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Effects of Maternal Work Incentives on Teen Drug Arrests

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Abstract

Purpose—This study exploits differences in the implementation of welfare reform across states and over time in the United States in the attempt to identify causal effects of welfare reform on youth arrests for drug-related crimes between 1990 and 2005, the period during which welfare reform unfolded.

Methodology—Using monthly arrest data from the U.S. Federal Bureau of Investigation's Uniform Crime Reports, we estimate the effects of welfare reform implementation on drug-related arrests among 15–17 year olds in the United States between 1990 and 2005. We use a difference-in-differences (DD) approach that exploits the implementation of welfare reform across states and over time to estimate effects for teens exposed to welfare reform.

Findings—The findings, based on numerous different model specifications, suggest that welfare reform had no statistically significant effect on teen drug arrests. Most estimates were positive and suggestive of a small (3%) increase in arrests.

Originality/Value—This study investigated the effects of a broad-based policy change that altered maternal employment, family income, and other family characteristics on youth drug arrests.

Keywords

teen drug use; welfare reform; maternal employment; drug arrests; drug crime

Introduction

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in the United States and earlier state legislation that preceded it (collectively referred to as welfare reform), provide an opportunity to study the effects of maternal work incentives, and by inference, employment, on behaviors of adolescent children. The broad goal of welfare

reform was to reduce dependence on cash assistance by promoting employment. Despite successful expansions of the Earned Income Tax Credit over the decades leading up to welfare reform that increased the labor supply of single mothers (Eissa & Liebman 1996), many mothers on welfare had not transitioned to work as of the early 1990s. The key strategy under welfare reform was to aggressively encourage maternal employment by imposing work requirements as conditions for receipt of cash assistance and time limited benefits.

In terms of caseloads, welfare reform has been deemed an unqualified success. Caseloads declined by 50% between 1997 and 2001 alone, and over half of cases are now “child only,” meaning that adults in the household are not eligible (Loprest 2012). Welfare reform was also successful in increasing employment. Employment rates of low-skilled mothers rose dramatically after the early 1990s, and there is strong consensus that welfare reform played a major role (Schoeni & Blank 2000; Ziliak 2006).

An implicit assumption underlying welfare reform was that the work-focused regime would disrupt the transmission of welfare dependence to the next generation by putting mothers to work, which can increase family resources and/or lead mothers to model socially desirable behavior, and as a result, change youths' expectations about welfare reliance as a long-term option. However, some families may not have achieved greater financial security under the new regime or may have confronted new work-family challenges, both of which could adversely affect children. Studies have found that welfare reform reduced high school dropout and teen fertility (Dave, Corman & Reichman 2012; Kaestner, Korenman & O'Neill 2003; Koball 2007; Lopoo & Deleire 2006; Miller & Zhang 2012; Offner 2005). However, very little is known about the effects of welfare reform on other teen behaviors, such as drug, alcohol, and tobacco use, that are associated with future economic success and well-being.

Drug use among teenagers is a serious public health concern in the United States. In 2015, over one-quarter of American youth in grade 10 (about 15–16 years old) reported having ever used illicit drugs during the past year (Johnston, O'Malley, Miech, Bachman & Schulenberg 2016). Teen drug use is associated with school problems, early initiation of sexual activity, delinquent behavior, physical and mental health problems, accidental injuries, problems with peer relationships, and family conflict (Ewing et al. 2015; Crowe & Sydney 2000). Furthermore, because drugs are illegal substances, use can result in arrest and imprisonment. Some users become involved in the drug trade, which carries stiff penalties, and youth who are incarcerated can suffer adverse physical and mental health consequences and are at increased risk of engaging in subsequent criminal behavior (Travis, Western & Redburn 2014). Having a criminal record can follow a teen into adulthood, limiting his or her employment and educational opportunities. Although the overall rate of illicit drug use among teens appears to have declined somewhat since 1996 (the year that the PRWORA legislation was enacted), rates of teen deaths from drug overdoses were at an all-time high in 2014, the most recent year reported (Rudd, Aleshire, Zibbell & Gladden 2016). However, none of these crude trends are necessarily related to welfare reform.

Economic theory suggests that teens' involvement with illicit drugs would be responsive to the new work-based regime. However, the expected direction of the overall effects is

ambiguous. On one hand, increased family resources and human capital that may result from maternal employment may increase both maternal investments in the health of their children and maternal modeling of socially favorable behaviors. As such, we would expect welfare reform to have led to decreases in drug-related behaviors among teenage children. On the other hand, requiring mothers to work may reduce the quantity or quality of adult supervision of their teenage children, which would decrease the costs to the teens of engaging in those behaviors. In addition, welfare reform makes welfare reliance less of an option for the future, which may lead teens, particularly girls, to invest more in their own human capital. Available evidence on these various potential pathways is reviewed below (see *Potential pathways from welfare reform to teen drug use*). Finally, previous research found that welfare reform led to decreases in education among mothers at risk for relying on welfare (Dave, Reichman, Corman, & Das 2011; Dave, Corman & Reichman 2012), and the expected effects of this substitution away from education-based human capital toward potential job-related human capital on adolescents' circumstances and behaviors are not clear. Overall, the aggregate net directional effects of welfare reform on teens' illicit drug use—necessary first steps to establish in order to understand the underlying processes—are ambiguous.

Using monthly arrest data from the U.S. Federal Bureau of Investigation's Uniform Crime Reports, we estimate the effects of welfare reform on drug-related arrests among teenagers in the United States between 1990 and 2005. We use a difference-in-differences (DD) approach that exploits the implementation of welfare reform across states and over time to estimate both short-run effects for teens exposed to welfare reform and longer-term effects for young adults who came of age when welfare reform was implemented.

Background

Welfare reform in the United States

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), often referred to as welfare reform, ended entitlement to welfare under Aid to Families with Dependent Children (AFDC) and replaced the AFDC program with Temporary Assistance for Needy Families (TANF) block grants to states. Although welfare reform is often dated to the PRWORA legislation, reforms started taking place in the early 1990s when the Clinton Administration greatly expanded the use and scope of “welfare waivers” to allow states to carry out experimental changes to their AFDC programs. Waivers were approved in 43 states, ranging from modest demonstration projects to broad statewide programs, and constituted the first phase of welfare reform. Many policies and features of waivers were later incorporated into PRWORA. Among the features of PRWORA and many waiver programs were work requirements as a condition of receiving welfare and lifetime limits on welfare receipt. 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. This policy shift in the United States spurred other countries, including Canada and many European countries, to undertake their own reforms (Boustany 2015).

