International Review of Law and Economics 40 (2014) 1–14

Contents lists available at ScienceDirect

International Review of Law and Economics

Effects of welfare reform on women's crime

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ARTICLE INFO

Article history:
Received 2 February 2014
Received in revised form 30 May 2014
Accepted 23 June 2014
Available online 9 July 2014

Keywords:
Women's crime
Employment and crime
Property crime
Violent crime
Welfare reform and crime

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 base the analyses on the supply of crime model in the human capital literature which emphasizes the importance of employment prospects in the legal and illegal labor markets. We find suggestive evidence that welfare reform decreased female arrests for property crimes (by 4–5%), but that it did not affect arrests for other types of crimes. The effects appear to be stronger in states with larger welfare caseload declines. As welfare reform policies are targeted to females, it is empirically validating that we do not find any substantively or statistically significant effects of welfare reform on crime among males. The findings point to broad-based work incentives—and, by inference, employment—as an important determinant of female property crime.

1. Introduction

Although crime is predominantly a male activity and the propensity to engage in crime is much higher for males than for females with similar characteristics, females account for a nontrivial proportion of arrests in the U.S. In 2011, females accounted for over 37% of arrests for serious felony property crimes (burglary, larceny/theft, motor vehicle theft and arson) and almost one fifth of arrests for the violent crimes of murder, manslaughter, rape, and felonious assault (U.S. Department of Justice, 2012). A large literature in economics, based on pioneering work by Becker (1968) and Ehrlich (1973), has investigated determinants of crime. Most studies have focused on reported crime rates, which do not provide information on the gender of the person committing the crimes. Therefore, because most crimes are committed by males, the findings from this literature do not necessarily apply to females.

Many studies in the existing literature have investigated the effects of aggregate employment variables on crime rates. Recent examples are studies by Raphael and Winter-Ebmer (2001), Gould et al. (2002), Corman and Mocan (2005), Edmark (2005), Ihlanfeldt (2006), and Lin (2008). All of these studies found that poor employment prospects (such as high unemployment rates) are positively associated with property crime rates, and—with the exception of the study by Ihlanfeldt, which focused on male youth in one city—positively, but neither strongly nor consistently, related to violent crime rates. Overall, this literature based primarily on males, suggests that the macro-level employment context could be an important determinant of female crime.

Sweeping policy changes in the 1990s dramatically altered work incentives for poor women in the U.S. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, often referred to as "welfare reform," ended entitlement to welfare benefits under Aid to Families with Dependent Children (AFDC) and replaced 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. 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).

In a recent comprehensive review, Bushway (2011) concluded that although there is a fair amount of evidence that crime is related to employment-related factors, the large-scale policy shift of welfare reform could potentially be leveraged to clarify the connections. Another advantage to exploiting the welfare reform legislation in this way—not discussed by Bushway—is that it would

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http://dx.doi.org/10.1016/j.irle.2014.06.003
0144-8188/

add to the almost non-existent literature on employment and crime among women.

Very few population-based studies have specifically focused on determinants of female crime, with the most noteworthy analyses having been conducted over 25 years ago. Bartel (1979) examined female arrests using a cross-section of states in 1970. She found that deterrence variables were associated with arrest rates in the expected direction, but that marriage and labor force participation rates, particularly of single women, were positively associated with arrest rates for property crime. Phillips and Votey (1987) found that unemployment rates appeared to be important 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). These sweeping changes, along with the availability of new data and modern econometric techniques, call for a re-examination of the determinants of female crime.

In this paper, we exploit welfare reform of the 1990s—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 on female crime. 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 and investigate the extent to which the effects are stronger in states with larger caseload declines, as economic theory would predict. We conduct a number of specification and robustness checks. This study makes an important contribution to the virtually non-existent literature on female crime by exploring the role of broad-based work incentives, and by inference employment, in a contemporary context. It also adds to the economics of crime literature by exploiting a human capital-related “natural experiment.” Finally, it provides important information about potential secondary effects of an important large-scale policy shift in the U.S.

2. Background

Although welfare reform is often dated to the 1996 PRWORA legislation, there were two distinct phases of implementation. The first phase started in the early 1990s when the Clinton Administration greatly expanded the use and scope of “welfare waivers,” which allowed states to implement experimental changes to their AFDC programs. Features of the various waivers, many of which were implemented statewide, were increases in the amount of earnings that recipients were allowed to keep while maintaining welfare eligibility (earnings disregards), work requirements as a condition for receiving cash assistance, time limits for the receipt of cash assistance, increased sanctions to recipients who failed to comply with work requirements or other program rules, and/or elimination of increases in benefits to families who had additional children while on welfare. The second phase of welfare reform was ushered in with the 1996 PRWORA legislation, which replaced the AFDC program with TANF block grants to states and imposed a focused national “work first” regime with features including work requirements as a condition of receiving welfare, earnings disregards, stricter sanctions for non-compliance with program rules, a two-year maximum length of a “welfare spell,” and notably, a lifetime limit of five years of welfare benefits over a person’s lifetime. These “carrots and sticks” provide incentives for women at risk for relying on public assistance (not just welfare recipients) to secure private sector employment, as these policies reduce the benefits of welfare reliance compared to work and limit the practical option of long-term reliance on public assistance.

