Job Displacement, Poverty, and Crime

John Wieselthier
UC Berkeley

September 2023

Abstract

This paper estimates the effects of reduced hours and job loss on criminal offense using administrative court records matched to a large class of payroll and credit data from 2000-2011 in Texas. I find that crime increases drastically after a negative employment shock, particularly among young men. Effects are exacerbated when looking specifically at economically-motivated crime and are present even in instances of partial separation. These effects are further concentrated among the poor and are sensitive to local economic conditions. Using a model of criminal choice, I provide evidence that motive drives a large share of this increase in criminal activity.

1 Introduction

Around 1 in 3 adults have a criminal record in the United States, the unemployment rate among ex-cons is around 45% in the year following release, and the 1-year recidivism rate is 44%. Though these numbers don't make the mechanisms surrounding the cycle of poverty and crime clear, they should make the phenomenon apparent. And though we know from prior work that incarceration induces future bouts of poverty, the extent to which poverty cycles back to criminal behavior, or even sparks it initially, is less clear (Pager (2003); Holzer, Raphael and Stoll (2006); Agan and Starr (2018)).

In this paper, I match administrative court records in Texas to payroll and credit data in order to determine the extent to which job displacement impacts earnings, poverty, and subsequent criminal behavior. In order to more precisely target exogenous displacements, I utilize large-scale layoff events in firms throughout the period; a natural experiment popularized by Jacobson, LaLonde and Sullivan (1993). I estimate the effects of these displacements on earnings, re-employment, credit, and subsequent criminal offenses. I find that criminal charges increase significantly among young men in the first few months following a layoff, and this increase is more prominent when looking specifically at changes in economically-motivated crimes. While those with priors are significantly more likely to reoffend than their law-abiding counterparts are to initially offend, there is still a significantly positive effect among those with no priors. Further, looking at partial separations, I find that the crime response is directly proportional to the share of a full-time job loss. Credit conditions, as well as prior income and local economic conditions, also significantly impact the magnitude of the effects that I find. Following this, I propose a model that allows us to disentangle "motive" from "opportunity;" an important distinction to make for policy makers. My results provide strong evidence that the "motive" margin is dominating in many situations, and suggest that poverty-alleviating measures would drastically abate criminal behavior following job displacements or other negative earnings shocks.

Even before Dostoevsky (1866), poverty and crime have been thought to circle one another near-endlessly. In more recent history, Economists became interested in the root cause, or motive surrounding crime. Becker (1968) and Ehrlich (1973) both sought to model criminal behavior as the natural result of individuals making rational choices between legal and illegal activities. In this work, and especially in Ehrlich's, a number of predictions were made with one central theme relevant to this work: if the relative value of crime increases, then so should the incentive for that individual to commit crime. An abundance of subsequent research aimed to expand upon and verify predictions

that arose from the Becker-Ehrlich framework. Witte (1980), Myers Jr (1983), and Andreoni (1991) are a few of the many papers that expand upon this work and, generally, focus on what happens when the cost of crime increases through deterrence measures. While these works find null effects of deterrence, Levitt (1995) uses election cycles as an instrument to show that police do reduce crime. Grogger (1998), Glaeser and Sacerdote (1999) and Gould, Weinberg and Mustard (2002), look more closely at the effect of local labor market conditions to show that wages are a significant predictor of crime (specifically property crime, as theory would predict).

More recently, Kling (2006) popularized evaluator instruments as a means of properly identifying a number of effects in instances where an evaluator (or judge) is randomly assigned. From experiments of this kind, we now know, for example, that juvenile incarceration decreases the likelihood of high school graduation and that spills over into adult incarceration probability (Aizer and Doyle Jr (2015)); pre-trial detention has a strong positive effect on probability of conviction and a negative effect on formal sector employment (Dobbie, Goldin and Yang (2018); and incarceration, on the extensive margin, reduces probability of reoffense and (Bhuller et al. (2020)). In other experiments, it has been shown that incarceration also reduces probability of reoffense on the intensive margin, with 1 additional year of incarceration reducing the 5-year reoffense rate by around 30% (Rose and Shem-Tov (2021)). Norris, Pecenco and Weaver (2021) find a similar reduction in long-term reoffense rates following incarceration sentences. In contrast, both Mueller-Smith (2015) and Franco Buitrago et al. (2022) find that incarceration increases the probability of reoffense; a conflicting finding that one may just chalk up to differences in institutional settings.

On the labor-side, research has shown that disemployment has a number of detrimental effects. Jacobson, LaLonde and Sullivan (1993) find that earnings recover relatively quickly following a separation, but that they stabilize at around 25% below their initial earnings level. This stable, lower-level seems to persist for as long as 15-20 years, which represents a significant reduction in a worker's lifetime wages (Von Wachter, Song and Manchester (2009), Lachowska, Mas and Woodbury (2020)). In addition to the long-run earnings loss, these kinds of displacements also significantly effect health and mortality rates (Sullivan and Von Wachter (2009)). The State can, of course, insure individuals against these kinds of shocks through programs like unemployment insurance. Unemployment benefits extensions, however, have been shown to have unclear effects: they, unsurprisingly, dampen the impetus to seek out work (and thus increase unemployment duration) but there is evidence that they also don't clearly increase re-employment wages (Schmieder, von Wachter and Bender (2016), Nekoei

and Weber (2017)). Deshpande and Mueller-Smith (2022) finds less ambiguous support in favor of the effect of benefits in reducing crime.

