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Hope Corman
Dhaval Dave
Ariel Kalil
Nancy E. Reichman

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Effects of Maternal Work Incentives on Youth Crime
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ABSTRACT

This study exploits differences in the implementation of welfare reform across states and over time to identify causal effects of maternal work incentives, and by inference employment, on youth arrests between 1990 and 2005, the period during which welfare reform unfolded. We consider both serious and minor crimes as classified by the FBI, investigate the extent to which effects were stronger in states with more stringent work incentive policies and larger welfare caseload declines, and use a number of different model specifications to assess robustness and patterns. We find that welfare reform led to reduced youth arrests for minor crimes, by 7-9 %, with similar estimates for males and females, but that it did not affect youth arrests for serious crimes. The results from this study add to the scant literature on the effects of maternal employment on adolescent behavior by exploiting a large-scale social experiment that is still in effect to this day, and provide some support for the widely-embraced argument that welfare reform would discourage undesirable social behavior, not only of mothers, but also of the next generation.

Hope Corman
Department of Economics
Rider University
2083 Lawrenceville Road
Lawrenceville, NJ 08648
and NBER
corman@rider.edu

Dhaval Dave
Bentley University
Department of Economics
175 Forest Street, AAC 195
Waltham, MA 02452-4705
and NBER
ddave@bentley.edu

Ariel Kalil
Harris School of Public Policy
University of Chicago
1155 E. 60th Street
Chicago, IL 60637
a-kalil@uchicago.edu

Nancy E. Reichman
Rutgers University
Robert Wood Johnson Medical School
Department of Pediatrics
Child Health Institute of New Jersey
89 French St., Room 1348
New Brunswick, NJ 08903
reichmne@rutgers.edu

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) and the waivers that preceded it (collectively referred to as welfare reform), provide an unprecedented opportunity to study the effects of maternal work incentives on the behaviors of low-income teenage children. Despite successful expansions of the Earned Income Tax Credit (EITC) 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 for reducing dependence of this group was to aggressively encourage maternal employment (which would allow them to qualify for the EITC), by imposing work requirements and time limits as conditions for receipt of cash assistance. The basic argument was that labor force participation would break a culture of dependence 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). Employment rates of low-skilled mothers rose dramatically after the implementation of welfare reform, including the pre-PRWORA waivers (Ziliak 2006), and there is strong consensus that welfare reform played a major role (Schoeni & Blank 2000). Recent research found that welfare reform decreased women's substance abuse (Corman et al. 2013) and crime (Corman, Dave & Reichman 2014), providing support for the "mainstreaming" argument.

An implicit assumption underlying welfare reform was that the work-focused regime would abort an assumed transmission of welfare dependence to the next generation by putting mothers to work (which can increase family resources or lead mothers to model mainstream behavior), changing youths' expectations about welfare as a long term option, and requiring teenage mothers to stay in school (a feature of the legislation). That is, the new regime was expected to mainstream not only poor mothers, but also their 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), providing some support for this argument. However, few studies have considered how the new regime has affected teens more broadly. Crime and delinquency are of particular relevance, as studies have found links between female-headed households and youth crime (Glaser and Sacerdote 1999; Cobb-Clark & Tekin 2011; Cormanor & Phillips 2002; Antecol & Bedard 2007), teens and young adults have the highest propensity to commit crimes (e.g., Ulmer & Steffensmeier 2014), and criminal records and incarceration greatly hamper human capital acquisition and upward socioeconomic mobility (e.g., Western 2002).

In this paper, we exploit differences in the implementation of welfare reform across states and over time to identify causal effects of the "work first" regime on youth arrests between 1990 and 2005, the period during which welfare reform unfolded. We consider both serious and minor crimes as classified by the FBI, differential effects by gender, and the extent to which effects are stronger in states with more stringent work incentive policies and larger caseload declines, as economic theory would predict. This study makes an important contribution to the relatively small literature on the effects of maternal employment on teen well-being, as well as to the literature on the economics of youth crime, by exploring the role of broad-based work incentives in a policy-relevant and contemporary context.

Background

Empirical studies of effects of welfare or employment on teenage behavior

As far as we know, there exist relatively few population-based or quasi-experimental studies of the effects of welfare transitions, welfare reform, or employment on teen delinquent or criminal behavior. Syntheses based on three pre-PRWORA welfare reform experiments (“waivers”), some of which included features such as work requirements and time limits that later were included in the PRWORA legislation (see Data section) did not find consistent evidence that work incentives had a significant impact on adolescents having trouble with the police or being suspended or expelled from school (Gennetian et al. 2002, 2004). Grogger and Karoly (2005), reviewing a broader set of waiver programs, concluded that there may have been adverse effects of welfare reform on outcomes generally for youths age 10 years and over. However, when considering outcomes directly related to youth behavior—being suspended or expelled from school and being involved in delinquent or criminal behavior—the number of relevant studies was small and the results from those were quite variable, precluding inferences about existence or direction of effects.

Using data from the Three-Cities Study, several studies found that transitions into (off of) welfare appear to adversely (favorably) affect teens’ delinquent behaviors including substance use, while transitions into/out of work appear to operate in the opposite direction (Chase-Lansdale et al. 2003; Chase-Lansdale et al. 2011; Coley et al. 2007; Lohman et al. 2004). However, also using data from the Three-Cities Study, Mahatmya and Lohman (2011) found no associations between welfare transitions, employment transitions, or stable employment on teen delinquency.

Aughinbaugh and Gittleman (2004) estimated the effects of maternal work in the past 3 years on teen substance use (alcohol, cigarettes, and marijuana) using data from the NLSY-CS in family fixed effects models and found no significant effects, even for a subsample of unmarried mother households. Vander Ven et al. (2001), also using the NLSY-CS, found no associations between mothers’ hours of work and delinquent behavior (including illegal activities and alcohol abuse) of their teenage children, although the authors did not look at a subsample of low-income households.

Corman et al. (2017) exploited differences in the implementation of welfare reform in the U.S. across states and over time in the attempt to identify causal effects of welfare reform on youth arrests for drug-related crimes using arrest data from the Federal Bureau of Investigation merged with implementation dates of welfare reform in each state. The authors 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 appeared to be stronger in states with more stringent work incentive policies, larger welfare caseload declines, and larger employment increases among low-educated unmarried mothers. Overall, the authors found no evidence that welfare reform led to decreases in arrests for drug offenses (and, by inference, decreases in drug use) among youth and may actually have led to increases in such arrests. However, the authors cautioned that the latter finding was preliminary, not fully robust, and should be further explored.

Altogether, the most relevant existing studies have not provided convincing evidence about the effects (existence or direction) of welfare reform or employment on teen delinquent or criminal behavior. The waiver experiments were conducted in very specific contexts, did not always have sufficiently large samples of adolescents, and often did not measure adolescent behavioral outcomes. The studies using data from the Three-Cities study did not address selection into welfare or employment transitions, and it has been shown that women who have a difficult time securing and maintaining employment and staying off welfare have fewer human capital resources (e.g., education, physical health, and mental health) than those who are more successful at transitioning from welfare to employment (Danziger, Kalil & Anderson 2000). In addition, results from three cities may not be generalizable to the nation, and the research design of those studies does not allow for welfare reform to affect behaviors through channels other than changes in welfare participation and/or work. Aughinbaugh and Gittleman focused more on substance use than crime and acknowledged that their estimated effects of maternal employment were imprecise and cautioned that not too much should be read into their null results, and Vander Ven et al. did not address the potential endogeneity of maternal employment. Finally, the results from the Corman et al. study were sensitive to model specification and thus inconclusive.

