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EFFECTS OF WELFARE REFORM ON WOMEN'S CRIME

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Working Paper 18887  
<http://www.nber.org/papers/w18887>

NATIONAL BUREAU OF ECONOMIC RESEARCH

1050 Massachusetts Avenue  
Cambridge, MA 02138  
March 2013

This research was funded in part by the Center for Health and Wellbeing at Princeton University. The authors are grateful for helpful comments from Karen Conway, Anca Cotet, Donka Mirtcheva, Hani Mansour, Michelle Phelps, Lucie Schmidt, and Anne Winkler, and for valuable research assistance from Taána Šepková, Dhiman Das, and Oliver Joszt. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 18887  
March 2013  
JEL No. H53,I3,J22,K42

**ABSTRACT**

We investigate the effects of broad-based work incentives on female crime by exploiting the welfare reform legislation of the 1990s, which dramatically increased employment among women at risk for relying on cash assistance. We find that welfare reform decreased female property crime arrests by 4–5%, but did not affect other types of crimes. The effects appear to be stronger in states with lower welfare benefits and higher earnings disregards, and in states with larger caseload declines. The findings point to broad-based work incentives—and, by inference, employment—as a key determinant of female property crime.

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Although crime is perceived to be a male activity and the propensity to engage in crime is higher for males than females, there is a secular trend in female crime in the U.S. that has received relatively little attention. In 1960, about 10% of arrestees for felony property crimes were women; that share increased to 35% by 2006. In 1960, 10% of arrestees for violent crimes of murder, manslaughter, and felonious assault were women; that share almost doubled by 2008 (Bartel 1979; U.S. Department of Justice). Thus, although women do commit fewer crimes than men, they account for a non-trivial and growing share of all crimes committed.

A large literature in economics, based on pioneering work by Becker (1968) and Ehrlich (1973), has focused on causes of crime. Most studies investigate determinants of reported crime rates, which do not provide information on the gender of the person committing the crimes. Therefore, because most crimes are committed by men, the findings from this literature do not necessarily apply to women. Within this literature, employment and labor market prospects stand out as important determinants of crime. Recent examples in the literature examining the effects of employment (or unemployment) on crime include studies by Raphael and Winter-Ebmer (2001), Gould, Weinberg and Mustard (2002), Corman and Mocan (2005), Edmark (2005), Ihlanfeldt (2006), and Lin (2008). These studies all found evidence that high unemployment and poor employment prospects are positively related to property crime, and that—with the exception of the study by Ihlanfeldt, which focused on male youth in one city—unemployment is positively, but neither strongly nor consistently, related to violent crime.

Very few population-based studies have specifically focused on causes of female crime, with the most noteworthy analyses having been conducted over 25 years ago. Bartel (1979) examined a cross-section of states in 1970, using female arrest rates as proxies for criminal activity. She found that women were responsive to deterrence variables in the expected direction,

and that marriage and labor force participation rates were associated with rates of female property crime. Phillips and Votey (1987) found that changes in marriage, labor force participation, and unemployment rates were important factors in explaining increases in arrest rates of women between 1952 and 1979. Although these studies were ground-breaking, labor force participation of women has increased, marriage has decreased, female headship has increased, and birth rates have decreased over the past half century (as described by McLanahan 2004), and, as indicated above, the share of crimes committed by women has increased. These sweeping changes, along with the availability of new data and modern econometric techniques, call for a re-examination of female crime. In this paper, we exploit a large-scale social experiment in the U.S. that dramatically increased employment among women at risk for relying on public cash assistance (i.e., generally, those with low human capital) to investigate the effects of work incentives—and, by inference, employment—on female crime.

The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, often referred to as welfare reform, ended entitlement to welfare benefits under Aid to Families with Dependent Children (AFDC) and replaced AFDC with Temporary Assistance for Needy Families (TANF) block grants to states. Features of the legislation included work requirements as a condition for receiving benefits, time limits on cash assistance, and increased state latitude in establishing program rules. Although welfare reform is often dated to the 1996 PRWORA legislation, reforms actually started taking place in the early 1990s with expansions in the use and scope of AFDC waivers. Many policies and features of state waiver programs were later incorporated into TANF. PRWORA emphasized a “work first” approach designed to reduce welfare dependence and reconnect members of an increasingly marginalized underclass to the mainstream ideals of a strong work ethic and civic responsibility (Katz 2001). Welfare reform

has been considered a success in that welfare rolls declined and employment rates of low-skilled mothers rose dramatically after implementation and a good portion of those changes can be attributed to welfare reform (Schoeni and Blank 2000; Ziliak 2006). Bell (2001) estimated that TANF reduced caseloads by between 19 and 35% (Bell 2001), and (Dave, Corman and Reichman 2012) found using the 1992–2002 Current Population Survey in a difference-in-difference framework that welfare reform overall raised the employment-to-population ratio among at-risk women between the ages of 21–49 by about 7–8 percentage points (Dave, Corman and Reichman 2012). The latter result is similar to estimates by other researchers (e.g., McKernan et al. 2000; Schoeni and Blank 2000).

Exploiting changes in the implementation of welfare reform across states and over time, we estimate the causal effects of the “work first” regime on adult women’s arrests from 1992 to 2002, the period during which welfare reform unfolded. We consider several different types of crime. We investigate the extent to which the effects are stronger in states with lower benefit generosity and higher earnings disregards, as economic theory would predict, and in states with larger caseload declines. The results from this study make an important contribution to the virtually non-existent literature on female crime by (indirectly) exploring the role of employment in a contemporary context, and provide important information about potential secondary effects of an important large-scale policy shift in the U.S.

### **Related literature**

There have been few population-based studies on the effects of welfare on crime. One exception is a comprehensive report by Hill and O’Neill (1993) that examined the effects of being on welfare, living in a neighborhood with a high welfare participation rate, and welfare generosity (among many other variables) on a broad range of “underclass behaviors.” Although

this study has been cited as evidence by “welfare as a root cause” proponents, the authors themselves are cautious about drawing conclusions from their results, which they considered preliminary. In their analysis of males aged 22–29 from the 1979 National Longitudinal Survey of Youth, they found no significant effects of family welfare participation or state welfare generosity on crime (measured as ever incarcerated). They did, however, find a significant and positive association between living in a zip code with a high level of welfare participation and crime, although causal inferences cannot be drawn and the potential mechanisms between welfare and male crime are not obvious.

