yet Titmuss's insights on this point, unlike many of his other contributions, have produced relatively little follow-up analysis.

That situation has started to change, but not nearly as quickly or as fully as in the other areas we have reviewed. Much of the credit for the shift must go to Christopher Howard's 1997 *The Hidden Welfare State*, which examines the use of tax breaks with social welfare aims, such as the Earned Income Tax Credit (EITC) for the working poor.[30] Howard begins his analysis with a startling finding: federal social spending is perhaps 150 percent as large as official spending figures indicate when tax breaks with social welfare aims are included in the tally. Through an analysis of the origins and development of four such tax breaks—the EITC, the targeted jobs tax credit, the special tax treatment of employer-sponsored pensions, and the home mortgage interest deduction—Howard concludes that these measures emerge and evolve quite differently than public social programs. Most notably, in contrast with public programs, they generally emerge with little debate or conflict as part of congressional revenue bills.

Howard's signal contribution was to stress a point that students of public policy know well, but which welfare state scholars had generally overlooked: governments have alternative instruments for achieving their ends. The welfare state literature had, not implausibly, identified spending as the key instrument of social policy. But in the process, it had missed other means by which policymakers could achieve their goals—from regulation to tax breaks, to judicial empowerment, to the use of government credit and insurance. Moreover, these less-studied instruments appeared to be *particularly* characteristic of the American social welfare framework, with its embedded aversion to the robust exercise of visible government power. Thus, for example, law professor Robert Kagan emphasized that the American welfare state was, in fact, unusually *interventionist*—but not in its spending role.[31] It was, rather, America's strong tradition of "adversarial legalism"—the use of courts and legal procedures to adjudicate societal disputes and alter behavior—that defined the distinctive U.S. social policy landscape.[32]

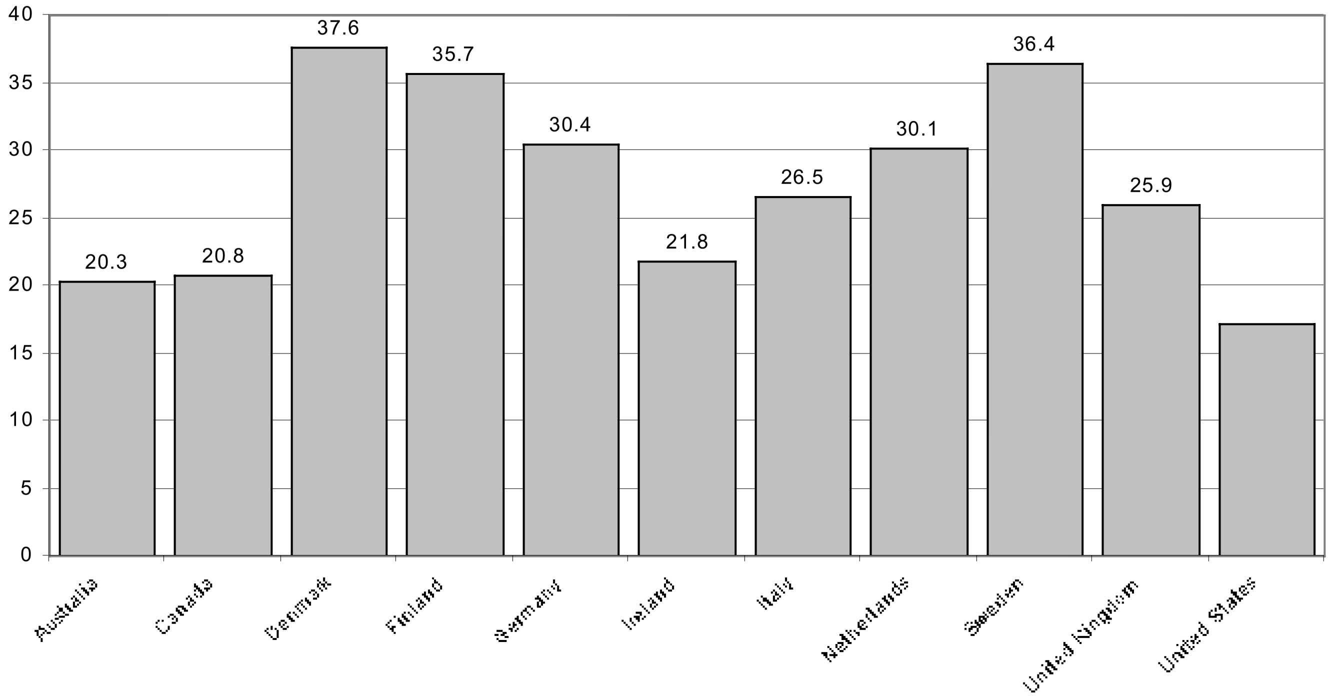
But while Howard and others examined the tools at policymakers' disposal, they had relatively little to say about the vast private-sector field of social welfare, including employer-sponsored benefits and charitable activities, that these tools were often designed to shape. Ironically, then, a line of research designed to showcase the limits of focusing on big government interventions has tended to direct attention back at public leaders and their choice of instruments, rather than to highlight the degree to which many social welfare outcomes are the product of an *interplay* of the goals of political leaders, on the one hand, and the aims of the organizations and actors—from corporations to nonprofits to private individuals—that these leaders hope to influence, on the other.

Recent scholarship, however, has started to highlight this even more "hidden" realm of U.S. social policy. Michael Katz's sweeping new history, *The Price of Citizenship,* for instance, mostly consists of a learned exploration of the standard social welfare concerns.[33] Yet it also devotes a long chapter to the growth and retrenchment of private social benefits in the nonprofit "third sector" and the workplace. Other recent historical works, including Colin Gordon's *Dead on Arrival,* Sanford Jacoby's *Modern Manors,* and Jennifer Klein's *For All These Rights,* place the evolution of private benefits at the center of their story of American welfare-state building, directly challenging the older historical view that these benefits became irrelevant when older forms of "welfare capitalism," increasingly enfeebled, were swept aside by government social programs during the New Deal.[34] Political scientists, for their part, have been slower to move into the field, perhaps because there is so little secondary historical work to build on. But work by Michael K. Brown, Marie Gottschalk, Beth Stevens, and myself indicates a growing interest in incorporating the role of private social benefits into theories of the welfare state.[35]

