The Effects of Job Loss on Crime: Evidence From Administrative Data

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Abstract

This paper investigates the effects of job loss on recidivism using a novel merge of employer-employee wage data to administrative records on crime. I first use firm-level employment shocks to study the reduced form impact of job loss on offending. I then use a kink in unemployment insurance benefits to distinguish economic incentives from other factors, such as incapacitation through time spent at work, as a mechanism. I find that property crimes and domestic violence rise sharply after a layoff and remain elevated for up to two years. Economic incentives are an important mechanism, consistent with Becker-Ehrlich models.

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1 Introduction

The individuals responsible for the bulk of serious crime work infrequently, in short spells, and for low pay. In the data used in this paper, for example, less than 32% of felons work in formal jobs each quarter, taking home $2,220 a month on average when they do. A large theoretical literature in economics beginning with Becker (1968) and Ehrlich (1973) argues that such abysmal labor market outcomes explain these individuals' decisions to commit crimes. When the returns to legal work are low, the relative attractiveness of illegal income increases and the potential costs of crime in terms of forgone income while incarcerated decreases. Poor labor market prospects, the theory claims, therefore incentivize individuals to reallocate their time and efforts towards crime.[1]

Identifying the causal effect of labor market opportunity on criminal offending, however, is difficult for two reasons. First, the researcher needs exogenous, individual-level variation since offending itself often causes job loss and aggregate variation (e.g., a local recession) can also influence offending through contextual factors such as the prevalence of criminal opportunities and the activities of peers (Freedman and Owens, 2016). Second, because the jobless, naturally, spend less time at work, the researcher needs variation in legal income conditional on employment status to distinguish the incentive effects of job loss from the hypothesis that "idle hands are the devil's workshop" – i.e., that more leisure time leads to more offending regardless of incentives, an effect that is quantitatively important in other contexts, such as schooling (Jacob and Lefgren, 2003).

This paper uses linked employer-employee wage data and administrative records on crime to overcome these challenges. To isolate exogenous firings, I study events where groups of

[1] Many policymakers agree. For example, in 2016 the Council of Economic Advisers argued that increasing the minimum wage to $12 by 2020 would eliminate half a million crimes annually (Council of Economic Advisers, 2016). In the same spirit, more than 150 cities and 28 states have adopted "ban the box" laws, which limit when employers can ask job seekers about their criminal histories, in an effort to help ex-offenders find work and stay out of prisons (Rodriguez and Avery, 2017), an important issue given that 76.6% of individuals released from incarceration are re-arrested within 5 years (Durose, Cooper and Snyder, 2014).

individuals are laid off due to large firm-level declines in employment. To account for changes in local contextual factors that may coincide with the layoff, I estimate the effects of job loss on offending, earnings, and employment relative to coworkers who were not initially fired. And to distinguish idle hands from economic incentives, I exploit a kink in unemployment insurance (UI) schedules that provides variation in legal income conditional on unemployment. This last exercise provides a direct test of whether offending responds to economic incentives as predicted by Becker-Ehrlich models.

I implement the study utilizing administrative data on arrests, charges, and incarceration for 340,000 criminal offenders in Washington State – a population responsible for 70-75% of annual felony charges statewide – linked to complete earnings histories from state UI wage records. Access to this large sample allows me to subset to a smaller analysis sample of high-tenure (for this population) workers who separate due to firm-level employment shocks. Because only offenders are included in the sample, in most analyses I focus on criminal activity after the first offense to avoid any selection on the outcome variable of interest. Since repeat offenders account for 80-90% of all felony charges in Washington over the 2000s, however, recidivism is arguably the most important behavior to study.

The results show that criminal offending spikes in the quarter of job separation by 30-50% of the mean, depending on the measure. Offending risk then remains elevated for several quarters and does not converge to pre-job loss levels until two years later. These effects translate into a 30% increase in the likelihood of any offending in the three years after job loss relative to coworkers who were not initially fired. Effects tend to be larger for felonies and more serious crimes, with the biggest impacts found for property offenses (e.g., thefts and robberies) and domestic violence offenses. The latter category increases by more than 100% of the quarterly mean at the time of separation.

Both economic incentives and idle hands are potentially important channels. Half of job separators fail to find work in the quarter after their layoff and they remain 18 p.p. less likely to work three years later. This translates into a \$5,000 drop in quarterly income at the time of job loss and a \$1,200 reduction three years on. Only a small part of the employment and income declines are explained by incarceration, which is about 1.5 p.p. more likely for separators at the same time horizon. If all offending effects operate through income, these declines would imply a semi-elasticity of quarterly offending rates to quarterly income of about 0.1.

These results are robust to very granular controls for the timing of events and the characteristics of firms and workers. Since the analysis includes individual fixed effects, the primary threat to identification is that firms select whom to fire based on changes in individual offending risk over time. That job separators show no difference in offending rates in the three years before the layoff suggests this is unlikely. The events studied are also unlikely to suffer from "reverse causation" due to groups of co-workers committing crimes together and being fired as a result, which would mechanically generate positive effects at the time of the layoff. Roughly 1% of the events have enough workers committing crimes at the time to possibly induce misclassification and estimates remain large and significant for offending that occurs strictly *after* the layoff.

After establishing the overall effects of layoffs, I exploit a kink in Washington State's UI benefit schedule, which is capped at a maximum weekly value, to test for the effects of economic incentives alone. This kink induces exogenous variation in the legal income the unemployed would forgo if apprehended for a crime, but is unlikely to affect incapacitation through time at work or in other activities. In fact, since additional UI benefits should discourage search effort (Card et al., 2015*b*), any incapacitation through search or work would generate *decreases* in crime at the kink. Instead, I find that crime increases at the kink and thus that UI benefits have a mitigating effect on the likelihood of offending after job separation. The implied elasticity of UI benefits to offending in the three quarters after separation is -0.5 (or a semi-elasticity of about -0.1).[2] As in the separation results, property

[2] This effect should be viewed as smaller than the semi-elasticity implied by the job separation estimates

crimes and domestic violence offenses respond most strongly.

These estimates are robust to a wide range of bandwidth choices and are not driven by covariate imbalance or sorting at the threshold. Since Washington State has updated and adjusted their UI benefit schedule multiple times over the sample period, the results are identified off many points in the pre-displacement earnings distribution, making them unlikely to be spurious. Consistent with the literature on UI benefit generosity and job search, log non-employment duration also responds at the kink to a degree consistent with what has been reported by other authors using similar research designs (Card et al., 2015*a*,*b*).

These results support a substantial theoretical literature in economics that models criminal behavior as the result of a utility maximizing choice over the expected costs and benefits of legal and illegal activities. Becker (1968) first formalized the idea that offenders respond to the expected *costs* of crime through the probability of conviction and the potential punishments faced. Ehrlich (1973)'s work several years later provided micro-foundations for the analysis by modeling individuals' time allocation between legal and illegal work and leisure.

A large literature has examined these theories empirically, producing mixed results (see Draca and Machin (2015) and Chalfin and McCrary (2017) for reviews). Isolating the role of economic incentives specifically has proven difficult, as illustrated by two early experiments in Baltimore, Texas, and Georgia that randomly enrolled recently released prisoners into UI schemes. These experiments found zero effects on recidivism, but large decreases in work and job search. Subsequent analyses suggested that the intervention's effects on the incentives to desist from crime were washed out by the increase in leisure time also generated by the treatment (Mallar and Thornton, 1978; Rossi, Berk and Lenihan, 1980). My results provide more direct support for Becker and Ehrlich's original theories by isolating the role of economic incentives specifically.

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given the three quarter offending window used here.

The results are also consistent with a large literature testing the reduced-form relationship between unemployment or wage growth in a given region or industry and local crime or recidivism rates (see, for example, Raphael and Winter-Ember (2001), Gould, Weinberg and Mustard (2002), Lin (2008), Yang (2017), Siwach (2018), as well as Ehrlich's original paper). After an early history that produced mixed and contradictory results, recent work with more credible research designs finds the expected relationship between property crime and both unemployment and wages, although estimates vary and appear sensitive to the time period studied and controls included.[3] A key contribution of this paper relative to these studies is to combine administrative data on both offending and earnings, which allows me to exploit idiosyncratic, individual-level variation in employment instead of making comparisons across local labor markets or industries. To my knowledge, this is the first paper to do so with U.S. data.

The findings also endorse the results of Bennett and Ouazad (2016), who study mass layoffs and crime in Denmark. These authors find that job displacement in a mass layoff increases the likelihood of property offenses by 0.38 p.p. in the year of separation and provide evidence that offending spikes four and seven years post-displacement are driven by changes in social insurance programs. Though smaller absolutely, these effects are similar to this study's when compared to mean rates in the respective samples. In addition to contributing evidence from the U.S., which has substantially different criminal justice policies, labor regulations, and social insurance programs than Denmark, my data allow for higher frequency analysis and a more in-depth investigation of the role of incentives.

In what follows, I first describe the conceptual framework in Section 2. I then describe the sample and data construction in Section 3, present the job separation results in Section 4, test for incentive mechanisms in Section 5, and conclude in Section 6.

[3] In a 1999 review, Freeman argued that "if your prior was that the relation was overwhelming, you were wrong. Joblessness is not the overwhelming determinant of crime that many analysts and the public a priori expected it to be." The U.S. experience in the 2008-09 recession, during which the crime decline that began in the 1990s accelerated, has cast further doubt on the importance of labor market outcomes for criminal behavior. I discuss the 2008-09 recession further in the conclusion of this paper.

2 Conceptual framework

The simplest possible rational model of crime – as examined by Becker (1968) – considers a discrete choice between illegal activity, which with probability $1-p$ generates utility U_i from successfully committing a crime without being apprehended and with probability p generates utility U_s from being caught and sanctioned, and a non-crime alternative, which delivers U_l with certainty. The non-crime alternative is typically thought of as a legal wage. The utility from crime, however, can include income generated by drug sales, thefts, or robberies as well as the direct utility from non-income generating crime (e.g., using illegal drugs).

This simple framework predicts that crime should fall when either the probability of apprehension (i.e., p), the severity of punishment (i.e., U_s), or the value of the non-crime alternative (i.e., U_l) increases. More complicated models, such as the time allocation model in Ehrlich (1973) or the dynamic model in Lochner (2004), generally yield similar predictions. In Ehrlich, for example, the difference between the illegal and legal "wage" determines hours spent generating illegal income vs. holding a formal job.[4]

Job loss can be modeled as a shock to the value of the non-crime alternative. This shock may operate both through a temporary reduction in legal income (given UI replacement rates below one) or long-term unemployment scarring due to the destruction of job-specific human capital, skill depreciation, or statistical discrimination against either the long-term unemployed or individuals with criminal records (Jacobson, LaLonde and Sullivan, 1993; von Wachter, Song and Manchester, 2009; Kroft, Lange and Notowidigdo, 2013; Jarosch and Pilossoph, 2017; Agan and Starr, 2017). As such, it may increase the incentives to commit crimes.

[4]The Ehrlich model is significantly richer than the standard Beckerian framework because individuals allocate time between leisure, legal work, and illegal work under uncertainty about both whether they will find a job and whether they will be caught and sanctioned for their crimes. As in a standard labor supply model, changes in the legal wage can induce offsetting income and substitution effects on hours worked, and likewise with the returns to illegal activities. Many predictions thus depend on curvature in utility and labor-leisure complementarities.