Expected effects

Economic theory suggests that the work incentives under welfare reform may increase household income (if earnings from work after transportation and childcare expenses exceed previous welfare income), which could lead to increased resources, such as employer-sponsored private insurance, which could in turn increase access to mental healthcare and improve teens' behaviors. However, maternal employment 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 undesirable effects on teens' behaviors. Welfare reform could also have more broad-based effects through changes in the normative climate, by leading mothers to model mainstream behaviors and making reliance on welfare less of an option for the future for their children. Finally, welfare reform could potentially improve teen behaviors through one of the explicit goals of PRWORA—to encourage marriage though initiatives such as marriage promotion programs, which could result in increased household income and fewer time constraints. The net result will depend on the combined effects of the various pathways. Below, we highlight salient findings from the loose patchwork of relevant studies to get a sense of the plausibility of various pathways.

Income

Akee et al. (2010) investigated the effects of a positive income shock (the establishment of ongoing cash distributions to Native Americans from casino revenues in a certain area, which were not available to non-Native Americans in the same area) on the delinquent behavior of teens and young adults and found that increases in income were associated with lower levels of drug dealing and minor crimes. One way increased income, generally, may improve teen behavior is by allowing or inducing mothers to move to neighborhoods with fewer peers who may influence teens to engage in delinquency. Damm and Dustmann (2014) investigated effects of assignment of refugees to a variety of counties and municipalities in Denmark and found that youth assigned to areas with higher rates of youth violence were more likely to engage in violent crime. Kling et al. (2005) examined data from the Moving to Opportunity randomized experiment, in which some families received housing vouchers to move to lower poverty neighborhoods, and found reduced violence on the part of both boys and girls whose families received vouchers compared to those in the control group. Surprisingly, they found some positive effects of the treatment on property arrests for boys, but concluded that the positive social benefits of the declines in violence outweighed the negative social effects of the observed increase in property crime among boys.

Supervision

Heiland, Price and Wilson (2014), using data from the Panel Survey of Income Dynamics and American Time Use Survey (ATUS), found that mothers who work full-time spend less time (and less quality time) with their teenage children compared to mothers who do not work. Kalenkoski, Ribar and Stratton (2009), using data from the ATUS, found that teens aged 15–18 in single-parent households, particularly boys, spend more time in unsupervised activities compared to teens with working fathers and homemaker mothers. Han and Waldfogel (2007), using NLSY-CS data for children aged 10–14, found that night, rotating, and irregular maternal work hours were associated with an adult being present after school and that mothers with rotating and irregular hours were more likely to know who their children were with when they were not in school.

Aizer (2004) used data from the NLSY Child Supplement (CS) for youth aged 10–14 to estimate the effects of after-school supervision on three youth delinquent behaviors—getting drunk or high, hurting someone, and stealing. Estimating both single-equation and family fixed effects models, she found robust favorable effects of parental after-school supervision on all three behaviors. Vander Ven et al. (2001), discussed earlier, also used the NLSY CS for youth aged 12–14 and found that increased supervision (measured as whether the mother knows who the youth is with) is associated with decreased delinquency (on a scale constructed from 9 items). Averett, Argys and Rees (2011), using Add Health Data for youth aged 12–19, found some evidence that having an older sibling was associated with reduced parental supervision, that parental supervision before school was associated with less drug selling, and that parental supervision after school was associated with a reduction in marijuana use, other illegal drug use, and stealing. Mahatmya and Lohman (2001), referred to above, found that students involved in after-school activities exhibited less delinquent behavior. Akee et al. (2010), also referred to above, found that exogenous increases in income led both mothers and fathers to spend more time supervising their teen children and reduced the likelihood that the teen children engaged in drug dealing and minor crime. See (2016), using data from the PSID, found no associations between maternal supervision time and their teen children's smoking, alcohol use, and marijuana use; however, the authors used a sample of adolescents living with both biological parents, who are not at high risk for welfare reliance.

Overall, these studies provide evidence that full-time employment and standard work schedules are associated with less supervision of teenage children in single-parent households and that maternal supervision reduces at-risk teens' delinquent behavior.

Parental Modeling

Case and Katz (1991) found that not only do neighborhoods and peers matter, but that parents' behaviors also affect teen behaviors. Using data from youths in low-income Boston neighborhoods in 1989, the authors found that having a family member in jail or with substance use problems increases the likelihood that a teen commits a crime. Recent research indicates that welfare reform led to decreases in both crime (Corman, Dave & Reichman 2014) and substance abuse (Corman et al. 2013) among women at risk for welfare reliance, which in concert with the Case and Katz findings suggests that it may also have decreased criminal behavior of their children.

Welfare no longer a long-term option

For forward-thinking low-income teenage females, knowing that time limits preclude long-term welfare reliance might lead them to invest more in their ability to be self-sufficient as adults. In addition, welfare rules do not allow minor mothers to receive welfare if they move away from their family home and are not in school. As indicated earlier, welfare reform reduced high school dropout, and the effects were stronger for girls than for boys (Dave, Corman & Reichman 2012; Koball 2007; Offner 2005). Since having a criminal background is a deterrent to getting a job in the legal sector, and since schooling can be a substitute for illegal behavior, it is plausible that welfare reform would lead to decreases in crime, particularly among girls.

Family structure

Although marriage promotion was an explicit goal of the PRWORA legislation, a large literature on the effects of welfare reform on marriage and a smaller one on cohabitation reveal mixed findings, and the literature on non-marital childbearing and female headship indicates slightly negative but inconsistent effects of welfare reform (Blank 2002, Moffitt 1992, 1995, 1998, Grogger & Karoly 2005, Gennetian & Knox 2003, Peters, Plotnick & Jeong 2003, Ratcliffe et al. 2002). These inconsistent results 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, as mentioned earlier.

Gender Differences

A large literature on criminal behavior indicates that females are less likely than males to commit crime and that factors that affect criminal behavior generally have less of an effect for females than for males (e.g., Levitt & Lochner 2001). Some of the studies reviewed above reveal gender differences vis-à-vis supervision, income-related factors, and mother's employment, although most did not consider gender differences at all. Kalenkoski et al. (2009) found that boys with single mothers spent more time unsupervised than did girls with single mothers, while Cobb-Clark and Tekin (2011) found that the presence of fathers had a greater effect on reducing risky behaviors for boys than for girls. Damm and Dustmann (2014) found that being assigned to a relatively high violence area resulted in greater rates of violence for boys than girls, while Kling et al. (2005) found that girls responded to moves to lower poverty neighborhoods by reducing their violent behavior more than boys did, and that males who moved to higher-income neighborhoods were more likely to commit property crimes into their 20s while the same move had no effect for females. Finally, the time-limited nature of cash assistance under reform, making welfare much less of a long-term option, would likely affect the behavior of girls more than boys, as discussed earlier.

Recap

There is little past literature directly relevant to our focus on the effects of welfare reform on teenage crime, and existing studies do not provide convincing evidence in this regard. However, studies of effects of income and low-income neighborhoods on teen delinquent behavior, maternal work on supervision of teenage children, supervision on teen delinquent behavior, and parent modeling of mainstream behavior support hypothesized pathways. Finally, both the broader literature and the few studies directly tied to our research question suggest that it is important to investigate gender differences.

Data

The two main sources of data for this study are: (1) Uniform Crime Reporting Program arrests 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/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 Arrests

Virtually all studies of crime in the economics literature use measures of reported crime rates or actual arrests as proxies for crime. The age of the person committing the crime is not available for the former, so for our analyses of the effects of welfare reform on youth crime we rely on the latter. For purposes of this analysis, we focus on minors—individuals less than 18 years old. Specifically, we aggregate monthly data on arrests by age, sex, reporting agency, and type of offense from the FBI crime reports to construct month/year/state/age/sex/offense measures of arrests. Although individual level surveys that ask about crime commission include more detailed characteristics of perpetrators and may include crimes not reported to the police or that do not result in arrests, crimes are likely to be underreported, information is rarely available for different offense types, and criminal activity in large geographic areas over time cannot be covered in that mode. Comprehensive reviews have found that Uniform Crime Reports are valid indicators of serious crimes (Gove, Hughes & Geerken 1985) and that both aggregate and individual level studies of crime often lead to similar conclusions—e.g., that criminal justice sanctions deter crime (Nagin 1998).