In the process, this new scholarship has fundamentally challenged the prevailing verdict on the American welfare state—that it is much smaller that its European counterparts. In my book, *The Divided Welfare State,* I show, to the contrary, that American social welfare spending is at or above the average for comparable advanced industrial democracies when properly measured. "Properly measured," in this case, means adjusting for relative tax burdens and including private employer-provided benefits that are substantially regulated or subsidized by government. Because U.S. tax levels are comparatively low and its private social welfare sector is far and away the largest in the world, these two simple adjustments, as Figures 1 and 2 show, raise U.S. social spending from approximately 17 percent of GDP to nearly 25 percent. The conclusion I reach is that "what is distinctive about U.S. social spending is not the *level* of spending, but the *source.*"[36] Building on this finding, Christopher Howard has recently gone even farther, arguing that the "question for future researchers . . . may not be why the U.S. government does relatively little compared to European governments. Rather, the question is why governments of similar size devote comparable resources to pursue similar policy objectives through such a diverse mix of policy tools."[37]

I would not go so far. Whether the policy approaches characteristic of the American social welfare framework really do "pursue similar policy objectives" as those pursued by direct-spending programs abroad is a highly open question, and on the available evidence the answer is no. The United States may spend as much as many European governments when private social benefits and tax policy are taken into account, but the distribution of these benefits is fundamentally different. For one, the *vertical* distribution of benefits—up and down the income ladder—is almost certainly much less favorable toward lower-income citizens. Employment-based benefits are much more prevalent and generous at higher ends of the wage scale,

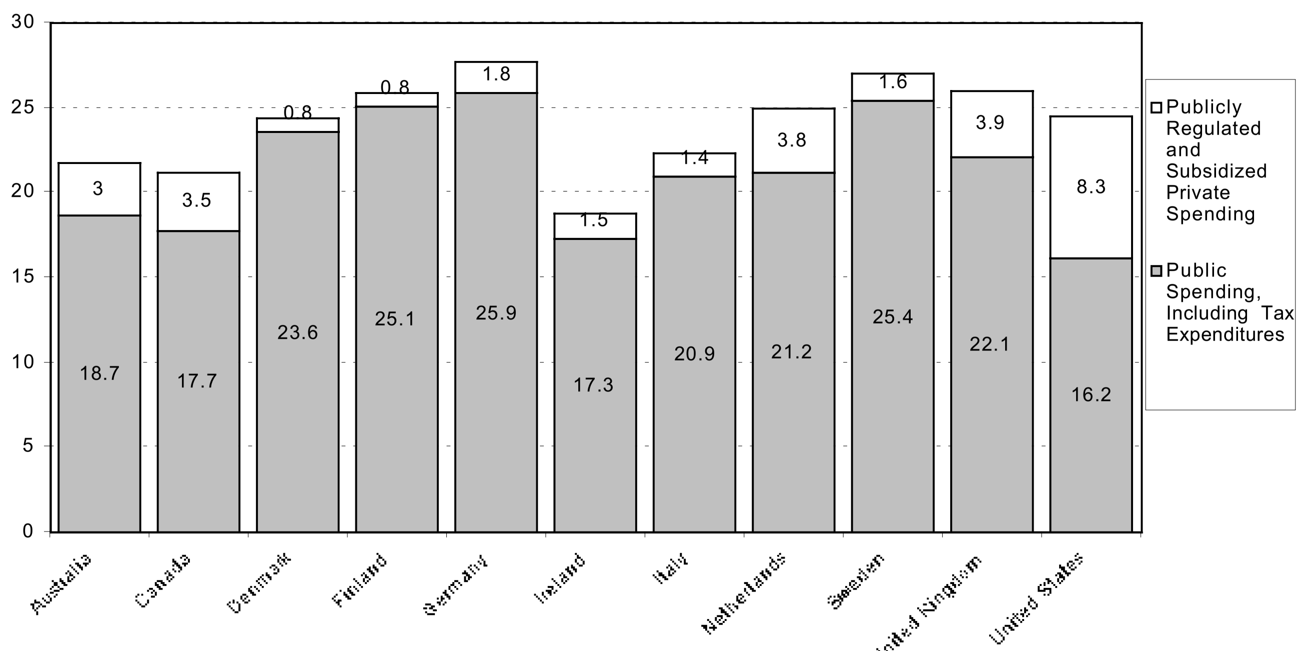
Fig. 1: Public Social Welfare Expenditures as a Percentage of GDP in Eleven Nations, 1995



Source: Calculated from Willem Adema, "Net Social Expenditure," *Labour Market and Social Policy-Occasional Papers No. 39* (Paris: OECD, August 1999), 30.

Notes: Public social welfare expenditures exclude education. They include cash benefits for a wide range of social contingencies—disability, old age, death of a spouse, occupational injuries, disease, sickness, childbirth, unemployment, poverty—as well as spending on housing, health care, services for the elderly and disabled, active labor-market policies, and other similar social benefits.

Fig. 2: After-Tax Public and Private Social Welfare Expenditures as a Percentage of GDP in Eleven Nations, 1995



Source: Calculated from Willem Adema, "Net Social Expenditure," *Labour Market and Social Policy-Occasional Papers No. 39* (Paris: OECD, August 1999), 30.

Notes: Public social welfare expenditures exclude education. They include cash benefits for a wide range of social contingencies—disability, old age, death of a spouse, occupational injuries, disease, sickness, childbirth, unemployment, poverty—as well as spending on housing, health care, services for the elderly and disabled, active labor-market policies, and other similar social benefits. Private social welfare expenditures are payments for the same purposes made by employers and other nongovernmental organizations, provided that such benefits are mandated, subsidized, or regulated by government. To prevent double-counting, tax breaks for private benefits are not included in the public spending estimate.

and tax subsidies, because they forgive tax that would otherwise be owed, are generally worth the most to taxpayers in the highest tax brackets. For another, the *horizontal* distribution of benefits across similarly situated workers is also almost certain to be much less equal. Employers decide whether to provide benefits, and there are whole industries and categories of employment where they are quite rare. Overall, only about two-thirds of workers receive health insurance through employment, and fewer than that have a pension plan at work, much less contribute to it.[38] Perhaps not surprisingly, then, comparative figures on redistribution suggest that taxes and government transfers in the United States reduce inequality significantly less than in any other rich democracy.[39]

Howard's assertion also raises a deeper question: By what standard are we to call the indirect policy tools and government-supported private benefits that are characteristic of American social provision part of that body of state activity conveniently, if often imprecisely, termed the "welfare state"? The scholarship just reviewed makes a strong case for thinking that these distinctive aspects of U.S. social policy should, indeed must, be analyzed in studies of U.S. social policy. But despite frequent use of the evocative (and highly contestable) term "private welfare state" to describe workplace benefits, much of this recent work has surprisingly little to say about why these benefits and tax breaks are on par with the public programs that students of the welfare state usually study. To the extent, moreover, that it simply assumed that the concept of the welfare state can be "stretched" to include all these diverse instruments and policies, then it is not immediately clear why it could not or should not stretch even further—to include almost anything that government does to affect social welfare.[40] Certainly, once the rubric of the welfare states opens up, it cannot be assumed that the generalizations about welfare state development advanced to explain public programs hold equally well in explaining other realms of social provision. Yet why and how indirect policies and private benefits differ from traditional programs—enough that they require new theories and histories, but not so much that they fall outside the bounds of social welfare policy—are questions scholars have only started to explore.