Effects of welfare reform on teen behaviors

Existing studies of effects of welfare reform on teen behaviors (other than those focusing on fertility or high school completion, mentioned earlier) have been based primarily on specific pre-PRWORA waiver experiments (looking at school behaviors) or the post-PRWORA “Three City Study.” Syntheses of the effects of waivers found no effects of a mother being assigned to a work group on her adolescent children's suspension or expulsion from school (Gennetian et al. 2002) and mixed effects of various waiver experiments on school behavior problems (e.g., suspension, expulsion) (Gennetian et al. 2004). Morris & Michalopoulos (2000) analyzed a welfare-to-work experiment in Canada in which an experimental group of single mothers receiving income assistance were offered an income supplement if they worked for at least 30 hours per week. They found that teen children of mothers in the experimental group were more likely to use marijuana (but not hard drugs) than teen children of mothers in a control group.

Chase-Lansdale et al. (2003), Chase-Lansdale et al. (2011), Coley, Bachman, Votruba-Drzal, Lohman & Li-Grinning (2007), Lohman, Pittman, Coley & Chase-Lansdale (2004), and other “Three City” studies focused on mothers' transitions into/out of welfare or work, finding that transitions into (off of) welfare were adversely (favorably) associated with teens' delinquent behaviors including substance use, while transitions into/out of work had associations in the opposite direction.

Dunifon, Hynes & Peters (2006) estimated effects of various state welfare policies on school suspension/expulsion and after-school activities using data from the Survey of Income and Program Participation, and found that children engaged in fewer activities in states that had generous earnings disregards (the inverse of the rate at which welfare benefits are reduced with respect to an increase in earnings) but that other policies did not appear to matter. However, that study focused on children ages 5 years and over, many of whom were not old enough to make autonomous decisions, and did not consider substance use.

Overall, results from the limited literature to date suggest mixed effects of welfare reform on teen behaviors, with very few studies looking specifically at substance abuse. We next turn to other strands of literature that can inform our expectations about *how* welfare reform might affect teens' involvement with illicit drugs. Although we cannot explicitly test most hypothesized pathways with our data, we review the empirical evidence on relevant linkages, both to integrate our study with the existing literature and to set the stage for future research.

Potential pathways from welfare reform to teen drug use

Maternal work is the most direct and obvious pathway by which welfare would be expected to affect teen drug use. Maternal employment could lead to increased income, which could lead to greater resources, such as employer-sponsored health insurance (which could increase access to mental healthcare) and supervised after-school activities. There is evidence that welfare reform increased private health insurance among women (DeLeire, Levine & Levy 2006; Kaestner & Kaushal 2003), and Le (2013) found that participation in extracurricular activities during high school is negatively associated with alcohol and marijuana use at age 18. Akee, Copeland, Keeler, Angold & Costello (2010) found that an

exogenous increase in income (distribution of profits from a new casino to American Indians, regardless of household characteristics) reduced parental stress, resulted in better relationships between parents and their teenage children, and reduced drug dealing and minor crime on the part of the teenage children. As such, increases in family income that may result from maternal employment under welfare reform would be expected to lead to reductions in youth drug use.

However, work could lead to a net increase in constraints (e.g., by decreasing time available for investments in one's children in the realms of supervision, provision of high-quality health inputs, or nurturing), which could lead to worse teen behavior. Aizer (2004) and Averett, Argys and Rees (2011) found evidence that parental supervision reduces teen drug use, and Heiland, Price and Wilson (forthcoming), found that mothers who work full time spend less time (and less quality time) with their children compared to mothers who do not work. Kalenkoski, Ribar and Stratton (2011) found that teens, especially boys, in single-parent households spend significantly more time in unsupervised activities compared to teens with a working father and a homemaker mother, and Han, Miller and Waldfogel (2010) found, in families with low levels of father presence, that the mother working a night shift increases the likelihood of youth substance use. Together, these studies suggest that work requirements under welfare reform may have increased teen drug use through reduced supervision, if there were not offsetting income effects.

Aughinbaugh and Gittleman (2004) directly investigated the extent to which maternal work in the past three years affected teen substance use (alcohol, cigarettes, marijuana, and other drugs), and found no significant effects. The authors suggested that these null results were due to imprecise estimation or potentially offsetting supervision and role modeling effects.

A second pathway by which welfare reform could theoretically affect teens' drug use is household composition. Antecol and Bedard (2007) found that paternal presence decreases the likelihood of early substance use, with similar results for boys and girls, and See (2016) found that more time spent with fathers may reduce future marijuana use. It thus appears that having two parents in the household is protective against teen substance use. Marriage promotion was an explicit goal of the PRWORA legislation, but a large literature on the effects of welfare reform on marriage and a smaller one on cohabitation reveal mixed findings (e.g., Blank 2007; Gennetian & Knox 2003; Grogger & Karoly 2005).

Despite the observed links between father presence and teen substance use, the lack of strong associations between welfare reform and both marriage and cohabitation suggest that family structure vis-à-vis two parent families is not an important pathway by which welfare reform would change teen behavior, despite the fact that youth crime is higher in female-headed households (Glaser & Sacerdote 1999; Cobb-Clark & Tekin 2014; Cormanor & Phillips 2002; Antecol & Bedard 2007). However, "doubling up," an arrangement that has been much less explored in the context of welfare reform despite high rates of reliance on family and friends among unmarried mothers during the post-welfare reform era (Teitler, Reichman & Nepomnyaschy 2004), is a potential pathway. Such arrangements may involve a child living with his/her parent(s) and a grandparent or other adult relative or non-relative. Doubling up can generate economies of scale through joint household production, pooled

resources, and specialization between market and household production (e.g., in-home child supervision), but it can increase constraints if the arrangement does not increase household efficiencies (e.g., if a relative who moves in is more of a burden than a help). Deleire and Kalil (2002) found that teenagers living with single mothers plus at least one grandparent generally exhibit behaviors (e.g., refraining smoking, drinking, and initiating sexual activity) that are at least as good, and often better, than those of teenagers who live with both married parents. These findings suggest that doubling up would be a relevant pathway, if welfare reform affects doubling up.