The FBI data include a record for each criminal justice agency in the U.S. for each month. Each agency's monthly record includes the number of arrests by crime 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 and month/year 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] We include both the total population in all agencies covering populations of at least 50,000 in the given state/ month/year and the total state population on the right-hand side in our models. These population measures account for

[1] We 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 state/month observations, or about 7% of all relevant state/month cells.

differences in the size of the underlying population that is represented or covered by the FBI reports.

The FBI reports total arrests for 29 separate categories (see Appendix Table A-1 for a list of the crime categories and their codes), and also classifies crimes as “serious” (categories 1 through 19) or “other” (categories 20 through 29). We use this same classification, referring to “other” crimes as minor. Serious crimes are murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, other assault, arson, forgery and counterfeiting, fraud, embezzlement, stolen property crimes, vandalism, weapons offenses, prostitution, other sex offenses, drug abuse violations, and gambling. Minor crimes are offenses against family and children, driving under the influence, liquor law violations, drunkenness, disorderly conduct, vagrancy, suspicion, curfew violations and loitering, runaways, and all other crimes except traffic violations.

Figure 1 presents arrest rates by age and gender for serious crimes, excluding those for drug abuse violations, during 1990 and 1991—the two year period before any welfare reform was implemented. We exclude arrests for drug abuse violations because a recent study comprehensively explored the effects of welfare reform on drug-related arrests (Corman et al. 2017).[2] For each month/year, we imputed the total number of arrests in the U.S. as a whole by inflating the number of arrests in our data by the fraction of population covered in our data. We then calculated the mean of the annual arrest rates across the 2 years of pre-welfare reform data (see figure notes for additional detail). The cohorts of interest in our analysis are two groups of minors, those ages 10–14 and 15–17 years. For comparison, we present arrest rates for adult teens (18–19 years old), as well as for two cohorts in their 20s. The data in Figure 1 are consistent with national reports indicating that crime and arrest rates peak in the mid-to late-teen

[2] However, in supplementary analyses, we include drug crimes in the category of serious crimes.

years and are generally three to five times higher for males than females (Loeber, Farrington & Petechuck 2013; Snyder 2012). Figure 2 presents comparable rates for minor offenses, which reveal similar patterns.

Figure 3 shows trends in average arrest rates per 100,000 population (from our data) from 1990 to 2005 for our focal cohorts (males and females ages 10–14 and 15–17 years) and a comparison cohort (males 25–29 years) for serious crimes. The comparison cohort is young enough to have many active crime participants but far less likely than the focal cohorts to have been affected by welfare reform. During the 1990s, arrest rates were high through the middle of the decade and then declined, for all 5 cohorts. This trend is consistent with that described by Blumstein and Wallman (2006), Goldberger and Rosenfeld (2008), and Zimring (2007), who explored why crime rates dramatically, unexpectedly, and systematically plunged in all areas of the U.S. in the late 1990s. The bottom line from this body of work is that there is no one reason for the dramatic shift, but key factors appear to be the decline in crack cocaine use, better policing, increased imprisonment, demographic shifts, legalized abortion in the 1970s, and economic expansion. The sharp decline in crime during the 1990s presents a methodological challenge for our study, as it occurred at the same time welfare reform was unfolding, making it important to disentangle the effects of welfare reform from other trends related to arrests.

Figure 4, which shows arrest rates for minor crimes for each of the 5 cohorts from 1990 to 2005, reveals a somewhat different pattern than that shown in Figure 3 for serious crimes. For males age 25–29 years, arrest rates declined throughout the period, whereas for the 4 minor cohorts, arrest rates increased until the latter part of the 1990s and then declined.

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 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).

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 was implemented in all states over a period of 17 months. Combining both waivers and TANF, states implemented any 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 year and month, a given 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, in a given year and month, the state had implemented TANF post-PRWORA. 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).[3] 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 lag. Specifically, in our main models, welfare reform (any, AFDC, or TANF) is coded as "yes" for a given year if it had been in effect for at least 12 months. We also explicitly explore the timing of effects relative to the timing of welfare reform implementation in alternate models described later.

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 arrests to welfare reform:

(1) $$\text{Ln } A_{stm} = \alpha + \text{Welfare}_{stm\text{-}12}\, \Pi + Z_{st}\, \beta + \text{State}_s\, \Omega + \text{Year}_t * \text{Month}_m\, \Psi + (\text{State}_s * \text{MonthsPre}_{st})\, \Phi + \varepsilon_{stm}$$

This is a quasi-reduced form crime production function, which directly relates the natural log of arrests for individuals under the age of 18 to welfare policy measures and a vector of covariates.[4]

In Equation 1, youth arrests (*Ln A*), measured for broad offense categories in state *s* during month *m* and year *t*, are a function of welfare policy (*Welfare*), characterized separately by the implementation of AFDC waivers and TANF in the given state with a 12-month lag, or alternatively, as any welfare reform (AFDC waiver or TANF) with a 12-month lag. As noted earlier, we model the policy response with a lag in order to allow for any delayed effects between

[3] We acknowledge that differences across state welfare programs are more nuanced and dynamic than what is captured by our measures. However, the current approach represents a broad first look at the effects of welfare reform on youth crime and is consistent with the literature on the effects of welfare reform on other outcomes.

[4] The 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. All models control for various measures of the relevant population base, thus allowing the coefficient of population to be unrestricted.

adoption of welfare reform, mothers' employment, children's criminal behavior, and ultimately arrests. We also present estimates for contemporaneous effects and for other lag structures.

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 population base, and enforcement. The parameter ε 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 and time (*Year*Month*) fixed effects, which account non-parametrically for national trends in criminal activity and the arrest rate, other concurrent national trends, and 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.

Ideally, we would estimate models of the effects of welfare reform on crime using data on the population of youth most likely to reside in welfare-receiving households — those with low-educated unmarried mothers. However, the FBI arrest data do not allow us to identify educational attainment, marital status, or other characteristics of arrestees' mothers, as the only available demographics are sex and age of the perpetrator. This is less of a limitation than it may seem because: (1) we would not want to restrict the sample to youth 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 are typically 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, though this 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 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.[5] We address the specter of potentially confounding effects from unobserved factors, including those from the 1994 crime bill, in a number of ways.

First, all models control parametrically for unobserved state-specific linear trends pre-welfare reform ($State_s * MonthsPre_{st}$). This relaxes the parallel trends assumption and allow for differential trends in youth crime across states prior to the implementation of welfare reform. We also 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,

[5] See: https://www.ncjrs.gov/txtfiles/billfs.txt.

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.

Second, we explicitly control for a large vector of concurrent state-specific time-varying factors (Z) that may impact youth arrest rates, including the maximum age of juvenile court jurisdiction, 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.[6] 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.[7] Specifications with

[6] Travis, Western & 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.

[7] The 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.

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.[8]

Fourth, in our preferred specifications, we control for the natural log of total arrests and the natural log of arrests related to the specific crime category being studied, 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. These controls 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. They may also capture overall shifts in arrests and overall shifts in a particular category of arrests in a given state due to further time-varying state characteristics such as enforcement and penalties, local economic conditions, and other unobserved factors. This strategy follows Dave et al. (2011, 2016), which employed controls for trends in male insurance coverage when estimating insurance rates among pregnant women, and trends in birth outcomes for a placebo group when estimating minimum wage effects on infant health.

