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Effects of Welfare Reform on Women's Voting Participation

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ABSTRACT

Effects of Welfare Reform on Women's Voting Participation*

Voting is an important form of civic participation in democratic societies but a fundamental right that many citizens do not exercise. This study investigates the effects of welfare reform in the U.S. in the 1990s on voting of low income women. Using the November Current Population Surveys with the added Voting and Registration Supplement for the years 1990 through 2004 and exploiting changes in welfare policy across states and over time, we estimate the causal effects of welfare reform on women's voting registration and voting participation during the period during which welfare reform unfolded. We find robust evidence that welfare reform increased the likelihood of voting by about 4 percentage points, which translates to about a 10% increase relative to the baseline mean. The effects were largely confined to Presidential elections, were stronger in Democratic than Republican states, were stronger in states with stronger work incentive policies, and appeared to operate through employment, education, and income.

JEL Classification: H0, I2, J2, J3

Keywords: employment, voting, welfare reform, income, civic participation, education, difference-in-differences

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I. Introduction

The broad goal of the landmark welfare reform legislation in the U.S. in the 1990s was to reduce dependence on government benefits by promoting work, encouraging marriage, and reducing non-marital childbearing. The legislation represented a convergence of dissatisfaction with the welfare system on both sides of the political spectrum, with welfare participation becoming viewed by many as a cause of dependence rather than a consequence of disadvantage. The key strategy for reducing dependence was to promote employment by imposing work requirements as a condition for receiving benefits as well as time limits on receipt of cash assistance. The basic argument was that labor force participation would break a “culture of poverty” by increasing self-sufficiency and reconnecting members of an increasingly marginalized underclass to the mainstream ideals of a strong work ethic and civic responsibility (Katz 2001).

In terms of increasing employment of low-skilled women and decreasing welfare caseloads, welfare reform has been deemed a great success. Temporary Assistance to Needy Families (TANF) caseloads declined by 50% between 1997 and 2001 alone, and over half of TANF cases are now “child only,” meaning that adults in the household are not eligible (Loprest 2012). Employment rates of low-skilled mothers rose dramatically since the early 1990s, and there is strong consensus that welfare reform played a major role (Schoeni & Blank 2000; Ziliak 2006).

A handful of studies have found that welfare reform reduced undesirable behaviors that have often been ascribed to “welfare as we knew it,” providing some support for the mainstreaming argument. Kaestner & Tarlov (2006) found that welfare reform reduced women’s binge drinking. Corman et al. (2013) found that welfare reform led to declines in illicit drug use

among women at risk for relying on welfare, with some evidence indicating that the effects operate, at least in part, through work incentive policies. Corman, Dave, and Reichman (2014) found that welfare reform led to reductions in women's property crime. This emerging literature supports the widely-embraced argument that welfare reform discourages anti-social behavior and suggests that disenfranchised women have been brought from the margins to the mainstream. However, as far as we know, the only studies that have directly tested the widely-held assumption that welfare reform encourages mainstream behavior (other than work, which is required) have focused on marriage or non-marital fertility and have generally revealed weak or ambiguous effects (e.g., Blank 2002, Grogger & Karoly 2005, Gennetian & Knox 2003). To directly test the "culture of poverty" argument that making welfare much less of an option encourages personal and civic responsibility, it is necessary to go beyond marital status by considering direct measures of mainstream behavior.

In this paper, we investigate the effects of welfare reform on voting, which is an important form of civic participation in democratic societies but a fundamental right that many citizens do not exercise. Exploiting changes in welfare policy across states and over time, and comparing relevant population subgroups within an econometric difference-in-differences framework, we use the November Current Population Surveys with the added Voting and Registration Supplement to estimate the causal effects of welfare reform on women's voting registration and voting participation from 1990 to 2004, the period during which welfare reform unfolded. We explore the extent to which effects appear to have operated through employment as well as the extent to which effects varied by state political orientations and welfare strictness. The findings provide important information that promises to inform culture of poverty debates and provide a more complete picture of the effects of a major policy shift in the U.S. that is still

very much in effect today. The findings also make an important contribution to the multi-disciplinary literature on the determinants of voting by providing a strong test of the effects of work incentives (and, by inference, employment) on voting behavior, as well as to the political science literature on how citizens' experiences with government programs affect their political participation.

II. Background

Welfare reform in the U.S.

The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, often referred to as welfare reform, ended entitlement to welfare benefits under Aid to Families with Dependent Children (AFDC) and replaced the AFDC program with Temporary Assistance to Needy Families (TANF) block grants to states. Features of the legislation were time limits on cash assistance, work requirements as a condition for receiving benefits, stricter sanctions for non-compliance with work requirements and other program rules, stronger child support enforcement, and family caps that limited benefits for additional children. The broad goals of PRWORA were to reduce dependence on government benefits by promoting work, encouraging marriage, and reducing non-marital childbearing.

Although welfare reform is often dated to the landmark 1996 PRWORA legislation, reforms actually started taking place in the early 1990s when the Clinton Administration greatly expanded the use and scope of "welfare waivers." Many policies and features of state waivers were later incorporated into PRWORA. However, PRWORA departed from its waiver precursors by imposing a "work first" approach that was designed to not only reduce welfare dependence, but also to reconnect members of an increasingly marginalized underclass to the mainstream ideals of a strong work ethic and civic responsibility (Katz 2001). PRWORA granted

considerable discretion to states in establishing welfare eligibility and program rules. As a result, there is substantial state policy variation within the broad national regime of time-limited cash assistance for which work is required.

Employment, welfare, and voting

Glaeser, Laibson & Sacerdote (2002) examined individual investments in activities that create “social capital,” defined broadly as connections within social networks such as community organizations and religious institutions. Individuals choose to engage in such behaviors if the benefits outweigh the costs. By increasing employment, welfare reform may increase civic participation (including, perhaps, voting) by shifting women from the individualistic job of homemaker to more socially interactive occupations and increasing their participation in unions, but it could also decrease civic participation through an increase in the opportunity cost of time. Welfare reform may also increase civic participation as a result of the new normative climate of increased personal and civic responsibility. In terms of voting in particular, Feddersen (2004) offered theoretical reasons why individuals vote even though doing so imposes a cost and is unlikely to affect the outcome, one of which involves belonging to a social network that has a stake in the election’s outcome.

Consistent with much empirical literature, Farber (2009) found, using the 2004 and 2006 November Current Population Surveys with the added Voting and Registration Supplement, that more educated individuals are more likely to vote and that voter turnout is substantially higher among those employed in the public sector than among those employed in the private sector or the non-employed, suggesting that both employment and connections to government may increase political participation. Findings by Schur (2003), using data from two nationally-representative U.S. household surveys conducted by the Rutgers Center for Public Interest

Polling following the November elections in 1998 and 2000, suggest that being employed increases an individual's political activities through increased income, civic skills, political efficacy, and recruitment at work. Although voting behavior has been studied by political scientists, psychologists, survey researchers, and economists, and we know that employment is a strong correlate of voting behavior, existing studies have not produced strong evidence of causal effects of employment on voting.