Research at the intersection of crime and labor is sparse primarily due to data reasons. Raphael and Winter-Ebmer (2001) an early attempt at this question and finds a positive effect of unemployment on the rate of criminal activity.[1] A few papers have also studied experiments similar to the one here in recent years. Using annual data in Norway, Rege et al. (2019) finds that crime increases by 15%-20% in the year following displacement, with most of the effect coming from property crimes. Bennett and Ouazad (2020) use similar data in Denmark and find an even larger effect among young men: an increase in crime rates of around 32% in the year following displacement, almost entirely driven by property crimes. Looking to a higher-crime setting, Khanna et al. (2021) find that crime increases by almost 50% among displaced workers in the year of the layoff event. Rose (2018) estimates a similar effect of around 30%-50% among prior offenders in Washington. My paper expands on these studies in a number of ways. First, the payroll data allows me to more precisely identify layoff events and better understand the timing of the displacement for the treated individuals. This distinction is important, as I show later in this paper that crime responses are strongest in the first few months following a layoff. The payroll data also allows me to identify partial separation events. In these events, I show that individuals exhibit responses proportional to their total hours (or earnings loss). Second, the data used here allows me to measure the effects of job loss on those with no priors. While prior offenders are at a greater risk, I show that even those with no criminal history prior to a layoff exhibit a strong response when displaced. Third, the coupled credit data allows me to speak more completely to the detrimental effects of job loss and more precisely identify the at-risk group. Finally, while other papers have used changes in benefits schedules to separately identify the "motive" and "opportunity" margins, the scope of my data allows me to take a slightly different approach in pinpointing the significance of these two margins in Appendix D.

The remainder of this paper proceeds as follows. Section 2 describes the setting and data utilized. Section 3 describes the empirical strategy for identifying causal effects and reports the majority of the results from the analysis. Section 4 lays out the model utilized to separately identify the two margins of interest: "motive" and "opportunity." Section 5 discusses the importance of a number of the parameters recovered and explores the extent to which some of the nuances in the experiment influence my estimates. Section 6 concludes.

---

[1]Also see Lin (2008) for a subsequent paper, which finds that a 1pp increase in unemployment increases property crime by around 4%.

2   Setting and Data

Broadly, I combine three pieces of data from Texas: administrative criminal court records from the Texas Department of Public Safety, payroll data, and corresponding credit information. Below, I briefly describe how the criminal courts operate in Texas, each piece of the data, as well as the matching procedure. A more complete description of the data and match is included in Appendix A.

2.1   Texas Criminal Court System

In Texas, criminal court cases are handled in both District and County Courts. District courts handle all felony cases and there are more than 450 of these courts across the state. Each District Court has one judge assigned to it. County Courts, on the other hand, serve to handle misdemeanor cases and there are more than 500 of County Courts across the state. At least one elected chief prosecutor serves in each county and these prosecutors can serve in multiple counties. In both types of Courts, judges and prosecutors are elected democratically and serve for 4 years per term.

Information on criminal court cases is provided by the Texas Department of Public Safety (DPS) and covers 2000-2011. The data is maintained by the Texas Department of Criminal History, and includes data from arresting agencies and prosecutors, in addition to courts. When compared to other states, the data contains less information on the crimiminal court process, however. Still, on the court side, this data does include information on defendants, offenses, dispositions, and sentences for the majority cases disposed in the criminal court system in Texas during this period, though. I say "majority" since the data only include court dispositions for offenses that are reported by an arresting agency. I drop arrests that are not matched to a court case following Feigenberg and Miller (2021).[2] Finally, there is sufficient identifying information in this data for a strong match to the subsequent data payroll and credit described below. More precise restrictions made on the defendants, minimal available court information, etc. are discussed in Appendix A.

2.2   Payroll and Credit Data

Payroll data is sourced from 4 large payroll management firms in Texas from 2000-2011. This does *not* include all payroll-covered employees from this period.[3] This data includes monthly information

---

[2]Around 88% of arrests during the period I'm studying have matched dispositions and court information.

[3]This includes around 3.9 million unique employees. This is ∼40% of all employees in Texas during this period. If one wants to be conservative, job separations can be thought of as just that: separations. I will try to infer some things about unemployment durations, however, using measures of market tightness later in the paper.

on wages, hours, and some other basic characteristics of the job and firm that the individual belongs to. Tenure requirements will be made in identifying layoffs in section 3 and are lower-bounds on an individual's true tenure duation at a firm, since it is constructed from this data. To be more precise, the data starts in January 2000, but the first layoff event that I will allow will be in July 2000, since one of the basic restrictions will be 6 months of tenure for employees included in either the separation or control group.[4]

Credit data is matched to the same set of employees that appear in the payroll data described above and spans the same period (2000-2011). This includes information on adverse financial events, credit card delinquency, credit due to collection, etc. I *do* see credit information for people outside of the aforementioned payroll-covered jobs, as long as they reside in Texas and have some established credit. This will allow me to somewhat speak to selection into payroll-covered jobs. Inference on job loss (or attainment) is not made on those outside of the payroll dataset, however. The credit panel and payroll data are matched separately (not as part of an exercise here) and have close to a 100% match rate. That is, virtually all those that appear in the payroll data have matched credit information. The few that do not appear in the credit data presumably have no established credit and are excluded from the entirety of the analysis so that the composition of individuals remains consistent.