Note 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 DDD framework if males were used as a direct control group, and older males are not a fully-

[8] We 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. These specifications did not suggest any significant or consistent effects, which is validating.

equivalent comparison group for studying youth crime in a DDD context. The use of older males as a comparison group would assume that, in the absence of welfare reform, for every one-percentage point change in arrests among older men there would be an equal change in arrests among youth—an assumption that is not supported by the time trends (Figures 1 and 2) and (later) explicitly rejected in all of our models. Instead, by controlling for older 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.

When we find credible reduced-form effects of welfare reform on crime, we conduct additional analyses. First, 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, and for states that had relatively large and smaller declines in their welfare caseloads. Second, we further consider potential time lags between the implementation of welfare reform and behavioral responses that result in arrests. It may take time for youth to understand the implications of the new regime for their ability to make ends meet, or it could take a number of crimes over months for an individual to be caught and arrested. We explore these possibilities and further check for differential trends and lead effects by estimating models that decompose the timing of the effects and estimate an event study. Third, we assess the plausibility of our estimates by conducting placebo tests using corresponding adult male arrests as the outcome. Given that welfare reform policies mostly impact women and their children, we do not a priori expect large or significant effects on adult male crime.[9] Any finding to the contrary would indicate that any observed effects of welfare reform on youth crime are likely confounded by unobserved state-specific trends.

[9] This assumes that there is no spillover to the market for adult male criminal activity.

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, along with corresponding arrest measures for males in all of 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-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

Main Estimates

We present our main estimates for arrests relating to serious and minor offenses among males and females ages 10–14 (models 1–4) and ages 15–17 (models 5–8) years old in Tables 2–5. All specifications control for state and year-by-month fixed effects as well as pre-welfare reform state-specific linear trends and the gender-specific natural logarithm of the state population under 18. As a baseline, we start with a parsimonious model (specifications 1 and 5) with state covariates, and then progressively add richer controls to account for unobserved state trends and policy endogeneity. Specifically, models (2) and (3) (correspondingly 6 and 7 for older youth) add arrests for all offenses and the specific offenses being studied for adult males.[10]. The final specification for each youth group (models 4 and 8) further add lagged economic indicators and welfare caseloads to address policy endogeneity concerns. We estimate the effects of any welfare reform, measured as the implementation of a major waiver to the state's AFDC program or the implementation of TANF, as well as differential effects of the AFDC waivers and TANF—all coded "yes" in a given year and month if welfare reform had been in effect at all at least 12 months prior.

Tables 2 and 3 present estimates for arrests relating to serious offenses for males and females, respectively. We do not find any evidence that welfare reform significantly impacted this category of arrests, for either gender, which is not surprising given that it includes arrests for violent and felony offenses that are not likely to be responsive to changes in maternal supervision and household income on the margin, at least within the range induced by welfare reform.

In Tables 4 and 5, we assess effects for minor offenses which may be more responsive at the margin to welfare-reform induced shifts in maternal supervision, time constraints, and household resources. These include offenses such as disorderly conduct, vagrancy, and status offenses (e.g., curfew and loitering violations).

Estimates for males (Table 4), for the most basic specification (models 1 and 5), suggest that welfare reform reduced minor offenses by 4.8% among youth ages 10–14, and by 7.5% among older youth ages 15–17, though both estimates are imprecise and statistically

[10] The coefficients for measures of the adult male arrest rates are generally positive and statistically significant in all models, indicating that these controls are addressing important potentially confounding factors within states over time. The coefficients, however, are significantly and uniformly less than one, suggesting that trends in juvenile crime and adult male crime do not vary together on a one-for-one basis and underscoring our rationale for not using adult males ages 25–49 as a direct comparison group for female crime within a DDD framework.

insignificant. The effect magnitudes are relatively robust to adding the measures of crime for older males in specifications (2) and (6), and to adding the extended state-level controls in specifications (4) and (8). Those models suggest declines of 6% among the younger adolescents, but none of these effects are statistically significant. We therefore interpret these estimates as suggestive and weak evidence of a decline in minor crimes among youths aged 10–14, though large standard errors also make it likely that welfare reform has had no major effects on criminal activity for this age group. For older adolescents however, we find more consistent evidence that the shift in welfare policy is associated with about a statistically significant 8% reduction in minor crimes. This robustness is reassuring in that the effects do not seem to be sensitive to omitted controls for unobserved trends. This is partly due to the controls for pre-reform state-specific trends, which we include in all specifications, and which to some extent already account for unobserved state-specific time-varying heterogeneity and policy endogeneity.[11] The added covariates do, however, reduce the sampling variance and improve the precision of the estimates. When we consider separate effects for AFDC waivers and TANF (model 7 for youths 15–17), estimates indicate somewhat larger declines associated with TANF (10%) compared to the waivers (8%) though the relatively large standard errors makes this difference statistically insignificant. Table 5 presents corresponding estimates for minor offenses among females. These effects are quite similar to those for males. Generally, welfare reform reduced arrests for minor offenses among females ages 15–17 by about 9–10%.

While our conceptual framework is generally agnostic about the direction of the effects given the multitude of potentially reinforcing or competing pathways discussed earlier, the pattern of results that we find is ex post validating when contrasted with the unconditional trends

[11] As expected, when we exclude these pre-reform state-specific trends, the estimates become more sensitive to additional controls.

presented in Figures 3 and 4. Specifically, the youth arrest time trends for both serious and minor crimes follow more or less similar trajectories in Figures 3 and 4, reaching a local peak around the mid-1990s and mostly declining over the period that welfare reform was unfolding. If our models had suggested blanket declines in youth arrests for both serious and minor crimes, this might cast doubt that we have adequately controlled for such time-varying trends. However, we find significant results of the effects of welfare reform only on minor crimes, while estimates for serious crimes (which also exhibited strong unconditional downward trends as welfare reform was being implemented) are economically and statistically insignificant.

Specification Checks

We conducted a series of supplemental analyses for minor crimes among youth ages 15–17, for which it appears that welfare reform had an effect. These are presented in Table 6 and 7. Models in Table 6 explore the timing of the effects, and those in Table 7 assess heterogeneity across states and also present some placebo checks.

The main effects presented above correspond to a 12-month lag with respect to policy implementation. Models (1) and (5) in Table 6 reproduce these effects (from our preferred specification 6 in Tables 4 and 5)[12] for males and females respectively. In models (2) and (6), we consider the 6-month lagged effect, and in models (3) and (7) we consider just the one-month lag. Expectedly, and in a pattern that is validating, the effect magnitudes decline as the lag gets shorter. For the one-month lagged effect, the estimate is halved and becomes statistically insignificant. Given the time it may take between the reform to mothers finding employment to

[12] Our preferred specification excludes the extended state-level covariates since these covariates are potentially endogenous, even though we have lagged these measures by 1–2 years, due to the strong correlation in a state's economic conditions and welfare caseloads over time. Our estimates and discussion are not materially changed if we include the extended state-level covariates in the models in Tables 6 and 7.

criminal activity and arrests, we would not expect strong substantial effects in months immediately subsequent to the reform.

In specifications 4 and 8, we estimate an event study, separating out the timing of the impact of welfare reform into lead effects, contemporaneous effects, and lagged effects. We include indicators for 37 or more month pre-policy adoption, 25–36 months pre-adoption, 13–24 months pre-adoption, 1–12 months pre-adoption (reference category), 0–11 months post-adoption (where t=0 indicates the month that the reform was adopted in the particular state), 12–13 months post-adoption, 24–35 months post-adoption, and 3 or more years post adoption. The coefficient of each of these indicators represents the effect of welfare reform on arrests for minor crimes during the specific time window, relative to the year directly prior to the policy adoption.

The event study fulfills two purposes. First, it allows us to assess the credibility of our research design. That the lead effects are small and statistically insignificant is validating. In our preferred specification, which control for adult male arrests, fixed effects at various levels, and pre-policy linear trends, there do not appear to be any residual differential trends between the reform and non-reform states. We also find no economically or statistically significant effects of welfare reform in the year that it was implemented; this is partly validating since if we had found large negative effects immediately, this would likely just reflect as spurious correlation with any declining crime trends and cast doubt on a causal interpretation.

