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From Policy to Polity: Democracy, Paternalism, and the Incorporation of Disadvantaged Citizens

Sarah K. Bruch,[a] Myra Marx Ferree,[a] and Joe Soss[b]

Abstract

This article investigates how experiences with public policies affect levels of civic and political engagement among the poor. Studies of "policy feedback" investigate policies not just as political outcomes, but also as factors that set political forces in motion and shape political agency. To advance this literature, we take up three outstanding questions related to selection bias, the distinction between universal and targeted programs, and the types of authority relations most likely to foster engagement among the poor. Using a longitudinal dataset from the Fragile Families and Child Wellbeing Study, which follows a cohort of low-income parents and their newborn children in 20 U.S. cities, we estimate effects associated with three types of means-tested public assistance. We find that these policies' effects are not an illusion created by selection bias; the effects of targeted programs can both promote and discourage engagement; and such effects tend to be more positive when a policy's authority structure reflects democratic rather than paternalist principles.

Keywords

citizenship, civic engagement, political participation, policy feedback, welfare state

Americans have grown accustomed to a political universe in which the poor participate far less than the rich. Yet this state of affairs is neither natural nor inevitable. Class biases in political engagement are much less extreme in other wealthy democracies and were considerably smaller in the United States only a few decades ago (Freeman 2004). The deep political marginality of the poor today is a constructed outcome that reflects a confluence of demobilizing factors in contemporary U.S. politics.

Lower-class Americans are less likely than their fellow citizens to have access to the skills and resources that facilitate political participation (Verba, Schlozman, and Brady 1995). They have fewer ties to civic organizations that recruit people into politics and little access to trade unions, which engage their European counterparts (Radcliff and Davis 2000). Parties and candidates are less likely to strategically target the

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poor for political mobilization (Rosenstone and Hansen 1993). The poor are also more likely to find that political contests are organized around issues that ignore their needs and interests (Schattschneider 1960). Moreover, participation by the poor is disproportionately constrained by such institutional restrictions as felony disenfranchisement laws (Uggen and Manza 2006), citizen-initiated voter registration (Piven and Cloward 1988), and workday rather than civic-holiday voting arrangements (Freeman 2004).

In the shadow of this powerful conjunction of forces, it is tempting to see political inactivity among the poor as unchangeable. In this article, we argue the opposite. Analyzing a wide variety of factors related to participation, we present evidence that social policy designs can structure low-income citizens' experiences with government in ways that raise or lower their levels of civic and political engagement.

Capitalist democracies, such as the United States, inevitably confront tensions between market-based inequalities and aspirations for an inclusive and egalitarian political order. The sources of these tensions, and the potential for welfare states to mediate them, have been a longstanding subject of inquiry for scholars working in the consensus and conflict traditions of sociology. Marshall (1964), for example, suggests that a liberal-democratic solution to the problem of ''citizenship and social class'' requires establishing solidaristic social rights. By institutionalizing these rights, Marshall argues, welfare states can mitigate the factors that turn socioeconomic disadvantage into civic marginality and, in the process, deepen the status and practice of citizenship. Writing from a conflict perspective, Piven and Cloward ([1971] 1993) emphasize the role of the welfare state as a ''secondary institution'' mediating tensions between states and markets. In their account, relief programs play a key role in regulating the lower classes' political and economic behaviors. Welfare policies, they suggest, can shape power relations, foster political contention, and cultivate political quiescence.

Despite the importance of these arguments for sociology over the past half-century, scholars are only now beginning to conduct systematic empirical studies of how welfare policies affect civic and political engagement. During the past decade, studies of ''policy feedback'' have increasingly investigated policies not just as political outcomes, but as factors that set political forces in motion and shape political agency in the citizenry (Pierson 1993; Svallfors 2007). Within this research, many studies suggest that welfare policies, depending on how they are structured, can deepen or ameliorate the political marginality of disadvantaged groups (Mettler and Soss 2004). Welfare programs distribute resources that can facilitate political action (Verba et al. 1995); they create incentives to participate by giving recipients a self-interested stake in defending program benefits (Campbell 2003). Welfare programs also provide direct experiences of government that can teach significant lessons about power, identity, and the desirability of exercising political voice (Mettler 2005; Soss 2000).

Recent studies suggest that citizens' experiences in social welfare programs can have significant political consequences (Kumlin 2004; Kumlin and Rothstein 2005; Lawless and Fox 2001; Mettler and Stonecash 2008). Yet there is little agreement about whether such feedback effects have been adequately distinguished from preexisting differences among individuals, about the specific design elements that produce such effects, or about how policies can best be designed to foster a more inclusive and engaged citizenry. We address outstanding questions in all three areas by analyzing the civic and political effects of citizens' experiences as participants in means-tested social welfare programs.

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POLICY FEEDBACK AMONG DISADVANTAGED TARGET GROUPS: OUTSTANDING QUESTIONS

Several studies suggest the potential for feedback effects in low-income populations. Verba and colleagues (1995), for example, find that participants in means-tested programs are less likely to engage in such political acts as voting, campaigning, and contacting public officials, while social insurance participants are significantly more likely to become politically engaged. Building on this work, Campbell (2003) finds that participation in Social Security Old Age Insurance has positive effects on political participation among senior citizens, with the largest boost occurring among low-income recipients. As Campbell emphasizes, low-income beneficiaries receive resources that facilitate participation, are mobilized by program-related interest groups, and have especially strong incentives to mobilize in defense of benefits.

The causal mechanisms emphasized in these studies focus on resource effects. Low-income groups tend to lack resources needed for political participation—such as money, skills, and time—as well as connections to organizations that recruit people into politics (Verba et al. 1995). From this perspective, social policies should mitigate the poor's political disadvantages most effectively when they offer higher benefits and give rise to organizations that offset the costs of participation. In the United States, such programs are usually found in the more universal social-insurance tier of the welfare state (Campbell 2007).

Alongside resources, feedback studies place equal emphasis on the cognitive (or interpretive) effects of policy-based experiences (Pierson 1993). From this perspective, policy designs do more than just distribute resources: they convey potent messages about political identities, possibilities, and realities (Schneider and Ingram 1997). Personal experiences with public policy can provide lessons about group status, government responsiveness, and the efficacy and wisdom of exercising one's voice as a citizen (Soss 2005). In this regard, several studies point to the importance of how policy designs structure authority relations.

Mettler (2005:85), for example, finds that under the G.I. Bill, poor and working-class veterans experienced program implementation ''marked by fairness and ease of accessibility.'' This experience boosted civic engagement by conveying full civic status, building civic commitment, and supplying a positive experience of government (Mettler 2005). Similarly, Soss (2000) finds that distinctive authority structures lead participants to draw strikingly different political lessons from their experiences with Aid to Families with Dependent Children (AFDC), Social Security Disability Insurance (SSDI), and Head Start. Consistent with this interpretation, studies suggest that clients' political engagement is often dampened when they develop negative views of their interactions with welfare officials (Lawless and Fox 2001); political disengagement deepens as citizens accumulate experiences with means-tested programs (Mettler and Stonecash 2008). In all these studies, feedback effects appear to depend on citizens' interpretations of program experiences—what they learn about government, participation, and their own place in the political order.