Three of four existing published studies (all cross-sectional) of the effects of welfare generosity on crime have produced consistent findings. Zhang (1997) examined states in 1987 and found a negative association between state AFDC generosity and property crime. Hannon & DeFronzo (1998) studied urban counties in 1990 and found that welfare generosity was negatively related to both violent and property crimes. Fishback, Johnson and Kantor (2010), studying a panel of 83 cities between 1930 and 1940, found that New Deal relief decreased property crime during the Great Depression. In contrast, Niskanen (2006) found that AFDC benefits are positively related to violent crime. All of these studies grouped adult men, adult women, and minors together, potentially masking variation across those groups for whom the hypothesized mechanisms would be quite different (potential mechanisms linking adult male crime to welfare are the least obvious). Additionally, all of these studies were based on data from before implementation of PRWORA or AFDC waivers.

We know of only two (very recent) studies that have specifically examined the impact of welfare reform on crime. Monte and Lewis (2011) examined a cohort of about 1,400 female welfare recipients in Illinois in 1998 and found that leaving welfare without employment was

associated with subsequent arrest. This study focused on a select sample of women who were on welfare post-reform, did not disaggregate by type of crime, and did not address the endogeneity of welfare/employment status.

Corman et al. (2013) investigated the impact of welfare reform on women's illicit drug use, drug-related arrests, and imprisonment for drug-related crimes from 1992 (the beginning of welfare reform) to 2002. Exploiting changes in welfare reform across states and over time, they examined the impact of welfare reform using a difference-in-differences (DD) methodology. The authors found consistent evidence from multiple nationally-representative data sets that welfare reform reduced illicit drug use and drug crime among women at risk of welfare receipt and some evidence that the effects operate, at least in part, through work incentives under TANF as opposed to bans from welfare participation imposed under PRWORA for individuals with convictions for drug felonies. In addition, they found that the DD analysis was an appropriate and useful methodology for investigating the policy effects of welfare reform on crime.

### **Theoretical Framework**

Bartel (1979) applied theoretical work of Becker (1968) and Ehrlich (1973) in a model of women's criminal behavior in which individuals maximize the following expected utility function:

$$(1) E(U) = (1-p)U(X_1, t_c) + pU(X_2, t_c)$$

$X_1$  is income when not apprehended,  $X_2$  is income when apprehended,  $t_c$  is consumption (or household) time, and  $p$  is the probability of being apprehended. If  $T$  is the total amount of time available, then a woman must allocate her time between legal ( $t_l$ ), illegal ( $t_i$ ), and consumption or household production ( $t_c$ ) activities. The woman's income equations are:

$$(2) X_1 = W_l(t_l) + W_i(t_i) + W_o$$

$$(3) X_2 = W_i(t_i) + W_l(t_l) - F_i(t_i) + W_o$$

Income if not apprehended is equal to the wages from illegal ( $W_i$ ) and legal ( $W_l$ ) activities times the amount of time spent in each activity, plus other income ( $W_o$ ). If apprehended, the woman faces a penalty ( $F_i$ ), which is a function of the amount of time she spends in illegal activities. A recent paper by Zhang (1997) specifically includes welfare payments in the model, such that  $W_o$  has 2 components: welfare payments and other non-welfare income such as child support payments or other family financial support.

This model implies that a woman will be more likely to engage in crime the greater the difference between her illegal and legal wage, the lower her other income, the lower the probability of apprehension, and the lower the penalty if apprehended. Additionally, women who have a high level of risk preference (or low level of risk aversion) or low level of disutility from criminal behavior will be more likely to commit crimes. Welfare reform could affect specific arguments in this system of equations or even lead to a shift in tastes. For example, a decrease in  $W_o$  (perhaps through reaching a time limit or banking one's lifetime allotment of welfare benefits) would make a woman more likely to engage in both legal and illegal work and spend less time in consumption or household production. The demands of work requirements may increase stress which could lead to anti-social behavior including violent crime (e.g., if some individuals relieve their stress through physical aggression). In terms of our model, this would reflect a taste shift toward aggression.

Given the large reductions in welfare caseloads and increases in employment attributed welfare reform, discussed earlier, and that legal and illegal work are, to some extent, substitutes according to this model, we expect that reform welfare affected women's crime primarily by altering the tradeoffs between legal and illegal work. As such, we also expect that welfare reform



affected income-generating (property) crimes more strongly than violent crime, which would be consistent with the empirical literature on the effects of employment on crime.

The welfare-reform induced changes in tradeoffs between legal and illegal work could play out in different ways. Work requirements could result in increased legal wages as a result of more work experience, which would increase the gains from legal work versus crime. The new regime may also increase the costs of apprehension and conviction by making the mark of a criminal record more consequential (in terms of fewer available jobs and lower wages) if the mother expects to rely on welfare less and to work more. Additionally, working in the legal sector (a primary result of welfare reform) could potentially serve to “mainstream” women who were previously “marginalized”—by lowering their rate of time preference, increasing their disutility from engaging in illegal activities, or both. However, women who find themselves unable to support their families through legal work and welfare payments or have difficulty complying with program rules may choose illegal work to make ends meet.

Overall, if welfare reform led to increases in income through legal employment (as was found by Schoeni and Blank 2000 for the early years of welfare reform) we expect that the new regime led to reductions in illegal income-generating activities (property crimes). If, on the other hand, welfare reforms led to net decreases in income among poor women—e.g., if increased legal earnings did not offset decreased welfare payments—then the “work first” regime may have led to increases in property crimes. Overall, the effects of welfare reform on women’s crime will depend on strength of the various countervailing forces and the type of crime.

## **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 1992 through 2002, which provide the number of arrests by age and gender for each month/offense category/reporting agency. (2) Implementation dates of welfare reform at the state level during the same time period, from the Assistant Secretary for Planning and Evaluation at the U.S. Department of Health and Human Services (1999) [http://aspe.hhs.gov/hsp/waiver-policies99/Table\\_A.htm](http://aspe.hhs.gov/hsp/waiver-policies99/Table_A.htm). The former is used to create measures of arrests and the latter is used to characterize welfare reform, as described below.