As far as we know, no studies have examined the causal effects of welfare policy on voting or any other form of civic participation. However, a growing political science literature suggests that citizens' experiences with social welfare programs can affect political participation—e.g., by providing lessons in how citizens and government relate, giving recipients a stake in maintaining or enhancing program benefits, or by providing resources that facilitate political action (Soss 1999; Bruch, Feree & Soss 2010). According to Soss (1999), clients experience a lack of power when dealing with welfare agencies, which translates to powerlessness in the face of government more generally and serves as a disincentive to vote. Recent findings by Sugie (2015)—that partners of incarcerated men are less likely to register and vote than similar partners of men who have not been incarcerated—are consistent with the scenario of marginalization through feelings of powerlessness in interactions with public institutions. This research suggests that welfare reform would increase women's political participation, including voting, by disconnecting them from a system that fosters feelings of powerlessness.

A qualitative study of community leaders found that the time constraints of complying with TANF requirements reduce community-building activities on the part of women (Jennings 2001). This finding—that time and resource constraints detract from civic participation—stands

in contrast to assumptions on both sides of the political spectrum that welfare reform would encourage mainstream behaviors. It also stands in contrast with the literature on socioeconomic characteristics and voting behavior, which suggests that employment increases voting but has not focused on the important but specific population of women at risk for relying on welfare. On the other hand, Andersen, Curtis & Grabb (2006) found that civic participation of American women decreased during the 1990s while it increased in other developed countries, and speculated that increasing time commitment to paid work alongside declining levels of public support may be responsible. However, the links between welfare, employment, and civic participation in the U.S. were not empirically established in that study. Moreover, voting—which the authors did not study—is likely to impose fewer time constraints than would other forms of civic engagement.

III. Data

We use data from the November Current Population Surveys (CPS) with the added Voting and Registration Supplement for the years 1990 through 2004, which span the implementation of welfare reform. The general CPS is a monthly nationally representative survey of over 50,000 households that collects detailed information on labor force participation as well as sociodemographic characteristics of each household member.

The Voting and Registration Supplement takes place bi-annually at the end of November, in even years when Congressional elections occur. Thus, every second supplement takes place during a Presidential election, when voting turnout is higher (DeSilver 2014). The survey asks household members if they are eligible to vote, whether they had registered to vote by the election that occurred that month, and whether they had voted in that election. The Voting and Registration Supplement was designed and conducted to produce comprehensive national data on voter characteristics, participation, and trends (i.e., not from specialized surveys or media

polls). McDonald (2007) cross-validated voter registration files and CPS data and found the two sources to be consistent in terms of demographic profiles of voters. In this study, we focus on women who are at least 21 years old and up to age 49, a group that is both eligible to vote and likely to have minor children living in their household. Of, those, we only include only women who are citizens and thus eligible to register to vote.

We initially follow the standard in the welfare reform literature by using state-specific and time-varying indicators for both AFDC waivers and TANF (Blank 2002). Twenty-nine states enacted AFDC waivers, across various months, from 1992–1996 (see Appendix Table 1). We use a dichotomous variable that = 1 if a statewide waiver was in place before November of that year that substantially altered the nature of AFDC with respect to time limits, sanctions, or work requirements. We also use a dichotomous indicator for whether, before November of that year, the state had implemented TANF. The data on whether states had waivers and when they enacted TANF come from U.S. Department of Health & Human Services (1999). Although the new welfare regime is very much in effect today, our observation window coincides with, and exploits, maximum policy and implementation change.

We also use a single measure of any welfare reform implementation, defined as either a major waiver to AFDC or TANF in a given state and year. This measure has also been used in the relevant literature (e.g., Dave et al. 2011). Given that voting data are biennial and voting rates are much higher in Presidential elections than in non-Presidential elections (DeSilver 2014), the separate measures of AFDC waivers and TANF are based on very few different elections.¹ Most notably, only 2 states (Michigan and New Jersey) had implemented waivers before the 1992 Presidential election and these were implemented in October 1992 when the election process was

¹ Presidential elections took place in 1992 (William J. Clinton v. George H.W. Bush), 1996 (William J. Clinton v. Robert Dole), 2000 (George W. Bush v. Albert A. Gore, Jr.), and 2004 (George W. Bush v. John F. Kerry).

well underway, while 28 states had implemented waivers before the 1996 Presidential election (see Appendix Table 1). As such, separate waiver effects would almost exclusively reflect the 1996 Presidential election (Clinton’s re-election), which had a historically low voter turnout (U.S. Census Bureau 2012). Thus, the single measure of any welfare reform is our preferred measure for this study. Nevertheless, we also show some estimates using the separate measures of AFDC waivers and TANF in order to map the study to the existing literature but urge the reader to interpret the separate effects, particularly those for AFDC waivers, with caution. Other advantages of the single measure of any welfare reform are that it facilitates ease of discussion and comparison across models and preserves statistical power for subsequent stratification analyses.

IV. Methods

We employ a quasi-experimental research design—akin to a pre- and post-comparison with treatment and control groups—in conjunction with multivariate regression methods, broadly referred to as difference-in-differences models, to estimate the effects of welfare reform on women’s voting behavior. We conduct numerous specification checks and tests to investigate the validity of the identification assumptions underlying our methodology, the robustness of our results, and patterns across subgroups of mothers.

The basic model can be expressed as follows, where Y refers to the outcome for the i^{th} individual residing in state s at time t :

$$(1) Y_{ist} = \alpha_1 + \pi_1(AFDCWaiver_{st}) + \pi_2(TANF_{st}) + X_{ist}\beta + Z_{st}\delta + State_s\lambda + Year_t\varphi + \varepsilon_{ist}$$

AFDC Waiver and TANF are dichotomous variables indicating whether a major waiver or TANF had been implemented.² X represents a vector of individual characteristics (age, age-squared, race, Hispanic origin, marital history (widowed/divorced/separated, never married), number of children in the household, number of adults in the household, and metropolitan statistical area (MSA) residence), and Z represents a vector of state-level characteristics (current and 1-year lagged state unemployment rate, current and 1-year lagged state personal income per capita, log female population, state poverty rate, state minimum wage, 1- and 2-year lagged welfare caseloads, percentages of the state legislature that were Democrat/Republican, Republican governor, Democratic governor, and registration/voting rates of males).³ We do not control for education, employment, or family income in the baseline specification since prior work has shown that education and employment, which are associated with income, were affected by welfare reform (e.g., Dave, Corman & Reichman 2012; Ziliak 2006) and are potential pathways by which welfare reform may affect voting behaviors. A full set of state (State) and year (Year) fixed effects are included to capture unobserved time-invariant state-specific factors, as well as overall national trends. In the above specification, α_1 , π_1 , π_2 , β , δ , λ , and φ represent the vector of parameters to be estimated.

Equation 1, estimated for the target population of women at risk of welfare participation, provides direct estimates of the impact of welfare reform policies (π_1 and π_2) and addresses a major identification problem that is present in any policy analysis—disentangling the effects of policy shifts from other factors that may also vary over time. This methodology is known as

² We present the general model with separate estimates for AFDC waivers and TANF, as some of our models are specified that way. However, as indicated earlier, our preferred specifications use a single indicator of whether the state had implemented an AFDC waiver or TANF.

