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CRITICAL-RETROSPECTIVE ESSAYS

Reconnecting the Political and the Economic in the New Gilded Age

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Affluence and Influence: Economic Inequality and Political Power in America, by **Martin Gilens.** Princeton, NJ: Princeton University Press, 2012. 352 pp. $24.95 paper. ISBN: 9780691162423.

Winner-Take-All Politics: How Washington Made the Rich Richer—and Turned Its Back on the Middle Class, by **Jacob S. Hacker** and **Paul Pierson.** New York: Simon and Shuster, 2010. 357 pp. $16.00 paper. ISBN: 9781416588702.

Capitalizing on Crisis: The Political Origins of the Rise of Finance, by **Greta R. Krippner.** Cambridge, MA: Harvard University Press, 2011. 222 pp. $21.50 paper. ISBN: 9780674066199.

The Undeserving Rich: American Beliefs about Inequality, Opportunity, and Redistribution, by **Leslie McCall.** New York: Cambridge University Press, 2013. 300 pp. $29.99 paper. ISBN: 9781107699823.

Polarized America: The Dance of Ideology and Unequal Riches, by **Nolan McCarty, Keith T. Poole,** and **Howard Rosenthal.** Cambridge, MA: MIT Press, 2006. 240 pp. $21.00 paper. ISBN: 9780262633611.

The Fracturing of the American Corporate Elite, by **Mark Mizruchi.** Cambridge, MA: Harvard University Press, 2013. 363 pp. $35.00 cloth. ISBN: 9780674072992.

Who Cares? Public Ambivalence and Government Activism from the New Deal to the Second Gilded Age, by **Kathryn Newman** and **Elisabeth Jacobs.** Princeton, NJ: Princeton University Press, 2010. 219 pp. $30.95 cloth. ISBN: 9780691135632.

The Land of Too Much: American Abundance and the Paradox of Poverty, by **Monica Prasad.** Cambridge, MA: Harvard University Press, 2012. 327 pp. $39.95 cloth. ISBN: 9780674066526.

Oligarchy, by **Jeffrey Winters.** New York: Cambridge University Press, 2011. 323 pp. $29.99 paper. ISBN: 9780521182980.

The social science literature on the fraught relationship between democracy and capitalism has ebbed and flowed over the years. A somewhat stylized reading of the recent history of political sociology and its political science cousin, American Political Development (APD), goes something like this: At almost the very moment when income and wealth inequality in the United States and elsewhere began its inexorable rise in the late 1970s, a new and influential theoretical stance rejecting a dominant position for capitalists and/or capitalism in the making of public policy and political change rapidly gained influence. In a landmark essay published in *Politics and Society* in 1980, Theda Skocpol announced a new research program aimed at ‘‘bringing the state back in'' (Skocpol 1980; see also Evans, Rueschemeyer, and Skocpol 1985). Building upon the sharply crafted theoretical prolegomenon in the first chapter of her 1979 award-winning book, *States and Social Revolutions*, Skocpol established a distinction between "state-centered" and "society-centered" explanations in studying political change. The state-centered model proposed to conceive of "state managers" as political actors endowed with a significant measure of autonomy and power in the making of social policy. Further, the principle constraints on egalitarian social policies came not from business elites from above nor via public opinion from the masses from below, and also not from the looming macroeconomic environment imposed by a capitalist economic system. Rather, the salient constraints were imposed by the different institutional configurations that defined national polities (with U.S. political institutions being particularly poorly suited to produce a generous welfare state).

A primary target of the new institutionalism in political sociology was the variant of neo-Marxist political sociology that placed democratic capitalism in its cross-hairs and had gained considerable influence in the 1970s (e.g., O'Connor 1973; Poulantzas 1978; Przeworski 1985; Block 1987); a secondary target was the kind of radical elite theory associated with C. Wright Mills (1956) and his intellectual heirs (especially Domhoff 1967, 2012). For both neo-Marxists and Millsian theorists, the central challenge had been to build a theory of the capitalist state in which the interlocking of the political and the economic could be theorized in a unified way. The neo-Marxists rooted the question in structural features of a capitalist political economy, in which state actors (of whatever partisan or political orientation) were ultimately dependent on a healthy investment environment to stimulate economic growth or face the risks of economic crisis. By contrast, the Millsians argued that the key to understanding the linkage was the dominance of economic (and other) elites in top positions inside the state and their ability to shape and influence public discourse and control the policy agenda.