### *Measures of Arrests*

We used the monthly data on arrests by age and gender from the FBI crime reports, collected by most large criminal justice agencies in the U.S., to create month/year/state measures of arrests. Although arrests or other proxies for crime (such as imprisonments) obtained from administrative data sources have a high degree of specificity, they do not include unreported crimes and little information is available on the characteristics of individuals committing the crimes. The alternative would be to use data from individual level surveys that ask about crime commission (and thus may include crimes not reported to the police), but in that mode crime is likely to be underreported. In addition, surveys that ask about crime rarely cover large geographic areas over time. 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., whether they reported to the FBI or not, indicating the population covered by that agency. Not all criminal justice agencies report on the number of arrests by month/offense category, and reporting most consistently in larger agencies. To obtain reasonably complete information, we limited our data

to agencies that cover at least 50,000 individuals. In 1996, the year that welfare reform was enacted, agencies with population of 50,000 or more comprised over half (about 55%) of the total U.S. population (147 million/268 million, calculated from the FBI files and U.S. Census data).

From these agency-based observations, we aggregated the data to the month/year/state level. Even among the larger 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 (or about 72%) were covered by agencies that reported arrests for each of the 12 months to the FBI. A few of the agencies reported arrests only for the month of December. Because some of these reports reflected annual rather than monthly figures, we dropped those agencies. For December of 1996, there were 28 agencies in this category, encompassing 6 million people. To control for both the total population in agencies covering populations 50,000 and above and the population actually covered by the FBI arrests in a particular offense category for the state/month/year, we include both the total state population in all agencies with populations 50,000 people or more and the total population covered by the FBI arrest data for that state/year/month/offense on the right-hand side in our models.

We consider different types of crimes, using the classifications provided by the FBI (see Appendix for a list of the crime categories and their codes). Categories 1 through 4 (murder/manslaughter, rape, robbery, and assault) are “violent index crimes.” Categories 5 through 7 (burglary, larceny, and motor vehicle theft) are “property index crimes.” We consider both property index crimes overall as well as the disaggregated category of larceny/theft (category 6). Categories 8 through 19 are a variety of other serious offenses, while categories 20

through 29 are more minor offenses.

### *Characterizing welfare reform*

Welfare reform in the U.S. 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 and Blank 2000).<sup>1</sup> Waivers altered the nature of welfare by implementing programs with some or all of the following features: work requirements, time limits, sanctions, earnings disregards, and family caps. Although states were required to report the specifics of their waiver plans to the U.S. Department of Health and Human Services (DHHS) in order to have them approved, specific features could be changed without official amendments to those plans (U.S. Department of Health and Human Services 1997).

The second phase of welfare reform came with the enactment of the PRWORA legislation, which dramatically restricted eligibility for cash assistance in the U.S. by expanding work requirements for those receiving welfare, establishing time limits on the receipt of welfare, and allowing states to impose stricter sanctions for non-compliance with work requirements. PRWORA also gave states increased latitude in establishing eligibility and program rules governing the administration of cash assistance (Blank 2002). States were required to submit plans for and—once approved, implement—TANF programs subject to federal guidelines and were (and still are) required to submit changes to their programs to DHHS. States implemented their approved TANF programs between September 1996 (Massachusetts, Michigan, and Vermont) and January 1999 (California) (Schoeni and Blank 2000). While work requirements, time limits, and sanctions are major components in TANF programs in all states, there is considerable variation across states in terms of specific policies along these and many other

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<sup>1</sup> Thirty states had implemented major waivers to their AFDC programs prior to TANF implementation.

dimensions. For example, in 1999, the maximum monthly welfare benefit for a family of three with no income ranged from \$164 in Alabama to \$923 in Alaska, earnings disregards for benefit computation (earnings not counted in the determination of welfare benefits) ranged from none in Arkansas and Wisconsin to 66.67% in Illinois, and the length of time on welfare before being required to work ranged from zero days in 37 states to 24 months in Maryland and West Virginia.<sup>2</sup>

Following the convention in the literature (reviewed in Blank 2002), we exploit differences in the timing of welfare reform across states with respect to both the timing of AFDC waivers and TANF implementation. In some models, we include separate measures of AFDC waiver and TANF implementation.<sup>3</sup> For waivers, we consider whether, in a given month, a given state had a statewide AFDC waiver in place that substantially altered the nature of AFDC with regard to time limits, Job Opportunities and Basic Skills training (JOBS) work exemptions, JOBS sanctions, earnings disregards, family caps, and/or work requirements. For TANF, we consider whether, in a given 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).<sup>4</sup>

## **Method**

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<sup>2</sup> See the Urban Institute's Welfare Rules Database (<http://anfdata.urban.org/wrd/WRDCategoryList.cfm> accessed 12/4/12).

<sup>3</sup> Information on state implementation of major AFDC waivers and TANF was obtained from the Assistant Secretary for Planning and Evaluation at the U.S. Department of Health and Human Services: [http://aspe.hhs.gov/HSP/Waiver-Policies99/policy\\_CEA.htm](http://aspe.hhs.gov/HSP/Waiver-Policies99/policy_CEA.htm).

<sup>4</sup> 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 women's crime and is consistent with the literature on the effects of welfare reform on other outcomes. Additionally, we consider specific features of TANF programs in secondary analyses described later.

The primary aim of this study is to estimate the impact of welfare reform on adult women’s criminal behavior, as measured by arrests. 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. Specifically, we estimate the following baseline model which relates changes in arrests to welfare reform:

$$(4) \quad \text{Ln } A_{smt} = \alpha + \text{Welfare}_{smt} \Pi + Z_{st} \beta + \text{State}_s \Omega + \text{Month}_m \Phi + \text{Year}_t \Psi + \varepsilon_{smt}$$

Equation (4) posits that female arrests ( $\text{Ln } A$ ), measured alternately by overall arrests and across specific 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 during the specific month and year, or alternatively, as any welfare reform (AFDC waiver or TANF). We present estimates from a semi-log model relating the natural log of arrests to a vector of covariates, separately controlling for the log of the relevant population base and allowing its coefficient to remain unrestricted.<sup>5</sup>

In addition, arrests depend on a vector of time-varying state-specific factors ( $Z$ ), including measures of the state’s economy and labor market conditions (unemployment rate, real personal income per capita, poverty rate, minimum wage), relevant population base (total state population; female population, covered population of reporting FBI agencies), and enforcement/treatment (criminal justice expenditures, state block grants on substance abuse prevention and treatment). The parameters of interest are the vector  $\Pi$ , which represents the “reduced form” or total effect of welfare reform on crime, operating through a variety of potential (and possibly competing) mechanisms. The parameter  $\varepsilon$  represents a state-time error

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<sup>5</sup> Estimates were not sensitive to alternate functional forms: 1) natural log of the probability of arrest:  $\ln(A_{smt}/\text{Population}_{st})$ ; and 2) logistic transformation based on the natural log of the odds of arrest:  $\ln((A_{smt}/\text{Population}_{st})/(1-(A_{smt}/\text{Population}_{st})))$ .

term. In all specifications, we include state fixed effects (*State*), which account for all unobserved time-invariant state specific factors, month (*Month*) fixed effects, which account for any seasonal factors affecting criminal behavior, and year (*Year*) fixed effects, which account for national trends in criminal activity and arrests.