Another pathway by which welfare reform may operate is by changing youths' expectations about welfare as a long-term option. For forward-thinking low-income teenagers, knowing that time limits preclude long-term welfare reliance might lead them to invest more in their ability to be self-sufficient as adults. Since having a criminal background is a deterrent to getting a job in the legal sector, it is plausible that welfare reform would lead to decreases in illicit drug involvement, particularly among girls.

A final pathway by which welfare reform is hypothesized to affect teen drug use is through changes in maternal behaviors, which can operate through work or more generally through the new normative climate. If welfare reform leads mothers to teach/model a lower rate of time preference or distaste for delinquent behavior, their teens may engage in less delinquent behavior. Previous research found that welfare reform led to reductions in illicit drug use, binge drinking, and property crime among adult women who were at risk for reliance on welfare (Corman, Dave, Das & Reichman 2013; Corman, Dave & Reichman 2014; Kaestner & Tarlov 2006), and youth with parents who engage in drug and alcohol use and criminal activity are more likely to engage in those behaviors themselves (e.g., Case & Katz 1991). Mendolia and Walker (2014) found that youth with a strong work ethic were less likely to have used marijuana or hard drugs by the time they were 20. If welfare reform has increased the work ethic of teens, through maternal work and/or behaviors or by making welfare less of an option for the future, then it may have led to reductions in drug use through that channel.

Data, Measures, and Trends

The two main sources of data for this study are: (1) Uniform Crime Reporting Program arrest data from the Monthly Master Files from the U.S. Department of Justice Federal Bureau of Investigation (FBI) for 1990 through 2005, which provide the number of arrests by age and gender for each month/year/offense category/reporting agency; and (2) implementation dates of welfare reform at the state level during the same time period. The former is used to create measures of arrests and the latter is used to characterize welfare reform, as described below.

Measures of drug arrests

Drug arrests are widely-used and validated measures of drug-related crime (e.g. Rosenfeld & Decker 1999; Warner & Coomer 2003) and have been used as proxies for illicit drug use in many studies (e.g., Corman et al. 2013; Chu 2015; Jacobson 2004). In this study, we use data

on drug arrests from the FBI's Uniform Crime Reports. Drug abuse violations constitute one of the 29 offenses categorized by the FBI.

The FBI data include a record for each criminal justice agency in the U.S. for each month of each year. Each agency's monthly record includes the number of arrests by arrest category, age category, and sex. To obtain reasonably representative information, we limited our sample to agencies that cover at least 50,000 individuals. In 1996, the year that PRWORA was enacted, agencies with populations of 50,000 or more people covered approximately 55% of the total U.S. population (147 million/268 million, calculated by the authors from the FBI and U.S. Census data). From these agency-based observations, we aggregated the data to the state level. Even among large criminal justice agencies, not all agencies report in all months. For example, in 1996, of the total 147 million people in the U.S. residing under the jurisdiction of agencies of 50,000 people or more, about 106 million people (about 72%) were covered by agencies that reported arrests to the FBI in all 12 months.¹ In our models, we control for both the total population in all agencies covering policy measures and a vector of populations of at least 50,000 in the given state/ month/year and the total state population (measured annually), as well as the coverage rate for reporting agencies. These population measures account for differences in the size of the underlying population that is represented or covered by the FBI reports. We further include the population of the relevant age/gender group being studied (for instance, males ages 15–17; females ages 15–17) in the given state and year.

Advantages of the FBI arrest data include measures that do not suffer from self-reporting biases and that likely reflect serious drug involvement as opposed to casual experimentation. Limitations include measures that do not capture illicit drug use unless it results in arrest and sparse information on the characteristics of teen arrestees and their families.

Characterizing welfare reform

Welfare reform was implemented in two general phases. The first phase consisted of pre-PRWORA waivers. Although not federally mandated, pre-PRWORA waivers were implemented in the majority of states by the time the federal PRWORA legislation was enacted in 1996 (Schoeni & Blank 2000). The second phase of welfare reform came with the enactment of PRWORA. States were required to submit plans for and—once approved, implement—TANF programs subject to federal guidelines and have been required to submit changes to their programs to the U.S. Department of Health and Human Services. States implemented their approved TANF programs between September 1996 (Massachusetts, Michigan, and Vermont) and January 1998 (California) (USDHHS 1999). Because PRWORA imposed a lifetime limit of 5 years' worth of benefits for most mothers in most states, lifetime benefits could not have been exhausted for many recipients until 2005.

Table 1 presents the implementation dates for both AFDC waivers and TANF for all states in the U.S. The waivers were introduced in 29 states over a period of 53 months, and TANF

¹We also dropped state/month observations for which fewer than 50% of the state's population (residing in agencies of 50,000 or more people) was represented. This resulted in a loss of about 450 observations, or about 7% of all relevant state/month cells. This results in arrest counts and rates that exceed overall arrest rates, since we over-represent urban areas.

was implemented in all states over a period of 17 months. Considering both waivers and TANF, states implemented welfare reform over a period of 64 months, spanning from October 1992 (MI and NJ being the earliest states to implement waivers) through January 1998 (CA being the last state to implement TANF).

Following the convention in the welfare reform literature (reviewed in Blank 2002), we exploit differences in the timing of both AFDC waivers and TANF implementation across states. In some models, we use separate measures for AFDC waiver and TANF implementation. For waivers, we consider whether, in a given month in a given year, a state had a statewide AFDC waiver in place that substantially altered the nature of AFDC with regard to time limits, work requirements, earnings disregards, sanctions, and/or family caps. For TANF, we consider whether the state had implemented TANF post-PRWORA in a given month in a given year. Most studies consider AFDC waivers and TANF separately, since they represent distinctly different phases of welfare reform. In other specifications, we include a single indicator for any welfare reform (AFDC waiver or TANF). In order to account for time lags between behaviors and arrests as well as to allow for time for the new policies to take effect, our welfare reform variables build in a 6-month lag. We also explicitly explore the timing of effects relative to the timing of welfare reform implementation in alternate models described later.