The new institutionalism was often accompanied by theoretical claims that appeared to flatly reject the view that structural economic factors or class forces from above (however constructed by either Marxists or Millsians) had much influence on the making of public policy (Skocpol, Weir, and Orloff 1988; Skocpol 1992; Orloff 1993; Amenta 1998). Under the label of "historical institutionalism," scholarship from the 1990s suggested that divergent national political pathways could be seen as having deep historical and structural components that were not easy to dislodge (e.g., Steinmo, Thelen, and Longstreth 1992). Timing and sequencing of policy and political contests were viewed as critical factors shaping later outcomes (Pierson 1993, 2000). For example, the fact that universal (white) male suffrage was embedded in the American Constitution but not in those of other countries played a key role in explaining why socialist movements (which often first unified the working class around demands for suffrage) appeared in other countries but not in the United States (Shefter 1994). The general idea was that the timing and sequence of policy developments created a kind of "path dependence" as a critical source of constraint. In this way, policies adopted at one point in time set limits on what is possible or can even be taken seriously at later points (Pierson 1993; Hacker 2002; Thelen 2004). Accounting for why the Swedish, German, and American welfare states, for example, could look so different by the end of the twentieth century becomes a matter of specifying the political-institutional constraints in each case and showing how they operated at key historical junctures.

The coming of institutional analysis to political sociology was intellectually powerful in a number of important ways. It pushed political sociologists to develop better theoretical understandings of how institutions matter and brought into the subfield insights from organizational theory and neo-institutionalism that had previously been at the margins. In the hands of a second generation of political sociologists and political scientists working in the neo-institutional vein, ever-more sophisticated ideas about institutional constraints were advanced. For example, the concepts of layering and drift provided ways of understanding how institutions change over time (Mahoney and Thelen 2010). Layering and drift can occur within institutions in a number of different ways: for example, the external environment can shift in ways that lead to internal change; or during implementation processes, regulatory bodies are sometimes given vague guidelines and layer rules upon rules that push in unanticipated directions (e.g., Hacker 2002; Sheingate 2010).

But the influence and contributions of institutional approaches came at some intellectual cost. At one level, the tension is a classic one: how we weight "structure" versus "agency." Political institutionalists sometimes end up in tautological positions where features of institutions are themselves the cause of institutional change. But there was a broader issue, one that has become more apparent in recent years. For many commentators, the institutionalist movement seemed to sever political sociology from its roots in the Marx-Weber tradition, where the very foundations for a sociology of politics arose in the first place: traditional political science tended to downplay or ignore altogether the social and class forces in the background of political life in highlighting the autonomous institutional power of the political.

A dramatic shift in the intellectual orientation of many political sociologists and APD scholars has taken place in the past decade, one that suggests a reengagement with the classic roots of the subfield. The motivating dynamic was the need to better understand the political roots of the dramatic increase in income and wealth at the very top. The explanations popular among economists, who sought to explain rising inequality by pointing to non-political factors such as globalization and skill-biased technological change (SBTC), are generally more helpful in explaining the relatively modest shifts in the income shares received by households at, say, the 80th percentile versus the 20th percentile. But the mechanisms critical to understanding the much larger contribution to growing inequality made by the rapid rise of the top one percent (and the top .01 percent) must include a political component. It is, therefore, a natural question for political sociologists, and in the past decade many have indeed turned to developing better understandings of how the politics of capitalist democracies link to and contribute to rising economic inequality. In this essay, I endeavor to chart some of the parameters of this movement and highlight its unresolved questions.

How real is this shift within the field? We can point immediately to a key example in the work of Jacob Hacker and Paul Pierson, two of the most influential and productive APD scholars. Although their 2010 book *Winner-Take-All Politics* did not break extensive new ground, it is an important marker of just how far the discussion within political sociology had advanced. Hacker and Pierson highlight the role of business activism through lobbying, campaign contributions, and influence on the Republican Party. They persuasively criticize the economic literature that has focused on the rise of SBTC for its neglect of the role of politics. Their analysis starts with a critical historical observation that much of the recent discussion of rising inequality (at least prior to Thomas Piketty's landmark historical analysis) has overlooked: what happened in the 1980s and onward is very much a return to the normal state of affairs, not a huge deviation. The period of declining inequality between World War II and the 1970s stands out as the exception, not the norm. It is somewhat startling to see a chapter entitled "A Brief History of Democratic Capitalism" tracing the parallels between the 1920s and the 2000s that combines a historical class analysis with an insightful treatment of how U.S. institutions facilitated the privileged position of finance and income and wealth concentration at the top in both periods. A more accurate title for the book might have been *The Return of Winner-Take-All Politics*.

Hacker and Pierson include evidence drawn from many ideas associated with the Millsian tradition but disciplined by a sustained focus on the transformation of the Republican Party as the primary organizational vehicle for promoting inegalitarian policies (or fighting redistributive ones). The political mobilization of business in the 1970s receives extended treatment, including conservatives' efforts in ways both direct (via lobbying, attacks on unions, etc.) and indirect (through financing political ideas). The critical changes in tax policy and deregulation and the explosive growth of the financial sector that have facilitated the high-inequality regime are viewed as the result of the success of these interventions. In other words, they hinge in large measure on organizational—or what might also be called "class"—disparities in political power as unions and anti-poverty organizations diminish in strength while business elites gain ground everywhere. While American political institutions may have facilitated these outcomes, their achievement required conscious and explicit class action over the past four decades.