In this respect, the literature on hidden forms of social policy runs into a problem that all of the literature we have reviewed faces: how at once to do justice to the complexities of U.S. social welfare history and provide relatively simplified historical and conceptual accounts that add to our common knowledge. It is fair to say that this is a problem that scholarship on indirect policy tools and private social benefits faces acutely. But failure to advance general claims that can unify disparate research agendas is a notable characteristic of nearly all the scholarship taken up thus far. Ironically, the clearest indication of this difficulty is not the *diversity* of recent scholarship, but the *common* problems into which it runs—confusion of intentions and effects, incomplete investigations of power, lack of comparative leverage, and ahistorical argumentation—problems that I shall argue could be at least partly addressed by thinking more seriously about the dynamics and determinants of over-time processes of policy development.

Intentions and Effects

To write about social welfare policy is to write about why people support and design policies that have specific real-world effects. There are, to be sure, purely symbolic policies that do not change the material world at all, but they are not without effect either. Responding symbolically is responding, and symbolic policies can have very real effects on how people think and act. In every discussion of public policy, therefore, two subjects are at least implicitly under consideration: why a particular course of action (or inaction) was pursued and what effects it had.

Most scholarship on public policy, unfortunately, is interested in only half of this simple equation. Economists and policy analysts, for example, have developed a rich body of theory and evidence concerning the effects of policies on income, wealth, employment, family structure, and so on. But they typically have little or nothing to say about why a policy was adopted in the first place.[41] By contrast, political scientists and other students of public affairs have developed a rich body of theory and evidence concerning the political processes that lead up to the passage of policies, including elections, the formation and expression of public opinion, congressional voting, and interest-group lobbying. But they typically have little or nothing to say about what *effects* a policy has once it is in place. Indeed, while coming at the problem from the completely opposite direction than that followed by economists and policy analysts, they make much the same assumption: that policy effects will flow from political intentions.[42]

Students of policy history are not so bifurcated in their interests. They usually look at the origins and development of important areas of public policy, and in that inherently developmental focus, they have an important comparative advantage. And yet policy historians, too, routinely fall prey to the temptation to conflate intentions and effects. Consider, for instance, the issue already introduced of whether discriminatory policy effects or discriminatory policy intentions are the most relevant evidence of racial or gender bias in social policy. On the one hand, we may emphasize the racist or sexist motives of policymakers, assuming that policies grounded in racist or sexist motives will be racist or sexist in operation. One the other, we may say that public policies are so race- or gender-"laden," because they intersect with elements of society deeply structured by race or gender, that we do not really have to attend to motives. The proof of racism or sexism is in the pudding, so to speak, of outcomes. Neither alternative, however, gets at what is truly important for understanding the role of race or gender in social policymaking: the actual, ongoing relationship between biased motives and biased effects. And, as noted in fact, most work that advances one or the other stance seems to do so less for theoretical reasons than to make a persuasive case for the importance of their favored factor—an effective technique, but one more prosecutorial than scientific.

Or consider the new literature on business power. As already noted, scholars in this tradition often point to the long-term benefits of social policies for sectors of the business community as evidence in favor of the thesis that these favored employers were either supportive in the first place. This, however, is a classic non sequitur. It may well be true, but it is not inherently so. Indeed, reading preferences off of results invites an additional analytic error: failure to investigate the extent to which the preferences are in fact a product, rather than a producer, of the policies to which they putatively gave rise.

In short, two barriers stand in the way of the facile equation of policy preferences and outcomes: effects may not reflect preferences, and preferences may flow from policy as well as the other way around. The great virtue of a long-term developmental approach is that, used carefully, it allows us to examine each of these potential relationships and to begin to explain when and why they hold true sometimes and not others.

Perils of Power Analysis

At root, most analyses of U.S. social policy are interested in power. Who set the agenda? Who decided which alternatives to consider? Who won out in debate over these alternatives? Who benefited or lost under the policies finally chosen, or not chosen? These are the classic questions of social welfare history. And most social welfare historians are hardly shy about answering these questions. "The limited development of the American welfare state reflects the weakness of U.S. labor." "The highly decentralized and limited quality of American antipoverty policies is a result of the institutional power of Southern Democrats." "The major social programs of the New Deal were shaped and bounded by the continuing power of business." Statements like these, and debates over their veracity, are the bread and butter of the field.

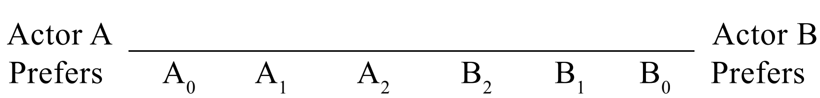
But that does not mean that we are consistently moving closer to definitive, or even tentatively agreed-upon, answers to such questions. Paradoxically, in fact, the more we know about the history of particular episodes, the less in agreement the field seems to be about who actually won out. Put simply, the problem in many cases is not that we lack sufficient knowledge, but that the frameworks within which we are interpreting this knowledge miss basic problems in the definition and measurement of power.

The most revealing example is the debate over the role of business during the New Deal, which has given rise to perhaps the best historiographic record we have on any important example of American policy formation. Understandably, much of the discussion has focused on fights over particular pieces of legislation. Yet political influence means something far broader than triumph in a "head-to-head" conflict over a particular provision. It means the ability to achieve ends consistent with one's underlying preferences, and that ability is often quite difficult to read off of individual policy fights.

The principal justification for widening our view is the concept of anticipated reactions. All political participants are in a position to calculate (with at least some degree of accuracy) the reactions of other actors. Given this capacity, a group's actions often will not reveal its preferences, but rather its strategic calculations of what is the best that can be accomplished given existing circumstances. An actor's expressed policy preferences may in fact be "induced" or "strategic"—that is, they reflect accommodations to circumstances that constrain what can be achieved. And an actor's capacity to achieve its induced preferences should not necessarily be construed as a sign of great influence.

