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Good Jobs and Recidivism

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I estimate the impact of employment opportunities on recidivism among 1.7 million offenders released from a California state prison between 1993 and 2009. The institutional structure of the California criminal justice system as well as location-, skill-, and industry-specific job accession data provide a unique framework to identify a causal effect of labor demand on criminal behavior. I find that increases in construction and manufacturing employment opportunities at the time of release are associated with significantly lower recidivism rates. Other types of employment opportunities, including those typically accessible to individuals with criminal records but characterized by much lower wages, do not influence recidivism rates. My results illustrate the importance of considering job quality when estimating the impact of employment opportunities on crime and when designing programs to help former inmates successfully reenter noninstitutionalized society.

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1 Introduction

Prisons in the United States are built with revolving doors—more than two-thirds of individuals released from prison in California recidivate (return to prison) within 3 years. The scale of incarceration in the United States is largely driven by the failure of former inmates to successfully reenter noninstitutionalized society. Released offenders undoubtedly face a number of social, housing, and financial challenges upon leaving prison, and an inability to obtain employment is often cited as an important factor that contributes to recidivism. It is an area of increasing concern among policymakers; nearly 40 percent of the $125 million appropriated for the *Second Chance Act Prisoner Reentry Initiative* in 2009 and 2010 was spent on programs to help released prisoners obtain employment (GAO, 2011).[1]

A great deal of empirical evidence supports basic theoretical predictions of a negative relationship between employment opportunities and criminal activity.[2] We may also expect local labor markets to influence released prisoners based on results from an emerging literature that documents long-term detrimental effects for those who enter more depressed local labor markets upon graduating high school or college (Kahn, 2010; Oreopoulos et al., 2012; Maclean, 2013; Cutler et al., 2014). Recently, Bell et al. (2014) estimated higher rates of life time crime and incarceration among those leaving high school during recessions in the US and UK. Surprisingly, prior research does not find strong ties between labor market conditions at the time of prison release and recidivism rates (Bolitzer, 2005; Raphael and Weiman, 2007; Raphael, 2010).[3] Moreover, recent evaluations of reentry programs in which minimum-wage jobs are randomly assigned to released offenders find mixed results as to whether these employment opportunities can reduce recidivism (Redcross et al., 2011; Jacobs, 2012).[4]

Do labor market opportunities matter for the 700,000 individuals exiting prison in the United States each year?[5] This article provides evidence that jobs do matter, but not all jobs are created equal. There are two important reasons it is necessary to distinguish between different types of job opportunities. First, a large portion of employment opportunities are not accessible to released offenders due to factors such as education requirements and employer reluctance to hire applicants with criminal records. Figure 1 displays the distribution of private-sector workers who had been recently incarcerated across industries.[6] Since legitimate employment

[1] In a recent book, Raphael (2014) provides a detailed discussion of labor market challenges for individuals with criminal records.

[2] Mustard (2010) and Bushway (2011) provide recent reviews of a large empirical literature within economics and criminology. The standard economic model of criminal behavior predicts that if released offenders make choices between labor, leisure, and crime, then an increase in job availability should decrease the amount of time devoted to criminal behavior since the opportunity cost of time spent in both criminal activity and in prison if caught rises (Becker, 1968; Ehrlich, 1973). Life course theories from the sociology and criminology literature emphasize employment as a turning point in the life of an ex-convict that reduces criminal behavior by encouraging non-criminogenic social ties (Laub and Sampson, 1993; Uggen, 2000).

[3] Bushway (2011) reviews research that has consistently found that "the average criminal is not very responsive to work incentives."

[4] While a program offered by the Center for Employment Opportunities (CEO) in New York City found reductions in future criminal activity for offenders randomly assigned transitional jobs (Redcross et al., 2011), the Transitional Jobs Reentry Demonstration (TJRD) implemented in Chicago, Detroit, Milwaukee, and St. Paul did not find differences in future criminal activity (Jacobs, 2012).According to Raphael (2014), "the juxtaposed evaluation of relatively similar intervention suggests that with regard to transitional job programs, the jury is still out."

[5] According to the "Prisoners in 2010" report prepared by the Bureau of Justice Statistics, 708,677 prisoners were released from state and federal correctional facilities during 2010 (Guerino et al., 2012).

[6] Using data from the National Longitudinal Survey of Youth 1979 (Bureau of Labor Statistics, 2011), I estimate that nearly 90 percent of the individuals recently released from jail or incarceration who were employed within

opportunities for former offenders are heavily concentrated in a small number of industries, it is not surprising that researchers using local unemployment rates to measure employment opportunities find small and/or insignificant effects. Aggregate fluctuations do not accurately measure changes in labor market conditions relevant to low-education individuals searching for work with a criminal record.

Second, within the limited set of employment opportunities accessible to this population, certain jobs are clearly superior to others. Figure 2 displays average monthly earnings for recently hired low-skill (high school diploma or less) employees in California—workers can expect to earn 33 to 100 percent more in construction or manufacturing than in retail or food services. In theory, a construction or manufacturing job opportunity should deter more crime than one with lower expected wages. A renewed focus on the quality of jobs for workers with low levels of educational attainment reveals a significant fraction of high-quality jobs in the construction and manufacturing industries (Holzer et al., 2011). These *good* jobs are distinguished from other types of jobs by higher levels of pay, benefits, and permanence. Further highlighting the relevance of these two industries, recent research suggests that fluctuations in the construction and manufacturing industries can explain most of the growth in unemployment among men without any post-secondary education in the United States (Charles et al., 2013). My results demonstrate that these two industries could also be very important determinants of crime rates, especially among former inmates.[7]

In this article, I link outcomes for over 1.7 million parole supervision terms in California with measures of employment opportunities at the time and location of labor market entry. Labor market data recording the number of low-skill individuals hired (job accessions) within each industry and county allow estimation of heterogeneous effects across different types of employment opportunities. The rigid institutional features of the California criminal justice system provide a setting in which the timing and location of release from prison are exogenous to variation in local labor market conditions. This setting eliminates two important concerns common to all prior studies that estimate the relationship between labor markets and crime: first, my estimates are not biased by a reverse-causal relationship between criminal activity and labor market conditions; and, second, the parole location restrictions mitigate any migration bias.

Overall, I find that the existence of low-skill manufacturing and construction employment opportunities at the time of labor market entry is associated with significant declines in the number of released offenders who return to prison. A one-standard deviation in the number of workers starting a new job in construction is associated with a 2.2 percent decrease in the number of released inmates returning to prison within a year; a one-standard deviation in manufacturing new hires is associated with a 1.36 percent decrease in one-year return rates. By contrast, low-skill employment opportunities in low-wage industries (such as retail and food services) or opportunities requiring higher levels of education do not have similar effects on offender behavior. Effects are largest among drug offenders and individuals released

a year worked within six industries: construction, food services, retail, manufacturing, administrative services and waste management, and other services. Other services includes occupations such as auto mechanics, hairdressers, laundry workers, and repair workers. I report private sector industry concentration since the labor market flow data used in my analysis is only available for private sector employers in California during my time period of analysis.

[7] Two recent papers in the criminology literature have investigated whether racial differences in recidivism rates can be attributed to racial differences in manufacturing job opportunity (Wang et al., 2010; Bellair and Kowalski, 2011). Using a Cox proportional hazards model, Bellair and Kowalski (2011) find that lower availability of manufacturing jobs in areas where black offenders are released can explain much of the racial differences in recidivism for a sample of 1,568 offenders released in Ohio during the first six months of 1999.

between the ages of 35 and 45. Several robustness or "falsification" tests support a causal interpretation of my results: I find no significant effect from increases in manufacturing and construction hires just prior to release; my results are robust to the inclusion of local crime rates as well as the inclusion of a lagged dependent variable; and, I find no effect of these male-dominated opportunities on female recidivism rates. These results help to eliminate concerns of any bias from unobserved determinants of offender behavior that could be correlated with manufacturing- and construction-specific labor demand at the time of release.

