

Inequality, American Democracy, and American Political Science: The Need for Cumulative Research

The members of the Task Force on Inequality and American Democracy were not of one mind about any of the subjects they took up—and this is one reason, among many, that it was the most vibrant and stimulating professional collaboration I have had the privilege to be part of. It would be presumptuous of me to speak for anyone but myself in explaining and justifying the Task Force’s work. And yet, there does seem to me at least one note of common explanation required up front. All the members of the Task Force, I am certain, saw their work as only the *beginning* of a discussion within the discipline—and, indeed, more broadly—about the relationship between inequality and contemporary American governance. Although we hoped to showcase and integrate the best existing research and theory, our main goal was to spark new questions, new research, new thinking, and new debates. And judging from this forum, as well as from the recent scholarship showcased in Larry Bartels’s (2006) and Kay Schlozman’s (2006) essays, we have.

Political equality is at the heart of democracy. Economic inequality threatens political equality. And good research on the extent to which economic inequality actually compromises political

equality is needed. These three convictions were, to my mind, the agreed-upon common denominators that animated the Task Force’s deliberations. Frances Fox Piven (2006), in her spirited but ultimately sympathetic chiding of the Task Force as “timid,” says that the trends documented by the Task Force represent “normal politics in the United States.” By trying to fit its conclusions into the “familiar mold” of citizen participation and representation, she argues, the Task Force missed a more insidious threat to American democracy—the Republican “political machine” that controls all three branches of government today.

A reading of the other responses in this forum makes clear, of course, that even the “familiar” concerns expressed by the Task Force are not universally shared. More important, as the report of the Task Force suggests—and as the Task Force’s edited book, *Inequality and American Democracy* (Jacobs and Skocpol 2005), documents in considerable detail—surprisingly little research and theorizing directly addresses the crucial question of how political equality is affected by the recent dra-

matic rise in economic inequality in the United States. Nor does the important yet limited scholarship that *is* available indicate that the mounting threats to American democracy discussed by the Task Force are merely “normal politics.” Against the backdrop of the much less economically unequal era immediately after World War II, the recent dynamics of American politics point to a substantial decline in the responsiveness of political elites to citizens of modest economic means, as well as a hardening of the longstanding divide in participation, organizational clout, and influence between the well off and the rest.

These worrisome conclusions are perhaps most clear, I must add, in the extensive portions of the Task Force materials that go *beyond* citizen participation to consider processes of governance and relationships of influence. No fair reader of the report, for example, could accuse the Task Force of being complacent about the potentially distorting influence of lobbyists and moneyed interests, or about the declining congruence between public policy and the expressed demands of citizens. The report’s bottom line is there for all to see: Something is rotten in the state of American politics.

If the members of the Task Force were “timid,” as Piven accuses, we were so only in the appropriate sense that we were restrained by the limited state of current knowledge. I share a number of the specific concerns voiced by Piven about the recent efforts of Republicans to limit political responsiveness and electoral accountability. (Indeed, along with Paul Pierson, I have just written a book that argues that the elite-level shift to the right in American politics has pulled policymaking away from a largely moderate public (Hacker and Pierson 2005).) Others on the Task Force may well share some of these concerns. But I can assure Piven that the choice of what to discuss or present was not restrained by any explicit or implicit constraints. Any restraint on our part—and, again, some clearly do not see the report as all that restrained—was instead based on the conviction that scholars acting in a professional capacity should not delve far afield from the available evidence and research. Such timidity, if that is what it should be called, is the essence of the social sciences.

Another guiding conviction of the social sciences is that claims about how the world works should be clear about the causal mechanisms at play and careful in testing the observable

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implications of these mechanisms using the best available evidence. Without denying the personal appeal of some of Piven's broadsides against the GOP "machine," I feel compelled to note that some of her assertions fall short of this standard. Is the mass media truly "allied" with the Republican "machine," for example? As Pierson and I point out in our new book, *Off Center*, the claims of "liberal media bias" are certainly overstated, and the routines of the media have frequently played into Republican hands. But asserting that media is in the pocket of the GOP "machine" strains credibility. Similarly, the claim that the Democratic strategy under Clinton was to "beat Republicans by adopting their positions" (43) is a vast oversimplification of a more complex reality. The point is not that these arguments could not be defended. It is that to be defended, they would require both clearer specification and more extensive research, much of which, so far as I know, does not exist. Piven is certainly free to make her case, and hopefully it will spark not just debate but inquiry. But the Task Force did not feel similarly free as it assembled a report reflecting the current level of scientific knowledge.