By most accounts, welfare reform has been deemed a great success. Bell (2001) estimated that PRWORA reduced welfare caseloads by between 19 and 35% (Bell, 2001), and Dave et al. (2012) found, using the 1992–2002 Current Population Survey in a difference-in-differences framework, that welfare reform overall raised the employment-to-population ratio among at-risk women aged 21–49 years by about 7–8 percentage points. The latter result is similar to estimates by other researchers (e.g., McKernan et al., 2000; Schoeni and Blank, 2000). Schoeni and Blank (2002) also found welfare reform led to increases in income through legal employment in the early years of welfare reform, and Meyer and Sullivan (2004) found that the consumption of most single mothers increased between 1996 and 2000, as PRWORA unfolded. The authors’ arguments for focusing on consumption rather than income were that consumption is the more direct measure of economic well-being and that there is significant underreporting of income among transfer recipients (particularly in-kind benefits).

2.1. Effects of welfare on crime

There have been few population-based studies on the effects of welfare policies or participation 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 (using a self-reported measure of 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. Fishback et al. (2010), studying a panel of 83 cities between 1930 and 1940, found that New Deal relief decreased property crime during the Great Depression. Zhang (1997) examined states in 1987 and found a negative association between state AFDC generosity and property crime. Hannon and Defronzo (1998) studied urban counties in 1990 and found that welfare generosity was negatively related to both violent and property crimes. 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 specifically examined effects of welfare reform on crime. Monte and Lewis (2011) examined a cohort of about 1400 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. They also found 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 DD was an appropriate and useful methodology for investigating the policy effects of welfare reform on crime.

3. Theoretical framework

Bartel (1979) applied the human capital model formulated by Becker (1968) and extended by Ehrlich (1973) to study determinants of women's criminal behavior. In this model, individuals maximize the following expected utility function:

$$E(U) = (1-p)U(X_1, t_c) + pU(X_2, t_c) \tag{1}$$

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:

$$X_1 = W_i(t_i) + W_l(t_l) + W_o \tag{2}$$

$$X_2 = W_i(t_i) + W_l(t_l) - F_i(t_i) + W_o \tag{3}$$

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. 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 possibly 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. However, legal work would reduce the time available for illegal work; for example, Jacob and Lefgren (2003) found that juvenile crime is lower on days when school is in session and that the effects appear to operate through an "incapacitation effect." It is also possible that the demands of work requirements increase stress which could lead to anti-social behavior including violent crime (e.g., if some individuals relieve their stress through physical aggression); in our model, this would reflect a taste shift toward aggression.

Given the large reductions in welfare caseloads and increases in employment attributed to welfare reform, discussed earlier, and that legal and illegal work are, to some extent, substitutes according to this model, we expect that welfare reform 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. First, the relative returns to legal versus illegal work are likely to be different under the "work first" regime. Work requirements could result in increased legal wages (as a result of more work experience), which would increase the gains from legal work versus illegal income-generating activities. If women expect to rely on welfare less and to work more, the mark of a criminal record would be more consequential (in terms of worse employment opportunities and lower wages). Additionally, working in the legal sector 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, it is possible that women who find themselves unable to support their families through legal work and welfare payments or have difficulty complying with program rules under the new regime turn to illegal work to make ends meet.

If welfare reform led to increases in income through legal employment, as findings by Schoeni and Blank (2000) and Meyer and Sullivan (2004) suggest was the case, 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.

4. 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; 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.

4.1. 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 sex of the person committing the crime is not available for the former, so for our analyses of the effects of welfare reform on female crime we rely on the latter. Specifically, we use the monthly data on arrests by sex, age, and type of offense from the FBI crime reports to construct month/year/state measures of arrests.[1] Although individual level surveys that ask about crime

[1] State prison admission data from the National Corrections Reporting System (NCRP) are also available by sex, age, and type of crime. However, a major limitation of the NCRP data for our purposes is that many individuals who are arrested for felonies end up being convicted of misdemeanors and never serve in a prison facility (because of plea bargaining). For example, in 2002, although there were about 1.6 million arrests for serious property crimes in the U.S., fewer than 140,000 individuals were sent to state prison facilities for such crimes that year (authors' calculations using NCRP and FBI arrest data).

Table 1
Annual arrest statistics: FBI arrests.

Age range (years)	(1) Arrest rate per 100,000 population 1992–2002		(2) U.S. Arrest Rate per 100,000, 1996
	21–49 female	21–49 male	All males and females
Property index	516.911	1304.910	793.2
Larceny/theft	449.917	883.867	577.3
Violent index	164.459	837.587	288.6
Other serious crimes	1207.411	4191.577	1685.5
Other serious crimes (other than drugs)	790.725	2446.678	1091.3
Minor offenses	1491.863	7163.218	3073.5

(1) Arrest rates were calculated as total number of arrests for each crime category summed over months, years, and reporting agencies in the U.S. These arrests represent agencies with a size of 50,000 or greater who reported arrests for each month/year. The denominator is the total population of the reporting agencies multiplied by the percent of the US population which is female 21–49 and male 21–49 in each year.
(2) U.S. arrest rates (column 4) were obtained from U.S. Department of Justice (1997).

commission include more detailed characteristics of perpetrators and may include crimes not reported to the police or that did not result in arrests, crime is 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 et al., 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 limit 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 month/year/state level. Even among large criminal justice agencies, not all agencies report in all months. For example, in 1996, of the total 147 million people in the U.S. residing under the jurisdiction of agencies of 50,000 people or more, about 106 million people (about 72%) were covered by agencies that reported arrests to the FBI in all 12 months.[2] 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 differences in the size of the underlying population that is represented or covered by the FBI reports.[3]

We consider different types of crimes, using the classifications provided by the FBI (see Appendix A1 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/theft (except motor vehicle), 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.