³ The legislature data are from several volumes of the Statistical Abstract of the United States, Composition of State Legislatures by Political Party Affiliation. Data on the Governor's party were obtained from the National Governor's Association. Male registration rates by state and year (from our CPS data) were included in models predicting female registration and male voting rates were included in models of women's voting behavior.

difference-in-differences (DD) and is standard in the economics literature evaluating the effects of welfare reform and other policies. Identification in the DD framework comes from comparing changes in women’s voting participation in states that have implemented welfare reform to changes in states that have not yet done so, with the implicit assumption being that the latter are a valid counterfactual for the former (that is, in the absence of welfare reform, trends in women’s voting participation would be parallel across the “treatment” and control states). However, this “parallel trends” assumption may not hold owing to a multitude of state-specific factors that may be related to voting behavior.

We address the issue of potentially confounding state-specific trends by estimating difference-in-difference-in-differences (DDD) models as represented by Equation 2 below, in which Target represents a dichotomous indicator equal to one if the individual is in the target group (population at risk of being on welfare) and zero if the individual is in the comparison group (population similar to the target group but not at risk of being on welfare).

$$(2) \quad Y_{ist} = \alpha' + (\alpha_1 - \alpha_1^*)Target_i + (\pi_1 - \pi_1^*)(AFDCWaiver_{st} * Target_i) + (\pi_2 - \pi_2^*)(TANF_{st} * Target_i) + \pi_1^*(AFDCWaiver_{st}) + \pi_2^*(TANF_{st}) + X_{ist}\beta' + Z_{st}\delta' + State_s\lambda' + \eta_{ist}$$

The choice of target and comparison groups is integral to a valid implementation of the DDD methodology. Identifying the target group—individuals who are at risk of relying on public assistance—is relatively straightforward; welfare recipients have traditionally come from low-educated unmarried-parent households. The assumption necessary for the DDD effect to represent an unbiased estimate is that in the absence of welfare reform, unobserved state-varying factors would affect the target and comparison groups similarly. If this assumption is valid, then π_1^* and π_2^* will capture the impact of the unmeasured factors that are correlated with welfare reform. In our main analyses, we compare unmarried women ages 21–49 that have a high school education or less and live with children (target group) to two alternative comparison groups: (1)

unmarried women with no children in the same age and education groups, and (2) married mothers in the same age and education groups. These comparison groups have been validated in prior work on the effects of welfare reform on women's education (Dave et al. 2011, 2012), drug use (Corman et al. 2013), and crime (Corman, Dave, and Reichman 2014) and in the broader welfare reform literature. We assess the sensitivity of our findings to the use of these two different comparisons group, as well as to the use of a third comparison group—unmarried mothers ages 21-49 with some post-high school education.

After establishing baseline estimates from DDD specifications, we consider alternative model specifications to address specific methodological challenges. First, we account for state-level factors that may be correlated with the implementation of welfare reform and that may also affect voting behaviors. Specifically, we consider the potential confounding effects of voting legislation that coincided, to some extent, with the implementation of welfare reform. The National Voter Registration Act (NVRA) of 1993 was designed to ease the process of registering to vote and maintaining registration by requiring states to allow individuals to register when they applied for or renewed a driver's license and when they visited an office providing public assistance to the poor or disabled (U.S. Justice Department, 2015). Six states already had liberal registration practices in effect when the NVRA was passed and the other states were required to implement the law in 1995 (with 1996 representing the first national election under this new regime). Although the NVRA was designed to increase voter turnout, there is convincing evidence that it was not effective in doing so (Knack, 1999, who focused on the 1996 election, and Brown and Wedeking, 2006, who focused on elections through 2004). The lack of evidence for the NVRA increasing voter turnout suggests that this legislation would not confound our

estimated effects of welfare reform on voting. Nevertheless, we assess the sensitivity of our estimates to controlling for when each state implemented the NVRA.⁴

We further control for state-specific linear trends and, more flexibly, include state*year indicators instead of NVRA implementation (alone or together with state-specific linear trends). The state*year indicators account for all observed and unobserved state-specific factors and thus also address concerns about policy endogeneity—the possibility that the timing of welfare reform implementation in states is a function of time-varying state-specific factors. We also address the issue of potentially different underlying trends in outcomes between the target and comparison groups by including an interaction between a linear trend and the target group.

Next, we assess differential effects across relevant state-level margins—namely, the dominant political party of the state legislature⁵ and strictness of the state’s welfare regime. Poor unmarried women tend to favor Democrats (Pew Research Center 2015) and may be more invested in the political process in states with party dominance that aligns with their political leanings. The latter analyses indirectly inform the extent to which the policy effects may be driven by shifts in employment by considering a dose-response check—that is, whether the observed policy effects are larger in states that had a more stringent push towards employment as part of their welfare regimes. We supplement these models by directly assessing the role of employment and family income in mediating the observed effects of welfare policy on voting participation. Finally, we assess the robustness of our estimates to using the third comparison

⁴ In contrast, both Knack (1999) and Brown and Wedeking (2006) found that election day registration (EDR), another voting-related policy implemented by some states that was not required by the NVRA but was consistent with its intent, did increase voter turnout. Only three states implemented EDR during the rollout of welfare reform—Idaho and Wyoming in 1994 and New Hampshire in 1996 (National Council of State Legislatures 2015). Excluding these states from our analyses does not materially affect any of the estimates presented or referred to in this paper.

⁵ Data on the composition of state legislatures by party were obtained from the National Conference of State Legislatures. We used two binary variables indicating Democratic Legislature >50% and Republican Legislature >50% (the third possibility is that neither party holds a majority). . In Nebraska, the state legislature is non-partisan so its representation in the national legislature is used.

group described earlier, and—using each of the primary comparison groups with education redefined accordingly—the extent to which effects were stronger for women with less than a high school education, who may have not have fared as well their more educated counterparts under the new welfare regime. That is, this group may have experienced a decrease in total income, with the loss of welfare benefits not being fully compensated by the increase in earnings from work.

V. Results

Table 1 shows mean percentages that registered and voted by year for the target group and our two primary comparison groups—(1) unmarried childless women ages 21–49 with a high school education or less, and (2) married mothers in the same age and education groups. For (1), the average difference between the target and comparison group in the percent registered to vote in the pre-welfare reform years (1990 and 1992) was about four percentage points, while the average difference in the post-welfare reform years (1998 to 2004) was about three percentage points. Thus, on average, the percent registered declined by about one percentage point. The corresponding difference voted was about four percentage points. For (2), the average difference between the target and comparison groups in the percent registered to vote in the pre-welfare reform years was about 13 percentage points, and this difference did not decrease in the post-welfare reform period. However, the corresponding difference that voted was about 2.5 percentage points. These mean differences are suggestive of modest positive effects of welfare reform on voting behavior of women at risk for welfare reliance. However, these differences may be confounded by other changes occurring over the period, and the multivariate DDD models in subsequent tables address this concern.