Ideally, we would estimate models of the effects of welfare reform on female arrests using data on the population of women most likely to be welfare recipients—low-educated unmarried mothers. However, the FBI arrest data do not allow us to identify educational attainment, marital status, or motherhood of arrestees, as the only available demographics are gender and age. We restrict our analyses to women aged 21–49 years, since women in this age group are most likely to be mothers. This is less of a limitation than it may seem because: (1) We would not want to restrict the sample to 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 women who have been arrested, and female arrestees are typically low-educated and thus at high risk of being former, current, or future welfare recipients. Data from the National Survey of Drug Use and Health (NSDUH) show that, among women 21–49 who have ever been arrested or booked for an offense, the vast majority (84%) have less than a college education, 66% are unmarried, 61% have minor children, and 42% currently receive some form of government assistance (authors' own calculations based on the 2010 NSDUH).

We do not compare target and comparison groups in a difference-in-difference-in-differences framework because a valid and suitable comparison group to female arrestees is not available. As indicated above, there is little individual-level information about arrestees in the FBI data. Males are not a fully-equivalent comparison group for studying female crime because

of substantial segmentation of offense types by gender as well as differential trends in arrest rates by gender over the past five decades (Steffensmeier and Schwartz 2004). The use of males as a comparison group would assume that for every one-percentage point change in crime among men there would be, in the absence of welfare reform, an equal change in crime among women—an assumption that is not supported by the clearly divergent pre-welfare reform trends. Indeed, this assumption is later explicitly rejected in all of our models. We do use males to conduct “placebo tests” when we find significant effects of welfare reform on crime for females. That is, we run equivalent models, for males, for whom we would expect welfare reform to have much weaker, if any, impacts on criminal behavior.

We extend the basic model specified in Equation (1) in a number of ways to assess robustness and plausibility. A challenge in any policy analysis is in disentangling the effects of the policy of interest from other time-variant factors that may also affect the outcome. Even after controlling for an extended set of state-specific time-varying factors that may impact crime, the possibility of confounding trends and other concurrent policy shifts remains. For instance, while overall crime rates were declining in the 1990s, the rates of decline varied across states owing to differential shifts in economic conditions, enforcement, the state’s criminal justice system, other funding or crime-prevention resources and other state-specific changes in the costs and benefits of engaging in criminal activity. We address this specter of state-specific confounding trends in various ways. First, in order to control for general state-specific trends in crime rates, we include both the natural log of all arrests and the natural log of arrests for the specific offense category being modeled, among *males*, in all specifications. These measures capture overall shifts in crime in a given state due to unobserved time-varying state characteristics. This strategy follows



Dave et al. (2011), who employed controls for trends in male insurance coverage when estimating insurance rates among pregnant women.

It is possible that state experimentation with welfare reform through waivers and the timing of TANF implementation are related to prior increases in welfare caseloads and prior economic conditions. In other words, there may be lagged unobservable time-varying factors related to the state's economy and its welfare caseloads that may be correlated with the state's decision on the timing of welfare reform implementation. We address this possibility 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. In alternate specifications, we also add state-specific linear trends. In addition to addressing the possibility that policy implementation may be otherwise endogenous to the state's history, the parametric state-specific trends further account for other systematically-varying state-level factors that may have coincided with welfare reform.

When we find reduced-form effects of welfare reform on crime, we conduct a number of additional analyses. First, we consider that there could be a time lag between the implementation of welfare reform and behavioral responses that result in arrests. It may take time for women 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 the possibility of lagged effects of the reform on female arrests by estimating models with one and six month lags of the welfare reform measures.

Second, we explore the main channel through which we would expect welfare reform to impact women's criminal activity—employment. Work is the centerpiece of the policy shift and there is strong consensus that welfare reform has indeed increased employment and decreased

caseloads as intended. We broadly test this mechanism through interaction terms between the overall welfare reform measures and further variation in specific features of state TANF programs as described later. These estimates also serve as plausibility checks by testing for evidence of dose-response relations.

Third, as an additional check of a dose-response relation, we test for differential effects based on the percentage reduction in the welfare caseloads realized over the sample period for each state. Specifically, we estimate the following model separately for each state, as part of a two-step procedure, in order to quantify the effects of welfare reform on each state's welfare caseloads conditional on the state's economy.

$$(5) \quad \text{Ln Caseload}_t = \delta + \Lambda \text{ Welfare}_t + \text{Economy}_t \Gamma + \delta_1 \text{ Trend} + \delta_2 \text{ Trend}^2 + v_t$$

The parameter  $\Lambda$ , which is estimated separately for each state, represents the percent reduction in welfare caseloads associated with welfare reform (defined as the implementation of an AFDC waiver or TANF, whichever occurred first), conditioning on the state's economic conditions (proxied by the unemployment rate and real personal income per capita) and linear and quadratic trend terms. We find that across all states the average  $\Lambda$  is estimated to be about 25%, suggesting that welfare reform is responsible for a 25% decline in caseloads over our sample period. This average reduction is consistent with estimates of 19 to 35% from the literature cited earlier that TANF reduced caseloads by between 19 and 35%. In the second step, we modify the baseline model (Equation 4) to allow an interaction between the main effect of welfare reform and the welfare reform-induced percent reduction in caseloads for each state:

$$(6) \quad (\text{Ln A})_{\text{smt}} = \alpha + \lambda_1 \text{ Welfare}_{\text{smt}} + \lambda_2 (\text{Welfare}_{\text{smt}} * \Lambda_s) \\ + Z_{\text{st}} \beta + \text{State}_s \Omega + \text{Month}_m \Phi + \text{Year}_t \Psi + \varepsilon_{\text{smt}}$$

The parameter  $\lambda_1$  represents the effect of welfare reform on crime, *among those states which had no reductions in welfare caseloads attributed to welfare reform*; hence, we expect  $\lambda_1$  to be insignificant and close to zero if our estimates are truly reflective of a causal effect. Similarly,  $\lambda_2$  represents the effect of welfare reform on crime for a 100% reduction in caseloads (unit change in  $\Lambda_s$ ), *among those states which experienced reduction in caseloads attributed to welfare reform*. If the effects identified in the baseline specifications are indeed driven by welfare reform, then we would expect significant and consistent effects in the same direction to be also reflected in  $\lambda_2$ . If there is a dose-response effect based on strictness of state policies, we would expect the magnitude of  $\lambda_2$  to be substantially larger than that of  $\lambda_1$ .