Table 1 presents annualized arrest rates for each of the 6 crime categories of interest. For each rate, the numerator is the total number of arrests for each sex/age/crime category/year in reporting agencies of 50,000 population or greater, and the denominator is the total population of reporting agencies (>50,000) times the proportional representation of that population in the entire U.S. for that age/sex/year.[4] Our arrest rates for males are about twice those for the U.S. overall in 1996 (mid-sample) (second set of columns), which is to be expected because: (1) Men 21–49 years old represent a high risk group compared to males of all ages. (2) Males have higher crime rates than females. (3) Our data cover only urban areas (population at least 50,000) which have higher crime rates than non-urban areas.

4.2. Characterizing welfare reform

As discussed earlier, 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 and 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 2 presents the implementation dates for both AFDC waivers and TANF for all states in the U.S. [5] 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 most models, we use separate measures for AFDC waiver and TANF implementation. For waivers, we consider whether, in a given

[2] 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). In robustness checks, discussed later, we explore alternative cut-off percentages.

[3] The (monthly) population data for each reporting agency are provided in the FBI data set. There is no double-counting in either the arrest or population data. A given arrest occurs only in one agency. For each arrest, only the most serious offense is reported. Population figures are not included for agencies such as State Highway Patrol, since the same population was included for a specific law enforcement agency.

[4] State-level population data by age group/sex/year were obtained from the Census as follows: https://www.census.gov/popest/data/intercensal/st-co/characteristics.html and https://www.census.gov/popest/data/intercensal/state/ST-EST00INT-02.html.

[5] Data on timing of state implementation of major AFDC waivers and TANF were obtained from USDHHS (1999).

Table 2
Implementation dates of welfare reform by state, U.S.

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

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

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 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 alternative specifications, we include a single indicator for any welfare reform (AFDC waiver or TANF).[6]

5. Method

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

$$\text{Ln}\,A_{smt} = \alpha + Welfare_{smt}\,\Pi + Z_{st}\,\beta + State_s\,\Omega + Month_m\,\Phi + Year_t\,\Psi + \varepsilon_{smt} \quad (4)$$

Specifically, we estimate semi-log models relating the natural log of the arrest rate, measured as the number of arrests per 1000 females, to a vector of covariates.[7]

In Eq. (4), the female arrest rate (*Ln A*)—measured for specific offense categories, in state *s*, during month *m* and year *t*—is 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). The arrest rate is 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 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. 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, month fixed effects (*Month*), which account for any seasonal factors affecting criminal behavior, and year (*Year*) fixed effects, which account for national trends in criminal activity and the arrest rate.

Ideally, we would estimate models of the effects of welfare reform on the female arrest rate 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 sex 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 aged 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 (DDD) framework because

[6] 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.

[7] The log transformation adjusts for the skewness of the distribution of state-level female arrest rates and also facilitates comparison of the estimates across models and outcomes in terms of relative percent change in the arrest rate.

a valid and suitable comparison group to female arrestees is not available. As indicated above, there is limited 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, in the absence of welfare reform, for every one-percentage point change in arrests among men there would be an equal change in arrests among women—an assumption that is not supported by the time trends and (later) explicitly rejected in all of our models. We do use males to conduct "placebo tests." That is, when we find significant effects of welfare reform on crime for females, we run equivalent models for males. We would expect welfare reform to have much weaker impacts on criminal behavior for males than for females because the policies most directly targeted females. Finding that this is the case would lend validity to our results for females.

A key methodological challenge lies in disentangling the effects of welfare reform from those of other time-variant factors that may be related to female arrests. Whole volumes edited by Blumstein and Wallman (2006) and Goldberger and Rosenfeld (2008) and written by Zimring (2007) explored why crime rates dramatically, unexpectedly, and systematically plunged in all areas of the U.S. in the 1990s, right as welfare reform unfolded. 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.

To the extent that the decline in crime in the 1990s was uniform across the nation, the common trend would be captured by the time fixed effects. However, the concern is that while overall crime rates were declining in the 1990s, the rates of decline varied across states owing to differential shifts in important trends such as economic conditions, enforcement, and other state-specific changes in the costs and benefits of engaging in criminal activity.

We address this specter of confounding state-specific trends in a number of ways. First, all specifications explicitly control for a large vector of concurrent state-specific time-varying factors (Z) that may impact the female arrest rate, including measures of the state's economy (real personal income per capita, poverty rate), labor market conditions (unemployment rate, minimum wage), relevant population base (total state population; covered population of reporting FBI agencies) and criminal justice system (criminal justice expenditures, state block grants on substance abuse prevention and treatment). We further include both the natural log of the total arrest rate and the natural log of the arrest rate 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.[8] This strategy follows Dave et al. (2011), who employed controls for trends in male insurance coverage when estimating insurance rates among pregnant women. In alternate specifications, we also add state-specific linear trends to further account for any other systematically-varying state-level factors that may have coincided with welfare reform.

Another important methodological challenge is the potential endogeneity of the timing of welfare reform. 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. 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. The specifications with the state-specific time trends also help to address the possibility that policy implementation may be otherwise endogenous to the state's history and thus alleviate this concern. We also explore the extent to which the implementation of welfare reform was associated with the state's pre-reform history by estimating models predicting whether and when a state implemented welfare reform, as a function of either the lagged level of female crime (total arrests or total arrests for property crime) or prior annual trends in female crime in the state, as well as by estimating lead effects in variants of our main analyses. Finding such associations and significant lead effects would weaken our inferences about the effects of welfare reform on female crime.