Trends in drug arrests

Figure 1 shows drug arrest rates by age and gender for the period 1990–2005, the period during which welfare reform took effect, in the United States. It is clear that males constitute the majority of those arrested for drug-related crimes in all 5 cohorts examined, from the early teens to the late 20s. In addition, males and females exhibit different patterns by age. For males, drug arrests peak before age 20, drop off by about 20% between the late teen years and early 20s, and then drop off another 30% into the late 20s. In contrast, female drug arrests do not peak until the early 20s and then drop off less steeply (by only 6%) into the late 20s. However, for both males and females, drug arrests are quite low until the middle teenage years. There are very few drug arrests before age 15 years; we thus focus on the 15–17 age range (minor children for who drug arrests are non-trivial) in this paper.

Figure 2 shows the trends in drug arrest rates over the same period for males aged 15–17 and 25–29 years, and Figure 3 shows corresponding trend data for females. It appears that drug arrests for teen males increased relative to that for adult males in the period during which welfare reform was implemented (1992–1997) and then declined more rapidly after 1998. A similar pattern is evident for females.

Figures 4 and 5, respectively, show corresponding trends in male and female arrest rates by the number of years since welfare reform was implemented in the state of the arrest. For these figures, we aggregated to the year level to smooth the month-to-month variations. Figure 4 presents a less clear picture than Figure 2 when comparing teen to adult males. From Figure 4, it appears that drug arrest rates increased more for male teens than for adult males in the period leading up to welfare reform, but that both began to decrease about two years post reform. From Figure 5, it appears that drug arrest rates for teen females declined post-welfare reform, but at a slower pace than the decline for adult women shown in Figure

5 and reported in Corman et al. (2013). However, these trend and event study graphs do not control for other potentially confounding changes.

Methods

We employ a quasi-experimental DD research design, which exploits variation in the timing of the implementation of welfare reform across states and over time, to estimate the following baseline model which relates changes in the arrest rate for drug-related offenses to welfare reform:

$$\ln A_{stm} = \alpha + \text{Welfare}_{stm} - \delta P + Z_{st}\beta + \text{States}\Omega + \text{Year}_t\Psi + \text{Month}_t\Theta + (\text{States} \times \text{Years}_{\text{pre}})\phi + \varepsilon_{stm}$$

(1)

This is a quasi-reduced form equation, which directly relates the natural log of arrests for individuals aged 15 to 17 to welfare policy measures and a vector of covariates.²

In Eq. (1), youth arrests ($\ln A$) related to illicit drugs, measured in state s during month m and year t , are a function of welfare reform implementation (*Welfare*), characterized separately by the 6-month lagged implementation of AFDC waivers and TANF in the given state, or alternatively, as the lagged implementation any welfare reform (AFDC waiver or TANF). Arrests are also a function of a vector of time-varying state-specific factors (Z), including measures of the state's economy and labor market conditions, relevant measures of the population base, and enforcement. The parameter represents a state-time error term. In all specifications, we include state fixed effects (*State*), which account for all unobserved time-invariant state specific factors, year (*Year*) fixed effects, which account for national trends in criminal activity and the arrest rates as well as other concurrent national trends, and month fixed effects (*Month*), which account for seasonality. The parameters of interest are the vector Π , which represents the “reduced form” or total effect of welfare reform on crime, operating through a variety of potential (and possibly competing) mechanisms discussed earlier. We estimate all models separately for males and females owing to substantial differences in the prevalence of drug use and drug-related crime, as well as potential heterogeneity in underlying mechanisms by gender.

Ideally, we would estimate models of the effects of welfare reform on drug arrests using data on the population of youth most likely to reside in welfare-reliant households— those with low-educated unmarried mothers. However, the FBI arrest data do not allow us to ascertain educational attainment, marital status, or other characteristics of arrestees' mothers; the only available demographics are sex and age of the perpetrator. This is less of a limitation than it may seem on the surface, because: (1) We would not want to restrict the sample to youth

²The log transformation adjusts for the skewness of the distribution of state-level youth arrests, and also facilitates comparison of the estimates across models and outcomes in terms of relative percent changes in arrests. We discuss the magnitudes of our effects in greater detail in the text. All models control for various measures of the relevant population base, thus allowing the coefficient of population to be unrestricted.

whose mothers are current welfare recipients, since potential welfare recipients are shown to behave strategically in their use of welfare benefits when faced with time limits and other regulatory constraints (DeLeire et al. 2006; Grogger 2004); (2) Our sample consists exclusively of youth who have been arrested, and youth arrestees typically come from disadvantaged families with single mothers (e.g. Harper & McLanahan 2004), a group at high risk of former, current, and future welfare participation. Data from the National Household Survey of Drug Abuse show that in 1992 (the onset of welfare reform), 14% of teens who had been arrested came from households currently receiving welfare, while by 2000 only about 4% of ever-arrested youth came from households who were currently receiving welfare (authors' calculations). This trend is consistent with the observed reduction in welfare caseloads during the period. A sizable share of the potential changes in drug-related arrests attributable to welfare reform should be from the 10% of the arrestee population that reduced their welfare participation. However, this 10% figure does not take into consideration households with previous recipients who may have transitioned into employment because of welfare reform or with potential recipients who may have contemplated going on welfare in the future. Nevertheless, only a subset of the arrestee population would be affected by welfare reform, which would limit statistical power in our analyses. We interpret the plausibility of our estimates and effect magnitudes and make inferences accordingly.

Identification in the DD framework comes from comparing changes in youth arrests within 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. Given the general noisiness of the arrest data and the multitude of state factors that may affect drug-related arrests, the “control” states which have not yet instituted welfare reform may not completely account for omitted variables. For instance, one particularly salient policy confounder is the Violent Crime Control and Law Enforcement Act of 1994, which was advertised as the largest crime bill in the history of the United States (U.S. Justice Department Fact Sheet 1994) and which provided for increased federal funding to states to fight crime.³ We address the specter of potentially confounding effects from unobserved factors, including those from the 1994 crime bill, in a number of ways.