Job loss may also affect crime through channels that do not involve economic incentives, however. First, job loss itself, which has been shown in other contexts to have large effects on mortality (Sullivan and von Wachter, 2009), may have a direct effect on some crimes that respond to psychological distress. Second, job loss increases time spent at home. Since the most criminally active tend to also be unemployed, this may increase offending through interactions with criminal peers in job losers' neighborhoods (Ludwig, Duncan and Hirschfield, 2001; Kling, Ludwig and Katz, 2005; Ludwig and Kling, 2007; Damm and Dustmann, 2014; Kirk, 2015; Billings and Schnepel, 2016). More time spent at home may also increase encounters with criminal opportunities and victims.[5] Both effects are evident in Jacob and Lefgren (2003)'s study of the incapacitation effects of schooling, which finds that property crimes decrease when school is in session but violent crimes increase due to the concentration of offenders and potential victims. After credibly estimating the reduced form effect of job loss on offending, the goal of this paper is to highlight the role of economic incentives in explaining the results relative to these alternative theories.

3 Data and sample

This section describes the data on arrests, charges, convictions, incarceration, and earnings that are used in subsequent analyses. I also describe the sample construction process, define several key variables, and provide descriptive statistics.

Arrest data come from a statewide database maintained for conducting criminal background checks. The database contains detailed records on arrests from the 1970s to the present for all offenses that lead to the recording of fingerprints. Fingerprints are almost universally taken for felony arrests but are often omitted for misdemeanor or traffic offenses.[6]

[5] The incidence of "white-collar" crime, such as fraud or embezzlement, is very low in this sample.

[6] A 2012 state audit of the arrests database found that more than 80% of cases disposed in Superior Court, which hears all felony cases, had a matching arrest. Only 58% of cases heard in courts of limited jurisdiction, which hear misdemeanor offenses, could be linked to arrests. Missing arrests were concentrated in DWIs and misdemeanor thefts and assaults.

I supplement arrest data with statewide records from court cases, which provide a very comprehensive measure of all interaction with the criminal justice system. These data contain detailed information on the outcomes of cases filed in all courts across the state, including juvenile and municipal courts, and is used by state agencies to conduct policy analysis mandated by the legislature. The data cover 1992 to 2016 and includes more than 15.9 million charges for more than 2.9 million individuals. Charge data include the dates of offense, charge filing, and disposition. In what follows, I date charges (as well as arrests) using the date of offense to avoid lags due to delays in detection or apprehension and court proceedings.

Finally, I include records from the Washington State Department of Corrections (DOC). DOC supervises all individuals sentenced to incarceration or probation, which includes virtually all felony offenders and some misdemeanor offenders.[7] The full sample consists of individuals released from DOC supervision between 1988 and 2016 and includes more than 340,000 offenders.

Detail on this population's demographic characteristics and offense history is provided in Table 1 Panel A. The sample is predominately white and male. On average offenders are 31 years old when they first come under DOC supervision and re-appear for a total of 3.5 separate supervision spells. Less than half of these spells are for new offenses – many represent probation violations and parole revocations. Most individuals are never incarcerated and serve probation sentences only. When they do serve time, their sentences are typically shorter than a year. As with most state prison systems, the majority of offenders committed property, violent (typically simple assaults), or drug offenses.

I link the sample of DOC offenders to quarterly earnings data from the State's UI system. The records were linked based on Social Security numbers collected and verified by the Social

[7] Washington State's 1984 Sentencing Reform Act technically eliminated probation and parole, although supervision and non-incarceration sentences managed by DOC survived in other forms and were substantially re-introduced in 2000.

Security Administration, which ensured a high match rate. Ninety one percent of offenders appear in earnings data at least once; the remaining 9% appear to be missing due to a lack of work, as opposed to poor quality identifiers. The earnings data details pay by employer for each quarter from 1988 through 2016 and includes information on the industry and county of the job. All earnings data is winsorized at the 95th percentile within quarter and inflated to 2016 dollars using the CPI-U West.[8] Like most UI wage records, however, the data do not include start and end dates for employment. Therefore, I define a separation as the last quarter in a series of consecutive positive earnings observations in a worker-firm pair.

Overall, work in the formal labor market for ex- and future felons is rare, short, and poorly rewarded. As shown in Table 1 Panel B, 70% of individuals of working age do not appear in earnings records each quarter. Employment spells are typically shorter than a year and pay roughly $1,900 a month (median). The majority of jobs are in the manufacturing, construction, retail trade, janitorial / waste services, and accommodation / food industries, which together comprise roughly 70% of total employment.

These means are consistent with existing literature that studies ex-offenders' employment and earnings in administrative data (e.g., Grogger (1995); Kling (2006)). Survey-based measures of ex-offenders' employment, however, typically show more labor market activity, likely because informal employment accounts for a meaningful share of future and ex-offenders' income (Holzer, 2007). Since the objective of this paper is to study the impact of job loss on offending rather than the effects of incarceration on earnings or related topics, potential underreporting in the UI data is less of a concern here.

Since the 1990s, ex-offenders' employment situation has deteriorated. As shown in Figure 1 Panel A, the share of individuals in the sample reporting positive earnings each quarter has declined by 5-7 p.p. since 1990 after adjusting for age. Most of these declines is accounted for by sharp drops in employment in manufacturing, construction, wholesale and retail trade,

[8] The data contain several large outliers, but are not sensitive to alternative winsorization thresholds (i.e., 90th or 99th percentiles).

and administrative and waste services, as shown in Panel B. Employment in these industries has failed to recover to rates seen in the early 2000s after the 2008-09 recession. Employment in other industries, such as food services, has remained relatively stable.

The dashed and dotted lines show that employment rates tend to be higher for individuals before their first spell under DOC supervision, which represents the individuals' first major criminal event and is responsible for their inclusion in my sample. However, all individuals suffered sharp declines in the 2001 and 2008-09 recessions, suggesting a pattern specific to low skill workers rather than ex-offenders alone. Comparison to American Community Survey (ACS) data confirms this: a demographically matched sample of ACS data for individuals with 12 years of education or less shows similar declines from 2000 to 2013 compared to my sample (available upon request).

Employment status is highly correlated with offending rates. Table 2 reports the fraction of people experiencing any incarceration, probation, and new offending in an average quarter separately for those who are employed, separating from their jobs, and with no income that quarter. The top two rows show that incarceration and probation rates follow a predictable pattern: individuals with positive earnings are less likely to have experienced incarceration or probation in that quarter. The bottom four rows show that employed individuals also have a lower risk of committing offenses that lead to new charges or arrests. Unemployed individuals, for example, are more than three times as likely to commit a felony offense than those employed. Job separators, however, have the highest risk, with offending and arrest likelihoods roughly 50% higher than the unemployed.

Since I observe the full earnings histories of individuals under DOC supervision at any point over the past three decades, this sample is well suited to analyzing the employment and earnings dynamics that precede an individual's first serious encounter with the law in Washington State. However, to avoid potential selection on the outcome variable of interest, I focus on earnings, employment, and offending after the event that led the individual to come under DOC supervision for the first time. The analysis thus speaks to effects on re-offending, although estimates are similar when all periods are considered. Since this population is responsible for a large share of serious crime committed in the state, however, the estimates are also relevant to explaining aggregate crime. Over the full sample period, individuals in the data account for 70-75% of annual felony charges and 65-70% of felony offenders recorded in court records. They account for 40-45% of all charges, including misdemeanor and traffic offenses, in Washington State courts in an average year.

4 Separation event studies

This section presents the results from event study analyses that estimate the causal effects of job loss on individuals' employment, earnings, and offending. It first describes the strategy used to identify exogenous job separations and the estimation strategy used in the resulting sample. It then details the results for labor market outcomes in graphical and numerical form. Finally, it presents effects on various categories of offending, including specific crime types.

4.1 Identification strategy

As noted in the introduction, a simple regression of offending on job loss is likely to be biased by reverse causality. In addition, other unobserved factors such as health crises and peers' activities may simultaneously cause both increases in offending and job loss. To overcome these issues, I focus on events where multiple individuals leave the same firm in the same quarter, making it likely that labor demand factors such as plant closures and head-count reductions generated the layoff.

Specifically, I focus on:

- Employees with at least four quarters of tenure, aged 18 to 65, and not deceased.
- Firms with at least 10 employees meeting these restrictions and where 30-90% of these employees separate in a single quarter.
- Firms without large fluctuations in the year before the layoff (>30% increase or decrease).
- Firms where employment remains below pre-layoff levels for at least four quarters, to rule out temporary furloughs or layoffs followed quickly by recalls.
- Events where no more than 50% of separating workers are employed at the same firm post-layoff, to avoid mislabeling changes in administrative firm identifiers or acquisitions and mergers as layoffs.

These restrictions make it very unlikely that offending itself or other factors cause individuals to lose their jobs, rather than the reverse. In fact, joint offending by multiple individuals at the same firm in the same quarter is rare in the data. About 1.3% of the layoff events that meet these sample restrictions also have more then 30% of workers committing offenses that quarter. Since I date offenses by the quarter in which they were committed, any offending prior to the quarter of the layoff that could also potentially contribute to the firing will appear in the results as a pre-trend.

While the restrictions used are similar in spirit to those used in the mass layoff literature beginning with Jacobson, LaLonde and Sullivan (1993), they are adapted to the sample and context at hand. Given the uniquely low levels of labor market attachment in this population, using the same restrictions as in the broader mass layoff literature would clearly not be possible. For example, less than 0.6% of employment observations in the data occur during same-employer spells lasting five years or longer and about 1% of individuals have any employment relation lasting more than five years. Rather than identifying a sample of displaced workers as they are traditionally defined, the point of these restrictions is instead simply to isolate a sample of exogenous job separations.

Unlike in Jacobson, LaLonde and Sullivan (1993) and other papers studying mass layoffs, I also do not require that individuals have positive earnings in every quarter before and after the event, either in the core specification or in robustness checks. This restriction would eliminate most of the sample. Since some individuals go to prison after offending and mechanically cannot have positive earnings, this restriction would also induce selection into the severity of offenses captured. The results are similar, however, if only individuals who report *at least one* quarter of positive earnings at some point after the layoff are included.

After applying these restrictions, I obtain roughly 450 layoff events. These events are spread across the sample period, but are more frequent during downturns. 111 layoffs occur in 2008 and 2009, for example, while 31 occur in 2001 (on average there are 22.4 per year). The industries represented reflect ex-offenders' overall employment patterns, with most events occurring in manufacturing, construction, waste services, and administrative and food services (66% of all events).

Firms have about 15 employees that meet the sample restrictions at the time of the layoff on average. About 6 to 7 of these workers separate, leaving roughly 2,800 total job separators in the sample. An additional 3,400 workers who meet the same sample restrictions also worked at these firms at the time of the layoffs, but did not lose their jobs. This group, whom I call "stayers" hereinafter, will be included in the analysis as a control group. Since many of these stayers will also separate in the future, this serves to attenuate some effects, but ensures that the results are not driven by firm-level shocks common to all workers regardless of their employment status, such as other factors coincident with local downturns (i.e., changes in property values). Including stayers also allows me to more accurately assess pre-trends before the employment spell began, when individuals are more likely to be unemployed and have higher average offending rates.[9]

[9]Results without stayers, reported in the Appendix, demonstrate these two impacts, but find similar effects overall.

The characteristics of separators and stayers are reported in Table 3. Separators are 38 years old on average and have been working at their firm for slightly under two years, as shown in columns 4, 5, and 6. They share similar gender and race characteristics to the overall population of Washington State offenders, but are less likely to have served time incarcerated. On average, separators committed roughly four felony offenses between 1992, when the criminal history data begins, and their layoff.

Stayers tend to remain at their firm for almost two extra years on average, as shown in columns 7, 8, and 9. They have similar characteristics to separators at the time of the layoff, although they are slightly older and have spent about two extra quarters with the firm. Stayers' and separators' criminal histories are similar, however, with roughly a quarter of both groups serving time in prison and virtually all offenders having served time on probation. Nevertheless, since separators and stayers may differ in other unobserved ways, I include individual fixed effects whenever possible to absorb any persistent differences in the likelihood of offending across these two groups. Very similar results are also found if re-weighting methods are used to balance separators and stayers along key covariates.