Consider Figure 3, which depicts the policy preferences of two political actors over a range of policy options stretching from A_0 to B_0. Actor A favors, in ascending order, those policies grouped toward the right end of the spectrum, while Actor B favors those at the left end. At any point, however, only a narrow subset of these alternatives is likely to be on the decision agenda. In one context, the conflict may be between options A_1 and A_2, while in another the choice is between options B_1 and B_2. Although the final choice between these two "viable" alternatives is the most visible part of the policymaking process, it is often not the crucial one. Rather, the most significant aspect of influence involves moving the decision-making agenda toward an actor's preferred end of the spectrum. A particular group may get its preferred outcome among those considered at this final stage, but only because it has recognized its weak position and made concessions that allow it to hold off a still-worse alternative. Thus, focusing only on the actual conflict between contending alternatives may give a very misleading impression of relative influence. For example, if Congress chose B_1 over B_2, we would be mistaken to conclude that Actor A has more influence than Actor B, even though Actor A obtained a preferred outcome within this particular subset. The crucial issue is to determine why the "viable" options were those at the right end of the spectrum, rather than why one of the options broadly favorable to Actor B was finally selected.

Fig. 3: Ranked Policy Preferences of Two Political Actors



For the most part, the debate over the role of business has failed to embrace this fundamental point. The admirable focus on the details of policy development necessarily draws attention to the particular choices made within a narrow range of options, rather than to the question of why only that particular subset was considered feasible. Not surprisingly, this focus also encourages confusion about what the historical record shows. In the fight between relatively narrow alternatives, there will be some victories for each side. Even the lines dividing the "sides" are likely to become ambiguous as different actors within groups take a variety of stances because of uncertainty or because they are situated somewhat differently. In the resulting confusion, both advocates and opponents of the business-power position will find evidence to support their claims.

Recognizing the significance of anticipated reactions makes the study of influence more difficult. The exertion of power often means convincing opponents that they cannot hope to accomplish their true goals, and must instead aim for something less ambitious. We need to determine whether a particular policy stance reflects a genuine preference or reluctant acquiescence in light of a weak political position.[43] We need to focus attention on agenda-setting and the identification of viable alternatives.[44] We need to know why the set of "feasible" initiatives comes to rest at a particular place–and we especially wish to know if this reflects indirect sources of influence. And we need to distinguish preferences from strategic courses of action that actors adopt to get as close as they can to their desired outcomes in a particular context. In all these inquiries, the long-term perspective encouraged by an emphasis on policy development can be invaluable.

Missing Obvious Comparisons

Particularly useful in making judgments about the distribution of influence, as well as about the relation between intentions and effects, are comparisons—comparisons over time, and comparisons across nations or other relevant political units. Policy historians, I have noted, too often overlook the obvious sources of analytic leverage that a long historical view permits. However, comparisons across nations (or across subnational political units, like states) are even less exploited. Much of the scholarship on social policy, to put it bluntly, is remarkably parochial.[45] While often justified by a concern with why the United States did not develop a "Western European-style welfare state," in the language of Margaret Weir, Ann Orloff, and Theda Skocpol, analysts usually engage with little more than a highly stylized conception of other countries' policies before delving into the specific of the U.S. case.[46] In the process, they often forfeit the chance to refine claims that could be easily examined in the context of other nations' experiences.

The reasons for this are not hard to fathom. The time and energy required to look into even a small aspect of U.S. social policymaking are formidable—which is why, one suspects, so few political scientists spend much time studying policy details. And the American welfare state often looks so exceptional in comparative relief that cross-national investigation seems unproductive or even misguided. The mistake, however, is to think that comparative inquiry necessarily has to involve entire systems of social provision in other nations—or even other nations at all. Rather, the goal should be to draw out the key implications of arguments made about U.S. social policy and then see if they hold in contexts different from the ones in which they were formulated. These alternative contexts can, and often should, be other nations. But they can also be policies at the state level, other national-level policies, and policy episodes at other times. In fact, they can even be hypothetical alternative scenarios—so-called counterfactuals—in which one or more factors deemed casually crucial are altered.[47]

Edwin Amenta's *Bold Relief* displays the benefits of these more limited comparative inquiries.[48] Amenta's thesis is that public-works programs at the national level were stymied by the emergence of a concerted conservative coalition, uniting antigovernment Republicans and southern Democrats intent on protecting the racial hierarchy and low-wage economy of southern states. To test this relatively familiar claim, however, he turns to traditionally untapped comparisons—looking, first, across the Atlantic to contrasting developments in Britain and then, within the United States, across state lines, in a careful comparative investigation of the implementation of work programs in southern and nonsouthern states. Through these internal and "shadow" comparisons, Amenta provides much stronger and richer support for his argument than he could otherwise, and he shows that full-scale cross-national comparative work is not necessary to overcome the well-known limitations of studies of a single nation or policy.

In my own work, I use a similar strategy to explore the effect of private social benefits on the political development of U.S. public social programs.[49] My thesis is that when employer-provided benefits become a core source of social protection for citizens, it is very difficult for government to assume responsibility in this area, both because the demand for government action becomes more muted and divided and because the providers and sponsors of private benefits become formidable opponents of government incursions onto their territory. My principal strategy for testing this claim is to compare policy developments between pension policy, where Social Security emerged before the widespread development of private pension plans, and health policy, where private health insurance covered a majority of Americans before public programs passed. Yet I also undertake more limited comparisons between the United States and Britain and Canada, to show how the particular development of private benefits in these nations did not pose as significant a hurdle to government action as it did in the United States, especially in the context of these nation's more facilitative political institutions.

Because studies of policy development are so deeply historical, scholars of policy history are often dismissed as "mere" storytellers or journalists. Yet it is precisely *because* of their long-term historical perspective that policy historians are uniquely situated to deal with the problems of causal complexity that plague all students of social life. This is so because the long historical view that policy historians typically adopt allows them to see and test relationships that more narrowly focused work too often misses—if, that is, they exploit the full power of their approach.

Failing to Historicize History

Perhaps the most common theme of recent works on the political development of U.S social policy is that contemporary debates have their roots in the past. Yet *why* the past is so important, and its effects so enduring, is much less clear. In some cases, the argument appears to be merely that past conflicts created present policies. In others, it seems deeper: that past policies have given rise to self-reinforcing dynamics that have helped push the American welfare state down highly resilient historical tracks. This does not, of course, exhaust the possibilities. In some cases, the claim is not about endurance but fragility—for example, the relative political *weakness* of the work-oriented focus of the New Deal relief policies highlighted by Amenta. But what is at stake in all these claims is the place of history, if you will, in policy history. Why must we take the long view in analyses of the American welfare state? Why are some policies resilient, while others are not? What explains continuity and change within specific policies? And how do policies reshape political life after they are enacted?

