LOCAL LABOR MARKETS AND CRIMINAL RECIDIVISM

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This Version: October 2015
PRELIMINARY AND INCOMPLETE: Please do not cite or circulate

Abstract

This paper estimates the impact of local labor market conditions on criminal recidivism using rich administrative prison records on over four million offenders from 43 states and over 2,800 U.S. counties from 2000-2013. I find that greater employment and wages in the county to which an offender returns upon release significantly decreases the risk of recidivism. The impact of higher wages on recidivism is larger in sectors that report being more willing to hire ex-offenders, and larger for both black offenders and first-time offenders. Wage effects are larger in states that legally restrict ex-offender eligibility for food stamps and welfare benefits, and employment effects are larger in states that prohibit private employers from discriminating on the basis of criminal history.
JEL Codes: J23, K14, K40

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1. Introduction

Over the past three decades, the United States has experienced an unprecedented increase in incarceration. As of 2013, the United States represents five percent of the world's population, but approximately 25 percent of its prison population (Walmsley 2013). One consequence of this mass incarceration is the number of offenders who reenter society. Every week, over 10,000 prisoners are released from federal and state prisons, with over 650,000 offenders returning to their communities on an annual basis (Carson and Golinelli 2014). According to the National Employment Law Project, over 30 percent of working-age adults have a criminal record.

Recidivism is prevalent among these released offenders. Within three years of release, over two-thirds of offenders are rearrested, over half reconvicted, and over 40 percent returned to custody (Beck and Shipley 1989, Langan and Levin 2002, Durose et al. 2014). One potential explanation for high recidivism rates is low employment and earnings opportunities for ex-offenders. The annual flow of released prisoners represents roughly 30 percent the yearly growth in the labor force (Freeman 2003), questioning the capacity of local labor markets to absorb and reintegrate these ex-offenders. One year after release, as many as 60 to 75 percent of former offenders are not employed in the legitimate labor market (Petersilia 2003, Visher et al. 2008). In the aftermath of the Great Recession, ex-offenders may have an even harder time finding employment.[1]

To what extent do local employment and earnings prospects affect recidivism among ex-offenders? Many cite employment among ex-offenders as a key component of successful reintegration into society (Uggen 2000, Kling 2006), but the vast majority of these ex-offenders have low human capital, job experience, and suffer from mental and substance abuse issues (Petersilia 2003, Visher et al. 2008), potentially impeding their ability to obtain employment. To the extent that individual characteristics such as ability or individual preferences for criminal behavior are the primary determinants of recidivism, stronger labor markets may have little impact on reoffending. In addition, the stigma associated with incarceration (Pager 2003, Holzer et al. 2003) and legal bans in certain occupations suggest that employers may be less likely to hire ex-offenders compared to other low-skilled workers. On the other hand, ex-offenders may be responsive to opportunities to engage in legitimate work, decreasing the returns to criminal activity and consequently rates of recidivism (Gelber et al. forthcoming, Gould et al. 2002, Raphael and Winter-Ember 2001).

This paper examines the link between local labor market conditions and criminal recidivism. These findings are of immense importance given the resources spent on job training programs in prisons and reforms to expand job opportunities among ex-offenders. In recent years, federal and state governments have also considered legal reforms to reduce the ability of employers to consider

[1]For instance, data from Indiana suggest that the employment rate for released offenders decreased from 40.1 percent in 2006 to 25 percent in 2009. See http://eric.ed.gov/?id=EJ1011632.

criminal history in making hiring decisions. For instance, in April of 2012, the Equal Employment Opportunity Commission issued guidance stating that an employer cannot categorically ban the hiring of a convicted felon unless the disqualification is "job-related" or "consistent with business necessity." In addition, cities and states have begun a movement to "ban the box," limiting the ability of employers to ask individuals about their criminal history in an effort to aid offender reentry into society.

However, examining the impact of local market conditions on recidivism has been hampered by three main problems. First, there are limited panel datasets that link prison spells for the same individual over time due to confidentiality concerns. Second, existing panel datasets contain very small samples, or a small number of states that give access to administrative correctional data, making estimates prone to external validity concerns. Third, state-level data often do not identify the local communities that offenders are likely to return to, making it impossible to identify the impact of local labor markets versus aggregate state economic conditions.

This paper uses annual offender-level administrative data on prisoner admissions and releases collected by the Bureau of Justice Statistics (BJS) as part of the National Corrections Reporting Program (NCRP). The NCRP gathers data from states on a voluntary basis. The data come from state Departments of Corrections and Parole and cover persons admitted to state prison, or released from state prison from 2000-2013. For individual offenders with multiple prison terms, the NCRP chronologically assembled prison spells for each offender, allowing for a large-scale and geographically representative analysis of criminal recidivism. With over four million offenders released from 43 states and over 2,800 counties, I am able to exploit variation in labor market conditions both across counties and time. I am also able to exploit cross-state differences in laws that affect the ability of employers to discriminate on the basis of criminal history.

Importantly, the data also contain information on the county in which each offender was sentenced, representative of where the offender resides and returns to after release, which is used to assign local labor markets. Controlling for demographic and offense level characteristics of each offender, and county and time fixed effects, I measure the effect of local labor market conditions on the probability of reentry into the state corrections system. These estimates are identified from differences in the timing and severity of local economic conditions.

I find that employment growth in an offender's local labor market significantly reduces the probability of returning to prison. Similarly, greater growth in average wages is negatively associated with the probability of returning to prison and has a substantially larger impact on recidivism than employment, suggesting that earnings opportunities are critical for reducing recidivism. The effects of local labor demand shocks on recidivism are largest in the sectors that report a willingness to hire ex-offenders. Black offenders, older offenders, and first-time offenders are most responsive to changes to wages.

I also find that wage effects are largest in states that restrict felon eligibility for food stamps and welfare, suggesting that obtaining economic resources through employment is a likely driver of my results. Finally, I find that employment effects are larger in states that prohibit private employers from discriminating against ex-offenders, consistent with employer demand being an important mechanism to reducing recidivism.

Ultimately, the typical wage growth during a business cycle decreases the risk of recidivism by 2.0 to 4.7 percent, suggesting that the cohorts of offenders released during the Great Recession may have been exposed to an increased risk of recidivism. States like California have also released some inmates in order to reduce overcrowding and save money in a depressed economy, but the tough job market may impede the ability of ex-offenders to find employment, potentially increasing future recidivism.

This paper is related to three strands of research. One strand explores the impact of arrest and incarceration on employment and earnings (Grogger 1995, Kling 2006, Western 2006, Mueller-Smith 2014). Another strand analyzes the impact of incarceration on future recidivism (Di Tella and Schargrodsky 2013, Aizer and Doyle 2015). Finally, a large sociological literature explores the stigma associated with incarceration and its impact on labor market outcomes (Pager 2003, Pettit and Lyons 2007).

A few studies are most directly related to this paper. First, Myers (1983) finds that among a sample of 432 male offenders released from Maryland state prisons, higher average weekly wages reduces the probability of rearrest. More recently, Sabol (2007) studies the relationship between local unemployment rates and employment prospects among a sample of released prisoners in Ohio. Using a duration model, he finds that a one percent increase in county unemployment rates decreases the probability of a released prisoner exiting the initial spell of unemployment by about five percent. Raphael and Weiman (2007) explore the relationship between county unemployment rates at the time of release (static rather than time-varying) on the probability of returning to prison among a sample of released prisoners in California. Using ordinary least squares regression, they find a small, but positive relationship between local unemployment rates and the probability of returning to custody. In another sample of released prisoners from California, Schnepel (2015) finds that increases in construction and manufacturing employment opportunities at the time of release are associated with significantly lower recidivism.

The remainder of the paper is structured as follows. Section 2 provides a conceptual framework for the relationship between local labor market conditions and criminal recidivism. Section 3 describes the data and provides summary statistics. Section 4 describes the empirical strategy. Section 5 estimates the impact of local labor market conditions on the hazard of returning to prison. Section 6 concludes.