To estimate the effects of layoffs, I use the following event study model:

$$y_{it} = \xi_q + \alpha_i + \sum_{s \in S} \beta_s \cdot D_{it}^s + \sum_{s \in S} \beta_s^T \cdot T_i \cdot D_{it}^s + u_{it} \tag{1}$$

where ξ_q are fixed effects for quarters since first release from DOC supervision, α_i is an individual fixed effect, D_{it}^s equals 1 when individual i is s periods from job separation, and T_i equals 1 if individual i was laid off at $s = 0$. The ξ_q fixed effects are included to control for declining offense hazards after initial conviction.[10] The primary coefficients of interest are the β_s^T, which capture the effects of layoffs on separators relative to stayers.

[10] One might also consider including time fixed effects instead of ξ_q. As with most of the country, crime in WA has generally been declining since the 1990s. Results are highly similar using time fixed effects. Both sets of fixed effects can be included by dropping two categories of one fixed effect (instead of the usual one), which also yields similar results.

With individual fixed effects, the β_s^T are identified by the *timing* of layoffs within each individual's employment and offending history, rather than correlations across individuals. While identification does not therefore require that firms randomly assign layoffs, the event must not coincide with periods when workers were likely to commit new offenses regardless of their employment and earnings. By including the stayers, this restriction is weakened further: the layoff's timing must be orthogonal to other factors that also influence offending *for separators only*.[11]

Finally, in addition to the sample restrictions described above, I focus on individuals with at least 12 quarters of valid data pre- and post-layoff that meets the sample restrictions. In order to make any pre-trends visually obvious, I normalize $s = -3$ to zero.

4.2 Earnings and employment effects

Job loss causes immediate declines in employment and earnings that recover quickly, but persist three years after the layoff and beyond. Panel A of Figure 2, which plots the event study coefficients β_s^T when the outcome is an indicator for having positive earnings in the quarter, shows that half of job separators find no work in the quarter after separation and that they remain roughly 18 p.p. less likely to be working up to three years later. About 75% of stayers are still employed at the same time horizon, so these effects represent a 24% decrease in employment rates. The effects are also large relative to the mean employment rate in the mass layoff sample, which is roughly 65%.

Panel A also shows the mechanical effect of my sample restrictions, which require both separators and stayers to be employed at their firm for at least four quarters pre-layoff. Hence both groups have 100% employment rates over this period, forcing the β_s^T to zero exactly. However, separators are less likely to be employed five quarters or more before the

[11] If firms selectively lay off individuals whom managers know *will* commit a crime this quarter regardless of their employment status, for example, the estimates may be positively biased.

layoff, which reflects the tenure differences between the two groups noted in the previous subsection.

Panel B shows that these decreases in employment are accompanied by large declines in quarterly income. Separators make about \$5,000 less immediately after their layoff and continue to earn \$1,000 less each quarter than stayers three years later. About half of this effect stems from lower overall employment rates: earnings conditional on positive remain about \$500 lower for separators after three years, as shown in Panel C. Relative to mean earnings for separators at $t = -1$ (\$9,257 per quarter), these effects represent a total earnings reduction of about 54%, in line with the effects on employment. The \$500 reduction in earnings conditional on positive after three years represents a 6% reduction relative to pre-layoff levels.

Estimates in table form corresponding to Panels A-C are presented in Table 4 for periods $t = -5$ to $t = 4$. As is obvious from the pictures, only employment shows a pre-trend significantly different from zero. And as noted above, the difference for $t = -2$ to $t = 0$ is not interpretable, since both separators and stayers have 100% employment rates over this period by construction. Differences before $t = -3$ reflect separators' lower average tenure at the time of the layoff. As I demonstrate below, however, these differences in employment rates do not generate corresponding differences in offending.

Layoffs thus have large and lasting impacts on employment and earnings. The findings are consistent with other studies focused on workers with higher earnings and stronger labor force attachment, such as Jacobson, Lalonde, and Sullivan's original 1993 study. It is perhaps surprising that workers in this sample, who tend to hold lower-skilled jobs, experience such long-term effects of job loss. While job-specific human capital may be lower for these types of workers, application screening and criminal background checks may also make it particularly challenging for ex-offenders to find new work. That a significant portion of the long-run effects on income comes through the extensive margin supports this idea.

These employment and earnings impacts may provoke criminal behavior both directly through economic incentives as well as indirectly through the loss of incapacitation through time at work. In the next subsection, I estimate effects on offending that capture both potential channels and save discussion of mechanisms for the final part of the paper.

4.3 Impacts on offending

Across multiple measures of criminal activity, layoffs generate an immediate response. Panel A shows the effect on an indicator for committing any offense that leads to new criminal charges, the most comprehensive measure of criminal activity available in the data. Offending risk is 2.2 p.p. higher in the quarter of job separation and 1.8 p.p. higher the quarter after. These effects reflect increases of roughly 40% relative to separators' average offending rates in the four quarters before the layoff and converge to zero after about five quarters.

Panel B shows that very similar effects are found when using an indicator for any fingerprinted arrest as the outcome variable. Arrest risk spikes by about 2.3 p.p. in the quarter of separation and 1.2 p.p. in the following one. These effects are about 50% of separators' pre-layoff average quarterly arrest rates.

Panels C and D show that estimates are similar when only relatively serious offenses are considered. Here, the outcomes are indicators for any felony arrest or any felony charges, respectively. While the effects are smaller in absolute terms, they are similar compared to their means. The effect on felony arrests also persists much longer than the overall effect, with point estimates that do not converge to zero until three years after the layoff.

Estimates in table form corresponding to Panels A-D are presented in Table 4. As noted previously, an important threat to identification in this context is reverse causation. That there is no significant difference in offending at $t = -1$ across all criminal outcomes demonstrates that the layoff events included in the analysis are not biased by job separation caused by offending just before the layoff. The lack of pre-trends for felony offenses plotted in panels C and D is particularly important since felony crimes are very likely to lead to job loss. Significant effects for $t = 1$ and later also demonstrate that reverse causation does not explain the results, since these offenses by construction occur strictly *after* job separation.[12]

A second threat to identification lies in omitted factors correlated with the timing of both the layoff and separators' likelihood of recidivism. The β_s^T coefficients for $s \in [-12, -1]$ provide one test for this threat by assessing pre-trends, which appear flat across all offending outcomes. As an additional test, I subject the event study estimates to increasingly granular sets of time-varying fixed effects, such as county-by-year-by-quarter and industry-by-year-by-quarter effects in Appendix Tables 9 and 10. The effect on new criminal charges at $t = 0$ ranges from 0.0205 to 0.0218 (relative to a baseline estimate of 0.0216) across all these estimates, suggesting omitted group-level shocks are unlikely to be driving the results.

Interpreting the intermediate- and long-run effects on all offending outcomes is complicated by incarceration, which rises as a result of the increase in offending. Three years after the layoff, separators are about 1 p.p. more likely to be in state prisons, as shown in Table 5 column 1, which reflects an increase of about 25% relative to the mass layoff sample mean. These effects represent lower bounds on total incarceration, since some individuals may be sent to local jails not captured in my data as a result of post-layoff offending. To account for how increased incarceration may attenuate long-run effects, Table 5 reports effects for the joint outcomes of being arrested *or* incarcerated and being charged *or* incarcerated in columns 2 and 3. Relative to estimates for being arrested or charged alone, these coefficients show much more persistence.

An additional way to test for intermediate and long-run effects is to examine effects on *ever* offending within a fixed window after job loss. To do so, I estimate successive regressions for an indicator for any offending within 1, 2, 3, etc. quarters from job loss on an indicator for

[12] Recall that all offending outcomes are dated using the actual date of offense, not the date charges were filed or the individual was arrested.

job separation at $t = 0$. Since each regression includes just one time period, it is not possible to include individual fixed effects. To account for differences between separators and stayers, I include fixed effects for age interacted with pre-layoff job tenure, as well as indicators for quarters since the individual's first DOC supervision spell. To verify that these controls absorb differences in offending risk between separators and stayers, I also regress offending in periods before the layoff on job separation at $t = 0$ and these controls.

The results are shown graphically in Figure 4. For event-times less than zero (i.e., before the layoff), the coefficient plotted is the effect of job separation at $t = 0$ on offending *in that period only* conditional on the controls. For event times zero or greater, the coefficient plotted is the effect on offending within event times 0 to t. The coefficient at eight quarters, therefore, can be interpreted as the effect of job loss on the likelihood of offending within two years of being laid off. I also include coefficients for offending within event times 1 to t to provide further evidence that reverse causation does not bias the results.

For both arrest offenses (Panel A) and new charges (Panel B), job separation does not predict offending rates prior to the layoff. This effect is essentially identical to what is shown in the pre-trends for Figure 4, except with tenure and age controls instead of individual fixed effects. Offending risk then jumps sharply at job loss and continues to rise, so that after three years job separators are roughly 7 p.p. more likely to have committed an offense than stayers. Effects are nearly as large when the period of the layoff itself is excluded. Since approximately 25% of stayers offend over the same three-year period, these effects represent a roughly 30% increase in risk.

The largest effects on offending coincide with the largest declines in both employment and earnings, making it difficult to distinguish between the effects of incentives and changes in incapacitation. Nevertheless, if the drop in income explained the full effect, the estimates imply a semi-elasticity of offending (measured in new criminal charges) to quarterly income of about 0.1 at $t = 0$ and 0.05 at $t = 1$.

Employment status is clearly an important mediator, however. I demonstrate this in Table 5 by using an indicator for being arrested *and* unemployed as the outcome in Specification 1. These effects, which are presented in column 4, are large and sustained long after the layoff. Moreover, they are much larger than what one would expect if the events were independent (i.e., the product of impacts on employment and arrest). Effects on an indicator for being arrested and employed reported in column 5, however, are negative. Most of the effect on offending, therefore, is generated by individuals who remain unemployed after the layoff.[13]

4.4 Effects by crime type

Table 6 shows estimated effects from Specification 1 for broad crime type categories. Violent crimes (assaults), property crimes, and drug crimes all appear to respond, although effects are only statistically significant in the quarter of the layoff and the quarter afterwards for property crimes, which include trespassing, theft, and burglary. The property crime effect is roughly 100% of the pre-layoff mean and is slightly larger for the quarter after the layoff, when total earnings declines are largest. Since property crimes are usually a source of illicit income, these effects are consistent with economic incentives motivating post-layoff offending. Property crimes may also respond to incapacitation through time at work, however, if the unemployed are exposed to more criminal opportunities to steal or burgle.

Digging further into the assault effects reveals an additional finding: Layoffs also generate large increases in domestic violence offending.[14] This crime category increases by roughly 100% of the pre-layoff mean and intensifies in the quarter after the layoff. While domestic violence may also respond to economic incentives directly, strong effects may also speak to

[13]Note that all individuals are employed by my definitions in the quarter of job separation $t = 0$ by construction, since they must have non-zero earnings from their pre-layoff employer at that time. Thus the effect on offending loads fully onto the "Employed & arrested" outcome at $t = 0$.

[14]Since the outcome is an indicator for any offending in a quarter in each crime category and individuals can commit multiple crimes, results for DV and non-DV assaults need not sum to the overall assault effect.

an important role for the psychological stress of job separation and increased time spent at home with potential victims.[15] These effects also run counter to gender wage gap theories suggested by Aizer (2010), since layoffs likely reduce income gaps between husbands and wives, but lead to more violence.