Winner-Take-All Politics is written for a broad audience and filled with telling anecdotes that make it a pleasure to read. Keeping in mind the book's ambition to speak to a general audience, the book is not usefully criticized as a full scholarly treatment; and there are many points where one would want further elaboration or evidence before accepting the book's core claims. One important question concerns the authors' analysis of public opinion. Hacker and Pierson argue that because the public has not moved rightward (based on conclusions drawn from a handful of repeated survey items examined more fully elsewhere; see Hacker and Pierson 2005), it has essentially played no role in the rise of political inequality. This assertion is at best only half right, as others have shown and as I will discuss in more detail below. And the full contours of the business offensive since the 1970s have been traced out in much more detail in the valuable work of business historians, most notably by Kim Phillips-Fein (2009) and Benjamin Waterhouse (2014). Sociologist Edward Walker (2014) has analyzed the increasingly sophisticated ''astroturf'' organizations developed by corporations to mimic civic participation in support of corporate objectives, while Isaac Martin (2008, 2013) has traced the origins and development of broad-based anti-tax and anti-government social movements that have supported public policies in the interest of the richest Americans. Anyone wanting a broader understanding of the impact of the corporate mobilization would benefit from the latter works.

Democratic Capitalism in the Age of Finance

If Hacker and Pierson's focus on the political class struggle represents one angle on democratic capitalism, investigations of the social and political underpinnings of the rising importance of finance represent another. One of the hallmarks of high-inequality regimes is the importance of financial profits and unearned income flowing to the top; this was true of the 1920s, and it is only slightly less true of the current era (especially if we focus on the top .01 percent, not the full one percent). The two most exciting contributions addressing this question are Greta Krippner's *Capitalizing on Crisis* and Monica Prasad's *The Land of Too Much*. Both books have received the American Sociological Association's annual distinguished book prize, and there are good reasons why. Both books offer magisterial arguments centering on the importance of the politics of finance in understanding political and economic American political development. Both books remind us of the power of viewing the present from a historical perspective, helping to restore faith in historically oriented political sociology in a period when scholarly tastes are moving in very different directions.

Prasad's sweeping revisionist account starts from the underlying motivations for the populist demand for expanded credit (''free silver''): the richness and vastness of farm land (''the land of too much'') and world-dominant agricultural productivity created tendencies for over-production and low crop prices, making it difficult or impossible for farmers to finance their debts under the constraints of the gold standard. Prasad views the agrarians and the populist wing of the Progressive movement as politically powerful until the time of the New Deal. Farmers wanted credit, and their successes extended to railroad regulation and progressive income taxes (but not, importantly, consumption taxes of the type that were being established in Europe at the same time). During the 1930s, the New Deal continued this pattern of expanding credit (for example, in the creation of the Federal Housing Administration, what Prasad refers to as ''mortgage Keynesianism'') as a way of attempting to stimulate economic activity. Strong banking regulations, always favored by the populists, were among the earliest New Deal measures. In agriculture, the introduction of new crop restrictions and income subsidy programs attempted to stabilize farming income. In Prasad's telling, New Deal policies culminated in an alternative to the European welfare states that developed after World War II. Europeans got broad and universal social benefits financed largely through consumption taxes, combined with wage restraints achieved through industry-wide bargains that sought to enhance export competitiveness, while Americans got easy credit and market- and work-oriented social programs.

At every turn Prasad provides provocative and counterintuitive claims and literature interventions, which at times are brilliant and at other times more contentious but always worth engaging. Given the theoretical and empirical ambitions of her work, there are, perhaps inevitably, some assertions and blind spots in *The Land of Too Much* that demand investigation. When the historical narrative gets to the New Deal, to my mind it minimizes the role of organized labor and the regulatory and tax reforms of the 1930s, which laid the foundation for four decades of declining inequality (including the financial sector regulations that created the era of ''boring banking''). Prasad focuses excessively, to my mind, on the extension of mortgage assistance and agrarian programs, which are part of the story but cannot profitably be characterized as central to the New Deal (especially the more progressive ''second'' New Deal from 1935 on). And the institutions and policy innovations—established during the New Deal and in some cases extended during World War II—included very high marginal tax rates that constrained incomes at the top much more effectively than anything before or since. The postwar business-labor compact that produced rising wages for workers and declining returns to the wealthy is also given short shrift; rising consumption was fueled primarily by rising incomes and only secondarily by rising credit in the postwar decades. Finally, the centrality of achievement-oriented programs—most notably education—in the archipelago of the American welfare state from the nineteenth century onward fits unevenly in Prasad's model. In recent years, the expansion of government-based loan programs (the credit model) to pay for the rising cost of college education has become important, but long before that the United States led the world in the quality and cost of its public education system; this was a hallmark of the American version of the welfare state (Garfinkel, Smeeding, and Rainwater 2010).