When we find credible reduced-form effects of welfare reform on crime, we conduct 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 welfare reform on the female arrest rate by estimating models with one, 12- and 24-month lags of the welfare reform measures. Second, as mentioned earlier, we assess the plausibility of our estimates by conducting placebo tests using corresponding male arrests as the outcome. Given that welfare reform policies mostly impact women, we do not a priori expect large or significant effects on male crime.[9] Any finding to the contrary would indicate that any observed effects of welfare reform on female crime are likely confounded by unobserved state-specific trends.

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 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.

6. Results

As discussed earlier, welfare reform has stronger conceptual and empirical links to property crime than to other types of crime. Table 3 presents estimates from models corresponding to Equation (4) for property crime—for all index property crimes (Codes 5–7 in Appendix A1) 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 the female arrest rate, controlling for the relevant monthly male arrest rates; state, month, and year fixed effects; the annual state unemployment rate; state

[8] Note that there is no restriction in this case that the unobserved factors affect both male and female crime identically. This is the assumption that is typically made in a DD framework if males were used as a direct control group. Instead, by controlling for male arrest rates, we are able to flexibly control for all common unmeasured factors that may be affecting male and female crime (even if differentially) as long as the effect is proportional.

[9] This assumes that there is no spillover to the market for male criminal activity. However, even if there are such spillovers (for instance, if male crime responded to shifts in household income or household time constraints), these second-order effects are likely to be much smaller than the first-order effects on female crime. Hence, we expect weak or null effects of welfare reform policies on male criminal activity.

Table 3
Welfare reform and property crime, FBI arrests of females ages 21–49, 1992–2002.

Outcome	Ln arrest rate – property “index” criminal offenses				Ln arrest rate – larceny/theft	
Specification	(1)	(2)	(3)	(4)	(5)	(6)
AFDC waiver	−0.02477	−0.04848***	−0.04962***		−0.04819***	
	(0.01630)	(0.01638)	(0.01615)		(0.01596)	
TANF	−0.02941	−0.04780*	−0.04497*		−0.04767*	
	(0.02587)	(0.02502)	(0.02453)		(0.02392)	
Any welfare reform				−0.04858***		−0.04808***
				(0.01521)		(0.01496)
Measures of male arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	No	Yes	Yes	Yes	Yes	Yes
Lagged economic conditions and welfare caseloads	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.93824	0.94514	0.94519	0.94520	0.94050	0.94051
Observations	5656	5656	5656	5656	5656	5656

Note: 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 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 arrest rate for all criminal offenses and the log of male arrest rate 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%.

* $0.05 < p \le 0.1$.
** $0.01 < p \le 0.05$.
*** $p \le 0.01$.

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. 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. Thus, Specification 4 is a variant of our preferred specification.

The estimates from Specification 1 suggest that AFDC waivers and TANF reduced the arrest rate for serious property offenses by 2.5 and 2.9%, 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 unobserved factors, 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 arrest rate for serious property crime by 4.5–5.0%. 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 the property crime arrest rate when using a single measure of welfare reform that combined AFDC waivers and TANF (Specification 4) was very similar to the estimated effect from the corresponding model that included separate indicators for AFDC waivers and TANF (Specification 3).[10] The estimated effects of welfare reform on the arrest rate 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 3 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. Pre-TANF data from the Survey of Income and Program Participation (SIPP) indicate that 44% of female-headed families and over 60% of poor families participated in a means-tested public assistance program in any given month.[11] Lifetime participation rates are higher. Interpreting these prevalence rates (40–60%) as a conservative proxy for the fraction of women arrestees who are at lifetime risk of welfare receipt, the ITT effect can be scaled up by a factor of about 2 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 a 9–10% decrease in property crime among those female arrestees who are directly affected by the policy. Implicit TOT effects rescaled in this manner should be interpreted with caution since small changes in the denominator (in this case, the fraction of the sample that is at risk of welfare receipt) and the underlying estimates can lead to relatively large differences. Nevertheless, the point stands that the impact of welfare reform among those who are actually at-risk of current or future welfare receipt is likely somewhat larger than the estimated intent-to-treat effects. 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 4 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 quite low and estimates never approach statistical

[10] We cannot reject the null hypothesis of statistically similar effects of AFDC waivers and TANF on property crime.

[11] See http://www.census.gov/sipp/p70s/p70-77.pdf (Appendix A1).

Table 4
Welfare reform and other crime, FBI arrests of females ages 21–49, 1992–2002.

Outcome	Ln arrest rate – violent "index" criminal offenses		Ln arrest rate – other serious criminal offenses		Ln arrest rate – other minor criminal offenses	
Specification	(1)	(2)	(3)	(4)	(5)	(6)
AFDC waiver	0.00240 (0.03486)		−0.00448 (0.01583)		−0.01130 (0.01604)	
TANF	−0.02742 (0.04191)		−0.01288 (0.02411)		0.01566 (0.02077)	
Any welfare reform		−0.00430 (0.03304)		−0.00635 (0.01587)		−0.00523 (0.01600)
Measures of male arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Lagged economic conditions and welfare caseloads	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.89933	0.89933	0.96811	0.96811	0.97558	0.97557
Observations	5644	5644	5668	5668	5656	5656

Note: 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 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 arrest rate for all criminal offenses and the log of male arrest rate 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%.