The main effects in Table 4 and 5 suggested a 7–10% decline in minor crimes. These are average effects realized for the average state over an average post-reform period of about 6 years. Second, the event study more formally decomposes the timing of the policy response. For both males and females (models 4 and 8 respectively) we find that reforms appeared to start having a discernible negative effect 12–23 months after it was implemented. Thus, arrests (for minor crimes) declined by about 5% for males and 7% for females in the second year post-reform, relative to the year prior to the reform. This decline became stronger over the next 12 months (over 24–35 months relative to the year prior to the reform), suggesting a reduction in arrests between 12–14%. For males, this decline does not appear to dissipate or cumulate over the longer term; that is, after the decline three years post-adoption, the coefficient on the indicator for periods further removed from reform implementation (≥ 3 Years post Welfare Reform) do not diminish substantially or become progressively more negative. For females, we find a slight dissipation, from a peak effect of about a 14% decline (months 24–35 post policy) to an average 11% decline 3 or mores year post reform. It is important to note that these models potentially capture a combination of exposure and age effects. For example, the effect of 3 years since welfare reform for a 17-year old would be for an individual who was about 14 years old when welfare reform was implemented, but for a 13-year old would be for an individual who was 10 years old when welfare reform was implemented. The estimated effects of 3 years since welfare reform could reflect lagged or cumulative exposure, but it is also possible that welfare reform has differential effects at different ages. Due to the perfect correlation between cumulative exposure and the age at which the individual was first, the effects of time since welfare reform should be interpreted as a conflation of these two potential effects. Thus, the pattern noted above for females is suggestive of effects of welfare reform that somewhat got weaker over the longer-term and/or that welfare reform had somewhat stronger effects for females who were exposed at relatively older ages.

In Table 7, we broadly test the work mechanism by first exploiting the extent to which states encouraged work under their TANF programs, as work-incentive policies (such as benefit generosity, time limits, earnings disregards, and sanctions for non-compliance) vary considerably across states. If the observed reduction in minor crime represents a causal effect, we would expect to see larger effects in states with stronger work incentives than in those with weaker work incentives. Specifications 1 and 2 in Table 7 test for this potential "dose-response" association by including an interaction between our welfare reform indicator and another indicator for whether the state had relatively strong work incentives under its TANF program.[13] Indeed, for both male and female youth, we find stronger negative effects of any welfare reform on minor crime in states with strong work incentives—about 10–13% compared to about 7–8% in states with weaker work incentives. These estimates are imprecise, however, due to inflated standard errors and reduced statistical power.

Second, we implement another dose-response check by assessing whether the declines in minor crime were stronger in states that experienced larger relative declines in their welfare caseloads over the period. In specifications 3 and 4, we interact the percent decline in welfare caseloads with the indicator for any welfare reform. In these specifications, the coefficient of the main welfare reform indicator is an extrapolation and would capture the effects on crime among states that had no reductions in welfare caseloads. We therefore expect the coefficient of the interaction term to be strongly negative, and certainly more negative than the main effect. Indeed, for both males and females, the interaction effects are strongly negative with much larger magnitudes than the main effects. These estimates suggest approximately a 10–16 % decline in minor crime (associated with a 100% reduction in welfare caseload**).** Given that the average

[13] We used the classification of states as having strong work incentives or not by Blank and Schmidt (2001), wherein states were considered to have strong work incentives if they had relatively low benefit generosity, high earnings disregards, strict sanctions, and strict time limits.

caseload decline was about 70%, these estimates suggest a decrease in arrests for minor crimes on the order of 7–11%, which is in line with the estimates in Tables 4 and 5.[14]

Results from the placebo test modeling arrests for serious and minor crime of adult males ages 25–29 and ages 25–49 as a function of any welfare reform (specification 5–8 in Table 7) indicate that welfare reform had an insignificant (and small magnitude) effect, suggesting that the observed associations between welfare reform and arrests for the affected groups are not spurious. We note that crime rates for adults males were declining (see Figures 3 and 4) over the same period as welfare reform unfolded. If our estimates for youth are mostly reflective of a declining trend in crime, we would have observed significant and large negative effects of adult males as well.

Additional Robustness Checks

Finally, in models not shown we confirmed that our results are not sensitive to alternate functional forms or model specifications that, alternatively: (1) expressed the outcome in level terms, as the natural log of arrests and controlling for the state's female population; (2) changed the outcome to a logistic transformation based on the natural log of the odds of arrest [$\ln((A_{st}/Population_{st})/(1-(A_{st}/Population_{st}))$]; and (3) used non-logged measures of the arrest rate or total arrests 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. In additional analyses, we ascertained that our estimates are insensitive to these cut-offs, and we further confirmed that our estimates are not sensitive to restricting our sample to

[14] Specifically, we compute the natural log difference in caseloads between the year prior to when the state instituted welfare reform and 2001. This log difference is interacted with the welfare reform indicator. The mean log difference is about 0.70.

individuals and agencies with a reported coverage of at least 50%. We also estimated models that included both pre-policy adoption linear trends and post-policy adoption linear trends, allowing both sets of trends to differ by state. These controls address the concern that differential trends may have shifted in ways that are correlated with when welfare reform was being implemented in states.[15] While the standard errors inflate and render some of our estimates statistically insignificant (p-value ranging from 0. 08 to 0.19), the effect magnitudes continue to suggest a 7–11% decline in minor crimes.

Conclusion

This study found robust evidence that welfare reform in the U.S. in the 1990s led to decreased youth arrests for minor crimes, in the range of 7 to 9 %, but had no effect on youth arrests for serious crimes. We thus infer that maternal employment reduces minor youth criminal behavior, which includes violations of liquor and curfew laws and disorderly conduct, but not serious youth criminal behavior, which includes violence and felony theft. As expected, the effects were stronger in states with stronger work incentive policies and larger caseload declines. Contrary to our expectations, results were similar for males and females, suggesting that the effects do not operate by changing adolescent girls' expectations about welfare reliance as an option for themselves in the future.

The results from this study add to the scant literature on the effects of maternal employment on adolescent behavior by exploiting a large-scale social experiment that imposed strong work incentives for low income mothers that is still in effect to this day, and provide some support for the widely-embraced argument that welfare reform would discourage undesirable

[15] In addition to limiting the identifying variation being exploited, these controls may also amount to "throwing out the baby with the bath water". If welfare reform is responsible for any shift in trends, then adding these controls will underestimate the true impact.

social behavior, not only of mothers, but also of the next generation. Future research is needed to explore heterogenous treatment effects by child and maternal characteristics, as well as to investigate hypothesized pathways.