In spite of these issues, it is fair to say that Prasad's work takes us a long way toward achieving a new synthesis built centrally around a political economy argument. Krippner's analysis parallels Prasad's, but adopts a fundamentally different line of attack: while Prasad focuses on the *demand* for credit from farmers and consumers from below as the key to understanding the historical role of finance in American political life, Krippner focuses on the *supply* of credit as the emerging solution to the crisis tendencies in what we formerly called ''late'' capitalism. Krippner's historical and theoretical focus is on the period from 1970 to 2000 (she somewhat bravely resists the temptation to extend her account through the financial crisis of 2008, although an account of the origins of that crisis are clearly evident in what she has done, providing a motivation for readers). Like Prasad, Krippner sees the extension of credit as a key mechanism for pacifying the masses but also emphasizes the role of finance in expanding corporate profits and keeping the economy humming. In her second chapter, Krippner provides a valuable and hard-won empirical demonstration (creatively drawing from a variety of data sources) of the rising importance of financial profits, most tellingly for non-financial firms, since the 1970s. She shows that many of the largest American manufacturing firms actually make more money through customer loans and returns on financial investments than they do on the products they make and sell. Her data suggest that about ten percent of all profits in the American economy in the 1950s were financial, but the figure rose to forty percent in the early 2000s. The puzzle she raises is, how did this state of affairs come to be?

In answer, Krippner draws upon a mid-1970s Habermasian/Bell theory of the coming crisis of democratic capitalism, a theory that seems much more prescient now than it did a decade ago. Habermas (1975) and Bell (1976) investigated how market capitalism could retain its legitimacy and prevent revolts from below once economic growth slowed (and, one might add in hindsight, all the more so in a world where almost all the benefits of more limited growth are flowing to the top one percent). Krippner argues that the solution lies in financialization, promoted by several policy decisions: most crucially, deregulation of interest rates and global flows, and secondarily, the anti-inflation Federal Reserve policies under Paul Volcker in the late 1970s that drove up interest rates but had the unanticipated consequence of encouraging a massive in-flow of foreign capital. Financialization generated a windfall for American consumers and households and arguably deferred the crisis tendencies that so many analysts were pointing to in the mid-1970s (cf. Streeck 2013).

Krippner's more circumscribed agenda leaves fewer holes to poke than Prasad's. My biggest concern is that her American-centered account focuses laser attention on political developments here that may be mere epiphenomena in the sense that financialization has occurred everywhere in the rich democratic world. This fact suggests that the specific processes through which financialization took place in the United States may be somewhat beside the point (although one could also argue that, to some extent, the global financialization movement was spurred by U.S. developments, raising a classic chicken/egg problem). The historical account, very much like Prasad's, has many moving parts that have to be placed in the right sequence and moving in the right direction to be completely persuasive, and there is a danger of reading backwards what was not at all obvious at the time (the Volcker story is an example: the unintended consequences interpretation strikes me as correct but not consistent with the macro argument of the book, which asserts that conscious political decisions drove financialization; indeed, the very opposite low-interest-rate regime of Greenspan can be seen as a more natural attempt to promote financialization). But what I appreciate is that Krippner makes explicit the idea that pro-financialization policies were driven at least in part by the need to conform to underlying public preferences. The availability of expanded credit, and its capacity to provide some benefits to the masses, makes the pro-finance regime a classic hegemonic case in the Gramscian sense. To be sure, it is not one that could last forever (as Gramsci always insisted about capitalist hegemony), but one whose power rests in part on the fact that expanded credit can provide real benefits for a wide audience.

Krippner's Michigan colleague Mark Mizruchi's recent *Fracturing of the American Corporate Elite* turns some of this discussion about the political impact of financialization on its head in interesting and controversial ways. Mizruchi's work also represents a return of two theoretical traditions in political sociology that had been thoroughly beaten up by institutionalists (and others) in the 1980s and 1990s: corporate liberalism and power structure research. Mizruchi's argument is that a key to understanding the low-tax, low-regulation environment of the current era lies in the death of the corporate statesmanship of the middle of the twentieth century that was prepared to compromise with the New Deal in the public interest. Corporate moderation involving a centrist adaptation to the welfare state and peaceful co-existence with organized labor was, Mizruchi argues, a powerful, even dominant, force inside corporate America.

Organizations such as the Committee on Economic Development (CED), a group of business leaders formed in 1942, and the Chamber of Commerce were vehicles for mid-century political involvement. And these corporate elites were, by today's standards, shockingly willing to accept public policies that are almost entirely unacceptable to current corporate leaders. Take taxes. It is surprising to learn, for example, that both the CED and the Chamber responded to the Korean War in the early 1950s by calling for very substantial tax increases to pay for it; they even supported a renewal of the excessive-profits corporate tax that was adopted during World War II. At a time when the highest marginal tax rate was 90 percent and corporate income taxes were much higher than they are today, the CED declared "we need still higher taxes—higher than we have ever had before, even at their wartime peak."