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 3 and 4, the estimated effects of the covariates (shown for Model 3, a typical—and preferred—specification in Appendix A2) are generally consistent with the literature. Criminal activity tends to be countercyclical—positively associated with the state unemployment rate. 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 some of these other covariates are not statistically significant partly due to the inclusion of the state fixed effects and the state-specific trends. The coefficients for measures of the male arrest rate 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 further analyses for property crime, the category of arrests for which it appears that welfare reform had an effect. In Table 5, we consider potential lagged effects, and present estimated effects of welfare reform on all index property crimes (Panel A), and for larceny/theft in particular (Panel B), from models corresponding to Specifications 3 and 4 of Table 3 but that include 1-, 12-, and 24-month lags of the welfare reform measures. We find that the estimated effects of welfare reform are quite similar when using 1-month lags as when considering contemporaneous effects in Table 3 and lead to the same inferences (Specifications 1 and 2). Specifications (3) and (4) introduce 12-month lags in welfare reform implementation to the model. The 12-month lagged effects for both phases of welfare reform are negative, suggesting that welfare reform is associated with a relatively concurrent decline in female arrest rates for serious property crime, including larceny and theft, and also appears to have a cumulative effect over the next 12 months.[12] Specifications (5) and (6) further consider longer 24-month lags, which are statistically and economically insignificant. Combining the coefficients of the 1-month and the 12-month lags (Specification 4), we find an overall negative response in female property crime rates over 12 months following implementation of welfare reform, on the order of about 7%. This response is about 40% larger than the contemporaneous effect (5%) identified in Table 3, which is reflective of a stronger cumulative effect of welfare reform likely due to time limits and lags in the timing of the work-related activity requirements in relation to benefit receipt for certain states.[13] We note, however, that even if the time limit for receiving welfare has not been exhausted, the woman is still required to participate in work-related activities while collecting benefits; hence there are potential immediate as well as lagged effects on criminal behavior.

6.1. Dose-response associations

The above models are consistent in their finding that welfare reform is associated with a decrease in property crime among women. If this effect is driven by a substitution of market work for non-market income-generating activities due to the reduction in welfare caseloads induced by welfare reform, then we would expect a dose-response relationship—that is, a larger decline in property crime being realized in those states where there were larger reductions in the caseload. We therefore test for differential effects of welfare reform on property crime based on the percentage reduction in the welfare caseloads realized over the sample period for each state. The methodology and findings, which are presented in Appendix A3, are suggestive of welfare reform leading to a larger drop in female property crime in states with larger welfare-reform-induced declines in welfare caseloads and therefore consistent with our expectations.

[12] The longer-lagged effects for AFDC Waivers are smaller than the one-month lagged effect or even the contemporaneous effects estimated in Table 3. This may partly be mechanical because the average state that had instituted major changes to their AFDC program did so in mid- to late-1994 and all states implemented TANF starting in 1996. Thus, there is not enough time lapse post-AFDC waivers but prior to TANF to be able to identify relatively long lagged effects of the waivers.

[13] The majority of states (36) and D.C. required work participation to be immediate. The remaining 14 states allowed a lag of 1–24 months (Source: Urban Institute's Welfare Rules Database (http://anfdata.urban.org/wrd/WRDCategoryList.cfm; accessed 12/4/12).

Table 5
Welfare reform and property crime, lagged effects for females ages 21–49, 1992–2002.

Specification	Ln arrest rate – property "index" criminal offenses					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
AFDC waiver (lag 1 month)	−0.04905***		−0.04062***		−0.03840**	
	(0.01551)		(0.01513)		(0.01445)	
AFDC waiver (lag 12 month)			−0.02503		−0.03225*	
			(0.01947)		(0.01849)	
AFDC waiver (lag 24 month)					0.01839	
					(0.01985)	
TANF (lag 1 month)	−0.05100**		−0.04233*		−0.04274*	
	(0.02393)		(0.02283)		(0.02237)	
TANF (lag 12 month)			−0.05322*		−0.05918**	
			(0.02826)		(0.02808)	
TANF (lag 24 month)					−0.01429	
					(0.03104)	
Any welfare Reform (lag 1 month)		−0.04949***		−0.03854***		−0.03707***
		(0.01521)		(0.01435)		(0.01381)
Any welfare reform (lag 12 month)				−0.03084		−0.03505*
				(0.01943)		(0.01771)
Any welfare reform (lag 24 month)						0.01157
						(0.01902)
Measures of male arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Lagged economic conditions and welfare caseloads	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.94520	0.94521	0.94525	0.94525	0.94520	0.94519
Observations	5656	5656	5656	5656	5656	5656

Specification	Ln arrest rate – larceny/theft					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B						
AFDC waiver (lag 1 month)	−0.04742***		−0.03865**		−0.03552**	
	(0.01541)		(0.01550)		(0.01468)	
AFDC waiver (lag 12 month)			−0.02617		−0.03605*	
			(0.01983)		(0.01888)	
AFDC waiver (lag 24 month)					0.02525	
					(0.02144)	
TANF (lag 1 month)	−0.05414**		−0.04508**		−0.04526**	
	(0.02317)		(0.02210)		(0.02178)	
TANF (lag 12 month)			−0.05787*		−0.06603**	
			(0.03177)		(0.03234)	
TANF (lag 24 month)					−0.01544	
					(0.03244)	
Any welfare reform (lag 1 month)		−0.04892***		−0.03732**		−0.03518**
		(0.01518)		(0.01481)		(0.01423)
Any welfare reform (lag 12 month)				−0.03269		−0.03875**
				(0.02037)		(0.01912)
Any welfare reform (lag 24 month)						0.01666
						(0.02060)
Measures of male arrests	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Lagged economic conditions and welfare caseloads	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.94051	0.94052	0.94058	0.94057	0.94054	0.94051
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 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 arrest rate for all criminal offenses and the log of male arrest rate 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%.