We estimate all models using Ordinary Least Squares and adjust standard errors on the conservative side to account for all arbitrary correlation within state cells over time. The inclusion of state-specific linear trends in most of 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.

## **Results**

As discussed earlier, welfare reform has stronger conceptual and empirical links to property crime than to other types of crime. Table 1 presents estimates from models corresponding to Equation (1) for property crime—for all index property crimes (Codes 5–7 in Appendix) and specifically for larceny/theft (Code 6). The latter constitutes the largest single category of serious arrests for women. Specification 1 considers the separate effects of each phase of welfare reform (AFDC Waivers and TANF) on female arrests, controlling for monthly

male arrests; state, month, and year fixed effects; the annual state unemployment rate; state per capita real personal income; the logs of relevant populations (see table notes); the logs of state criminal justice expenditures and drug abuse prevention and treatment block grant; state minimum wage; and state poverty rate. In specification 2, we add state-specific linear trends to account for residual unobserved state-specific time-varying confounders.<sup>6</sup> In specification 3—our preferred specification—we add lagged economic conditions and welfare caseloads (see table notes) to address potential endogeneity of policy implementation. Specification 4 includes the same right-hand variables as specification 3, but uses a single indicator for any welfare reform (AFDC waiver or TANF) in order to maximize precision. The estimates from specification 1 suggest that AFDC waivers and TANF reduced serious property arrests by 2.3 and 2.5%, respectively, among women ages 21–49, although the effects are not statistically significant at conventional levels. However, in specification 2, which adds state-specific linear trends to further account for time-varying state-specific unobservables, and specification 3, which also includes lagged measures of the state’s economy and welfare caseloads in order to address potential policy endogeneity, we find that welfare reform significantly decreased women’s arrests for serious property crime by 4.4 to 4.9%. Thus, while the estimated effects of welfare reform on property arrests become stronger and more significant when accounting for state-specific linear trends, they are insensitive to the inclusion or exclusion of lagged state economic conditions and welfare caseloads. The estimated effect of welfare reform on arrests for property crime when using a single measure of welfare reform that combined AFDC waivers and TANF (specification 4) was very similar to the estimated effects from the corresponding model that included separate indicators for AFDC waivers and TANF (specification 3). The estimated

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<sup>6</sup> All estimates are fully robust to additional controls for state-specific quadratic trends (results available upon request).

effects of welfare reform on arrests for larceny/theft in particular, using either separate indicators of AFDC waivers and TANF or the combined measure of any welfare reform (specifications 5 and 6, respectively), are very similar to those from the corresponding models for property arrests overall (specifications 3 and 4, respectively). These results are consistent with the hypothesized scenario that welfare reform increased the returns to legal work compared to illegal income-generating activity.

The estimates in Table 1 reflect “intent-to-treat” (ITT) effects, since not all women arrestees are at risk of welfare receipt and consequently impacted by the policy shifts. As indicated earlier, data from the NSDUH (authors’ own calculations, not shown) indicate that among women ages 21–49 who were ever arrested or booked, about 40% are currently on some form of governmental assistance. Interpreting this prevalence as a conservative proxy for the fraction of women arrestees who are at risk of welfare receipt, the ITT effect can be scaled up by a factor of 2 to 3 in order to arrive at an estimate of the “treatment-on-the-treated” (TOT) effect - that is, the effect of welfare reform on crime among women who are impacted by shifts in welfare policy. Using the scale factor, estimates from Table 1 suggest that welfare reform is associated with an 11–12% decrease in property crime. In order to maintain consistency with the results presented in the tables, we do not scale up the estimates in the ensuing discussion.

Estimates in Table 2 are based on our preferred specifications (those corresponding to specifications 3 and 4 in Table 1) for violent index crimes (Codes 1–4), other serious offenses ranging from simple assault to gambling (Codes 8–19), and other, more minor offenses such as driving under the influence of alcohol and vagrancy (Codes 20–29). Although the signs of the welfare reform coefficients are generally negative as they were for property crime, the magnitudes are low and estimates never approach statistical significance (i.e., the t-values are

uniformly less than 1). We thus conclude that welfare reform did not have appreciable effects on violent crime, other serious offenses, or minor offenses committed by women. The weaker effects for these other types of crimes than for property crime are consistent with our expectation that the effects of welfare reform on crime would operate largely by changing relative returns to legal and illegal work.

In the models summarized in Tables 1 and 2, the estimated effects of the covariates (not shown in tables) are generally consistent with the literature. Criminal activity tends to be countercyclical—positively associated with the state unemployment rate and negatively associated with increases in real personal income per capita. States with higher minimum wages tend to have fewer arrests. Increases in substance abuse prevention and block grant spending are associated with lower crime rates and criminal justice expenditures are associated with higher crime rates, but both effects are statistically insignificant. For the latter, the positive sign may reflect greater resources allocated to criminal justice systems in states with higher crime rates. The coefficients for measures of male arrests are 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 female and male crime are not commensurate and underscoring our rationale for not using males as a direct comparison group for female crime within a DDD framework.

We conducted a series of supplemental analyses for property crime arrests, the category of arrests for which it appears that welfare reform had an effect. First, we considered potential lagged effects. In Table 3, we present estimates of the effects of welfare reform on all index property crimes, and for larceny/theft in particular, from models corresponding to specifications

3 and 4 of Table 1 but that include one or six-month lags of the welfare reform variables. We find that the estimated effects of welfare reform are quite similar when using one month lags as when considering contemporaneous effects in Table 1 and lead to the same inferences. However, the effects are somewhat smaller in magnitude when using six month lags than when using one month or no lags and are uniformly statistically significant only for larceny/theft.

Second, we conducted placebo tests using male property arrests as the outcome. Finding that welfare reform has similar or larger effects for males, for whom changes in incentives as a result of the policy shift should be much smaller and less direct than for females, would suggest that the observed associations between welfare reform and crime for females are spurious. These results are presented in Table 4. Specifications 1 through 4 present estimates from our preferred specification (corresponding to specification 1 in Table 3) for all property arrests and specifications 5 through 8 present estimates from our preferred specification for larceny/theft. For each outcome, models in the first two columns control for female arrests, whereas the models in the third and fourth columns do not. It is reassuring, and validating of our results for females, that welfare reform had uniformly insignificant (and small magnitude) effects on adult male arrests for property crime.

