

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

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FROM POLICY TO POLITY

This article investigates how experiences with public policies affect levels of civic and political engagement among the poor. Previous studies suggest that direct encounters with welfare institutions can produce “feedback effects” on political attitudes and behaviors that vary by policy design. To advance this literature, we take up three outstanding questions related to problems of selection bias, the distinction between universal and targeted programs, and the types of authority relations most likely to foster engagement among the poor. Taking advantage of a dataset with unique benefits for the study of feedback effects among low-income populations, we estimate effects associated with three types of means-tested public assistance. We find that the feedback effects of these policies are not an illusion created by selection bias; the feedback effects of targeted 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.

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

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 handful of decades ago (Freeman 2004). The deep political marginality of the poor in the U.S. today is a constructed outcome. It 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 the civic organizations that recruit people into politics and little access to the trade unions that work to engage their European counterparts (Radcliff and Davis 2000). The poor are less likely to be strategically targeted for political mobilization by parties and candidates (Rosenstone and Hansen 1993), and they are 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 institutional restrictions such as felony disenfranchisement laws (Uggen and Manza 2006), citizen-initiated 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 an immovable object. 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 governments 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 both the consensus and conflict traditions of sociology. T.H. Marshall (1964), for example, suggested that a liberal-democratic solution to the problem of “citizenship and social class” requires the establishment of solidaristic social rights. By institutionalizing these rights, Marshall argued, 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) placed equal emphasis on the welfare state as a “secondary institution” mediating the tensions between states and markets. In their account, relief programs play a key role in regulating the political and economic behaviors of the lower classes. Welfare policies, they suggest, have the potential to shape power relations, foster political contention, and cultivate political quiescence.

Despite the importance of these arguments for sociology over the past half century, scholars have only recently begun to conduct systematic empirical studies of how welfare policies affect civic and political engagement. Over the past decade, studies of “policy feedback” have increasingly investigated policies, not just as political outcomes, but also as factors that set political forces in motion and shape political agency in the citizenry (Pierson 1993; Svallfors 2007). Within this body of research, a number of studies suggest that welfare policies can deepen or ameliorate the political marginality of disadvantaged groups, depending on how they are structured (Mettler and Soss 2004). Welfare programs distribute resources that can facilitate political action (Verba, Schlozman, and Brady 1995). They create incentives to participate by giving recipients a self-interested stake in defending program benefits (Campbell 2003). And

they provide direct experiences of government that can teach significant lessons about power, identity, and the desirability of exercising political voice (Mettler 2005; Soss 2000).

Today, a growing number of studies suggest that citizens' experiences in social welfare programs can have significant political consequences (Mettler and Stonecash 2008; Kumlin and Rothstein 2005; Kumlin 2004; Lawless and Fox 2001). Yet there is little agreement about whether such feedback effects have been adequately distinguished from pre-existing 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. In what follows, we seek to 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.

Policy Feedback among Disadvantaged Target Groups: Outstanding Questions

A number of studies suggest the potential for feedback effects in low-income populations. Verba, Schlozman, and Brady (1995), for example, find that participants in means-tested programs are less likely to engage in political acts such as voting, campaigning, and contacting public officials, while social insurance participants are significantly more likely to become politically engaged. Building on this work, Andrea 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. In interpreting these results, Campbell emphasizes how 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, Schlozman, and Brady 1995). From this perspective, social policies should mitigate the political disadvantages of the poor most effectively when they offer higher benefits and give rise to organizations that offset the costs of participation. In the U.S., 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 have the power to teach lessons about group status, government responsiveness, and the efficacy and wisdom of exercising voice as a citizen (Soss 2005). In this regard, several studies point to the importance of how policy designs structure authority relations.

Under the G.I. Bill, for example, Suzanne Mettler (2005: 85) finds that 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, Joe Soss (2000) finds that distinctive authority structures encouraged 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 the dampening of political engagement occurs primarily when clients develop negative views of their interactions with welfare officials (Lawless and Fox 2001), and grows deeper 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 come a long way over the past decade. As empirical evidence and conceptual sophistication have grown, however, important new questions have emerged concerning methodology, interpretation, and theory. Three areas of scholarly disagreement strike us as especially worthy of attention.

1. Selection Bias. Studies that link program experiences to political behavior have typically relied on one of three analytic strategies: interpretive analyses of client statements that link program 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* depress 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, such as experiences with substance abuse or domestic violence, that might be expected to promote welfare reliance and dampen civic engagement. This problem is compounded by the relatively small number of program recipients included in these datasets. 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 pre-existing differences across relevant subpopulations – remains an open and contested question.

2. Design Variation: To date, most of the studies reporting positive feedback effects have focused on more universal policies serving broad constituencies, such as the G.I. Bill (Mettler 2005) and Social Security (Campbell 2003). By contrast, the most negative effects have been associated with programs targeted at the poor, such as AFDC (Soss 2000; Mettler and Stonecash 2008). As a result, research in this area has often been assimilated into arguments about the political inferiority of targeted programs and the democratic benefits of universal programs (Skocpol 1991; Campbell 2007). 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” – which, in turn, reflect contrasts based on systems of gender, class, and racialized ethnicity (Weir, Orloff, and Skocpol 1988; Orloff 1996; Gordon 1994, 2002; Quadagno 1996; Mink 1998; Lieberman 2001).

To be sure, these contrasts highlight political dynamics that have 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). In contrasting the feedback effects of universal and targeted programs, scholars 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 U.S. have long been decentralized (Mettler 1998), a single aid program may have different designs in different states (Soss, Fording, and Schram 2008).

Thus, when it comes to policy feedback, it may well be that “programs for the poor are not always poor programs” (Howard 2006). In previous research, recipients of means-tested benefits have usually been lumped together, either by combining diverse categories of public aid (Verba et al. 1995; Mettler and Stonecash 2008) or by combining beneficiaries from states with markedly different policy designs (Soss 2000; Campbell 2003). In so doing, researchers may have obscured important relationships. By more carefully delineating target groups and policy differences, we can 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 have suggested 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 Lawrence Mead (1986; 2005), argue that social disorder and weak self-discipline leave the welfare poor unable to shoulder the burdens of civic obligations. Thus, civic and political incorporation are ill-served by “permissive” rights-oriented welfare programs. They

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 (Soss 2000; Mettler 2005).

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 the 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* 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 (a) the assertion that civic incorporation of the welfare poor requires bypassing liberal-democratic values in favor of hierarchical designs that emphasize direction, supervision, and penalty and (b) the assertion that such paternalist designs deepen civic marginality while civic incorporation will be fostered by more democratic designs that enable recipients to participate in decision processes and check the arbitrary exercise of authority.

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 differ in their authority structures yet all serve low-income populations. Second, we employ a quasi-national dataset that allows us to directly measure key personal characteristics that may distinguish welfare 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 show 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 & 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. In the analysis that follows, 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 divergent 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 governmental 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 (Weaver 2000; Mead 2004). 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 (Soss 2000; Schram et al. 2007). 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 several programs, such as Section 8 and Hope VI, as well as publicly and privately owned public housing units. 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 decision processes that affect us and the ability to check arbitrary exercises of authority over us (Shapiro 1999: 19-29). The incorporating design of Head Start lies on the positive end of this dimension; the paternalistic design of TANF lies on the negative end; and the formal-bureaucratic design of public housing occupies an intermediate position. In many respects, this comparison parallels the analysis offered by Soss (2000), who 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. These findings suggest a clear set of hypotheses, but the interpretive approach and small sample used by Soss provide a relatively weak basis for inferring and generalizing causal effects.