* $0.05 < p \leq 0.1$.

** $0.01 < p \leq 0.05$.

*** $p \leq 0.01$.

6.2. Exogeneity of variation in welfare reform

The identifying assumption underlying the DD methodology is that crime rates in states that had not yet implemented welfare reform provide a valid counterfactual for states that implemented welfare reform earlier. Another (related) assumption underlying our analyses is that the implementation of policy is not correlated with unobserved state characteristics—i.e., that conditional on observed state characteristics, state and time fixed effects, and state-specific linear trends, the variation in the timing of welfare reform across states is plausibly exogenous.[14] In our main models,

[14] Policy is deemed to be endogenous, and hence "treatment" effects would be biased, if policy implementation in some time period T is correlated with the state-specific error terms at time $t < T$. In this case, the policy measure is "predetermined"

we controlled for a number of lagged covariates related to the state's economy and prior experience with welfare caseloads in our main models. In addition, we conducted a number of specification checks to explore these issues of potential endogeneity.

First, we exploit the timing of implementation to investigate the plausibility of the identifying variation in our welfare reform measures. Table 6 details these specification checks, which essentially amount to testing for lead effects. Models (1) and (2), akin to a within-state event study, separate the timing and effects of welfare reform into three periods for each state—post welfare reform (which represents all periods subsequent to the implementation of welfare reform), 2 or more years pre-reform (which represents all periods two or more years prior to implementation), and the reference category (one year prior to implementation). If changes in crime are caused by changes in welfare policy, as opposed to changes in policy being caused by shifts in crime rates, then negative effects on crime should not occur until after any implementation of policy. It is validating that the coefficients on the *post* indicator are quite similar to those in our main models (welfare reform is associated with an approximate 5% reduction in the female arrest rate for property crime, relative to the year prior to implementation). In contrast, the coefficient on the *pre* indicator is insignificant and close to zero, suggesting that there is no change in the trend in the arrest rate for those states/periods that have yet to implement welfare reform.

In Specifications 3–8, we more directly assess whether the pre-policy trends in crime rates are similar across states that are early versus later adopters of welfare reform. These models inform the validity of using changes in crime rates in states which have not yet implemented welfare reform as a counterfactual for those that have. The sample is limited to states and periods prior to the implementation of welfare reform, and models control only for state and time fixed effects in order to conservatively check for unconditional differences in crime trends prior to policy implementation. Models 3–5 test whether the number of years until welfare reform implementation in a given state is associated with prior crime rates in that state, and Models 6–8 test for differential trends based on a binary indicator for 4 or more years prior to implementation relative to 1–3 years pre-implementation. The magnitudes for these "lead" effects are all close to zero and statistically insignificant, again suggesting that there are no discernible differences in trends in arrests rates related to female property crime (Models 3 and 6), larceny and theft (Model 4 and 7), and total female crime (Models 5 and 8) based on when the state implemented welfare reform.

Supplemental models predicting whether and when a state implemented an AFDC waiver, and alternately any welfare reform, based on either the lagged level of female crime (total arrests or total arrests for property crime) or prior annual trends in female crime in the state, also confirm that policy endogeneity is not a concern (results not shown). For models predicting AFDC waiver implementation, the sample period was by definition restricted to periods prior to TANF implementation. For models predicting any welfare reform implementation (AFDC waiver or TANF), the full sample period was utilized. We employed alternate lag structures ranging from 1 to 24 months. The findings from these analyses suggest that states' decisions with regards to the timing of the implementation of welfare reform were not driven by their prior crime history, either in levels or trends, and are thus exogenous to crime.

and not strictly exogenous. For instance, policy endogeneity may result if a state's implementation of welfare reform in a given period is correlated with past trends in female crime rates or factors correlated with crime.

Table 6
Welfare reform and property crime, lead effects & pre-welfare reform trends for females ages 21–49.

Outcome	Ln arrest rate – property "index" criminal offenses	Ln arrest rate – larceny/theft	Ln arrest rate – property "index" criminal offenses	Ln arrest rate–larceny/theft	Ln arrest rate – all crime	Ln arrest rate – property "index" criminal offenses	Ln arrest rate – larceny/theft	Ln arrest rate – all crime
Specification	1	2	3	4	5	6	7	8
Sample	1990–2002	1990–2002	1990–1996	1990–1996	1990–1996	1990–1996	1990–1996	1990–1996
Welfare reform – post	−0.05496***	−0.05204***						
	(0.01544)	(0.01497)						
Welfare reform – 2 or more years pre	−0.00076	−0.00914						
	(0.01915)	(0.01825)						
Years to welfare reform			0.00460	0.00076	0.03190			
			(0.01403)	(0.01480)	(0.01914)			
Welfare reform – 4 or more years Pre						−0.01409	−0.00188	0.00167
						(0.04230)	(0.04093)	(0.03669)
Measures of male arrests	Yes	Yes	No	No	No	No	No	No
State-specific linear trends	Yes	Yes	No	No	No	No	No	No
Lagged economic conditions and welfare caseloads	Yes	Yes	No	No	No	No	No	No
Adjusted R^2	0.94310	0.93922	0.88291	0.87171	0.92028	0.88288	0.87166	0.92025
Observations	6663	6663	2804	2804	2804	2804	2804	2804

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. Specifications 1–2 also control for 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 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 arrest rate for all criminal offenses and the log of male arrest rate 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%.

