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Hope Corman
Dhaval M. Dave
Nancy E. Reichman

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ABSTRACT

This study explores how a major public policy change—the implementation of welfare reform in the U.S. in the 1990s—shaped the age gradient in female crime. We used FBI arrest data to investigate the age-patterning of the effects of welfare reform on women's arrests for property crime, the type of crime women are most likely to commit and that welfare reform has been shown to affect. We found that women's property crime arrest rates declined over the age span; that welfare reform led to an overall reduction in adult women's property crime arrests of about 4%, with the strongest effects for women ages 25–29 and in their 40s; that the effects were slightly stronger in states with stricter work incentives; and that the effects were much stronger in states with high criminal justice expenditures and staffing. The key contributions of this study are the focus on a broad and relevant policy-based "turning point" (change in circumstances that can lead people to launch or desist from criminal careers), addressing the general question of how a turning point shapes age gradients in criminal behavior, and the focus on women in the context of the age patterning of crime.

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

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

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

Introduction

Social scientists have been studying the course of criminal careers for decades, both to increase scientific understanding of criminal behavior and to inform public policy. A strong and dominant finding has been that criminal behavior declines with age after increasing during adolescence, although there are some deviations from this pattern across time and place and the peak ages and rates of decline vary by type of crime (Ulmer and Steffensmeier 2014). Because the majority of crimes are committed by men, most of the evidence on the age-crime gradient has pertained to men, although similar age-crime gradients have recently been found for women (Greenfield and Snell 1999), whose criminal behavior has been increasing both in absolute terms and relative to men (Campagniello 2014).

Existing theory regarding criminal behavior suggests, and some empirical evidence indicates, that life events or noteworthy changes in circumstances—sometimes referred to as "turning points"—can lead people to launch criminal careers or desist from criminal activity (Sampson and Laub 1990). Turning points that have been studied include individual-level life events and circumstances, including marriage, employment, and health shocks in families (e.g., Corman et al. 2011, Horney et al. 1995; Laub et al. 1998; Sampson et al. 2006; Uggen 2000). According to Sampson and Laub (2016), research is needed on how broad societal changes (which can have large societal impacts) affect participation in criminal activities.

Little is known about the extent to which turning points shape age gradients in criminal behavior and, more specifically, how lifecourse criminal trajectories are affected by broad societal changes including government policies. That is, at what stages in the criminal career do turning points change lifecourse trajectories, and how are trajectories affected by broad social policies?

In this study, we address these gaps by considering the extent to which a salient turning point, a broad-based public policy that strongly encouraged long-term employment of low income mothers and has been shown to lead to decreases in women's crime (Corman, Dave and Reichman 2014), has differential effects by age. Specifically, we use arrest data from the Federal Bureau of Investigation (FBI) to investigate the age-patterning of the effects of welfare reform in the U.S. on women's arrests for property crime, the type of crime women are most likely to commit (Campagniello 2014) and that welfare reform has been shown to affect (Corman, Dave and Reichman 2014). We also explore the extent to which specific state-level welfare and criminal justice policies shape the age gradients in the effects of welfare reform on women's property crime.

The findings from this study add to our understanding of the effects of turning points on criminal careers, contribute to the relatively small literature on female crime despite significant increases in recent decades, and have implications for the long run impacts of a significant policy-based turning point, as effects at younger ages would have more consequential effects on criminal careers (and, by inference, crime rates) than those at older ages.

Welfare reform in the U.S. as a turning point

The landmark 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) ended entitlement to welfare under Aid to Families with Dependent Children (AFDC) and replaced the AFDC program with Temporary Assistance for Needy Families (TANF) block grants to states. This national legislation and policies in many states that preceded it (collectively referred to as welfare reform) were designed to reduce dependence on cash assistance by promoting employment of women at risk of relying on welfare. The key strategies underpinning the legislation were to impose work requirements and time limits as conditions for

receipt of cash assistance, which strongly incentivized women at risk for relying on public assistance (not just welfare recipients) to secure employment by reducing the benefits of welfare reliance compared to work and eliminating the practical option of long-term reliance on public assistance. Welfare reform was successful in that it increased employment of low-skilled women (Fang and Keane 2004; Ziliak 2016), increased their hours and weeks worked (Ziliak 2016), and decreased welfare caseloads (Loprest 2012).

Corman, Dave, and Reichman (2014) exploited changes in the implementation of welfare reform across states and over time in order to estimate the causal effects of the work-based regime on adult women's arrests from 1992 to 2002, the period during which welfare reform unfolded. Using monthly state-level data on arrest rates compiled by the FBI in conjunction with dates of state welfare reform implementation and other relevant factors, the authors found that welfare reform led to decreases in female arrests for property crimes (by 4–5%), but that it did not affect arrests for other types of crimes. Although the authors did not frame their study in terms of welfare reform as a turning point, this large-scale policy shift can reasonably be viewed as an exogenous and substantial turning point in the lives of low income women.

Female property crime in the U.S.

We focus on property crime, which is the type of crime that women are most likely to commit and that Corman, Dave and Reichman (2014) found was affected by welfare reform. The FBI has designated eight serious offenses as "index crimes" for which data are consistently collected, occur nationwide, and are likely to be reported to the police. Three of the eight index crimes are designated as property crime offenses—burglary, motor vehicle theft, and larceny-theft. Women are more likely to engage in serious property crime than violent crime or arson; in 1991, for example, the arrest rate of adult females for property crimes was about 414 per 100,000

women, compared to 206 for violent index crimes and 160 for drug crimes, with 51 for sale-manufacturing and 109 for possession (Snyder, Cooper and Mulako-Wangota 2018). Figure 1 shows a clear age gradient in female property crime during the period 1988 to 1991 (pre-welfare reform) (calculated by the authors from FBI arrest data).

Welfare reform as a potential shifter of the age gradient in female crime

In the current study, we investigate the extent to which welfare reform as a turning point had differential effects on female property crime by age. We also explore the extent to which specific state-level welfare and criminal justice policies shaped the age gradients in the effects of welfare reform on women's property crime.