My estimates suggest that any rise in recidivism rates as a result of the 30-percent decline in low-skill manufacturing employment between 1993 and 2006 (as depicted in Figure 3) in California was partially offset by the 60-percent growth in construction employment. Specifically, my estimates show, holding all other factors constant, that the proportion of released offenders returning to prison within one year would have been over 12 percent higher had there been no growth in construction jobs during the past two decades, and 10 percent lower had there been no decline in low-skill manufacturing jobs.[8] Figure 4 plots recidivism rates over my time period of analysis as well as the share of low-skill employment in *good* jobs (construction and manufacturing) and that in retail and food services. Around 2004 lower-wage jobs in retail and food service become a larger share of the low-skill labor force than construction and manufacturing which coincides with an upward trend in recidivism rates.

While my results are specific to individuals released from prison in California, they contribute to research that estimates the relationship between local labor markets and local crime rates in three important ways. First, my results add to the economics literature by helping explain the large disparity between standard OLS and IV (instrumental variable) estimates in recent work. Most studies estimating the relationship between crime rates and unemployment rates typically find a modest relationship in standard OLS regression models, but find estimates from instrumental variable (IV) models two to three times larger than the OLS estimates.[9] All of the studies finding large IV estimates use an interaction between manufacturing employment and a macroeconomic shock to provide exogenous variation in local labor market conditions (Raphael and Winter-Ebmer, 2001; Öster and Agell, 2007; Lin, 2008).[10] Through choosing an instrumental variable involving an interaction between manufacturing employment and a macroeconomic shock, these studies each estimate a parameter measuring the effect of unemployment on the criminal behavior of individuals affected by a shock to the manufacturing sector.[11] My results suggest that released offenders are sensitive to fluctuations in employment

[8]Figure 3 shows a decline from 3.5 percent of California's working-age population employed in low-sill manufacturing in 1993 to 2.5 percent in 2006 prior to the Great Recession. The percent of the working-age population employed in low-skill construction increased from 0.97 in 1993 to 1.6 in 2006. Extrapolating to my estimates: assuming a constant separation rate, a 10 person per 1000 working-age-population decline in new hires would lead to a one-percentage point decline in the employment-to-population ratio observed over the time period. This implies a 12.6 percent decline in recidivism (7×0.018) associated with construction growth and a 10 percent increase in recidivism (10×0.010) associated with manufacturing decline.

[9]Many of studies estimate a relationship between local labor market conditions and crime rates using panel data techniques, and most find a small but statistically significant relationship (Freeman, 1995; Raphael and Winter-Ebmer, 2001; Donohue and Levitt, 2001; Gould et al., 2002; Machin and Meghir, 2004; Levitt, 2004; Öster and Agell, 2007; Lin, 2008; Fougère et al., 2009; Gronqvist, 2013). To address an endogenous relationship between crime rates and labor market conditions, recent research uses an instrumental variable strategy (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Öster and Agell, 2007; Lin, 2008).

[10]Using an instrumental variable strategy that does not rely on shocks to the manufacturing sector, Gould et al. (2002) find IV estimates slightly smaller than the corresponding OLS estimates.

[11]Lin (2008) reports results from models using six different instruments in Table 6 of his paper. Overall, models using instruments involving a state's employee percentage in the manufacturing sector yield the largest estimates. Results using union membership rates are also much larger than OLS estimates. This is not surprising since industries more likely to hire individuals with criminal records, such as manufacturing, also

opportunities in the manufacturing sector. It is likely that a substantial fraction of those affected by changes in manufacturing opportunities may be individuals on the margin of criminal activity. Therefore, the local average treatment effects identified in the IV specifications may not be very informative about the overall effect of a change in employment on the rate of crime.

Second, while the focus of the analysis is on job opportunities, my research contributes to prior work demonstrating the influence of wages on crime (Grogger, 1998; Doyle et al., 1999; Gould et al., 2002; Machin and Meghir, 2004). Doyle et al. (1999), Gould et al. (2002), and Machin and Meghir (2004) each estimate an effect of low-skill wages greater than that of local unemployment rates. Recently, Mocan and Unel (2011) estimate large elasticities between low-skill earnings and criminal activity using both aggregate panel data and longitudinal individual-level data. While these prior results estimating the relationship between average low-skill wages and local crime rates appear to be driven by changes in wages in the retail trade sector, my results suggest that expected wage changes caused by industry employment trends could also be important determinants of crime rates.

Finally, estimates specific to released prisoners are of interest in their own right. Released prisoners are large contributors to the overall crime rate. Using data from a national recidivism study, Rosenfeld et al. (2005) found that prisoners released in 1994 accounted for more than 10 percent of all arrests for property crime and more than 15 percent of arrests for violent crime from 1994 through 1997—an alarming rate considering this group represented less than 0.5 percent of the U.S. adult population. If offense rates were similar for prisoners released in years before and after 1994, these estimates would suggest that a substantial portion of crime can be attributed to recently-released prisoners. Furthermore, estimates specific to a population searching for work with criminal records is important given the rapid increase in the number of individuals with criminal records as well as increases in the use of criminal background checks in the hiring process.[12] These trends disproportionately disadvantage certain demographic groups in the United States; it is now estimated that 25 percent of African Americans versus 6 percent of non-African Americans have a prior felony conviction (Shannon et al., 2012).

This paper is organized as follows: Section 2 describes the institutional setting of parole in California; Section 3 describes the offender and labor market data used in my analysis; I outline my empirical methodology and describe my labor market measures of interest in Section 4; I discuss the estimates from several econometric specifications in Section 5; and I provide concluding remarks in Section 6.

2 Parole in California

Prior to the passing of a determinate sentencing law in 1976, the decision to release an offender to parole supervision before the completion of his or her sentence was made by a parole board.[13] The 1976 law eliminated this discretionary step for the majority of prisoners and required released offenders to complete a mandatory post-prison parole term, regardless of whether the offender was released before the completion of his prison sentence. During the

have higher rates of union membership.

[12]See Raphael (2014) for a discussion of the widespread use of criminal background screening in the United States. Finlay (2008) and Lee (2012) discuss the impact of the expansion of access to criminal records on employment and crime outcomes.

[13]The Uniform Determinate Sentencing Act, SB 42, passed in 1976 and became effective during 1977, beginning the "Determinate Sentencing Era" in California.

time period of my analysis, the length of time a convicted offender spends in prison is solely determined by his sentence and the amount of time subtracted for good behavior.[14]

Parole supervision is typically required for three years from the date of prison release for individuals incarcerated in California, although the Board of Parole Hearings (BPH) releases many offenders from supervision after 13 months.[15] The basic requirements of parole to which all California parolees must adhere include: immediately reporting to the assigned parole agent in the offender's last legal county of residence, reporting any address or employment change to the parole agent, and obeying all parole agent instructions (Grattet, Petersilia and Lin, 2008). Some parolees are subject to other special requirements such as drug and alcohol testing, registration as a sex offender, or not associating with gang members.

A released offender must return to his last county of legal residence in California unless he applies for and receives permission to relocate. Throughout my analysis I use the county of sentencing as a proxy for each individual's location post-release. The county of sentencing is likely the offender's county of pre-incarceration residence, given evidence from the criminology literature on criminal mobility.[16] Raphael and Weiman (2007) analyze prisoners released in California and document that more than 90 percent of prisoners released are returned to the county of sentencing.

The key outcome in my analysis is recidivism, defined as a return to prison. The Bureau of Justice Statistics measures recidivism as "criminal acts that result in the rearrest, reconviction, or return to prison with or without a new sentence during a three-year period following the prisoner's release." Since my data does not include individual arrest data, I use the "return to prison" version of the recidivism definition.[17] To avoid any selection bias caused by the early release of certain offenders from parole supervision, I focus my analysis on outcomes during the first year of parole but also report estimates from models with three-year outcomes. The majority of offenders who eventually return to prison do so within the first year.

If a parolee violates any of the supervision requirements he or she can be sent back to prison, but most returns in California are the result of a criminal violation (rather than a technical parole violation). An in-depth study of the California parole system during 2003 and 2004 by Grattet, Petersilia and Lin (2008) found that 84 percent of the parolees who returned to prison committed at least one criminal violation.[18]

[14]Good behavior time can be up to 50 percent for nonviolent offenders and 15 percent for a violent offense, but this has changed over time.