Accordingly, we seek 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. Thus, while we might expect resources distributed by public programs to generate effects as well, 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 softened this hard-line 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, Fording, and Schram 2008). In some states more than others, 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 non-compliance. If policy-based experiences with authority relations influence civic and political engagement, then TANF effects should vary significantly across such states. If pre-existing client characteristics drive participation differences, we should find few differences across states with more versus less paternalist TANF programs. 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 the institutional focus of scholars 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 inter-party competition varies widely across the states, influencing the strength and targeting of party mobilization efforts (Key 1949; Avery and Peffley 2005; Frymer 1999). If these sorts of 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.

Thus, we 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 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, the 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, relations with authorities have been empirically confirmed as an influence path in interview-based studies of policy feedback (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). Thus, 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 previously observed in these qualitative studies.¹

¹ A second 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. Such a “free time effect,” however, does not strike us as a convincing alternative for several reasons. First, like the group-dynamics mechanism discussed in the text, it lacks the empirical support that

Data

The Fragile Families and Child Wellbeing Study is 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 non-marital births in large U.S. cities, however 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 both mothers and fathers. Mothers and fathers of the focal child were re-interviewed when the child was 12-18 months (Year 1 Follow-up), and three years old (Year 3 Follow-up).²

The FF data include measures of participation in a variety of public assistance programs and in a number of broader civic and political activities. In addition to providing an unusually large sample of disadvantaged persons, the FF 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 that is

interpretive field studies have provided for the authority-relations mechanism. Second, although it may seem logical to assume 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 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 state program with a more paternalist authority structure. Fourth, as Verba, Schlozman, and Brady emphasize in *Voice and Equality* (2005), time has a complicated (and in their study, inconsistent) relationship to political participation. In many cases, people with less free time are more likely to be engaged in politics (as in other spheres – that's why they have so little time). Thus, even if we set aside doubts about the relationship 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. However, our measures of program participation are drawn from all three waves.

known to have low participation rates and to experience 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, Year 1, and Year 3 Follow-ups) 86% of the fathers were interviewed at least once, and 82% of mothers were interviewed at all three waves. The overall response rate for the Year 3 Follow-up was 77% (86% for mothers and 67% for fathers). The analysis sample is restricted to the 7529 respondents who were interviewed in the Year 3 Follow-up. Multiple imputation was utilized to deal with missing values.³ The FF sample is not representative of the US: it is drawn only from large metropolitan areas and includes an over-sample of unmarried births, and as a result is younger and more highly disadvantaged. Within these parameters, however, it should represent the propensity for political or civic participation among low-income targets of means-tested public assistance.⁴

Measures

Our analyses are based on four outcome measures: voting; political participation; civic participation; and a combined measure of overall engagement. *Voting* indicates whether the respondent reported voting in the November 2000 election. *Political Participation* is a dichotomy coded one if the respondent reported voting, participating in a political, civic, or

³ Respondents absent from the Year 3 Follow-up are dropped because this is the only wave that included the political and civic participation questions (2266 dropped). One additional respondent was dropped to avoid a weak imputation of state values for the District of Columbia. Multiple imputation was conducted with STATA's *ice* program using all variables included in the models to create five imputed datasets. . Table S1 in the online supplement provides sample descriptives with and without the imputation of missing values. All analyses were also run 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 make use of 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 U.S. 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 cities over 200,000 in the US.

human rights organization in the past 12 months, or taking part in a political demonstration or march. *Civic Participation* is also a dichotomy coded one for participation in any of the following in the past 12 months: a group affiliated with his/her church, 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 the 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. The variable is coded to have a range of 0-2, with the ordered categories identifying respondents who are entirely isolated from civic and political life (0), who have had the most minimal engagement (1), and who have been more fully incorporated (2, which indicates 2 or more).⁵

Our independent variables include several demographic predictors: whether the respondent is female; whether he or she has a parent who graduated from college; race/ethnicity; non-citizen status; age in years; educational attainment; marital status; and household income to poverty ratio (the 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, experiencing 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, these measures are included here to control for differences that could have produced selection bias in previous studies. Each is a dichotomous indicator, except for material hardship, which is a count of the number of

⁵ More detailed descriptions and documentation of all measures appear in Online Supplement C.

hardships reported by the respondent e.g. telephone service disconnected, electricity turned off, and gas/oil service turned off.⁶

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

Two additional variables measure variation in state TANF policies. The generosity of the state maximum benefit adjusted for the cost of living differences across states. A second variable indicates the 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 collapse the resulting 0-5 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 The proximate economic context of the respondent is captured by the percent of families below the poverty line

⁶ All individual level predictors were taken from Year 3, with those missing values in Year 3 given 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 produced substantively identical, statistically significant results indicating that the analysis is robust to this specification.

⁸ Each state and city factor was taken for the location where the respondent lived at the baseline interview.

at the tract level, and the unemployment rate in the Metropolitan Statistical Area (MSA). Broader socioeconomic context is indicated by state-level measures: the percent below poverty and the percent African American.⁹ Political institutional context is also captured by two measures: interparty competition and the ease of state registration laws. The latter is measured 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, late closing date, and mail registration. A dichotomous indicator for the South (as defined by the Census Region South) is also added 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 relationships associated with each of the 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

⁹ We measure all but two of the contextual factors at the state level because they are serving 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 both suggest the need to do so. Tract poverty is used because it is as close as we can get to the “neighborhood poverty” effects emphasized by students of poverty and by students of political participation (see e.g. Cohen and Dawson 1993). Unemployment is measured at the MSA level because it corresponds to the labor market boundaries that define, in most cases, people’s opportunity for employment (see e.g. 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 the states in our sample. Unlike the indicator for South, however, the ideology measure does not produce a significant coefficient.

¹¹ Because of the nested structure of the FF data, our observations are not wholly independent. The FF observations are in many cases couples, nested within tracts, nested within cities, nested within states. Three methods that can account for non-independence are: clustered robust standard errors; survey estimation techniques; and multilevel modeling. Utilizing clustered robust standard errors or survey estimation techniques are not possible 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 is

RESULTS

Descriptive statistics for the sample are displayed in Table S1 of the Online Supplement. The respondents are young parents with an average age of 30, relatively low levels of education (57% of the sample have 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% of the sample report at least one) as well as higher rates of criminal conviction (7%) and substance abuse (24%) than would be found in the U.S. population. Receipt of governmental 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% lived in public housing, and 7% 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 in Head Start exhibit higher levels of all three types of participation, relative to participants in the other two programs. Compared to the full sample (which includes 52% who receive 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 those in public

inadvisable at the city level because of the limited number of cities in relation to the number of predictors in the models. Fixed effects models for cities are not utilized because they do not allow political institutional and policy factors to be estimated. For further discussion of the alternative methods of adjusting for non-independence, and model results obtained utilizing these methods, see Online Supplement B.