To say all this is not to retreat into the sterile view that the social sciences cannot get out in front on controversial issues, which is what I believe the Task Force has rightly done. Nor is it to prejudge what type of evidence or theoretical approach should be used. But it is to emphasize the crucial importance of clarity about causal mechanisms and their evidentiary implications—careful attention to which almost inevitably tempers the more ambitious or totalizing claims that analysts are tempted to make. In reading Piven's essay, I wonder, for instance, how the "machine" so completely shapes opinion and insulates itself from accountability, or how she would explain the closeness of the 2004 presidential race or the abject failure of President George W. Bush's drive to partially privatize Social Security. Piven mentions the Social Security campaign as an example of how the "machine" seeks to "reinforce the political attitudes the machine propounds" (44). But while Bush certainly did affect public opinion as he doggedly traveled the nation plugging his Social Security plan, the effect was something well short of reinforcement.

Again, I am certain that a scholar as resourceful, creative, and accomplished as Piven has answers to all these questions, perhaps convincing ones. What I want to emphasize, however, is that research in the "familiar mold" that she sniffs at has something powerful going for it—it explains its logic and identifies its confirming evidence in ways that allow us to see where that logic and that evidence fall short. So, for example, the copious research and theorizing about the influence of the median voter has led to the development of theory and evidence that shows when and why that influence is attenuated (e.g., Fiorina 2004). To cite another example, the growing body of work on the sources of legislative gridlock has led to new models and new research that highlight the factors that lessen or accentuate the tendency of American politics toward stalemate (e.g., Binder 2003). Some of these ventures represent empirical wheel-spinning or theoretical make-work. And there is never a good excuse for scholarship merely concerned with rescuing old models. But the ongoing refinement, and sometimes replacement, of theories and concepts rooted in sustained lines of empirical inquiry is what ultimately drives the social sciences forward. And it is precisely this sort of cumulative research that is most needed in understanding the multi-faceted relationship between inequality and democracy in the United States.

This last point brings me to purview of the working group I chaired—namely, the historical and contemporary relationship between public policy and political equality in the United States. As our working group's report explained, the claim that public policies, once enacted, reshape politics through processes

of "policy feedback" is commonplace in political science (for the seminal review, see Pierson 1993). Again and again, we are told that policies create new constituencies and new bureaucracies, bestow resources on some and strip them from others, and shape the interpretive frameworks that citizens and elites use to understand the world around them. The ubiquity of such claims is not surprising. Indeed, in an age in which government is such a large factor in everyday life, it would be surprising if public policies did *not* have major political consequences. And yet, systematic research on the effects of public policies on political inequality turns out to be almost non-existent. In particular, scholars have only started to explore the ways in which policies shape the political views, resources, and activities of the mass public. And they have barely begun to consider how these political effects mediate, and are mediated by, rising economic inequality in the United States.

Hence the mission of the "policy feedbacks" working group was to explore what we know about the relationship between U.S. public policies and political equality. In the process, we canvassed the best new studies on the topic—such as Suzanne Mettler's new book on the GI Bill (2005) and Andrea Campbell's recent work on Social Security (2003). We also examined the growing body of increasingly sophisticated work on the distributional effects of U.S. social policies in cross-national perspective. And we tried as best we could to bring together existing research in new ways, reaching into sociology, economics, and policy studies to find evidence that had not yet been integrated into political analysis.