The study of policy feedback has evolved significantly over the past decade. With increasing empirical evidence and conceptual sophistication, important new questions concerning methodology, interpretation, and theory have emerged. Three areas of scholarly disagreement strike us as especially worthy of attention.

1. Selection Bias

Studies that link program experiences to political behavior typically rely on one of three analytic strategies: interpretive analyses of client statements that link program

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experiences to political orientations (e.g., Soss 2000), multivariate analyses that compare program recipients to others who share their demographics (e.g., Mettler 2005), and multivariate analyses that compare participants across programs while controlling for demographic differences (e.g., Campbell 2003). Our ability to infer and generalize causal effects from these studies is restricted by a number of methodological limitations, the most significant of which is the potential for selection bias (Heckman 1979). Welfare claimants may differ from non-claimants in ways that distinguish them from others who share their demographics *and* depressed political engagement. As a result, previous findings of feedback effects may ''reflect the people who enter programs rather than what the program does'' (Mead 2001:676).

The threat of selection bias in feedback studies flows in part from the omission of relevant personal characteristics from empirical analyses. National surveys designed for the study of mass politics include few direct measures of life conditions that might promote welfare reliance and dampen civic engagement (e.g., experiences with substance abuse or domestic violence). The relatively small number of program recipients included in these datasets compounds this problem. Qualitative studies offer a stronger basis for exploring the causal mechanisms that link program experiences to political beliefs, but their small, community-based samples do little to overcome these problems and, in fact, offer a weaker basis for statistical generalization. As a result, the effect of program experiences—net of preexisting differences across relevant subpopulations—remains an open and contested question.

2. Design Variation

To date, most studies reporting positive feedback effects focus on universal policies serving broad constituencies, such as the G.I. Bill (Mettler 2005) and Social Security (Campbell 2003). By contrast, the most negative effects are associated with programs targeted to the poor, such as AFDC (Mettler and Stonecash 2008; Soss 2000). As a result, research in this area often is assimilated into arguments about the political inferiority of targeted programs and the democratic benefits of universal programs (Campbell 2007; Skocpol 1991). In this view, social welfare policies can be split roughly into two tiers, with social insurance programs on top and targeted public assistance programs on the bottom (Nelson 1990). This division corresponds to theories of dual social citizenship that link the bifurcation of beneficiaries to contrasts between the ''deserving and undeserving.'' This, in turn, reflects contrasts based on systems of gender, class, and racialized ethnicity (Gordon 1994, 2002; Lieberman 2001; Mink 1998; Orloff 1996; Quadagno 1996; Weir, Orloff, and Skocpol 1988).

These contrasts highlight political dynamics that shaped the U.S. welfare state and inform its operation today. Yet the cleaving of the system into two opposite channels falters when one looks closely at the variety of ways welfare programs differ (Howard 2006). Scholars contrasting the feedback effects of universal and targeted programs have overlooked significant differences among programs within each tier, as well as within-program differences across political jurisdictions, such as states.

Must social policy designs be universal to have positive effects on political behavior? Broadly inclusive designs have an intuitive connection to some positive outcomes, such as feelings of civic solidarity. In principle, however, there is no reason why a means-tested design should preclude resource and interpretive effects that promote civic and political engagement. Targeted public assistance programs differ considerably in the resources they provide, the obligations they impose, and the ways they structure authority relations (Mead 2004). Moreover, because public aid programs in the United States have long been decentralized (Mettler

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1998), a single aid program may have different designs in different states (Soss, Fording, and Schram 2008).

When it comes to policy feedback, it may well be that programs for the poor are not always poor programs (Howard 2006). Most researchers lump together recipients of means-tested benefits by combining either diverse categories of public aid (Mettler and Stonecash 2008; Verba et al. 1995) or beneficiaries from states with markedly different policy designs (Campbell 2003; Soss 2000). In so doing, researchers may obscure important relationships. By more carefully delineating target groups and policy differences, we assess the blunt distinction between universal and targeted programs and seek more precise accounts of how policies affect civic and political participation.

3. Authority Structures

Although positive feedback effects are usually viewed as properties of universal designs, two lines of argument suggest that means-tested programs can advance civic and political incorporation. These arguments, however, remain sharply divided on the kinds of authority structures that should produce positive effects. New paternalists, such as Mead (1986, 2005), argue that social disorder and weak self-discipline leave the welfare poor unable to shoulder the burdens of civic obligations. Civic and political incorporation are thus ill-served by ''permissive'' rights-oriented welfare programs and are better served by programs that impose order on recipients' lives and enforce civic obligations through directive and supervisory methods (Mead 1986, 2005). In this view, civic and political incorporation should be hastened when policies adopt more paternalist authority structures that ''make citizens first'' by enforcing work and other civic obligations (Mead 2005:194).

Schneider and Ingram (1997) and Soss (2000) argue, by contrast, that policies tend to promote engaged citizenship when they reject paternalist authority relations and position recipients as secure and equal citizens engaged in participatory processes. Echoing participatory democratic theorists (Pateman 1970), these scholars argue that policy designs serve democracy best when their authority structures reflect democratic principles and convey the value of engagement. Even in the absence of participatory mechanisms, this perspective predicts more positive effects when authority relations emphasize fair procedures and clear rules protecting client security, autonomy, and voice (Mettler 2005; Soss 2000).

The distinction between these two views should not be confused with the separate question of whether receipt of public aid should be made conditional on a client's fulfillment of obligations (Mead and Beem 2005). In principle, aid recipients can be obligated to meet the dictates of directive, supervisory, and disciplinary authorities, or they can be obligated to participate in decision-making processes and to share responsibilities for shaping the programs they rely on. The key opposition here is between two assertions: (1) Civic incorporation of the welfare poor requires bypassing liberal-democratic values in favor of hierarchical designs that emphasize direction, supervision, and penalty. (2) Such paternalist designs deepen civic marginality, while democratic designs that enable recipients to participate in decision processes and check the arbitrary exercise of authority will foster civic incorporation.

In this article, we seek to raise the quality of evidence available to scholars as they grapple with these three areas of debate. To do so, we examine how experiences with three government policies affect patterns of civic and political engagement. Several features of our analysis merit note. First, we focus on three means-tested policy designs that serve low-income populations but differ in their authority structures. Second, we employ a quasi-national dataset that allows us to directly measure key personal characteristics that might distinguish welfare

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recipients from others who share their demographic profiles. Third, to strengthen our evidence for feedback effects and to test competing claims about how paternalist designs affect incorporation, we examine how interstate differences in the design of a single program influence patterns of engagement in a single target population. We find that policy designs can have significant effects on civic and political engagement among the poor; the feedback effects of means-tested programs can be positive as well as negative; and such effects tend to be more positive when a policy's authority structure reflects democratic rather than paternalist principles.

RESEARCH DESIGN AND MAJOR HYPOTHESES

Our analysis focuses on three government programs that target low-income people but, by design, establish different authority relations: Temporary Assistance to Needy Families (TANF), Head Start (including Early Head Start), and public housing assistance. Encounters with different kinds of authority relations serve as the key causal mechanism, leading us to expect that experiences with the three programs will produce different feedback effects on civic and political engagement.