First, we parametrically include state-specific linear trends pre-welfare reform ($State_s * YearsPre_{st}$) in our main analyses, thus relaxing the parallel trends assumption and allowing for differential trends in youth drug-related arrests across states prior to the implementation of welfare reform. We also assess the sensitivity of the results to adjusting for stronger non-parametric controls for differential pre-policy trends. Finally, we further consider potential time lags between the implementation of welfare reform and behavioral responses that result in arrests. Lagged policy effects are quite plausible; for example, it could take some time for youth to understand the implications of the new regime and it may take a number of crimes over months for an individual to be caught and arrested. We assess and test for these possibilities by estimating models that decompose the timing of the effects.

³See: <https://www.ncjrs.gov/txtfiles/bills.txt>.

Second, we explicitly control for a large vector of concurrent state-specific time-varying factors (Z) that may impact youth arrest rates, including measures of the state's economy (real personal income per capita, poverty rate), labor market conditions (unemployment rate, minimum wage), and the relevant population base (total state population; covered population of reporting FBI agencies). The 1994 crime bill, mentioned above, provided increased funding for police and authorized substantial funding for prison construction.⁴ In order to control for these potentially confounding effects, all specifications further include measures of the state's criminal justice system, including total criminal justice-related expenditures and the total number of full-time police officers.

Third, we address potential policy endogeneity (i.e., state experimentation with welfare reform through waivers and the timing of TANF implementation may be related to prior increases in welfare caseloads and prior economic conditions, which may be related to crime rates) by controlling for lagged state-level economic indicators (state-level unemployment rate and personal income per capita) and lags of the state's welfare caseloads.⁵ Specifications with the state-specific pre-welfare reform time trends also help to address the possibility that policy implementation may be otherwise endogenous to the state's history and thus mitigate this concern.⁶

Fourth, in most specifications, we control for the natural log of drug related arrests among young adult males (ages 25–29). Males in this age group should be affected minimally, if at all, by shifts in welfare policy, for two reasons: First, male adults are not eligible for welfare and thus would not be affected directly by shifts in policy. Second, the majority of males in this age group were adults when states were implementing welfare reform, and therefore we do not expect lagged effects through changes in maternal employment or other factors for this group. This control may capture confounding effects from the 1994 crime bill since most of the provisions of the legislation should have had similar impacts on teens and adults. It may also capture overall shifts in drug-related arrests in a given state due to further time-varying state characteristics such as enforcement and penalties, local economic conditions, and other unobserved factors.⁷ This strategy follows Dave et al. (2011), which employed controls for trends in male insurance coverage when estimating insurance rates among pregnant women.

⁴Travis, Western and Redburn (2014) found, however, that actual funding for prisons was considerably less than the funds initially allocated in the bill. In addition, these authors found that the rate of growth in imprisonment rates had been high since the 1970s and did not seem to increase following the passage of the 1994 crime bill.

⁵The lagged economic indicators are in addition to the contemporaneous measure of the state's economy contained in the vector Z . We do not control for contemporaneous welfare caseloads as these would comprise an endogenous control being a function of welfare reform. The lagged measures of welfare caseloads can help account for the possibility that state-specific timing of welfare reform implementation may be driven by the state's previous levels and trends in caseloads. However, given the high degree of correlation in a state's welfare population over time, the estimates from these models are suggestive and only intended to gauge the sensitivity of our estimates to additional selection on observed state-specific factors.

⁶We also explored the extent to which the implementation of welfare reform was associated with the state's pre-reform history by estimating models directly predicting whether and when a state implemented an AFDC waiver, and alternately, any welfare reform, as a function of either the lagged level of youth crime (total arrests) or prior annual trends in youth crime in the state, as well as estimating lead effects in variants of our main analyses. These specifications did not suggest any significant or consistent effects, which is validating.

⁷Note that there is no restriction in this case that the unobserved factors affect both adult male and youth crime identically. This is the assumption that is typically made in a DD framework if males were used as a direct control group, but is not supported by the time trends (Figures 1 and 2) and (later) explicitly rejected in all of our models. Instead, by controlling for male arrest rates, we are able to flexibly control for all common unmeasured factors that may be affecting adult male and youth crime (even if differentially) as long as the effect is proportional.

Finally, we conduct a series of supplemental analyses that explore potential heterogeneous effects by estimating effects separately for states with relatively strong and relatively medium or weak work incentives built into their TANF programs, for states that had relatively large and smaller declines in their welfare caseloads, and for states that experienced relatively large and smaller increases in the employment rates of low-educated unmarried mothers, a group at high risk for relying on welfare. If there were effects of welfare reform on drug-related arrests, these should be most apparent in states with stronger work incentives, that had larger declines in welfare caseloads, and that had larger increases in employment of low-educated unmarried mothers.

We estimate all models using Ordinary Least Squares (OLS) and adjust standard errors on the conservative side to account for arbitrary correlation within state cells over time. The inclusion of state-specific pre-reform linear trends in our models confines the variation we exploit to yield plausibly causal estimates. While these strategies reduce statistical power, we draw inferences from the weight of the evidence generated by our various specifications, patterns across estimates, and multiple robustness and consistency checks.

OLS estimates of state-month/year panels yield treatment effects that ignore population size, and the resulting estimates are averages over states rather than over individuals. We therefore weight all models by the state's population of females ages 21–49 (the potential pool who would be exposed to welfare reform), which would result in an individual-weighted average causal effect and may help to increase the precision of the regression estimates (Angrist & Pischke 2009). Our estimates are not sensitive to weighting.

Results

Tables 2 and 3 present estimates for Equation (1), for males and females, respectively. We discuss the results for males (Table 2) in detail; the estimates for females (Table 3) generally follow a similar pattern. Coefficients from Model 1 (Table 2) suggest that welfare reform is associated with an 11% increase in drug-related arrests among males ages 15–17, though the estimate is not statistically significant at conventional levels (p -value = 0.103). The next two specifications separate welfare reform into AFDC waivers and TANF; Model 2 controls for a basic set of covariates whereas Model 3 adds lags for the state's economic conditions and welfare caseloads to adjust for the potential endogeneity of welfare reform implementation. The coefficients for TANF are significant (suggesting an increase in drug arrests of 10–11%) in contrast to those for the waivers (point estimates suggesting a similar increase in drug arrests of 10–11%). The confidence intervals for both coefficients overlap substantially and we cannot reject the null that both phases of welfare reform had similar effects on teen drug arrests. These models control for state, month, and year indicators, as well as state-specific pre-policy linear trends. The findings are directionally in contrast to the unadjusted trends in Figures 2 and 4, underscoring the need to account for confounding trends.