2. Conceptual Framework

In this section, I discuss how local economic conditions may affect recidivism directly through the probability of being employed or indirectly through its impact on other individuals, like potential criminal partners of released ex-offenders. Given both direct and indirect effects, my estimates should be interpreted as the reduced form effect of better labor market conditions on recidivism. In later sections, I consider to what extent my estimates can be explained by the direct effect of employment.

Increases in both employment and earnings reduce the likelihood of recidivating by increasing the probability of obtaining work and increasing the potential return to work. Employment can also be a form of "incapacitation" by keeping ex-offenders occupied and less likely to engage in criminal activity. However, ex-offenders may be unresponsive to labor market opportunities if the traits that are correlated with criminal behavior are immutable and essential to getting a job. For instance, offenders have low ability, low levels of work experience, and a high prevalence of mental and substance abuse issues. In fact, human capital may erode during prison, furthering reducing employability. Additionally, the ability to search and maintain a job may be dependent on getting affordable housing and public assistance. Federal laws allow local public housing authorities to deny housing to those with criminal records, and most states restrict certain ex-offenders with drug felony convictions from being eligible from federally funded public assistance and food stamps.

Employer demand for ex-offender labor may also depend on local labor market conditions. Generally, the stigma of incarceration makes the hiring of ex-offenders undesirable to employers, even compared to other low-skilled labor. Moreover, employers in many states can legally deny jobs to individuals with a criminal record, and even those who were arrested but never convicted. Employers may not be willing to hire ex-offenders unless the labor market is especially tight.

In addition to the direct effect of local labor markets on recidivism via employment, there are potential indirect effects on recidivism. For instance, if criminal partners are more likely to be employed during good economic times, the incentive for any individual to recidivate may decrease regardless of his or her own employment status. Conversely, improved economic conditions may increase, rather than reduce, recidivism depending on who benefits. For example, Freedman and Owens (2014) find that when labor demand shocks are concentrated among a certain set of individuals, those who do not benefit (particularly those with a prior record) are more likely to commit crimes.

3. Data

3.1. National Corrections Reporting Program

Data on prison spells are obtained from the NCRP. The data are constructed using administrative data voluntarily provided by states on prison admissions and releases from 2000-2013, with almost all offenders entering prison between 1990 and 2013. At least 38 states have provided some data since 2000 and by 2013, 48 states provided data.[2]

Prior to 2013, the NCRP data comprised separate and non-linkable files for prison admissions, prison custody, and prison releases. In 2013, the BJS retroactively linked prison spells from 2000 onwards using inmate ID numbers, dates of birth, admission, and release, and offense and sentencing information in the NCRP data. Years in which data were incomplete or in which counts where substantially different from National Prisoner Survey (NPS) statistics were excluded. The majority of states (44 total) were able to have records linked for some period of time they submitted data between 2000 and 2013.[3] See Appendix Table 1 for a list of the states in the sample and the years for which they provided data used to construct reliable prison spells. The data contain information on the exact prison admission date and release date for each prison spell. Demographic characteristics for each offender include age, race, Hispanic ethnicity, education (highest grade completed), gender, and whether the individual has previously been convicted of a felony. From these variables, I construct age and prior felony status as of the first observed prison release so that all demographic characteristics are time-invariant throughout the sample period.

For each individual and prison spell, I observe the most serious offense of conviction. From offense types provided by each of the participating states, the BJS created a uniform classification of 171 offense types, distinguishing between completed crimes, attempts, and conspiracies to commit the substantive offense. For instance, separate offense types are recorded for completed, attempt, and conspiracy to commit petty larceny/theft involving goods under $200. I also observe the number of conviction counts for each offense, the sentence imposed for each offense, as well as the total sentence imposed. Because I observe the exact prison admission date and prison release date for each period of incarceration, I calculate the actual total time served for each period

[2]Two other studies using the NCRP prison admissions and release data to explore trends over time have used a subset of states to ensure reliability. First, Pfaff (2011) compared counts of individuals entering and exiting into state prisons from NCRP (1983-2002) to other official counts such as the National Prisoner Statistics (NPS) Series from the BJS. According to Pfaff, eleven states consistently reported data: California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Neal and Rick (2014) conduct several checks both internally within the NCRP data and with other data sources such as the NPS using data from 1983 to 2009. After several tests, the authors exclude their analysis to seven states: California, Colorado, Michigan, New Jersey, South Carolina, Washington, and Wisconsin. In unreported results, my findings are robust to these subsamples of states.

[3]For a description on how prison terms were created, see http://www.icpsr.umich.edu/files/NACJD/ncrp/white-paper-computing-code.pdf.

of incarceration, which differs from the total sentence imposed because of early release through parole or good time credited.

The NCRP provide additional details on each period of incarceration. For each prison spell, I observe the type of facility the prisoner entered into, most commonly a state prison facility. In addition, I observe the reason why the offender entered into the custody of the correctional facility, such as whether the offender was committed by the court (representing a new offense), or recommitted due to a parole revocation, either with a resulting new sentence or no new sentence. The NCRP data also report why the prisoner was released, such as due to a mandatory or discretionary parole release, shock probation, or the expiration of the sentence - the four most common types of prison release.[4] Finally, I observe the agency that assumed custody at the time of the release, such as state parole or probation. Offense information is all defined at the time of release from the first prison spell, and time-invariant throughout the sample period.

These data are used to examine the determinants of criminal recidivism defined as reentry into custody. The period between release from prison after the first observable spell and reentry represents a non-custody spell. I make three sample restrictions. First, I drop observations in which county of sentencing is missing, about 2.7 percent of the observations, leaving me with a sample from 43 states. Second, left-censored spells are dropped because it is not possible to determine when an offender was released from prison, another 13.8 percent of the sample. Third, I drop individuals who were released from prison prior to 2000. After these restrictions, there are a total of 4,084,436 offenders released from prison between 2000-2013 and therefore 4,084,436 non-custody spells.

These data are uniquely suited for a study of the local labor market impacts on recidivism for several reasons. First, the sample size is unprecedentedly large, comprising over four million individuals released from prison between 2000 and 2013, orders of magnitudes larger than prior studies. Second, the data contain information on the the county where each sentence was imposed, which is where the overwhelming majority of prisoners are returned to from prison. Offenders who are turned over to state parole are generally required under state statute to remain in the original county of conviction or last county of residence (Raphael and Weiman 2007, Sabol 2007, Schnepel 2015). As a result, I use sentencing county as a proxy for each offenders' local labor market.[5] Third, the period covered by the data contain substantial variation in economic conditions, with a period of economic expansion (2000-2007) followed by the Great Recession (2008-2013).

[4]Prisoners can be conditionally released by parole boards through discretionary release, or by statute through mandatory release when their time served plus any good time earned equals the original sentence. Another form of conditional release comes through shock probation, where a judge can impose a brief period of incarceration designed to shock a first-time offender, followed by release under supervision. In other states, prisoners may be released unconditionally following the expiration of their sentence, in which case they have served the maximum court sentence and there is no supervision upon release.

[5]Any mismeasurement of the county that an offender returns to will attenuate my estimates towards zero.

There are three main limitations to the data. First, the NCRP data only contain information on individuals who were incarcerated in state prison. As a result, this study cannot identify the labor market impacts on the initial observed entry into the prison system, and consequently the decision to commit the initial crime. Because the data also only link prison spells within a state, any reoffending in a different state is not captured and is indistinguishable from an individual who is not recommitted in the same state.[6] Third, the data only capture return to custody, not rearrest or prosecution. As a result, the estimates in this paper may underestimate the impact of local labor market conditions on broader definitions of recidivism.