2.3   Matching and Coverage

Identifying information from the DPS was provided to the management firm, in order to match to payroll and credit data. The most important covariates for matching were name, date of birth, and address history. The final match was made using a standard distance criterion with a few simple simple conditions specified in cases of multiple matches or no/weak matches. A more precise overview of the matching procedure is outlined in Appendix A.

As mentioned in subsection 2.2, the payroll data covers a large subset of payroll-covered jobs in Texas. Using the American Community Survey in 2000 and 2010, as well as the Economic Census as benchmarks, I estimate that between 37% and 42% of all employees in Texas over this period. When restricted to private-sector employment, this represents closer to 50% of all employees in Texas. When compared to estimates from Orbis, I see slightly fewer of the total firms that operate in Texas, which makes sense given that employers reporting payroll data here are mostly larger companies, with closer

---

[4]I mention this since tenure duration is a common covariate in a test of randomization, but only becomes reasonably "precise" after a few years; say, around 2004 or 2005.

to 90% of Fortune 500 companies included in the payroll data.[5] The most important thing to note, as a result, is that non-presence in the payroll data does not necessarily imply unemployment. The most conservative interpretation of the treatment implied by a layoff event later in this paper should be just that: a layoff.

3 Estimation and Results

In the standard Becker-Ehrlich framework, job loss can be thought of as a negative shock to the value of an individual's legal job market participation. Empirically, this can operate through a number of channels: a reduction in legal income, unemployment scarring or skill depreciation over the unemployment term, etc. (see Von Wachter, Song and Manchester (2009) for an overview). The increase in free time alone can also act as a mechanism leading to an increase in crime.[6] And while there are a number of ways to think about job loss, a common experiment follows Jacobson, LaLonde and Sullivan (1993), in which layoff events are used as a negative employment shock to a set of individuals.[7] Standard OLS estimation might proceed by:

$$y_{it} = \beta_t D_{it} + X_i' \theta_t + \eta_{it} \tag{1}$$

where $y_{it} \coloneqq \mathbb{1}\{i \text{ offends in period } t\}$, $D_{it} \coloneqq \mathbb{1}\{i \text{ employed in period } t\}$, and β_t is our parameter of interest. However, there likely exists selection bias in the OLS estimation of β_t, in that employers are less likely to hire, or more likely to fire, "riskier" individuals. Layoff events will serve to circumvent this selection problem.

Following Jacobson, LaLonde and Sullivan (1993) and a number of follow-up studies, I include various restrictions on individuals, firms, and events in order to assuage concerns surrounding non-randomness of the separations. I restrict the sample to those with at least 6 months of tenure prior to a separation event. I restrict firms to those that have at least 25 empoyees at time of separation.

---

[5]Each census reveals between 9.2 and 9.7 million full-time employees, while I see around 3.9 million people in the payroll data at some point between 2000 and 2011. "Orbis" references a dataset on general firm activity maintained by Bureau van Dijk. General numbers and proportions to all employees are are very close to those reported by Mello (2018) who uses similar data in Florida.

[6]The importance in distinguishing these mechanisms is further explained and explored more completely in Appendix D.

[7]Estimation in an experiment like this can proceed in (at least) 4 ways: comparing a separator's outcome to themselves post-event, comparing separators to stayers in the same firm, comparing separators to stayers across the sample, and comparing separators to matched stayers. The primary estimating equation here is comparing separators to stayers that were displaced at the same time, but I show analogous results for the other estimation variations in Appendix F.

Separation events are then defined as 20-80% of employees that meet the tenure threshold being separated in a given month, subject to firm employment remaining below pre-separation levels for one year and firm employment remaining stable (<25% variation in either direction) in the 6 months preceding an event.[8] Our standard estimating equation can then be written as:

$$y_{it} = \alpha_i + \gamma_t + X'_{it}\beta + \sum_{k \in K} D_{it}^k \cdot \delta_k + \varepsilon_{it} \qquad (2)$$

where $y_{it} := \mathbb{1}\{i \text{ offends in period } t\}$ and $D_{it}^k := \mathbb{1}\{i \text{ displaced } k \text{ periods before } t\}$. Under this specification, δ_k will compare differences in offense probabilities for workers whose displacement status changes to changes for workers who were not displaced at the same time. As discussed in subsection 2.1, the DPS data includes both arrest information and court information by construction. The timing of criminal "offense" will be defined as the arrest date, but take the charge date if the arrest date is missing.[9] If an individual is not convicted of the crime they are charged with, they will not be considered to have offended. Similarly, if an individual pleads to a lesser charge, the lesser charge will be coded as the crime they initially committed. Finally, I use the estimation procedure developed by Borusyak, Jaravel and Spiess (2021), in order to account for the staggered treatment timing and heterogeneous treatment effects present in this setting.