Head Start is a national program that seeks to advance the social and cognitive development of low-income children from birth to age five. In addition to providing educational, health, nutritional, and social services to low-income children, Head Start includes a significant parental-involvement component. The emphasis on parental participation in local site councils reflects the program's origins as a Community Action Program created by the Economic Opportunity Act of 1964, which sought to encourage the ''maximum feasible participation'' of poor people as an empowerment strategy. By prioritizing parental involvement in local programs, Head Start exemplifies what we call an *incorporating* model of authority relations between clients and government officials.

TANF, a means-tested cash-aid program for families with children, presents a very different model of authority relations. Widely portrayed as an overly permissive handout in the 1980s and 1990s, the program was redesigned in 1996 to emphasize work requirements, time-limited aid, and a more directive and supervisory orientation toward clients (Mead 2004; Weaver 2000). Client experiences in this program are structured in a highly paternalist manner, focusing on relationships with frontline caseworkers who hold substantial discretion to define obligations and distribute benefits, services, and punishments (Schram et al. 2009; Soss 2000). For these reasons, we describe this program as exemplifying a *paternalistic* design.

Our third program is public housing. The structure of public housing policy is complex, with the federal government providing guidelines for local housing authorities in their administration of publicly- and privately-owned public housing units, as well as programs like Section 8 and Hope VI. Amid this complexity, however, there is a consistency in the formal bureaucratic oversight provided by the federal government. Although some housing benefits are conditioned on behavioral restrictions, such as those related to felony convictions, authority relations in this area follow a more distant, rule-bound model. Centralized administration emphasizes regulations to ensure impartial treatment in application and assignment processes. Interactions between officials and recipients are more limited than in the other programs we consider, emphasizing neither participatory involvement nor directive and supervisory discretion. Accordingly, we characterize this program as following a *bureaucratic* model of authority relations.

Authority relations in the three programs fall along a continuum defined by core democratic principles: the ability to participate in

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decision processes that affect us and the ability to check arbitrary exercises of authority over us (Shapiro 1999). Head Start's incorporating design lies on the positive end of this dimension; TANF's paternalistic design lies on the negative end; and public housing's formal-bureaucratic design occupies an intermediate position. In many respects, this comparison parallels Soss's (2000) analysis, in which he finds positive political effects associated with Head Start, negative effects associated with AFDC, and more modest effects associated with the formal-bureaucratic design of SSDI. Soss's findings suggest a clear set of hypotheses, but his interpretive approach and small sample provide a relatively weak basis for inferring and generalizing causal effects.

Accordingly, we conduct more rigorous tests of three related hypotheses: (1) experiences with incorporating authority relations in Head Start will have positive effects on civic and political participation; (2) experiences with paternalist authority relations in TANF will have negative effects; and (3) experiences with formal-bureaucratic authority relations in public housing assistance will have limited or null effects. While we might also expect resources distributed by public programs to generate effects, our main hypotheses focus on effects that flow from experiences with different types of authority relations.

To extend this analysis, we investigate interstate differences in the degree to which TANF programs establish paternalist authority relations. Under welfare reform, states were given substantial latitude in designing their TANF programs to meet federal welfare-to-work goals. Predictably, some states placed greater emphasis on directive measures and punitive tools, while others took a softer approach and added more work-support services and benefits (Mead 2004). Lawmakers' choices across various design dimensions combined to produce distinctive TANF regimes in different states (Soss et al. 2008). In some states, paternalist authority relations are heightened by restrictive and punitive TANF design features, such as shorter time limits, family caps, stronger work requirements, and harsher sanctions for client noncompliance. If policy-based experiences with authority relations influence civic and political engagement, then TANF effects should vary significantly across states. However, if preexisting client characteristics drive participation differences, we should find few differences across states. Finally, if paternalists are correct that more authoritative designs hasten civic incorporation, then positive civic and political effects should be more likely in states that have more directive and supervisory TANF designs.

In addition to these tests, interstate comparisons allow us to address another limitation of feedback studies. Despite scholars' institutional focus in this area (Pierson 2006), studies that seek to isolate feedback effects on participation have paid little attention to the broader landscape of political institutions. In the U.S. political system, state governments retain primary authority to regulate voter eligibility, registration, and participation. State differences in these institutional features shape the scope and bias of electoral participation, especially among the poor (Piven and Cloward 1988, 2000). Likewise, the vitality of interparty competition varies widely across states, influencing parties' strength and mobilization efforts (Avery and Peffley 2005; Frymer 1999; Key 1949). If these institutional differences go unmeasured, cross-state differences in participation patterns may be falsely attributed to variations in social welfare policy design. Controlling for such institutional variation across political jurisdictions strengthens inferences about policy-design effects.

We thus add two hypotheses to our original group: (4) the civic and political effects of TANF experiences will vary significantly across states with more versus less paternalistic designs; and (5) political engagement among TANF recipients will be significantly

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lower in states that have more restrictive electoral rules and weaker party competition, but these effects will not eliminate the feedback effects of TANF policy-design variation.

Because our Fragile Families (FF) data do not reveal the qualitative nature of program experiences, cognitive effects of authority relations can only be inferred. The FF data provide an unusually strong basis for detecting policy feedback, but they do not allow us to distinguish between the authority-relations/political-learning mechanism we emphasize and the possibility that participants are more or less mobilized by their social interactions with one another in public programs. We favor the former mechanism over the latter because, unlike a group-dynamics mechanism, interview-based studies of policy feedback confirm relations with authorities as a path of influence (Mettler 2005; Soss 2000). Prior research also suggests that program experiences have long-term, cumulative effects on engagement, which points to a cognitive-learning mechanism rather than just an effect of immediate interaction contexts (Mettler and Stonecash 2008). Program effects that can be statistically distinguished from individual-level and contextual influences and that also match process observations from field studies strike us as most plausibly explained by the learning dynamics observed in these qualitative studies.[1]

DATA

The Fragile Families and Child Wellbeing Study (FF) provides a longitudinal dataset that follows a cohort of parents and their newborn children in 20 U.S. cities with populations over 200,000 (Reichman et al. 2001). The cities were intentionally chosen to provide variation in labor markets and welfare policy regimes. The sampling strategy was designed to achieve a nationally representative sample of nonmarital births in large U.S. cities; the sample also includes a subsample of births to married parents. Between February 1998 and September 2000, births were sampled from hospitals in the selected cities, and baseline interviews were conducted with mothers and fathers. Mothers and fathers of the focal child were re-interviewed when the child was 12 to 18 months (Year 1 Follow-up) and 3 years old (Year 3 Follow-up).[2]

The FF data include measures of participation in a variety of public assistance programs and broader civic and political activities. In addition to providing an unusually large sample of disadvantaged persons, the data include a rich collection of items describing individuals' backgrounds and life conditions, such as measures of domestic violence experiences, substance abuse histories, mental health status, and economic hardship. These features make the FF data ideal for investigating citizen engagement in a population known to have low participation rates and a variety of hard-to-measure life conditions that might depress participation.