Similar patterns are evident in Figures 1a&b and Figures 2a&b, for our target group (unmarried mothers age 21–49 with at most a high school education) and the two primary comparison groups. The time series in these figures correspond to numbers of years before and after welfare reform was implemented in the woman’s state of residence.⁶ Before welfare reform, women in the target group were less likely to register and to vote than those in the comparison groups. However, as welfare reform was implemented, the target group appeared to exhibit behavior more like that of the comparison groups (particularly the first).⁷ This possible narrowing of the differences in registration and voting patterns between women most at risk of welfare receipt and similar women who were unlikely to be impacted by welfare policy, even without conditioning on any other factors, is suggestive that welfare reform may have played some role in increasing the probability of voting among low-educated unmarried mothers. However, as for the differences in means, these trends may be confounded by other changes and the multivariate DDD models in subsequent tables address this concern.

From our November CPS data, we also found that employment increased much more substantially for our target group than for either of the comparison groups after the implementation of welfare reform. Specifically, we compared the percentages of each group that were employed before and after welfare reform was implemented in their state of residence. In unadjusted results, the employment rate of the target group grew 7.5 percentage points relative to comparison group 2 and grew 18.2 percentage points relative to the growth in employment in comparison group 1⁸ This difference, which was expected based on past literature, provides

⁶ A value of negative 1 corresponds to the twelve month period preceding the implementation of welfare reform in the woman’s state, and a value of 0 corresponds to the first twelve months after welfare reform implementation.

⁷ Note that there could be up to a two year lag between the implementation of welfare reform and a Congressional election, and there could be up to a four year lag between the implementation of welfare reform and a Presidential election.

⁸ Some of this difference may be due to unobserved trends and economic conditions. The conditional DDD estimate on employment (discussed later), based on Equation 2, is 6.4 percentage points and 10.0 percentage points for

further validation of our target and comparison groups. Also validating, more generally, is that in Table 1, we can see that for all low-educated women in the relevant age range, both registration and voting were higher in Presidential election years than in non-Presidential election years.

Table 2a presents regression results from linear probability models predicting voting behavior using our first comparison group (unmarried women with no children). Table 2b presents corresponding estimates using the second comparison group (married mothers with at most a high school education). In both tables, the estimates in the first two columns are for “registered to vote” as the dependent variable; those in the next two columns are for “voting in any even-numbered year,” those in the fifth column are for “voting in a Presidential election year,” and those in the last column are for “voting in a non-Presidential even-numbered year” (Congressional election). For the first two outcomes there are two sets of regressions. The first corresponds to Equation 2, where AFDC waiver programs and TANF programs are allowed to have separate effects. The second combines AFDC waivers and TANF into one indicator—any welfare reform. For Models 5 and 6, we present results only for “any welfare reform,” our preferred measure for this paper for reasons discussed earlier.

These DDD estimates are consistent with the trends presented in Table 1 and the figures, and indicate that welfare reform was associated with an increase in registration and voting among low-educated unmarried mothers. Model 1 in Table 2a suggests that TANF increased registration and voting by 3.5 and 2.7 percentage points, respectively, but that the AFDC waivers had no effect. However, as indicated earlier, the separate estimates for AFDC waivers and TANF, particularly the former, should be interpreted with caution because they are based on few elections. Using the same comparison group and the single measure of any welfare reform (our

groups 2 and 1, respectively (p-value=0.000), which is also consistent with the literature of the effects of welfare reform on employment. .

preferred measure for this study), we find that welfare reform increased registration and voting by 2.9 and 1.9 percentage points, respectively, and that the effects on voting were much stronger in Presidential election years (2.7 percentage points). These results translate to 6.0 , 7.0, and 6.4 % increases relative to the baseline means for registration, voting in any election, and voting in a Presidential election, respectively, for the target group.

The results are similar when using the other comparison group (Table 2b), although the AFDC waivers as well as TANF were positively associated with registration and voting (and statistically significant for the former). Results using the single measure of any welfare reform, our preferred measure for this study, suggests that welfare reform increased voting registration by about 2.8 percentage points (Model 2), voting in any election by 2.3 percentage points (Model 4), and voting in a Presidential election by 3.9 percentage points, with the last translating to about a 9.2% increase relative to the baseline mean.

The magnitudes in Tables 2a&b are plausible given that similar specifications using both comparison groups find increases in employment on the order of 6–10 percentage points among the target group of women (reported in footnote 8). If we assume that employment is the main channel by which welfare reform affected voting behaviors, this would suggest a marginal “treatment-on-the-treated” effect of employment on voting of .19 to .36.⁹ This compares to an average propensity to vote, conditional on employment, of 0.396, among the target group prior to welfare reform. Given that the effects are strongest in Presidential election years, the remaining analyses focus on voting in Presidential years and use the relevant subset of the data. However, the pattern of results is similar when we use the full sample instead (not shown).

⁹ To get these numbers, we divide the coefficients in specification (5) in Tables 2a and 2b by the growth in employment relative to the comparison group. That is, .19 is obtained by dividing .0188 by .10 and .36 is obtained by dividing .0233 by .064.

As indicated earlier, a key concern underlying the DDD estimates in Tables 2a&b relates to unobserved time-varying state factors that may be correlated with the timing of welfare reform and which may also impact voting participation. A related concern is that the comparison groups may not be perfect counterfactuals for the target group, and thus may not fully purge the effects of all unobserved time-varying state-specific factors. We address these concerns in the models shown in Tables 3a&b.

Results shown in the first columns of both Table 3a and Table 3b (Model 1) are the “baseline estimates” from Model 5 in Table 2a and Table 2b, respectively, which use the single measure of any welfare reform. One potentially important policy confounder, as described in the previous section, is the National Voter Registration Act, which most states implemented in 1995 though several states had implemented similar provisions earlier. Model 2 in each of these tables controls for the implementation of the law or whether the state had implemented similar (or more liberal) provisions in the past. While 1996 was the first national election year under the NVRA, 14 states implemented welfare reform after the 1996 election, and 13 states had provisions similar to (or more liberal than) the NVRA prior to 1996. Thus, there is substantial variation in welfare reform implementation even after controlling for provisions of the NVRA. The estimated effects of any welfare reform on voting participation are unaffected when introducing this control. Model 3 in each of these tables additionally controls for unobserved time-varying state factors by including state-specific linear trends, and the estimates are slightly smaller than those in Models 1 and 2. Model 4 controls for all observed and unobserved time-varying state factors through the inclusion of a full set of state*year fixed effects instead of indicators for NVRA or state-specific linear trends¹⁰. The estimated effects of welfare reform decline somewhat in

¹⁰ Note that all state/year variables drop out of the equation when state/year dummies are included.

magnitude but remain statistically significant. As indicated earlier, saturating the model with state*year fixed effects also addresses concerns about policy endogeneity.