* $0.05 < p \le 0.1$.

** $0.01 < p \le 0.05$.

*** $p \le 0.01$.

6.3. Additional specification and robustness checks

We implemented several additional checks to verify that the results in Tables 3 and 4 are robust to alternate specifications and adjustments for sampling issues, and to assess plausibility (results not shown unless indicated otherwise). First, 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 $[\ln((A_{smt}/Population_{st})/(1-(A_{smt}/Population_{st})]$; (3) used non-logged measures of the arrest rate or total arrests as outcomes; (4) controlled for additional unobserved state-specific variation through state-specific quadratic trends; and (5) were much more parsimonious than Specification 2 in Table 3.

Second, we 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 individuals and agencies with a reported coverage of at least 50% in order to minimize measurement error and maximize data consistency. In supplementary analyses, we ascertained that our estimates are insensitive to both cut-offs.

Third, we re-implemented all analyses by aggregating the monthly arrest data to the annual level. While annual aggregation may smooth idiosyncratic variation in the number of criminal agencies reporting each month as well as any state-specific seasonal variation in crime rates, such aggregation also excludes potentially meaningful month-specific variation in the implementation of AFDC waivers and TANF. We found that the direction and magnitude of the crime responses in the models that used annual arrest data were similar to those reported on here based on monthly data.

Fourth, we further explored the finding of no significant effects of welfare reform on female arrests for the category of "other serious non-violent offenses." About 35% of these offenses constitute drug-related offenses, which Corman et al. (2013) found were negatively affected by welfare reform. Thus, it is possible that drug-related arrests offset opposing effects for other types of arrests in this category. We estimated models for other serious non-violent crimes that excluded drug-related offenses and continued to find insignificant effects of welfare reform with similar magnitudes as in the models for other serious non-violent offenses in Table 4.

Fifth, 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. We found that our results were fully robust, in terms of direction, magnitude, and statistical significance, when the sample was limited to the years 1995–2001 (results not shown).[15] In addition, 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).[16] We found that the estimates and conclusions were 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.

Sixth, we conducted placebo tests using the male property arrest rates as the outcome measure. 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 arrests for females are spurious. These results are presented in Table 7. Specifications 1 through 4 present estimates from our preferred models (corresponding to Specifications 3 and 4 in Table 3) for all property arrests and Specifications 5 through 8 present estimates from our preferred models 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. As we expected, welfare reform had uniformly insignificant (and small magnitude) effects on adult male arrests for property crime.

Finally, we explored the sensitivity of our findings to the years studied. Our main analyses were limited to an observation window of 1992–2002, the period which enveloped welfare reform. Using 2000 or 2005 as the endpoint or expanding the sample to 1988 or 1990 did not materially change the results, which were driven by the period of maximum variation in welfare reform, the 1990s.

6.4. Effects in context

In this section, we project the numbers of property arrests and crimes that 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 Current Population Survey 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 that welfare reform led to a 4.9% decrease in property crime arrests (18,440 fewer arrests), assuming a stable population base, we can estimate the change in employment that would lead to one less arrest. Specifically, we find that for every 33 additional at-risk women employed, one property-related arrest appears to have been averted.[17] We use this estimate to project how many actual crimes were prevented. According to U.S. Department of Justice (2003), approximately 11% of reported crimes result in arrests, and according to the U.S. Department of Justice (2004), 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 every 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 0.70 actual property crime (reported or unreported) were averted.

While these imputed estimates help to frame the potential importance of welfare reform overall in affecting women's property

[15] Eleven states had already implemented major waivers to their AFDC programs between 1992 and 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.

[16] The average federal credit in 2001 was approximately $1600 for a family claimant.

[17] Thus, the marginal effect of employment on property crime arrest is 0.03 (which is 1 divided by 33); 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).

Table 7
Welfare reform and property crime, FBI arrests for males ages 21–49, 1992–2002.

Outcome	Ln arrest rate – property "index" criminal offenses				Ln arrest rate – larceny/theft			
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AFDC waiver	0.01297		0.01933		0.00131		0.00828	
	(0.02079)		(0.02078)		(0.03157)		(0.03235)	
TANF	0.00068		0.00928		−0.01670		−0.00872	
	(0.01860)		(0.01845)		(0.02780)		(0.02622)	
Any welfare reform		0.01021		0.01708		−0.00273		0.00447
		(0.01848)		(0.01848)		(0.02878)		(0.02907)
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 and welfare caseloads	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.96821	0.96821	0.96262	0.96262	0.95536	0.95536	0.94813	0.94814
Observations	5656	5656	5656	5656	5656	5656	5656	5656

Note: 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 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 arrest rate for all criminal offenses and the log of the female arrest rate 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%.

crime, they should be interpreted with caution. Welfare reform consisted of several elements, including lifetime limits, term limits, earnings disregards, work requirements as a condition for receiving welfare benefits, sanctions for non-compliance, family caps, and various other components. Different states chose to focus on different subsets of these features in waivers and all states were given substantial latitude under PRWORA in designing their TANF plans (e.g., states were allowed to impose stricter lifetime and term limits than the national guidelines). It is not possible to disentangle the specific contributions of various components of welfare reform. Nevertheless, the general thrust of these policy components is clear–they were designed to reduce dependence on welfare and promote employment. Hence, we interpret the reduced-form effects as being primarily driven by the rise in employment and reduction in welfare caseloads. Unfortunately, with available data it is not possible for us to definitively rule in or out the various hypothesized scenarios, such as income or incapacitation effects.