[15]The Board of Parole Hearings consists of 17 commissioners appointed by the Governor and confirmed by the California State Sentate. Commissioners sometimes conduct revocation hearings themselves, but most BPH reviews are made by Deputy Commissioners.

[16]Papers investigating the distance between a crime committed and a place of residence has consistently found that a crime is most often within a few miles of the offender's residence (Bernasco et al., 2012; Wiles and Costello, 2000). A difference in the county of residence and county of sentencing would most likely introduce measurement error and attenuate my estimates.

[17]Note that this recidivism definition is contingent on a parolee returning to prison in California. Since released offenders are required to complete their parole supervision within their county of residence, it is unlikely that any offenders supervised through the California parole system would be returned to a prison outside of the state correctional system.

[18]This calculation is based on my own tabulation of the restricted data from the study available through the Interuniversity Consortium for Political and Social Research (ICPSR) (Study #27161). The data is described in detail by Grattet, Petersilia and Lin (2008). For offenders released for the first time onto parole for a conviction, 89 percent of parolees reincarcerated return because of some criminal violation.

3 Data

3.1 Offender Data

I use prison release and parole outcome data from the National Corrections Reporting Program (NCRP) for prisoners released from 1993 through 2008 (Bureau of Justice Statistics, 2009). The NCRP provides information on every prisoner entering and exiting the custody of the California Department of Corrections and consists of three separate individual-level data sets: prison admissions (Part 1), prison releases (Part 2), and parole releases (Part 3). I observe whether an individual released from prison in California returns to prison within a specified time period by matching the prison release record with a parole release record using a combination of three variables common to each data set: exact date of birth, exact date of prison release, and county of sentencing.[19]

I focus my analysis on working-age (18 to 65) males released from a California State Correctional Facility to mandatory parole supervision during the years 1993 through 2008. Table 1 provides descriptive statistics for my estimation sample of over 1.7 million parole terms. An alarming number of prisoners released return to prison and do not successfully complete their parole supervision with two-thirds returning to prison while on parole (68%). More than half (53%) of those released return within one year from their date of release. Among male offenders paroled in California, 40 percent are white, 30 percent black, and 30 percent Hispanic. The average sentence for my estimation sample is slightly greater than 3 years. When classified by the most serious type of offense associated with an offender's incarceration spell, drug and property offenders each represent one-third of the total sample and 23 percent were incarcerated for a violent crime.[20]

3.2 Labor Market Data

The Quarterly Workforce Indicator (QWI) data provide quarterly employment totals, counts of job accessions and separations, and average earnings by county, industry, and skill level (United States Census Bureau LEHD Program, 2011). This data set offers several advantages over traditionally used county unemployment rates or total employment levels. [21] First, the QWI data include employment flows rather than just reporting employment (or unemployment) levels. Using the QWI, I am able to extract the number of workers who started a new job in a specified county and quarter who were not "recalls", or workers previously employed by the same employer within the same year. Furthermore, I can distinguish between workers of different education levels and specific industries. Counts of low-skill workers who started a

[19] The combination of date of birth, date of release, and county of sentencing is unique for all but 0.15 percent of the total number of observations. These observations are deleted from my analysis. Also, approximately 5 percent of individuals released from prison between 1993 and 2006 to parole are never observed in the parole release data set. Individuals still on parole after three years of supervision are likely parole absconders, meaning they have violated their parole supervision agreement but cannot be located. The NCRP does not allow me to observe whether or not a parolee absconds supervision. My models treat all individuals who do not return to prison within one year as still on parole supervision. Estimates from models dropping potential absconders (individuals not observed in the parole release data set within three years of release) are similar to those presented.

[20] I classify the type of offender by the offense carrying the longest sentence length among up to 3 conviction offenses provided in the NCRP data. The violent category include murder, assault, sex crimes, robbery, and weapons offenses. Property include burglary and theft offenses.

[21] The Census Bureau publishes the QWI data at lehd.did.census.gov/led/. The complete set of QWI data is available for downloading through the Cornell Virtual Reasearch Data Center at www.vrdc.cornell.edu/qwipu/.

new job by industry provide a more detailed measure of labor demand compared to fluctuations in unemployment rates or employment-to-population ratios.

Second, the QWI data are derived from administrative earnings records which measure labor market conditions with less error than estimated county unemployment rates available through the Local Area Unemployment Statistics (LAUS) program at the Bureau of Labor Statistics (BLS) (Bartik, 1996).[22] The QWI tabulates employment data reported by firms to the California Unemployment Insurance (UI) program, which represent more than 99 percent of wage and salary civilian employment in the state.[23] Since the UI employment records do not contain information on demographic characteristics of each employee, the Longitudinal Employer-Household Dynamics (LEHD) program links records from state unemployment insurance programs to Census Bureau data to provide a longitudinal employment and earnings database with demographic characteristics. The QWI data set is an aggregated version of this individual-level data, providing more precise measures of labor market conditions for different types of workers than the traditionally used labor market data.[24]

For each of the 64 potential release quarters between January 1993 and December 2008, I obtain quarter-of-release county job accessions.[25] I aggregate county-level job accession data to commuting zones. Commuting zones are geographic units used to define local labor markets. Commuting Zones were developed by Tolbert and Sizer (1996) using county-level commuting data from the 1990 Census. Commuting zones have been used extensively in the economics literature to define local labor markets.[26]

4 Empirical Methodology

To measure the impact of labor demand on recidivism, the following equation is estimated using panel data of individuals released from a California state prison to parole supervision during the period 1993-2008:

$$\ln\left(\text{Recid}_{czt}\right) = \alpha + \beta^{k}\text{New Hires}_{zt}^{s,k} + \mathbf{X}_{czt}'\Pi + \mathbf{Z}_{czt}'\Gamma + \tau_t + \phi_c + \lambda_{ct} + \epsilon_{czt}$$

Each observation is a cohort of released prisoners entering parole supervision with c indexing the county of release (as proxied by the county of sentencing), z indexing the commuting zone, and t indexing the quarter-of-release.[27] The dependent variable represents the natural log

[22] LAUS county unemployment rates are imputed using the "handbook method." The rates are imputed using self-reported data from the Current Population Survey (CPS) and various other data sources. More information is available at www.bls.gov/lau/laumthd.htm.

[23] Employment counts by month, county, and industry are also available from the Quarterly Census and Wage (QCEW) data provided by the BLS. This data is formerly known as "202 data" and comes from state UI systems. However, unlike the QWI data, the QCEW data does not include counts by the education level of the employee. The QCEW data also does not include employment flows (accessions and separations).

[24] For a full description of QWI data and imputation methods used for missing data see Abowd et al. (2009).

[25] Unfortunately public sector data is not reliable prior to the second quarter of 2000 in the California QWI data. For this reason, my analysis focuses on private sector accessions. A comparison of estimates using only private sector totals post Q2 2000 with estimates using public and private yield very similar results

[26] Commuting Zones have been used recently by Autor and Dorn (2013). I am grateful for crosswalk files provided by David Dorn at `http://www.cemfi.es/~dorn/data.htm` (*accessed 10 September 2014*). All results are robust to redefining local labor markets at the county level. These results are available upon request.

[27] My analysis focuses on cohort models as the variation of interest is at the aggregate level rather than the individual level. Moreover, these cohort specifications are less computationally intensive due to the large number of individual-level observations. Results from analysis at the individual level using both logit and linear probability models are consistent with the cohort-model results and are available upon request.

of the number of former inmates within each release cohort returning to prison within one year. The dependent variable is logged to facilitate comparison of estimates across multiple specifications.