Another empirical concern related to state trends is that within a given state, there may be differential outcome trends between the target and comparison groups. In Model 5, we control for any such differential pre-policy trends by including an interaction between a linear trend and the target group. When using the first comparison group (Table 3a), the estimated effect of any welfare reform in this specification is lower than that in any other the other specifications and marginally significant. When using the second comparison group (Table 3b), the estimated effect becomes larger than that in any of the other specifications and suggests a 5 percentage point increase in voting participation; this compares to an estimated 4 percentage point increase in the baseline specification (Model 1) that does not control for the state*year indicators or differential pre-policy trends. Given the general robustness of the estimates across all of these specifications and to preserve degrees of freedom, we use Model 1 from these tables as the baseline for comparison in subsequent specifications.

The results in Tables 2a&b and 3a&b consistently suggest that welfare reform is associated with an increase in voting participation, especially in Presidential election years. These are mean effects realized over all states, weighted by the target population in each state. Table 4 assesses heterogeneity in these effects across relevant state margins. Models 1 and 2 (using comparison group 1) and 5 and 6 (using comparison group 2) stratify the sample based on whether the state's legislature had a Republican or a Democratic majority. As expected, we find that the increase in voting participation associated with welfare reform was driven primarily by Democratic states; in states with a Republican majority in the legislature, welfare reform is not associated with any significant increase in voting behaviors. Models 3 and 4 (and Models 7 and

8) stratify the sample into strict/moderate vs. weak work incentives based on the typology developed by Blank and Schmidt (2001) that incorporates various features of states' TANF programs (benefit generosity, earnings disregards, sanctions, and time limits) and categorizes states as strong, weak, or mixed in overall work incentives. If our estimated effects of welfare reform on voting represent causal links and operate through employment (the first-order effect of welfare reform), we would expect the effects to be stronger in states that enacted stricter pro-employment policies under welfare reform. Due to limited sample sizes upon stratification, standard errors inflate and render some of the estimates imprecise. However, the effects are clearly larger in states with stronger employment-based incentives, suggesting that the effects of welfare reform operate, at least in part, through employment.

Table 5 directly assesses the mediating effects of employment as well as those of education and family income. Models 1 and 5 add employment status (an indicator for whether the woman is currently employed) to the baseline specifications (Model 5 in Tables 2a&b, respectively). The effects of welfare reform decline in magnitude, and, as expected, current employment is positively associated with voting participation. Models 2 and 6 instead include categorical indicators for weekly hours worked, with non-employment as the reference category. Models 3 and 7 further include the natural log of family income, and the final specifications (Models 4 and 8) add an indicator for high school completion. Comparing the estimated effects of welfare reform in Models 4 and 8 with those from the corresponding baseline models, we find that a large part of the effects of welfare reform on voting appears to be mediated by employment, income, and education – factors that prior studies found or suggest were affected by welfare reform. The estimates in Models 4 and 8 again suggest that employment and family income increase voting participation, although conditional on employment, more working hours

reduce voting participation, likely owing to time constraints. Higher educational attainment is also associated with increased voting participation. The mediation analyses in Table 5, which are consistent with the hypothesized effects, should be interpreted with caution since the mediators are endogenous and constitute what Angrist and Pischke (2009) refer to as “bad controls.” However, they provide a useful first look at hypothesized pathways.

In supplementary analyses (not shown) we assess the sensitivity of our estimates to a third comparison group, as guided by the literature (e.g., Dave et al. 2011)—unmarried mothers age 21–49 with at least some post high school education. We find consistent evidence, regardless of the comparison group being utilized, that welfare reform is associated with a 3–5 percentage point increase in voting participation. We also estimate models for the least educated of the target sample (those less than a high school education), adjusting the education cutoff for the comparison groups accordingly. When limiting the sample to women in the lowest tail of the education distribution, we find positive but much smaller and statistically insignificant effects of welfare reform compared to our main models, suggesting that welfare reform exacerbated both time and income constraints for this group.

VI. Conclusions

This study found robust evidence that welfare reform in the U.S. in the 1990s increased the likelihood of women’s voting in an election by about 2 percentage points (for any even-year election) to 3–4 percentage points (Presidential election), which translates to about a 6 to 9% increase relative to the baseline means. The effects were largely confined to Presidential elections, were stronger in states that had a majority of Democrats as state legislators, were stronger in states with stronger work incentive policies, and appeared to operate through employment, education, and income.

The findings from this study inform culture of poverty debates by providing a rigorous test of the widely-embraced argument, on both sides of the political spectrum, that welfare reform brings women from the margins to the mainstream and encourages pro-social behavior. It complements previous studies finding that welfare reform reduced women's binge drinking, illicit drug use, and property crime (Kaestner & Tarlov 2006; Corman et al. 2013; Corman, Dave, and Reichman 2014). As far as we know, no previous population-based studies have investigated the causal effects of welfare policy on voting, on any other form of civic participation, or on any non-targeted mainstream behaviors more generally (targeted behaviors being working and marriage), and no such studies of which we are aware have found undesirable effects on non-targeted behaviors. As such, this study adds to the growing evidence that welfare reform encourages mainstream behavior.

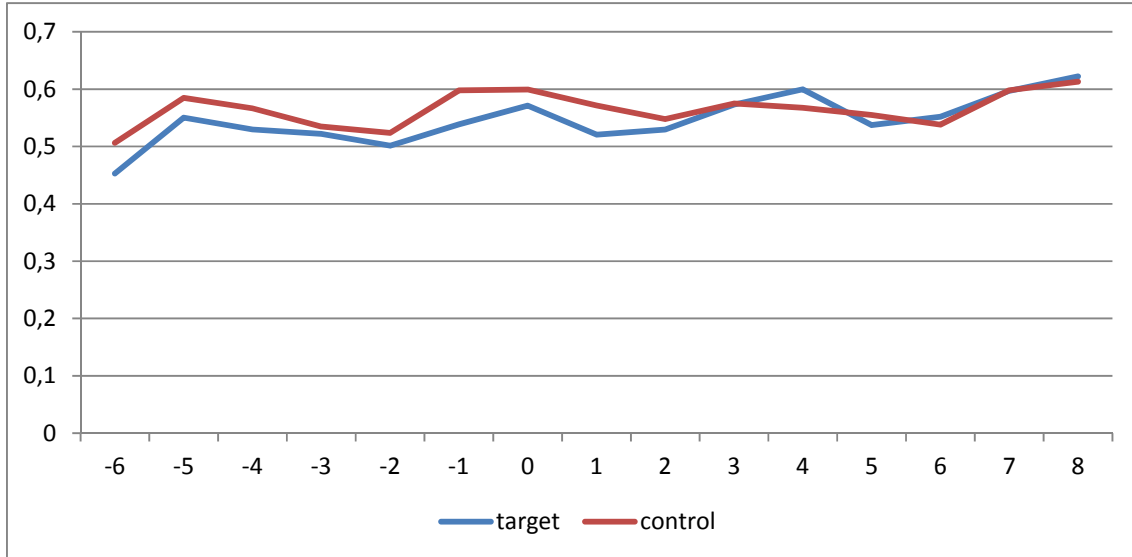
The findings make important contributions to the multi-disciplinary literature on the determinants of voting by providing a strong test of the effects of work incentives (and, by inference, employment) and to the political science literature on how citizens' experiences with government programs affect their political participation. Finally, the findings provide a more complete picture of the effects of a major policy shift in the U.S. that is still very much in effect today and under which the next generation has been raised.