The FF Baseline sample consists of 4,898 families, including 3,712 unmarried couples and 1,186 married couples. Over the three interviews (Baseline and Years 1 and 3 Follow-ups), 86 percent of fathers were interviewed at least once, and 82 percent of mothers were interviewed at all three waves. The overall response rate for the Year 3 Follow-up was 77 percent (86 percent for mothers and 67 percent for fathers). We restricted the analysis sample to the 7,529 respondents who were interviewed in the Year 3 Follow-up. We used multiple imputation to deal with missing values.[3] The FF sample is not representative of the United States: it is drawn only from large metropolitan areas and over-samples unmarried births; as a result, the sample is younger and more highly disadvantaged than the general population. Within these parameters, however, the sample should represent the propensity for political or civic participation among low-income targets of means-tested public assistance.[4]

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MEASURES

Our analyses are based on four outcome measures: voting, political participation, civic participation, and a combined measure of overall engagement. *Voting* indicates whether a respondent reported voting in the November 2000 election. *Political participation* is a dichotomous variable coded one if a respondent reported voting; participating in a political, civic, or human rights organization in the past 12 months; or taking part in a political demonstration or march. *Civic participation* is also a dichotomous variable coded one for participation in any of the following during the past 12 months: a church-affiliated group, a service club, a labor union or other work-related group, or a community organization. The final dependent variable, *degree of engagement,* is based on an individual's overall experience with these diverse participatory acts. The underlying construct we seek to capture here is the degree of political incorporation versus marginality. The measure uses the number of reported experiences with political and civic participation to construct an ordinal indicator of involvement, ranging from 0 to 2: respondents who are entirely isolated from civic and political life (0), those who have the most minimal engagement (1), and those who are more fully incorporated (2, which indicates 2 or more).[5]

Our independent variables include several demographic predictors: whether a respondent is female, whether a respondent has a parent who graduated from college, race/ethnicity, noncitizen status, age in years, educational attainment, marital status, and household income to poverty ratio (i.e., a respondent's yearly income divided by a poverty threshold adjusted for the number of children and total members in the household).

A second set of measures taps experiences associated with social marginality, such as having material hardships, drug or alcohol abuse, experiencing domestic violence, not living with the focal child, having moved in the past two years, and having a criminal conviction. Usually unavailable in studies of political participation, we include these measures to control for differences that might have produced selection bias in previous studies. Each variable is a dichotomous indicator, except for material hardship, which is a count of the number of hardships reported by a respondent (e.g., telephone service disconnected, electricity turned off, or gas/oil service turned off).[6]

Our key predictors indicate receipt of public aid. Receiving assistance from Head Start Programs is defined as currently using these types of care arrangements for a child or reporting having done so since a child's first birthday. The public housing measure indicates whether a respondent reported living in a public housing project during any of the three interviews. Finally, the TANF measure indicates whether a respondent received TANF at the Year 1 or Year 3 Follow-up.[7]

Two additional variables measure variation in state TANF policies. The first measures generosity of the state maximum benefit adjusted for cost of living differences across states. A second variable indicates restrictiveness of state TANF programs based on three program rules. To create an additive Paternalism Index, we assigned states a value of one for work requirements greater than federal requirements, a value of one for time limits shorter than federal requirements, and a value of zero to three to capture the stringency of sanctions for noncompliance. To facilitate comparison, we collapsed the resulting zero to five index into a dummy variable, with a value of one given to states that have a value of three or higher.

The FF data include geographic identifiers that allow us to control for features of social, political, and economic context that are likely to be associated with political participation (Cho, Gimpel, and Dyck 2006; Huckfeldt 1979, 1986; Leighley and Vedlitz 1999).[8] We capture a respondent's proximate economic context by the percent of families

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below the poverty line at the tract level and the unemployment rate in the Metropolitan Statistical Area (MSA). Broader socioeconomic context is indicated by state-level measures: percent below the poverty line and percent African American.[9] Political institutional context is captured by two measures: interparty competition and ease of state voter-registration laws. We measure the latter by a three-item additive index that gives one point for each of three procedures thought to ease the process of electoral registration: motor voter laws, late closing date for registration, and mail registration. We add a dichotomous indicator for the South (as defined by the Census Region South) to account for unobserved regional differences, including those related to political culture.[10]

ANALYTIC STRATEGY

We estimate multivariate models predicting our outcome variables, using binary logistic regression for the dichotomously coded outcomes and ordered logistic regression for *degree of engagement.* After analyzing relations associated with each type of public assistance, we test models that distinguish between more and less paternalist state TANF designs and examine their effects as interaction terms in our fully specified model. We conclude with a stringent final test, in which we model the effects of all three types of program participation simultaneously to assess the independent effects of each on political and civic participation.[11]

RESULTS

Descriptive statistics for the sample are displayed in Table S1 of the online supplement (http://asr.sagepub.com/supplemental). Respondents are young parents with an average age of 30, relatively low levels of education (57 percent of the sample attained a high school diploma or less), and an average household income less than two and a half times the poverty line. The sample also has relatively high rates of material hardship (69 percent of the sample reported at least one hardship), as well as higher rates of criminal conviction (7 percent) and substance abuse (24 percent) than would be found in the U.S. population. Receipt of government assistance is also high, as one would expect in a low-income sample experiencing a major transition (the birth of a baby). Twenty percent reported receiving TANF, 19 percent lived in public housing, and 7 percent of the sample participated in Head Start.

Figure 1 presents the participation rates for respondents receiving each type of public assistance, for the full sample, and for respondents not participating in TANF, public housing, or Head Start programs.[12] As expected, respondents who did not rely on these three programs have high rates of participation for all three outcomes—voting, political participation, and civic participation. Respondents who used Head Start exhibit higher levels of all three types of participation, relative to participants in the other two programs. Compared with the full sample (which includes 52 percent who received no aid at all), this difference is small but consistently positive. At this initial descriptive level, TANF recipients do not exhibit lower rates of participation than do those in public housing, even though both groups have significantly lower levels of participation than the sample as a whole or those receiving Head Start.[13]

Multivariate Regression Results

Table 1 presents models of the relation between TANF receipt and political and civic participation. Looking across the models, we find a negative association between TANF receipt and all four participation outcomes. The odds ratio of .85 for TANF receipt, for example, indicates a 15 percent decrease in the odds of voting, holding all other factors constant. As expected, we find that demographic factors, measures of individual life conditions, and contextual factors all

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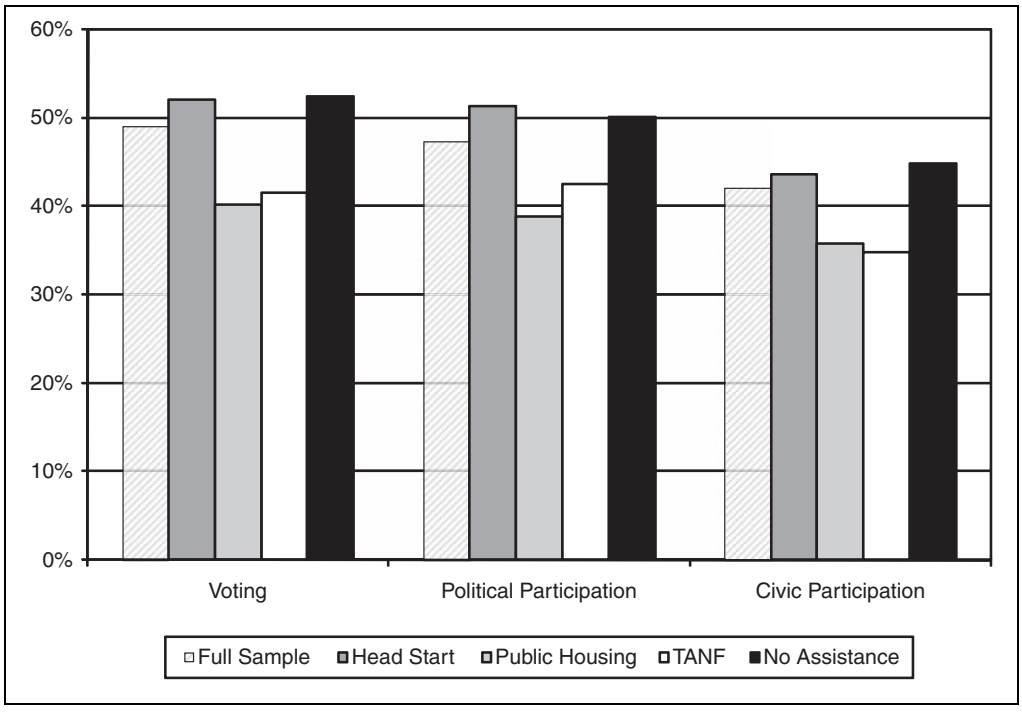