The political moderation of the corporate sector in the mid-twentieth century stemmed from the business networks centered on commercial banking. In the era of "boring banking" ushered in by New Deal finance reforms, stable interlocking corporate directorates with prominent roles played by bank directors provided a context, in Mizruchi's view, for exchanging ideas, spreading information, and developing policies in the public good. This era began to decline in the 1970s, however, as financialization began to take off (and the possibility of outsize profits and executive salaries began). We now understand that corporate executives were happier to deal with strong unions than weak and declining ones (where the costs of having a unionized workplace were not necessarily shared by all of your competitors; see Swenson [2002] for an elegant theorization of this idea). But as the right-wing agenda, pushed by a minority of corporate figures before 1970, began to gain ground, everything that held corporate moderates together began to fall apart. The merger wave of the mid-1980s and the growing importance of short-term profits and share prices eventually created a new kind of CEO (and aspiring CEOs further down the food chain), whose success hinged on creating shareholder value by whatever means necessary.

Any assessment of Mizruchi's thesis has to rest on the complicated question of how to weight various factors in a moving tableau. Was corporate moderation in the middle of the twentieth century driven by network-based centralization, or was it a *reaction* to the strength of the Democratic Party, organized labor, the civil rights movement, and liberalism more generally? Did corporate fragmentation (and the funding of new conservative think tanks and business associations like the Business Roundtable in the 1970s, the right-wing successor to CED) drive the rightward shift of American politics from the 1970s onward? Or was that shift rooted in political forces that are exogenous to Mizruchi's model? My own view is that corporate elites are best viewed as actors engaged in political contests on a shifting terrain where the rise of modern finance, the decline of unions, and the strengthening of anti-government forces from the late 1970s on (especially with the election of Ronald Reagan) made it entirely rational for CEOs to support policy agendas that may prove destructive in the long run. At the end of the book, Mizruchi offers an almost wistful call for the old corporate elite to find its voice in the public interest. But it is hard to see where the motivation to pursue a new centrist course might come from, except in the example of a few outliers like Warren Buffet.

The Social and Ideological Bases of Political Inequality

The prominence of finance in the recent political sociology of the American state provides one angle for thinking about the linkage of the political and the economic. But underlying many of these works is an uneasy view of the "democracy" side of democratic capitalism. Both Prasad and Krippner invoke notions of "legitimacy" and public opinion in their writings, but they never systematically analyze it. As we think about the extraordinary shifts in distribution and life chances inaugurated by the high-inequality regime, we have to ask a fundamental question: why, in a democratic polity where the interests of the 99 percent have ample opportunity to demand an alternative course, have egalitarian policies lost ground in the past forty years? What are the microfoundations of popular support for the high-inequality regime?

In their important but less well-known work (at least among sociologists), the political scientists Nolan McCarty, Keith Poole, and Howard Rosenthal have provided a novel and unique synthesis that offers a plausible set of conclusions that have to be, I think, a substantial part of the answer to this question (for a recent update, incorporating the research on campaign finance by Adam Bonica, see Bonica et al. 2014). The backstory of *Polarized America* starts with the influential work of Poole and Rosenthal beginning in the 1980s (see especially Poole and Rosenthal 1997) that developed a technique for measuring the ideological positioning of every member of Congress based on a two-dimensional algorithm (with one dimension being a liberal-conservative dimension and the other focused on the cross-cutting and salient issues of the day). This work, which has become the standard in the field, builds upon a comprehensive database of each Congressional vote and the voting patterns of each member of Congress since 1798.

For the current period, their data rigorously demonstrate what some journalistic commentators have also noted: it is the Republican Party pulling to the right that is driving the peak levels of polarization reached in the early 2000s and continuing up to the present, not a bipartisan pulling away from the center. While on some measures and key cross-cutting issues Congressional Democrats look slightly more liberal in the 2000s than in the 1970s, that movement is slight and dominated by non-economic issues (as well as the historic shift represented by the loss of the Southern Democratic contingent, which was once populist but had become very centrist on economic policy questions by the 1970s).

The analytical point of departure for *Polarized America* is the relationship between economic inequality and rising polarization: the authors argue that polarization contributes to a kind of political gridlock that makes it difficult to slow or reverse rising inequality. Although McCarty et al. position their book—with its use of formal models and a largely non-sociological reference list—toward a political science and formal political economy audience, it is also an inspired reengagement with the classical tradition of Seymour Martin Lipset and his intellectual heirs, who emphasized the social basis of political behavior and placed the social foundations of political parties and elections at the center of their analysis of democratic capitalism. Key to McCarty et al.'s contribution is the political impact of rising immigration on the electorate. The political disenfranchisement of legal immigrants and others (such as convicted felons) has reshaped the overall electorate; the authors show that the median American voter is significantly wealthier than the average adult, a result which, they argue, pushes American democracy away from the redistribution predicted in classical political economy models (where public demands for redistribution will grow as inequality rises). These impacts are further skewed by the population who votes among those who are eligible: poor and working-class citizens participate at much lower rates than better-educated and more affluent voters. Finally, they note the clear link between rising inequality at the top and rapidly growing political campaign contributions. The very rich have much larger income streams than their predecessors a generation ago, and the authors draw attention to the enormous "soft money" donations to independent expenditure groups that are essentially unregulated. These latter donations, they argued in 2006, have been used to fund more extreme candidates (although in their 2014 update, using Bonica's more systematic evidence, they pull back a bit from that conclusion in noting that many of the very largest donors are somewhere in the political center). Perhaps even more importantly, both parties are increasingly reliant on political donations from donors in the top .01 percent of the income distribution, which for Democrats has meant that the relative importance of unions and liberal groups has declined. Financial executives and fund managers have been an increasingly important donor group, which surely helps to account for some of the adoption or maintenance of finance-favorable policies identified by Krippner and Prasad.