Fixed effects for year-by-quarter of release (τ_t) and county of sentencing (ϕ_c) are included in all specifications along with a county-specific linear time trend (λ_{ct}). I also add county-specific quadratic trends to allow for non-linear trends as well as county-quarter fixed effects to control for county-specific seasonal patterns. Other control variables include characteristics of each release cohort ($\mathbf{X}'_{ct}$) as well as county-level characteristics ($\mathbf{Z}'_{ct}$). Release cohort controls include: percent black, percent hispanic, average age, percent with a prior felony conviction, average sentence length, average percent of sentence served, as well as the percent of offenders in each crime category (drug, property, violent). County-level control variables include: low-skill and female share of total employment, percent in poverty, median household income (CPI adjusted), the natural log of the police force size, and the arrest clearance rate for total offenses. To control for the supply of labor, I include the unemployment rate during the quarter prior to release as well as the size of the release cohort. To account for correlation within counties and commuting zones over time, I cluster standard errors at the commuting zone-level in all empirical specifications. There are 15 commuting zones in California. Results are robust to the wild-cluster bootstrap procedure suggested by Cameron et al. (2008) and Cameron and Miller (2013).[28] Since observations are at the cohort level, all specifications are weighted by the average size of county release cohorts.

The variables of interest, New Hires$^{s,k}_{zt}$, measure the number of workers (per 1000 working-age population) of skill-level s starting a new job within industry k and commuting zone z, during quarter t. Since total hires can be decomposed into recalls (workers starting a job at an employer who had employed them during the previous year) and other new hires, I use counts of new hires not including recalls to best measure labor demand relevant to those just entering the labor market after a period of incarceration.[29] The coefficient, $\beta^{s,k}$ measures the effect of a change in the number of workers hired equivalent to one person per 1000 working-age population within a commuting zone for skill-level s and industry k.

Models are first estimated where the variable of interest represents the total number of new hires across all education levels and industries. Since work that requires more than a high school diploma is not relevant to the typical parolee—85 percent of males with an incarceration history in the United States do not have any education beyond a high school degree (Raphael, 2014)—I decompose total new hires into low-skill (high school graduate and below) and high-skill (any college and above). Still, a significant fraction of low-skill job openings may also be irrelevant to individuals recently released from prison. Certain employers are prohibited by law from hiring convicted felons, and many others choose not to consider applicants with criminal records.[30] Using information available through the National Longitudinal Survey of Youth 1997 (Bureau of Labor Statistics, 2011), I identify six primary industries in which former inmates find work: construction, food services, retail, manufacturing, administrative services

[28] All specifications were re-estimated using the *cgmwildboot* program created by Judson Caskey and accessed at `https://sites.google.com/site/judsoncaskey/data` on 10 September 2014.

[29] Results using total new hires (recalls + other hires) are very similar to those reported using non-recall new hires.

[30] Bushway and Sweeten (2007) estimate that employment of convicted felons is prohibited for approximately 800 occupations across the country. A 2003 survey of California employers found that 60 percent of employers always check the criminal backgrounds of job applicants and over 70 percent of employers would “probably not accept" or “definitely not accept" an individual with a criminal record for the most recent non-professional, non-managerial job opening (Raphael, 2010, 2014).

and waste management, and other services.[31] To measure the effects of different types of job opportunities relevant to released offenders, I include counts of new hires specific to each of the six primary (relevant) industries along with low-skill accessions among the other 13 less-relevant industries and high-skill accessions.

4.1 Identification of Labor Market Parameters

I interpret variation in job accessions used to identify the effects of employment opportunities on recidivism as arising from changes in aggregate labor demand that are uncorrelated with the criminal propensity or other characteristics of different prison release cohorts. My analysis controls for unobserved differences between prisoners released at different times at the state level as well as changes to any state parole policies by including year-by-quarter fixed effects. The labor market effects in each of the models specified are identified from deviations in job accessions from an arbitrary common trend across counties, and deviations from within-county linear, quadratic, and seasonal trends.

An interpretation of my results as measuring the effect of job opportunities on offender behavior relies on skill- and industry-specific new hires within commuting zones providing an accurate measure of employment opportunities for individuals released from prison and not a measure of some other location-specific factor impacting offender behavior (such as gentrification of neighborhoods or housing availability). To test whether some unobserved factor is driving my results, I include job accession measures during the quarter-of-release as well as job accession measures in the quarter prior to release. Controlling for the number of new hires when an offender is released from prison, the number of new hires prior to release will not measure employment opportunities, but will be correlated with other unobserved factors that may be driving my results. I detect no effect among labor demand measures prior to labor market entry as reported in Table 4. Furthermore, since manufacturing and construction employment opportunities are primarily relevant to men, I test whether these fluctuations influence female behavior since any unobserved determinants of offender behavior should impact both male and female offenders. I only find effects among male offenders (results specific to female offenders are reported in Table 6), supporting my interpretation of the results as measuring the effect of changes in job opportunities.

Estimates of the labor demand effects could also be biased if unobserved criminogenic characteristics of the community to which the prisoner is released are correlated with both recidivism and industry-specific labor market conditions. To assess whether unobserved criminogenic factors, such as changes in the market for crack cocaine or changes in policing strategies, are influencing my results, I estimate an additional specification including the county crime rates just prior to release. It is reasonable to assume that any unobserved criminogenic factors would be correlated with the amount of crime in the community so including the crime rate as a control should influence the estimated effect of the labor market measures if there is an omitted variable bias arising. Results from specifications including crime rates are presented in Table 4 and are very similar to the main results presented in Table 3. I also estimate models including lagged values of the dependent variable ($\ln(\text{Returns})$) to control for any other omitted time varying characteristics. Again, Table 4 reports estimates consistent

[31]The fraction of employed former inmates working in each industry is displayed in Figure 1. In this nationally representative survey, I am able to record the industry code of the first job (within one year) following a spell of jail or incarceration. Among all individuals who report employment in the year following release, I calculate the probability of employment within each industry sector.

with baseline effects in Table 3.[32]

While very unlikely in my period of analysis, it could be the case that more prisoners are released during periods of a state or county budget crises which could plausibly be correlated with industry-specific labor demand fluctuations. Although the timing of prison release is determined by the original sentence less automatic credits for good behavior and year-by-quarter fixed effects pick up any state-wide trends in return rates, I test whether job accession variables are related to the number of prisoners released. Replacing our dependent variable with the total number of prisoners released in each county-quarter cohort, Table 4 shows no influence of construction or manufacturing job accessions on the size of each release cohort.

I do not find any evidence of a bias from any of these primary threats to identification and interpretation of my results as the effect of job opportunities on recidivism. Overall, the variation in the job accessions appears to be independent of potential confounding factors.

5 Results

First, I estimate the relationship between total job accessions at the time of release on the number of parolees returning to prison within one year (Table 3). Results presented in Column (1) include county fixed effects, year-by-quarter fixed effects, and a county-specific linear trend. Column (2) adds a county-specific quadratic trend, and Column (3) adds county-quarter specific effects. The estimated coefficients are very small in magnitude and not statistically distinguished from zero. Changes in aggregate labor demand appear to have very little effect on the probability of returning to prison. Counts of new hires disaggregated by education level also do not influence recidivism rates as reported in the second panel of Table 3.

Once low-skill counts of new hires are disaggregated by industry, I find large and statistically significant declines in recidivism rates associated with increases in the number of low-skill construction and manufacturing workers hired during the quarter of prison release which are fairly consistent across specifications (1) through (3). The preferred model in Column (3), which includes county-specific linear and quadratic trends as well as county-quarter fixed effects, reports a 1.8 percent decrease in recidivism associated with one extra construction hire per 1000 working-age individuals in a commuting zone during the quarter of prison release. A one-standard deviation change in the number of construction low-skill new hires is equal to 1.24 (as reported in Table 2). A similar increase in low-skill manufacturing hires is associated with a 1.0 percent decrease in recidivism. I do not detect a statistically significant influence of any other relevant industry (food services, retail, admin/waste, or other services) and detect a small and marginally significant increase in recidivism associated with an increase in the number of high-skill job accessions. These effects could be explained by an increase in the private return to criminal activity associated with increases in wealth or inequality unrelated to an offender's own employment prospects.[33]

All results are robust to inclusion of lagged job accession measures. Although only coefficients on the lagged construction and manufacturing accessions are reported in Table 4, lagged

[32] Because the introduction of a lagged dependent variable in a fixed effects model can introduce bias if the number of time periods is small, the purpose of including a lagged dependent variable is solely to provide additional evidence that the estimated effect of labor market fluctuations on recidivism is not biased by omitted variables.