5 Incentives vs. "idle hands"

As discussed above, distinguishing between the role of economic incentives and incapacitation in the effects of job loss is important theoretically. If the incentive shocks of job loss are unimportant and incapacitation effects appear to dominate, an alternative to Becker-Ehrlich models would be more appropriate. For example, it may instead be that crime is driven by quasi-random interactions with potential victims or co-offenders and that these interactions occur primarily during individuals' leisure hours. In such a "random interaction" model offenders' time spent at home, at work, or elsewhere would be a more important determinant of criminal activity than their income. Nor would such models require offenders to behave rationally or discount the future.

The distinction between incentives and incapacitation is also important for policy. If the latter is critical, policies that keep ex-offenders occupied in training, volunteer work, or education programs would be as effective at reducing recidivism as employment programs. And pure income support programs, such as UI or food stamps, would be ineffective. Many states also exclude ex-offenders from social support programs such as public housing. If economic incentives are an important factor for recidivism, such programs may be counter-productive, but if random interactions are more important, these policies may reduce crime by separating offenders from potential victims and co-offenders.

[15] As Ehrlich wrote in 1973: "Since those who hate need not respond to incentives any differently from those who love or are indifferent to the well-being of others, the analysis...would apply, with some modifications, to crimes against the person as well as to crime involving material gains" (pg. 532).

To investigate whether economic incentives – holding incapacitation constant – impact crime, I exploit institutional features of Washington State's UI system that provide exogenous variation in offenders' legal income conditional on unemployment. Weekly UI benefits in Washington are calculated as a fixed fraction of earnings before job loss, but subject to a minimum and maximum amount. These rules generate "kink points" in benefits as a function of pre-job loss earnings. By comparing ex-offenders' recidivism rates on either side of these kink points I can estimate the effect of the marginal UI dollar on recidivism risk. Similar kinks have been exploited in other contexts to estimate the effect of UI benefits on unemployment duration and re-employment wages (see, for example, Card et al. (2015*a*) or Card et al. (2015*b*)).

In Washington, workers are eligible for UI if they worked at least 680 hours in the year before they were fired. Individuals with criminal records or on probation are eligible for full benefits. Weekly benefits are a fixed fraction of earnings over a base period (see Appendix Figure 7 for an illustration). Since 1990, the base period, benefits fraction, and minimum and maximum benefits for the state's UI program have changed several times. Most recently, benefits were calculated as 3.85% of average gross wages in the two highest earning quarters for the first four of five quarters before claiming. In 2016, minimum weekly benefits were $162 and maximum benefits were $681, corresponding to average monthly earnings of $1,402 and $5,896, respectively. Maximum and minimum benefits generally increase each year by an amount proportional to the increase in state median wages, except for a period when they were frozen in the mid-2000s.[16] The various changes in rules since 1990 are useful in my context, since they imply that estimates are not identified by a single locus in the pre-displacement earnings distribution.

I focus on the maximum threshold since very few eligible individuals have implied benefits below the minimum. I calculate implied benefits for all individuals who separate from their

[16] These two years (2004 and 2005) are also excluded from the analysis, since it is unclear what the actual prevailing maximum and minimum benefits that applied at the time of job separation are, although this restriction does not affect the results except through the sample size.

jobs into non-employment and who appear eligible for UI from 1992 to 2014. Since I do not observe hours, I restrict to separators employed for at least two quarters before job loss and who have base period earnings that imply more than 680 hours of work at the prevailing minimum wage. I then plot the fraction of separators who are charged with a new offense within three quarters of job loss, or roughly the period over which benefits could be claimed, against their implied benefit's distance from the maximum.

Note that since I do not observe actual benefits received, these estimates are reduced form. When calculating an elasticity, I use the change in weekly benefits slope at the maximum kink implied by full compliance. A final estimate of the effect of actual UI *receipt* on offending would need to be adjusted by a first stage estimate of the kink in received benefits as a function of pre-displacement earnings. This adjustment factor could be above or below one depending on how benefit take-up rates vary around the threshold. That results for the effect of UI on non-employment duration presented below are close to other estimates in the literature suggests it is not very different from one.

Figure 5 Panel A plots the first key result. The x-axis is expressed in terms of monthly earnings relative to the amount that would deliver the prevailing maximum benefit at the time of job separation. Thus, all points to the right of the red line would receive the maximum benefit, whereas benefits are increasing in monthly earnings for all individuals to the left of the line. Each dot represents the mean for a $50 monthly earnings bin.

The downwards slope to the left of the threshold implies that individuals with higher pre-displacement earnings (and higher benefits) are less likely to offend after job loss. To the right of the threshold, when benefits are no longer increasing due to the cap, the relationship between pre-displacement earnings and offending flattens considerably, implying a significant mitigating effect of UI benefits on crime. Using a local linear specification in a $1,400 monthly earning bandwidth around the threshold yields an implied reduced form elasticity of UI benefit amounts to three-quarter offending rates of -0.54 (se: 0.12), or a semi-elasticity of about 0.1.

I report regression kink estimates for specific types of offending in Table 7. Although all categories exhibit a change in slope at the threshold, only charges in municipal courts (which includes many alcohol-related crimes), property crimes, and domestic violence offenses have shifts that are distinguishable from zero at conventional confidence levels. A similar set of crimes thus appear to respond to UI as in the layoff analysis.

The main effects are robust to a wide variety of bandwidths, including the bandwidth chosen by the Calonico, Cattaneo and Titiunik (2014) data-driven procedure, as shown in Panel B. In Appendix Table 8 I provide the coefficient estimates underlying the main figure and explore robustness to alternative specifications, including allowing for a level-shift at the threshold (in addition to the change in slope), triangular kernel weighting, and quadratic fits on either side of the threshold. The results are robust to many variations on the linear specification, but smaller and less precisely estimated when quadratic fits are used.

The estimates are also robust to changes in the time window after job separation used to measure offending outcomes and the pre-displacement tenure requirements used to define the sample. Due to the restrictions on pre-displacement hours at the minimum wage and the focus on the maximum earnings benefit, for example, little changes if individuals with three or four quarters of tenure before job loss are used. Results are also similar if offending is measured within two quarters of job loss instead of three.

For this estimate to capture the causal effect of UI on offending, other characteristics of job separators that may also influence crime must evolve smoothly over the threshold. Since there are many such potential characteristics, I perform a simple dimension reduction in order to test for smoothness. Appendix Figure 8 Panel A plots the fitted values from a regression of offending within three quarters of job separation on fixed effects for year-by-quarter, employment spell length, number of employment spells, county and industry, gender and race, and year of birth against monthly earnings relative to the maximum benefit threshold.

Unlike offending, this index does not show a kink. As in regression discontinuity designs, valid regression kink estimates also require no strategic sorting across the relevant threshold. Appendix Figure 8 Panel B tests this requirement by plotting the density of observations around the threshold. The density evolves smoothly, with no apparent bunching on either side.

As a final check on the design, I verify that non-employment duration also responds to UI at the maximum benefits threshold as documented in previous work. I do so in Panel C by plotting the log number of quarters to re-employment using the same bin-size and running variable as in Panel A. While unemployment duration is slightly increasing in benefit amounts to the left of the threshold, it is decreasing to the right. The implied duration elasticity of 0.18 is slightly smaller than the wide range of estimates reported in the literature, which span 0.3 to 2 (Card et al., 2015*a*).[17]

There is little reason to believe that UI benefits around the maximum threshold affect criminal propensities through other channels such as incapacitation. If anything, since offenders become *more* likely to find work quickly to the right of the threshold, any marginal effects on incapacitation through employment or job search would bias effects on offending towards zero. These effects thus point to a direct role for legal income in reducing offending through economic incentives, consistent with Becker-Ehrlich theories. They also suggest that social insurance may play an important role in reducing recidivism, which is a potentially policy-relevant topic for further research.

[17]Like the estimates for offending, this estimate represents a reduced form effect and should be adjusted by UI take-up rates in the eligible population. It should also be interpreted in light of the estimated effects on crime, which may increase time to find new jobs if individuals are re-incarcerated.

6 Conclusion

This paper estimates the causal effects of earnings and employment shocks on criminal activity. I find that job loss leads to sharp drops in employment rates and long-run reductions in earnings. Layoffs also generate sharp spikes in the probability of crime leading to arrest or new criminal charges in the quarter of separation and after, with effects concentrated in property and domestic violence offenses. Three years after a layoff, job losers are about 30% more likely to have committed a crime than their former coworkers. If the offending effects are driven by income alone, the estimates imply a semi-elasticity of quarterly offending rates to quarterly income of about 0.1.

I then show that economic incentives themselves, rather than the stress of job separation or any incapacitation effects of employment, are an important mechanism. I do so using a kink in Washington State's UI benefits schedule at the maximum weekly benefit. These results show that offending in the three quarters after job loss also responds to UI, with property and domestic violence offenses reacting most strongly. This direct evidence of the impact of economic incentives supports traditional rational models of criminal behavior as proposed by Becker (1968) and Ehrlich (1973).

The estimates in this paper are slightly smaller than what has been reported previously in studies that examine the effect of labor market aggregates on crime or recidivism. Yang (2017), for example, shows that a 1% increase in log wages decreases the quarterly hazard of returning to prison for recently released offenders by 0.4%. Since many arrests and charges do not lead to new prison time, this effect should be viewed as larger than my estimates. However, my estimates speak to the effects of individual-level income and employment shocks, not declines in average earnings or employment rates for a local area or group of individuals. The effects of a persistent lack of labor market opportunity, which affect many factors besides individual earnings, likely differ from the effects of the shocks measured here.

The results show that employment programs and income support for ex-offenders could reduce recidivism. Nevertheless, the data also reveal the limited scope for income and unemployment shocks to explain aggregate patterns in crime. Eighty percent of arrestees in the data had no income in the quarter of arrest, implying other factors besides acute earnings shocks may underlie their criminal behaviors. And perhaps most importantly, employment and earnings remain exceptionally low even well before an individual's first arrest.

Ex- and future offenders' low levels of labor force attachment also helps reconcile the disconnect between evidence on the effects of earnings, unemployment, and wages and aggregate time series for crime and the labor market. While patterns in the 1990s conformed to expectations about the effects of improving economic conditions on crime, the U.S. experience in the 2008-2009 recession has decidedly not. If formal labor market work is a relatively rare experience for most individuals at risk to commit a serious criminal offense, other factors such as changes in enforcement, criminal opportunity, demand and supply of illegal drugs, and social contexts may dominate connections between the formal labor market and crime.