Following the economic model of crime formulated by Becker (1968), extended by Ehrlich (1973), and modified by Corman, Dave, and Reichman (2014), potential criminals act to maximize an expected utility function depicted below in Equation 1:

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

wherein p is the probability of apprehension and conviction and (1-p) is the probability of not being apprehended. If the individual is not apprehended, income will equal X_1, which as shown in Equation 2 is a function of the wage in the legal sector (W_l) times the amount of time spent working in the legal sector (t_l), plus the return (wage) in the illegal sector (W_i) times the time devoted to the illegal sector (t_i). W_0 represents unearned income. If the individual is apprehended, income will be X_2, as shown in Equation 3 wherein income is reduced by F, which may include a fine, legal expenses, and incarceration, as well as reduced future wages due to a criminal record.

$$X_1 = W_i(t_i) + W_l(t_l) + W_0 \quad (2)$$

$$X_2 = W_i(t_i) + W_l(t_l) - F_i(t_i) + W_0 \quad (3)$$

Welfare reform imposed strong incentives to engage in legal work, which can be a substitute for illegal work. As indicated above, an arrest results in a lower income in the current period, if caught. Additionally, an arrest may result in a lower wage in the future, which may be more consequential for women under welfare reform, since they will no longer have the option of long-term welfare receipt. Indeed, Corman, Dave and Reichman (2014) found that welfare reform (which increased legal work) led to a reduction in female property crime arrests. In the current paper, we explore the degree to which welfare reform, as well as specific policy features of state welfare programs and state criminal justice systems, had differential effects on property crime arrests of women of different ages.

We expect that the strongest effects would be for younger women, who face longer time horizons potentially subject to work requirements and lifetime limits under welfare reform. However, many of the youngest women (those under age 25) would not be affected by welfare reform since they have not yet had children, which is a key eligibility criterion for welfare; the mean age of first birth in 1990 (pre-welfare reform) was 24.2 years (Mathews and Hamilton 2002). Thus, the strongest effects would be predicted for young women, but not necessarily the youngest who may be less likely consider how welfare reform may affect them when they transition to motherhood. Women at older ages may also be relatively responsive: Since arrest rates for female property crime decline with age (as shown in Fig. 1), the additional incentive of welfare reform might push older women (the few who have still not desisted by, for example, their 40s) to retire from crime a bit earlier than originally intended. Over the long run, the greater the effects for women of relatively young ages, the greater will be the overall reductions in crime, since younger women commit the most crimes and their earlier desistance will generate the largest lifecourse effects.

We also expect that welfare reform will have stronger effects on women's property crime in states with stricter work incentive policies in their welfare programs, owing to the stronger "treatment," and in states with stricter criminal justice policies, where the cost of engaging in criminal behavior is higher. The age/crime gradient may also be different in states with stricter work incentive and criminal justice policies. To the extent that crime of younger women is more strongly affected by welfare reform, we would expect that the age/crime gradient to be flatter in states with stricter welfare and criminal justice policies. If older women are more affected by welfare reform, we would expect the age/crime gradient to be steeper in states with stricter policies. In other words, as incentives to engage in property crime shift over the life cycle for women, there may be compounding effects with incentives presented by welfare reform and deterrence presented by stricter criminal justice policies.

Data

Following Corman, Dave and Reichman (2014), 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), 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. The former is used to create measures of arrests and the latter is used to characterize welfare reform, as described below. We use arrest data from 1990 through 2002, the period during which welfare reform was implemented by states. Given our focus on adult women with dependent children (women without dependent children are not eligible for welfare), we focus on arrests of women ages 21 through 49 years, and then separately analyze arrests for different age groups within this broader range.

Comprehensive reviews found that Uniform Crime Reports of arrests are valid indicators of serious crimes (Gove, Hughes and Geerken 1985; Nagin 1998), and much of the criminology literature uses arrests as proxies for crime. For ease of discussion, we at times use the terms "crime" and "arrests" interchangeably from this point forward, acknowledging that not all crimes result in arrests and that people can be arrested for crimes they did not commit.

Measures of Arrests

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. For consistency with the Corman, Dave and Reichman (2014) study, upon which this study builds, we limit our sample to agencies that cover at least 50,000 individuals and 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 (see that study for more detail). We consider burglary, larceny/theft (other than motor vehicle), and motor vehicle theft, which are classified as "property index crimes."[1]

The raw FBI data include arrests by individual years of age through age 24 and in 5-year increments starting at age 25. We thus consider the following age categories: 21–24, 25–29, 30–34, 35–39, and 40–49 years, with the last category spanning 10 years because of the relatively low number of arrests in this age group (see Fig. 1). In some analyses, we further consolidate the age categories by decades: 21–29, 30–39, and 40–49.

Measures of welfare reform implementation

[1] In order to achieve a more representative sample with consistent reporting, we also limited the sample to state/month observations for which at least 50% of the state's population was represented by the reporting agencies. In robustness checks, we confirm that our estimates and conclusions are not sensitive to alternate sample cut-offs.

The first phase of welfare reform consisted of pre-PRWORA waivers, which allowed states to implement experimental changes to their AFDC programs. 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 implemented their TANF programs, which were required to meet (but could be more strict than) federal guidelines between September 1996 and January 1998 (USDHHS 1999). Specifically, 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 through January 1998.

Following Corman, Dave and Reichman (2014) and consistent with the convention in the welfare reform literature (reviewed in Blank 2002), we exploit differences in the timing of welfare reform implementation across states. For waivers, we consider whether, in a given year and month, a given state had a statewide AFDC waiver in place that substantially altered the nature of AFDC with regard to time limits, work requirements, earnings disregards, sanctions, and/or family caps. For TANF, we consider whether, in a given year and month, the state had implemented TANF post-PRWORA. Many studies include separate measures for AFDC waivers and TANF, since they represent distinctly different phases of welfare reform. We show specifications with the separate indicators, but in most of our models we use a single indicator for any welfare reform (AFDC waiver or TANF) in order to maximize statistical power.