Figure 1. Political and Civic Participation by Type of Government Assistance

significantly affect participation outcomes among the poor. Yet the negative effects of TANF receipt remain discernible in all four models after controlling for these effects.

Consistent with our fifth hypothesis, ease of voter registration is associated with a significant increase in the odds of voting in this low-income sample, holding all other variables constant. In addition to confirming the importance of registration rules for low-income voters (Avery and Peffley 2005; Piven and Cloward 1988), this result provides a helpful point of comparison for assessing the feedback effects observed here. The 15 percent decline in odds associated with experiences in TANF is almost as large as the 17 percent decline in odds associated with more restrictive registration laws.

Table 2 compares results of interest from fully specified models for each type of government assistance, analyzed separately, and for each participation outcome. (The coefficients for TANF participation in the first row reproduce results from Table 1. See Tables S2 and S3 in the online supplement for full model results for public housing and Head Start.) In the first column, the outcome is voting, and the odds ratios indicate the effect associated with each program type, analyzed sequentially in separate models.

Unlike TANF participation, public housing and Head Start participation have no significant effects on the likelihood of voting. Similar results emerge when we turn to our broader measure of political participation, which includes not only voting but also participating in a political demonstration or a politically oriented group. TANF participation is associated with a 15 percent reduction in the odds of participating in at least one of these political activities. By contrast, the two means-tested programs that do not exhibit paternalistic authority relations have no effects on voting or broader forms of political engagement.

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Table 1. Odds Ratios of All Outcomes with TANF

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
Type of Government Assistance				
TANF	.849*	.849*	.843*	.860*
	(.062)	(.061)	(.060)	(.055)
Demographics				
Female	1.322**	1.278**	.926	1.023
	(.093)	(.089)	(.060)	(.061)
Has college-educated parent	1.495**	1.538**	1.274**	1.502**
	(.109)	(.111)	(.092)	(.099)
African American	1.728**	1.699**	1.439**	1.606**
	(.121)	(.118)	(.091)	(.093)
Latino	1.015	1.088	1.019	1.047
	(.125)	(.131)	(.108)	(.101)
Noncitizen		.072**	.641**	.258**
		(.011)	(.067)	(.025)
Age	1.053**	1.053**	1.034**	1.050**
	(.005)	(.005)	(.004)	(.004)
Education	1.618**	1.608**	1.389**	1.592**
	(.053)	(.052)	(.041)	(.043)
Married	1.190*	1.177*	1.663**	1.528**
	(.080)	(.076)	(.099)	(.084)
Income-to-poverty ratio	1.071**	1.079**	1.018	1.039**
	(.016)	(.019)	(.010)	(.012)
Social Marginality				
Material hardships	.984	1.012	1.098**	1.072**
	(.014)	(.014)	(.015)	(.013)
Conviction	.457**	.501**	1.039	.741**
	(.054)	(.056)	(.106)	(.069)
Substance use	1.021	1.047	.764**	.851**
	(.068)	(.068)	(.047)	(.047)
Not living with focal child	.680**	.795*	.938	.871
	(.069)	(.076)	(.084)	(.072)
Moved between T1 and T3	.740**	.755*	.790*	.767*
	(.085)	(.085)	(.087)	(.078)
Domestic violence	.849	.826	.945	.877
	(.122)	(.115)	(.083)	(.091)
Economic Conditions				
Percent families in poverty (tract)	1.816*	1.925**	1.512	1.727**
	(.434)	(.457)	(.342)	(.351)
MSA unemployment	1.088**	1.080**	.887**	.967
	(.029)	(.028)	(.022)	(.021)
Political Conditions				
Ease of state registration laws	1.166**	1.161**	1.137**	1.169**
	(.063)	(.061)	(.056)	(.052)
Party competition	2.145**	1.921*	1.906**	1.921**
	(.564)	(.483)	(.451)	(.410)
South	.634**	.621**	.964	.763**
	(.055)	(.053)	(.078)	(.055)

(continued)

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Table 1. (continued)

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
Social Conditions				
Percent state population in poverty	.964**	.975	.989	.985
	(.014)	(.013)	(.013)	(.011)
Percent state pop. African	1.008	1.008	.998	1.002
American	(.008)	(.008)	(.007)	(.006)
LR chi^2 (df)	1271.92 (22)	1842.31 (23)	748.21 (23)	1722.79 (23)
Pseudo R^2	.14	.18	.07	.10

Note: N = 7,529. Noncitizens are excluded from voting models (N = 6,774). Values represent coefficients (log odds) from logistic regression converted to odds ratios for ease of interpretation. Standard errors are in parentheses. Model statistics provided from imputed dataset #1.
*$p < .05$; **$p < .01$ (two-tailed tests).

Turning to effects on civic participation (Column 3 in Table 2), we again find that TANF receipt is associated with a significant reduction in engagement—a 16 percent reduction in the odds of participating in any civic organization. By contrast, Head Start participation is associated with a 23 percent increase in the odds of a parent participating in a civic organization. This pattern repeats in Column 4. Consistent with our first three hypotheses, overall levels of civic and political participation are significantly diminished by participation in TANF, significantly enhanced by involvement with Head Start, and unaffected by receipt of public housing benefits. Figure 2 displays effect sizes for TANF and Head Start relations shown in Table 2.

Head Start's positive effects on civic engagement are consistent with research suggesting that Head Start involvement positively affects parents in areas as diverse as employment, engagement with children's schools in later years, levels of psychological distress, feelings of mastery, and life satisfaction (see Parker, Piotrowski, and Peay 1987; Pizzo and Tufankjian 2004). Indeed, studies find that positive social and economic outcomes for Head Start parents are significantly greater among parents with children enrolled at high-involvement centers where parental participation is greatest (Oyemade, Washington, and Gullo 1989). Our finding for Head Start is also consistent with interview-based research suggesting that Head Start experiences are associated with more positive views of participation and higher levels of political efficacy (Soss 2000). We do not find a direct effect of Head Start experiences on political participation, but as Verba and colleagues (1995) demonstrate, civic involvement positions individuals in ways that should facilitate political recruitment and reduce barriers to future political engagement.