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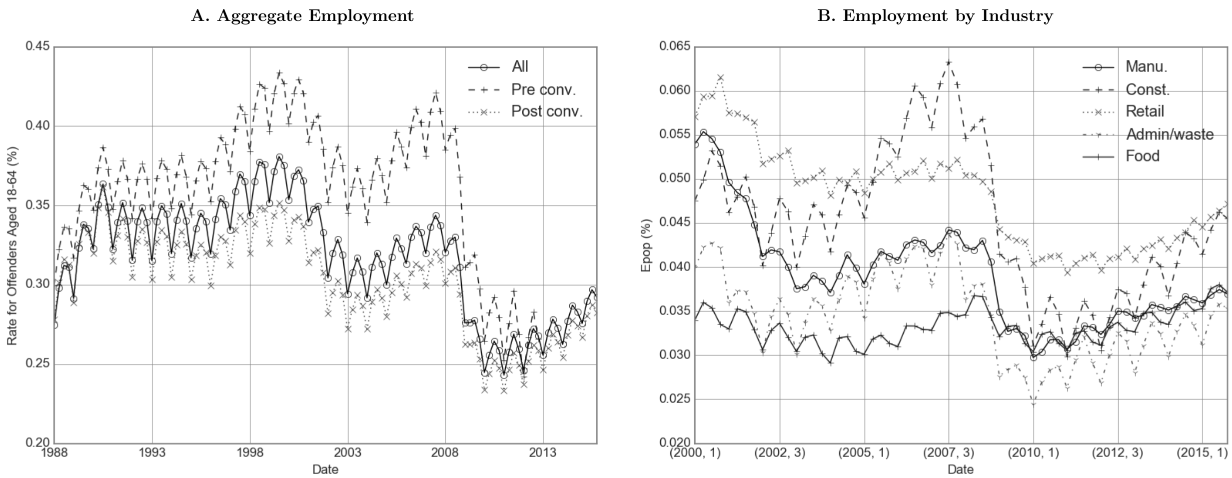
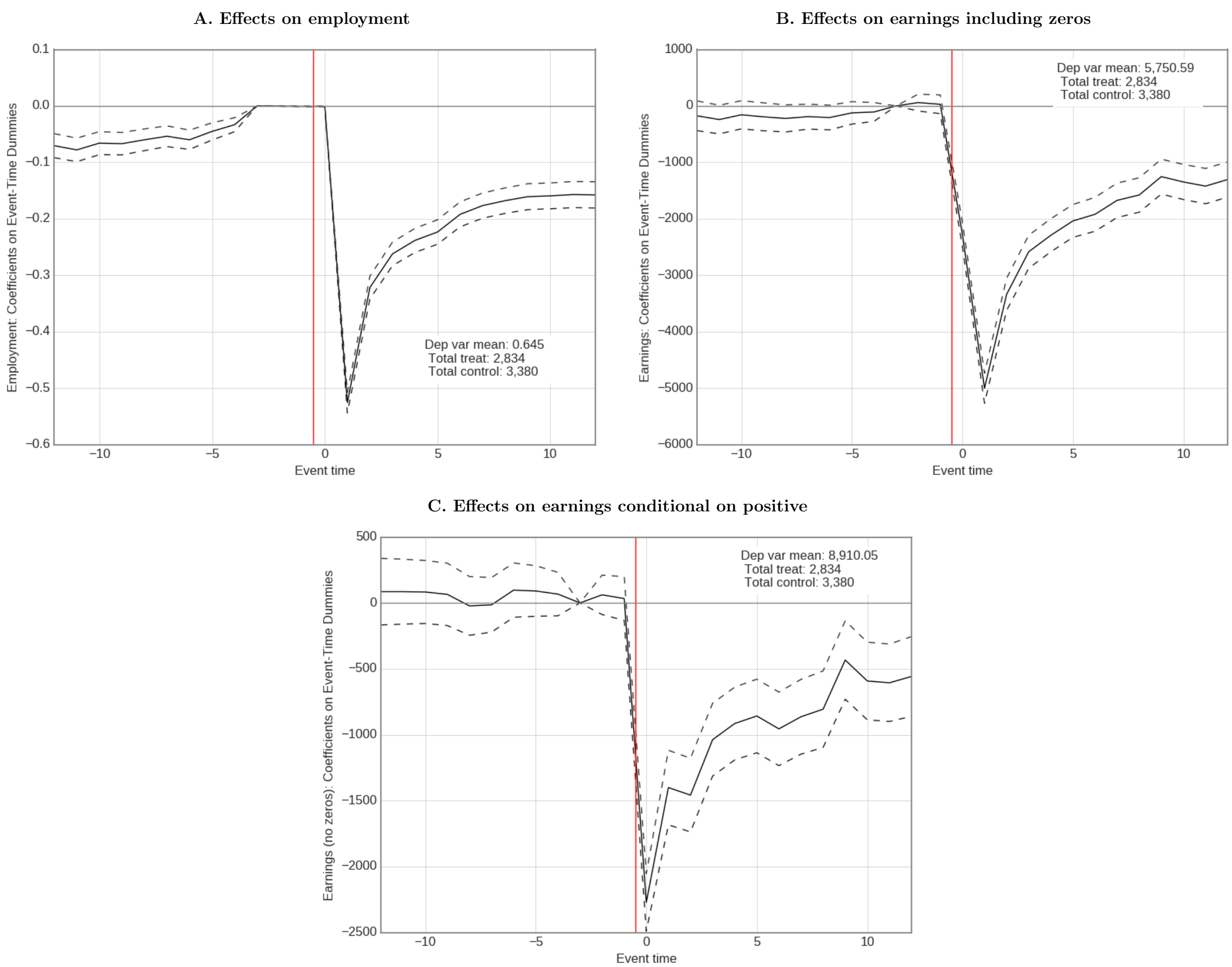
Figure 1: Ex- and future offenders' aggregate employment patterns



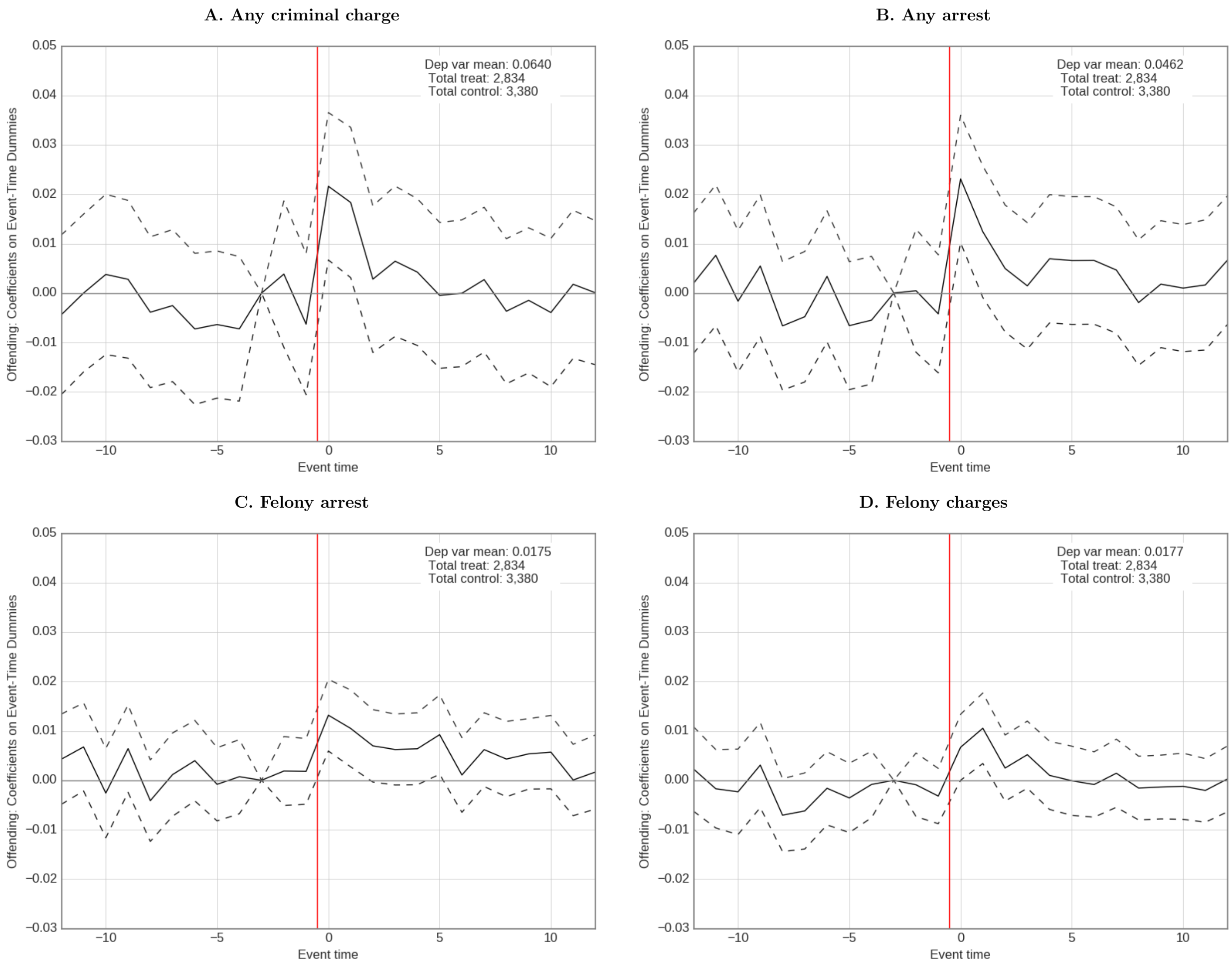
Notes: Sample is as defined in Section 3. Employment is defined as an indicator for any positive earnings in a given quarter. Industry of employment is unavailable pre-2000 and is assigned using the industry of the individual's top-paying firm each quarter. All series are age adjusted by regressing the outcome on a full set of interacted year and quarter effects and a set of dummies for each age (e.g., 20, 21, etc.). Fitted values are plotted for a 30-year-old. Data post-2013 for pre-conviction individuals is omitted due a lack of sample.

Figure 2: Effects of layoffs on earnings and employment



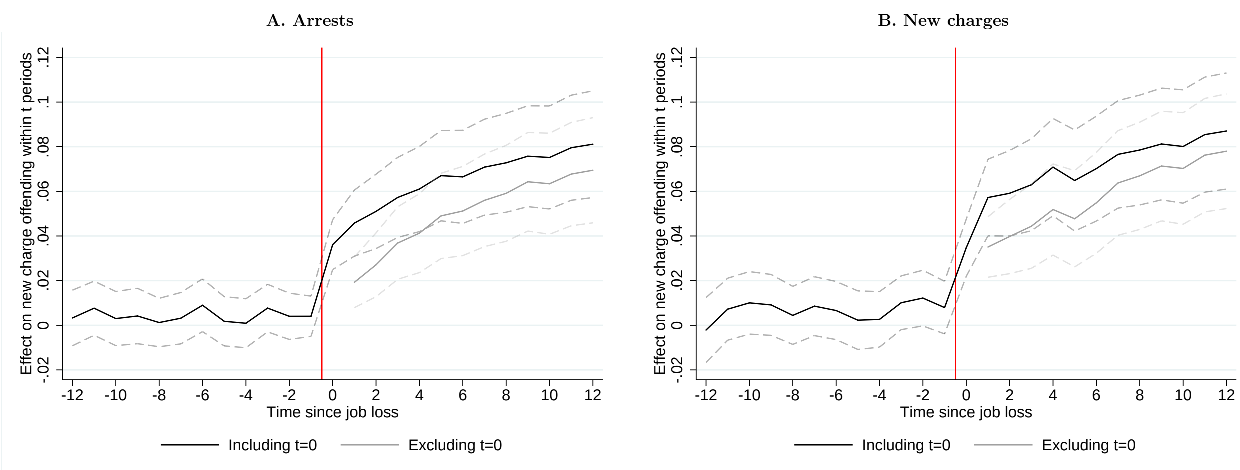
Notes: Figures plot estimates of β_s^T from Specification 1 for the outcome listed in the subheading in the mass separation subsample. Event time is measured in quarters. Effects for event times < -12 and > 12, when the sample is potentially unbalanced, are captured by binned indicators and not shown. Dashed lines represent 95% confidence intervals formed from standard errors clustered at the individual level.