Table 1 presents the unconditional probabilities of returning to prison for the 4,084,436 released offenders on their first observed prison release and by prisoner characteristics.[7] Because I only include one prison release per offender, I do not capture the recidivism of offenders who recidivate two or more times, who are likely higher risk. For the full sample of over four million prisoners released between 2000-2013 in 43 states, 15 percent return to prison within one year of release and 30 percent return to prison within five years of release.[8]

Black offenders have higher rates of recidivism than white and Hispanic offenders. Males and younger offenders are also more likely to return to prison than females and older offenders, respectively. The higher the educational attainment of an offender, the lower the rate of recidivism. Offenders who have a prior felony incarceration have roughly similar recidivism rates compared to those without. By type of primary offense, prisoners convicted of property offenses are more likely to recidivate than those convicted of violent or drug offenses. By type of prison admittance, prisoners who enter into custody due to a parole or probation violation have higher rates of recidivism than those with a new court commitment. Finally, prisoners released through mandatory parole are more likely to recidivate than those released through the discretion of a parole board, or those released through shock probation. Prisoners who serve the full maximum court sentence have the lowest rates of recidivism, likely because they are not under supervision following release and therefore cannot be recommitted for technical parole violations.[9]

Figure 1 presents unconditional empirical hazard rates by month since release, calculated as the number of failures (offenders returning to prison) in month t divided by the size of the risk set at the beginning of the month. This figure shows that the hazard rate peaks within the first year

[6]The BJS estimates that within five years of release, approximately ten percent of released offenders are arrested in a state other than the one that released them. See http://www.bjs.gov/content/pub/pdf/rprts05p0510.pdf. Incarceration in another state is therefore some fraction of this ten percent of released offenders. However, if offenders move out of states with poor economic conditions, this mobility leads to a downward bias in the main estimates.

[7]I define recidivism as return to prison whether due to a "new commitment" or technical parole violation given evidence that law enforcement officials often classify a new offense as a technical violation because it is easier to ensure a period of incarceration (Kuziemko 2013).

[8]These recidivism rates are comparable to other statistics on recidivism using the NCRP Data. See Rhodes et al. (2014).

[9]See http://www.doccs.ny.gov/Research/Reports/2014/2010_releases_3yr_out.pdf.

since release and then declines exponentially.

Given that the hazard rate is approximately flat three years post-release, I censor all spells at 36 months (or 12 quarters) after release in order to focus on the risk of returning to prison in the three years post-release. I transform the data to allow for multiple observations per offender, such that each observation represents a quarter in each offender's non-custody spell. I allow for multiple observations per offender to allow local economic conditions to vary during the course of each non-custody spell. For instance, an offender released in June 2000 and who returned to custody in June 2001 would have four observations for each quarter between release and readmission. The full sample of 4,084,436 released offenders results in 35,780,841 observations in my estimation sample.

3.2. Local Labor Market Variables

Ideally, one would like to observe local labor market demand for ex-offenders. Prior studies (Raphael and Weiman 2007, Sabol 2007) have utilized local unemployment rates as the sole proxy for ex-offender labor demand. In contrast, I proxy for labor demand using both employment and wages. Unlike the unemployment rate, employment varies solely due to the number of employees at any point in time, rather than also varying due to changes in labor force participation. Wages may also be a better proxy for the labor market prospects of ex-offenders given that wages reflect longer-term changes to labor market conditions than unemployment, which is often short-lived and highly cyclical (Gould et al. 2002).

I obtain employment and earnings measures from the Quarterly Workforce Indicators collected from the Longitudinal Employer-Household Dynamics dataset, which allow for disaggregation by sector and worker demographics.[10] From the Quarterly Workforce Indicators data, I obtain county-level quarterly data on employment and average monthly earnings.[11] These metrics capture both employment and earnings opportunities in each offender's local labor market, proxying for the same underlying unobserved demand for ex-offender labor, and are highly correlated. In my estimation sample, the conditional correlation between employment and wages is 0.18.

I obtain all employment and earnings characteristics in the aggregate, by sector, and by sex-education bin. Unlike prior studies, I focus on employment and wages of male workers with less than a high school degree, a low-skilled demographic group most comparable to the population of ex-offenders. In contrast to the demographic and offense characteristics in the NCRP data, these local labor market variables can vary over the course of each offender's non-custody spell.

Because I observe the county in which each prisoner is sentenced at the beginning of the first

[10]Public data from the Quarterly Workforce Indicators are available for all states other than Massachusetts, and for most years between 2000-2013.

[11]Nominal wages are converted to real wages using the CPI.

observable spell, labor market variables are assigned to each prisoner in each month out of custody based on this county. One issue is that my estimates may be biased downwards from selective migration across counties if offenders leave their county of conviction (either by permission or by absconding from supervision) for counties with better local economic conditions. In the full sample of approximately four million released offenders, I observe that among those who recidivate during the sample period, the county of conviction for the new offense differs from the county of the first observed offense 10.1 percent of the time, consistent with national statistics that indicate a nine percent rate of absconding from parole (Bonczar 1999). The estimates should be interpreted with this caveat in mind.

Table 2 presents summary statistics for the estimation sample (35,780,841 observations) during the time period. Between 2000-2013, 50.3 percent of released offenders are white and 39.0 percent are black. 20.2 percent of released offenders are Hispanic and over 86 percent are male. The average age at release is 34.9 years and over half of offenders have less than a high school degree. Only 1.0 percent of released offenders have a college degree. 23.0 percent of offenders in the sample were previously incarcerated for a felony.

The three most common crimes are violent, property, and drug offenses, representing 22.2 percent, 26.4 percent, and 30.7 percent of offenders, respectively. The average number of convicted counts per offender is 1.2, and offenders serve approximately 2.2 years in prison. The most common reason for entrance into prison is a new court commitment, representing 83.7 percent of all offenders. Another 4.6 percent enter prison due to a parole revocation and 8.2 percent enter prison due to a probation revocation. Approximately 28.1 percent of offenders are released under discretionary parole, 19.1 percent released under mandatory parole, and 30.6 percent serve their full sentence in prison. Another 10.6 percent are released via shock probation.

Summary statistics on log employment and average monthly wages for low-skilled men are presented in the bottom panel of Table 2. There is substantial variation in labor market conditions for low-skilled men across counties and time. In the estimation sample, log employment ranged from 2.9 to 12.8, and log average monthly wages ranged from 5.6 to 9.9. County labor market conditions also vary within state. In California, the state with the largest number of released prisoners, log employment for low-skilled men ranged from 4.4 to 12.8, and log average weekly wages ranged from 6.6 to 9.9.

4. Empirical Methodology

To estimate the effect of local labor market conditions on criminal recidivism, I estimate proportional hazard models that allow for an unrestricted baseline hazard in the duration of non-custody spells and that allow for time-varying covariates. This specification has been used widely

in the literature on unemployment duration (Meyer 1990, Card et al. 2007), with analogous discrete duration models used in the literature on welfare exits and reentry (see Hoynes 2000).

In a general duration model, the hazard rate represents the probability of leaving a state in the t^{th} period given continuous participation in that state for the last $t-1$ periods. From this hazard rate, one can construct a duration density distribution and a survivor function, which can be conditioned on various covariates and on initial entry into a state. The parameters of the continuous time duration model are estimated using maximum-likelihood.

Non-custody spells are censored if an individual has still not been recommitted by the end of the sample period for each state. For instance, Arizona provided data from 2000-2012, so all spells are censored as of December 2012. Recall that given that the hazard rate declines rapidly after approximately three years post-release (Figure 1), I censor all spells at 36 months after release in order to focus on the risk of returning to prison in the three years after release.[12]

In particular, I estimate a model with the following specification:

$$h_{itc} = \alpha_t \text{exp}(\beta_1 X_i + \beta_2 L_{tc} + \gamma_t + \delta_c + \epsilon_{itc}) \tag{1}$$

The dependent variable, h_{itc}, is the hazard rate in quarter t for offender i living in county c returning to prison within three years, with each spell beginning in the quarter-year that the offender is first released from prison.[13] α_t denotes the baseline hazard. I estimate α_t using a piece-wise exponential function using dummy variables for each quarter post-release. Under this semi-parametric specification, α_t is assumed constant in each quarter.