As noted earlier, only a subset of the arrestee population is potentially impacted by welfare policy. Thus, these effect magnitudes, suggesting a 10–11% increase in drug arrests, are implausibly large since the reduced-form DD effect is an average effect realized over those who are being affected and those who are not being affected. This suggests that Models 1–3

are not fully accounting for underlying trends, and that the estimates from these models likely reflect unobserved state factors.

As discussed earlier, drug arrests are affected by a myriad of factors that may have changed during the implementation of welfare reform. For this reason, Models 4 through 10, which control for drug arrests among young adult males ages 25–29, represent our preferred specifications. Also as discussed earlier, males ages 25–29 are young enough for their drug-related behaviors to be affected by factors similar to those affecting drug-related behaviors of teens, but old enough to have been unaffected by welfare reform.⁸ In fact, the elasticity of drug arrests for male teens with respect to the drug arrests for the young adult males is highly significant in all specifications (at about 0.66; not shown), suggesting common trends in drug arrests for both groups that are presumably driven by common underlying factors.

The addition of the control for the young adults' drug arrests results in a large magnitude reduction in the estimated effect of welfare reform on teen drug arrests. The effect size now varies between 2 and 5% and is estimated with much imprecision (t-values of coefficients are all generally less than 1). While the results from Models 1 through 3 suggest that welfare reform increased male teen drug arrests by about 10–11%, those from Models 4 through 6 make these results less convincing. Overall, it appears that welfare reform may be associated with a small, though insignificant, positive effect on drug-related arrests. However, we cannot rule out the possibility that welfare reform did not affect teen drug-related arrests, as earlier models (Models 1–3) pointed to implausibly large effects and substantial confounding from unobserved state factors. Thus, the possibility of additional unobserved selection remains.

Models 7 and 8, respectively, add stronger controls for pre-policy differential trends and decompose the timing of the policy effects in the event that the small-to-nil effects may be masking some temporal heterogeneity. While the state-specific linear pre-policy trends in Models 1–6 adjust for parametric deviations from the parallel trends assumption underlying the DD analysis, in Model 7 we instead include indicators for the number of years prior to welfare reform implementation in the state. This latter set of controls essentially captures lead policy effects and adjusts for unobserved (linear or non-linear) differential trends in outcomes across states. The indicators for years prior to policy implementation are jointly significant, suggesting that it is important to control for these differential pre-policy trends. The point estimate for welfare reform is similar to those reported for Models 4–6 and remains statistically insignificant.

The main effects in Models 4–7 suggest at most a 3–4% increase in drug-related arrests, if there is an effect. Taken at face value, these are average effects realized for the average state over an average post-reform period of about 8–9 years. The final model, Model 8, represents an event study that decomposes the timing of this average effect and explicitly considers the number of years since welfare reform was implemented in order to explore lagged and cumulative responses. We include indicators for any welfare reform implementation in the current year as well as for any welfare reform implementation 1–2, 3–4, and 5 or more years

⁸Estimates are similar if we broaden the age group to those ages 25–49.

in the past—all interpreted relative to the year just prior to welfare reform. The results indicate almost a 3.8% increase in drug arrests during the implementation year, which should be interpreted with caution given that strong contemporaneous effects would not allow for lags between welfare reform implementation, maternal employment, teen drug activity, and potential arrests. The coefficients for the other indicators suggest slightly larger effects 1–2 years post-policy implementation (on the order of 5.2%) and slightly smaller effects 3–4 and 5+ years post-welfare reform (around 4% increases). However, we cannot reject the null hypothesis of equal effect sizes across these time intervals (implementation year, 1–2 years, 3–4 years, and 5+ years post-welfare reform).

Table 3 presents corresponding results for female teens ages 15–17. As these estimates generally follow the same patterns as those discussed above for males, we summarize the key findings. First, the effect magnitudes are slightly smaller for females than for males, suggesting an increase in drug-related arrests on the order of 7–9% in Models 1 through 3. However, with the addition of further controls for differential trends and confounding factors in Models 4–8, our preferred models, effect sizes are even smaller and more imprecisely estimated than those from the corresponding models for males in Table 2. Second, while these estimates are generally robust to all of the controls for pre-policy trends and other relevant factors, they are more imprecise than for males given the lower prevalence of drug-related arrests for females and subsequently inflated standard errors. Note that Models 4–8 symmetrically control for male drug-related arrests (ages 25–29), as we did for the models for male teens in Table 2. We do not control for young adult female drug arrests in the 25–29 age bracket because previous research found that adult women's drug use was affected by welfare reform (Corman et al. 2013). Although young adult drug arrest rates of males are not as good a control for models of female teen drug arrests than for those of male teen drug arrests, we do find that the coefficient on young adult male arrests is highly significant and has an elasticity of 0.50 compared to that of 0.66 for male teens (not shown in tables). Thus, controlling for the drug-related arrests of young adults males does capture a non-negligible portion of overlapping trends in these models of female teen arrests, and hence the unobserved factors driving these trends. Fourth, the timing of the estimated effects mirrors those for male teens, where coefficients are measured with imprecision, and we cannot reject the null hypothesis of no effect at any point in time.

In Table 4, we explore potential heterogeneous treatment effects by the extent to which states encouraged work under their TANF programs, the extent to which states experienced welfare caseload declines, and the extent to which states experienced increases in employment rates of mothers at risk for reliance on welfare. If there were effects of welfare reform on drug-related arrests, they should be most apparent in states that had stronger work incentives, that experienced larger declines in welfare caseloads, and that had larger increases in employment of low-educated unmarried mothers. For ease of comparison, we focus on models with the main indicator for any welfare reform. The results correspond to Model 4 in Tables 2 and 3 (the model that generally suggested the weakest effects).