Perhaps not surprisingly, we found that scholars know the most with regard to policies' distributional effects. Over the last decade, remarkable (and, in many quarters, unappreciated) progress has been made in understanding how taxes and public spending affect the distribution of market income—in the United States as well as in other advanced capitalist democracies. As we noted in our review of this work, the United States stands out as a clear outlier in these studies, doing much less through taxes and spending to reduce market inequality than do other affluent nations. This is not simply an issue of comparative spending levels. When relative tax burdens and private social spending by employers are taken into account, the U.S. social welfare framework is roughly as large as other nations' frameworks. But it redistributes income much less extensively, leaving larger gaps between rich and poor and more people in poverty than do other affluent nations.

Equally important, these recent studies also make clear that the United States has done much less than other rich capitalist nations to respond to the rise in market inequality that occurred across the developed world in the 1980s and 1990s. Thus, to understand the recent rise in economic inequality in the United States, as Larry Bartels notes in his essay (2006), one must ask why American policymakers have done so little to moderate the rising gulf between rich and poor, or to respond to the new and newly intensified economic risks of the post-1970s era (Hacker 2004). Decisions *not* to act in the face of contrary pressure—a classic example of the "nondecisions" so hotly considered in political science a generation ago—must be considered in any analysis of changing policy effects.

In short, existing scholarship has said a great deal about how public policies affect the distribution of income in the United States. Much less has been said, unfortunately, about how policies affect the distribution of political power, especially when the focus shifts from elite-level effects to the consequences of policies for the mass public. Nonetheless, the working group found a small but rapidly expanding body of scholarship that shows that policies can have major consequences for political participation, resources, and, ultimately, representation. Medicare, Social Security, and the GI Bill—to name just three of the

large-scale policy interventions we discuss—have each mobilized and enriched key segments of the public, changing fundamentally the landscape of participation and organization in the United States. For the most part, these changes have been positive, in the sense of creating greater political involvement and greater feelings of political efficacy. And they have also, for the most part, pushed in the direction of political equality, lessening the disparities in resources and participation that are so striking in American politics today.

Yet this positive relationship appears to have come undone in recent decades. Recent trends in economic and social policies, the working group concluded, have undercut the gains in participation, resources, and efficacy that emerged out of mid-century public policies. Here, the working group was treading the most speculative ground. But lest Piven's essay suggests that the working group's report does not take on contemporary dynamics, the report clearly indicated that the erosion of public spending programs and the shift toward more indirect or hidden forms of government, such as tax expenditures for private investment accounts, posed the risk of exacerbating economic and political inequality. These changes, we argued, could undercut the positive role of government in mitigating participatory inequalities and fostering political efficacy among citizens of all economic backgrounds, but especially the disadvantaged.

What the working group did not consider—and, given the existing research, could not in good faith consider—was whether political elites have consciously pursued these policy shifts *in order to* exacerbate political inequality. Piven wants a more robust use of the policy-feedback concept to look at the

ways in which cogs in the GOP “machine” have harnessed policy to transform American politics. Such analysis of the means by which politicians use public policy to shape future politics is indeed woefully underdeveloped, and Piven rightly calls for more of it—though she oddly misses some of the best recent scholarship attempting to do just this (notably, Steven Teles's work (1996; forthcoming) on the cultivation by conservatives of parallel policy paths to undermine existing public programs). And yet, not only is Piven's own analysis of these attempts to foster policy feedbacks obviously (and no doubt intentionally) one-sided, attributing almost limitless foresight and capacity to conservative elites, but, in addition, the research agenda that she recommends depends precisely on the kind of cumulative research on the political effects of public policies that the working group reviewed. For we simply cannot know when and why politicians are able to use policies to achieve political ends—and when and why such efforts succeed or fail—without a broader and deeper understanding of the specific mechanisms by which public policies reshape the landscape of political life.

Thus, I return full circle: to the need for new research and new theories and, yes, new debates. Political science should not be afraid to find uncomfortable truths, and in my view, the Task Force on Inequality and American Democracy unearthed many. But before we extend our field of vision too far, before we leap from the firm plank of knowledge into a sea of swirling unknowns, we should better grasp what we know and do not know—and how, with good social science, we can come to know more.

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