X_i includes time-invariant characteristics of each offender at the time of first release: race, ethnicity, age, age squared, highest graded completed, prior felony indicator, main offense type, number of convicted counts, type of prison admission (new commitment, parole violation, etc), type of facility, reason for release, time served, and time served squared. I also include indicators for missing data on each of these time-invariant characteristics.

The independent variables of interest, L_{tc}, include time-varying labor market conditions in quarter t in county c: log employment and log average monthly wages of low-skilled workers. In additional specifications, I separately estimate the effect of employment and wages by sector, demographic characteristics of offenders, and by type of convicted crime.

The coefficient of interest is β_2, which captures the effect of local labor market conditions on the hazard of return to custody. When the independent variable is log employment (or log average wages), β_2 can be interpreted as the elasticity of the hazard rate with respect to employment (or

[12] Results are robust to alternative censoring lengths.

[13] Some offenders have multiple prison spells during the time period, but I analyze only the first return to prison following Hoynes (2000) and Blank and Ruggles (1996). In unreported results, I find similar results using all spells for each offender.

wages), such that a one percent increase in employment (wages) leads to a β_2 percent change in the hazard. Because each offender is observed multiple times in the data to allow for the time-varying nature of economic conditions over the course of the non-custody spell, standard errors are clustered at the offender level. Results are robust to clustering standard errors at the county or state level.

My identification strategy exploits the exact timing of each offender's release from prison. Intuitively, I compare recidivism outcomes of observably similar offenders who have served the same amount of time for the same crime, but who return to counties when labor market conditions are more or less favorable. If the exact timing and county of release is uncorrelated with unobservable characteristics of prisoners, my estimates capture the causal effect of local labor markets conditions on recidivism.

In order to obtain unbiased estimates of the impact of local labor market conditions on recidivism, local labor market conditions must be exogenous. One potential threat to exogeneity is if certain types of offenders with low human capital, job skills, and higher rates of recidivism, are located in areas with poor economic conditions. To account for these time-invariant unobservable characteristics, I control for county fixed effects, δ_c. I also control for time fixed effects, γ_t, to account for factors such as changes in criminal justice policy or welfare changes that may be correlated with both local economic conditions and recidivism. In robustness checks, I also control for additional county variables, and add county-specific time trends to account for trends in recidivism that are unrelated to local labor market conditions. These controls eliminate any omitted county effects or year effects that may bias the estimates.

Another concern is that demand for low-skilled workers, as measured by average wages, may be endogenous to recidivism rates. To identify exogenous variation in low-skilled labor demand, I explore the sensitivity of the main results to controlling for average state wages for low-skilled men in each quarter (excluding own county). Excluding own county from this measure of average wages removes any changes in local wages that may be correlated with unobservable characteristics of workers in the county or county labor supply shocks.

In addition, I consider the impact of low-skilled wages in particular sectors known for a willingness to hire ex-offenders. In the spirit of Bartik (1991), Blanchard and Katz (1992), and Aizer (2010), among others, I interact the average wage for low-skilled men in industry j in the state excluding county c with the proportion of low-skilled men employed in that sector in the initial year 2000 (from the Quarterly Workforce Indicators). Intuitively, this variation captures the fact that state-level increases in wages for workers in a particular industry will lead to larger predicted increases in wages in counties with a higher share of low-skilled men (a proxy for ex-offenders) in those industries. According to Raphael (2010), establishments willing to hire ex-offenders (particularly construction) also hire less educated individuals (high school degree or less) compared to

sectors less willing to hire ex-felons. For instance, the construction industry hires a large number of ex-offenders, and construction wages increased during the housing boom, differentially affecting some counties with very low baseline employment in construction (2 percent) compared to some counties with over 25 percent of low-skilled men employed in construction.

5. Results

5.1. Main Hazard Estimates

Table 3 presents main results where the dependent variable is the hazard rate for returning to prison within three years. A positive coefficient indicates that a variable increases the recidivism risk. I control for the full set of offender demographic characteristics (age at release and its square, race, Hispanic ethnicity, highest grade completed, gender, prior felony status), offense and charge characteristics (most serious convicted offense, number of convicted counts, time served and its square), type of admittance and exit from prison, and the type of facility released from. The specification also includes indicators for missing variables. These variables are time-invariant for each offender. All standard errors are clustered at the individual offender level.

In columns 1 and 2 of Table 3, I control for county log employment of men with less than a high school degree, low-skilled workers most similar to ex-offenders. Local labor market conditions for this demographic group are likely representative of the employment conditions facing released offenders. I allow for local labor market conditions to vary each quarter to capture the fact that released offenders face different conditions at each point in time after release, which may affect the risk of recidivism. Recall that given the functional form on log employment, β_2, the coefficient of interest, can be interpreted as the elasticity of the hazard rate with respect to labor market conditions. With the addition of county fixed effects, β_2 is identified from fluctuations in labor market conditions over time within each county. In columns 3 and 4, I control for county average wages for low-skilled workers to estimate the effect of earnings opportunities on recidivism. Finally, in column 5, I control for both county log employment and average wages of low-skilled men.

Parameter estimates presented for some key offender and offense characteristics in Table 3 indicate that black offenders are significantly more likely to recidivate than similar white offenders (the omitted group) after controlling for observables. Non-Hispanic defendants are more likely to recidivate than similar Hispanic defendants. In contrast, female defendants are significantly less likely to recidivate than male defendants. Recidivism is also decreasing in educational attainment. Compared to the omitted group of offenders with less than an eighth grade degree, offenders with a high school degree, some college, or a college degree, are increasingly less likely to recidivate. Finally, older offenders, those with no prior felony incarceration, and those who have served more

time for the current offense, are less likely to recidivate than their counterparts.

Turning to the local labor market variables, low-skilled employment is negatively associated with the hazard rate. According to column 1, a one percent increase in low-skilled employment reduces the hazard rate by 0.02 percent. This magnitude increase with the addition of county fixed effects. According to column 2, a one percent increase in employment reduces the hazard rate by 0.05 percent.

Higher average wages also reduce the hazard of returning to prison within three years. Both with and without county fixed effects, a one percent increase in average wages reduces the hazard by 0.38 percent to 0.40 percent (columns 3 and 4). These results suggest that ex-offenders are responsive to labor market conditions, particularly earnings prospects in comparison to employment opportunities. In column 5, I control for both employment and earnings of low-skilled men, which are positively correlated.[14] Interestingly, the coefficient on low-skilled employment becomes positive and significant, while the coefficient on low-skilled wages remains large and negative. These results suggest that increases in earnings for low-skilled men are most associated with reductions in recidivism risk. According to column 5, a one percent increase in average wages reduces the recidivism risk by 0.41 percent.

Between 2003-2008, within-county low-skilled employment increased on average by ten percent, and real low-skilled wages increased by four percent. Similarly, on average, county employment increased by ten percent and real wages increased by six percent between 2008-2013, variation that is typical during a standard business cycle (Hoynes 2000). Placing the estimates in context of this variation, a ten percent increase in low-skilled employment reduces the three-year recidivism risk by 0.5 percent. In comparison, a five percent increase in real wages reduces the recidivism risk by approximately 2.1 percent.

In Table 4, I consider the impact of wages in the aggregate and in sectors known for a willingness to hire ex-offenders: manufacturing, construction, and transportation (Holzer et al. 2004, Raphael 2010).[15] To the extent that local labor market conditions affect criminal recidivism through potential labor market employment and earnings opportunities, the results may be present in the job sectors most willing to hire released prisoners. In Panel A, I present results for county-level low-skilled wages. A one percent increase in low-skilled wages in the construction sector reduces the recidivism risk by 0.20 percent, compared to 0.35 percent for low-skilled wages in the manufacturing sector, and 0.09 percent in the transportation sector. These results indicate that wage increases in some of the sectors most likely to hire ex-offenders substantially reduces the risk of recidivism, suggesting that the impact of improved labor market conditions on recidivism may be

[14] A regression of low-skilled employment on average low-skilled monthly wages, controlling for county and year fixed effects, yields a coefficient of 0.180.