$$y_{it} = \theta_{j(i)} + \gamma_t + X'_{it}\beta + \sum_j \sum_t D_{it}\lambda'_{j(i)t}\delta_{j(i)t} + \varepsilon_{it} \qquad (3)$$

This is a two-way fixed effect regression of the outcome (crime by or at time t) on firm (j) and time (t) fixed effects, a set of controls (X), and fixed effects for every treated ($j(i), t$) cell. Under parallel trends, the coefficients on the fixed effect for our treated cells ($\delta_{j(i),t}$) are the treatment effects on displaced workers from firm j at time t. $\bar{delta}_{l,l+k}$ is computed at each horizon k as the (weighted) average of all of the $\delta_{j(i),t}$ such that individuals were displaced from firm j at period l and $t = l + k$. $\bar{\delta}_{l,l+k}$ will be referred to as δ_k throughout the text and should be interpreted as the average treatment effect of the displacement k periods out from separation. Throughout the analysis, standard errors are clustered at the firm-level.[10]

---

[8]I flex a number of these restrictions in Appendix F. Further discussion of these restrictions and choice relative to other papers is given in Appendix A.

[9]I am only missing arrest date for around 8% of the sample. In addition, in cases where arrest date and charge date both exist, the difference in timing is only 6 days on average. Because of this, I don't see a strong reason to exclude observations where arrest date is missing.

[10]There is a slight distinction between firm clustering, individual clustering, and firm-by-individual clustering given the structure of the panel and the estimation dictated by both Equation 2 and Equation 3. This is made more clear

I begin by showing results for the full sample, but quickly restrict the sample of separators (and stayers) to those that tend to be most susceptible to the draw of crime as an alternative: young men. I refer to these as "high-risk" stayers and separators, and these are identified simply as males aged 20-35, inclusive.[11] Table 1 displays summary statistics for my full sample, separators, stayers, young male separators, and young male stayers. There are 3,356,805 individuals in my payroll data for more than 1 month, which is the minimum necessary condition to be included. Among ∼ 6300 separation events identified at the month-level, there are roughly 240,000 separators and 617,000 stayers (within firm). When restricted to young men, the relative proportion of separators-to-stayers remains about the same. Those in my sample seem to reflect the general workforce in Texas, though monthly income is slightly higher, which might reflect the quality of jobs in my sample. The only other things to note are that the tenure restriction on the events does not restrict the sample by much, as one might be able to infer from the statistics shown, and that young men are drastically more likely to have had prior interactions wiith the criminal court system in Texas when compared to the rest of the sample.

3.1 The Effect of Separations on Crime

Table 2 shows the main JLS results following Equation 2, adjusting coefficients and standard errors so that they reflect annual changes in outcomes.[12] Looking at Panel A, pre-trends are fairly well-behaved across all outcomes shown. There is a slight downward trend in earnings and hours leading up to the event. Ashenfelter (1978) originally noted this dip and it's been replicated in numerous quasi-experiments similar to this one (Jacobson, LaLonde and Sullivan (1993) and Von Wachter, Song and Manchester (2009) for examples). Immediately following a separation event, employment, earnings, and hours drop substantially and remain at depressed levels across the horizon.[13] The probability of of committing a misdemeanor increases by 1.1pp in the month of separation, 2.4pp in the second month, and 2.8pp in the third relative to a baseline of around 1.7%. Similarly, the probability of committing a felony increases by .9pp in the month of separation, 2pp in the second month, and 2.3pp in the third relative to a baseline of around 1.4pp. Economic crimes follow a similar pattern

in Appendix A.

[11]The general results are simply attenuated versions of the results for young men, in most cases. I show the analogous results in Appendix F. Figure C.3 also shows the evolution of the crime estimates across the age profile, for both men and women.

[12]As with other places in the paper, this is simply multiplying the coefficients and standard errors by 12 so that they can be more easily compared to the baseline year-over-year changes.

[13]It is important to note that re-employment and changes in earnings/hours aren't precisely measured, since I only have payroll-covered employees in Texas in the sample. Negative changes across these three outcomes are almost universally over-estimated, in that separators could find re-employment in non-payroll-covered jobs or in other states; and that outcome would not be observed. Regardless, these three outcomes should be thought of as a first-stage of sorts.

across the horizon.[14] Panel B of Table 2 shows analogous results for our "target" demographic: men ages 20-35, inclusive. Compared to panel A, all crime outcomes are exacerbated, though the general trend is retained, in that criminal activity increases immediately, continues to increase during the first few months, and then (slightly) abates.