Models in Table 5 indirectly test the key hypothesized pathway through which welfare reform would be expected to lead to reductions in property crime among women—increases in the returns to legal work versus illegal income-generating activities. To the extent that market work and property crime are substitutes, welfare reform-driven increases in employment and decreases in caseloads should lead to a reduction in property crime. The specifications in Table 5 assess whether the declines in property crime are expectedly larger in those states in which incentives for legal versus illegal cash-generating activities were stronger and in which caseload

declines were larger. The specific features of state TANF programs most immediately and directly relevant to tradeoffs between legal work and property crime are cash benefits generosity and earnings disregards. Due to limited statistical power, we estimate differential effects across states along these dimensions through interaction terms rather than stratification of the full sample, and use the combined measure of any welfare reform. We would expect the effects of welfare reform on property crime to be stronger in states with lower cash benefits, higher earnings disregards, and larger caseload declines. We find in specifications 1 and 2 that the effects did appear to be stronger in states with lower cash benefits and higher earnings disregards, respectively, although the reductions in property crime associated with the policy variables are measured with much imprecision and, as such, those results should be interpreted as suggestive but preliminary. Specification 3 estimates Equation (3) to further assess whether the overall decline in serious crime is driven by a welfare reform-induced decrease in caseloads. It is validating that the magnitude of the main effect of welfare reform is much reduced and becomes statistically insignificant, suggesting that welfare reform should not, and indeed does not, have major effects on crime in states where it was not successful in reducing caseloads. The effect of the interaction term is negative with a high magnitude, but is measured with some imprecision (p-value of 0.12). Nevertheless, that estimate suggests that welfare reform led to a larger drop in female property crime in states with larger welfare-reform-induced declines in welfare caseloads. Specifically, a 100% reduction in caseloads (between 1992 and 2003) that can be attributed purely to welfare reform is associated with about a 5.9% reduction in serious property crime.

Finally, we considered the potential confounding effects of expansions of the federal earned income tax credit (EITC), which is a refundable tax credit for low- and middle-income families with qualifying children. An EITC expansion that passed in 1993 and became effective



in tax year 1995 raised the maximum credit for all qualifying families and further increased the differential in maximum benefits between families with two or more children relative to those with only one child. In 2001, the income level at which the EITC began to phase out for couples was further increased. These EITC expansions have been linked to shifts in labor supply, especially at the extensive margin (e.g., Eissa and Hoynes 2006; Hotz and Scholz 2003; Meyer 2002). The inclusion of the fixed time effects in our models should account for these EITC-induced trends in employment, labor supply, and income. In addition, our results are fully robust, in terms of direction, magnitude, and statistical significance, when the sample is limited to the years 1995–2001 (results not shown).<sup>7</sup> Although it is possible that the response to EITC was dependent on work effort, which in turn was impacted by welfare reform, this potential indirect effect would be a part of the total effect of welfare reform captured in our reduced form analysis, since this response would ultimately have been induced by a shift in welfare policy. Finally, some states offer an earned income tax credit through their state income tax systems, although in 2001 only 14 states and D.C. did so and the average benefit level was only about 16% of the federal level (Hotz and Scholz 2003).<sup>8</sup> The estimates and conclusions are not materially altered when separately controlling for the presence of state-level EITC programs and the average benefit level (as a percent of the federal level) among states that have such programs (results not shown).

### **Effects in Context**

In this section, we situate our estimates in the broader context. First, we calculate the elasticity of female property arrests with respect to employment and reconcile that estimate with

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<sup>7</sup> Eleven states had already implemented major waivers to their AFDC programs between 1992–1994, an additional 8 states implemented major waivers in various months of 1995, and 10 states implemented waivers in 1996 prior to TANF implementation. Thus, there is still considerable variation in AFDC waivers across states and over time to identify the effects of waivers over this more limited time period.

<sup>8</sup> The average federal credit in 2001 was approximately \$1600 for a family claimant.

elasticities from existing studies of unemployment on crime. Second, we use our estimates to project the number of property crimes that were likely averted due to the “work first” regime. Third, we derive a ballpark estimate of the contribution of welfare reform to overall changes in female property crime between 1992 and 2002.

We calculate the elasticity of female property arrest with respect to employment using our estimated effects of welfare reform on female property crime in conjunction with previous estimates of the effects of welfare reform on female employment. As indicated earlier, we found in other work that welfare reform increased employment among at-risk women by about 7–8 percentage points (Dave, Corman and Reichman 2012). Given a baseline employment-to-population ratio of 59% among less-than-college educated single mothers based on the 1992 March CPS, this translates to a 12–14% increase in employment assuming a stable population base. From the current study, the reduced form effect of welfare reform on property crime is a 4.4–4.8% decline among all women ages 21–49. However, not all women who are arrested are impacted by welfare policy. As noted earlier, estimates from the 2010 NSDUH suggest that about 40% of women who are ever arrested currently receive some form of public assistance. Thus, we scaled our estimate of the effect of welfare reform on property crime among at-risk women to be 11–12%. Taking the ratio of these reduced-form effects, we derive the structural effect of employment on property crime of at-risk women. Specifically, dividing the percent change in property crime by the percent change in employment, we obtain an estimate of the elasticity to range from approximately -0.8 to -1.

Our estimated elasticity is not directly comparable to elasticity estimates from the previous literature, but worth a comparison. Raphael and Winter-Ebmer (2001) found that a one percentage point increase in the unemployment rate increases property crime by 1.6–2.4%,

Gould et al. (2002) found a similar sized effect of 1–2%, and more recently Lin (2008) found a much stronger effect—4% increase in property crime associated with a one percentage point increase in unemployment. In all cases, converting a one percentage point increase in unemployment to a percent change in unemployment would result in elasticities well below our estimated range. However, there are a number of reasons why we do not expect our estimates to be fully comparable to those from the other studies. First, we consider arrests rather than crime rates. Second, we focus on women, where other studies have looked at overall crime, most of which is committed by men. Third, we are considering employment, not unemployment which conflates changes due to job loss/gain with changes due to labor force participation. Fourth, because we study a broad-based and successful policy initiative with strong built-in work incentives, we observed much larger changes in employment than the previous studies. Finally, the other studies have been challenged in addressing issues of endogeneity, relying on identifiers plus state- and time-trend variables in attempts to isolate causal effects. Ours is the first and only study to use a “natural experiment” approach to more directly investigate the effects of an exogenous shock in employment on crime. Despite all of the differences between our study and the others, we arrive at the same conclusion that crime is quite responsive to changes in employment.