The deepest failure of social welfare history to date is its inability or unwillingness to engage with these seminal issues. This failure is all the more glaring because, in the last decade, mainly because of the pathbreaking scholarship of Harvard political scientists Paul Pierson and Theda Skocpol, a relatively powerful set of generalizations has emerged about the effect of public policies, once implemented, on political dynamics going forward.[50] This notion of "policy feedback" has built upon and furthered a related theoretical enterprise in the social sciences: the exploration of processes of "path dependence" in which early developments structure later ones by giving rise to institutions and dynamics that are inherently difficult to reverse. For the most part, however, policy historians have not engaged with these theoretical currents, and in the rare cases when they are invoked, they are usually treated quite superficially.

As Pierson argues in a 1993 article, the idea that "policies make politics" has a distinguished lineage in political science. Yet much of the writing on the subject, including recent works that use the term, remains extremely vague about *why* and *how* policies remake politics. Pierson's main contribution in his 1993 article was to flesh out the meaning of policy feedback, identifying three main categories of feedback effects: consequences for governing elites, effects on interest groups, and impacts on the mass public. In subsequent writings, Pierson has deepened this framework by linking it to the concept of "path dependence." The long-term feedback effects of policies, he argues, are a powerful example of a so-called path-dependent social process. By pushing policymaking down enduring, self-reinforcing tracks, early policy choices—for example, the creation of a pension system reliant on future workers' contributions to meet its benefit obligations and sustain its finances—can shape what elites consider viable and possible, what interest groups demand (and whether they form at all), and how the public thinks about state activity and responds to competing policy alternatives.[51]

It is no coincidence that Pierson formulated his argument in the context of his work on welfare state retrenchment, *Dismantling* *the Welfare State*, for social welfare policies are probably among the most likely to produce long-term feedback effects.[52] As I argue in *The Divided Welfare State*, social policies generally entail the creation of large-scale institutions with substantial set-up costs, benefit sizable and resourceful organized groups of citizens, and create long-lived expectations on which individuals premise crucial life and organizational decisions—each factors that increase the chance that early policy choices will be consequential down the line. Importantly, however, not only government programs share these characteristics. Many private social benefits, such as employer-provided pensions and health insurance, have similar features. In fact, because these benefits are generally less visible and their development in response to subsidy and regulatory policies harder to predict, they are in some respects more prone to path dependence. Early choices taken with little foresight or in pursuit of goals quite different from those eventually achieved can create powerful organizations and vested interests that subsequently become important players on the political landscape.[53]

Not all social policies share these features, of course. Social Security pensions, for example, have promoted widespread mobilization among the aged, who are well poised to fight cuts. Their generosity, lack of stigma, visibility, and receipt by a large and easily identifiable group—all, as Andrea Campbell has argued in a recent important book, have encouraged the formation of an extremely powerful support coalition poised to protect and enlarge the program.[54] Cash assistance for the poor, in contrast, has given rise to an extremely weak, fragmented, and politically demobilized constituency, which was unable to present an effective and united front against the 1996 welfare-reform law.[55] Antipoverty benefits are ungenerous, they carry significant stigma, and they are received by a fluctuating population of citizens who, in general, have meager political capacity. Little wonder, then, that the main barrier to the alteration of welfare benefits has not been resistance from beneficiaries but lack of agreement among would-be reformers.[56]

A common criticism leveled against path-dependence claims is that they are overly static. Yet the concept presents a vision that is far from static. Rather, the crux of the claim is that path-dependent processes make some courses of action more likely and others less likely. Options that were once feasible no longer are, but other options may well *emerge* as a result of past policy choices. Indeed, the next frontier for research on policy feedback and path dependence is to specify more precisely when policies are highly resilient to change and when they are not, and in what ways previous policies will influence future policy choices when policies are in fact open to revision.

Happily, this appears to be the direction in which institutionally minded political scientists are heading. Pierson, for example, has written a new book, *Politics in Time,* that lays out his own arguments about how path dependence creates change as well as continuity.[57] Kathleen Thelen and Eric Schickler, in very different ways, have pushed forward the study of institutional development by tracing the evolution of German labor-market institutions and the U.S. Congress, respectively.[58] In my own recent writings, I have argued for a fourfold model of policy change that builds on the work of these authors.[59] Once policies with strong support coalitions are in place, I argue, "big bangs" of policy reform or replacement are rare, requiring as they do a consolidation of political power that most nation's political institutions, but particularly the United States's, make difficult. Nonetheless, even without epochal transformations, social policies may change markedly through three less studied processes, as outlined in Figure 4. The first is what Thelen calls "conversion"—the internal transformation of policies without formal policy change. In programs run by private organizations or frontline caseworkers, there are numerous opportunities to reorient programs without going through the normal legislative process, and many of the most consequential changes in U.S. social policy over the past two decades—such cutbacks in antipoverty benefits and the decline and restructuring of tax-subsidized workplace benefits—have occurred through such conversion processes.

The second process of change that occurs without formal transformation of the existing program is what Schickler terms "layering," the creation of new policies that can alter the operation of older policies. "Layering" requires legislative action, but it does not require dismantling older programs—a far more difficult prospect. Thus, for example, conservative critics of Social Security have been consistently rebuffed in their effort to scale back the program significantly, but they have succeeded handsomely at capitalizing on periods of conservative ascendance to enact new policies encouraging highly individualized tax-subsidized retirement accounts, like 401(k) pension plans.