There are a few things to note regarding Table 2 and the way in which Equation 2 is specified before continuing on, since most results will also be subject to the same subtleties. First, $y_{it} \coloneqq \mathbb{1}\{i \text{ offends in period } t\}$, so coefficients here are *not* the cumulative probability of having committed a crime by t months out. In addition, the cumulative probability is not as simple as summing up the coefficients through t, since individuals can reoffend within the same time frame. Throughout the paper, when I refer to the cumulative effect, I am instead using $Y_{it} \coloneqq \mathbb{1}\{i \text{ offends } \mathbf{by} \text{ period } t\}$. Another important thing to note is that individuals are frequently charged with multiple different crimes at the same time. For instance, one might commit robbery and be subject to a misdemeanor charge and multiple felonies. In that case, I treat the individual as having committed both a felony and a misdemeanor in that month. In this way, distinguishing between changes in the severity of crimes following displacement events is not as easy as just looking at the change in misdemeanors vs the change in felonies, for instance. Types of crimes, such as economic and violent crimes, interact the same way; a crime can be both economically-motivated and violent in nature, though most charges are categorized as one or the other.

As noted in section 3, most of the results that follow will focus on men ages 20-35.[15] It's evident from most of the crime literature that one shouldn't expect strong responses from most other demographic groups, which I show directly in Table 3. While the overall effect on crime is around 2.8pp 1-year after a layoff, the majority of this is driven by men aged 20-35. The effect for this group is 5.6pp, which represents a near doubling of criminal activity in the year following displacement. This trend continues slightly into the second year, where they exhibit a 7.1pp increase (a $\sim$60% relative increase). While the effect is still present for older men, it dissipates quickly as we move along the age profile (see Figure C.3). And while relative effects are fairly large for young women (1.4pp after 1 year; also roughly a doubling of baseline rates), the estimates are quite noisy. Also consistent with prior literature (e.g. Von Wachter, Song and Manchester (2009)), earnings are significantly depressed even among those that reattach to firms within my sample. This $\sim 13\% - 15\%$ relative decrease in

---

[14]Regardless of the type of crime one is looking at here, rates remain significantly elevated for over a year.

[15]I show analogous results for these other group in the Appendix C, as well as some evidence that effects drop drastically across the age profile.

earnings 1-2 years out from a layoff should be likely taken as a lower bound on the true change in earnings, since some subset are likely still unemployed at these horizons.

Continuing on, one of the primary questions that arise is the nature of the type of crime that job displacement might induce. One broad way to categorize crimes is by identifying violent crimes and economically-motivated crimes. Violent crimes are those that involve force or threat of force, while economic crimes should be thought of as property crimes+, where the "+" includes things like bribery, fraud, illicit sales, etc.[16] Figure 1 shows how separation events affect both of these outcomes among young men. Perhaps unsurprisingly, both increase drastically following separation. Economically-motivated crimes increase by around 6pp at their peak from a baseline of around 3pp, while violent crimes increase by around 3pp at their peak from a baseline of around 2pp. Those effects, in and of themselves, seem very large and I'll more closely explore what is driving the drastic increase in both types of crimes in subsection 3.4 and Appendix D.

3.2   Prior Offenders

Predictably, those with priors are more likely than their law-abiding counterparts to offend following a separation event. Perhaps surprisingly, however, is the fact that the relative change in risk is roughly the same for both groups. Panel A of Figure 2 shows results where the control group for separators with priors and those without is all (young male) stayers, following my standard estimation procedure. Panel B restricts the control group to those with priors when comparing changes for separators with priors, and to those without priors when comparing changes for separators without priors. In both specifications, those with priors are substantially more likely than those without to offend following a separation. At the peak, those with priors are between 11pp and 14pp more likely to commit an offense, while those without are between 5pp and 7.5pp more likely. The cumulative 1-year effects for those with priors are between 6pp and 8pp, while those without are between 3pp and 4pp. Formulated as a triple-differences, those with priors are between 3pp and 6pp more likely than their no-prior counterparts to commit an offense in the year following a separation event. By triple-differences, I mean to modify our standard estimation by looking at the difference between the difference between separators and stayers, and those with priors and no priors, before and after a separation event at a

---

[16]I broadly follow the FBI's Uniform Crime Reporting (UCR) in categorizing crimes across these lines. It's important to note that economically-motivated crimes can be violent crimes and vice versa; they are not separately identified. In addition, the combination of economically-motivated crimes and violent crimes does not constitute the full breadth of crime types. See subsection A.2 for more detail.

firm. This can be written simply as:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \in K} D_{it}^k \delta_k + \Gamma \mathbb{1}\{\text{Prior}_{it_0}\} + \mathbb{1}\{\text{Prior}_{it_0}\} \sum_{k \in K} D_{it}^k \Delta_k + \epsilon_{it}$$

where $\mathbb{1}\{\text{Prior}_{it_0}\}$ is an indicator for whether or not individual i has a prior at the time of the separation event. These changes represent a roughly 60%-70% increase in risk in the year following separation for both groups. In comparison to findings in prior literature, this is fairly high for both those with priors (Rose (2018) estimates 30%-50% in Washington) and those without priors (Bennett and Ouazad (2020) estimates 30% in Denmark). The slight difference in effects when compared to these other settings could be driven by differences in benefits schedules, local labor market conditions, prosecutorial and policing discretion, etc.[17]