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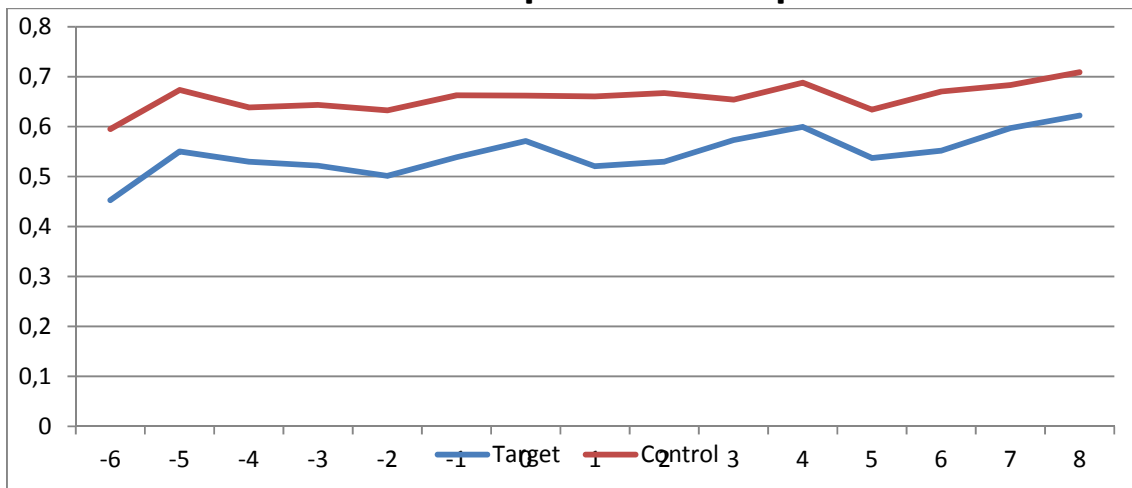
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**Figure 1a –Registered to Vote by Years Since Welfare Reform
—Comparison Group 1**



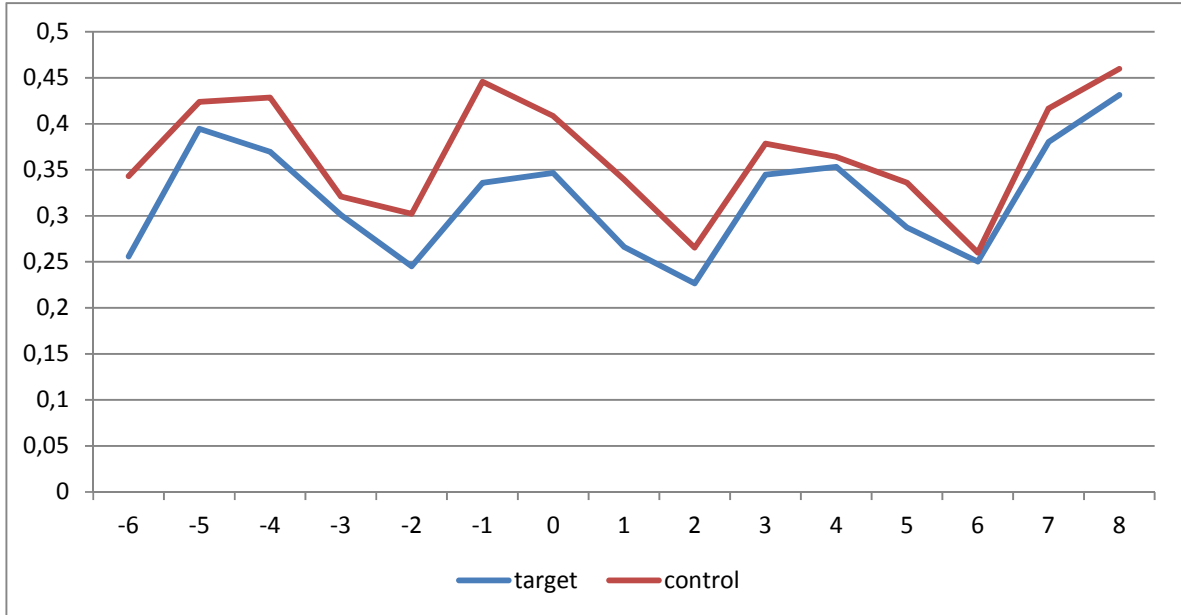
Target Group – Unmarried mothers, age 21-49, high school education or less
 Comparison Group – Unmarried women with no children, age 21-49, high school education or less

**Figure 1b –Registered to Vote by Years Since Welfare Reform
—Comparison Group 2**



Target Group – Unmarried mothers, age 21-49, high school education or less
 Comparison Group -- Married mothers, age 21-49, high school education or less

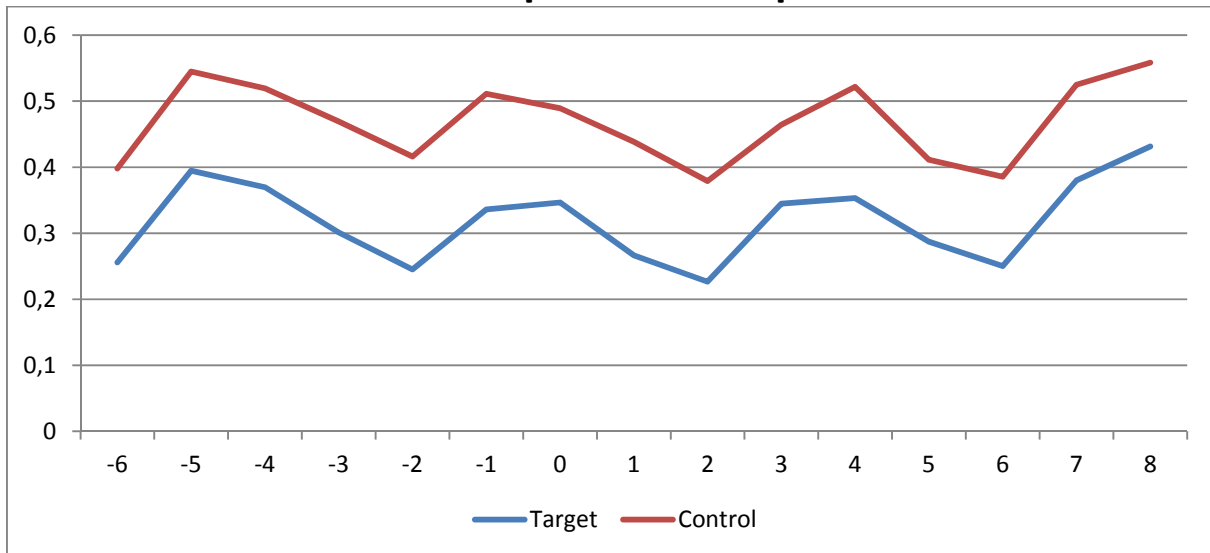
**Figure 2a –Voted in Election by Years Since Welfare Reform
—Comparison Group 1**