[33] A few researchers discuss ways in which improvements in economic conditions can *increase* criminal behavior. Crime could increase if improving labor market conditions provide more opportunities to steal (when people are at work) and increase the value of the objects typically stolen (i.e. your neighbor buys a brand new car) (Freedman and Owens, 2012; Cantor and Land, 1985).

values for each of the relevant industries, other low-skill new hires, and high-skill new hires were included in the regression. The estimated effects of industry-specific labor demand are also robust to the inclusion of county crime rates (property, violent, and drug) just prior to release. Table 4 also reports estimated coefficients from a specification including a lagged dependent variable (the natural log of the number of prisoners returned in the prior cohort) further supporting my interpretation of the estimated effects.

Tables 5, 6, and 7 further examine the relationship between low-skill industry-specific new hires and recidivism for different types of parolees. The average one-year recidivism rate for each group is reported at the bottom of each column for each subsample.

I find large and statistically significant effects of construction and manufacturing labor demand for individuals incarcerated for drug crimes. The hiring of one construction worker per 1000 working-age individuals in a commuting zone at the time of release is associated with a 2.4 percent decrease in one-year recidivism rates. A similar increase in the number of manufacturing workers hired decreases recidivism rates by 1.5 percent. Table 5 reports statistically significant effects for construction opportunities among property offenders and effects for manufacturing opportunities among violent offenders. This finding may be due to employer demand; Holzer et al. (2006) and Raphael (2010) report evidence from employer surveys suggesting that among the very few employers who are willing to consider applicants with criminal record, most favor drug offenders over property or violent criminals. The heterogeneous effects could also be due to supply factors; to the extent that offenders continue to commit the same types of crimes, my results support the notion that violent offenders are less motivated by economic incentives. However, I do find evidence that manufacturing opportunities can influence recidivism rates among violent offenders.

Columns (5) and (6) of Table 5 split offenders by whether they are released to parole for the first time and are not recorded to have any prior felony convictions (first-time offenders) and those who are repeat offenders. I estimate a similar response to construction fluctuations among both groups, but a much larger decrease in recidivism for first-time offenders in response to an increase in the number of manufacturing opportunities at the time of release. Again, this could be driven by employer demand since employers likely prefer first-time offenders.

To investigate whether changes in skill- and industry-specific job opportunities have differential effects by race and ethnicity, I separately estimate models for black (non-Hispanic), and Hispanic, and white (non-Hispanic) offenders (Table 6). The effects of increases in construction opportunities on recidivism are similar across these three groups. I find a larger response to manufacturing opportunities among Hispanic offenders. These result suggests that diminished access to relevant job opportunities could potentially contribute to the large racial and ethnic differences in crime and recidivism rates. Two recent papers in the criminology literature have investigated whether racial differences in recidivism rates can be attributed to racial differences in manufacturing job opportunity (Wang et al., 2010; Bellair and Kowalski, 2011). Using a Cox proportional hazards model, Bellair and Kowalski (2011) find that lower availability of manufacturing jobs in areas where black offenders are released can explain much of the racial differences in recidivism for a sample of 1,568 offenders released in Ohio during the first six months of 1999. In my setting, fluctuations in demand for low-skill manufacturing workers influence return rates among black offenders, but this effect is not statistically significant. My results suggest that access to construction jobs may be a more important determinant of racial and ethnic differences in recidivism rates.

Table 7 presents estimated coefficients for models restricting the estimation sample to certain age groups. The results indicate that the parole behavior of older offenders (up to the age of 55) is much more responsive to relevant labor market fluctuations than the behavior of younger offenders. Individuals released between the ages of 35 and 45 respond most to construction and manufacturing opportunities. This result is consistent with the notion of this type of work being difficult for older individuals who are more likely to have physical limitations. This pattern of results is also consistent with recent research in the life course literature which suggests that work effects are not uniform across age groups and predict a larger change in criminal behavior among older offenders presented with employment opportunities (Uggen, 2000). I do not find significant decreases in recidivism in response to employment fluctuations in the relevant sectors for offenders released under the age of 25.

Finally, Table 8 reports estimates for 3 year return rates. As previously discussed, these estimates could be biased by parolees released from parole supervision early (after 13 months) since these parolees are not observed for the same time period as those serving the standard 3 year parole term. While effects on construction opportunities are not statistically significant, the magnitude is consistent with the main results. A one unit increase in manufacturing opportunities at the time of release is associated with a 1.3 percent decline in 3 year recidivism rates. A larger long-term effect for manufacturing employment could be driven by differences in the permanence of manufacturing jobs compared to construction jobs. Redefining my labor market demand variables of interest using the average number of quarterly hires within the first two quarters post release (Column (2)), or within the first year post-release (Column (3)) also yield consistent results. Although manufacturing hire coefficients are no longer statistically significant, they are consistent in magnitude. Quarter-of-release measures are preferred since jobs available at the time of release plausibly have a larger influence on offender behavior as found in the CEO experimental evaluation in which recidivism effects of randomly assigned jobs were driven by those assigned work within the first three months since leaving prison (Redcross et al., 2011). Finally, to confirm results are not driven primarily by the Los Angeles area commuting zone which represents more than 50 percent of California's parole population, I present results excluding this commuting zone in column (4) of Table 8. Estimates are smaller in magnitude, but consistent with those found for the entire sample and significant at the 99% confidence levels.

6 Remarks

Overall, my empirical results support predictions from the standard theoretical models that relate crime to economic incentives. They indicate that individuals recently released from prison adjust their criminal activity in response to changes in certain types of labor market opportunities. Specifically, offenders released from prison in California are less likely to return to prison if a greater number of construction and/or manufacturing opportunities are available at the time of release. Compared to other low-skill jobs accessible to individuals with criminal records, such as those in retail and food services, construction and manufacturing jobs pay significantly higher wages and are much more likely to be associated with other benefits which characterize *good* jobs from others.

This study provides an explanation why prior research estimating the impact of local labor market conditions on recidivism find small and/or statistically insignificant effects (Raphael and Weiman, 2007; Bolitzer, 2005); aggregate fluctuations do not provide accurate measures of employment opportunities relevant to released offenders. This study also helps explain mixed results in experimental evaluations of transitional job reentry programs (Redcross et al., 2011; Jacobs, 2012; Raphael, 2014); the returns to crime for many offenders may be larger than the returns to minimum-wage short-term jobs.

While the effect of construction and manufacturing job opportunities on criminal activity among released offenders may be driven by the effect of having a *good* job on criminal behavior, the reduced form effect estimated also includes behavioral responses to fluctuations in the offenders' expectations of finding a job as well as any social spillovers from the response of other individuals in the community to these skill- and industry-specific fluctuations. Overall, my study provides robust evidence that the *quality* of employment opportunities for released offenders may be more important than the *quantity* of employment opportunities. These results suggest that further experimental evaluations of employment reentry programs should vary the quality of jobs provided to released offenders to test the degree to which job quality can prevent future criminality.

The results also suggest that policies and/or programs that create more *good* job opportunities for released offenders can reduce incarceration rates. These policies might include: economic stimulus aimed at certain industries with high concentrations of good jobs, such as recent federal efforts to grow local manufacturing sectors; policies that increase the quality of work within industries which do not offer *good* jobs for low-skill workers, such as minimum wage increases or the implementation of "living wages"; or policies and programs which augment the skill levels of this population to increase the number of high quality opportunities accessible, such as education or skill-specific training programs in prison facilities. Further research is needed to evaluate the effect of these types of policies and programs and to assess their cost-effectiveness.