3.3 Partial Separations

In thinking about the way that layoff events operate when compared to my central question, it becomes clear that some things are being obfuscated. In particular, while displacements themselves are of interest, they are a rather extreme event relative to what we think the actual treatment might be: loss of income, or an increase in free time. As such, looking at partial separations might make clear whether job loss itself is of import, or if it is some combination of these other variables. There are a few ways to characterize a partial separation, but the simplest that I propose is as follows: take Equation 2 and redefine an event as a cut in 10+ hours in a given month, restricted to firms with ≥20% of employees receiving this same cut in a given month but who *don't* meet full separation restrictions.[18] In this way, I will pool all hours cut combinations that meet these restrictions. It's important to note that most, but not all accounts have hours recorded; and so the sample here is slightly smaller and the composition changes when compared to Table 1.

The average number of hours cut among those who meet these restrictions is around 20 (see Figure F.4). Figure 3 is the partial separation analogue to Figure 1 and looks like a generally attenuated version of it. The cumulative 1-year effect on economically-motivated crimes is around 1.7pp, while it was around 4.2pp for our standard layoff events. Similarly, the 1-year effect on violent

---

[17]Given that the difference in the coefficients shrinks when I make the control group more precise, it's likely the case that those with priors are necessarily more likely to reoffend, even absent a separation under my standard specification. I attempt to assuage concerns surrounding non-randomness of layoff decisions in and more closely examine the parameter that is recovered in Appendix E.

[18]So, firms that have actual separation events will not meet the criteria for a partial separation event, even if they are cutting hours among some subset of their employees at the same time that they are laying off others, for instance.

crimes is around 1.1pp, while it was around 2.2pp for our standard layoff events. Given that the treatment is around half the magnitude of a standard layoff (in terms of hours and earnings lost), this lends itself to the hypothesis that the crime response is proportional to the magnitude of the employment/earnings shock. Of course, this was a rather coarse way of examining partial separations, so I next look at the intensive margin a bit more carefully.

I modify Equation 2 to allow the parameter to be interpreted as the per-hour loss (per week) effect on crime (or any outcome). This can be done simply as:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \in K}[H_{i0|H \geq 10} \cdot D_{it}^k \cdot \delta_k] + \eta_{it} \tag{4}$$

where $H_{i0} \in [10, 40]$ is the number of hours cut per week in a partial separation event and with the same restrictions on partial separations as noted above. Figure 4 shows the effect of 1 hour lost on economic and violent criminal charges separately. Similarly to Figure 3, we can see that both economic crime and violent crime increase drastically in the months following a partial separation event. The cumulative 1-year effect on economically-motivated crimes is around .11pp per hour lost, while the cumulative 1-year effect on violent crimes is around .06pp per hour lost. Straightforward projection of these estimates again reveals that the crime response appears to be directly proportional to the share of a full-time (40-hour/week) job loss. Though it's noisy, I also estimate the 1-year cumulative effect at each hour-lost threshold between 10 and 40 to show this relationship more clearly. This is estimated by:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{h \in H}[\sum_{k \in K} \cdot D_{it}^k \cdot \delta_{h,k}] + u_{it} \tag{5}$$

where $H = \{10, 11, \ldots, 39, 40\}$ and (partial separation) events are defined the same way as above. The outcome in this case (Y_{it}) is the probability of having ever committed a crime by time t. In this way, each $\delta_{h,k}$ is the cumulative JLS estimate k periods after displacement for those who lost h hours during the partial separation event. Figure 5 plots each of these 31 coefficients for $k = 12$. The results of this estimation procedure reinforce the idea that the effect is proportional to the magnitude of the (relative) treatment. Though not shown, this pattern of proportionality broadly holds for different horizons (i.e. at $k \in \{3, 6, 18, 24\}$).

3.4 Liquidity-Constraint and Motive

In the standard Becker-Ehrlich framework, an individual weighs the costs and benefits of committing crime relative to their outside option (participation in the formal labor market). While I won't speak to the costs in this work, the relative benefit should change as an individual gets closer to some minimum level of consumption that the state does not provide.[19] Though the credit data does not provide a perfect estimate of liquidity, or need, tehre are a number of proxies that can be used to estimate an individual's proxity to this threshold.

Following a separation event, both collections balances and instances of major derogatory events increase significantly. Figure 6 shows the evolution of these two variables.[20] Collections balances are around $1060 higher a year out from a layoff evet when compared to stayers within the same firm. Similarly, cumulative instances of derogatory events are around 6.5% higher after 1 year.