The third process of change is perhaps the least recognized, and often the most important—what I call "drift" within the bounds of

Fig. 4: Four Modes of Policy Change

Barriers to Authoritative Policy Change	**Barriers to Internal Policy Conversion:** High (Low levels of policy discretion, strong policy support coalitions)	**Barriers to Internal Policy Conversion:** Low (High levels of discretion, weak support coalitions)
High (Many veto players)	Drift (Transformation of stable policy due to changing circumstances) Illustrative Example: Erosion of Scope of Protection of Existing Public Social Programs and Private Benefits	Conversion (Internal adaptation of existing policy) Illustrative Example: Employers' Restructuring of Publicly Subsidized Voluntary Workplace Benefits
Low (Few veto players)	Layering (Creation of new policy without elimination of old) Illustrative Example: Creation and Expansion of Tax Subsidies for Private Retirement Accounts	Revision (Formal reform, replacement, or elimination of existing policy) Illustrative Example: 1996 Welfare Reform

formally stable policies. Drift occurs when changes in the environment of policies make them less capable of achieving their initial goals, but the policies are not updated, either because the gap between goals and reality is not recognized or, more interesting still, is recognized, but there is active opposition to the updating of policies to reflect their context.[60] In the past three decades, the employment market and structure of families have changed dramatically. Yet most of the American welfare state has not. The result is a growing gulf between the new social risks that citizens face and the existing framework of social benefits on which they depend. This gulf is no accident: Opponents of the welfare state have faced great difficulties in cutting it back. But they have proved extremely capable of blocking the updating of social policies to reflect changing social realities—as they did, for example, when they decisively defeated President Clinton's ill-fated 1993 health plan.[61]

Although the issue of institutional development is moving to the center of the debate in political science, it is certainly premature to declare that robust generalizations about processes of institutional change are destined to sift into studies of policy history, much less that generating arguments of this sort will become a primary concern of social welfare historians. Yet for social policy historians to ignore these emerging issues would be to pass up a tremendous opportunity not only to learn from work in other fields but also to inform it, as well as to develop a set of explanatory tools that could create greater cohesion and clarity in a field that, for all its richness and depth, still needs both.

The Promise of a Policy-Development Approach

Once, the story of American welfare-state building was clear and agreed upon. The United States scarcely had a welfare state at all. The social policies enacted in many European nations had foundered on the shoals of America's antigovernment heritage. And what programs had emerged were the product of two great spasms of twentieth-century liberal achievement: the New Deal and the Great Society—the first of which challenged the privileged position of business in the American political economy; the second of which attacked the structures of hierarchy that had subjugated women and blacks, denying them an equal place in American civic and social life.

The last decade of research has upended this traditional master narrative. The racially biased aspects of the welfare state did not just vanish; they remain part and parcel of the language, and the dilemmas, of the welfare state today. Women were not just subjects of the welfare state; they helped construct it, and in distinctive ways. Business was not always monolithically opposed to social programs. Plenty of welfare-state building was done during the calm eddies of social welfare history—before, after, and between the big bangs. And antigovernment values were never as fixed or as determinative as once suggested. A "weak" state sometimes made "strong" policies. Above all, it made policies that were not simply a pale imitation of European experience but often were singularly distinctive in their justification, structure, and effects.

Yet while the traditional linear story of hampered yet inexorable progress is no longer tenable, we should not pretend that the loss of that larger narrative has come without cost. In this case, the cost has been a fragmentation of the field, a proliferation of nonintersecting claims, and an eschewal of the broader perspective that once cast the welfare state as a window into bigger scholarly questions and vaster historical tides.

Just as widely distributed social policies always resist undoing, there can be no going back to the less complicated social welfare history of the past. Nor should we. As the scholarship reviewed in this article shows, the gains in knowledge are breathtaking. The study of the American welfare state is now among the most vibrant, broad-ranging, sophisticated, and engaging areas of history and political science—and one in which an unusually diverse range of disciplines and perspectives inform one another. But in any field, there are times for pushing forward in multiple directions regardless of the resulting fragmentation, and there are times for bringing together the insights thus won to construct a more compelling overall picture. The contention of this article is that we have reached the second of these times, and serious effort should be made to integrate the fruits of the multipronged, creative, and amazingly productive research endeavors of the last decade.

Where should such an effort begin? At the beginning, so to speak—with the foundational issue of what the American welfare state looks like and why. In responding to this enduring puzzle, four insights of recent scholarship will need to be front and center: the pivotal role of the United States's fragmented and decentralized political institutions, the special character and place of race in American social life, the heavy U.S. reliance on hidden or indirect policy tools, and the distinctive prominence of business in the making (and perhaps unmaking) of the American welfare state. Our aim, in bringing these insights to bear, should be to move beyond black-and-white portraits of state weakness or state strength. Scholars should seek to uncover the fault lines in even the strongest exercises of state power and the hidden wellsprings of power in even the seemingly weakest policy interventions. The goal should also be to move beyond the focus on "leading" policy issues and debates—those prominent moments of contention and change about which so much is written.[62] Instead, scholars should trace the unfolding historical development of specific policies across long periods of attention and inattention, action and inaction. And finally, in doing so, the aspiration should be to construct general claims about U.S. social policymaking that are formulated and examined in the context of explicit comparisons across time and space, whether the comparative contrast be other nations, other policies, other times, or all three.

In all this, our eyes should remain fixed on the ultimate goal: to understand not merely the details of social welfare policies, but what they do—to and for citizens and to and for polities and societies. The welfare state expresses, at root, a sense of solidarity, a belief in shared fate. At this moment, when our fates seem shared more in fear than in hope, the link between policies and the collective commitments that they reflect and nurture is as vital a subject for American politics and society as it is for policy historians.

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Notes

1. Alice Kessler-Harris, *In Pursuit of Equity: Women, Men, and the Quest for Economic Citizenship in 20th-Century America* (Oxford, 2001); Michael B. Katz, *The Price of Citizenship: Redefining the American Welfare State* (New York, 2001); Robert Lieberman, *Shifting the Color Line* (Cambridge, Mass., 1998); Peter Swenson, *Capitalists Against Markets: The Making of Labor Markets and Welfare States in the United States and Sweden* (New York, 2002).

2. Paul Pierson, *Dismantling the Welfare State? Reagan, Thatcher, and the Politics of Retrenchment* (New York, 1994); Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, Mass., 1992).

3. Edward D. Berkowitz, *America's Welfare State: From Roosevelt to Reagan* (Baltimore, 1991), xi.

4. Gøsta Esping-Andersen, *The Three Worlds of Welfare Capitalism* (Princeton, 1990).

5. Gunnar Myrdal, *An American Dilemma* (New York, 1944).

6. Lieberman, *Shifting the Color Line;* Jill S. Quadagno, *The Color of Welfare: How Racism Undermined the War on Poverty* (New York, 1994); Linda Faye Williams, *The Constraint of Race: Legacies of White Skin Privilege in America* (University Park, Pa., 2003); Gareth Davies and Martha Derthick, "Race and Social Welfare Policy: The Social Security Act of 1935," *Political Science Quarterly* 112, no. 2 (1997); Michael K. Brown, *Race, Money, and the American Welfare State* (Ithaca, 1999).