¹² Those not assisted by TANF, public housing or Head Start programs include those 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.

housing, even though both of these 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 relationship between TANF receipt and political and civic participation. Looking across the models in Table 1, we find a negative association between TANF receipt and all four of our participation outcomes. The odds ratio of .85 for TANF receipt, for example, indicates a 15% 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 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, the 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 (Piven and Cloward 1988; Avery and Peffley 2005), this result provides a helpful point of comparison for assessing the feedback effects observed here. The 15% decline in odds associated with experiences in TANF is almost as large as the 17% 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 for full model results for public housing and Head Start.) In the first column, the

¹³ The descriptive results presented for voting exclude non-citizens.

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 participation and Head Start participation have no significant 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 politically oriented group. TANF participation is associated with a 15% 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.

Turning to effects on civic participation (Column 3), we find again that TANF receipt is associated with a significant reduction in engagement – this time a 16% reduction in the odds of participating in any civic organization. By contrast, Head Start participation is associated with a 23% *increase* in the odds of the parent participating in a civic organization. This pattern repeats itself in Column 4. Consistent with our first three major 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 all significant relationships shown in Table 2.

The positive effects of Head Start on civic engagement are consistent with a broader literature suggesting that Head Start involvement has positive effects on 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 Pizzo and Tufankjian 2004; Parker, Piotrowski, and Peay 1987). Indeed, some 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, Schlozman, and Brady (1995) have demonstrated, civic involvement positions individuals in ways that can be expected to facilitate political recruitment and reduce barriers to future political engagement.

In sum, our analysis points to negative effects of TANF on both 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. The 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 more than one program at a given time. If experiences with policy designs have political consequences, then one 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 it demands that the effects of each design be robust enough to emerge when the same individuals are participating in multiple

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.

programs. In this sample of the FF data, 32% of those who receive TANF, Head Start, or public housing receive two or more of these 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 both political and civic engagement. Comparing these coefficients, we find that the positive civic effects of Head Start are large enough to counterbalance the negative effects of TANF experiences. Among respondents enrolled in both programs (3% of the total sample, 16% of TANF recipients), the two effects cancel out to leave a net impact that is close to 0 and statistically insignificant. 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 in order 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 a highly paternalist state TANF program or a less paternalist state TANF program. Respondents who did not receive TANF benefits provide the baseline category.

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.

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 more strongly paternalist manner, program experiences are associated with significant decreases in the 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 Thus, 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 the claims of new paternalists, directive and punitive welfare programs do not appear to hasten civic and political incorporation; they appear to undermine them.

DISCUSSION & CONCLUSION

The results of our analysis 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 context. The Fragile Families dataset offers a large and geographically diverse sample of urban low-income Americans and provides a set of control variables that is far richer 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 the literature on political participation,

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 also 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.

such as individual educational attainment (Verba, Schlozman, and Brady 1995). Yet the feedback effects of program experiences emerge here as robust and stable relationships with more than a trivial impact on civic and political incorporation.

With these results in hand, we may now return to the three “outstanding questions” identified at the start of our analysis. The findings presented here 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: the sample for our analysis is restricted to quite 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 still maintain that different populations select into Head Start and TANF programs, but this objection has far less traction for analyses that focus solely on the TANF program 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: substance abuse, domestic violence, material hardships, criminal convictions, living apart from one’s child, and so on.¹⁷ No analysis of observational data can definitively settle questions of selection bias, but our results make it exceedingly difficult to claim that feedback effects are just an illusion created by selection effects.

¹⁷ Moreover, if differences in state TANF designs do affect patterns of selection into TANF programs, the direction of this bias is unclear. If tougher rules and penalties disproportionately deter the least motivated and efficacious individuals, as one might plausibly 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.

Our findings also challenge the idea that positive feedback effects flow solely from universal social policies. Our findings, of course, 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 able to produce 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, Schlozman, and Brady 1995).

The findings presented here 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. However, in cases where this dominant pattern is broken, 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 (Soss 2000) allows for considerable confidence that targeted programs can produce positive or negative feedback effects depending on how they structure authority relations.

Finally, the results presented here run counter to the new-paternalist claim that civic and political incorporation are facilitated by welfare designs that are more directive and supervisory (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 have 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, the Head Start program 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, Fording, and Schram 2008). Our findings contradict the paternalist argument that we will “make citizens” most effectively by “telling the poor what to do” (Mead 2005; 1998). To the contrary, our results support theoretical claims that meaningful experiences of democratic relations promote broader engagement in the activities of democratic citizenship (Pateman 1970; Schneider and Ingram 1997).

Looking to the future, we believe 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 course, that differ in their authority structures

and in the lessons they impart. Researchers cannot assume that these lessons will be consonant simply because an individual's status (disadvantaged or advantaged) guides them toward a particular category of public programs. Instead, we must ask how policy experiences intersect in the lives of citizens and interact 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 they may get 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 the need for caution in 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)? For example, are we 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, alternatively, do more 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, the findings presented here raise important questions, not just for scholars, but also for citizens and policymakers. When public policies produce unintended consequences, empirical research can help pull the 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 the evidence for policy feedback effects grows stronger, we are approaching the point where difficult questions must be addressed more squarely. How much do Americans really 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 tradeoffs has received little scrutiny. Instead, the turn toward paternalism over the past two decades has reflected an assumption that positive outcomes of all stripes will emerge together 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. When we design or reform the policies of the welfare state, 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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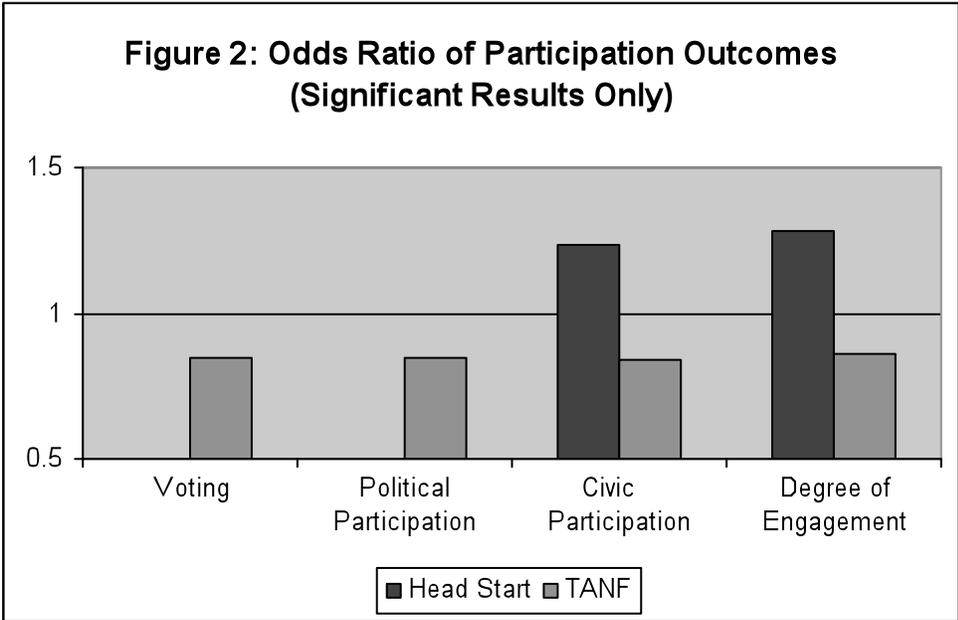
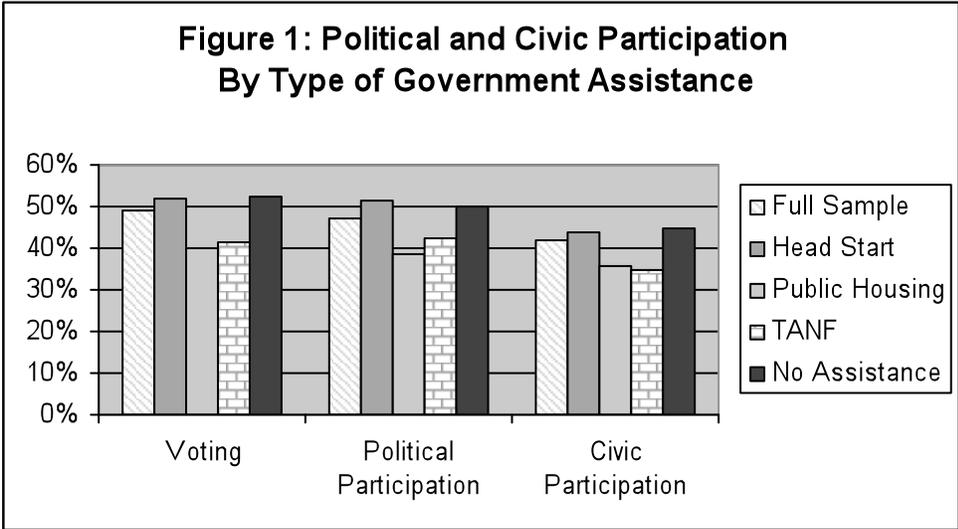
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Note: These odds ratios correspond to the model results shown in Table 2.