Target Group – Unmarried mothers, age 21-49, high school education or less

Comparison Group – Unmarried women with no children, age 21-49, high school education or less

**Figure 2b –Voted in Election by Years Since Welfare Reform
—Comparison Group 2**



Target Group – Unmarried mothers, age 21-49, high school education or less

Comparison Group -- Married mothers, age 21-49, high school education or less

Table 1
Means: Women 21–49, U. S. Citizens
November Current Population Surveys with Voting and Registration Supplements
1990–2004
Target & Comparison Groups

	Target		Comparison Group 1		Comparison Group 2	
	Unmarried Mother High School Education or Less		Unmarried No Children High School Education or Less		Married Mothers High School Education or Less	
	Registered	Voted	Registered	Voted	Registered	Voted
1990	0.490	0.260	0.526	0.325	0.622	0.405
1992	0.559	0.416	0.602	0.493	0.680	0.597
1994	0.483	0.226	0.521	0.275	0.614	0.384
1996	0.572	0.350	0.594	0.408	0.663	0.501
1998	0.533	0.228	0.532	0.264	0.650	0.360
2000	0.586	0.385	0.580	0.405	0.692	0.550
2002	0.550	0.238	0.542	0.266	0.648	0.371
2004	0.627	0.463	0.624	0.483	0.713	0.588

Table 2a
Effects of Welfare Reform on Registration & Voting
Target Group: Unmarried Mothers, Ages 21–49, High School Education or Less
Comparison Group 1: Unmarried Women with No Children, Ages 21–49, High School Education or Less

Model Outcome	(1) Registered	(2) Registered	(3) Voted	(4) Voted	(5) Voted in Presidential Election	(6) Voted in Non- Presidential Election
AFDC Waiver	0.0125 (0.0221)		0.0227 (0.0138)			
TANF	-0.0162 (0.0177)		-0.0155 (0.0161)			
Waiver*Target	-0.0026 (0.0163)		-0.0240 (0.0145)			
TANF*Target	0.0354*** (0.0101)		0.0269*** (0.0091)			
Any Welfare Reform		-0.0081 (0.0144)		-0.0048 (0.0116)	-0.0487** (0.0191)	0.0368 (0.0249)
Welfare Reform*Target		0.0293*** (0.0080)		0.0188** (0.0080)	0.0274** (0.0112)	0.0100 (0.0111)
Observations	32234	32234	32790	32790	15669	17121

Notes: Coefficients from OLS models are reported. State-clustered standard errors are reported in parentheses. All models control for state and year fixed effects and include the following covariates: age, age-squared, race (black, other race), Hispanic, marital history (widowed/divorced/separated, never married), number of children <18, number of household members 18+, MSA residence, current and 1-year lagged state unemployment rate, current and 1-year lagged state personal income per capita, log female population, state poverty rate, state minimum wage, 1- and 2-year lagged welfare caseloads, % state legislature Democrat/Republican, Republican governor, Democrat governor, and registration/voting rates of males. Asterisks denote statistical significance as follows: *p ≤ .10; **p ≤ .05; ***p ≤ .01.

Table 2b
Effects of Welfare Reform on Registration & Voting
Target Group: Unmarried Mothers, Ages 21–49, High School Education or Less
Comparison Group 2: Married Mothers, Ages 21–49, High School Education or Less

Model Outcome	(1) Registered	(2) Registered	(3) Voted	(4) Voted	(5) Voted in Presidential Election	(6) Voted in Non- Presidential Election
AFDC Waiver	-0.0123 (0.0118)		-0.0179 (0.0118)			
TANF	0.0011 (0.0149)		0.0001 (0.0142)			
Waiver*Target	0.0421** (0.0159)		0.0255 (0.0155)			
TANF*Target	0.0238** (0.0097)		0.0228** (0.0092)			
Any Welfare Reform		-0.0056 (0.0110)		-0.0134 (0.0101)	-0.0457*** (0.0148)	0.0263* (0.0153)
Welfare Reform*Target		0.0272*** (0.0096)		0.0233*** (0.0087)	0.0390*** (0.0133)	0.0154 (0.0104)
Observations	54470	54470	55048	55048	25796	29252

Notes: Coefficients from OLS models are reported. State-clustered standard errors are reported in parentheses. All models control for state and year fixed effects and include the following covariates: age, age-squared, race (black, other race), Hispanic, marital history (widowed/divorced/separated, never married), number of children <18, number of household members 18+, MSA residence, current and 1-year lagged state unemployment rate, current and 1-year lagged state personal income per capita, log female population, state poverty rate, state minimum wage, 1- and 2-year lagged welfare caseloads, % state legislature Democrat/Republican, Republican governor, Democrat governor, and registration/voting rates of males. Asterisks denote statistical significance as follows: *p ≤ .10; **p ≤ .05; ***p ≤ .01.

Table 3a
Effects of Welfare Reform on Voting (Presidential Elections)
Accounting for Differential Trends
Target Group: Unmarried Mothers, Ages 21–49, High School Education or Less
Comparison Group 1: Unmarried Women with No Children, Ages 21–49, High School Education or Less

Model	(1)	(2)	(3)	(4)	(5)
Any Welfare Reform	-0.0487** (0.0191)	-0.0532** (0.0199)	-0.0526** (0.0226)	-0.3110*** (0.0086)	-0.3099*** (0.0087)
Welfare Reform*Target	0.0274** (0.0112)	0.0272** (0.0112)	0.0264** (0.0114)	0.0227** (0.0109)	0.0197* (0.0112)
Motor Voter Law	No	Yes	Yes	No	No
State Linear Trend	No	No	Yes	No	No
State * Year Indicators	No	No	No	Yes	Yes
Target *	No	No	No	No	Yes
Linear Pre-policy Trend					
Observations	15669	15669	15669	20682	20682

Notes: See Table 2a. Model 1 is identical to Model 5 in Table 2a. Models 2 through 5 add covariates to that model, as indicated.

Table 3b
Effects of Welfare Reform on Voting (Presidential Elections)
Accounting for Differential Trends
Target Group: Unmarried Mothers, Ages 21–49, High School Education or Less
Comparison Group 2: Married Mothers, Ages 21–49, High School Education or Less

Model	(1)	(2)	(3)	(4)	(5)
Any Welfare Reform	-0.0457*** (0.0148)	-0.0489*** (0.0143)	-0.0516*** (0.0150)	-0.3604*** (0.0048)	-0.3686*** (0.0058)
Welfare Reform*Target	0.0390*** (0.0133)	0.0390*** (0.0132)	0.0387*** (0.0130)	0.0248** (0.0112)	0.0507*** (0.0158)
Motor Voter Law	No	Yes	Yes	No	No
State Linear Trend	No	No	Yes	No	No
State * Year Indicators	No	No	No	Yes	Yes
Target *	No	No	No	No	Yes
Linear Pre-policy Trend					
Observations	25796	25796	25796	35482	35482

Notes : See Table 2b. Model 1 is identical to Model 5 in Table 2b. Models 2 through 5 add covariates to that model, as indicated.