Other key measures

In secondary runs, we consider differential effects by state policy variations in the implementation of welfare reform post-PRWORA (a more nuanced welfare reform effect) and

by state criminal justice policies (to assess the effects of welfare reform under different criminal justice contexts). Blank and Schmidt (2001) characterized state work incentives according to four significant aspects of their post-PRWORA welfare programs—benefit generosity, earnings disregards, sanctions, and time limits—as strong, medium, or weak, and combined this information to classify states as having strong, mixed, or weak work incentives. High benefit generosity, low earnings disregards, lenient sanctions, and lenient time limits are indicators of weak work incentives. Blank and Schmidt categorized states with weak incentives in at least one of the four categories and strong incentives in no other categories as having weak work incentives, and states with strong incentives in at least one category and weak incentives in no other categories as having strong work incentives. All other states were categorized as having mixed work incentives. For this study, we dichotomously classified states as having weak (versus mixed or strong) work incentives using the Blank and Schmidt classifications, resulting in 10 states (including DC) being classified as having weak work incentives and all others being classified as having mixed or strong work incentives.

Welfare reform has been successful in reducing caseloads while increasing employment among low-skilled women; over our sample period spanning 1990–2002, the median state experienced a decline in the number of welfare recipients by over 55%. While some of this decline reflects the strength of the economy over the 1990s, studies have consistently found that as much as one-third to one-half of the caseload decline can be explained by welfare reform (Grogger & Karoly 2005; Loprest 2012; Dave, Corman and Reichman 2012). As an alternate proxy for differential work incentives, we assess whether states that experienced larger declines in welfare caseloads also experienced larger declines in property crime arrests, and whether these differences were compounded or moderated over the life cycle. Specifically, we classify states

into equal tertiles, based on the percentage decrease in welfare caseloads between 1990 and 2002 (low: decline<43%; medium: decline between 43–60%; and high: decline>60%) and decompose the average effects of welfare reform across these sets of states.[2]

A substantial body of evidence indicates that increases in criminal justice resources lead to lower crime rates (Eide 2000; Nagin 2013). Using data from the U.S. Department of Justice Bureau of Justice Statistics, we characterize state criminal justice policy using a broad measure of per capita total justice system expenditures by state and local governments for each state/year and by per capita total full time employees in criminal justice. There are three components of these expenditures: police, courts, and corrections, with police representing the largest share (U.S. Department of Justice 2017).

Methods

We exploit variation in the timing of welfare reform implementation across states within a quasi-experimental difference-in-differences (DD) research design, to estimate the overall effects of welfare reform on women's property crime and assess how this broad pro-employment policy shift may have altered the women's lifecourse criminal trajectories. We begin with the following baseline specification, akin to a reduced-form crime production function, relating changes in arrests for property index crimes to welfare reform:

$$\text{Ln } A_{smt} = \text{Welfare}_{smt}\,\Pi + Z_{st}\,\beta + \text{State}_s\,\Omega + (\text{Year}_t * \text{Month}_m)\,\Psi\,[+ (\text{State}_s * \text{Time Pre-Welfare Reform}_{mt})\,\Theta]\,[+ (\text{State}_s * \text{Time}_{mt})\,\Phi] + \varepsilon_{smt} \quad (4)$$

[2] Data on the number of welfare recipients was obtained from the University of Kentucky Center for Poverty Research UKCPR National Welfare Data, 1980-2015 database, available at http://www.ukcpr.org/data (accessed 12/11/17).

In Equation 4, the outcome represents the natural log of total arrests related to property index offenses for females ages 21–49 in state *s*, during month *m* and year *t*.[3] Arrests are a function of welfare policy (*Welfare*), characterized separately by the enactment of major waivers to AFDC and implementation of TANF, and in alternate specifications by an indicator for the implementation of any welfare reform (either a major AFDC waiver or TANF). We also control for an extensive vector of time-varying state covariates (Z), including measures of the state's economic and labor market conditions (unemployment rate, per capital personal income, poverty rate, minimum wage), relevant population base and data coverage (log of the state's total population, log of the state's female population for the relevant age group, log of the agency population for months with arrest reports, percent of the population covered by the reporting agencies), and criminal justice system (log of the state's criminal justice expenditures, log of full time equivalent number of police officers). The parameters of interest are Π, which capture the "reduced form" or total effect of welfare policies on property crime arrests, operating through all potential competing and/or reinforcing pathways. The parameter ε reflects a state-time disturbance term, which we assume to be correlated within states over time. Hence, we report and draw inferences based on state-clustered standard errors, which adjust for any arbitrary correlation with state cells over time.

All specifications further include state fixed effects (*State*), which control for all unmeasured time-invariant state heterogeneity, and year-by-month fixed effects (*Year* * *Month*), which control for any seasonality and national trends in criminal behavior and arrests. Furthermore, all models are weighted by the state's female population (ages 21–49). Unweighted

[3] The log transformation adjusts for the skewed distribution of arrests across states and time, and also affords a convenient comparison of the estimates across models and age groups in terms of (approximate) relative percent changes in arrests. As part of our robustness checks discussed below, we also estimate alternate functional forms, all of which yield highly similar estimates.

models would assign each state-year-month equal weight, and the DD effect would capture an average causal effect over states rather than over individuals. Population-based weighting to state-aggregated panels would yield an average policy response over individuals. Population weighting in such models can also improve the precision of the estimates since arrest rates in a small state may be more variable from year-to-year (due to sampling variation) than in a larger state; population-based weights can thus give a larger weight to more precise measurements over time (Angrist and Pischke 2014).[4]

A key methodological challenge in identifying a plausibly causal effect in the context of a DD model lies in separating the effect of welfare reform from those of other state-level time-varying factors that may be related to female property crime arrests. For instance, state trends in crime rates over this period may also be related to crack cocaine use and waning of this epidemic, differential policing, enforcement, and incarceration, demographic shifts, economic conditions, legalized abortion, and other unobserved factors (Corman, Dave and Reichman 2014). To the extent that that these factors uniformly affected female property crime across the nation, they would be captured by the time fixed effects. However, the concern relates to the possibility that these factors may have differentially affected women across states.