7. Brown, *Race, Money, and the American Welfare State,* 3.

8. Martin Gilens, *Why Americans Hate Welfare: Race, Media, and the Politics of Antipoverty Policy* (Chicago, 1999).

9. Davies and Derthick, "Race and Social Welfare Policy: The Social Security Act of 1935."

10. Lieberman, *Shifting the Color Line.*

11. Esping-Andersen, *The Three Worlds of Welfare Capitalism.*

12. See, for example, Gwendolyn Mink, *The Wages of Motherhood: Inequality in the Welfare State, 1917–1942* (Ithaca, 1995); Kessler-Harris, *In Pursuit of Equity;* Linda Gordon, *Pitied but Not Entitled: Single Mothers and the History of Welfare, 1890–1935* (New York, 1994); Skocpol, *Protecting Soldiers and Mothers;* Suzanne B. Mettler, *Dividing Citizens: Gender and Federalism in New Deal Public Policy* (Ithaca, 1998).

13. Skocpol, *Protecting Soldiers and Mothers.*

14. Linda Gordon, ed., *Women, the State, and Welfare* (Madison, 1990).

15. Kessler-Harris, *In Pursuit of Equity;* Mettler, *Dividing Citizens;* Mink, *The Wages of Motherhood.*

16. See Ann Shola Orloff, "Gender and the Social Rights Citizenship: The Comparative Analysis of Gender Relations and Welfare States," *American Sociological Review* 58 (1993); Dorothy McBride Stetson and Amy G. Mazur, eds., *Comparative State Feminism* (Thousand Oaks, Calif., 1995).

17. Joe Soss and Suzanne Mettler, "Beyond Representation: Policy Feedback and the Political Roots of Citizenship," paper presented at the Annual Meeting of the Midwest Political Science Association, Chicago, 2003.

18. See, in particular, Isabela Mares, *The Politics of Social Risk: Business and Welfare State Development* (New York, 2003); Colin Gordon, *New Deals: Business, Labor, and Politics in America, 1920–1935* (Cambridge, 1994); idem, *Dead on Arrival: The Politics of Health Care in Twentieth-Century America* (Princeton, 2003); Cathie Jo Martin, *Stuck in Neutral: Business and the Politics of Human Capital Investment Policy* (Princeton, 2000); Swenson, *Capitalists Against Markets;* Sanford M. Jacoby, *Modern Manors: Welfare Capitalism Since the New Deal* (Princeton, 1997). An important spur for much of this work is an emerging literature on "varieties of capitalism." This work argues that capitalism comes in at least two alternative forms: it may be oriented around the short-term, hyper-competitive, and based on arms-length contracts (the American, or "liberal market economy," model), or it may be long-term, consensual, and based on interlocking financial and social ties (the Continental European, "coordinated market economy," model). And while social welfare policies that strengthen workers' autonomy and power might interfere with the normal competitive market in the first model, they may be highly market-enhancing in the latter. For example, in an economy based on high skills and wages, protecting workers against the risk of occupational displacement encourages workers to invest in skills that are very specific to an industry or firm—skills they would otherwise fear investing in, because of their lack of transferability from job to job. Peter A. Hall and David Soskice, eds., *Varieties of Capitalism: The Institutional Foundation of Comparative Advantage* (New York, 2001); Torben Iversen and David Soskice, "An Asset Theory of Social Policy Preferences," *American Political Science Review* 95, no. 4 (2001).

19. The main exponent of this view is Peter Swenson, whose work I will discuss shortly.

20. Isabela Mares, "Negotiated Risks: Employers' Role in Social Policy Development" (Ph.D. diss., Harvard University, 1998); Gordon, *New Deals* and *Dead on Arrival*.

21. It might be supposed that this is the same as saying that social policies are economically beneficial; yet it is not. Many policies that are good for economic growth have no organized defenders. Moreover, the fact that certain policies benefit business leaves open the critical *historical* question of whether capitalists were behind their creation. The powerful, and controversial, claim of the new business-power literature is that capitalists have a first-choice preference for key social programs *before* they are enacted.

22. Swenson, *Capitalists Against Markets*.

23. On the distinction between structural and instrumental power, see Charles E. Lindblom, "The Market as Prison," *Journal of Politics* 44, no. 2 (1982); Jacob Hacker and Paul Pierson, "Business Power and Social Policy: Employers and the Formation of the American Welfare State," *Politics and Society* 30, no. 2 (2002).

24. Swenson, *Capitalists Against Markets*.

25. G. William Domhoff, *Who Rules America?* (Englewood Cliffs, N.J., 1967); Gabriel Kolko, *The Triumph of Conservatism* (New York, 1977).

26. On the limits of a historical "snapshot" rather than a "moving picture," see Paul Pierson, *Politics in Time* (Princeton, forthcoming).

27. On the power conferred on business by America's decentralized federalism before the New Deal, see David Brian Robertson, "The Bias of American

Federalism: The Limits of Welfare State Development in the Progressive Era," *Journal of Policy History* 1, no. 3 (1989). The argument about the decline in business power during the New Deal is elaborated in Hacker and Pierson, "Business Power and Social Policy."

28. The term is from Jacob S. Hacker, *The Divided Welfare State: The Battle over Public and Private Social Benefits in the United States* (Cambridge, 2002).

29. Richard M. Titmuss, *Essays on "the Welfare State,"* 3d ed. (London, 1976).

30. Christopher Howard, *The Hidden Welfare State: Tax Expenditures and Social Policy in the United States*, Princeton Studies in American Politics (Princeton, 1997).

31. Robert A. Kagan, *Adversarial Legalism: The American Way of Law* (Cambridge, Mass., 2001).

32. Ibid. See also Thomas F. Burke, *Lawyers, Lawsuits, and Legal Rights: The Battle over Litigation in American Society* (Berkeley and Los Angeles, 2002).

33. Katz, *The Price of Citizenship*.

34. Gordon, *Dead on Arrival;* Jacoby, *Modern Manors;* Jennifer L. Klein, *For All These Rights: Business, Labor, and the Shaping of America's Public-Private Welfare State* (Princeton, 2003). For the more traditional narrative, see Stuart D. Brandes, *American Welfare Capitalism, 1880–1940* (Chicago, 1970).

35. Michael K. Brown, "Bargaining for Social Rights: Unions and the Reemergence of Welfare Capitalism, 1945–1952," *Political Science Quarterly* 112, no. 4 (1997–98); Marie Gottschalk, *The Shadow Welfare State: Labor, Business, and the Politics of Welfare in the United States* (Ithaca, 2000); Hacker, *The Divided Welfare State;* Beth Stevens, "Labor Unions, Employee Benefits, and the Privatization of the American Welfare State," *Journal of Policy History* 2, no. 3 (1990).