The second major contribution is an assertion that a polarized national government may contribute to rising inequality (or at least be poorly equipped to stop the rise of inequality). One mechanism they postulate is a version of policy drift, where the failure to update the minimum wage or revise financial-sector rules that bankers and hedge fund managers have learned how to exploit, even after 2008, is vitally important. Policy drift in a polarized political system favors the rich because of their command over greater resources and their access to experts who can manipulate existing rules and regulations. A polarized political system seems particularly unlikely to change broad-based tax reform that favors the rich. It is also less likely, in the face of political institutions requiring super-majorities, to enact sweeping changes to social policies as societal needs shift (for example, federal policy has done little to support job creation even as the proportion of working-age adults in jobs has declined).

The issues identified by McCarty et al. provide a different view of inequality, connecting a bottom-up account with developments at the very top. The startling link between a high-immigration regime and income inequality is a plausible interpretation of American political developments and may help to account for the greater space for reformist politics in the middle decades of the twentieth century when the immigration door was largely closed. The degree to which polarization cripples government responsiveness to social problems in general, however, is less clear; there have been many important policies adopted even in the face of polarization (since 2000, this list would include tax-cutting, major wars, health care reform, and the largest stimulus package ever in 2009). Perhaps the strongest piece of evidence that polarization cripples egalitarian policymaking can be seen in the weak response to the 2008 financial crisis, a matter the authors have taken up in a recent study (McCarty, Poole, and Rosenthal 2013).

An alternative to focusing on mechanisms rooted in the social bases of politics is to turn more explicitly to public opinion and its impact on policy outcomes. A generation of research suggests that sustained public support for new policies, or opposition to existing policies, will influence public policy (Manza and Cook 2002). The starting question here is why the mass public (as opposed to political elites) was convinced to go along with the emerging high-inequality regime. Two contrasting views on this issue are represented in the work of Leslie McCall, on the one hand, and Kathryn Newman and Elizabeth Jacobs on the other.[1] McCall's comprehensive analysis of the public opinion question in *The Undeserving Rich* heightens the puzzle of the popular foundations of a high-inequality regime: she shows that across many well-measured survey items, Americans have persistently opposed unfair advantages and outsized income and wealth returns to the rich. Her investigation of recent public-opinion literature on inequality provides a thorough dissection of the multiple ways previous scholarship has characterized the public mood, and her comprehensive examination of public opinion trends (and non-trends) over the past 40 years in the face of rising inequality offers a thorough portrait.

McCall's empirical contributions reconfirm the standard view that Americans oppose inequality in principle but are reluctant to endorse specific policies that might reduce it. But she goes beyond the conventional wisdom in two important ways. First, she argues that principled opposition to inequality is sharpest when the question centers on the possibility that the rich can use their wealth to secure unfair advantages or that they receive income by cheating the system. It is in this sense that the rich are "undeserving." But if their wealth is acquired through hard work, most Americans have no problem with it. Second, McCall argues that public concern about inequality is heightened when media attention to the topic increases. She develops a content analysis of media discussions of inequality, finding a peak in the mid-1990s. (Her study unfortunately concludes before the appearance of the Occupy Wall Street movement in 2011 and the mammoth increase in media attention to inequality that followed in its wake.) I am a bit skeptical about whether the apparent uptick in egalitarian sentiment in the mid-1990s was anything more than trendless fluctuation and, perhaps more importantly, whether it was really those survey respondents consuming the (increased) news coverage of inequality who were expressing more egalitarian views. That said, McCall's conclusion that Americans would respond to certain types of egalitarian policy proposals, but not others, is surely correct. There would be popular potential for increased expenditures on education, for example, or policies that are designed to produce greater tax fairness (for example, in treating all income streams equally rather than privileging capital income). The problem is that such policies would have little to no impact on inequality generated at the very top of the distribution.

McCall's findings and conclusions on policy preferences are essentially consistent with the analysis and reconstruction of historical survey data undertaken by Kathryn Newman and Elizabeth Jacobs in their 2010 study *Who Cares?* Newman and Jacobs examine public attitudes in the 1930s, the 1960s, and the current era (using both poll data and presidential correspondence).

[1] For a stand-alone review of McCall's *The Undeserving Rich*, see p. 539 of this issue [ed.].

Their message is fairly consistent and straightforward: Americans do not support programs of redistribution that reward people who are not working (e.g., proposals in the 1930s to create universal cash benefits for the aged failed in favor of employment-linked benefits, and proposals to provide a guaranteed income floor such as the Nixon Administration's Family Assistance Plan never really had a chance in the court of public opinion). In general, redistribution through government institutions works best when it is opportunity-oriented and founders when it seeks more direct measures to help those in need. In order to do more, politicians have to be willing to do things that are not likely to be popular.