We address this concern regarding unobserved state-specific time-varying confounding factors in a number of ways. First, all models already include a rich set of covariates (Z) relating to the state's economic, labor market, and criminal justice conditions. Second, we extend the specifications to include both the natural log of arrests for all offenses and that for property

[4] Methodological studies (Angrist and Pischke 2014; Colman and Dave 2016) generally recommend weighting by population when estimating treatment effects based on state panels, although they also recommend comparing both weighted and unweighted estimates to ensure that the regression estimates and standard errors are not highly sensitive to weighting. We confirm that our results and conclusions are not dependent on applying state-population weights to the models.

crimes among *males*. These measures capture all time-varying unmeasured factors in a state affecting overall crime, and specifically property crime, in that state.[5] Third, we control for lagged state-level economic indicators and welfare caseloads, in order to account for potential policy endogeneity. If early vs. later adopters of welfare reform differed based on their prior trends in welfare caseloads and their prior economic conditions, then these measures would account for the state's pre-reform history. Fourth, we control for state-specific linear pre-policy trends, by including an interaction between each state indicator and the number of months prior to when the state implemented welfare reform (*State * Time Pre-WelfareReform*). These controls parametrically account for any unmeasured systematic differential trends across states prior to policy implementation, and help to address deviations from the parallel-trends assumption underlying the DD analysis. Finally, in alternate specifications, we also supplement the vector of controls with state-specific linear trends to further account for any systematically-varying state-level factors that may have coincided with welfare reform and that may be affecting women's crime over the sample period (*State * Time*).

We further extend these analyses to address specific issues that are the focus of this study. This includes estimating Equation 4 separately for streamlined age groups (ages 21–24, 25–29, 30–34, 35–39, 40–49) in order to assess differential responses to welfare reform across

[5] Dave et al. (2010) and Wehby et al. (2016) follow a similar strategy to control for unobserved state-specific trends in insurance coverage among pregnant women and for unobserved state trends in birth outcomes. Note that this does not amount to using males as a comparison group, as would be the case in a difference-in-difference-in-differences (DDD) specification. This would not be appropriate as males are not a fully-equivalent comparison group for studying female crime. Doing so would impose the restriction that, if not for welfare reform, a one-percentage change in arrests among men would lead to an equal change in arrests among women. Alternately, by controlling directly for male arrests in the specifications, there is no restriction being imposed that unobserved factors have to affect both male and female criminal behaviors identically. The inclusion of measures of male arrests more flexibly controls for common unmeasured factors that may be affecting male and female crime, as long as the effect is proportional (it is not required to be one-to-one). In supplemental models, we also use males as a "placebo check," given that males are generally not eligible to receive welfare and thus would not be directly impacted by welfare policies.

the age distribution. If welfare reform differentially impacts engagement in property crime across age groups, then this would indicate that the policy shift has also affected the shape of the age-crime gradient. . We then assess how the age-crime gradient and welfare policies mutually interact with the strength of a state's work incentives under welfare reform. This specification captures nuances in welfare policies across states, and also affords a validation check. If it is indeed the welfare policies that are exerting a causal effect on women's crime engagement, and differentially over their life cycle, then we would expect stronger effects in states that had stronger work incentives embedded in their welfare regimes. Finally, we assess how the age-crime gradient and the shift in the welfare regime interact with the state's criminal justice system. Each of these factors – age, welfare reform, and criminal justice policy– is associated with shifts in women's crime. Hence, the latter specifications explore if, and the extent to which, these factors interact and potentially reinforce each other in affecting property crime.

Results

Table 1 presents estimates from models corresponding to Equation 4 for property crime arrests among females ages 21–49. The estimate in Model 1 is from a parsimonious specification that only includes basic controls in conjunction with state and time fixed effects, suggesting an insignificant and small decline (1.6%) in property crime arrests associated with welfare reform. As we progressively add controls for unmeasured time-varying state heterogeneity, with measures of male arrests in Model 2 and lagged economic conditions and welfare caseloads in Model 3, the effect becomes stronger and statistically significant. These models suggest that welfare reform led to an approximate 3–4% decline in arrests for property crime.[6] We decompose this overall effect into an effect stemming from early AFDC waivers and an effect

[6] Specifically, the coefficient in Model 3 indicates that welfare reform led to a 4.2% [$(e^{-0.0427} - 1)*100$] decline in arrests for property index crimes.

stemming from the implementation of the federal welfare reform (TANF) in Model 4. Both aspects of welfare reform led to significant reductions in arrests; while the magnitude is somewhat larger for TANF (about 5.4% relative to 4% for AFDC waivers), confidence intervals overlap considerably and we are not able to reject the null of equal effect sizes. This is perhaps not surprising given that most of the components that characterized the early reform efforts through the AFDC waivers were later incorporated into TANF. The final two specifications control for differential pre-policy trends and state linear trends respectively. The robustness of the estimates to these controls provides some validation of the research design. Overall and as found by Corman, Dave and Reichman (2014), the estimates in Table 1 indicate a non-negligible decline in property crime among females ages 21–49, on the order of about 4–5%, as a result of welfare reform.

In Table 2, we explore heterogeneity in this average effect across the age distribution, and assess how the shift in the welfare regime affected the age-crime gradient. For parsimony of presentation, we focus on Specification 3 from Table 1, although estimates and patterns discussed below are not sensitive to this decision. Model 1 in Table 2 replicates the estimate for women ages 21–49 for ease of comparison, and the other models present estimates for narrower age bands. While estimates are negative across all age groups, there is considerable heterogeneity in the effect sizes.