[15] See http://articles.latimes.com/2010/nov/30/business/la-fi-felon-jobs-20101130.

driven by the direct employment channel.

As mentioned previously, county-level wages may be endogenous. To identify plausibly exogenous variation in low-skilled labor demand, I control for average state wages for low-skilled men excluding own county, removing any changes in local wages that may be correlated with unobservable characteristics of workers in the county or county labor supply shocks. Results are presented in Panel B. I find that low-skilled wages in all sectors significantly reduces the risk of recidivism, with a one percent increase in wages associated with a 0.69 percent lower hazard. Similarly, a one percent increase in construction wages reduces the recidivism hazard by 0.35 percent, compared to 0.94 percent for manufacturing wages, and 0.41 for transportation wages. These estimates suggest that a five percent increase in real wages, typical variation over the course of a business cycle, reduces the recidivism risk by 1.8 percent to 4.7 percent.

Next, I consider the differential effects of state-level increases in low-skilled wages in each industry by the proportion of low-skilled men employed in those industries as of 2000, the beginning of the sample period. While statistics on the share of ex-offenders employed in each industry is lacking, low-skilled men are a potentially valid proxy for this group. In Panel C, I interact industry state-level wages (excluding own county) with the share of low-skilled men in that county-industry, intuitively capturing the fact that state-level increases in wages for workers in a particular industry will lead to larger predicted increases in wages in counties with a higher share of low-skilled men in those industries. Consistent with this prediction, I find that plausibly exogenous increases in construction wages reduce recidivism disproportionately more in counties with a higher share of low-skilled men employed in the construction sector. Offenders released to counties with a ten percent higher baseline construction share experience an additional 0.19 percent reduction in recidivism risk for a one percent increase in wages. For the transportation sector, an increase in wages also has a larger effect on recidivism in those counties with a higher baseline share of low-skilled men employed in transportation, with a one percent increase in wages reducing the recidivism risk by an additional 0.18 percent for counties with a ten percent higher baseline share. In contrast, the impact of increases in manufacturing wages on recidivism risk does not appear to vary significantly with baseline employment shares in manufacturing.

5.2. Treatment Heterogeneity

In Table 5, I estimate hazard models controlling for low-skilled employment and average wages separately by several offender characteristics to assess whether certain types of offenders are more sensitive to local labor market fluctuations. The specifications also control for the full set of demographic and offense characteristics, as well as county and year fixed effects. In column 1 of Table 5, I replicate the main findings from column 5 of Table 3 for the full sample of released offenders.

In columns 2 and 3, I separately estimate the main specification for white offenders and black offenders, respectively. The impact of low-skilled employment is negative for white offenders, in contrast to positive for black offenders. These results suggest that holding low-skilled wages constant, the impact of additional low-skilled jobs reduces the recidivism risk of white offenders but increases the recidivism risk of black offenders. In contrast, I find that increases in low-skilled wages in the local labor market reduce recidivism risk significantly more for black offenders than for white offenders. A one percent increase in low-skilled wages reduces the hazard rate by 0.29 percent for white offenders, compared to 0.57 percent for black offenders. These results are interesting given a literature that finds that employers are less willing to hire black ex-offenders compared to similar white ex-offenders (see Pager 2003), potentially explaining the oppositely signed coefficients on low-skilled employment. If the impact of wages on recidivism risk is coming from the direct employment channel, this pattern of findings suggests that conditional on earning wages, black offenders are more responsive to increases in wages that similar white offenders.

In columns 4 through 6, I estimate the main specification separately for offenders in different age groups. I find evidence that older ex-offenders are more responsive to increases in earnings prospects. Offenders aged 40 and up experience the largest reduction in recidivism risk with increases in low-skilled wages, potentially due to employer preference for these older offenders with the lowest average recidivism risk. A one percent increase in low-skilled wages reduces the recidivism risk by 0.35 percent for offenders aged 25 or under, 0.39 percent for offenders aged 25 to 40, and by 0.49 percent for offenders released at age 40 or older.

Differences in the effect of local labor market conditions also appear by prior criminal history. In columns 7 and 8, I find evidence indicating that first-time offenders are much more responsive to changes in low-skilled wages than those with a prior felony. Among those without a prior felony, a one percent increase in wages reduces the recidivism risk by 0.67 percent, compared to 0.19 percent for those with a prior felony. Again, if the impact of wages on recidivism occurs through its effect on employment, these results may reflect an employer preference for first-time versus repeat offenders.

In Table 6, I present additional subsample results by crime type. I find evidence that offenders convicted of violent, property, and drug crimes are equally responsive to changes in low-skilled wages, with a one percent increase in wages reducing the recidivism risk by 0.45 percent.

Finally, I combine all these characteristics into a single risk index to test for heterogeneous results. First, I estimate a hazard model controlling for the full set of offender, crime, and prison admission/release characteristics. I use a split-sample estimator to predict recidivism risk in a five percent random sample to avoid the bias that arises from endogenous stratification (Abadie et al. 2014). I then use these estimates to construct a predicted ex ante risk of recidivating in the other 95 percent of my estimation sample. I divide offenders into above and below median

risk of recidivating during the sample period, with those in the below median group having a 14.2 percent probability of recidivating in three years post-release compared to 36.8 percent for the above median group. In Appendix Table 2, I present the main hazard model results separately for these two groups of offenders. I find that low-skilled employment and wage increases have a larger effect on the recidivism risk of lower-risk offenders compared to higher-risk offenders.

5.3. Alternative Specifications

In Table 7, I test the robustness of the main results under several alternative specifications. In column 1, I replicate the main results from the preferred specification controlling for low-skilled employment and wages (column 5 from Table 3). In column 2, I add county-specific linear trends to the preferred specification, such that my estimates are identified from deviations from county trends. The results are essentially unchanged with the addition of these trends, suggesting that differential trends in recidivism across counties cannot explain the findings.

In column 3, I add additional county-year controls for county population, per capita personal income, and personal current transfer receipts, obtained from the Bureau of Economic Analysis (BEA). Personal current transfer receipts are benefits received by persons for which no current services are performed.[16] Results are qualitatively similar with the addition of these county-level controls.

In columns 4 and 5, I explore whether I find similar results splitting the sample before and after the Great Recession. Given the housing and related construction boom pre-2007, and subsequent bust post-2007, the availability of low-skilled jobs differed greatly across these two time periods. I replicate the main specification separately for offenders released from 2000-2006 (column 3) and those released from 2007-2013 (column 4). I find similar results across both time periods, but with the wage effects on recidivism almost twice as large during the housing boom compared to the bust.

Next, I analyze two main sources of bias. One potential concern is if the timing of release from prison is correlated with local economic conditions. For instance, state parole boards may let out certain lower-risk prisoners earlier during worse economic times. If these early release prisoners also have a lower propensity to recidivate, I may underestimate the effects of local economic conditions on recidivism. However, this bias is unlikely for two reasons. First, the only state with an official early release policy in the last decade is California, whose Realignment program did not begin until the end of my sample. Absent an official policy, parole boards are not authorized to consider economic conditions in making parole decisions. Second, many states eliminated parole, and thus discretionary release, prior to 2000.

[16] According to the BEA, transfer receipts accounted for almost 15 percent of total personal income at the national level in 2005.

Nevertheless, to test the magnitude of this potential bias, I replicate my main results on a subsample of states in which there is no discretion in prison release date. As of the beginning of my sample period, 16 states had abolished discretionary parole for almost all offenders: Arizona, California, Delaware, Florida, Illinois, Indiana, Kansas, Maine, Minnesota, Mississippi, North Carolina, Ohio, Oregon, Virginia, Washington, and Wisconsin.[17] NCRP prison spell data are available for all these states except for Virginia. In column 6 of Table 7, I replicate the preferred specification in these 15 states with no discretionary parole. I find similar and larger results in this sample of states, with a one percent increase in wages reducing the recidivism risk by 0.70 percent, suggesting that correlation between the timing of release and economic conditions may underestimate the true impacts.