Work-incentive policies under TANF (such as benefit generosity, time limits, earnings disregards, and sanctions for non-compliance) vary considerably across states. The first row in Table 4 explores potential dose-response associations based on the strength of states' work

incentives under TANF by stratifying the sample into states that had relatively strong work incentives (Model 1 in Table 4 for is for males and Model 2 is for females) and those that had relatively weak work incentives (Models 3 and 4, for males and females, respectively). For males, the effects of welfare reform on drug-related arrests are not statistically significant for either the states with strict work incentives or those with weaker work incentives, and the effect magnitude is greater for the states with weaker work incentives. For females, welfare reform is negatively associated with drug-related arrests among states with strong work incentives. Overall, we do not find evidence of stronger positive effects of welfare reform in states with strict work incentives than in states with weaker work incentives, for either males or females.

The second row in Table 4 explores potential dose-response associations based on states' welfare caseload declines by stratifying states into those that had caseload declines greater or equal to the median (60%) and those that had declines less than the median, and the third row explores potential dose-response associations by stratifying states into those that had increases in employment rates of low-educated unmarried mothers that were greater than or equal to the median (11.6 percentage points) and those that had increases less than the median. These results generally suggest that, for both male and female teens, the effects of welfare reform—if there are any—appear to be larger in states that had larger caseload declines and employment increases.

In supplementary specifications (not shown), we utilized the state's welfare caseload (total number of welfare recipients in the state) as an alternate measure of welfare reform. Kaestner and Kaushal (2003) argued that the caseload changes do a better job (than indicators of welfare reform implementation) of capturing the effects of welfare reform in a given state after controlling for the state's economic conditions. That is, after accounting for the state's economy and other factors, a larger decline in caseload would reflect a stronger and more binding impact of welfare reform for that state that includes both stricter work incentives and more stringent enforcement of the policy components. The results when using this alternative measure of welfare reform, which suggest a small and insignificant increase in drug-related teen arrests of about 1% (for both males and females) associated with the observed decline in caseloads over our sample period, were not substantively different from those when using our main indicators of welfare reform.

Finally, in models not shown we confirmed that our main results are not sensitive to alternate functional forms or model specifications that, alternatively (1) changed the outcome to a logistic transformation based on the natural log of the odds of drug arrest [$\ln((A_{st}/\text{Population}_{st})/(1-(A_{st}/\text{Population}_{st})))$], and (2) used non-logged measures of the drug arrest rate as outcomes. We further assessed sensitivity to how we treated agencies that were small or had limited coverage. Given that not all criminal justice agencies provide complete reports on the number of arrests by month and offense type, our main analyses were based on agencies that covered at least 50,000 in order to minimize measurement error and maximize data consistency. We confirmed that our estimates are not sensitive to restricting our sample to individuals and agencies with a reported coverage of at least 50% or to that specific cut-off. We also determined that are results are not sensitive to aggregating the FBI arrest data to the annual level.

Conclusion

This study investigated the effects of welfare reform in the United States on teen drug arrests. More specifically, this study exploited differences in the implementation of welfare reform across states and over time in the attempt to identify causal effects of welfare reform on youth arrests for drug-related crimes between 1990 and 2005.

We considered differential effects by gender, short-run effects for teens exposed to welfare reform as well as longer-term effects for young adults who came of age when welfare reform was implemented, and the extent to which effects appear to be stronger in states with more stringent work incentive policies, larger welfare caseload declines, and larger employment increases among low-educated unmarried mothers. The findings, based on numerous different model specifications, suggest that welfare reform had no statistically significant effect on teen drug arrests, although most estimates were positive and suggestive of a small increase in arrests. Future research should replicate this study with other data and more fully explore the underlying processes.

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Figure 1. Drug Arrest Rates by Sex, Age 1990–2005

Notes: Arrest rates are computed as follows: The numerator is the sum of all drug-related arrests in the age/gender category in reporting agencies of 50,000 or greater. The denominator was derived from state-level data. We multiplied the state/age/gender population by the percent of the population covered by reporting agencies of 50,000 or greater. We took the sum of the state populations to get the total denominator. The fraction of arrests divided by population was then multiplied by 100,000. Annual population data by state, single year of age, and sex were obtained from the US Census Bureau: 1990s: https://www.census.gov/popest/data/state/asrh/1990s/st_age_sex.html 2000s: <https://www.census.gov/popest/data/intercensal/state/files/ST-EST00INT-AGESEX.csv>

Figure 2. Male Drug Arrest Rates by Year

Note: Sources of data—see Figure 1.

Figure 3. Female Drug Arrest Rates by Year

Note: Sources of data—see Figure 1.

Figure 4. Male Arrest Rates by Years Since Welfare Reform

Note: Sources of data—see Figure 1. Years before/after welfare reform calculated based on welfare dates in Table 1.

Figure 5. Female Arrest Rates by Years Since Welfare Reform

Note: Sources of data—see Figure 4.

Table 1

Implementation Dates of Welfare Reform by State, US

	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	Feb-96	Feb-97	Feb-96
Alaska		Jul-97	Jul-97	Oct-95	Dec-96	Oct-95
Arizona	Nov-95	Oct-96	Nov-95		Dec-96	Dec-96
Arkansas	Jul-94	Jul-97	Jul-94		Oct-96	Oct-96
California	Dec-92	Jan-98	Dec-92	Oct-92	Jul-97	Oct-92
Colorado		Jul-97	Jul-97		Jul-97	Jul-97
Connecticut	Jan-96	Oct-96	Jan-96		Nov-97	Nov-97
DC		Mar-97	Mar-97	Jul-96	Jan-97	Jul-96
Delaware	Oct-95	Mar-97	Oct-95		Jul-97	Jul-97
Florida			Oct-96	Jul-96	Oct-96	Jul-96
Georgia	Jan-94	Jan-97	Jan-94		Oct-96	Oct-96
Hawaii	Feb-97	Jul-97	Feb-97	Feb-93	Oct-96	Feb-93
Idaho		Jul-97	Jul-97		Mar-97	Mar-97
Illinois	Nov-93	Jul-97	Nov-93		May-97	May-97
Indiana	May-95	Oct-96	May-95		Oct-96	Oct-96
Iowa	Oct-93	Jan-97	Oct-93	Jun-94	Dec-96	Jun-94
Kansas		Oct-96	Oct-96	Sep-96	Oct-96	Sep-96
Kentucky		Oct-96	Oct-96	Jun-96	Nov-96	Jun-96
Louisiana		Jan-97	Jan-97	Jan-93	Oct-96	Jan-93
Maine		Nov-96	Nov-96	Jul-94	Sep-96	Jul-94
Maryland	Mar-96	Dec-96	Mar-96	Jul-95	Feb-97	Jul-95
Massachusetts	Nov-95	Sep-96	Nov-95	Jan-96	Jan-97	Jan-96
Michigan	Oct-92	Sep-96	Oct-92		Jan-97	Jan-97
Minnesota		Jul-97	Jul-97	Jan-96	Sep-97	Jan-96
Mississippi	Oct-95	Jul-97	Oct-95		Jan-97	Jan-97
Missouri	Jun-95	Dec-96	Jun-95		Jan-97	Jan-97