Effects among the youngest women (ages 21–24) are quite small (2.5%) and not significant. This is consistent with many of women in this age range not yet having children and therefore not being eligible for welfare (and therefore not being directly affected by the treatment). Hence, the “treated” population among women ages 21–24 is likely to be small, and thus the average policy response over this age group will be muted. We find substantially

stronger effects for women in their mid-to-late 20s, representing a decline of about 4.2%. The larger effect partly reflects that the vast majority (about 67%) of low-educated women ages 25–29 had children and over half of those with children were unmarried (based on the 1996 March Current Population Survey, authors' calculations), and thus the "treated" sample is larger. These women also face longer time horizons potentially subject to the new welfare regime, including time limits and downward-revised expectations regarding welfare as a longer term option. For women in their 30s, we find some negative effects of welfare reform on property crime (mostly for women in their early 30s), though the effect is imprecisely estimated and weaker. The strongest effects of the new welfare regime were for the oldest age group, women between the ages of 40–49, suggesting a decrease in property crime arrests by about 5.7%. As older women are less likely to commit property crime (or crime in general), welfare reform appears to compound any disincentives to engage in criminal behaviors, especially for women who may be on the margin between desisting and continuing. The age-crime gradient is negatively sloped (Fig. 1), and welfare reform therefore somewhat steepens this gradient, especially the gradient post age 30.[7]

In Table 3, we explore heterogeneity in these effects based on the strength of the work incentives embedded in each state's welfare system post-reform. As described earlier, we follow the classification scheme developed by Blank and Schmidt (2001), and followed in prior work (Dave et al. 2011, 2012; Corman et al. 2014), categorizing states as having strong, mixed/medium, or weak work incentives based on state variation in time limits, benefits generosity, earnings disregard, and sanctions. As before, we estimate differential effects of

[7] Note that in absolute terms, welfare reform still leads to a larger reduction in the number of arrests among women ages 25–29 (given the higher level of crime from this age group). Furthermore, as we parse out effects across finer age groups or age groups wherein crime is less frequent, the arrest data invariably become noisier. This is reflected in the inflated standard errors, which preclude very precise comparisons or statistically significant differences.

welfare reform across the age distribution, but now interact the welfare reform indicator with whether the state's welfare regime contains strong or medium versus weak work incentives (Models 1–4).

For the broadest age group, 21–49, welfare reform led to about a 1.6% decrease in property crime among states with weak work incentives; states with stronger incentives and more pro-employment features experienced an additional decline of 2.8%, for a total decrease of about 4.4%. Overall, this pattern is validating of a dose-response check; if it is indeed welfare reform and its pro-employment push that is driving the decline in female crime, then the decline should be larger among states with regimes that incorporated stronger work incentives—and it is.

This pattern persists when we disaggregate by age: ages 21–29 (Model 2), ages 30–39 (Model 3), and ages 40–49 (Model 4). As before, we find the strongest effects of welfare reform on arrests among the youngest (ages 21–29) and oldest (ages 40–49) women, and specifically in states with relatively stronger work incentives. Combining the coefficients of the welfare reform indicator and the interaction term, reform leads to approximate 3.7%, 2.9%, and 6.1% declines in property crime among females in their 20s, 30s, and 40s, respectively – all of which is again suggestive of a steepening of the age-crime gradient.[8]

In Models 5–8, we assess these patterns in an alternate manner, by directly classifying states that experienced larger vs. smaller declines in welfare caseloads over the sample period. We interact the welfare reform indicator with indicators for whether the states were in the lowest, middle, or top tertile in terms of caseload decline. For the broadest age group, we find that

[8] In supplementary analyses (not reported), we further attempt to disentangle which specific components of welfare reform may be driving these differential effects. These results suggest that the strongest negative effects (declines in property crime) appear to be in states with a high level of earnings disregard; that is, states which reduce welfare benefits at a lower rate in response to an increase in earned income. As women are allowed to keep a larger portion of their welfare benefits as they work, higher net income is then associated with a larger decline in property crime.

reform is associated with a decrease in property crime among all three sets of states; however, the negative effect is significantly larger among states that experienced the largest decrease in caseloads, about 5.5% relative to 2–3% for the other states. This pattern continues as we explore heterogeneity across the age groups. The largest declines, as before, are for women in their 20s (4.7%) and in their 40s (6.5%), particularly in states that experienced the largest decrease in caseloads. In general, declines in arrests among women ages 40–49 are larger in all three sets of states relative to those among women in their 20s. This consistent pattern is indicative of a more elastic response in terms of crime desistance among older women overall, which appears to be compounded in states with larger decreases in their welfare rolls. It is important to note that most of the comparisons in Table 3 are imprecise, owing to the parsing of estimates across the age gradient and levels of work incentives, and should therefore be viewed as suggestive.

For women on the margin of engaging in or desisting from criminal offenses, the effects of welfare reform may be further moderated by the state's criminal justice system. Table 4 presents models that explore heterogeneity based on whether the state had high or low (relative to the median) per capital criminal justice spending (Models 1–4) and high or low (relative to the median) per capita full-time equivalent number of people employed in the criminal justice system (Models 5–8). To bypass endogenous sample selection, we classify states as above/below the median based on levels in 1990, which predated welfare reform. Across all models and age groups, we find consistent evidence that the shift in welfare regime led to a decrease in property crime; however, these effects materialize mainly among states that had relatively high levels of criminal justice spending and staffing.[9] Furthermore, unlike before, we find that welfare reform

[9] Baseline criminal justice spending is weakly negatively correlated (correlation of about -0.2) with welfare caseload decline (between 1990 and 2002); hence, these effects are not capturing differences across states experiencing larger vs. smaller decreases in caseloads. States with high vs. low criminal justice spending and staffing per capita are also not necessarily clustered in specific regions. For instance, while high spending states (as of 1990) include some in

led to a significant decline in property crime arrests among women ages 30–39, but the decrease was again is concentrated in states with larger criminal justice systems at baseline. These estimates suggest that any disincentives to engage in criminal behavior provided by welfare reform were possibly reinforced by any additional incentives to desist from crime in states with larger criminal justice systems. This interactive effect is present for all age groups. While these patterns are suggestive, they warrant further study since states with larger criminal justice systems in 1990 also tended to have higher levels of crime in 1990, including women's property crime. Hence, these effects also suggest that welfare reform led to larger declines in women's property crime in states that had higher levels of crime at baseline.

Additional Specification and Robustness Checks

We implemented several additional checks, relaxing some of the sample restrictions and estimating alternate specifications, in order to verify that the results are robust. First, we tested alternate functional forms and model specifications that: 1) expressed the outcome as a rate, as the natural log of the arrest rate (per 1000 women in the relevant age group); 2) changed the outcome to a logistic transformation based on the natural log of the odds of the arrest rate; 3) utilized non-logged measure of the arrest rate or total arrests as outcomes; and 4) extended the models to include state-specific quadratic time trends.