A second potential source of bias comes from the unobservable propensity to commit crime during different economic conditions. An offender who commits an offense during good economic times is likely unobservably different from one who commits an offense during bad economic times. If local conditions affect the initial entry into prison of individuals with particular unobserved characteristics, this selection could affect my estimates in two ways. First, offenders who commit a crime may be forward-looking and anticipate the local economic conditions in the year of prison release, but this is unlikely among a population of offenders. Second, even if offenders are not forward-looking, there may be correlation between economic conditions when an offender commits a crime and when he is released from prison.[18]

To partially test for this source of bias, I explore whether correlation in local labor market conditions at the time of the offense and the time of release can explain my results. In column 7 of Table 7, I replicate the main specification but add additional controls for employment and average wages both in the quarter-year of admission to prison, and lagged one year to account for the delay from offense commission to prison admission.[19] If correlation is driving my estimates, then current labor market conditions upon release should have no impact on the risk of recidivism after controlling for conditions during and before admission to prison. I find that current employment growth and wages are still highly predictive of recidivism risk even after controlling for historical conditions, suggesting that correlation between past and current labor market conditions cannot fully account for my findings.

Finally, in column 8, I replicate the main specification on the sample of 18 states that provided data for the full sample period from 2000-2013 to explore whether selective reporting biases the

[17] See http://www.bjs.gov/content/reentry/releases.cfm.

[18] Generally, correlation of unemployment rates fades after three years (Oreopoulos et al. 2012). In my estimation sample, the average time served in the data is 2.3 years once a defendant enters prison. Correlation is further mitigated because the most relevant date is not when the offender enters prison but when the crime is committed.

[19] Unfortunately, the NCRP data do not contain information on when the offense was committed, or when the prisoner was arrested. However, the BJS estimates that the median time from arrest to sentencing for felony convictions in state court was 265 days in 2006. See http://www.bjs.gov/content/pub/pdf/fssc06st.pdf.

findings. I find very similar results in this subsample, with a one percent increase in low-skilled wages associated with a 0.32 percent decline in the recidivism risk.[20]

5.4. Mechanisms

In this section, I explore the differential impact of local labor markets on recidivism using cross-state differences in generosity or friendliness towards ex-offenders. I collect information on state laws from The Sentencing Project, the Pew Charitable Trusts, and the Legal Action Center.[21] For instance, the majority of states have full or partial bans on food stamps and welfare benefits for drug felons. The availability of welfare may be critical to preventing recidivism given the limited economic resources for ex-offenders (Harding et al. 2014).[22] States also differ in whether there are legal restrictions on the ability of private employers to discriminate against ex-offenders. During the sample period, six states in the sample (Kansas, Montana, New York, Ohio, Pennsylvania, and Wisconsin) had passed laws restricting the ability of private employers to discriminate on the basis of criminal history unless there is a relationship between the prior conviction and job sought. In all other states during the time period, employers could deny employment to ex-offenders for no reason at all.

In columns 1 and 2 of Table 8, I present results separately for those states with any form of welfare ban on ex-felons and those without any welfare ban. In columns 3 and 4, I present results separately for states with and without food stamp bans for ex-offenders. Finally, in columns 5 and 6, I explore the results separately by states with and without laws that prohibit private employers from discriminating on the basis of criminal history.

I find that the effect of wages on recidivism is substantially larger in those states with welfare bans for ex-offenders, with a one percent increase in wages reducing the recidivism risk by 0.53 percent, compared to 0.15 percent in states with no welfare ban. Similarly, I find that the impact of employment and wages on recidivism is also larger in those states with food stamp bans compared to states without food stamp bans. In states with bans on food stamp eligibility for ex-offenders, a one percent increase in employment reduces the recidivism risk by 0.15 percent and a one percent increase in wages reduces the recidivism risk by 0.45 percent, compared to 0.18 percent in states

[20] In unreported results, I find similar results estimating a discrete hazard model under a logit specification, and estimating an ordinary least squares specification for the probability of returning to custody within three years of release controlling for economic conditions in the first quarter post-release.

[21] See http://www.pewtrusts.org/en/research-and-analysis/blogs/stateline/2015/07/30/states-rethink-restrictions-on-food-stamps-welfare-for-drug-felons.

[22] James Cannon Jr., an ex-offender who now works as an advocate against felon discrimination, said he received $200 a month in food stamps in 2009 and 2010, after his release. Fresh from a stint in a Minnesota workhouse, he applied to no fewer than 80 jobs, he said. Before long, he'd gotten 80 rejections. Only Taco Bell bothered to interview him. "Food stamps was the only option," he said. "That was something I never had to do in the past." See http://www.huffingtonpost.com/2013/07/11/food-stamps-felons_n_3574412.html.

with no ban. The larger results in states with bans on welfare or food stamps imply that earnings opportunities are even more critical in states where ex-offenders have limited alternative resources for economic self-sufficiency. The findings suggest that ex-offenders are responsive to economic incentives likely through a combination of employment and public assistance.

Finally, I find that the impact of higher wages on reducing recidivism is larger in states that allow private employers to discriminate based on criminal history, but that the impact of employment is larger in states that prohibit discrimination. In states that prohibit discrimination, a one percent increase in employment reduces the recidivism risk by 0.34 percent. The employment effects may reflect the higher probability of getting a job when employers cannot deny employment to ex-offenders for no reason.

6. Conclusion

This paper estimates the impact of local labor market conditions on criminal recidivism using administrative prison data on over four million released offenders from 43 states and over 2,800 counties from 2000-2013. As measures of local labor market opportunities, I obtain information on quarterly county-level employment and wages. Estimating the transition back into prison using hazard models, I find that ex-offenders are responsive to both local employment growth and increases in wages experienced upon release from prison. The typical wage growth during a business cycle decreases the risk of recidivism by 2.0 to 4.7 percent. These results are robust to unobserved county differences and the selection of high-risk individuals into bad neighborhoods.

How much can these estimates be explained by the direct employment channel? My findings measure the effect of increases in low-skilled wages on recidivism, which incorporate the impact of increases in wages on the probability of being employed, and the impact of employment on recidivism. Sabol (2007) finds that among ex-offenders, a one percentage point increase in the unemployment rate decreases the likelihood of being employed by five percentage points. Holzer and Offner (2002) find that a one percentage point increase in the local area unemployment rate decreases the probability that less-educated black males are employed by 2.7 percentage points.

Taking these estimates and assuming that a one percentage point decrease in the unemployment rate is associated with a 0.7 percent increase in real wages (Federal Reserve Bank of Chicago 2014), a one percentage point increase in real wages increases the probability of employment by 3.9 to 7.1 percent. Scaling my reduced form estimates by this elasticity implies that obtaining employment reduces the recidivism risk by 5.8 to 10.5 percent. These findings are largely consistent with prior work, which has found that obtaining employment as an ex-offender reduces the hazard of returning to prison by 17 percent (Tripodi et al. 2010), or that summer youth employment reduces the probability of incarceration by 10.3 percent (Gelber et al. forthcoming). As a result,

the magnitudes of my findings are consistent with a direct employment channel of improved local labor markets.

Overall, the findings suggest that the large number of ex-offenders released during the Great Recession likely had substantial consequences for recidivism, particularly because of contractions in industries traditionally open to hiring ex-offenders, such as manufacturing and construction.[23] Between the fourth quarter of 2007, and the first quarter of 2009, real average monthly earnings for low-skilled men fell by 12 percent, with real wages in the construction and manufacturing sectors falling by 19 percent and 12 percent, respectively. Even by the first quarter of 2013, real wages for low-skilled men remained depressed, still 4.5 percent less than in the fourth quarter of 2007. The estimates in this paper suggest that compared to a counterfactual in which real wages remained constant during the time period, the recidivism risk of offenders released during the Great Recession increased by 5.0 to 7.8 percent. With approximately one million offenders released during the Recession, the heightened recidivism rate during depressed economic times may account for an additional 50,000 offenders returning to prison within the coming years.[24] Among those offenders who recidivate, the average time served upon return to prison is over one year. With an average cost of $30,000 to house an inmate in state prison, these offenders may entail over $1.5 billion in costs, in addition to decreases to public safety. These results suggest that prisoner reentry policies should be paired with employment opportunities, particularly during hard economic times.