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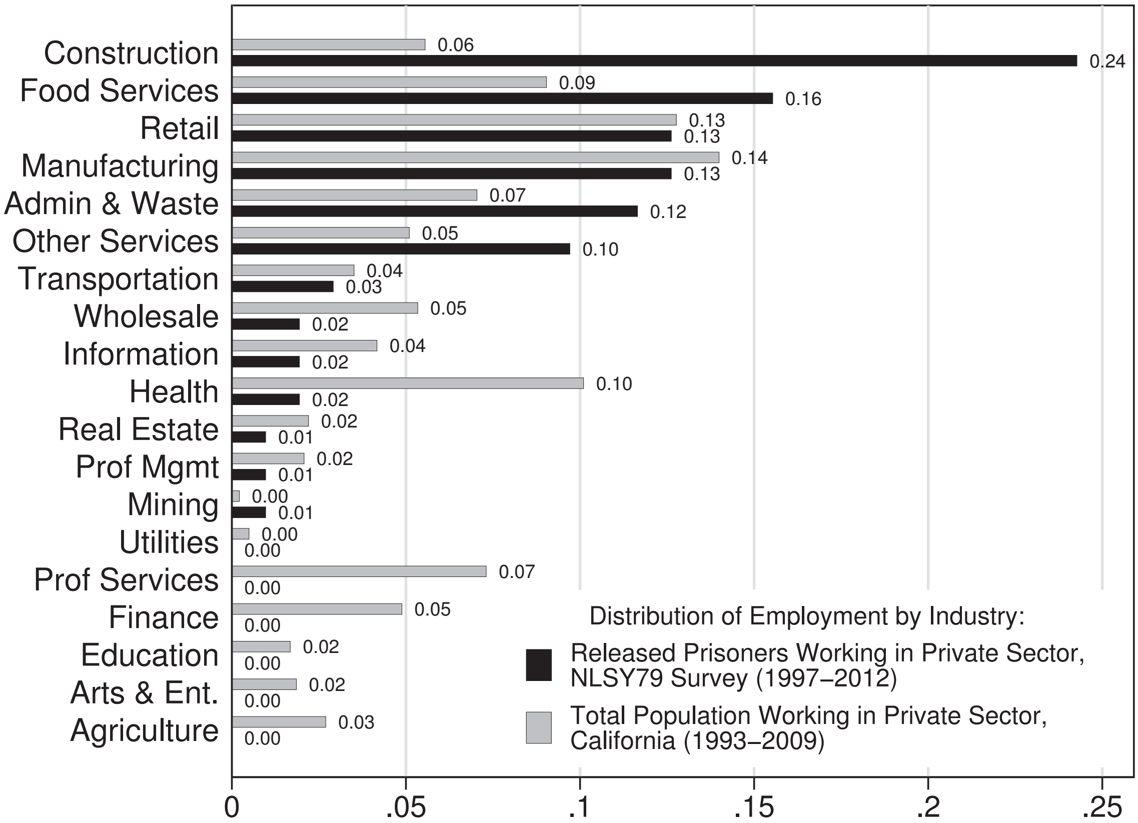
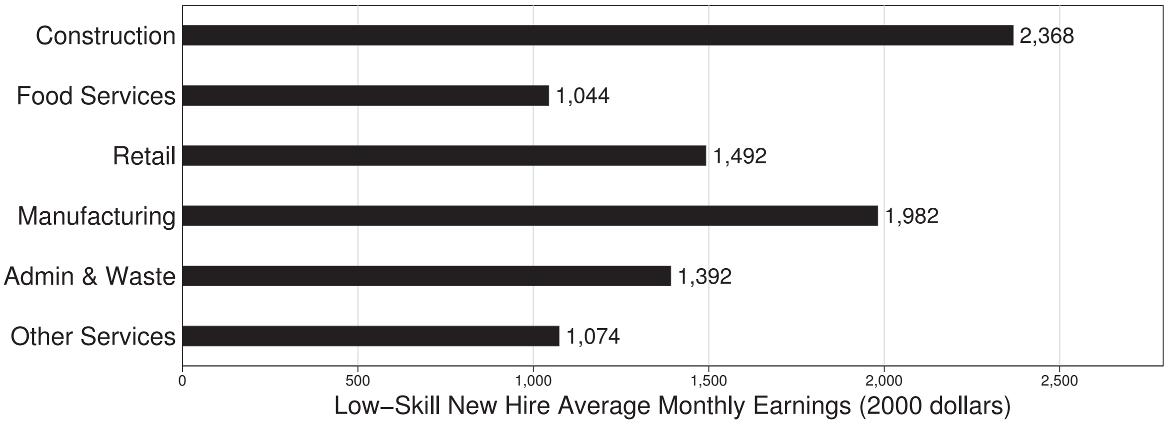
7 Figures and Tables

Figure 1: Which industries employ offenders released from incarceration?



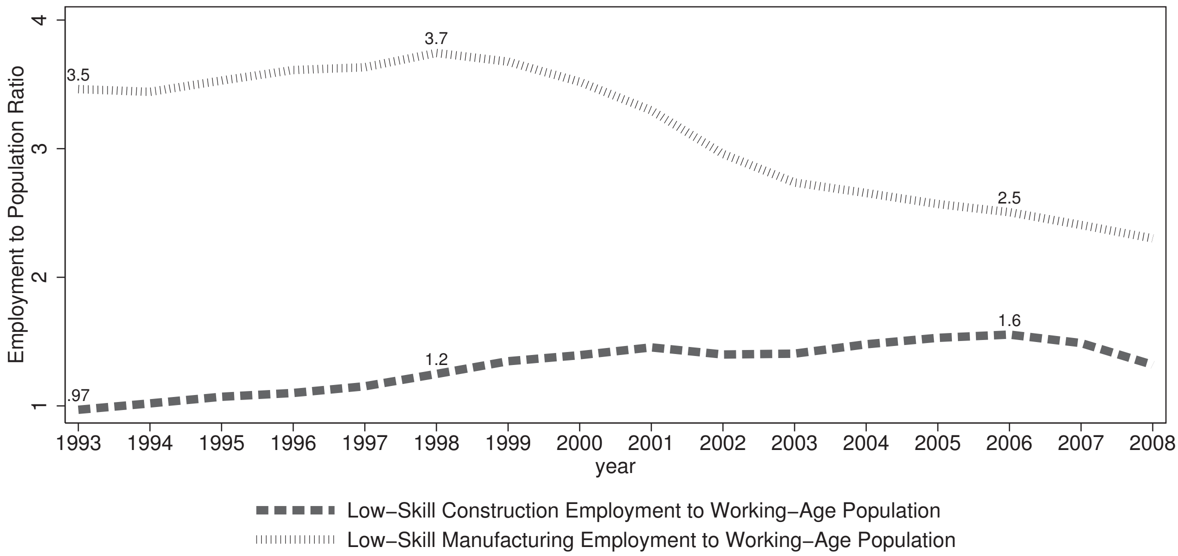
The industry distribution of recently released prisoners was calculated from National Longitudinal Survey of Youth 1979 (NLSY79) for the years 1997-2012 (Bureau of Labor Statistics, 2011). For each survey participant exiting jail or incarceration, the first industry of employment within 12 months from release was obtained (*n=103*). These estimates are based on a small number of individuals in a national sample which is, on average, younger than the average prisoner released in California. However, the distribution of workers across industries is consistent with the estimated industry distribution of individuals in the Survey of Inmates in State and Correctional Facilities (SISCF) reporting pre-incarceration occupations.

Figure 2: How much can released offenders expect to earn in relevant industries?



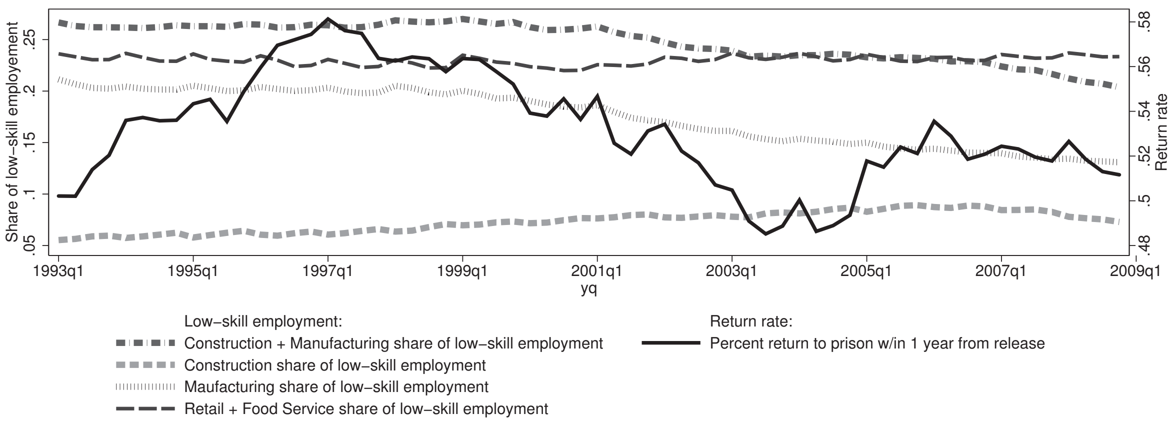
Average monthly earnings by industry for low-skill (high school graduate and less) individuals hired by private sector employers is calculated using statewide California data from the Quarterly Workforce Indicator (QWI) dataset for the years 1993-2009. The QWI contains skill- and industry-specific average monthly earnings for individuals hired during each quarter. These average earnings by quarter are converted to 2000 dollars using the CPI index and then averaged over all quarters between 1993 and 2009 for each industry relevant to released offenders.