Second, we relaxed our sampling restrictions that included limiting analyses to reporting agencies that covered at least 50,000 individuals and agencies with a reported coverage of at least 50% in order to minimize measurement errors and maximize reporting consistency. Our estimates are not sensitive to utilizing the full sample, or relaxing these cutoffs.

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the northeast (NY, NJ, MA, NH) and the west (CA, OR, WA), they also include states in the south (DE, FL, GA, VA) and Midwest states (IL, MI).

Third, our analyses are not sensitive to aggregating the monthly arrest data to the annual level. While annual aggregation may smooth out some of the noise in the monthly data and seasonal factors, it also washes out meaningful variation from month-specific implementation of AFDC waivers and TANF. Nevertheless, the magnitudes of the effects, patterns, and our inferences are not altered from utilizing state-annual aggregates. Fourth, also re-estimated all models without utilizing population weights, and confirmed that our estimates are not substantially different across weighted and unweighted models. We gain some efficiency, however, when we use the population weights and the standard errors are lower in some cases. Finally, we explicitly utilized male arrests as a placebo check in models presented in Table 5. Males are generally not eligible for welfare, and thus would not be directly impacted by shifts in welfare policies. If there are any effects, they would be indirect (by affecting female household members), and would be second- or third-order effects. Hence, we would expect much smaller, and possibly nil, effects of welfare reform on property crime arrests among males. Table 5 reports these estimates for the most basic specification (Model 1 from Table 1 for Models 1–4 in Table 5) and for the most extended specification (Model 6 from Table 1 for Models 5–8 in Table 5).[10] Except for the degree allowed by a Type I error, estimates are statistically insignificant, very small in magnitude or close to zero, and never indicate any evidence of a commensurate decline in property crime among males for any age group.

Estimates in Context

Given established patterns of steep declines in criminal behavior (and women's property crime arrests in particular) with age, we most likely under-estimated the reductions in women's property crime that can be plausibly be attributed to welfare reform, particularly for women in

[10] We do not control for any measures of male arrests on the right hand side of the equation for any of these models.

the 25–29 year age range. For example, if welfare reform increased desistance among women in their 20s, for whom we found evidence of a reduction in arrests of about 4% for women in their mid-to-late 20s, this effect would reduce crime over the life course for that cohort. We thus project an estimate of the overall effect of welfare reform, which takes into consideration potential compounding effects over the life course, as follows.[11]

If the direct effect of welfare reform persists (we estimated a 2% reduction for women in their 30s), there will be a smaller criminal cohort of women in their 30s (owing to the greater share who desisted in their 20s), on the order of about 3%.[12] We thus infer that not only did welfare reform lead to 4% fewer arrests of women in their mid-to-late 20s, but that is also led to 5% fewer arrests of these women in their 30s. Continuing to the 40s, our estimated direct effect was about 5% and the projected indirect effect is 2%,[13] resulting in an additional 7% reduction in arrests. Overall, welfare reform is projected to reduce women's arrests by 4% in their mid-to-late 20s, an additional 5% in their 30s, and an additional 7% in their 40s (few women engage in criminal activities beyond that age range). These projections make clear that the overall effects of welfare reform will likely be substantially larger than what is suggested by our short-run estimates.

Conclusion

This study explored how a major public policy change—the implementation of welfare reform in the U.S.—shaped the age gradient in female property crime arrests, both overall and by

[11] This exercise is intended to illustrate potential compounding effects over the life course, given what we know about age gradients in crime and desistence. It ignores potential incarceration effects, assumes that the direct effects of welfare reform persist throughout the woman's adult life, and assumes that the effect sizes do not vary by cohort.
[12] To arrive at this figure, we assumed (based on the figures from Figure 1) that about 74% of the mid-to-late 20s cohort would have persisted into their 30s. That is, the number of women engaging in crime would have been 74% of the original cohort in the absence of welfare reform, but now this number is reduced by 4% of the 74%, or by about 3%.
[13] We arrived at this figure this number by multiplying the total reduction in the 30s (5%) by the fraction of women who persist from the 30s to the 40s (about 45%) (again, based on figures from Figure 1).

different state-level welfare and criminal justice policy contexts. We found that women's property crime arrest rates decline over the age span; welfare reform led to an overall reduction in adult women's property crime arrests of about 4%, with the strongest effects for women ages 25–29 and women in their 40s; these effects were slightly stronger effects in states with stricter work incentives in their welfare programs; and the effects of welfare reform on women's property crime were much stronger in states with high levels of per capita criminal justice expenditures and staffing, for all age cohorts.

The strong effects for women in their mid-to-late 20s were expected, given that welfare reform imposed strong incentives to engage in legal employment, criminal activity can reduce future legal employment opportunities, and young women have a long time horizon potentially subject to welfare reform. The findings of strong effects for women in their 40s are also consistent with the hypothesized scenario that welfare reform would accelerate decisions to desist among older women who are on the margin of retiring from criminal careers in the near future.

The three key contributions of this study are the focus on the effects on crime of a broad and relevant policy-based turning point in criminal careers, the focus on the general question of how a turning point shapes age gradients in criminal behavior, and the focus on women in the context of the age patterning of crime. The role of broad societal changes as turning points in decisions to desist or enter into criminal careers, the potential effects of turning points on age gradients in crime, and the processes underlying crime trajectories of women represent large knowledge gaps that this study has made important steps in addressing. That said, our findings cannot be generalized to other substantial macro-level events, such as the Great Recession in the late 2000s, or to men; our focus was on a specific (but serious, relatively prevalent, and relevant)

category of crime; we used arrests as a proxy for criminal behavior (in keeping with much of the literature); and we considered patterns of crime by age rather than changes in individuals' criminal behavior over time (as have many previous studies of the lifecycle of crime, owing to lack of individual-level longitudinal data with sufficient sample sizes and measures of crime).

The results from this study also add to the growing knowledge about the indirect or unintended (that is, not directly intended) effects of welfare reform in the U.S. The findings strongly suggest that welfare reform has conferred societal benefits in terms of a reduction in female property crime that is not only likely to persist (because welfare reform is currently very much in effect today), but is also likely to compound in the future (owing to the strong effects for relatively young women). That said, this socially desirable outcome must be considered in light of the strong economy during the period studied (i.e., the effects of welfare reform on women's crime may been very different during the Great Recession) and in conjunction with the effects of welfare reform on other relevant outcomes—both for mothers and their children—before coming to the general conclusion that society is better off as a result of this major policy shift.