One limitation of this paper is that it only measure the impact of improved local labor markets on recidivism. Improved employment and earnings prospects likely also impact individual health and other non-criminal outcomes, as well as outcomes for families and communities. In addition, this paper focuses only on the impact of labor market conditions on reentry. Future work analyzing the effect of other components of successful prisoner reintegration, such as access to housing and public assistance, is critical.

[23] "We have a record high number of people coming out of prison each year into the highest rate of unemployment since the Great Depression," said Marc Mauer of the nonprofit Sentencing Project. "As difficult as the recession has been on people, it's twice as difficult for people with a felony to make it in this economy." See http://articles.latimes.com/2010/nov/30/business/la-fi-felon-jobs-20101130.

[24] I assume that the Great Recession began in December 2007 and ended in June 2009 and that the annual number of released offenders is 650,000.

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Table 1. Distribution of Time Until Return to Prison

		Probability of Return to Prison in			
	No. of Obs	$\leq$ 1 Year	$\leq$ 2 Years	$\leq$ 3 Years	$\leq$ 5 Years
All Prisoners	4,084,436	0.145	0.225	0.266	0.301
Demographics					
White	1,925,624	0.138	0.214	0.252	0.286
Black	1,503,502	0.147	0.238	0.286	0.328
Hispanic	706,979	0.139	0.200	0.229	0.251
Male	3,551,407	0.150	0.233	0.275	0.312
Female	532,011	0.113	0.172	0.201	0.229
Age Under 25	832,112	0.204	0.310	0.361	0.404
Age 25-40	1,991,006	0.142	0.224	0.266	0.303
Age Over 40	1,260,900	0.110	0.171	0.203	0.231
Less HS Degree	1,344,320	0.136	0.225	0.274	0.319
HS Degree	1,081,698	0.125	0.198	0.236	0.272
College Degree	27,808	0.076	0.122	0.148	0.178
Prior Felony Incarceration	670,950	0.152	0.229	0.269	0.305
No Prior Felony	2,171,954	0.140	0.219	0.260	0.295
Type of Offense					
Violent Offense	909,154	0.134	0.211	0.252	0.286
Property Offense	1,132,165	0.177	0.267	0.310	0.347
Drug Offense	1,178,231	0.130	0.208	0.249	0.284
Reason for First Prison Spell Admittance					
Court Commitment	3,322,518	0.135	0.212	0.251	0.286
Parole Revocation	203,344	0.209	0.325	0.379	0.422
Probation Revocation	328,048	0.192	0.290	0.339	0.382
Reason for First Prison Spell Release					
Discretionary Parole	1,188,865	0.166	0.260	0.302	0.334
Mandatory Parole	770,370	0.236	0.336	0.382	0.414
Shock Probation	418,513	0.125	0.217	0.265	0.307
Expiration of Sentence	1,074,312	0.049	0.101	0.138	0.180

Notes: This table presents descriptive statistics for the unconditional probabilities of returning to prison by demographic characteristics for the full sample of prisoners released between 2000-2013 in 43 states.

Table 2. Summary Statistics of Prisoners Released 2000-2013

Variable	Mean	SD
NCRP Data		
White	0.503	0.500
Black	0.392	0.488
Hispanic	0.201	0.401
Male	0.865	0.342
Female	0.135	0.342
Age at Release	34.946	10.815
Less HS Degree	0.516	0.500
HS Degree	0.405	0.491
Some College	0.064	0.244
College Degree	0.011	0.103
Prior Felony Incarceration	0.230	0.421
Violent Offense	0.224	0.417
Property Offense	0.268	0.443
Drug Offense	0.300	0.458
Number of Counts	1.226	1.309
Time Served (Years)	2.231	3.458
Court Commitment	0.839	0.368
Parole Revocation	0.048	0.214
Probation Revocation	0.079	0.269
Discretionary Parole	0.282	0.450
Mandatory Parole	0.191	0.393
Shock Probation	0.106	0.307
Expiration of Sentence	0.310	0.462
Labor Market Variables (in Logs)		
Low-Skilled Employment	9.028	1.898
Low-Skilled Wages	7.208	0.135
Low-Skilled Construction Employment	6.792	1.933
Low-Skilled Construction Wages	7.336	0.195
Low-Skilled Manufacturing Employment	7.144	1.874
Low-Skilled Manufacturing Wages	7.370	0.184
Low-Skilled Transportation Employment	6.061	2.081
Low-Skilled Transportation Wages	7.265	0.176

Notes: This table presents summary statistics on the full sample of released prisoners from 2000-2013 from 43 states. The dataset contains one observation for each quarter transition in the non-custody spell.

Table 3. Main Results

	(1)	(2)	(3)	(4)	(5)
Black	0.109^{***}	0.159^{***}	0.099^{***}	0.159^{***}	0.159^{***}
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Not Hispanic	0.404^{***}	0.219^{***}	0.414^{***}	0.220^{***}	0.220^{***}
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
Female	-0.291^{***}	-0.310^{***}	-0.291^{***}	-0.309^{***}	-0.309^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
HS Degree	-0.027^{***}	-0.078^{***}	-0.024^{***}	-0.078^{***}	-0.078^{***}
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Some College	-0.098^{***}	-0.150^{***}	-0.097^{***}	-0.150^{***}	-0.150^{***}
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
College Degree	-0.261^{***}	-0.296^{***}	-0.259^{***}	-0.296^{***}	-0.296^{***}
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Age at Release	-0.041^{***}	-0.044^{***}	-0.042^{***}	-0.044^{***}	-0.044^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
No Prior Felony	-0.290^{***}	-0.465^{***}	-0.295^{***}	-0.466^{***}	-0.466^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Time Served (Years)	-0.008^{***}	-0.008^{***}	-0.008^{***}	-0.009^{***}	-0.009^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Labor Market Variables					
Log Low-Skill Emp.	-0.021^{***}	-0.053^{***}			0.048^{***}
	(0.001)	(0.014)			(0.015)
Log Low-Skill Wage			-0.378^{***}	-0.396^{***}	-0.413^{***}
			(0.008)	(0.019)	(0.020)
Observations	34,911,191	34,911,191	34,916,231	34,916,231	34,911,191
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	No	Yes	No	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate regression. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

Table 4. Results by Sector

	All Sectors	Construction	Manufacturing	Transportation
	(1)	(2)	(3)	(4)
Panel A: County Wages				
Log Low-Skill Wage	-0.396^{***}	-0.204^{***}	-0.353^{***}	-0.089^{***}
	(0.019)	(0.010)	(0.014)	(0.011)
Panel B: State Wages				
Log State Low-Skill Wage	-0.693^{***}	-0.354^{***}	-0.944^{***}	-0.412^{***}
	(0.023)	(0.013)	(0.023)	(0.020)
*Panel C: State Wages*Share*				
Log State Low-Skill Wage		-0.144^{***}	-1.329^{***}	-0.396^{***}
		(0.036)	(0.060)	(0.045)
Log State Low-Skill Wage*Share		-1.919^{***}	0.087	-1.787^{***}
		(0.284)	(0.233)	(0.654)
Observations	34,916,231	34,903,356	34,850,568	34,848,520
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

Table 5. Results by Offender Demographics

	All	White	Black	< 25	25 to 40	> 40	Prior Felony	No Prior
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Low-Skill Emp.	0.048^{***}	-0.042^{**}	0.133^{***}	0.093^{***}	0.044^{**}	0.078^{**}	0.016	0.190^{***}
	(0.015)	(0.020)	(0.027)	(0.028)	(0.022)	(0.033)	(0.039)	(0.021)
Log Low-Skill Wage	-0.413^{***}	-0.290^{***}	-0.571^{***}	-0.351^{***}	-0.393^{***}	-0.488^{***}	-0.194^{***}	-0.674^{***}
	(0.020)	(0.027)	(0.034)	(0.036)	(0.028)	(0.041)	(0.051)	(0.028)
3 Yr Recidivism	0.267	0.252	0.286	0.361	0.266	0.203	0.269	0.260
Observations	34,911,191	16,522,968	12,949,879	6,575,260	17,057,789	11,275,527	5,542,220	18,717,021
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