Next, we project how many property arrests and crimes were prevented as a result of the work incentives underlying welfare reform. In 1992, the first year of welfare reform, 4.61 million less-than-college-educated single mothers were employed (according to CPS data) and 376,333 property crime arrests of women ages 21–49 took place (according to FBI data). Given that welfare reform increased employment among these women by about 12–14% (599,300 additional women employed), and given that welfare reform led to a 4.7% decrease in property

crime arrests (17,688 fewer arrests), we can estimate the change in employment that would lead to one less arrest. Specifically, we find that for each 34 additional at-risk women employed, one property-related arrest appears to have been averted.<sup>9</sup> We use this estimate to project how many actual crimes were prevented. According to U.S. Department of Justice (2003)<sup>10</sup>, approximately 11% of reported crimes result in arrests, and according to the U.S. Department of Justice (2004)<sup>11</sup>, about 40% of victims of property crime report their crime to police. Thus, for each property arrest, about 9 property crimes are reported, and for each 9 crimes reported, about 23 crimes actually occurred. Overall, we estimate that for each at-risk woman who became employed as a result of welfare reform, about 0.03 property crime arrests and 0.7 actual property crime (reported or unreported) were averted.

Finally, we derive a ballpark estimate of the potential impact of welfare reform on the change in female property crime between 1992 and 2002. From our data, we found that property crime arrests of women declined by about 30% during this period. Our estimates in Table 1 suggest that welfare reform is associated with about 4.7 percentage points of this decline, and thus can explain about 15–16% of the observed decline in female property crime arrests over this period.

## **Conclusion**

As far as we know, this is the first study of employment or work incentives on female crime in several decades as well as the first study of the effects of the welfare reform in the 1990s on violent crime, property crime, other serious crime, and/or minor offenses among

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<sup>9</sup> Thus, the marginal effect of employment on property crime arrest is 0.03 (which is 1 divided by 34); this compares to the average probability of a property crime arrest relative to employed at-risk women of 0.08 (376,333/4,610,000).

<sup>10</sup> Source: U.S. Department of Justice, Federal Bureau of Investigation, Crime in the United States, 2002 (Washington, DC: USGPO, 2003)

<sup>11</sup> Source: U.S. Department of Justice, Bureau of Justice Statistics, Criminal Victimization in the United States, 2002 Statistical Tables, NCJ 200561, Table 91 [Online]. Available: <http://www.ojp.usdoj.gov/bjs/pub/pdf/cvus02.pdf> [Mar. 3, 2004].

females in the U.S. We found consistent and robust evidence that welfare reform led to a decreased in female arrests for serious property offenses by 4.4–4.9%, but had no significant impact on violent offenses, other serious crimes, or minor offenses. The negative effects of welfare reform on property crime appeared to be stronger in states with lower welfare benefits and higher earnings disregards, as economic theory would predict, and in states with larger caseload declines, suggesting that welfare reform led women to substitute legal work for illegal income-generating activities. These patterns, along with the null results for non-property crimes, suggest that potential countervailing effects of not being able to make ends meet under the new regime and increases in stress are not driving associations between welfare reform and property crime and have not resulting in increases in violent crime.

Extrapolating from our results, we calculated an elasticity of women's property crime with respect to employment of  $-0.8$  to  $-1.0$ , suggesting that getting women at risk for welfare into the labor force has a strong negative effect on their arrests, and by inference, their participation in property crime. We estimate that for each at-risk woman who became employed as a result of welfare reform, about 0.03 property crime arrests and 0.7 actual property crimes (reported or unreported) were averted, and that welfare reform can potentially explain about 15–16% of the observed decline in female property crime arrests between 1992 and 2002.

The findings from this study are important for understanding the role of employment as a determinant of female crime and for ascertaining the full effects of a major policy shift that is still playing out to this day. We offer the caveat that we have estimated average effects that coincided, for the most part, with a strong economy. Schoeni and Blank (2000), for example, show that the gains from welfare reform were not uniformly distributed across all at-risk women. Thus, the overall effects could mask considerable heterogeneity within the population of low-

educated women and might look very different during periods of economic recession. Future research is needed to replicate and further explore the findings from this study, particularly whether the results can be generalized to male crime, which has to our knowledge not been studied using a natural employment-related experiment as we have been able to do for female crime.

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**Table 1**  
**Welfare Reform and Property Crime**  
**FBI Arrests**  
**Females, Ages 21–49, 1992–2002**

Outcome	Ln Arrests – Property “Index” Criminal Offenses				Ln Arrests – Larceny / Theft	
Specification	(1)	(2)	(3)	(4)	(5)	(6)
AFDC Waiver	-0.02290 (0.01662)	-0.04782*** (0.01644)	-0.04904*** (0.01609)		-0.04744*** (0.01595)	
TANF	-0.02528 (0.02687)	-0.04660* (0.02428)	-0.04407* (0.02388)		-0.04630* (0.02342)	
Any Welfare Reform				-0.04792*** (0.01498)		-0.04719*** (0.01481)
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Linear Trends	No	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions & Welfare Caseloads	No	No	Yes	Yes	Yes	Yes
Adjusted R-squared	0.97750	0.98000	0.98002	0.98002	0.97809	0.97809
Observations	5656	5656	5656	5656	5656	5656

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. All models control for indicators for state, month, and year, in addition to the state unemployment rate, state real per capita personal income, log of total state population, log of the agency population for months with arrest reports, log of the state female population, log state criminal justice expenditures, log state substance abuse prevention and treatment block grant, state minimum wage, and state poverty rate. Measures of male arrests include the log of the male arrests for all criminal offenses and the log of male arrests for outcome-specific offenses. Lagged covariates include one-year lags of the state unemployment rate and real personal income per capita, and one- and two-year lags of the state welfare caseloads. Sample is limited to agencies with a reported coverage of at least 50%.