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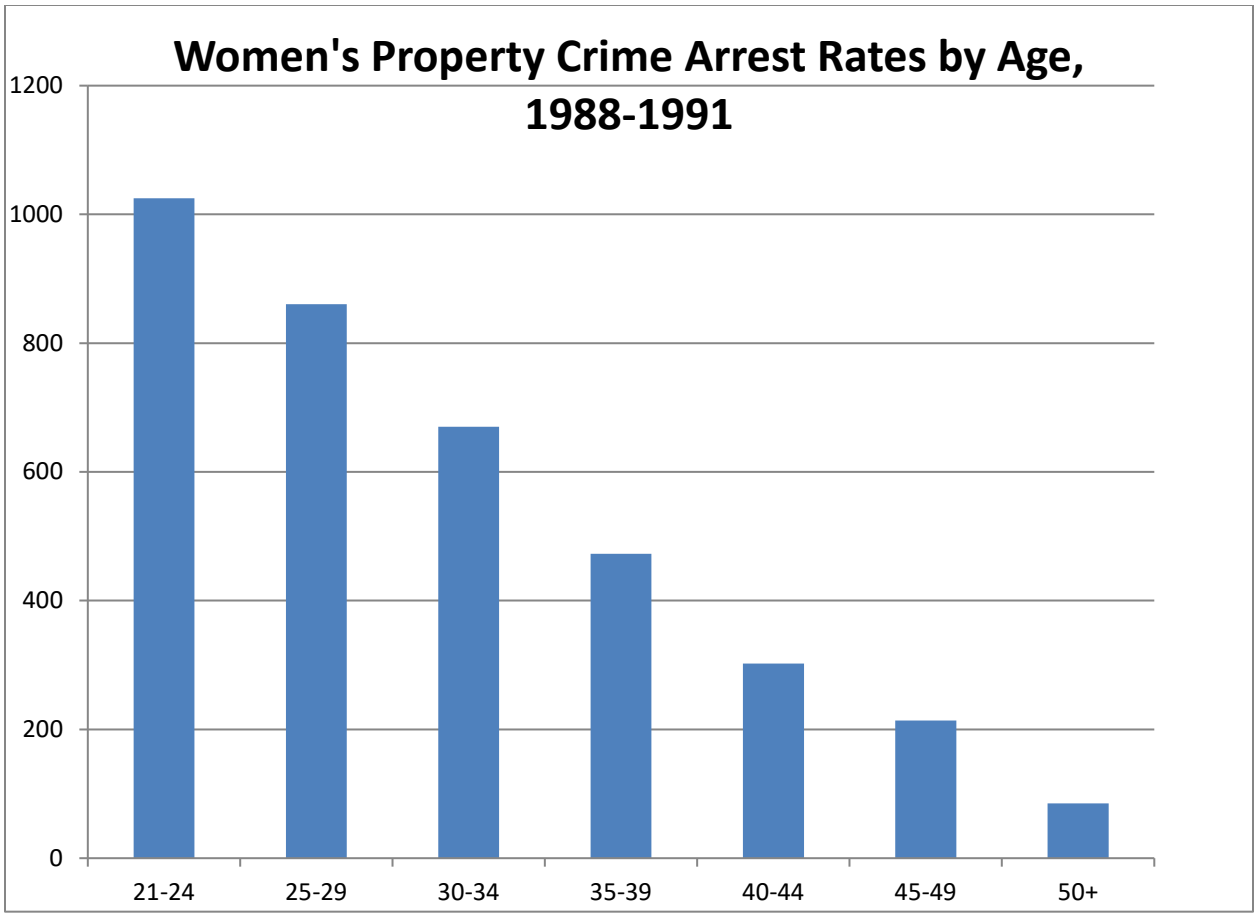

Figure 1

Rates are per 100,000 women, based on criminal justice agencies serving populations of at least 50,000 and reporting arrests for at least half of the year.

Table 1
Effects of Welfare Reform on Property Crime
FBI Arrests
Females, Ages 21–49 years, 1990–2002

Outcome	Ln Arrests for Property Index Crimes					
Specification	1	2	3	4	5	6
Any Welfare Reform	-0.0159 (0.0250)	-0.0316* (0.0179)	-0.0427** (0.0186)	_	-0.0377** (0.0166)	-0.0488** (0.0185)
AFDC Waiver	_	_	_	-0.0402** (0.0194)	_	_
TANF	_	_	_	-0.0540*** (0.0181)	_	_
State Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Year * Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Measures of Male Arrests	No	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions and Welfare Caseloads	No	No	Yes	Yes	Yes	Yes
State-specific linear pre-policy trends	No	No	No	No	Yes	No
State-specific linear trends	No	No	No	No	No	Yes
Observations	6298	6298	6298	6298	6298	6298

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 and year*month, 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 of full time equivalent number of police officers, state minimum wage, state poverty rate, and percent of the population covered by reporting agencies. Measures of male arrests include the log of the male arrests for all criminal offenses (ages 21–49) and the log of male arrests for property index crimes (ages 21–49). Lagged covariates include one-year lags of the state unemployment rate, real personal income per capita, and welfare caseloads (total and child-only caseloads). Sample is limited to agencies with a reported coverage of at least 50%.

Table 2
Effects of Welfare Reform on Property Crime
FBI Arrests, Females, 1990–2002
Differential Effects across Age Distribution

Outcome	Ln Arrests for Property Index Crimes					
Specification	1	2	3	4	5	6
Age Group	Ages 21–49	Ages 21–24	Ages 25–29	Ages 30–34	Ages 35–39	Ages 40–49
Any Welfare Reform	-0.0427**	-0.0258	-0.0426*	-0.0347	-0.0043	-0.0591*
	(0.0186)	(0.0272)	(0.0238)	(0.0260)	(0.0275)	(0.0297)
State Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Year * Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions and Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6298	6298	6298	6298	6298	6298

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. The regression models are based on Specification 3 from Table 1. All models control for indicators for state and year*month, 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 of full time equivalent number of police officers, state minimum wage, state poverty rate, and percent of the population covered by reporting agencies. Measures of male arrests include the log of the male arrests for all criminal offenses (ages 21–49) and the log of male arrests for property index crimes (relevant age group). Lagged covariates include one-year lags of the state unemployment rate, real personal income per capita, and welfare caseloads (total and child-only caseloads). Sample is limited to agencies with a reported coverage of at least 50%.