As shown in Figure 7, those with low income and those with large collections balances in the pre-period exhibit stronger criminal responses than their counterparts. Looking to the first of the two figures, "bottom 25% income" is defined by the average of an individual's income in the 6 months prior to a layoff event. Those that match this definition exhibit a roughly 7.3% 1-year cumulative increase in charges, while their counterpart exhibits an effect closer to 5%. Similar to the caveat in Figure 2, there is a slight difference in baseline criminal behavior between the two groups, with those falling in the bottom 25% being around 20% more likely to commit a crime than their counterpart. As such, the actual difference-in-differences in criminal behavior following a layoff between the bottom 25% and top 75% (when defined by pre-period income) is only around 20%. The second of the two figures instead splits the separators by outstanding collections accounts 2 periods prior to the layoff event (i.e. period $t = -2$). Here, the difference between the two groups appears to be much more stark. Those with high collections balances (i.e. poor credit) in the pre-period are exhibit closer to a 10% increase in 1-year cumulative charges. While this group is similarly negatively selected, there is a difference-in-differences in 1-year criminal offense following a layoff of closer to 50%. Also interesting

---

[19]There are a few ways for the cost of crime to change, but it can mostly be manipulated by either increasing the probability of detection or the cost of detection. See Levitt (1995), Di Tella and Schargrodsky (2004), and related literature for the effect of police presence. Barbarino and Mastrobuoni (2014), Bhuller et al. (2020), and Rose and Shem-Tov (2021) are recent papers that look at the effect of incarceration itself. For other margins, such as fines, see Polinsky (2006) for theoretical work and Morrison and Wieselthier (2022) for estimates. Chalfin and McCrary (2017) also provides an extensive review of the recent literature.

[20]It's important to note that both variables are slightly staggered due to the reporting nature of the credit data. For instance, an account that is reported to collections won't be reflected in an individual's collections balance for 1-2 months, in expectation. Since most of the action is seen after a few months, I've expanded the horizon to 12 months for clarity.

to note is the fact that pointwise crime rates don't seem to abate in the same way as they do when looking at other specifications. That is, those with poor credit in the pre-period continue to exhibit drastically higher criminal offense rates for a longer period of time, which is consistent with the idea of high collections balances being a good proxy for "need" in the Becker-Ehrlich framework.

3.5 The Complementary Effects of Local Conditions

Before turning to my model, there is one more set of reduced-form estimates that will be of use: the effect of local economic conditions.[21] Here, I think about local conditions as impacting individuals in three ways. First, an individual can live in an impoverished area, or an area that might more heavily induce them into comitting crime following a separation. Second, an individual can be separated at a time when general labor market conditions are worse (i.e. an economic downturn). Finally, an individual can be subject to a downturn that disproportionately affects their local labor market.

The first panel of Table 4 shows cumulative 6-month and 1-year JLS effects by firm location. In particular, I split the sample by income per capita and employment rates, respectively, in the county where the firm is located.[22] Increases in crime rates following separation are substantially higher in poorer counties regardless of how it is defined. The differences in effects that I see here persist even if I consider the fact that dependent means are slightly higher in these poorer counties (.0047 pre-event within my sample). Controlling for these differences in pre-period criminal behavior directly still leaves a more than 50% difference in 1-year cumulative JLS effects for those working at firms located in poor counties when compared to their counterpart.[23]

Panel B of the table shows these effects relative to the timing of the 2008 recession. "Pre-recession" is defined as layoff events occuring before December 2007, while "Recession" spans December 2007 through June 2009, and "Post-Recession" spans June 2009 through the end of my sample.[24] For

---

[21]The social planner's optimal response will be a function of local conditions, since they will be shown to also influence the relative benefit of crime. As mentioned elsewhere in the paper, I will not be able to speak to the cost of crime shifting; only the benefit in relation to it.

[22]The majority (91%) of workers live in the same county as the firm location, as recorded in the payroll data. Results are similar if residential county is used instead.

[23]I control for these by adjusting the control sample in the specification. Equation 2 compares all individuals displaced at time time t to those who were not displaced at the same time. Instead, I can compare individuals displaced within poor counties to those not displaced within poor counties by restricting the control sample, which is what I reference here. 1-year cumulative JLS coefficients are .083 and .052 for those working in poor counties and not poor counties, respectively, when this adjustment is made. Similarly, coefficients are .086 and .050 if I look at low-employment and high-employment counties with the same adjustment.

[24]Note that cumulative JLS effects are unable to be calculated as I approach the end of my sample. In particular, this stymies my ability to estimate the effects in the post-recession period, as a number of layoff events (that I do observe) are not observed early enough to measure proper outcomes.

reference, Figure F.5 shows how separations evolve over time and should make it fairly clear that more layoffs occured during the recession than at other times (even in Texas, which was not particularly hard hit by the recession). Looking back to Table 4, we see that 1-year cumulative effects on crime are also around 50% higher during the recession when compared to the pre-period. While this appears to remain elevated in the post-recession period, estimates are noisy due to censoring of the outcome of interest.

Panel C shows the cumulative JLS effects split by local impact of the recession on a commuting zone (CZ). Estimates of local impact by CZ are taken from Yagan (2019). Unfortunately (or not), Texas was not particularly hard hit by the recession when compared to other states, so there isn't that much variation in this measure within Texas; and, while I do see a slight difference in impacted areas, these effects are noisy. I use a time-variant measure similar to this in subsection 4.1 to instrument for unemployment duration. See results there for an alternative (slightly different) estimate of the impact of local conditions on crime.