Table 4
Effects of Welfare Reform on Voting (Presidential Elections)
Heterogeneous Effects Along Relevant State Margins

Model Sample	Comparison Group 1				Comparison Group 2			
	(1) State Legislature Republican	(2) State Legislature Democratic	(3) Work Incentives Strong	(4) Work Incentives Weak	(5) State Legislature Republican	(6) State Legislature Democratic	(7) Work Incentives Strong	(8) Work Incentives Weak
Any Welfare Reform	-0.0573*	-0.0618*	-0.0500**	0.0867	-0.0147	-0.0888***	-0.0556***	0.0979**
	(0.0312)	(0.0308)	(0.0216)	(0.0507)	(0.0176)	(0.0215)	(0.0156)	(0.0339)
Welfare Reform*Target	0.0151	0.0337**	0.0326***	-0.0192	0.0053	0.0469***	0.0509***	-0.0512
	(0.0177)	(0.0152)	(0.0105)	(0.0546)	(0.0351)	(0.0155)	(0.0133)	(0.0422)
Observations	5669	9144	13852	1817	9448	14926	22591	3205

Notes: See Tables 2a&b. Model specification for all models in this table is the same as for Model 5 in Tables 2a&2b. Models 1 and 5 include state/year observations in which the percentage of state legislators that were Republican was greater than 50%. Models 2 and 6 include state/year observations in which the percentage of state legislators that were Democratic was greater than 50%. For Nebraska, we use the corresponding percentages of national legislators from that state, since no Nebraska state legislators have official party affiliation (that is, they are all nonpartisan).

Table 5
Effects of Welfare Reform on Voting (Presidential Elections)
Mediating Effects of Employment, Income, and Education

Model	Comparison Group 1				Comparison Group 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Welfare Reform	-0.0471** (0.0187)	-0.0462** (0.0187)	-0.0492*** (0.0183)	-0.0488*** (0.0181)	-0.0437*** (0.0146)	-0.0438*** (0.0146)	-0.0357** (0.0157)	-0.0367** (0.0155)
Welfare Reform*Target	0.0156 (0.0113)	0.0153 (0.0112)	0.0197* (0.0116)	0.0168 (0.0117)	0.0331** (0.0132)	0.0327** (0.0132)	0.0225* (0.0127)	0.0182 (0.0126)
Employed	0.1378*** (0.0110)				0.0852*** (0.0077)			
Weekly Hours < 10		0.0522* (0.0282)	0.0581* (0.0304)	0.0479 (0.0303)		0.1044*** (0.0218)	0.0952*** (0.0223)	0.0847*** (0.0229)
Weekly Hours 10 – 20		0.0854*** (0.0188)	0.0701*** (0.0191)	0.0603*** (0.0192)		0.1004*** (0.0113)	0.0767*** (0.0114)	0.0631*** (0.0113)
Weekly Hours 20 – 30		0.1082*** (0.0156)	0.0747*** (0.0154)	0.0563*** (0.0156)		0.0780*** (0.0114)	0.0519*** (0.0105)	0.0386*** (0.0103)
Weekly Hours 30 – 40		0.1514*** (0.0110)	0.0996*** (0.0112)	0.0778*** (0.0114)		0.0906*** (0.0085)	0.0453*** (0.0081)	0.0299*** (0.0083)
Weekly Hours > 40		0.1393*** (0.0179)	0.0783*** (0.0169)	0.0566*** (0.0172)		0.0752*** (0.0127)	0.0266** (0.0117)	0.0123 (0.0119)
Ln Family Income			0.0750*** (0.0054)	0.0624*** (0.0052)			0.1018*** (0.0054)	0.0830*** (0.0052)
High School Graduate				0.1290*** (0.0118)				0.1568*** (0.0100)
Observations	15669	15669	15669	15669	25796	25796	23740	23740

Notes: See Tables 2a&b. Model specification for all models in this table is the same as for Model 5 in Tables 2a&2b, with the addition of mediators indicated.

Appendix Table 1
Implementation Dates of Welfare Reform by State, U.S.

	10/92 to 2/97	9/96 to 1/98	10/92 to 1/98		10/92 to 2/97	9/96 to 1/98	10/92 to 1/98
	AFDC Waiver	TANF	Any Welfare Reform		AFDC Waiver	TANF	Any Welfare Reform
Alabama		Nov-96	Nov-96	Montana	Feb-96	Feb-97	Feb-96
Alaska		Jul-97	Jul-97	Nebraska	Oct-95	Dec-96	Oct-95
Arizona	Nov-95	Oct-96	Nov-95	Nevada		Dec-96	Dec-96
Arkansas	Jul-94	Jul-97	Jul-94	New Hampshire		Oct-96	Oct-96
California	Dec-92	Jan-98	Dec-92	New Jersey	Oct-92	Jul-97	Oct-92
Colorado		Jul-97	Jul-97	New Mexico		Jul-97	Jul-97
Connecticut	Jan-96	Oct-96	Jan-96	New York		Nov-97	Nov-97
DC		Mar-97	Mar-97	North Carolina	Jul-96	Jan-97	Jul-96
Delaware	Oct-95	Mar-97	Oct-95	North Dakota		Jul-97	Jul-97
Florida			Oct-96	Ohio	Jul-96	Oct-96	Jul-96
Georgia	Jan-94	Jan-97	Jan-94	Oklahoma		Oct-96	Oct-96
Hawaii	Feb-97	Jul-97	Feb-97	Oregon	Feb-93	Oct-96	Feb-93
Idaho		Jul-97	Jul-97	Pennsylvania		Mar-97	Mar-97
Illinois	Nov-93	Jul-97	Nov-93	Rhode Island		May-97	May-97
Indiana	May-95	Oct-96	May-95	South Carolina		Oct-96	Oct-96
Iowa	Oct-93	Jan-97	Oct-93	South Dakota	Jun-94	Dec-96	Jun-94
Kansas		Oct-96	Oct-96	Tennessee	Sep-96	Oct-96	Sep-96
Kentucky		Oct-96	Oct-96	Texas	Jun-96	Nov-96	Jun-96
Louisiana		Jan-97	Jan-97	Utah	Jan-93	Oct-96	Jan-93
Maine		Nov-96	Nov-96	Vermont	Jul-94	Sep-96	Jul-94
Maryland	Mar-96	Dec-96	Mar-96	Virginia	Jul-95	Feb-97	Jul-95
Massachusetts	Nov-95	Sep-96	Nov-95	Washington	Jan-96	Jan-97	Jan-96
Michigan	Oct-92	Sep-96	Oct-92	West Virginia		Jan-97	Jan-97
Minnesota		Jul-97	Jul-97	Wisconsin	Jan-96	Sep-97	Jan-96
Mississippi	Oct-95	Jul-97	Oct-95	Wyoming		Jan-97	Jan-97
Missouri	Jun-95	Dec-96	Jun-95				

Source: U.S. Department of Health and Human Services (1999).