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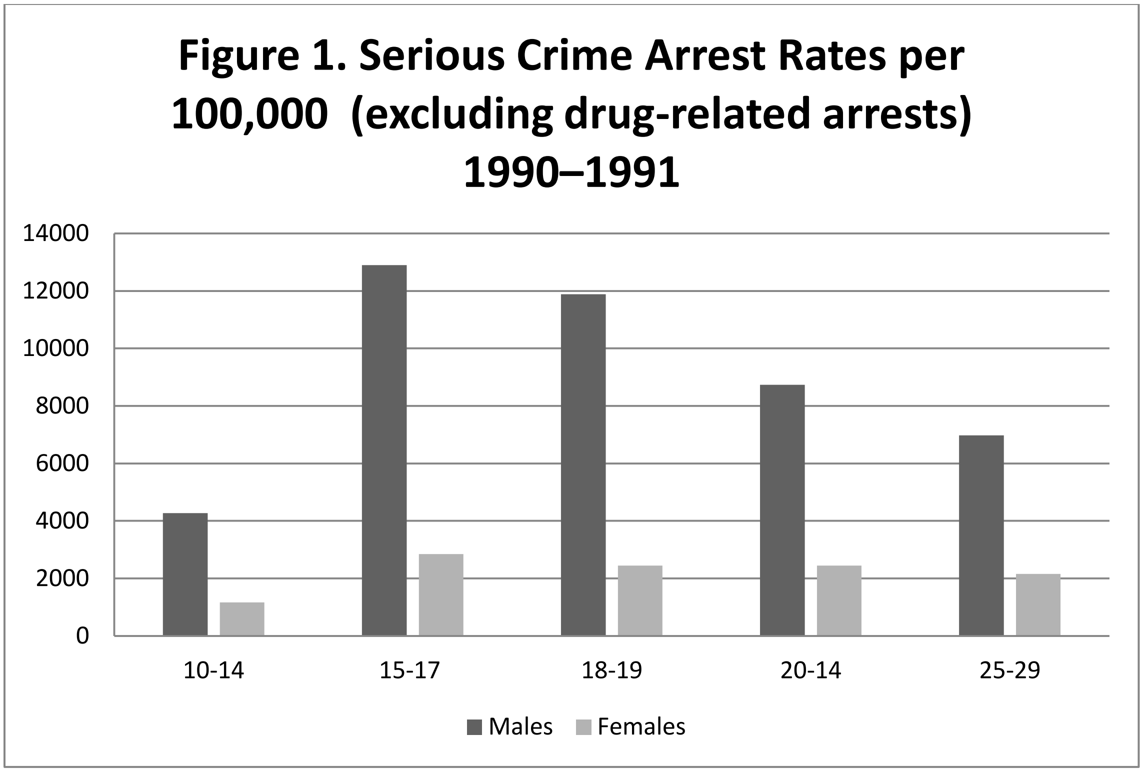
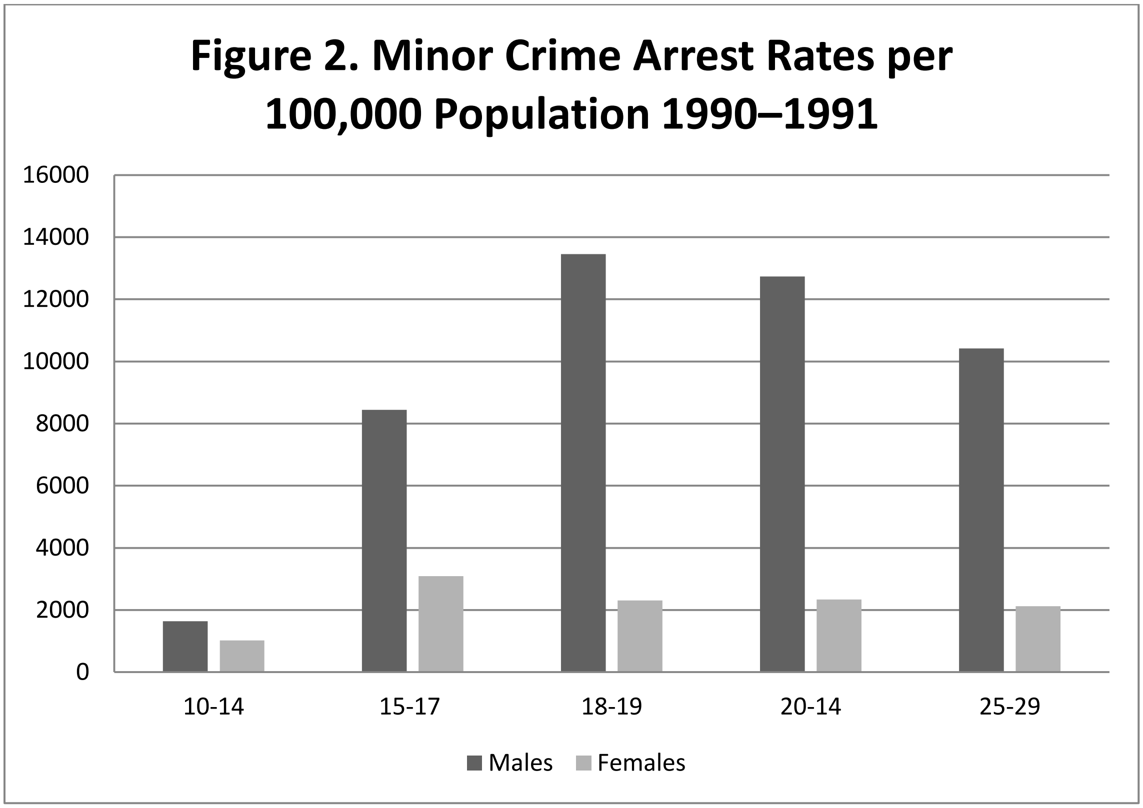
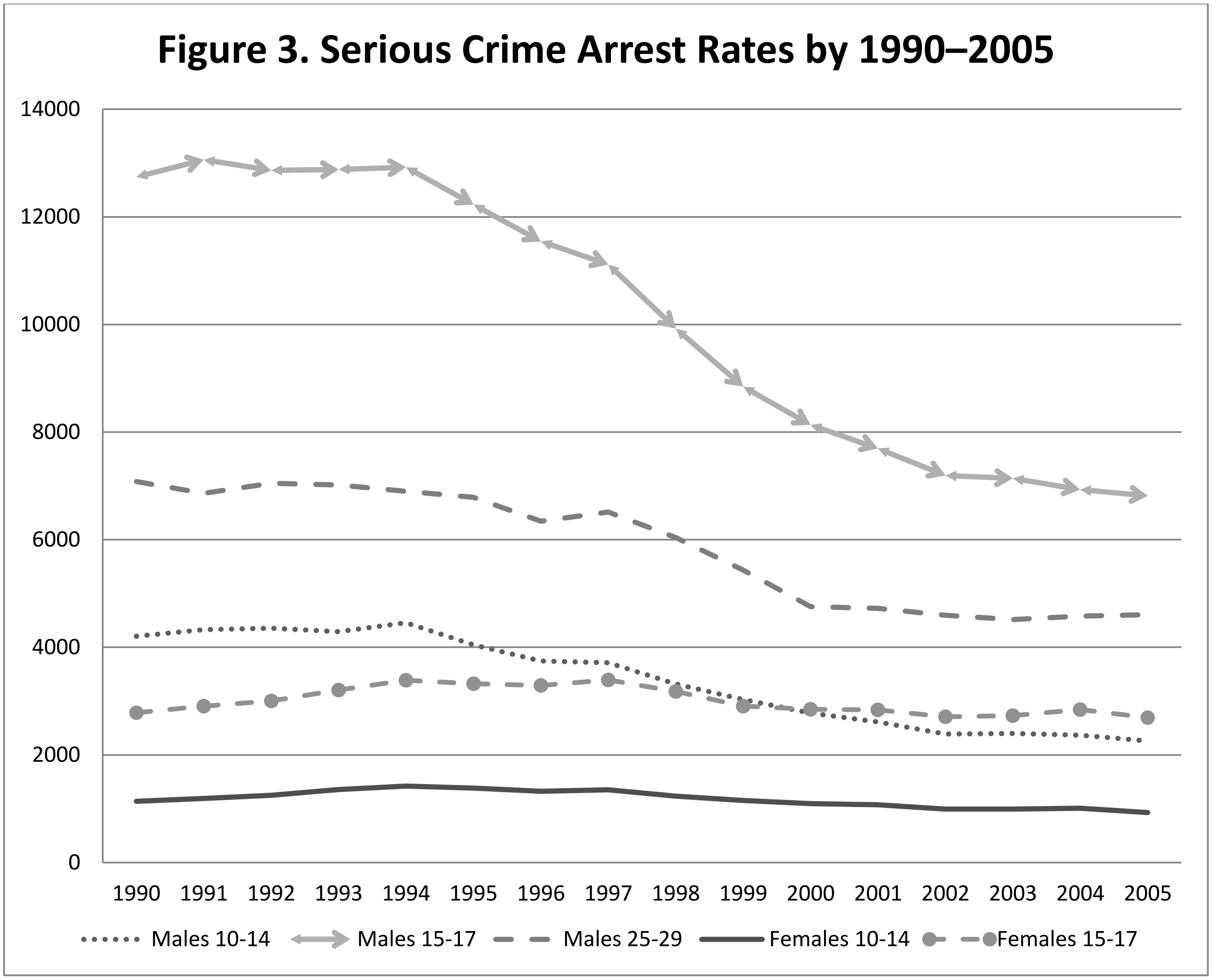