36. Hacker, *The Divided Welfare State*.

37. Christopher Howard, "Is the American Welfare State Unusually Small?" *PS: Political Science and Politics* 36, no. 3 (2003): 415.

38. Hacker, *The Divided Welfare State*.

39. Jacob S. Hacker, "Privatizing Risk without Privatizing the Welfare State: The Hidden Politics of Social Policy Retrenchment in the United States," *American Political Science Review* (forthcoming).

40. On the obstacles to scholarly progress that conceptual disagreement creates, see Paul Pierson, "Coping with Permanent Austerity: Welfare State Restructuring in Affluent Democracies," in *New Politics of the Welfare State*, ed. Paul Pierson (New York, 2001), 421.

41. At the extreme, they simply assume that the effects *are* the motives, as summed up in the influential statement of the economist George Stigler that the "announced goals of a policy are sometimes unrelated or perversely related to its actual effects, and the *truly intended effects should be deduced from the actual effects*." George J. Stigler, *The Citizen and the State* (Chicago, 1975), 140.

42. This argument is elaborated in Paul Pierson, "The Limits of Design: Explaining Institutional Origins and Change," *Governance: An International Journal of Policy and Administration* 13, no. 4 (2000). To be sure, political scientists have long recognized the "law of unintended consequences." But it is fair to say that they have shown little inclination to come up with systematic arguments about when and why we should expect unanticipated effects. Nonetheless, based on what we know about political action, it is possible to identify at least two critical factors that are may influence the likelihood of unforeseen consequences. First, unintended effects are most likely when policies are highly complex—affecting many different dimensions of society simultaneously—for in these circumstances the limits of humans' ability to calculate multiple and interactive effects and the possibility of emergent "system effects" looms large. Robert Jervis, "Complexity and the Analysis of Political and Social Life," *Political Science Quarterly* 112, no. 4

(1997–98). Second, they are likely when policymakers have focused on the short-term (to use economics lingo, their "time horizons" are short), for in these circumstances actors are likely to pay little attention to the potential long-term or interactive elements of their policies. Needless to say, social welfare policies are often characterized by the first feature, and in many cases, especially when passed to respond to pressing social needs, the second as well.

43. One of the benefits of a substantial and intensively investigated archival record is that it makes it possible to trace the strategic behavior of actors over time. In many cases, one can actually document private acknowledgments that public positions represent a strategic accommodation to political realities.

44. John W. Kingdon, *Agendas, Alternatives, and Public Policies*, 2d ed. (New York, 1995); Frank R. Baumgartner and Bryan D. Jones, *Agendas and Instability in American Politics* (Chicago, 1993).

45. A case in point is Derthick's brilliant and highly influential study of the development of Social Security in the United States. Derthick's argument is that the advocacy role of administrators and the closed circle of participation in the program's development are the prime reasons for its explosive growth. Yet she does not test her argument against the development of social insurance programs in other nations—which would show that the expansionary tendencies of the American system were scarcely unique and suggest that broader social forces were at least as powerful spurs to expansion as the specific characteristics of the American policymaking environment. Martha Derthick, *Policymaking for Social Security* (Washington, D.C., 1979).

46. Margaret Weir, Ann Shola Orloff, and Theda Skocpol, "Introduction: Understanding American Social Policies," in *The Politics of Social Policy in the United States* (Princeton, 1988), 9.

47. The logic of counterfactuals, and its relation to historical and cross-national comparison, is discussed further in Jacob S. Hacker, "Learning from Defeat? Political Analysis and the Failure of Health Care Reform in the United States," *British Journal of Political Science* 30 (2000). See also James D. Fearon, "Couterfactuals and Hypothesis Testing in Political Sciences," *World Politics* 43, no. 2 (1991).

48. Edwin Amenta, *Bold Relief: Institutional Politics and the Origins of Modern American Social Policy* (Princeton, 1998); Edwin Amenta, "Making the Most of a Case Study: Theories of the Welfare State and the American Experience," *International Journal of Comparative Sociology* 32, no. 1–2 (1991).

49. Hacker, *The Divided Welfare State*.

50. Skocpol, *Protecting Soldiers and Mothers;* Pierson, *Dismantling the Welfare State?;* Paul Pierson, "When Effect Becomes Cause: Policy Feedback and Political Change," *World Politics* 45, no. 4 (1993); idem, "Increasing Returns, Path Dependence, and the Study of Politics," *American Political Science Review* 94, no. 2 (2000).

51. Pierson, "When Effect Becomes Cause: Policy Feedback and Political Change"; idem, "Not Just What, but *When:* Timing and Sequence in Political Processes," *Studies in American Political Development* 14, no. 1 (2000).

52. Pierson, *Dismantling the Welfare State?*

53. Hacker, *The Divided Welfare State*, 52–58.

54. Andrea Louise Campbell, *How Policies Make Citizens: Senior Political Activism and the American Welfare State* (Princeton, 2003).

55. R. Kent Weaver, *Ending Welfare as We Know It* (Washington, D.C., 2000); Joe Soss, "Lessons of Welfare: Policy Design, Political Learning, and Political Action," *American Political Science Review* 93, no. 2 (1999).

56. Steven M. Teles, *Whose Welfare? AFDC and Elite Politics* (Lawrence, Kan., 1996).

57. Pierson, *Politics in Time*.

58. Kathleen Thelen, "Historical Institutionalism in Comparative Politics," *Annual Review of Political Science* 2 (1999); Eric Schickler, *Disjointed Pluralism: Institutional Innovation in the U.S. Congress* (Princeton, 2001).

59. Hacker, "Privatizing Risk without Privatizing the Welfare State."

60. Of course, drift can and does run in the opposite direction—that is, toward expansion. The proliferating use of disability insurance as a means of early retirement in Europe is a powerful contemporary example.

61. Jacob S. Hacker, *The Road to Nowhere: The Genesis of President Clinton's Plan for Health Security* (Princeton, 1997); Theda Skocpol, *Boomerang: Clinton's Health Security Effort and the Turn against Government in U.S. Politics* (New York, 1996).

62. As Derthick writes, in a statement as true today as when she penned it, "Much of the scholarly literature that analyzes policymaking focused on 'leading' or controversial cases—moments of crisis or innovation that are intrinsically interesting and undoubtedly important, but not in themselves typical. Policymaking is a compound of exciting innovative events . . . and not-so-exciting routines that are performed without much widespread mobilization, intense conflict, or much awareness of what is going on except among the involved few." Derthick, *Policymaking for Social Security,* 9.