In summary, our analysis points to TANF's negative effects on political and civic participation, a positive association between Head Start and civic participation, and a null finding for receiving housing assistance.[14] As a whole, these findings confirm our hypotheses about how the structure of authority relations in means-tested programs matters for civic and political involvement. Results suggest that the distinction between universal and targeted programs may sometimes be a misleading guide to feedback effects in target groups. Depending on how they structure authority relations, targeted programs can have positive, negative, or null effects on civic and political engagement. Design differences appear to matter greatly. To strengthen this evidence, we pursue a simultaneous test of the feedback effects indicated so far.

Low-income individuals often receive assistance from multiple programs at a time. If experiences with different policy designs have political consequences, one

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Table 2. Odds Ratios of Political and Civic Participation by Receipt of Government Assistance Taken Separately

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	.849*	.849*	.843*	.860*
	(.062)	(.061)	(.060)	(.055)
Head Start	1.155	1.146	1.233*	1.285**
	(.122)	(.120)	(.122)	(.115)
Public Housing	.910	.906	.941	.906
	(.068)	(.066)	(.065)	(.056)

Note: N = 7,529. Voting models include citizens only (N = 6,774), other models include a variable for citizenship. Values represent coefficients (log odds) from logistic regression converted to odds ratios for ease of interpretation. Standard errors are in parentheses. Each odds ratio is taken from models estimated separately where each outcome is predicted with only one type of receipt. Full model results available in Table 1 (TANF) and Tables S2 (Public Housing) and S3 (Head Start) in the online supplement.
*$p < .05$; **$p < .01$ (two-tailed tests).

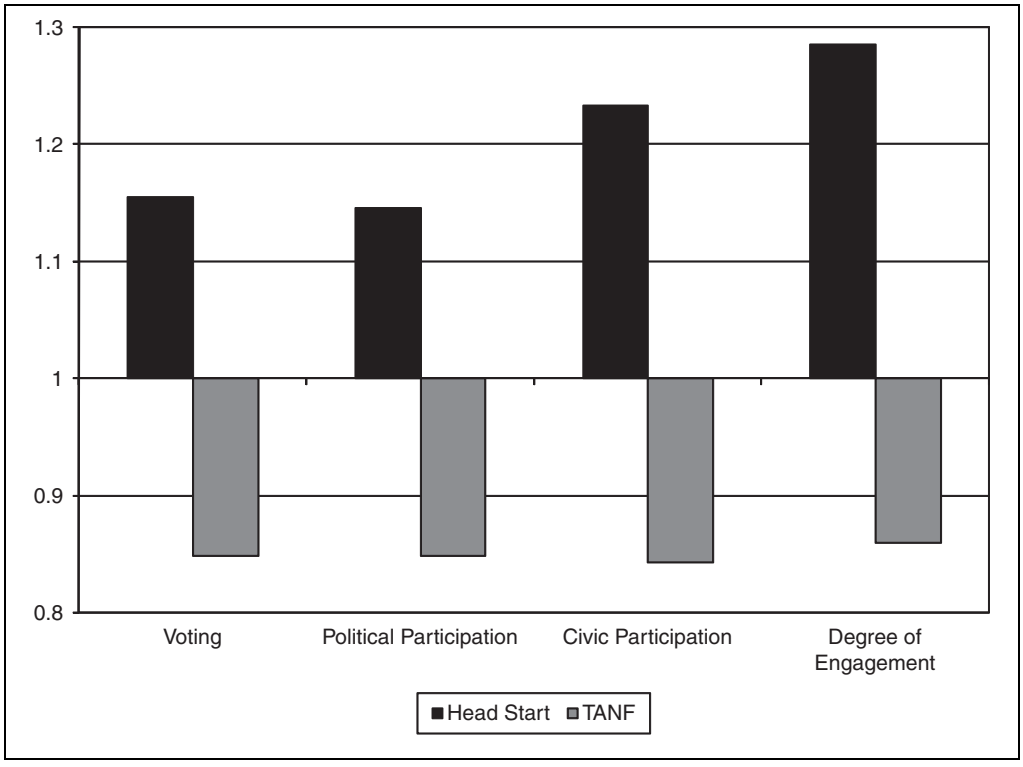


Figure 2. Odds Ratios of Participation Outcomes
Note: These odds ratios correspond to the model results shown in Table 2. The odds ratios of Head Start for voting and political participation are not statistically significant but are in the expected direction and nearly as large.

should be able to discern their separate effects in an analysis that considers multiple programs simultaneously. Indeed, a simultaneous analysis provides a tough test because

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it demands that the effects of each design be robust enough to emerge when the same individuals are participating in multiple programs. In our sample, 32 percent of respondents who received TANF, Head Start, or public housing received two or more types of assistance.

The results, shown in Table 3, strongly corroborate our earlier analyses. Participation in public housing has no discernible effect. By contrast, Head Start participation has a positive effect on civic engagement, while TANF participation has a negative effect on political and civic engagement. Comparing these coefficients, we find that Head Start's positive civic effects are large enough to counterbalance the negative effects of TANF experiences. Among respondents enrolled in both programs (3 percent of the total sample, 16 percent of TANF recipients), the two effects cancel each other out, leaving a statistically insignificant net impact that is close to 0. Our ability to discern these cross-cutting effects in a simultaneous model underscores not only the robustness of feedback effects associated with each program but also the key substantive point that means-tested programs for the poor can differ dramatically in their civic effects.

Finally, if experiences with authority relations in welfare programs influence civic and political engagement, then the effects of TANF participation should vary across states with more versus less paternalist TANF designs (as indicated by work requirements, time limits, and sanctions). Table 4 presents select results from models that test whether feedback effects vary significantly across state TANF programs.[15] In addition to the variables included in our other models, we control here for interstate differences in TANF benefit generosity to more precisely estimate the effects of paternalist design features. (Full results are presented in Table S5 of the online supplement.) The results of interest are for two dummy variables indicating participation in either a high- or a low-paternalist state TANF program. Respondents who did not receive TANF benefits provide the baseline category.

The results are unequivocal. Across all four outcome variables, we find that the effects of TANF participation on political and civic engagement depend on the degree of paternalism in a state's TANF program. In TANF programs that structure authority relations in a strongly paternalist manner, program experiences are associated with significantly lower odds of all forms of civic and political engagement. By contrast, experiences with less paternalist TANF programs produce no discernible effects on the outcomes analyzed here.[16] It appears that the negative effects of welfare receipt reported in our earlier models were driven primarily by experiences with more paternalist TANF designs. Contrary to paternalists' claims, directive and punitive welfare programs do not seem to hasten civic and political incorporation; instead, they undermine them.