Like McCall, Newman and Jacobs are determined to see the glass as half full, while providing evidence to suggest that egalitarian politics are crippled by public preferences. What I am especially attracted to in the Newman and Jacobs work is the simple possibility that since the origins of the American welfare state, in the broadest possible sense Americans have more or less gotten a version of what they want. Policies that support opportunity, such as education and employment-linked policies (and, we might argue, policies that support credit, a topic they do not investigate), have triumphed while those that provide cash or other benefits to those who are not working have always been limited. A tax system that is progressive but also allows tax breaks for investment and charity is entirely consistent with these preferences (and tax cutting is popular, as long as some of the benefits go to working families). This line of argument provides, I think, the most parsimonious and elegant solution to the puzzle of the comparative weakness and limited generosity of the American welfare state (cf. Brooks and Manza 2007).

McCall's and Newman/Jacobs' analyses suggest a critical conclusion, one that is sometimes elided by both books' determined optimism: opportunity policies may help reduce the income gap between, let us say, individuals at the eightieth percentile versus those at the fiftieth or thirty-third percentile; but this is simply not the critical issue about rising inequality. The gap between the eightieth (or even ninetieth) percentile and lower earners has changed very little compared to the mammoth changes between the ninety-ninth percentile or the 99.9th percentile and everyone else. Opportunity policies, for example those that help disadvantaged kids or single mothers or downsized middle-aged workers, will do little to nothing to constrain high incomes at the top. Increasing tax fairness would help somewhat (e.g., taxing capital gains at the same rate as other income), and that approach might be consistent with public preferences. Beyond that, there is little in either study suggesting that public preferences can be reliably mobilized for an egalitarian agenda.

There is another angle on these questions. In recent decades, even popular human capital policies have not been sustained to the same degree as in the early postwar decades. Martin Gilens' extraordinary recent work *Affluence and Influence* raises some important doubts about the argument that Americans want more social policy than they are likely to get even if Democratic politicians would just demand it with more vigor. Gilens offers an exceptionally gloomy view of the relationship between inequality and the responsiveness of government policy to public preferences. He has assembled an enormous database of survey and polling questions asked over a 35-year period and matching data on later policy outcomes. Although his dataset draws from a wide range of different types of polls and surveys, all have in common some information about the respondents' income; and although this is often imprecise it allows Gilens to identify that across many (but not all) policy domains, the rich (those at ninetieth income percentile) get what they want (their average policy preference) far more often than the poor (respondents at the tenth percentile). For example, many rich Americans (like poor Americans) support employment-based social programs. Indeed, one of the findings that many sociologists will be surprised about is how often the class differences in opinion are more modest than we typically assume. But the poor are much more supportive of other social programs; and by comparison with European welfare states, those rarely are adopted.

Gilens' basic set of findings, demonstrating the policy bias toward responsiveness to the rich, is a major achievement. But he goes much further than any of the other books under review here in attempting to systematically explain why the rich get more responsiveness. He focuses on three specific possibilities: partisan control of government, interest-group lobbying, and political money (three of the primary culprits pointed to by Hacker and Pierson in their explication of organizational advantage). In each case, he attempts to systematically examine how they operate. Examining 1700 policy questions with both survey data and some Congressional policy activity, Gilens finds little evidence that after the policy preferences of the ninetieth percentile are taken into account, interest group intervention matters. This is not to say that it never matters; just that once the rich favor or oppose a policy, adding interest group lobbying to the mix fails to improve the odds of successful policy outcome. Partisan control of government appears to be a much stronger factor, with the poor getting significantly better responsiveness when the Democrats are in control, but not enough to reverse or even slow rising inequality. The Obama era only seems to underscore this conclusion.

Toward Oligarchy?

The growing concentration of wealth and income at the top should not, as noted above, be viewed as a unique recent development. The history of wealth regimes is also usefully supplemented by comparative-historical examinations, the task of a remarkable piece of scholarship by Jeffrey Winters in his 2011 book *Oligarchy*. Winters postulates that all societies, with the possible exception of some Communist countries in certain periods, contain some people (oligarchs) who control wealth assets that are of sufficient magnitude to face threats from governments, other oligarchs, or from below. He usefully identifies four distinct ideal-types or systems of oligarchic rule (civil, warring, ruling, and sultanistic) present in social and political regimes around the world. In any of these systems, the crucial question for oligarchs is whether they can protect their wealth from various challenges. In warring oligarchies, the principal threats come from other oligarchs, who will need to become warlords or command significant military power; by contrast, in ruling oligarchies (a common type in European city-states and Ancient Rome), a collective group governs through institutional rules that are enforceable against any deviating oligarch. Sultanistic oligarchs arise in cases of corrupt governments where the line between public and private power blurs (Putin's Russia would be a prime contemporary example). In civil oligarchies such as the United States, the crucial threat to wealth comes from taxation, and the question is the extent to which oligarchs can (legally) reduce their tax burdens. The key players in a civil oligarchy are not warlords or government officials, but rather an ''income defense industry'' made up of tax lawyers, lobbyists, and accountants who devise schemes to reduce the tax burdens on high-net-worth individuals. Each type receives extended treatment with rich historical content.