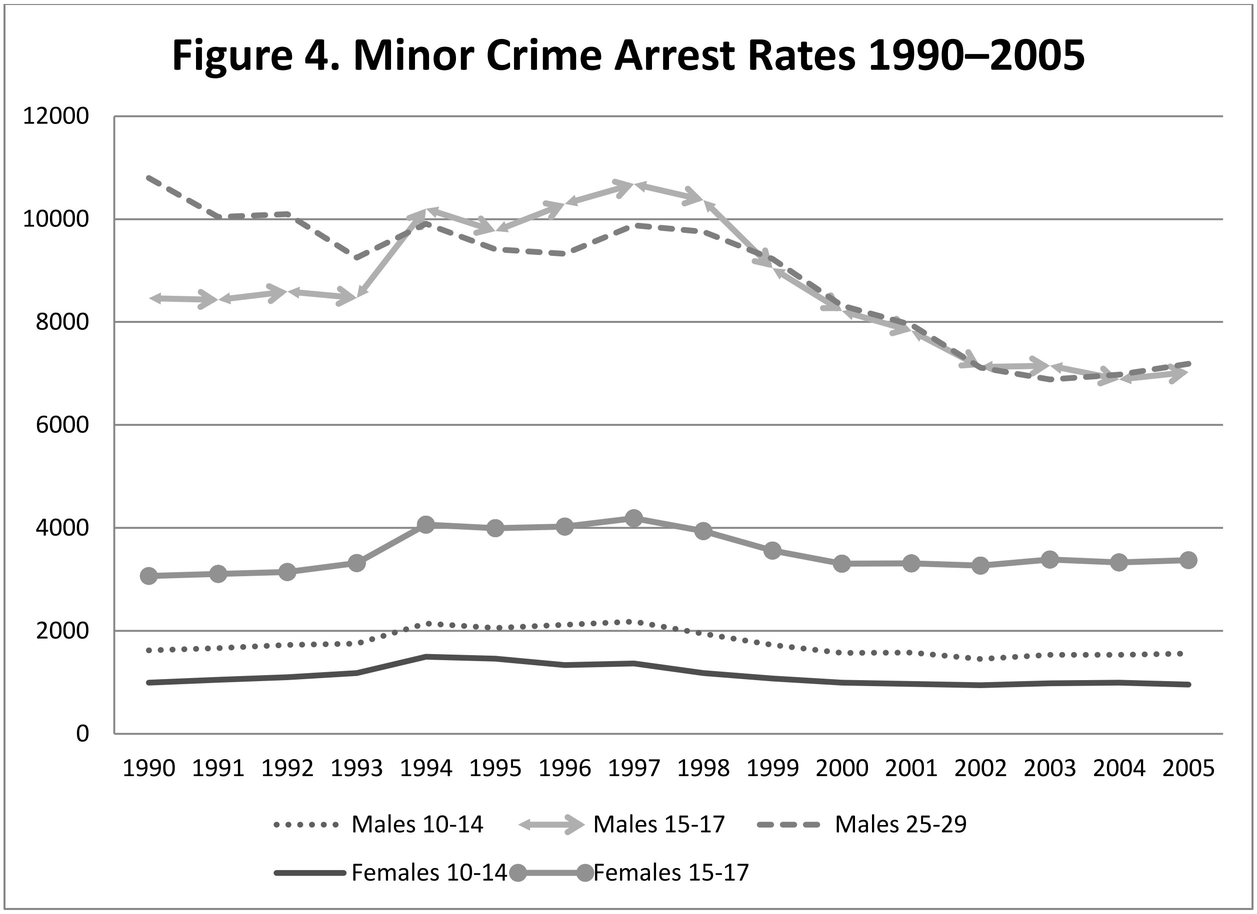
Notes: Arrests are based on data from criminal justice agencies with populations of at least 50,000 and coverage for at least half of the year. We added arrests from all such agencies in all states in a given year and divided this annual total by the total population of those reporting agencies for that state/year as a fraction of the total state population in that year. We then computed the mean for 1990–1991.
Source of population data by state, age, and sex: U.S. Census https://www.census.gov/popest/data/state/asrh/1990s/st_age_sex.html for the 1990s (accessed 5/21/15).
Serious crimes included are murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, other assault, arson, forgery and counterfeiting, fraud, embezzlement, stolen property crimes, vandalism, weapons offenses, prostitution, other sex offenses and gambling.



Notes: For sources and computation, see notes to Figure 1. Minor crimes are offenses against family and children, driving under the influence, liquor law violations, drunkenness, disorderly conduct, vagrancy, suspicion, curfew violations and loitering, runaways, and all other crimes except traffic violations.



Notes: Arrests are based on data from criminal justice agencies with populations of at least 50,000 and coverage for at least half of the year. We added arrests from all such agencies in all states in a given year and divided this annual total by the total population of those reporting agencies for that state/year as a fraction of the total state population in that year. We then computed the mean for the 16 years (1990–2005).
Source of population data by state, age, and sex: U.S. Census
https://www.census.gov/popest/data/state/asrh/1990s/st_age_sex.html for the 1990s (accessed 5/21/15).
https://www.census.gov/popest/data/intercensal/state/files/ST-EST00INT-AGESEX.csv for the 2000s (accessed 5/21/15).
Serious crimes included are murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, other assault, arson, forgery and counterfeiting, fraud, embezzlement, stolen property crimes, vandalism, weapons offenses, prostitution, other sex offenses, and gambling.



Notes: For sources and computation, see notes to Figure 3. Minor crimes are offenses against family and children, driving under the influence, liquor law violations, drunkenness, disorderly conduct, vagrancy, suspicion, curfew violations and loitering, runaways, and all other crimes except traffic violations.

Notes: Arrests are based on data from criminal justice agencies with populations of at least 50,000 and coverage for at least half of the year. We added arrests from all such agencies in all states in a given year and divided this annual total by the total population of those reporting agencies for that state/year as a fraction of the total state population in that year. We then computed the mean for 1990–1991.
Source of population data by state, age, and sex: U.S. Census https://www.census.gov/popest/data/state/asrh/1990s/st_age_sex.html for the 1990s (accessed 5/21/15).
Serious crimes included are murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, other assault, arson, forgery and counterfeiting, fraud, embezzlement, stolen property crimes, vandalism, weapons offenses, prostitution, other sex offenses and gambling.

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

	10/92 to 2/97 **AFDC Waiver**	9/96 to 1/98 **TANF**	10/92 to 1/98 **Any Welfare Reform**		10/92 to 2/97 **AFDC Waiver**	9/96 to 1/98 **TANF**	10/92 to 1/98 **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).