DISCUSSION AND CONCLUSIONS

Our results provide the strongest evidence to date that social-welfare program designs can influence levels of civic and political engagement among the poor. We find robust effects associated with program experiences across a range of outcomes, even after controlling for an unusually broad set of demographic factors, indicators of social marginality, and features of social, economic, and political contexts. The Fragile Families dataset offers a large and geographically diverse sample of urban, low-income Americans and provides a far richer set of control variables than one usually finds in behavioral studies of policy feedback. As one would expect, our results do not suggest that the effects of means-tested policies are as large as those associated with factors that stand at the center of political participation literature, such as educational attainment (Verba et al. 1995). Yet the feedback effects of program

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Table 3. Odds Ratios of Political and Civic Participation by Receipt of Multiple Types of Government Assistance

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	.851*	.852*	.838*	.859*
	(.063)	(.062)	(.060)	(.055)
Head Start	1.179	1.171	1.259*	1.312**
	(.125)	(.123)	(.125)	(.118)
Public Housing	.927	.922	.957	.918
	(.070)	(.068)	(.067)	(.058)

Note: N = 7,529. Voting models include citizens only (N = 6,774), other models include a variable for citizenship. Values represent coefficients (log odds) from logistic regression converted to odds ratios for ease of interpretation. Standard errors are in parentheses. Full model results are available in Table S4 in the online supplement.
*$p < .05$; **$p < .01$ (two-tailed tests).

Table 4. Odds Ratios of Political and Civic Participation by Receipt of TANF in States with High and Low Paternalism Index Scores

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
Receiving – High	.802*	.789*	.689**	.720**
	(.076)	(.074)	(.064)	(.059)
Receiving – Low	.903	.916	1.028	1.023
	(.086)	(.086)	(.093)	(.083)

Note: N = 7,529. Voting models include citizens only (N = 6,774), other models include a variable for citizenship. Models also include a measure of TANF benefit generosity. Values represent coefficients (log odds) from logistic regression converted to odds ratios for ease of interpretation. Standard errors are in parentheses. Full model results available in Table S5 in the online supplement.
*$p < .05$; **$p < .01$ (two-tailed tests).

experiences emerge here as robust and stable, with more than a trivial impact on civic and political incorporation.

With these results in hand, we now return to the three outstanding questions identified at the start of our analysis. Our findings are hard to square with the claim that selection bias accounts for the apparent link between welfare experiences and political participation. Robust effects emerge consistently across multiple outcomes in our analysis, and they do so in precisely the pattern suggested by political learning approaches to policy feedback. These findings are bolstered by a host of factors that run counter to a selection-bias interpretation: our sample is restricted to disadvantaged populations; the programs compared here all restrict beneficiaries on the basis of means-testing; and our findings across programs are corroborated by a within-program analysis of effects associated with different state TANF designs. Skeptics may maintain that different populations select into Head Start and TANF programs, but this objection has far less traction for analyses that focus solely on TANF and compare more versus less paternalist designs. Indeed, even if differences in TANF designs give rise to small amounts of selection bias, our analysis uses direct measures to control for precisely the kinds of factors that might distinguish TANF populations: for example, substance abuse, domestic violence, material hardships, criminal convictions, and living apart from one's child.[17] No analysis of observational data can definitively settle questions of selection bias,

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but our results make it exceedingly difficult to claim that feedback effects are simply an illusion created by selection effects.

Our findings also challenge the idea that positive feedback effects flow solely from universal social policies; of course, they say little about whether the virtues of universal design have been overstated. We offer no comparisons to such designs, and we are unable to assess the size of Head Start's positive effects relative to those of, say, Social Security or the G.I. Bill (Campbell 2003; Mettler 2005). There continue to be good reasons to believe that universal designs may generate positive civic and political effects that targeted designs do not. But the results presented here argue against blunt distinctions between universal and targeted programs that ignore design differences within categories. Means-tested programs vary in their civic and political effects, with some producing significant positive outcomes. The negative effects of paternalist TANF programs stand in contrast to the null effects of public housing assistance and the positive effects of Head Start. Low-income individuals who experience the incorporating design of Head Start are more likely to become engaged in civic organizations and tend to participate in all civic and political activities analyzed here at higher levels. Although we do not find direct evidence of an effect on political participation measured alone, evidence suggests that, as a result of the civic effects of Head Start experiences, these individuals are better positioned for recruitment into political action (Verba et al. 1995).

Our findings are consistent with a perspective on policy feedback that emphasizes political learning and the lessons citizens draw from their direct experiences of authority in government programs (Soss 2000). Rather than emphasizing targeting per se, this perspective focuses on how policy designs structure authority relations between citizen-clients and government officials. Insofar as the designs of authority relations vary systematically across targeted and universal programs, this perspective fits with "two-channel" analyses of policy feedback in the U.S. welfare state. In cases where this dominant pattern is broken, however, as in Head Start, it appears that programs targeted at the poor can produce significant positive effects on civic incorporation. Indeed, the convergence of our survey-based results and earlier results from interpretive field research (e.g., Soss 2000) allows for considerable confidence that targeted programs can produce positive or negative feedback effects depending on how they structure authority relations.

Finally, our results run counter to the new-paternalist claim that civic and political incorporation are facilitated by more directive and supervisory welfare designs (Mead 1986, 2005). Our evidence suggests that experiences with paternalist TANF designs significantly depress civic and political involvement. These negative effects are driven primarily by the most paternalist designs: they wash out in states that pursued less paternalist TANF designs, and they stand in stark contrast to the null effects of public housing and the positive effects of Head Start. As a holdover from the "maximum feasible participation" era of the 1970s, Head Start continues to engage parents in participatory, site-based policy councils. By contrast, welfare reform has institutionalized a strongly hierarchical model of state authority, in which recipients are positioned as objects of official direction, surveillance, and punishment (Hays 2003; Soss et al. 2008). Our findings contradict the paternalist argument that we will "make citizens" most effectively by "telling the poor what to do" (Mead 1998, 2005). To the contrary, our results support theoretical claims that meaningful experiences of democratic relations promote broader engagement in activities of democratic citizenship (Pateman 1970; Schneider and Ingram 1997).

Looking to the future, our analysis highlights two critical issues for students of policy feedback and political behavior. First, individuals are likely to experience a variety of public policies, simultaneously and over the life

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course, that differ in their authority structures and in the lessons they impart. Researchers cannot assume that these lessons will be consonant simply because status (disadvantaged or advantaged) guides individuals toward a particular category of public programs. Instead, we must ask how policy experiences intersect in citizens' lives as sources of political thought and action. In so doing, we should pay particular attention to how citizens respond to conflicting policy experiences—how the divergent lessons they draw may coexist as elements of ambivalent political orientations or, alternatively, how the lessons may be weighted or knit together as complex narratives that allow for summary judgments.

Second, the restricted age range of the Fragile Families sample—young parents of very young children—suggests a need for caution when generalizing our findings to older cohorts. More importantly, however, it raises important questions of timing and sequence that have long concerned students of political learning and socialization. Are citizens more susceptible to policy-based learning at some points in the life course than at others (Sears 1990)? Are we, for example, more likely to observe feedback effects on political attitudes and behaviors when individuals experience policies during their younger, impressionable years (Niemi and Hepburn 1995)? Likewise, do earlier policy experiences have greater weight because they structure interpretations of later experiences, or do recent experiences provide more salient cues that override policy-based lessons of the past (Sapiro 1994)? To ask these questions is to highlight the newness of policy feedback studies, the complexity of policy-based learning processes, and the need for longitudinal analyses of citizens' political development.