Figure 3: Effects of layoffs on arrests and criminal charges



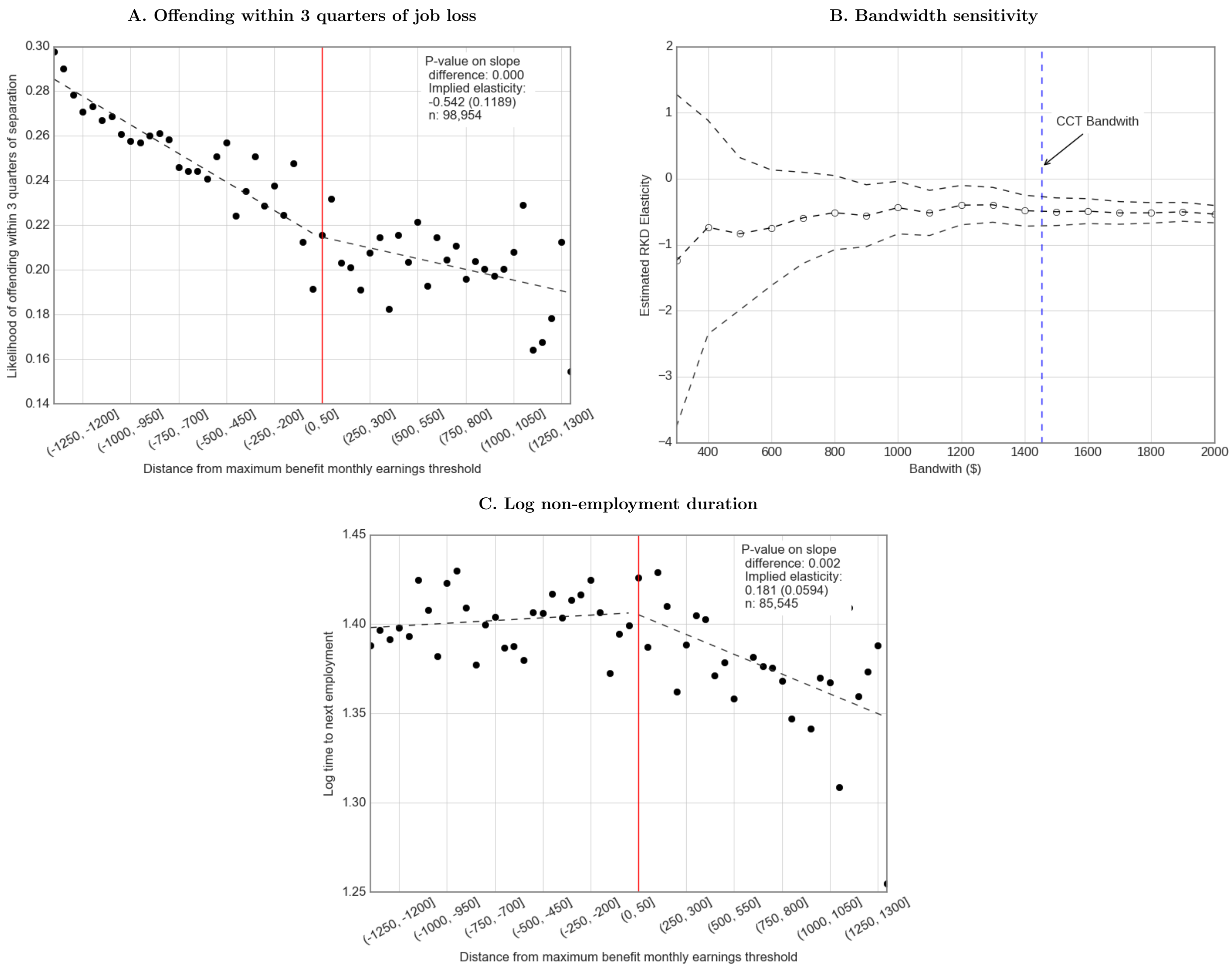
Notes: Figures plot estimates of β_s^T from Specification 1 for the outcome listed in the subheading in the mass separation subsample. Event time is measured in quarters. Outcomes are binary indicators for whether the event occurred at any point in the quarter. Effects for event times $<$ -12 and $>$ 12, when the sample is potentially unbalanced, are captured by binned indicators and not shown. Dashed lines represent 95% confidence intervals formed from standard errors clustered at the individual level.

Figure 4: Effects of job loss on cumulative offending



Notes: In both panels, the coefficients plotted for event times before job loss (i.e., $t < 0$) are the effects of job separation at $t = 0$ (treatment) on any offending in event time t from an OLS regression of the outcome on an indicator for treatment, age-by-pre-layoff-tenure fixed effects, and quarters since first DOC supervision spell effects. Coefficients for event times ≥ 0 are from the same regression but using any offending between event times 0 and t as the outcome. The grey line plots effects for any offending in event times 1 to t. Panel A uses arrests to measure criminal activity, while Panel B uses new criminal charges. Dashed lines are 95% confidence intervals from heteroscedastic-robust standard errors.

Figure 5: Effect of UI on recidivism



Notes: Sample and specifications as described in text. Distance to maximum benefit is calculated using the prevailing maximum benefit amount at time of job loss. Individuals without subsequent employment are excluded from Panel C, reducing the sample size slightly.

Table 1: Summary statistics

A. Demographic characteristics			
	Mean (1)	Median (2)	Std. (3)
Male	0.79	-	0.41
Race			
White	0.79	-	0.40
Black	0.13	-	0.33
Age at first admit	31.01	28.25	10.83
Total spells	3.53	1.00	5.98
New offense spells	1.35	1.00	0.72
Incarceration spells	1.30	0.00	3.01
Probation spells	2.23	1.00	3.02
Never inc.	0.60	-	0.49
Incarceration length (years)	0.92	0.25	2.32
Probation length (years)	1.90	1.00	2.95
Offense types			
Property	0.32	-	0.47
Violent	0.20	-	0.40
Drug	0.24	-	0.43
Sexual	0.07	-	0.26
Unknown	0.16	-	0.36
B. Employment characteristics			
Ever employed	0.91	-	0.29
Employment rate	0.32	-	0.46
Employment spells	4.40	4.00	3.39
Spell length (qtrs)	6.51	3.00	10.88
Mean earnings (qtrs)	2,105	0	4,263
Mean earnings (no zeros)	6,660	5,633	5,211
Industry Emp. Share (post-2000)			
Manufacturing	0.14	-	0.35
Construction	0.17	-	0.37
Retail Trade	0.13	-	0.34
Waste Services	0.14	-	0.35
Accommodation and Food	0.13	-	0.33
Total person-x-quarter Obs.	30,941,422		
Total individuals	342,668		

Table 2: Incarceration, probation, and offending rates by employment status

	All		Employed		Separators		Unemployed	
	Mean (1)	Std. (2)	Mean (3)	Std. (4)	Mean (5)	Std. (6)	Mean (7)	Std. (8)
Incarceration rates	0.057	0.232	0.014	0.117	0.027	0.161	0.075	0.264
Probation rates	0.187	0.39	0.165	0.371	0.239	0.427	0.185	0.388
Offense rates								
Any charges	0.077	0.267	0.058	0.233	0.138	0.345	0.074	0.262
Felony charges	0.024	0.154	0.012	0.109	0.039	0.193	0.026	0.159
Arrest	0.063	0.243	0.036	0.187	0.102	0.302	0.065	0.246
Felony arrest	0.025	0.157	0.011	0.105	0.039	0.192	0.028	0.164
Total person-qtr obs.	30,941,422		6,463,272		3,315,809		21,162,341	

Notes: All includes all individual aged 18 to 65 and not deceased. Employed indicates positive earnings from the same employer in a quarter and the following one. Separating indicates positive earnings from an employer but no earnings from the same firm in the next quarter. Unemployed includes individuals with no earnings. The categories are mutually exclusive and collectively exhaustive. Rates are means of indicators for any occurrence of the relevant variable in a quarter.

Table 3: Summary statistics for mass separation sample

	All			Separators			Stayers				
	Mean (1)	Median (2)	Std. (3)	Mean (4)	Median (5)	Std. (6)	Mean (7)	Median (8)	Std. (9)	Difference (10)	P-value (11)
Age	39.3	38.8	9.39	38.7	38.2	9.56	39.8	39.2	9.24	-1.100	0.0000
Male	0.831	-	0.375	0.823	-	0.381	0.836	-	0.37	-0.013	0.1380
Black	0.131	-	0.338	0.144	-	0.351	0.123	-	0.328	0.021	0.0055
On probation	0.17	-	0.375	0.194	-	0.396	0.152	-	0.359	0.042	0.0000
Earnings at $t = -1$	10,406.2	9,725.2	5,519.6	9,544.2	8,590.0	5,521.3	11,020.1	10,421.8	5,436.2	-1475.9	0.0000
Tenure at separation	10.3	8.0	7.57	9.17	7.0	6.43	11.2	9.0	8.19	-2.030	0.0000
Total tenure	15.6	11.0	12.0	9.17	7.0	6.43	20.3	17.0	12.9	-11.130	0.0000
Previously incarcerated	0.243	-	0.429	0.258	-	0.438	0.232	-	0.422	0.026	0.0056
Prior incarceration length	3.96	0.0	11.9	3.91	0.0	11.1	3.99	0.0	12.5	-0.080	0.7661
Previously on probation	0.973	-	0.163	0.975	-	0.155	0.971	-	0.168	0.004	0.2377
Prior probation length	22.0	14.0	23.8	20.2	13.0	21.2	23.3	15.0	25.5	-3.100	0.0000
Offenses since 1992	13.5	8.0	17.4	14.5	8.0	19.0	12.9	7.0	16.2	1.600	0.0000
Felonies since 1992	3.69	2.0	6.08	3.82	2.0	7.04	3.59	2.0	5.3	0.230	0.0884
Total N.	5,968			2,834			3,380				

Notes: Sample corresponds to core restrictions described in the text. P-values are taken from an OLS regression of the relevant characteristic on an indicator for separation. All duration variables (e.g., tenure, incarceration length, etc.) are measured in quarters. Since the criminal history data begins in 1992, previous offenses and felonies are measured from that date on.

Table 4: Baseline event study estimates

	Labor Market			Crime			
	(1) Employment	(2) Earnings	(3) Earnings > 0	(4) Charges	(5) Arrests	(6) Felony Arrest	(7) Felony Charges
$t = -5$	-0.0451***	-123.1	90.51	-0.00638	-0.00661	-0.000821	-0.00359
	(0.0077)	(101.0)	(97.5)	(0.0076)	(0.0066)	(0.0038)	(0.0036)
$t = -4$	-0.0332***	-107.3	66.95	-0.00726	-0.00549	0.000713	-0.000852
	(0.0063)	(84.7)	(83.6)	(0.0074)	(0.0066)	(0.0038)	(0.0034)
$t = -2$	-0.000454**	59.45	61.37	0.00383	0.000461	0.00186	-0.000923
	(0.00020)	(75.9)	(75.9)	(0.0075)	(0.0063)	(0.0035)	(0.0033)
$t = -1$	-0.000766***	28.87	33.06	-0.00631	-0.00423	0.00179	-0.00321
	(0.00027)	(84.3)	(84.3)	(0.0072)	(0.0061)	(0.0034)	(0.0028)
$t = 0$	-0.000843***	-2277.1***	-2272.3***	0.0216***	0.0231***	0.0132***	0.00667**
	(0.00031)	(110.8)	(110.8)	(0.0076)	(0.0065)	(0.0037)	(0.0034)
$t = 1$	-0.526***	-5003.4***	-1401.0***	0.0183**	0.0124*	0.0105***	0.0105***
	(0.0093)	(135.3)	(143.6)	(0.0077)	(0.0068)	(0.0039)	(0.0036)
$t = 2$	-0.322***	-3339.3***	-1458.1***	0.00280	0.00497	0.00696*	0.00247
	(0.010)	(144.8)	(142.0)	(0.0075)	(0.0065)	(0.0037)	(0.0034)
$t = 3$	-0.262***	-2584.4***	-1037.6***	0.00644	0.00145	0.00622*	0.00517
	(0.011)	(148.4)	(140.2)	(0.0077)	(0.0065)	(0.0036)	(0.0035)
$t = 4$	-0.238***	-2292.8***	-915.1***	0.00423	0.00693	0.00638*	0.000983
	(0.011)	(148.9)	(140.3)	(0.0075)	(0.0066)	(0.0037)	(0.0035)
N	663,948	663,948	428,515	663,948	663,948	663,948	663,948
Dep. Var. Mean	0.645	5750.586	8910.050	0.064	0.046	0.018	0.018
Treated Mean $t = -1$	1.000	9257.411	9257.411	0.057	0.036	0.011	0.006

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table lists estimates of β_s^T from Specification 1 for the outcome listed beneath the column header in the mass separation subsample. Event time is measured in quarters. Criminal outcomes are binary indicators for whether the event occurred at any point in the quarter. Standard errors are clustered at the individual level.