**Table 2**  
**Welfare Reform and Other Crime**  
**FBI Arrests**  
**Females, Ages 21–49, 1992–2002**

Outcome	Ln Arrests – Violent “Index” Criminal Offenses		Ln Arrests – Other Serious Criminal Offenses		Ln Arrests – Other Minor Criminal Offenses	
	(1)	(2)	(3)	(4)	(5)	(6)
AFDC Waiver	0.00226 (0.03493)		-0.00370 (0.01595)		-0.01107 (0.01589)	
TANF	-0.02859 (0.04222)		-0.01133 (0.02403)		0.01544 (0.02068)	
Any Welfare Reform		-0.00467 (0.03313)		-0.00539 (0.01592)		-0.00512 (0.01584)
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions & Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.96694	0.96694	0.98913	0.98913	0.99098	0.99098
Observations	5644	5644	5668	5668	5656	5656

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. All models control for indicators for state, month, and year, in addition to the state unemployment rate, state real per capita personal income, log of total state population, log of the agency population for months with arrest reports, log of the state female population, log state criminal justice expenditures, log state substance abuse prevention and treatment block grant, state minimum wage, and state poverty rate. Measures of male arrests include the log of the male arrests for all criminal offenses the log of male arrests for outcome-specific offenses. Lagged covariates include one-year lags of the state unemployment rate and real personal income per capita, and one- and two-year lags of the state welfare caseloads. Sample is limited to agencies with a reported coverage of at least 50%.

**Table 3**  
**Welfare Reform and Property Crime**  
**Lagged Effects**  
**Females, Ages 21–49, 1992–2002**

Outcome Lagged Welfare Reform Indicator	Ln Arrests – Property “Index” Criminal Offenses				Ln Arrests – Larceny / Theft			
	1-month		6-month		1-month		6-month	
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AFDC Waiver	-0.04804*** (0.01536)		-0.02594 (0.01600)		-0.04616*** (0.01529)		-0.02803* (0.01548)	
TANF	-0.05026** (0.02327)		-0.03338 (0.02841)		-0.05284** (0.02268)		-0.04951* (0.02627)	
Any Welfare Reform		-0.04853*** (0.01496)		-0.02742* (0.01651)		-0.04763*** (0.01497)		-0.03229** (0.01598)
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions & Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.98002	0.98003	0.97809	0.97809	0.97999	0.97999	0.97807	0.97807
Observations	5656	5656	5656	5656	5656	5656	5656	5656

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. All models control for indicators for state, month, and year, in addition to the state unemployment rate, state real per capita personal income, log of total state population, log of the agency population for months with arrest reports, log of the state female population, log state criminal justice expenditures, log state substance abuse prevention and treatment block grant, state minimum wage, and state poverty rate. Measures of male arrests include the log of the male arrests for all criminal offenses and the log of male arrests for outcome-specific offenses. Lagged covariates include one-year lags of the state unemployment rate and real personal income per capita, and one- and two-year lags of the state welfare caseloads. Sample is limited to agencies with a reported coverage of at least 50%.

**Table 4**  
**Welfare Reform and Property Crime**  
**FBI Arrests**  
**Males, Ages 21–49, 1992–2002**

Outcome	Ln Arrests – Property “Index” Criminal Offenses				Ln Arrests – Larceny / Theft			
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AFDC Waiver	0.01208 (0.02044)		0.00051 (0.03134)		0.01845 (0.02037)		0.00754 (0.03207)	
TANF	-0.00081 (0.01858)		-0.01799 (0.02793)		0.00779 (0.01802)		-0.00982 (0.02588)	
Any Welfare Reform		0.00919 (0.01817)		-0.00364 (0.02860)		0.01606 (0.01804)		0.00365 (0.02877)
Measures of Female Arrests	Yes	Yes	No	No	Yes	Yes	No	No
State-specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions & Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.98849	0.98849	0.98384	0.98384	0.98581	0.98581	0.98032	0.98032
Observations	5656	5656	5656	5656	5656	5656	5656	5656

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. All models control for indicators for state, month, and year, in addition to the state unemployment rate, state real per capita personal income, log of total state population, log of the agency population for months with arrest reports, log of the state female population, log state criminal justice expenditures, log state substance abuse prevention and treatment block grant, state minimum wage, and state poverty rate. Measures of female arrests include the log of the female arrests for all criminal offenses and the log of female arrests for outcome-specific offenses. Lagged covariates include one-year lags of the state unemployment rate and real personal income per capita, and one- and two-year lags of the state welfare caseloads. Sample is limited to agencies with a reported coverage of at least 50%.

**Table 5**  
**Welfare Reform and Property Crime: Differential Effects by State Policy and State Change in Caseloads**  
**FBI Arrests**  
**Females, Ages 21–49, 1992–2002**

Outcome Specification	(1)	(2)	(3)
Any Welfare Reform	-0.04562** (0.01778)	-0.03478* (0.02103)	-0.02569 (0.02345)
Welfare Reform*Low Benefits Generosity	-0.01235 (0.04785)		
Welfare Reform*High Earnings Disregard		-0.03687 (0.03395)	
Welfare Reform*Caseload Change			-0.05937 (0.03803)
Measures of Male Arrests	Yes	Yes	Yes
State-specific Linear Trends	Yes	Yes	Yes
Adjusted R-squared	0.98000	0.98001	0.98002
Observations	5656	5656	5656

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. All models control for indicators for state, month, and year, in addition to the state unemployment rate, state real per capita personal income, log of total state population, log of the agency population for months with arrest reports, log of the state female population, log state criminal justice expenditures, log state substance abuse prevention and treatment block grant, state minimum wage, and state poverty rate. Measures of male arrests include the log of the male arrests for all criminal offenses and the log of male arrests for outcome-specific offenses. Sample is limited to agencies with a reported coverage of at least 50%. Low Benefit Generosity is a dichotomous indicator for whether the state is classified as having low benefits generosity. High Earnings Disregard is a dichotomous indicator for whether the state is classified as having high earnings disregard. These classifications are based on Blank and Schmidt (2001). Caseload Change represents the percentage reduction in welfare caseloads in a given state, over 1992–2002, driven by welfare reform. This is predicted from a first-stage model (see Equation 5 in text) relating the log of welfare caseloads to an indicator for any welfare reform, the state unemployment rate and real personal income per capital, and a trend term, estimated separately for each state.



Appendix: Crime categories and codes in Federal Bureau of Investigation (FBI) crime reports

**Serious Criminal Offenses**

<b>UCR Code</b>	
<b>Violent “Index” Crimes</b>	
01	Murder and Non-Negligent Manslaughter, Manslaughter by Negligence
02	Forcible Rape
03	Robbery
04	Aggravated Assault
<b>Property “Index” Crimes</b>	
05	Burglary - Breaking or Entering
06	Larceny - Theft (except motor vehicle)
07	Motor Vehicle Theft
<b>Other Serious Criminal Offenses</b>	
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**

<b>UCR Code</b>	
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