Table 1: Odds Ratio of All Outcomes with TANF

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
<u>Type of Government Assistance</u>				
TANF	0.849* (0.062)	0.849* (0.061)	0.843* (0.060)	0.860* (0.055)
<u>Demographics</u>				
Female	1.322** (0.093)	1.278** (0.089)	0.926 (0.060)	1.023 (0.061)
Has college educated parent	1.495** (0.109)	1.538** (0.111)	1.274** (0.092)	1.502** (0.099)
African American	1.728** (0.121)	1.699** (0.118)	1.439** (0.091)	1.606** (0.093)
Latino	1.015 (0.125)	1.088 (0.131)	1.019 (0.108)	1.047 (0.101)
Non-citizen		0.072** (0.011)	0.641** (0.067)	0.258** (0.025)
Age	1.053** (0.005)	1.053** (0.005)	1.034** (0.004)	1.050** (0.004)
Education	1.618** (0.053)	1.608** (0.052)	1.389** (0.041)	1.592** (0.043)
Married	1.190* (0.080)	1.177* (0.076)	1.663** (0.099)	1.528** (0.084)
Income-to-poverty ratio	1.071** (0.016)	1.079** (0.019)	1.018 (0.010)	1.039** (0.012)
<u>Social Marginality</u>				
Material hardships	0.984 (0.014)	1.012 (0.014)	1.098** (0.015)	1.072** (0.013)
Conviction	0.457** (0.054)	0.501** (0.056)	1.039 (0.106)	0.741** (0.069)
Substance use	1.021 (0.068)	1.047 (0.068)	0.764** (0.047)	0.851** (0.047)
Not living with focal child	0.680** (0.069)	0.795* (0.076)	0.938 (0.084)	0.871 (0.072)
Moved between T1 & T3	0.740** (0.085)	0.755* (0.085)	0.790* (0.087)	0.767* (0.078)
Domestic violence	0.849 (0.122)	0.826 (0.115)	0.945 (0.083)	0.877 (0.091)
<u>Economic Conditions</u>				
% families in poverty (tract)	1.816* (0.434)	1.925** (0.457)	1.512 (0.342)	1.727** (0.351)
MSA unemployment	1.088** (0.029)	1.080** (0.028)	0.887** (0.022)	0.967 (0.021)

<u>Political Conditions</u>				
Ease of state registration laws	1.166** (0.063)	1.161** (0.061)	1.137** (0.056)	1.169** (0.052)
Party competition	2.145** (0.564)	1.921* (0.483)	1.906** (0.451)	1.921** (0.410)
South	0.634** (0.055)	0.621** (0.053)	0.964 (0.078)	0.763** (0.055)
<u>Social Conditions</u>				
% state population in poverty	0.964** (0.014)	0.975 (0.013)	0.989 (0.013)	0.985 (0.011)
% state pop African American	1.008 (0.008)	1.008 (0.008)	0.998 (0.007)	1.002 (0.006)
LR chi2 (df)	1271.92 (22)	1842.31 (23)	748.21 (23)	1722.79 (23)
Pseudo R2	0.14	0.18	0.07	0.10

Note: N=7529. Non-citizens are excluded from voting models (N=6774). 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)

Table 2: Odds Ratio of Political and Civic Participation by Receipt of Governmental Assistance Taken Separately

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	0.849* (0.062)	0.849* (0.061)	0.843* (0.060)	0.860* (0.055)
Head Start	1.155 (0.122)	1.146 (0.120)	1.233* (0.122)	1.285** (0.115)
Public Housing	0.910 (0.068)	0.906 (0.066)	0.941 (0.065)	0.906 (0.056)

Note: N=7529. Voting models include citizens only (N=6774), 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 of these odds ratios is taken from models estimated separately where each outcome is predicted with only one type of receipt. Full model results available in Tables 1 (TANF), S2 (Public Housing) and S3 (Head Start) in the Online Supplement.

* $p < .05$, ** $p < .01$ (two tailed tests)

Table 3: Odds Ratio of Political and Civic Participation by Receipt of Multiple Types of Governmental Assistance

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	0.851* (0.063)	0.852* (0.062)	0.838* (0.060)	0.859* (0.055)
Head Start	1.179 (0.125)	1.171 (0.123)	1.259* (0.125)	1.312** (0.118)
Public Housing	0.927 (0.070)	0.922 (0.068)	0.957 (0.067)	0.918 (0.058)

Note: N=7529. Voting models include citizens only (N=6774), 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 Ratio 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	0.802* (0.076)	0.789* (0.074)	0.689** (0.064)	0.720** (0.059)
Receiving – Low	0.903 (0.086)	0.916 (0.086)	1.028 (0.093)	1.023 (0.083)

Note: N=7529. Voting models include citizens only (N=6774), 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)

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

Supplemental Materials

(for reviewers and to be put on the web for readers)

Table S1: Sample Demographic, Social Marginality, Government Assistance, Political and Civic Participation and Contextual Characteristics