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

Table 2
Effects of Welfare Reform on Drug-related Arrests Males, Ages 15–17

Outcome Model	Ln Drug-related Arrests							
	1	2	3	4	5	6	7	8
Any Welfare Reform	0.1060 (0.0638)			0.0360 (0.0467)			0.0314 (0.0539)	
AFDC Waiver		0.1045 (0.0696)	0.1130 (0.0707)		0.0385 (0.0502)	0.0470 (0.0469)		
TANF		0.1141*** (0.0416)	0.0976** (0.0462)		0.0222 (0.0365)	0.0232 (0.0371)		
Welfare Reform Policy Year (t=0)								0.0376 (0.0322)
1–2 Years Post Welfare Reform (t=1,2)								0.0518 (0.0695)
3–4 Years Post Welfare Reform (t=3,4)								0.0385 (0.1001)
5+ Years Post Welfare Reform (t=5)								0.0401 (0.1058)
State, Month, Year Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Pre-Policy Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Extended Covariates	No	No	Yes	No	No	Yes	No	No
Indicators for Years Prior to State Policy Implementation	No	No	No	No	No	No	Yes	Yes
Log Drug Arrests Males Ages 25–29	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	8841	8841	8841	8624	8624	8624	8624	8624

Notes: Coefficients from OLS models are reported. Standard errors are adjusted for arbitrary correlation within states over time and reported in parentheses. All models are weighted by the state's population of females ages 21–49. Basic covariates include the state's unemployment rate, per capita real personal income, log of total population, log of population of males ages 15–17, log of the agency population for the arrest reports, log state criminal justice expenditures, log full-time equivalent police officers, state minimum wage, state poverty rate, and the coverage rate for reporting agencies. All models further control for state, month, and year fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capita real personal income, and 1- and 2-year lags of the state's welfare caseload. Asterisks denote statistical significance as follows:

*** p-value 0.01;

** 0.01 < p-value 0.05;

* 0.05 < p-value 0.1.

Table 3
Effects of Welfare Reform on Drug-related Arrests Females, Ages 15–17

Outcome Model	Ln Drug-related Arrests							
	1	2	3	4	5	6	7	8
Any Welfare Reform	0.0668 (0.0470)			0.0165 (0.0346)			0.0323 (0.0359)	
AFDC Waiver		0.0622 (0.0495)	0.0695 (0.0485)		0.0150 (0.0353)	0.0220 (0.0310)		
TANF		0.0921* (0.0466)	0.0797 (0.0515)		0.0246 (0.0419)	0.0252 (0.0450)		
Welfare Reform Policy Year ($t=0$)								0.0329 (0.0385)
1–2 Years Post Welfare Reform ($t=1,2$)								0.0505 (0.0373)
3–4 Years Post Welfare Reform ($t=3,4$)								0.0557 (0.0728)
5+ Years Post Welfare Reform ($t=5$)								0.0189 (0.0973)
State, Month, Year Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Pre-Policy Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Extended Covariates	No	No	Yes	No	No	Yes	No	No
Indicators for Years Prior to State Policy Implementation	No	No	No	No	No	No	Yes	Yes
Log Drug Arrests Males Ages 25–29	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	8841	8841	8841	8624	8624	8624	8624	8624

Notes: Coefficients from OLS models are reported. Standard errors are adjusted for arbitrary correlation within states over time and reported in parentheses. All models are weighted by the state's population of females ages 21–49. Basic covariates include the state's unemployment rate, per capita real personal income, log of total population, log of population of females ages 15–17, log of the agency population for the arrest reports, log state criminal justice expenditures, log full-time equivalent police officers, state minimum wage, state poverty rate, and the coverage rate for reporting agencies. All models further control for state, month, and year fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capita real personal income, and 1- and 2-year lags of the state's welfare caseload. Asterisks denote statistical significance as follows:

*** p-value 0.01;
 ** 0.01

Table 4
Heterogeneous Effects of Welfare Reform on Drug-related Arrests Males & Females, Ages 15–17

	Ln Drug-related Arrests			
	Males 1	Females 2	Males 3	Females 4
I. Work Incentives	Strict Work Incentives		Medium/Weak Work Incentives	
Any Welfare Reform	0.0086 (0.0542)	-0.1456** (0.0624)	0.0381 (0.0381)	0.0494 (0.0354)
Observations	2364	2364	6260	6260
II. Welfare Caseload	Welfare Caseload Decline		Welfare Caseload Decline < Median	
Any Welfare Reform	0.1090** (0.0459)	0.0488 (0.0495)	-0.0263 (0.0354)	0.0050 (0.0369)
Observations	4403	4403	4221	4221
III. Employment	Employment Increase among Low-Educated Single Mjournals		Employment Increase among Low-Educated Single Mjournals < Median	
Any Welfare Reform	0.1655*** (0.0432)	0.0916* (0.0456)	-0.0037 (0.0349)	0.0275 (0.0270)
Observations	4386	4386	4238	4238

Notes: Coefficients from OLS models are reported. Standard errors are adjusted for arbitrary correlation within states over time and reported in parentheses. All models are weighted by the state's population of females ages 21–49. Specifications correspond to Model 4 in Tables 2 and 3. Typology of strict, medium, and weak work incentives is based on Blank and Schmidt. Between 1992 and 2001, median welfare caseload decline across states was 60%. Median increase in the employment rate for low-educated single mjournals was 11.6 percentage points. Asterisks denote statistical significance as follows:

p-value 0.01;

**
0.01 < p-value 0.05;

*
0.05 < p-value 0.1.