This is a bold materialist argument; Winters has little use for vague theories of ''elites'' and social status for defining who is at the top of a system of inequality. His focus on income defense also provides a startling alternative to conventional thinking about political inequality. In particular, it brings the global context of wealth accumulation into sharp relief and forces political sociologists to think in larger terms about the specifics of economic and political domination by the top .01 percent of the population. The contemporary United States, Germany, or Sweden for that matter, are all variations on the same theme. It may be harder for middle-class Swedes to avoid high taxes than middle-class Americans, but for the very rich the mechanisms of tax avoidance are not terribly different. What is unique about the U.S. system of domestic tax avoidance is the existence of a continually expanding array of exemptions and exclusions, or special tax treatment of certain classes of income, that the income defense industry has created over the years—exceptions that enhance the (after-tax) income shares of the American super-rich.

Winters shows how the ordinary income-defense activities of American oligarchs ceaselessly expand the largely invisible system of tax expenditures that amount to what Suzanne Mettler (2011) has called the "submerged state" (see also Fischer et al. 1996; Howard 2011). The size and scope of tax expenditures (known more popularly as tax breaks) contribute to making the American welfare state much less progressive and redistributive on a per-dollar-spent basis (Garfinkel, Rainwater, and Smeeding 2010). Some of the benefits of tax expenditures go to small businesses and working-class and even poor families (e.g., the Earned Income Tax Credit, or various tax credits that have earnings caps). Some benefits are enjoyed by middle-class families (perhaps most notably the tax exemption for employer-provided health insurance). But the vast bulk of the benefits go to high earners and the largest corporations: tax breaks for specific corporations or industries, special reduced rates for capital gains and other forms of financial profit; the home mortgage deduction (most of which goes to the owners of expensive homes), and so on. As Mettler and others have noted, the submerged state produces a kind of policy-making that is often difficult to challenge: potential opponents know little to nothing about it, while a tiny group, often with considerable resources, has an intense interest in preserving it.

Concluding Thoughts

The current wave of interest in the underpinnings of political inequality in the United States has returned political sociology to its roots, and the growing empirical sophistication in the literature that is now regularly appearing in top sociology and political science journals, supplementing and advancing the broader theses of the books reviewed here, has been a heartening and important development. Yet there remain important challenges for social scientists and political sociologists to tackle on the road to better understanding the linkages between economic and political power. One key question in need of further research is exactly how economically powerful actors influence policy. We have an enormous amount of empirical and theoretical work on the inputs and outputs of politics, but relatively little work has successfully cracked open the black box of the political system itself.

First, we need further investigation of how corporate and economic elites use the political system in less visible ways to get the legislation or regulation they want. Conventional research methods, looking at actual voting behavior in Congress, produce what still seems to be a largely implausible set of conclusions about the lack of influence of political money and corporate lobbying. One intriguing possibility was opened up in the 1990s in the work of Dan Clawson and his colleagues on how corporate PACs operate (Clawson, Neustadtl, and Weller 1998). They found in their interviews with the heads of corporate PACs that a lot of the return on their investments (via campaign donations) comes in the form of access and the ability to secure obscure tax breaks hidden in the bowels of long pieces of legislation. The Clean Air Act of 1990, they note, is over 800 pages in length: a few pages of regulations and hundreds of pages of exemptions, many very specific but written in obscure language, making it extremely difficult to track who is getting out of what. In the age of "big data," however, what was once difficult or overwhelming to chart and analyze might begin to become visible. Systematic data analysis of access to members of Congress and their staff, attendance at fundraising events or other forms of contact, machine reading of obscure legislation, and better analysis of long-term campaign finance data all portend a future in which we will at least be able to track—if not necessarily stop—specialized tax expenditures.

Second, we need to better understand how the mammoth increase in expenditures on corporate lobbying has created a new kind of career pathway (via the availability of often very high-paying employment opportunities) for former public servants. The old "revolving door" theory of government needs a new scholarly examination in the age of inequality (but cf. Diermeier, Keane, and Merlo [2005] for an intriguing attempt to quantify the monetary value of a Congressional career on lifetime earnings). Data gathered by the Center for Responsive Politics shows that over two-thirds of living former members of Congress are working as registered lobbyists, a figure that does not include another fifth who work as corporate political consultants without being required to formally register as such. High-level congressional staff often enjoy similar career trajectories upon leaving public service. Another common pathway involves former government regulators moving to the firms they used to be involved in regulating, most notably in finance. Does this life-cycle pattern represent, as the journalist Mark Leibovich (2013) suggests in his witty takedown of Washington, D.C. insiders, a cynical world in which policymakers behave in ways that will ingratiate themselves with future employers? Such themes, central to the Millsian account of political inequality, may yet prove vitally important for a full understanding.

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