	Non-Imputed Mean (SD)	Imputed Mean (SD)
<u>Demographic Characteristics</u>		
Female	.56 (.50)	.56 (.50)
Has college educated parent	.19 (.39)	.19 (.39)
African American	.49 (.50)	.49 (.50)
Latino	.11 (.31)	.11 (.31)
Non-citizen	.09 (.29)	.10 (.30)
Age	29.49 (6.81)	29.49 (6.81)
Education	1.28 (.99)	1.27 (.99)
Married	.37 (.48)	.37 (.48)
Income-to-poverty ratio	2.34 (3.21)	2.24 (3.20)
<u>Social Marginality Characteristics</u>		
Material hardships	1.84 (1.97)	1.84 (1.97)
Conviction	.07 (.25)	.07 (.25)
Substance use	.24 (.43)	.24 (.43)
Not living with focal child	.11 (.31)	.11 (.31)
Moved between T1 & T3	.06 (.24)	.06 (.24)
Domestic violence	2.05 (.32)	2.07 (.32)
<u>Government Assistance</u>		
Participated in Head Start/Early Head Start	.07 (.26)	.07 (.26)
Lived in public housing project	.19 (.39)	.19 (.39)
Received welfare/TANF	.20 (.40)	.20 (.40)
Received TANF in high paternalism state	.10 (.30)	.10 (.30)
Received TANF in low paternalism state	.10 (.30)	.10 (.30)
<u>Political and Civic Participation</u>		
Voted in November 2000 election	.44 (.50)	.44 (.50)
Participated in political group, demonstration, or voted	.47 (.50)	.47 (.50)
Participated in civic organization or group	.42 (.49)	.42 (.49)
Degree of engagement	.95 (.83)	.95 (.83)
<u>Economic Conditions</u>		
% families in poverty (tract)	.18 (.14)	.18 (.14)
MSA unemployment	3.71 (1.13)	3.71 (1.14)
<u>Political Conditions</u>		
Ease of state registration laws	1.81 (.54)	1.81 (.54)
Party competition	.85 (.12)	.85 (.12)
South	.36 (.48)	.36 (.48)
<u>Social Conditions</u>		
% state population in poverty	11.57 (2.60)	11.57 (2.60)
% state pop African American	13.51 (5.97)	13.51 (5.97)
N	4688-7529	7529

Note: The number of observation range for the unimputed represents the number of cases without missing data on each item. Statistics for the imputed data are taken from the first of the five imputed datasets created using STATA's ice command.

Table S2: Odds Ratio of All Outcomes with Public Housing Assistance

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
<u>Type of Government Assistance</u>				
Public housing	0.910 (0.068)	0.906 (0.066)	0.941 (0.065)	0.906 (0.056)
<u>Demographics</u>				
Female	1.286** (0.087)	1.235** (0.083)	0.893 (0.056)	0.993 (0.057)
Has college educated parent	1.492** (0.109)	1.535** (0.110)	1.274** (0.092)	1.499** (0.099)
African American	1.718** (0.120)	1.691** (0.117)	1.428** (0.091)	1.600** (0.093)
Latino	1.011 (0.125)	1.084 (0.130)	1.014 (0.107)	1.044 (0.101)
Non-citizen		0.074** (0.012)	0.656** (0.068)	0.263** (0.026)
Age	1.053** (0.005)	1.053** (0.005)	1.034** (0.004)	1.051** (0.004)
Education	1.620** (0.054)	1.610** (0.052)	1.393** (0.041)	1.593** (0.044)
Married	1.205** (0.081)	1.191** (0.077)	1.684** (0.100)	1.545** (0.085)
Income-to-poverty ratio	1.073** (0.016)	1.080** (0.019)	1.019 (0.010)	1.040** (0.012)
<u>Social Marginality</u>				
Material hardships	0.981 (0.014)	1.009 (0.014)	1.094** (0.014)	1.070** (0.013)
Conviction	0.454** (0.054)	0.497** (0.056)	1.031 (0.105)	0.736** (0.069)
Substance use	1.020 (0.068)	1.047 (0.068)	0.764** (0.047)	0.851** (0.047)
Not living with focal child	0.686** (0.068)	0.802** (0.077)	0.949 (0.085)	0.877 (0.072)
Moved between T1 & T3	0.735** (0.085)	0.750* (0.085)	0.786* (0.087)	0.763** (0.078)
Domestic violence	0.843 (0.121)	0.821 (0.115)	0.940 (0.082)	0.873 (0.091)
<u>Economic Conditions</u>				
% families in poverty (tract)	1.837* (0.445)	1.951** (0.469)	1.490 (0.343)	1.755** (0.364)
MSA unemployment	1.087** (0.029)	1.079** (0.028)	0.886** (0.022)	0.965 (0.021)

<u>Political Conditions</u>				
Ease of state registration laws	1.159** (0.063)	1.153** (0.061)	1.132* (0.056)	1.162** (0.052)
Party competition	2.171** (0.571)	1.942** (0.488)	1.930** (0.456)	1.939** (0.414)
South	0.641** (0.056)	0.628** (0.053)	0.973 (0.078)	0.770** (0.055)
<u>Social Conditions</u>				
% state population in poverty	0.963** (0.014)	0.975 (0.013)	0.989 (0.013)	0.985 (0.011)
% state pop African American	1.009 (0.008)	1.009 (0.008)	0.998 (0.007)	1.003 (0.006)
LR chi2 (df)	1268.74 (22)	1839.19 (23)	743.46 (23)	1720.12 (23)
Pseudo R2	0.14	0.18	0.07	0.10

Note: N=7529. Non-citizens are excluded from voting models (N=6774). 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)

Table S3: Odds Ratio of All Outcomes with Head Start Participation

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
<u>Type of Government Assistance</u>				
Head Start	1.155 (0.122)	1.146 (0.120)	1.233* (0.122)	1.285** (0.115)
<u>Demographics</u>				
Female	1.255** (0.086)	1.206** (0.082)	0.867* (0.055)	0.957 (0.056)
Has college educated parent	1.496** (0.109)	1.539** (0.111)	1.275** (0.092)	1.503** (0.099)
African American	1.698** (0.118)	1.670** (0.115)	1.413** (0.089)	1.578** (0.091)
Latino	1.007 (0.124)	1.079 (0.129)	1.008 (0.107)	1.037 (0.100)
Non-citizen		0.073** (0.012)	0.657** (0.068)	0.263** (0.026)
Age	1.054** (0.005)	1.053** (0.005)	1.035** (0.004)	1.051** (0.004)
Education	1.627** (0.054)	1.616** (0.052)	1.397** (0.041)	1.600** (0.044)
Married	1.207** (0.081)	1.194** (0.077)	1.690** (0.101)	1.551** (0.085)
Income-to-poverty ratio	1.074** (0.016)	1.082** (0.019)	1.019 (0.010)	1.041** (0.012)
<u>Social Marginality</u>				
Material hardships	0.980 (0.014)	1.008 (0.014)	1.093** (0.014)	1.068** (0.013)
Conviction	0.455** (0.054)	0.498** (0.056)	1.034 (0.106)	0.741** (0.069)
Substance use	1.019 (0.068)	1.045 (0.068)	0.762** (0.047)	0.848** (0.047)
Not living with focal child	0.692** (0.069)	0.809* (0.078)	0.956 (0.085)	0.887 (0.073)
Moved between T1 & T3	0.734* (0.085)	0.749* (0.084)	0.784* (0.086)	0.761** (0.077)
Domestic violence	0.843 (0.122)	0.821 (0.116)	0.939 (0.081)	0.872 (0.091)
<u>Economic Conditions</u>				
% families in poverty (tract)	1.707* (0.405)	1.812* (0.427)	1.410 (0.317)	1.614* (0.326)
MSA unemployment	1.088** (0.029)	1.081** (0.028)	0.886** (0.022)	0.967 (0.021)