Table 6. Results by Crime Type

	Violent	Property	Drug
	(1)	(2)	(3)
Log Low-Skill Emp.	0.214^{***}	0.030	0.076^{**}
	(0.034)	(0.026)	(0.031)
Log Low-Skill Wage	-0.459^{***}	-0.426^{***}	-0.445^{***}
	(0.043)	(0.034)	(0.038)
3 Yr Recidivism	0.252	0.310	0.290
Observations	7,728,421	9,330,878	10,476,178
Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

Table 7. Alternative Specifications

	Baseline Results	County Trends	County Controls	Released < 2007	Released $\geq$ 2007	No Parole	Lags Admittance	Balanced Panel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Low-Skill Emp.	0.048^{***}	0.052^{**}	0.091^{***}	-0.015	0.022	0.169^{***}	0.069^{***}	0.149^{***}
	(0.015)	(0.023)	(0.016)	(0.024)	(0.033)	(0.023)	(0.016)	(0.019)
Log Low-Skill Wage	-0.413^{***}	-0.333^{***}	-0.337^{***}	-0.581^{***}	-0.300^{***}	-0.701^{***}	-0.366^{***}	-0.315^{***}
	(0.020)	(0.022)	(0.020)	(0.028)	(0.030)	(0.033)	(0.020)	(0.024)
Observations	34,911,191	34,911,191	34,911,191	16,764,237	18,146,954	14,297,633	34,372,166	18,431,361
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Column 1 replicates the main estimates from column 5 in Table 3. Column 2 adds county-specific linear trends. Column 3 adds county-year controls for population, per capita personal income, and personal current transfer receipts. Column 4 estimates the main specification on offenders released between 2000-2006. Column 5 estimates the main specification on offenders released between 2007-2013. Column 6 estimates the main specification on offenders released in 15 states with no parole. Column 7 estimates the main specification adding controls for labor market conditions in the quarter-year of prison admission and lagged one year before admission. Column 8 estimates the main specification on offenders released in 18 states that provided data from 2000-2013. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

Table 8. Results by State Laws

	Welfare Ban		Food Stamp Ban		Private Discrimination	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Log Low-Skill Emp.	0.007	0.351^{***}	-0.152^{***}	0.133^{***}	0.032^{**}	-0.339^{***}
	(0.016)	(0.044)	(0.018)	(0.031)	(0.016)	(0.072)
Log Low-Skill Wage	-0.532^{***}	-0.154^{***}	-0.448^{***}	-0.179^{***}	-0.523^{***}	-0.149^{**}
	(0.022)	(0.044)	(0.026)	(0.029)	(0.021)	(0.060)
Observations	27,560,307	7,350,884	23,196,729	11,714,462	30,906,942	4,004,249
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Specifications include demographic, offense, and prison admittance and entry characteristics. Standard errors are clustered at the offender level.

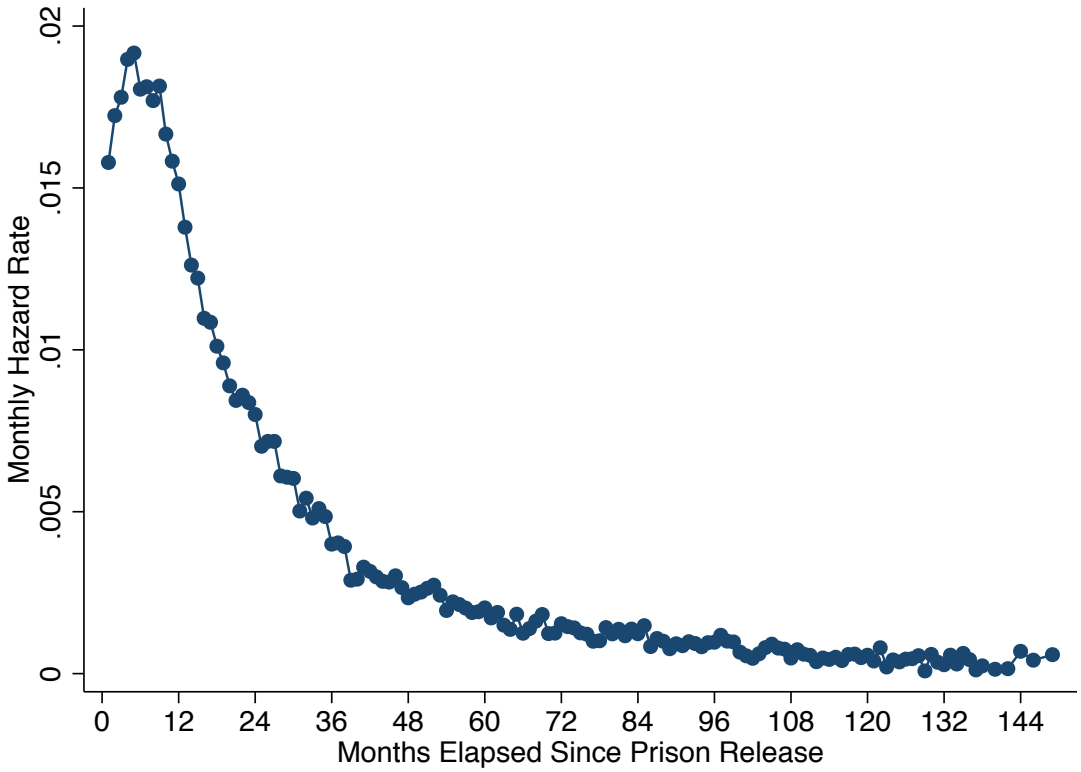

Figure 1. Hazard for Probability of Returning to Prison

Notes: Data are from the NCRP for prisoners released from 2000-2013.

Appendix Table 1. States in NCRP Data on Prison Spells

State	Years Data Provided
Alabama	2007-2013
Alaska	2005-2012
Arizona	2000-2012
California	2000-2013
Colorado	2000-2013
Delaware	2009-2013
Florida	2000-2013
Georgia	2000-2013
Idaho	2008-2012
Illinois	2000-2003
Indiana	2002-2013
Iowa	2002-2013
Kansas	2011-2013
Kentucky	2000-2013
Maine	2012-2013
Maryland	2000-2012
Massachusetts	2010-2013
Michigan	2000-2013
Minnesota	2000-2013
Mississippi	2004-2013
Missouri	2000-2013
Montana	2010-2013
Nebraska	2007-2013
Nevada	2009-2013
New Hampshire	2011-2013
New Jersey	2003-2013
New Mexico	2010-2013
New York	2000-2013
North Carolina	2000-2013
North Dakota	2002-2013
Ohio	2009-2013
Oklahoma	2000-2013
Oregon	2001-2013
Pennsylvania	2001-2013
Rhode Island	2004-2013
South Carolina	2000-2013
South Dakota	2000-2012
Tennessee	2000-2013
Texas	2005-2013
Utah	2000-2013
Washington	2000-2013
West Virginia	2000-2013
Wisconsin	2000-2013
Wyoming	2006-2013

Notes: This table lists the states and years available in the NCRP data.

Appendix Table 2. Results by Risk Index

	Below Median	Above Median
	(1)	(2)
Log Low-Skill Emp.	-0.139^{***}	0.014
	(0.028)	(0.018)
Log Low-Skill Wage	-0.429^{***}	-0.375^{***}
	(0.041)	(0.023)
3 Yr Recidivism	0.14	0.37
Observations	16,528,126	16,626,015
Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes

Notes: This table presents proportional hazard estimates for the full sample of prisoners released between 2000-2013 in 43 states. Each column represents a separate specification. Predicted risk is estimated in a five percent split sample, as described in the text. Standard errors are clustered at the offender level.