Table 2
Males Ages 10–14 & 15–17
Effects on Ln Arrests – Serious Offenses (excluding Drug Violations)
1990 – 2005

Sample	**Ages 10–14**				**Ages 15–17**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform	0.0525	0.0307		0.0221	0.0232	-0.0103		-0.0012
	(0.0599)	(0.0499)		(0.0615)	(0.0507)	(0.0341)		(0.0335)
AFDC Waiver			0.0209				-0.0105	
			(0.0453)				(0.0346)	
TANF			0.0873				-0.0091	
			(0.0995)				(0.0488)	
Extended Covariates	No	No	No	Yes	No	No	No	Yes
Arrests – All Offenses Males 25-29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Arrests – Serious Offenses Males 25-29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8921	8895	8895	8895	8921	8895	8895	8895

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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. . All models further control for state and year*month fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capital 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
Females Ages 10–14 & 15–17
Effects on Ln Arrests – Serious Offenses (excluding Drug Violations)
1990 – 2005

Sample	**Ages 10–14**				**Ages 15–17**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform	0.0656	0.0449		0.0545	0.0277	0.0005		0.0132
	(0.0498)	(0.0414)		(0.0372)	(0.0423)	(0.0325)		(0.0301)
AFDC Waiver			0.0364				-0.0005	
			(0.0375)				(0.0326)	
TANF			0.0930				0.0064	
			(0.0905)				(0.0482)	
Extended Covariates	No	No	No	Yes	No	No	No	Yes
Arrests – All Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Arrests – Serious Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8921	8895	8895	8895	8921	8895	8895	8895

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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. All models further control for state and year*month fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capital 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 4
Males Ages 10–14 & 15–17
Effects on Ln Arrests – Minor Offenses
1990 – 2005

Sample	**Ages 10–14**				**Ages 15–17**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform	-0.0483	-0.0569		-0.0628	-0.0747	-0.0837**		-0.0785*
	(0.0682)	(0.0643)		(0.0582)	(0.0490)	(0.0402)		(0.0413)
AFDC Waiver			-0.0753				-0.0814*	
			(0.0543)				(0.0424)	
TANF			0.0482				-0.0972*	
			(0.1569)				(0.0506)	
Extended Covariates	No	No	No	Yes	No	No	No	Yes
Arrests – All Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Arrests – Serious Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8920	8902	8902	8902	8920	8902	8902	8902

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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. All models further control for state and year*month fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capital 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 5
Females Ages 10–14 & 15–17
Effects on Ln Arrests – Minor Offenses
1990 – 2005

Sample	**Ages 10–14**				**Ages 15–17**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform	-0.0685	-0.0719		-0.0740	-0.0919	-0.0942*		-0.1051*
	(0.0642)	(0.0605)		(0.0539)	(0.0567)	(0.0543)		(0.0559)
AFDC Waiver			-0.0981*				-0.1036**	
			(0.0525)				(0.0512)	
TANF			0.0772				-0.0382	
			(0.1372)				(0.0893)	
Extended Covariates	No	No	No	Yes	No	No	No	Yes
Arrests – All Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Arrests – Serious Offenses Males 25–29	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8920	8902	8902	8902	8920	8902	8902	8902

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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. All models further control for state and year*month fixed effects. Extended covariates include the 1-year lag of the unemployment rate and per capital 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 6
Effects on Ln Arrests – Minor Offenses
Lagged Effects & Event Study
1990 – 2005

Sample	**Males Ages 15–17**	**Males Ages 15–17**	**Males Ages 15–17**	**Males Ages 15–17**	**Females Ages 15–17**	**Females Ages 15–17**	**Females Ages 15–17**	**Females Ages 15-17**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform: 12-month lag	-0.0837**				-0.0942*			
	(0.0402)				(0.0543)			
Any Welfare Reform: 6-month lag		-0.0712*				-0.0795		
		(0.0389)				(0.0521)		
Any Welfare Reform: 1-month lag			-0.0337				-0.0572	
			(0.0467)				(0.0556)	
>3 Years Pre Welfare Reform				-0.0267				-0.0292
				(0.1045)				(0.1226)
25–36 Months Pre Welfare Reform				-0.0148				0.0137
				(0.0710)				(0.0874)
13–24 Months Pre Welfare Reform				-0.0450				-0.0324
				(0.0471)				(0.0586)
1–12 Months Pre Welfare Reform				Ref.				Ref.
0–11 Months Post Welfare Reform				0.0153				-0.0067
				(0.0378)				(0.0346)
12–23 Months Post Welfare Reform				-0.0482				-0.0739
				(0.0485)				(0.0543)
24–35 Months Post Welfare Reform				-0.1181**				-0.1393**
				(0.0518)				(0.0581)
≥3 Years Post Welfare Reform				-0.1135*				-0.1101
				(0.0607)				(0.0906)
Observations	8902	8902	8902	8902	8902	8902	8902	8902

Notes: 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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. All models further control for state and year*month fixed effects. Asterisks denote statistical significance as follows: *** p-value ≤ 0.01; ** 0.01 < p-value ≤ 0.05; * 0.05 < p-value ≤ 0.1.

Table 7
Effects on Ln Arrests
Heterogeneous Effects and Placebo Effects
1990 – 2005

	State Heterogeneity				**Placebo Effects**			
Outcomes	**Ln Arrests Minor Offenses**				**Ln Arrests Serious Offenses**		**Ln Arrests Minor Offenses**	
Sample	**Males Ages 15–17**	**Females Ages 15–17**	**Males Ages 15–17**	**Females Ages 15–17**	**Males Ages 25–29**	**Males Ages 25–49**	**Males Ages 25–29**	**Males Ages 25–49**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Any Welfare Reform	-0.0715*	-0.0766	0.0122	0.0588	0.0542	0.0447	-0.0278	-0.0516
	(0.0424)	(0.0579)	(0.0900)	(0.1057)	(0.0393)	(0.0435)	(0.0387)	(0.0419)
Any Welfare Reform* Strong Work Incentives	-0.0274	-0.0585						
	(0.0707)	(0.0712)						
Any Welfare Reform* % Decrease in State Welfare Caseload			-0.0988	-0.1630				
			(0.0937)	(0.0979)				
Arrests – All Offenses Males 25–29	Yes	Yes	Yes	Yes	No	No	No	No
Arrests – Serious Offenses Males 25–29	Yes	Yes	Yes	Yes	No	No	No	No
Observations	8902	8902	8902	8902	8918	8928	8902	8915

Notes: 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 capital real personal income, log of total population, log of population of males (ages 10–14 or 15–17 as appropriate), 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, maximum age of juvenile court jurisdiction the coverage rate for reporting agencies, and pre-welfare reform state-specific linear trends. All models further control for state and year*month fixed effects. Strong work incentives are defined based on Blank and Schmidt (2001). Percent decrease in welfare caseloads refers to the decrease occurring between the year prior to when the state instituted welfare reform and 2001. Asterisks denote statistical significance as follows: *** p-value ≤ 0.01; ** 0.01 < p-value ≤ 0.05; * 0.05 < p-value ≤ 0.1.

Appendix A1: Crime categories and codes in FBI crime reports

Serious Criminal Offenses

UCR Code	
Violent "Index" Crimes	
01	Murder and Non-Negligent Manslaughter, Manslaughter by Negligence
02	Forcible Rape
03	Robbery
04	Aggravated Assault
Property "Index" Crimes	
05	Burglary - Breaking or Entering
06	Larceny/Theft (except motor vehicle)
07	Motor Vehicle Theft
Other Serious Criminal Offenses	
08	Other Assault
09	Arson
10	Forgery and Counterfeiting
11	Fraud
12	Embezzlement
13	Stolen Property - Buying, Receiving, Possession
14	Vandalism
15	Weapons - Carrying, Possessing, etc.
16	Prostitution and Commercialized Vice
17	Sex Offenses (except Forcible Rape and Prostitution)
18	Drug Abuse Violations
19	Gambling

Other Offenses

UCR Code	
20	Offenses Against Family and Children
21	Driving Under the Influence
22	Liquor Laws
23	Drunkenness
24	Disorderly Conduct
25	Vagrancy
26	All Other Offenses (except traffic)
27	Suspicion
28	Curfew and Loitering Law Violations
29	Runaways