<u>Political Conditions</u>				
Ease of state registration laws	1.160** (0.063)	1.154** (0.061)	1.130* (0.056)	1.160** (0.052)
Party competition	2.218** (0.584)	1.981** (0.498)	1.972** (0.467)	1.990** (0.425)
South	0.640** (0.056)	0.627** (0.053)	0.974 (0.078)	0.770** (0.055)
<u>Social Conditions</u>				
% state population in poverty	0.962** (0.014)	0.974 (0.013)	0.988 (0.012)	0.984 (0.011)
% state pop African American	1.009 (0.008)	1.009 (0.008)	0.998 (0.007)	1.003 (0.006)
LR chi2 (df)	1269.11 (22)	1839.04 (23)	747.34 (23)	1725.87 (23)
Pseudo R2	0.14	0.18	0.07	0.10

Note: N=7529. Non-citizens are excluded from voting models (N=6774). 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)

Table S4: Odds Ratio of All Outcomes with All Three Types of Government Assistance

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
<u>Type of Government Assistance</u>				
TANF	0.851* (0.063)	0.852* (0.062)	0.838* (0.060)	0.859* (0.055)
Head Start	1.179 (0.125)	1.171 (0.123)	1.259* (0.125)	1.312** (0.118)
Public Housing	0.927 (0.070)	0.922 (0.068)	0.957 (0.067)	0.918 (0.058)
<u>Demographics</u>				
Female	1.311** (0.093)	1.260** (0.089)	0.904 (0.059)	0.997 (0.060)
Has college educated parent	1.492** (0.109)	1.535** (0.110)	1.273** (0.092)	1.500** (0.099)
African American	1.730** (0.121)	1.703** (0.118)	1.436** (0.092)	1.609** (0.093)
Latino	1.016 (0.125)	1.090 (0.131)	1.018 (0.108)	1.050 (0.101)
Non-citizen		0.072** (0.012)	0.644** (0.067)	0.259** (0.025)
Age	1.053** (0.005)	1.053** (0.005)	1.034** (0.004)	1.051** (0.004)
Education	1.613** (0.053)	1.603** (0.052)	1.386** (0.041)	1.586** (0.043)
Married	1.192** (0.080)	1.179* (0.076)	1.667** (0.099)	1.533** (0.084)
Income-to-poverty ratio	1.071** (0.016)	1.078** (0.018)	1.018 (0.010)	1.039** (0.012)
<u>Social Marginality</u>				
Material hardships	0.982 (0.014)	1.011 (0.014)	1.096** (0.015)	1.070** (0.013)
Conviction	0.459** (0.054)	0.502** (0.057)	1.043 (0.107)	0.746** (0.070)
Substance use	1.019 (0.068)	1.045 (0.068)	0.762** (0.047)	0.849** (0.047)
Not living with focal child	0.679** (0.068)	0.793* (0.076)	0.937 (0.084)	0.869 (0.071)
Moved between T1 & T3	0.738** (0.085)	0.753* (0.085)	0.788* (0.087)	0.765** (0.078)
Domestic violence	0.846 (0.122)	0.823 (0.115)	0.941 (0.082)	0.874 (0.090)
<u>Economic Conditions</u>				
% families in poverty (tract)	1.873* (0.457)	1.990** (0.481)	1.522 (0.352)	1.777** (0.369)
MSA unemployment	1.085** (0.029)	1.077** (0.028)	0.885** (0.022)	0.964 (0.021)

<u>Political Conditions</u>				
Ease of state registration laws	1.158** (0.063)	1.152** (0.061)	1.129* (0.056)	1.158** (0.052)
Party competition	2.175** (0.573)	1.942** (0.489)	1.940** (0.460)	1.957** (0.419)
South	0.635** (0.056)	0.623** (0.053)	0.966 (0.078)	0.765** (0.055)
<u>Social Conditions</u>				
% state population in poverty	0.964* (0.014)	0.976 (0.013)	0.990 (0.013)	0.986 (0.011)
% state pop African American	1.009 (0.008)	1.009 (0.008)	0.998 (0.007)	1.003 (0.006)
LR chi2 (df)	1275.51 (24)	1845.94 (25)	753.99 (25)	1734.09 (25)
Pseudo R2	0.14	0.18	0.07	0.11

Note: N=7529. Non-citizens are excluded from voting models (N=6774). 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)

Table S5: Odds Ratio of All Outcomes with TANF & Paternalism Index

Predictors	Model 1: Voting	Model 2: Political Participation	Model 3: Civic Participation	Model 4: Degree of Engagement
<u>Type of Government Assistance</u>				
Receiving TANF – High	0.802* (0.076)	0.789* (0.074)	0.689** (0.064)	0.720** (0.059)
Receiving TANF – Low	0.903 (0.086)	0.916 (0.086)	1.028 (0.093)	1.023 (0.083)
TANF benefit generosity	0.952 (0.036)	0.975 (0.037)	0.983 (0.035)	0.992 (0.032)
<u>Demographics</u>				
Female	1.337** (0.094)	1.280** (0.089)	0.927 (0.060)	1.024 (0.061)
Has college educated parent	1.497** (0.109)	1.541** (0.111)	1.282** (0.093)	1.512** (0.100)
African American	1.728** (0.121)	1.697** (0.118)	1.435** (0.091)	1.599** (0.093)
Latino	1.025 (0.127)	1.094 (0.131)	1.025 (0.109)	1.051 (0.101)
Non-citizen		0.073** (0.012)	0.649** (0.068)	0.260** (0.026)
Age	1.053** (0.005)	1.053** (0.005)	1.034** (0.004)	1.050** (0.004)
Education	1.616** (0.053)	1.606** (0.052)	1.387** (0.041)	1.591** (0.043)
Married	1.191** (0.080)	1.178* (0.076)	1.666** (0.100)	1.530** (0.084)
Income-to-poverty ratio	1.072** (0.016)	1.079** (0.019)	1.019 (0.010)	1.040** (0.012)
<u>Social Marginality</u>				
Material hardships	0.983 (0.014)	1.012 (0.014)	1.100** (0.015)	1.075** (0.013)
Conviction	0.461** (0.055)	0.504** (0.057)	1.049 (0.107)	0.746** (0.070)
Substance use	1.020 (0.068)	1.046 (0.068)	0.762** (0.047)	0.850** (0.047)
Not living with focal child	0.679** (0.068)	0.793* (0.076)	0.932 (0.083)	0.866 (0.071)
Moved between T1 & T3	0.740* (0.085)	0.755* (0.085)	0.790* (0.087)	0.767* (0.078)
Domestic violence	0.848 (0.121)	0.825 (0.114)	0.943 (0.082)	0.876 (0.090)
<u>Economic Conditions</u>				
% families in poverty (tract)	1.844* (0.442)	1.947** (0.463)	1.544 (0.351)	1.764** (0.360)
MSA unemployment	1.084** (0.029)	1.077** (0.028)	0.881** (0.022)	0.962 (0.021)

<u>Political Conditions</u>				
Ease of state registration laws	1.154** (0.063)	1.153** (0.061)	1.129* (0.056)	1.163** (0.053)
Party competition	1.852* (0.546)	1.808* (0.513)	1.935* (0.520)	2.000** (0.484)
South	0.593** (0.066)	0.607** (0.066)		0.792* (0.073)
<u>Social Conditions</u>				
% state population in poverty	0.964* (0.014)	0.974 (0.014)	0.984 (0.013)	0.980 (0.012)
% state pop African American	1.009 (0.008)	1.008 (0.008)	0.995 (0.007)	1.000 (0.006)
LR chi2 (df)	1274.60 (24)	1844.36 (25)	760.03 (25)	1734.38 (25)
Pseudo R2	0.14	0.18	0.07	0.11

Note: N=7529. Non-citizens are excluded from voting models (N=6774). 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)

ONLINE SUPPLEMENT B – Accounting for Non-Independence of Observations

Three methods offer plausible approaches to accounting for the non-independence of observations in our analysis: clustered robust standard errors; survey estimation techniques; and multilevel modeling.