Figure 3: California Construction and Manufacturing Employment, 1993-2008.



The low-skill employment to population ratios for the construction and manufacturing industries are calculated using statewide California data from the Quarterly Workforce Indicator (QWI) dataset for the years 1993-2009.

Figure 4: Low-skill employment shares by industry and recidivism among released prisoners in California, 1993-2008.



The share of low-skill (high school graduate and less) private sector employment by industry is calculated using statewide California data from the Quarterly Workforce Indicator (QWI) dataset for the years 1993-2009. The number of released prisoners returned within one year by year-quarter release cohorts is calculated using prison and parole release data from the National Corrections Reporting Program for the years 1993-2009.

Table 1: Descriptive Statistics of Offenders Released from a California State Correctional Facility into Parole Supervision between 1993 and 2009

	Average Characteristics of CA Prisoners Released, 1993-2008
Return Rates	
Return rate (w/in 3 years)	0.68
	(0.07)
Return rate (w/in 1 year)	0.53
	(0.09)
Demographic Characteristics	
Male	0.90
	(0.02)
Age at prison release	35.23
	(1.37)
Black	0.30
	(0.16)
Hispanic	0.30
	(0.14)
White (non black, non hispanic)	0.40
	(0.17)
Crime and Incarceration Characteristics	
Sentence length (months)	37.93
	(4.28)
Percent of sentence served	0.59
	(0.07)
Prior felony conviction	0.25
	(0.09)
First parole term	0.36
	(0.07)
Type of Crime (most serious)	
Drug	0.33
	(0.06)
Property	0.32
	(0.04)
Violent	0.23
	(0.04)
Observations	1,915,180

Source: National Corrections Reporting Program (Bureau of Justice Statistics, 2009)
Means are reported with standard deviations in parenthesis.

Table 2: Descriptive Statistics of Commuting Zone Labor Market Measures, California 1993-2008

	CA Quarterly Labor Market Measures by Commuting Zone, 1993-2008
All New Hires	118.53
	(40.22)
Low-Skill New Hires	42.72
	(28.75)
High-Skill New Hires	35.91
	(9.74)
Construction Low-Skill New Hires	3.57
	(1.24)
Manufacturing Low-Skill New Hires	2.29
	(1.36)
Food Services Low-Skill New Hires	3.52
	(1.18)
Retail Low-Skill New Hires	3.40
	(0.94)
Admin/Waste Low-Skill New Hires	3.98
	(1.87)
Other Services Low-Skill New Hires	2.08
	(1.02)
All Other Low-Skill New Hires	23.15
	(28.25)
Unemployment Rate	9.06
	(5.04)
Low-Skill Share of Employment	0.36
	(0.07)
Female Share of Employment	0.46
	(0.03)
Observations	1,020

Source: Quarterly Workforce Indicator Data (United States Census Bureau LEHD Program, 2011)

Means are reported with standard deviations in parenthesis. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing. Commuting zone boundaries used are introduced in Tolbert and Sizer (1996), with crosswalk files provided by David Dorn on his website `http://www.cemfi.es/~dorn/data.htm` (*accessed 10 September 2014*).

Table 3: New Hires and Recidivism

	(1)	(2)	(3)
New Hires	−0.000	0.000	−0.000
	(0.000)	(0.000)	(0.000)
Total Hires by Skill Level			
Low-Skill New Hires	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
High-Skill New Hires	0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)
Total New Hires by Skill Level and Industry			
Construction Low-Skill New Hires	−0.015**	−0.013***	−0.018***
	(0.005)	(0.004)	(0.003)
Manufacturing Low-Skill New Hires	−0.004	−0.006*	−0.010**
	(0.003)	(0.003)	(0.004)
Food Services Low-Skill New Hires	0.006	0.002	0.004
	(0.005)	(0.006)	(0.009)
Retail Low-Skill New Hires	0.004	0.002	0.000
	(0.005)	(0.005)	(0.006)
Admin/Waste Low-Skill New Hires	0.001	0.000	−0.001
	(0.002)	(0.002)	(0.001)
Other Services Low-Skill New Hires	0.003	0.002	−0.000
	(0.002)	(0.002)	(0.002)
All Other Low-Skill New Hires	0.000	−0.000	0.001
	(0.000)	(0.000)	(0.001)
High-Skill New Hires	0.000	0.001*	0.002*
	(0.001)	(0.001)	(0.001)
Observations (cohorts)	2,944	2,944	2,944
Number of Individuals	1,714,664	1,714,664	1,714,664
Average Return Rate	0.573	0.573	0.573
County and Year-Quarter FE	Y	Y	Y
County Linear Trend	Y	Y	Y
County Quadratic Trend	N	Y	Y
County-Quarter FE	N	N	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4. The estimation sample includes all male offenders released to mandatory parole supervision in California between January 1993 and December 2008. Control variables include: unemployment rate (quarter prior to release), low-skill employment share, female employment share, percent in poverty, median household income, log of police, arrest clearance rate, natural log of release cohort size, percent black, percent hispanic, average age-at-release, percent with prior felony conviction, average sentence length, average percent of sentence served.

Table 4: Specifications to Test Causal Interpretation

	(1) Hires Prior to Release	(2) Include Crime Rates	(3) Include Lag Dep Var	(4) Dep Var = ln(Released)
Quarter of Release				
Construction Low-Skill New Hires	−0.015***	−0.016***	−0.016***	−0.002
	(0.003)	(0.002)	(0.003)	(0.008)
Manufacturing Low-Skill New Hires	−0.011***	−0.010**	−0.009**	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Food Services Low-Skill New Hires	0.005	0.003	0.004	−0.004
	(0.010)	(0.009)	(0.009)	(0.011)
Retail Low-Skill New Hires	−0.001	−0.002	0.000	0.010*
	(0.006)	(0.006)	(0.005)	(0.005)
Admin/Waste Low-Skill New Hires	−0.001	−0.001	−0.001	0.000
	(0.002)	(0.002)	(0.001)	(0.002)
Other Services Low-Skill New Hires	0.001	0.000	−0.000	0.002
	(0.003)	(0.003)	(0.002)	(0.005)
All Other Low-Skill New Hires	0.001	0.000	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.001)
High-Skill New Hires	0.001	0.002*	0.002*	−0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Quarter Prior to Release				
Construction Low-Skill New Hires	−0.003			
	(0.005)			
Manufacturing Low-Skill New Hires	0.002			
	(0.004)			
Property Crime Rate		0.001		
		(0.001)		
Violent Crime Rate		0.000		
		(0.001)		
Drug Arrest Rate		0.005		
		(0.004)		
Ln(Recid)			0.084**	
			(0.033)	
Observations (cohorts)	2,898	2,898	2,898	2,898
Number of Individuals	1,695,705	1,695,705	1,695,705	1,695,705
Average Return Rate	0.575	0.575	0.575	0.575
County and Year-Quarter FE	Y	Y	Y	Y
County Linear Trend	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y
County-Quarter FE	Y	Y	Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4. The sample and controls included are as listed in Table 3. While only results on the lag construction and manufacturing accessions are reported, the specification for Column (1) includes lags for each category of accessions. Crime rates included in Column (2) are obtained from FBI Uniform crime reports.