Finally, we note that our treatment-on-the-treated estimates are based on the margin of women who are likely to be arrested for property crime. It is possible that hardcore active offenders are less deterred by losing welfare benefits or less able to find work due to poor human capital skills, which is consistent with our null findings for felony and violent arrests.

7. Conclusion

As far as we know, this is the first study of the effects of employment context on female crime in several decades as well as the first study of the effects of the welfare reform in the 1990s on crime. We found suggestive but very robust evidence that welfare reform led to a decrease in female arrests for serious property offenses by 4.4–4.9%, but had no significant effects on arrests for violent offenses, other serious crimes, or minor offenses. The negative effects of welfare reform on property crime appeared to be stronger in states with larger caseload declines, suggesting that welfare reform led women to substitute legal work for illegal income-generating activities. Extrapolating from our results, we estimate that for each at-risk woman who became employed as a result of welfare reform, about 0.030 property crime arrests and 0.70 actual property crimes (reported or unreported) were averted.

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. In addition, this study contributes more generally to the human capital and crime literature by exploiting a natural experiment. We offer the caveat that we have estimated average effects that coincided, for the most part, with a strong economy. That is, the overall effects could mask considerable heterogeneity within the population of low-educated women and might look very different during periods of economic recession.

More generally, this study represents an important, broad, and initial investigation of the effects of a very substantial employment-related policy shift on crime. Future research is needed to replicate and further explore the findings, particularly in terms of elucidating the underlying pathways, identifying the specific components of state welfare packages that appear to be salient, and determining the extent that our findings for work incentives for females also apply to males.

Acknowledgments

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 participants at the 5th Transatlantic Workshop on the Economics of Crime, and for valuable research assistance from Tat'ána Čepková, Dhiman Das, Farzana Razack, Victoria Halenda and Oliver Joszt.

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, 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 (other than 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 law violations
23	Drunkenness
24	Disorderly conduct
25	Vagrancy
26	All other offenses (except traffic)
27	Suspicion
28	Curfew and loitering law violations
29	Runaways

Appendix A2. Parameter estimates for Model 3 in Table 3

Outcome: Ln Arrests – Property "Index" criminal offenses for females ages 21–49	
AFDC waiver	−0.04962*** (0.01615)
TANF	−0.04497* (0.02453)
State unemployment rate	0.01753** (0.00856)
Unemployment rate: 1-year lag	0.01473 (0.01020)
State real personal income per capita	0.00002 (0.00001)
Income: 1-year Lag	−0.00001 (0.00001)
Log total state population	1.47572 (1.29052)
Log agency population	0.04482 (0.07729)
Log state criminal justice expenditures	0.00543 (0.12833)
State minimum wage	−0.03274* (0.01633)
State poverty rate	0.00399 (0.00282)
Log state substance abuse prevention and treatment block grant	−0.05008 (0.07358)
State welfare caseloads (1000s): 1-year lag	−0.00003 (0.00004)
State welfare caseloads (1000s): 2-year lag	0.00006 (0.00005)
Log Arrests for Property Index Crimes for Males Ages 21–49	0.46743*** (0.07687)
Log total arrests for males ages 21–49	0.32562*** (0.05535)
State fixed effects	Yes
Month fixed effects	Yes
Year fixed effects	Yes
State-specific linear trends	Yes
Adj-R^2	0.980
Observations	5656

Note. See Table 3.
* $0.05 < p \le 0.1$.
** $0.01 < p \le 0.05$.
*** $p \le 0.01$.

Appendix A3. Testing for dose–response associations for property crime

First, we estimated 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.

$$Ln\,Caseload_t = \delta + \Lambda\,Welfare_t + Economy_t\,\Gamma + \delta_1\,Trend + \delta_2\,Trend^2 + \nu_t \tag{5}$$

The parameter Λ, 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 Λ 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 literature cited earlier that TANF reduced caseloads by between 19% and 35%.

Second, we modified the baseline model (Eq. (4)) to allow an interaction between the main effect of welfare reform and the welfare reform-induced percent reduction in caseloads for each state:

$$(Ln\,A)_{smt} = \alpha + \lambda_1 Welfare_{smt} + \lambda_2 (Welfare_{smt} * \Lambda_s) + Z_{st}\,\beta + State_s\,\Omega + Month_m\,\Phi + Year_t\,\Psi + \varepsilon_{smt} \tag{6}$$

The parameter λ_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 λ_1 to be insignificant and close to zero if our estimates are truly reflective of a causal effect. Similarly, λ_2 represents the effect of welfare reform on crime for a 100% reduction in caseloads (unit change in Λ_s), among those states which experienced reduction in caseloads attributed to welfare reform. If there is a dose-response effect based on strictness of state policies, we would expect the magnitude of λ_2 to be substantially larger than that of λ_1. Indeed we find that the main effect of welfare reform (λ_1) is statistically insignificant and much reduced in magnitude (−0.022 for property index crime; −0.016 for larceny/theft); the effect of the interaction term (λ_2) is negative with a high magnitude (−0.064 for property index crime; −0.077 for larceny/theft) and statistically significant at the 10 percent level. Specifically, a 100% reduction in caseloads that can be attributed purely to welfare reform is associated with about a 6.4% reduction in serious property crime and 7.7% reduction in larceny and theft.

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