Clustered robust standard errors (CSREs) allow researchers to correct for the clustering of observations by inflating standard errors according to the degree of non-independence. Unfortunately, this method proved not to be a viable option for the analyses presented here. When CSREs are employed, the models produce coefficient estimates for individual variables, but they consistently fail to compute model fit statistics because the number of predictors exceeds the number of clusters. There simply is not enough information to calculate the tests. Even if this practical difficulty did not exist, the use of CSREs is not recommended when observations are distributed across clusters in an unbalanced manner (i.e., unequal numbers of observations per cluster), as is the case here with the Fragile Families data. Moreover, CSREs offer a method of correcting standard errors that is, in general, weaker than multilevel modeling: both approaches correct the degrees of freedom in numerator of the standard error so that it reflects the number of clusters, but only the latter proves a correction for the denominator degrees of freedom.

Survey techniques offer a second approach to adjusting our analyses to reflect the non-independence of observations. This method is appropriate if the data were collected by a survey that was sampled by clusters. And indeed, the Fragile Families data meet this criterion because surveys were conducted by sampling from hospitals in cities. Unfortunately, as in the clustered robust standard error approach, models using survey techniques produce coefficient estimates for individual variables but fail to generate model fit statistics because the number of predictors

exceeds the number of clusters. Again, we confront the problem of not having enough information to calculate the tests.

Multilevel modeling (MLM) offers a third approach to adjusting for the clustered nature of our data. MLM allows adjust for the non-independence of observations by taking into account the clusters (or “level 2 units”) in which the observations are nested. This technique is used in many cases to partition the variance of outcomes and estimate the amount of variance explained by variables at different levels of analysis. A primary factor to consider when evaluating the suitability of MLM is the number of clusters available in the data. As the number clusters get smaller, so does one’s ability to estimate complex models that include multiple variables observed in the level 2 units.

When we apply MLM techniques to our models, we are able to successfully obtain estimates of both individual coefficients and model fit statistics. Nevertheless, we remain concerned that the number of clusters in our dataset (20 cities) is quite small in relation to the number of predictors used for estimation (22-25, with 6-8 of those at the cluster level). The unfavorable ratio of clusters to predictors may bias results in unknown ways because we confront an uncertain amount of estimation error. A general rule of thumb for MLM estimation is that one should have something on the order of 1-2 cluster-level predictors per 10 clusters. Because our models include 3-4 cluster-level predictors per 10 clusters, the rule of thumb suggests that MLM is inadvisable.

Despite the shortcomings of each approach, we have sought ensure that our results are robust to these techniques by estimating our models with each and then comparing the results to the findings we report in the manuscript based on unadjusted models. Table S6 and S7 show the results produced when each approach is applied to an analysis where TANF receipt predicts

voting participation. The multilevel models presented here are specified with a random intercept, meaning that the intercept is allowed to vary across clusters/cities. The first column of Tables S6 and S7 correspond to the estimates presented in Tables 3 and 4. Using the clustered robust standard error and survey adjustment methods, the point estimates do not change, only the standard errors. In multilevel modeling however, the point estimates are slightly different because the variability in the predictors and residuals is treated differently in a multilevel model.

Table S6: Comparison of Methods for Taking into Account the Clustered Nature of the Fragile Families Data Predicting Voting

Predictors	No adjustment	Clustered standard errors	Survey method	Multilevel
TANF	0.849* (0.063)	0.849* (0.052)	0.849* (0.052)	0.866 (0.064)
LR chi2 (df)	1271.92 (22)	Not estimated	Not estimated	869.94 (22)
Pseudo R2	.14	.14	Not estimated	Not estimated

Note: N=7529. Voting models include citizens only (N=6774), 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. Wald chi2 estimated for multilevel model.

* $p < .05$, ** $p < .01$ (two tailed tests)

Table S7: Comparison of Methods for Taking into Account the Clustered Nature of the Fragile Families Data Predicting Voting

Predictors	No adjustment	Clustered standard errors	Survey method	Multilevel
Rec TANF – High	0.802* (0.076)	0.802* (0.074)	0.802* (0.074)	0.772** (0.075)
Rec TANF – Low	0.903 (0.086)	0.903 (0.059)	0.903 (0.059)	0.983 (0.099)
LR chi2 (df)	1274.60 (24)	Not estimated	Not estimated	875.14 (24)
Pseudo R2	.14	.14	Not estimated	Not estimated

Note: N=7529. Voting models include citizens only (N=6774), 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. Wald chi2 estimated for multilevel model.

* $p < .05$, ** $p < .01$ (two tailed tests)

Table S8: Odds Ratio of Political and Civic Participation by Receipt of Governmental Assistance Taken Separately using Multilevel Modeling

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	0.866 (0.064)	0.857* (0.063)	0.843* (0.060)	0.850* (0.062)
Head Start	1.158 (0.123)	1.142 (0.120)	1.233* (0.122)	1.255* (0.135)
Public Housing	0.938 (0.070)	0.932 (0.068)	0.941 (0.065)	0.979 (0.069)

Note: N=7529. Voting models include citizens only (N=6774), 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 of these odds ratios is taken from models estimated separately where each outcome is predicted with only one type of receipt. Degree of Engagement is coded dichotomously because the multilevel modeling program utilized does not allow ordered logistic regressions (xtme- commands in Stata).

* $p < .05$, ** $p < .01$ (two tailed tests)

Table S9: Odds Ratio of Political and Civic Participation by Receipt of Multiple Types of Governmental Assistance using Multilevel Modeling

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
TANF	0.865 (0.065)	0.857* (0.063)	0.838* (0.060)	0.842* (0.062)
Head Start	1.177 (0.126)	1.163 (0.123)	1.259* (0.125)	1.276* (0.137)
Public Housing	0.953 (0.072)	0.948 (0.070)	0.957 (0.067)	0.995 (0.071)

Note: N=7529. Voting models include citizens only (N=6774), 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. Degree of Engagement is coded dichotomously because the multilevel modeling program utilized does not allow ordered logistic regressions (xtme- commands in Stata).