Table 5: Heterogeneous Effects by Type of Criminal

	(1) Drug	(2) Property	(3) Violent	(4) First	(5) Repeat
Construction Low-Skill New Hires	−0.024***	−0.018**	−0.004	−0.019**	−0.017***
	(0.005)	(0.006)	(0.007)	(0.007)	(0.003)
Manufacturing Low-Skill New Hires	−0.015**	−0.009	−0.016**	−0.022**	−0.008*
	(0.006)	(0.008)	(0.007)	(0.009)	(0.004)
Food Services Low-Skill New Hires	0.013	−0.000	0.012	0.005	0.006
	(0.015)	(0.010)	(0.012)	(0.015)	(0.011)
Retail Low-Skill New Hires	0.005	−0.001	−0.004	−0.004	0.001
	(0.006)	(0.004)	(0.009)	(0.010)	(0.005)
Admin/Waste Low-Skill New Hires	−0.004	0.005*	−0.007*	−0.007	0.001
	(0.004)	(0.002)	(0.003)	(0.004)	(0.001)
Other Services Low-Skill New Hires	0.009	−0.003	−0.008***	0.000	0.000
	(0.007)	(0.004)	(0.003)	(0.005)	(0.003)
All Other Low-Skill New Hires	0.002***	0.001	−0.000	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
High-Skill New Hires	0.000	0.000	0.002**	0.004**	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Observations (cohorts)	2,911	2,936	2,923	2,929	2,942
Number of Individuals	555,620	542,247	416,826	572,107	1,142,502
Average Return Rate	0.537	0.647	0.544	0.413	0.652
County and Year-Quarter FE	Y	Y	Y	Y	Y
County Linear Trend	Y	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y	Y
County-Quarter FE	Y	Y	Y	Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4 separately run for cohorts defined by the criminal history of offenders. The sample and controls included are as listed in Table 3. For Columns (1), (2), and (3), cohorts are defined by the most serious offense for which offenders are incarcerated. Column (4) provides results for cohorts of offenders released for the first time to parole without any prior felony convictions. Column (5) provides results for cohorts of individuals who either have a prior felony conviction or prior parole failure.

Table 6: Heterogeneous Effects by Gender and Race/Ethnicity

	(1) Black	(2) Hispanic	(3) White	(4) Female
Construction Low-Skill New Hires	−0.020***	−0.015*	−0.014***	−0.004
	(0.004)	(0.008)	(0.004)	(0.013)
Manufacturing Low-Skill New Hires	−0.012	−0.025*	−0.001	0.014
	(0.008)	(0.014)	(0.003)	(0.008)
Food Services Low-Skill New Hires	0.004	0.010	0.006	0.004
	(0.010)	(0.020)	(0.011)	(0.029)
Retail Low-Skill New Hires	−0.010	0.009	0.003	0.005
	(0.006)	(0.011)	(0.007)	(0.019)
Admin/Waste Low-Skill New Hires	−0.006***	−0.005	0.001	−0.009
	(0.001)	(0.006)	(0.002)	(0.008)
Other Services Low-Skill New Hires	0.002	−0.005	−0.002	0.025***
	(0.005)	(0.009)	(0.002)	(0.008)
All Other Low-Skill New Hires	0.001*	−0.000	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
High-Skill New Hires	0.002**	0.003	0.000	−0.002
	(0.001)	(0.002)	(0.001)	(0.003)
Observations (cohorts)	2,638	2,686	2,941	2,717
Number of Individuals	511,845	532,680	669,577	182,083
Average Return Rate	0.662	0.490	0.588	0.489
County and Year-Quarter FE	Y	Y	Y	Y
County Linear Trend	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y
County-Quarter FE	Y	Y	Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4 separately run for cohorts defined by the race/ethnicity of offenders. The sample and controls included are as listed in Table 3. Column (1) reports results for cohorts of black (non-Hispanic) offenders and Column (3) reports results for cohorts of white (non-Hispanic) offenders.

Table 7: Heterogeneous Effects by Age at Release

	(1) 18 to 25	(2) 25 to 35	(3) 35 to 45	(4) 45 to 55	(5) 55 to 65
Construction Low-Skill New Hires	−0.012	−0.018***	−0.019**	−0.027*	0.031
	(0.009)	(0.006)	(0.007)	(0.014)	(0.022)
Manufacturing Low-Skill New Hires	−0.006	−0.012	−0.018***	0.004	−0.009
	(0.009)	(0.008)	(0.005)	(0.008)	(0.019)
Food Services Low-Skill New Hires	0.010	0.009	0.001	0.019	−0.053
	(0.014)	(0.012)	(0.014)	(0.017)	(0.055)
Retail Low-Skill New Hires	−0.004	0.004	−0.006	−0.000	0.007
	(0.012)	(0.008)	(0.007)	(0.012)	(0.040)
Admin/Waste Low-Skill New Hires	−0.001	−0.000	0.000	−0.004	0.001
	(0.006)	(0.003)	(0.003)	(0.005)	(0.014)
Other Services Low-Skill New Hires	−0.010	−0.002	0.005	0.002	−0.016
	(0.008)	(0.003)	(0.004)	(0.006)	(0.013)
All Other Low-Skill New Hires	0.003**	0.000	0.002***	0.000	−0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
High-Skill New Hires	−0.000	0.001	0.002	0.001	−0.002
	(0.002)	(0.001)	(0.001)	(0.002)	(0.005)
Observations (cohorts)	2,837	2,937	2,928	2,829	2,167
Number of Individuals	184,372	607,284	514,209	217,077	37,567
Average Return Rate	0.631	0.580	0.578	0.542	0.530
County and Year-Quarter FE	Y	Y	Y	Y	Y
County Linear Trend	Y	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y	Y
County-Quarter FE	Y	Y	Y	Y	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4 separately run for cohorts defined by the age of offenders at the time of release from prison. The sample and controls included are as listed in Table 3.

Table 8: Other Outcomes and Robustness Checks

	(1) Dep Var: Return w/in 3 year	(2) New Hires w/in 6mo	(3) New Hires w/in 12mo	(4) Exclude Los Angeles
Construction Low-Skill New Hires	−0.008	−0.018***	−0.016***	−0.012***
	(0.005)	(0.003)	(0.003)	(0.004)
Manufacturing Low-Skill New Hires	−0.013**	−0.009	−0.007	−0.006***
	(0.005)	(0.008)	(0.010)	(0.002)
Food Services Low-Skill New Hires	0.012	0.002	0.011	0.006
	(0.009)	(0.010)	(0.012)	(0.010)
Retail Low-Skill New Hires	−0.001	0.000	0.007	0.003
	(0.004)	(0.007)	(0.007)	(0.006)
Admin/Waste Low-Skill New Hires	0.004	0.001	0.003	−0.002
	(0.003)	(0.002)	(0.003)	(0.003)
Other Services Low-Skill New Hires	0.002	−0.000	0.001	−0.001
	(0.002)	(0.002)	(0.003)	(0.002)
All Other Low-Skill New Hires	0.001*	0.001	0.003**	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
High-Skill New Hires	0.001	0.001	−0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations (cohorts)	2,944	2,887	2,835	2,624
Number of Individuals	1,714,664	1,713,117	1,711,166	866,355
Average Return Rate	0.700	0.574	0.574	0.578
County and Year-Quarter FE	Y	Y	Y	Y
County Linear Trend	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y
County-Quarter FE	Y	Y	Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-commuting zone correlation in parentheses. There are 15 commuting zones in California. Statistical significance of results is robust to the "Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis" procedure suggested by Cameron et al. (2008). All specifications are weighted by the average size of release cohorts by county. All job accession measures (new hires) are calculated as counts per 1000 working age persons in the commuting zone containing the county of sentencing.

This table reports results from linear regressions as specified in Section 4. The sample and controls included are as listed in Table 3. Column (1) replaces the dependent variable of returns within one year with one measuring the number of returns within 3 years. Columns (2) and (3) report results from specifications redefining the accession measures with averages over the first 2 quarters and full year post-release. Column (4) reports estimates of the baseline model excluding the Los Angeles commuting zone which accounts for over half of the individual observations.