Finally, our findings raise important questions, not just for scholars, but also for citizens and policymakers. When public policies produce unintended consequences, empirical research can help pull negative effects of well-intentioned policy designs out of the shadows and into the light of political deliberation (e.g., Moffitt 1992). When this happens, attentive members of the political community have good reasons to reevaluate their policy-design preferences in light of a broader range of goals and values. As evidence for policy feedback effects grows stronger, we are approaching the point where difficult questions must be addressed more squarely. How much do Americans care about whether the civic and political lives of our communities include the least advantaged among us? How much priority should we place on democratic values relative to other values that guide our social policy designs?

Little is known about how the pursuit of civic and political incorporation would influence the achievement of other policy goals in public assistance programs. The potential for synergies or trade-offs has received little scrutiny. Instead, the turn toward paternalism over the past two decades reflects an assumption that positive outcomes of all stripes will emerge if policies are designed to ''send a clear message'' about the types of behaviors expected from citizens. Our analysis raises serious questions about this assumption. Whatever their effects may be for outcomes related to work and family (Grogger and Karoly 2005; Handler and Hasenfeld 2007), paternalist designs have significant negative consequences for civic and political engagement among the disadvantaged. As findings of such effects accumulate, the legitimacy of treating them as ''unintended'' should rightly wane. In designing or reforming welfare policies, we confront choices that have real consequences for the vitality and inclusiveness of civic and political life. It is time we faced up to them.

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Notes

1. An alternative to authority relations as a causal mechanism is free time. In this view, individuals participating in more paternalist—and hence, demanding—TANF programs may simply have less time to participate in civic and political activities. This ''free time effect,'' however, does not strike us as a convincing alternative for several reasons. First, like the group-dynamics mechanism, it lacks the empirical support that interpretive field studies provide for the authority-relations mechanism. Second, although it seems logical that TANF programs that require a larger number of work-activity hours leave less free time for civic and political engagement, the number of required work-activity hours is not employed as a component of our state TANF paternalism index, and the limited available evidence suggests that the number of required work activity hours does not vary greatly across state TANF programs. Third, setting aside the question of variance in required work participation hours, we know of no evidence indicating that clients spend more time on program activities (or have less free time in general) in states that have adopted more paternalist program rules on other dimensions (e.g., sanctions). We see little basis for assuming that clients have less time available if they participate in a program with a more paternalist authority structure. Fourth, as Verba and colleagues (1995) emphasize, time has a complicated (and in their study, inconsistent) relation to political participation. In many cases, people with less free time are more likely to be engaged in politics (that is why they have so little time). Even if we set aside doubts about the relation between free time and TANF program paternalism, it is not clear that differences in free time explain why some low-income people are more likely to participate in politics than others.
2. The data for our analysis come primarily from the Year 3 Follow-up interview, when respondents were asked questions related to political and civic participation. Our measures of program participation, however, are drawn from all three waves.
3. We dropped respondents absent from the Year 3 Follow-up because this is the only wave that includes the political and civic participation questions (2,266 dropped). We dropped one additional respondent to avoid a weak imputation of state values for the District of Columbia. We conducted multiple imputation with STATA's ice program using all variables included in the models to create five imputed datasets. Table S1 in the online supplement (http://asr.sagepub.com/supplemental) provides sample descriptives with and without the imputation of missing values. We also ran all analyses without imputation using listwise deletion, and none of the substantive results differ across analyses using the two strategies for dealing with missing data.
4. The analyses for this project do not use weights provided with the FF data because the unit of analysis underlying these weights is the family, while our unit of analysis is the adult individual. Weights for the FF data are designed to make the sample representative of cohort births and parent couples in the 77 U.S. cities with populations over 200,000 at the time of the Baseline, Year 1, and Year 3 surveys. No available weight can be applied to make the FF data approximate a random sample of adult individuals in the United States, or even in the 77 cities. Moreover, the use of inappropriate weights would skew our analysis of state-contextual effects because the sampled cities are not geographically representative of all U.S. cities with a population over 200,000.
5. More detailed descriptions and documentation of all measures appear in the online supplement, Part C.
6. All individual-level predictors are from Year 3; for respondents missing values in Year 3, we used their Baseline or Year 1 values.
7. We exclude TANF receipt reported at Baseline because TANF and food stamps were not distinguished in this wave. However, analyses including these recipients produce substantively identical, statistically significant results, indicating that the analysis is robust to this specification.
8. We took each state and city factors for the location where a respondent lived at the time of the Baseline interview.
9. We measure all but two of the contextual factors at the state level because they serve as controls in an analysis of state policy effects. We measure tract poverty and MSA unemployment at lower levels of aggregation because theory and empirical research suggest the need to do so. We use tract poverty because it is as close as we can get to the neighborhood poverty effects emphasized by students of poverty and political participation (see Cohen and Dawson 1993). We measure unemployment at the MSA level because it corresponds to the labor market boundaries that, in most cases,

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define people's opportunities for employment (see Fernandez and Su 2004).

10. All reported results are robust to the replacement of our South indicator with a measure of state government ideology that varies continuously across states in our sample. Unlike the indicator for South, however, the ideology measure does not produce a significant coefficient.
11. Because of the FF data's nested structure, our observations are not wholly independent. In many cases, the FF observations are couples, nested within tracts, nested within cities, nested within states. Three methods that can account for nonindependence are clustered robust standard errors, survey estimation techniques, and multilevel modeling. It is not possible to use clustered robust standard errors or survey estimation techniques with our models because of the large number of predictors versus the number of clusters. A multilevel model is inadvisable in the first two clusters (couples and tracts) because of the small number of observations in the clusters, and it is inadvisable at the city level because of the limited number of cities in relation to the number of predictors in the models. We do not use fixed-effects models for cities because they do not allow estimation of political, institutional, and policy factors. For further discussion of the alternative methods of adjusting for nonindependence, and model results obtained using these methods, see the online supplement, Part B.
12. Respondents not assisted by TANF, public housing, or Head Start programs include individuals receiving no government assistance and those receiving some other form of government aid, such as nutritional assistance from the Women, Infants, and Children (WIC) program.
13. The descriptive results presented for voting exclude noncitizens.
14. In the FF dataset, men make up a substantial minority of TANF recipients but constitute a very small minority of Head Start recipients. To check for gender composition effects, we reran our models for women-only samples. All results are robust to this specification.
15. In Table 4, high-paternalist state TANF programs include CT, DE, FL, GA, IN, IA, LA, MA, MI, NC, OH, SC, TN, TX, VA, and WI. Low-paternalist state TANF programs include CA, IL, KY, MD, MS, NJ, NY, and PA.
16. These results are robust to two alternative specifications. First, we obtained identical results with separate models that analyzed samples stratified by TANF regime type (i.e., a model limited to respondents from states with high-paternalist TANF programs and a separate model limited to respondents from states with low-paternalist TANF programs). Second, we tested a model that included an interaction term for TANF paternalism*TANF benefit generosity. The interaction was statistically insignificant, indicating that greater benefit generosity does not significantly mitigate the effects of experiencing a highly paternalist TANF program.
17. Moreover, if differences in state TANF designs do affect patterns of selection into TANF, the direction of this bias is unclear. If tougher rules and penalties disproportionately deter the least motivated and efficacious individuals, as one might assume, then highly paternalist TANF designs should raise the proportion of welfare recipients who are *more* likely to participate in civic and political activities. This pattern is, of course, precisely the opposite of what we find in our multivariate analyses.

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