Table 5: Event study estimates for additional outcomes

	Incapacitation			Employment interactions	
	(1)	(2)	(3)	(4)	(5)
	Incarceration	Arrested or incar.	Charged or incar.	Unemployed & arrested	Employed & arrested
$t = -5$	0.00663*	-0.000410	-0.000294	0.00657***	-0.0132**
	(0.0035)	(0.0072)	(0.0081)	(0.0025)	(0.0063)
$t = -4$	0.00335	-0.00233	-0.00480	0.00288	-0.00837
	(0.0027)	(0.0069)	(0.0077)	(0.0020)	(0.0063)
$t = -2$	-0.000867	-0.00271	0.00197	0.00000141	0.000459
	(0.0023)	(0.0066)	(0.0077)	(0.00012)	(0.0063)
$t = -1$	-0.00291	-0.00688	-0.00917	-0.0000286	-0.00420
	(0.0026)	(0.0065)	(0.0076)	(0.00014)	(0.0061)
$t = 0$	0.00354	0.0230***	0.0235***	0.000114	0.0230***
	(0.0030)	(0.0069)	(0.0079)	(0.00014)	(0.0065)
$t = 1$	0.00429	0.0141**	0.0207**	0.0356***	-0.0232***
	(0.0031)	(0.0072)	(0.0081)	(0.0034)	(0.0060)
$t = 2$	0.00635*	0.0108	0.00777	0.0185***	-0.0135**
	(0.0034)	(0.0071)	(0.0080)	(0.0031)	(0.0059)
$t = 3$	0.00825**	0.00695	0.0123	0.0139***	-0.0125**
	(0.0035)	(0.0070)	(0.0082)	(0.0031)	(0.0058)
$t = 4$	0.00904**	0.0127*	0.0115	0.0176***	-0.0107*
	(0.0035)	(0.0071)	(0.0080)	(0.0032)	(0.0059)
N	663,948	663,948	663,948	663,948	663,948
Dep. Var. Mean	0.040	0.084	0.102	0.022	0.025
Treated Mean $t = -1$	0.021	0.056	0.077	0.000	0.036

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table lists estimates of β_s^T from Specification 1 for the outcome listed beneath the column header in the mass separation subsample. Event time is measured in quarters. Outcomes are binary indicators for whether the event occurred at any point in the quarter. Standard errors are clustered at the individual level.

Table 6: Baseline event study estimates by crime type

	(1) Assault	(2) Property	(3) Drug	(4) DV Assaults	(5) Non-DV Assaults
$t = -5$	-0.00363	0.00270	-0.00310	0.00633**	-0.00290
	(0.0028)	(0.0037)	(0.0026)	(0.0028)	(0.0027)
$t = -4$	-0.00328	0.00294	0.00107	0.00193	0.00334
	(0.0026)	(0.0036)	(0.0026)	(0.0025)	(0.0028)
$t = -2$	0.00107	0.00320	0.00104	0.000990	0.00208
	(0.0030)	(0.0036)	(0.0026)	(0.0024)	(0.0028)
$t = -1$	-0.00117	0.00111	-0.0000687	0.00159	-0.000409
	(0.0027)	(0.0034)	(0.0021)	(0.0024)	(0.0026)
$t = 0$	0.00434	0.0102***	0.00256	0.00648**	0.00476*
	(0.0029)	(0.0037)	(0.0026)	(0.0026)	(0.0028)
$t = 1$	0.00327	0.0117***	0.00443	0.00725***	0.00524*
	(0.0030)	(0.0039)	(0.0028)	(0.0027)	(0.0030)
$t = 2$	0.00439	0.00432	-0.00122	0.00250	0.00252
	(0.0030)	(0.0037)	(0.0026)	(0.0027)	(0.0027)
$t = 3$	0.00453	0.00116	0.00229	0.000428	0.000951
	(0.0033)	(0.0035)	(0.0025)	(0.0025)	(0.0028)
$t = 4$	0.00316	0.00416	0.00189	0.00231	0.00248
	(0.0032)	(0.0036)	(0.0027)	(0.0026)	(0.0029)
N	663,948	663,948	663,948	663,948	663,948
Dep. Var. Mean	0.014	0.012	0.009	0.006	0.007
Dep. Var. Mean $t = -1$	0.007	0.011	0.004	0.007	0.006

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table lists estimates of β_s^T from Specification 1 for the crime type listed beneath the column header in the mass separation subsample. Event time is measured in quarters. Outcomes are binary indicator for whether the event occurred at any point in the quarter and are not mutually exclusive, since individuals can be arrested for multiple offense simultaneously. Standard errors are clustered at the individual level.

Table 7: RKD estimates for specific crime types

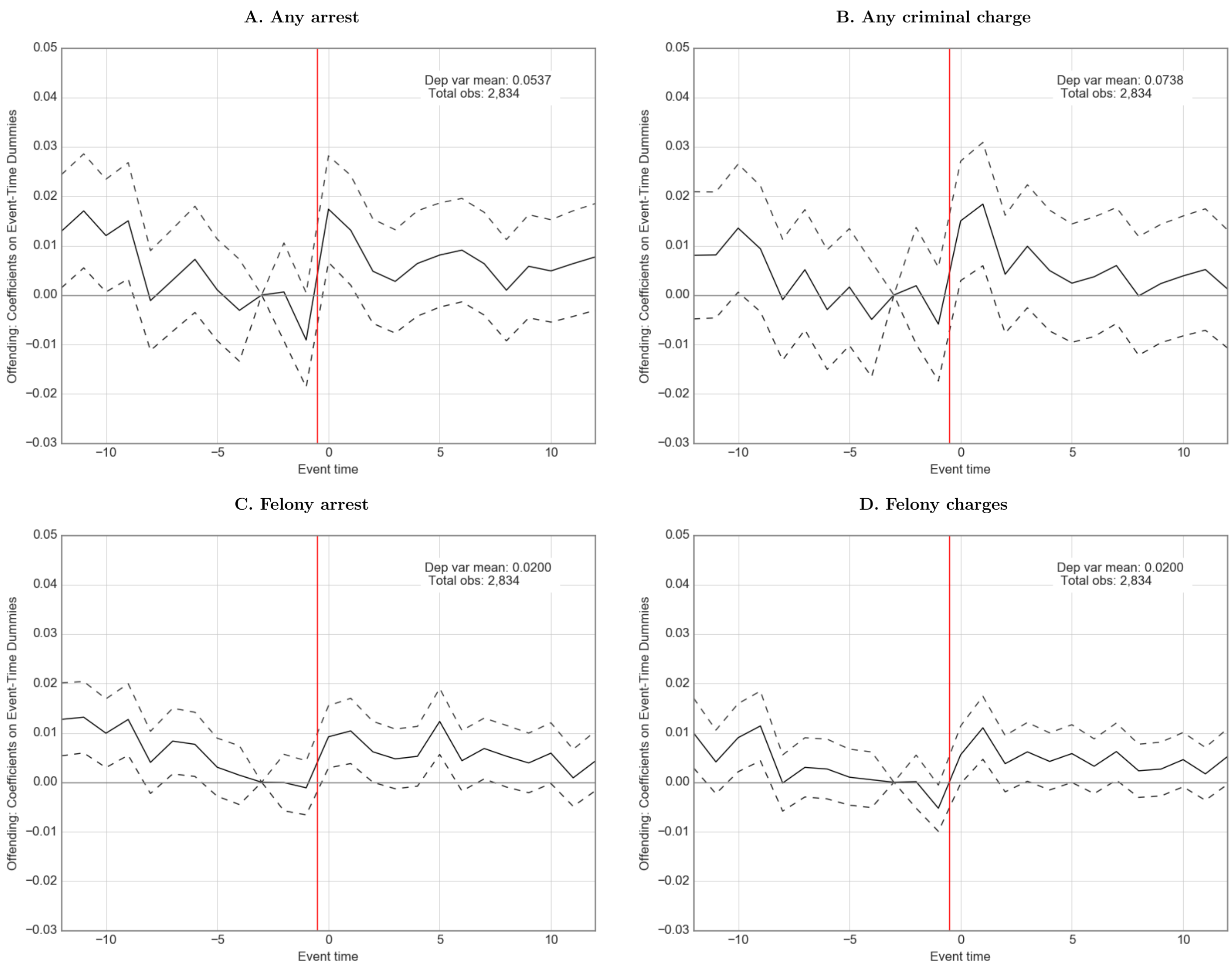
	(1) Felony	(2) Municipal	(3) Drug	(4) Property	(5) Assault (non-DV)	(6) DV
Slope	-0.00100	-0.00285***	-0.00864***	-0.0158***	-0.00398*	-0.00741***
	(0.000731)	(0.000831)	(0.00185)	(0.00217)	(0.00184)	(0.00181)
△ intercept right of thresh	-0.00101	-0.000731	0.000982	-0.00198	-0.00239	-0.000716
	(0.00108)	(0.00119)	(0.00275)	(0.00312)	(0.00278)	(0.00269)
△ slope right of thresh	0.00266	0.00554**	0.00576	0.0167***	0.00110	0.00779*
	(0.00152)	(0.00170)	(0.00368)	(0.00420)	(0.00369)	(0.00363)
Intercept	0.00566***	0.00629***	0.0352***	0.0477***	0.0394***	0.0345***
	(0.000666)	(0.000737)	(0.00167)	(0.00195)	(0.00169)	(0.00164)
N	98954	98954	98954	98954	98954	98954
Dep. var. mean	0.00631	0.00834	0.0407	0.0578	0.0411	0.0393
Elasticity	1.824	3.416	0.636	1.361	0.109	0.875
(se)	1.038	1.049	0.406	0.342	0.364	0.408

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The outcome in each column is an indicator for new criminal charges of the type given in the column header in the first three quarters after job separation. Estimates are weighted using a triangular kernel and the same bandwidth as in core estimates.