* $p < .05$, ** $p < .01$ (two tailed tests)

Table S10: Odds Ratio of Political and Civic Participation by Receipt of TANF in States with High and Low Paternalism Index Scores using Multilevel Modeling

Predictors	Voting	Political Participation	Civic Participation	Degree of Engagement
Receiving – High	0.772** (0.075)	0.770** (0.074)	0.681** (0.065)	0.729** (0.068)
Receiving – Low	0.983 (0.099)	0.963 (0.095)	1.039 (0.097)	1.008 (0.099)

Note: N=7529. Voting models include citizens only (N=6774), 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. Degree of Engagement is coded dichotomously because the multilevel modeling program utilized does not allow ordered logistic regressions (xtme- commands in Stata).

* $p < .05$, ** $p < .01$ (two tailed tests)

ONLINE SUPPLEMENT PART C – Description of Measures

Voting: If the respondent reported being eligible to vote, they were asked, “Did you vote in the November 2000 election?” Respondents answering affirmatively to the voting question were coded as voting.

Political Participation: Respondents are coded as having participated politically if they reported voting, participating in a political, civic, or human rights organization in the past 12 months, or ever participating in a political demonstration or march. Respondents were read the following statement: “Now I have some questions about your involvement or participation in different kinds of groups and organizations. Please tell me whether you have participated in any of the following in the past 12 months, that is, since (FIRST OF MONTH ONE YEAR AGO). Have you participated in...” A list of several types of organizations and groups followed, with one being, “A political, civic, or human rights organization”. The final component of the political participation measure comes from a question that asked, “Have you ever taken part in a political demonstration or march?”

Civic Participation: Respondents are coded as having participated civically if they reported participating in a civic group in the past 12 months. Respondents were read the following statement: “Now I have some questions about your involvement or participation in different kinds of groups and organizations. Please tell me whether you have participated in any of the following in the past 12 months, that is, since (FIRST OF MONTH ONE YEAR AGO). Have you participated in...” A list of several types of organizations and groups followed. The following groups or organization were included in the civic participation measure: “A group affiliated with your church in the past year; a service club, such as the Police Athletic League, or

the Scouts; a labor union or other work-related group; or a community organization, such as a neighborhood watch.

TANF Participation: Respondents were coded as receiving TANF if they answered affirmatively to the questions asking whether the respondent currently or in the time since the last interview was/is receiving welfare/TANF in the Year 1 Follow up or Year 3 Follow up.

Head Start/Early Head Start Participation: Respondents were coded as participating in the Head Start/Early Head Start Program if they indicated that their primary care arrangement was Head Start/Early Head Start or that they received assistance to help pay for child care from Head Start/Early Head Start in the Year 1 or Year 3 Follow up.

Public Housing Participation: Respondents were coded as receiving public housing assistance if they answered affirmatively that the home they currently lived in was a public housing project at Baseline, Year 1 or Year 3 Follow up.

Education: The educational attainment of the respondent was coded into four categories as follows: 0=less than high school graduate; 1=high school graduate or obtained GED or ABE; 2=some college or tech degree; and 3=four year college degree (BA) or higher.

Criminal Conviction: Respondents were coded as having a criminal conviction if they responded affirmatively to a question asking if they had ever been convicted of any charges not counting minor traffic violations at Year 1 or Year 3 Follow up. Information was taken from Year 3, and when missing filled in with Year 1 data.

Not Residing with Child: Respondents were coded as not living with the focal child if they reported that the child spent none of the time living with them at the Year 3 Follow up.

Domestic Violence: The measure of domestic violence included in the models is a composite of two items asking the respondents in the Year 3 Follow up interview how often the mother/father

of the child slaps or kicks you, and how often the mother/father hits you with fist or an object that can hurt you. There are three response categories: often, sometimes, or never. The measure is coded to have higher values indicate more domestic violence.

Material Hardships: The measure of material hardships is a count of how many material hardships were reported by the respondent, which I have capped at a maximum of 5. The hardships are asked of experiences in the past year and are taken from the Year 3 Follow up interview. The hardships included are: telephone service disconnected; electricity turned off; gas/oil service turned off; home uncomfortably cold for 48 hours or more; no running water for 48 hours or more; receive free food or meals; unable to pay full rent or mortgage; evicted from home; unable to pay full gas/oil/electricity bill; borrowed money from friends/family; moved in with other people because of financial problems; stayed in a shelter/car/abandoned building; did not see a doctor or go to the hospital; cut back on buying yourself clothes; or worked overtime or took second job.

Substance Abuse: The measure of substance abuse is calculated using the code provided on the Fragile Families website. The indicator of substance abuse is positive if either the respondent is found to be alcohol dependent or drug dependent. Alcohol dependence is measured as having 4 or more drinks in one day in the past 12 months. Drug dependence is indicated by any use of the following drugs: sedatives; tranquilizers; amphetamines; analgesics; inhalants; marijuana; cocaine; LSD/hallucinogens; or heroin.

Percent in Poverty: Percent of families living below the federal poverty line in the tract in 1999. Source: 2000 Census.

Unemployment Rate: Unemployment rate in the Metropolitan Statistical Area in the year of the mother's baseline interview. Source: Bureau of Labor Statistics.

Ease/Difficulty of Registration Index: This index combines three separate measures of registration laws at the state level: motor voter, closing date, and mail registration. Each of the three laws is coded as a dichotomous indicator and then combined into an additive index that ranges from 0-3. The motor voter element indicates whether states had Motor Voter registration implemented or not. The closing date element is a dichotomous coding of the number of days between voter registration closing and election day, recoded so that high values indicate a later closing date (easier to register). Having 25 days or higher is coded as easy registration (because of the reverse coding). The mail registration element indicates whether a state allowed voter registration by mail. Source: Council of State Governments 1996.

Interparty Competition: The difference of proportions of seats controlled by Democrats and Republicans in a state's lower and upper legislative chamber. Source: Soss et al. 2001.

Indicator for South: The South is defined as the U.S. Census Bureau defined region – South (DE, FL, GA, MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX).

Percent African American: The percent African American in the state in 1999. Source: 2000 Census, U.S. Census Bureau.

Percent in Poverty: The percent of people in poverty in the state in 1999. Source: 2000 Census, U.S. Census Bureau.

TANF Benefit: The average monthly benefit for AFDC/TANF families in 2000 taken from Table 7-9 of the 2004 Green Book, and adjusted for the difference in the cost of living across states using the Berry, Fording, and Hanson cost-of-living index for 2000. This measure was divided by 100 so that the unit of change for the coefficient is \$100 instead of \$1.

TANF Restrictions Index: This index is an additive measure of work requirements (whether a state required recipients to find work sooner than the federal standard of 24 months, coded 1 for

states with stricter than federal standards); time limits (whether a state adopted a maximum time limit for receiving benefits shorter than the federal standard of 60 months, coded 1 for states with time limits shorter than 60 months); and stringency of sanctions (penalties for failing to comply with the new welfare rules, coded 0 for states with weak sanctions which are sanctions that are delayed and applied to benefits received by the adult but not the child; coded 1 for states with moderate sanctions which are sanctions that are delayed but applied to the whole family; and coded 2 for states with strong sanctions which are sanctions that are immediately applied to benefits for the whole family) that ranges from 0-5 with higher values indicating more punitive/harsh restrictions. A restrictions index score of 3 or higher is considered punitive.