Table 3
Effects of Welfare Reform on Property Crime
FBI Arrests, Females, 1990–2002
Differential Effects across Age Distribution and Work Incentives

Outcome	Ln Arrests for Property Index Crimes							
Specification	1	2	3	4	5	6		
Age Group	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49
Any Welfare Reform	-0.0163	0.0168	-0.0088	-0.0115	_	_	_	_
	(0.0235)	(0.0289)	(0.0376)	(0.0437)				
Any Welfare Reform *	-0.0280	-0.0540	-0.0205	-0.0498	_	_	_	_
Strict / Medium Work Incentives	(0.0288)	(0.0346)	(0.0417)	(0.0522)				
Any Welfare Reform *					-0.0270	-0.0370	-0.0087	-0.0334
Change in Caseloads-Low	_	_	_	_	(0.0263)	(0.0292)	(0.0284)	(0.0415)
Any Welfare Reform *					-0.0204	-0.0131	-0.0255	-0.0587
Change in Caseloads-Medium	_	_	_	_	(0.0239)	(0.0291)	(0.0261)	(0.0377)
Any Welfare Reform *					-0.0570**	-0.0479	-0.0250	-0.0673*
Change in Caseloads-High	_	_	_	_	(0.0257)	(0.0316)	(0.0284)	(0.0372)
State Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions and Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6298	6298	6298	6298	6298	6298	6298	6298

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. The regression models are based on Specification 3 from Table 1. All models control for indicators for state and year*month, 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 of full time equivalent number of police officers, state minimum wage, state poverty rate, and percent of the population covered by reporting agencies. We also allow the year effects to differ across states with weak vs. strict/medium work incentives (Models 1–4) and across states with low/medium/high caseload change (Models 5–8). Measures of male arrests include the log of the male arrests for all criminal offenses (ages 21–49) and the log of male arrests for property index crimes (relevant age group). Lagged covariates include one-year lags of the state unemployment rate, real personal income per capita, and welfare caseloads (total and child-only caseloads). Sample is limited to agencies with a reported coverage of at least 50%. States are classified as having strict or medium (relative to weak) work incentives, based on Blank and Schmidt (2001). States are classified as low, medium, or high in terms of change in caseloads, based on tertiles of the percentage decrease in caseloads between 1990 and 2002.

Table 4
Effects of Welfare Reform on Property Crime
FBI Arrests, Females, 1990–2002
Differential Effects across Age Distribution and Criminal Justice Spending

Outcome	Ln Arrests for Property Index Crimes							
Specification	1	2	3	4	5	6		
Age Group	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49
Any Welfare Reform	0.0108	0.0491**	0.0332	-0.0058	-0.0085	0.0018	0.0354	0.0001
	(0.0203)	(0.0227)	(0.0236)	(0.0356)	(0.0225)	(0.0289)	(0.0215)	(0.0376)
Any Welfare Reform *	-0.0987***	-0.1583***	-0.1053***	-0.0980*	_	_	_	_
Criminal Justice Spending-High	(0.0251)	(0.0285)	(0.0334)	(0.0507)				
Any Welfare Reform *					-0.0612**	-0.0687*	-0.1063***	-0.1062*
Criminal Justice FTE-High	_	_	_	_	(0.0291)	(0.0403)	(0.0315)	(0.0541)
State Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Measures of Male Arrests	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Economic Conditions and Welfare Caseloads	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6298	6298	6298	6298	6298	6298	6298	6298

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. The regression models are based on Specification 3 from Table 1. All models control for indicators for state and year*month, 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 of full time equivalent number of police officers, state minimum wage, state poverty rate, and percent of the population covered by reporting agencies. Measures of male arrests include the log of the male arrests for all criminal offenses (ages 21–49) and the log of male arrests for property index crimes (relevant age group). Lagged covariates include one-year lags of the state unemployment rate, real personal income per capita, and welfare caseloads (total and child-only caseloads). Sample is limited to agencies with a reported coverage of at least 50%. States are classified as having high criminal justice spending or full-time employment (FTE) if they are above the median in per capita terms during the baseline period of the sample (1990).

Table 5
Effects of Welfare Reform on Property Crime
FBI Arrests, Males, 1990–2002

Outcome	Ln Arrests for Property Index Crimes							
Specification	1	2	3	4	5	6	7	8
Age Group	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49	Ages 21–49	Ages 21–29	Ages 30–39	Ages 40–49
Any Welfare Reform	0.0206	0.0170	0.0166	0.0456*	0.0114	0.0070	0.0119	0.0205
	(0.0199)	(0.0190)	(0.0266)	(0.0252)	(0.0248)	(0.0249)	(0.0281)	(0.0225)
State Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Measures of Male Arrests	No	No	No	No	No	No	No	No
Lagged Economic Conditions and Welfare Caseloads	No	No	No	No	Yes	Yes	Yes	Yes
State-specific linear rends	No	No	No	No	Yes	Yes	Yes	Yes
Observations	6298	6298	6298	6298	6298	6298	6298	6298

Notes: Coefficients from OLS semi-log models are presented. Standard errors are adjusted for arbitrary correlation within state cells, and reported in parentheses. Models 1 through 4 are based on Specification 1 from Table 1, with the exclusion of male arrests. Models 5 through 8 are based on Specification 6 from Table 1, with the exclusion of male arrests. All models control for indicators for state and year*month, 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 of full time equivalent number of police officers, state minimum wage, state poverty rate, and percent of the population covered by reporting agencies. Lagged covariates include one-year lags of the state unemployment rate, real personal income per capita, and welfare caseloads (total and child-only caseloads). Sample is limited to agencies with a reported coverage of at least 50%.