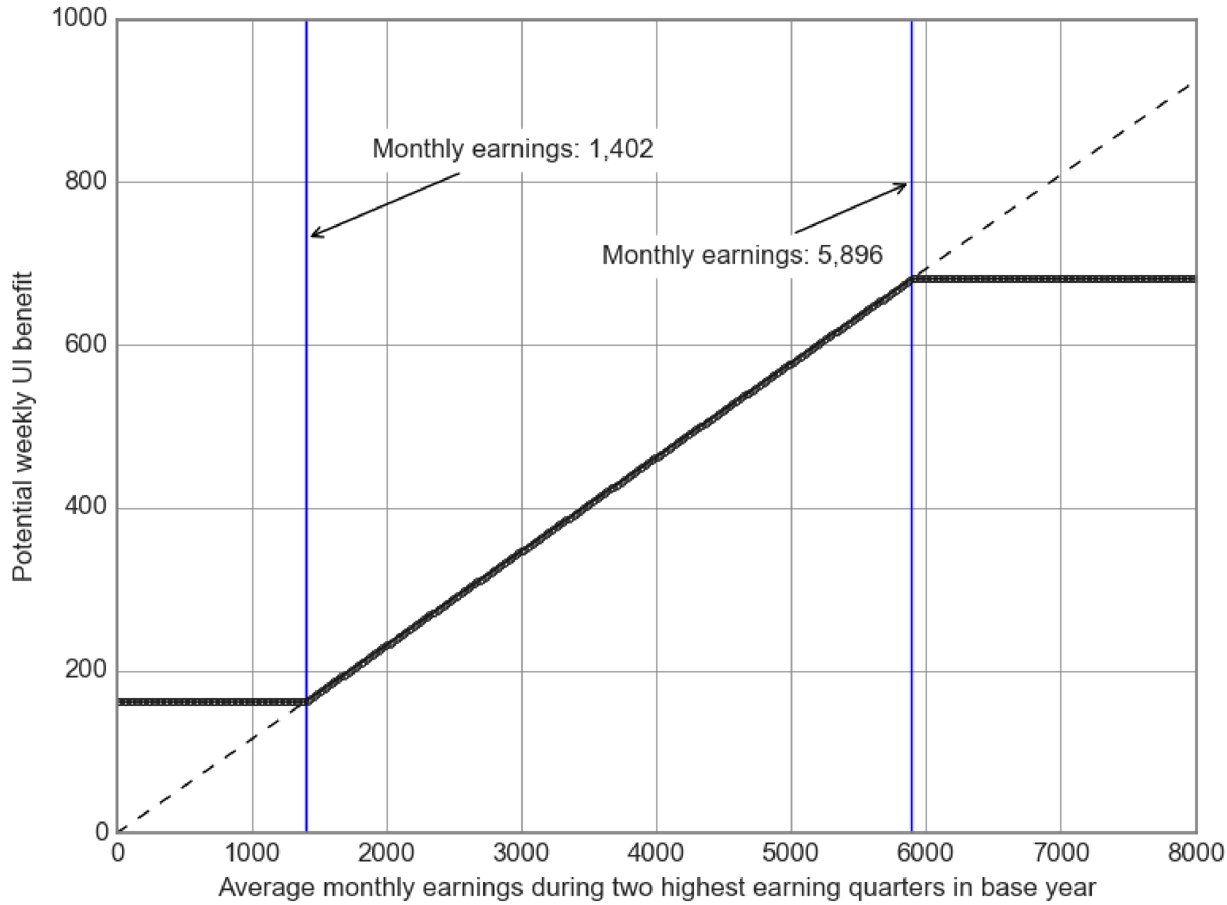
A For online publication: Supplementary figures and tables

Figure 6: Estimates without stayers: Effects on arrests and criminal charges



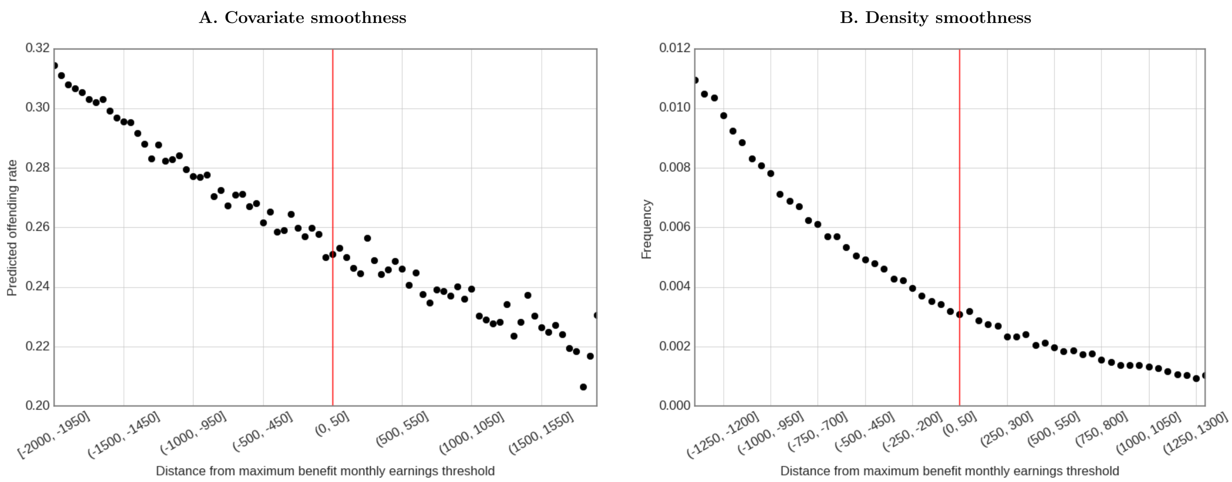
Notes: Sample includes only the separators in Figure 2. Figures plot coefficients on the primary event-time indicators (the β_s), since only treated units are included. Dotted lines represent 95% confidence intervals formed from standard errors clustered at the individual level.

Figure 7: Example Washington State UI schedule



Notes: 2016 maximum, minimums, and benefit rates shown.

Figure 8: RKD validation: Covariates and density



Notes: Panel A plots $50-bin average predicted new charges within three quarters of job loss from an OLS regression that includes year-by-quarter fixed effects, indicators for employment tenure overall and with the pre-job loss employer, indicators for the industry and county of the job, and indicators for race, gender, and birth-year. Panel B reports the sample density. Distance to maximum benefit is calculated using the prevailing maximum benefit amount at time of job loss.

Table 8: Robustness of RKD estimates to alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Slope	-0.0511***	-0.0501***	-0.0501***	-0.0402**	-0.0297	-0.0298
	(0.00336)	(0.00398)	(0.00401)	(0.0141)	(0.0171)	(0.0171)
△ slope right of thresh	0.0319***	0.0333***	0.0333***	0.0110	0.0199	0.0199
	(0.00801)	(0.00851)	(0.00806)	(0.0303)	(0.0311)	(0.0311)
△ intercept right of thresh		-0.00312	-0.00312		-0.0101	-0.0101
		(0.00629)	(0.00603)		(0.00899)	(0.00899)
Slope^2				0.00751	0.0134	0.0133
				(0.00962)	(0.0111)	(0.0111)
△ slope^2 right of thresh				-0.000582	-0.0188	-0.0188
				(0.0154)	(0.0224)	(0.0224)
Intercept	0.215***	0.216***	0.216***	0.218***	0.222***	0.222***
	(0.00299)	(0.00370)	(0.00366)	(0.00442)	(0.00576)	(0.00576)
N	98954	98954	98954	98954	98954	98954
Dep. var. mean	0.247	0.247	0.247	0.247	0.247	0.247
Triangular kernel			Yes			Yes
Elasticity	0.575	0.597	0.597	0.196	0.348	0.349
(se)	0.144	0.153	0.145	0.539	0.544	0.544

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample and specifications as described in text. Distance to maximum benefit is calculated using the prevailing maximum benefit amount at time of job loss. The outcome throughout is any new criminal charges in the first three quarters after job separation. Triangular kernel indicates that observations are weighted by their distance to the threshold.

Table 9: Sensitivity of event studies to fixed effects (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$t = -5$	-0.00596	-0.00609	-0.00606	-0.00597	-0.00597	-0.00622	-0.00597	-0.00578
	(0.0085)	(0.0082)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)
$t = -4$	-0.00677	-0.00683	-0.00681	-0.00659	-0.00659	-0.00783	-0.00659	-0.00727
	(0.0085)	(0.0079)	(0.0074)	(0.0074)	(0.0074)	(0.0074)	(0.0074)	(0.0075)
$t = -2$	0.00401	0.00411	0.00410	0.00384	0.00384	0.00314	0.00384	0.00326
	(0.0085)	(0.0079)	(0.0075)	(0.0075)	(0.0075)	(0.0075)	(0.0075)	(0.0076)
$t = -1$	-0.00622	-0.00611	-0.00609	-0.00603	-0.00603	-0.00610	-0.00603	-0.00593
	(0.0085)	(0.0078)	(0.0072)	(0.0072)	(0.0072)	(0.0073)	(0.0072)	(0.0073)
$t = 0$	0.0216**	0.0218***	0.0218***	0.0217***	0.0217***	0.0211***	0.0217***	0.0211***
	(0.0085)	(0.0080)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0077)
$t = 1$	0.0184**	0.0187**	0.0187**	0.0184**	0.0184**	0.0182**	0.0184**	0.0186**
	(0.0085)	(0.0082)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0078)
$t = 2$	0.00278	0.00313	0.00312	0.00295	0.00295	0.00288	0.00295	0.00272
	(0.0085)	(0.0079)	(0.0075)	(0.0075)	(0.0075)	(0.0076)	(0.0075)	(0.0076)
$t = 3$	0.00625	0.00662	0.00663	0.00643	0.00643	0.00668	0.00643	0.00745
	(0.0085)	(0.0080)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0078)
$t = 4$	0.00426	0.00468	0.00468	0.00455	0.00455	0.00403	0.00455	0.00419
	(0.0085)	(0.0079)	(0.0075)	(0.0075)	(0.0075)	(0.0076)	(0.0075)	(0.0076)
N	663,948	663,948	663,948	663,948	663,948	663,948	663,948	663,948
Age		Yes	Yes					
Indiv			Yes	Yes	Yes	Yes	Yes	Yes
Year-x-qtr				Yes	Yes		Yes	
County					Yes			
County-x-year-x-qtr						Yes		
NAICS2							Yes	
NAICS-x-year-x-qtr								Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table lists estimates of β_s^T from Specification 1, except including the fixed effects listed at bottom of table. The outcome variable is an indicator for any charges. Standard errors are clustered at the individual level.

Table 10: Sensitivity of event studies to fixed effects (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$t = -5$	-0.00733	-0.00761	-0.00789	-0.00615	-0.00606	-0.00659	-0.00597
	(0.0076)	(0.0079)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)
$t = -4$	-0.00789	-0.00759	-0.00806	-0.00685	-0.00648	-0.00717	-0.00659
	(0.0075)	(0.0077)	(0.0074)	(0.0074)	(0.0074)	(0.0075)	(0.0074)
$t = -2$	0.00399	0.00373	0.00210	0.00376	0.00392	0.00418	0.00384
	(0.0076)	(0.0078)	(0.0075)	(0.0075)	(0.0075)	(0.0076)	(0.0075)
$t = -1$	-0.00626	-0.00594	-0.00677	-0.00631	-0.00581	-0.00566	-0.00603
	(0.0073)	(0.0076)	(0.0072)	(0.0073)	(0.0072)	(0.0073)	(0.0072)
$t = 0$	0.0217***	0.0205***	0.0212***	0.0214***	0.0221***	0.0222***	0.0217***
	(0.0076)	(0.0079)	(0.0076)	(0.0076)	(0.0076)	(0.0077)	(0.0076)
$t = 1$	0.0193**	0.0175**	0.0185**	0.0180**	0.0190**	0.0188**	0.0184**
	(0.0078)	(0.0081)	(0.0077)	(0.0078)	(0.0077)	(0.0078)	(0.0077)
$t = 2$	0.00398	0.00410	0.00236	0.00263	0.00366	0.00343	0.00295
	(0.0076)	(0.0078)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0075)
$t = 3$	0.00739	0.00803	0.00636	0.00624	0.00710	0.00704	0.00643
	(0.0078)	(0.0080)	(0.0078)	(0.0077)	(0.0077)	(0.0078)	(0.0077)
$t = 4$	0.00563	0.00275	0.00402	0.00458	0.00503	0.00541	0.00455
	(0.0076)	(0.0078)	(0.0075)	(0.0075)	(0.0075)	(0.0076)	(0.0075)
N	663,948	663,948	663,948	663,948	663,948	663,948	663,948
Age	Yes						
Indiv	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-x-qtr				Yes	Yes	Yes	Yes
County-x-NAICS-x-year	Yes						
County-x-NAICS-x-year-x-qtr		Yes					
Year-x-qtr-age			Yes				
County-x-age				Yes			
NAICS-x-age					Yes		
County-x-NAICS-x-age						Yes	
Firm							Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table lists estimates of β_s^T from Specification 1, except including the fixed effects listed at bottom of table. The outcome variable is an indicator for any charges. Standard errors are clustered at the individual level.