3.6   Employment Scarring and Reoffense

One concern with displacement is that there is long-term employment scarring which spills over into lifetime earnings. Jacobson, LaLonde and Sullivan (1993) and Von Wachter, Song and Manchester (2009) find that earnings are still around 20% lower after 15-20 years, while I found that re-employment earnings are around 15% lower after 2 years for young men (see Table 3 and Figure F.11). Alluding to the set up in Appendix D, earnings (or lack thereof) should be one of the primary channels through which offense is induced. As such, we should expect to see that crime does not fully abate upon reattachment due to depressed wages.

As a simple approach, I compare changes in an individual's relative earnings and criminal behavior following reattachment to the labor market. Figure 8 estimates event studies of the form:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{j \neq -7} \beta_j \mathbb{1}\{Reattachment_{it} = j\} + \varepsilon_{it} \tag{6}$$

among individuals who experienced (full) separation events and then reattached to the labor market within at most 6 months and who experience at least 1 month of unemployment (zero earnings). As such, the pre-period estimates for criminal charges (periods -6 through -1) are the average differences in charges relative to period -7 (when their prior employment is guaranteed); not necessarily the

16

average difference in charges following a separation.[25] The post-period estimates are the average change in criminal outcomes and relative earnings when compared to period -7 as the anchor; again, because that is the most recent period for which every individual that was separated and reattached is guaranteed to have been previously fully employed. For both outcomes, periods are binned at periods -13 and 13. Similar to the findings in Table 3, those who resecure employment find that their earnings are around 15% to 20% lower than they were prior to separation. And while criminal activity seems to abate, it does not quite reach pre-period levels, with an average increase of around 1pp within one year of reattachment (a relative increase of around 20%).

4   Discussion

I aim to tackle two separate issues in this section. First, I recenter the discussion of unemployment vs. displacement in this setting; a distinction that has maybe been obfuscated in this paper. Following that, I consider the social planner's problem from a cost-benefit perspective. That is, what is the long-run cost of exogenous separation to the state?[26]

4.1   Unemployment or Displacement?

Due to the limitations of the payroll data, I can only measure displacement precisely throughout the paper. Throughout section 3 I was careful to reference the treatment as being just that, but that leaves the question of unemployment duration largely open. Despite only having around 40% of the available employment data, I estimate labor market tightness here for each individual at the time of their layoff and use that as an instrument for the individual's re-employment. In this way, I aim to estimate the effect of unemployment duration on criminal behavior.[27]

First, to the point of market tightness, I estimate a leave-out measure for market tightness for each individual at the time of their layoff event. Each individual's commuting zone is identified by *their* residence (not the firm or set of firms). I could estimate this using more precise geographic

---

[25]For instance, an individual that is laid off and then finds employment again 3 periods later would be fully employed in periods {-4,-5,-6,-7,. . . }.

[26]I look more closely at the natural experiment I'm utilizing in Appendix E. In particular, I compare the main results to outcomes where firms are instead closed and consider whether layoffs are truly recovering something resembling the local average treatment effect (LATE), as well as some of the limitations in using closures instead.

[27]I estimate something similar in subsection 3.5 with a few disctinctions. More importantly, there I am only considering the effect that the local (spatial or temporal) labor market might have on the JLS estimates that I estimate. Here, I am instead first considering the effect that local conditions have on an individual's own unemployment duration and using that to instrument for the effect of unemployment (rather than displacement) on crime. Figure 8 also shows the basic changes in criminal behavior upon reattachment, largely ignoring the fact that reattachment timing and probability are both endogenous.

areas, but it quickly becomes untenable as the number of firms within a zone diminishes. There are 62 commuting zones and the measure will be calculated for each individual as:

$$Z_{ij0} = \frac{1}{n_{j(i)0} - 1} \sum_{k \neq i} \mathbb{1}\{j(k) = j(i)\} D_{k0} \tag{7}$$

where i indexes the individuals, j indexes the commuting zones, 0 is the time of the layoff event, $k \neq i$ indexes all other individuals that are unemployed at the time of individual i's layoff event (not disemployed at time 0), $n_{j(i)0}$ is the number of currently unemployed individuals in county j at time t, and D_{k0} is the current amount of time (in months) that k has been unemployed at the time of i's layoff (i.e. not shown up in the data following their own separation). Important is that I am only considering individuals unemployed if they are exogenously displaced through a full separation event. This measure is essentially the leave-out-mean unemployment duration at time t for all other people in county j that were *previously* displaced through a layoff event. One potentially serious source of bias in this measure is if firms that I see are underrepresented in the population of that CZ. In that case, the measure will be upwards biased, since I will misclassify people that are re-employed into unobserved firms as long-term unemployed. The only comfort that I can provide to this point is that 83% of people that are displaced are re-employed into a firm that I see within 24 months. Another possible problem is that layoff events may induce general equilibrium effects that influence an individual's local labor market above and beyond what I am measuring at the time of layoff. For a more precise examination of potential mechanisms affecting reattachment rates, see Andersson et al. (2018). As 2 alternative measures of local labor market conditions, I will use CZ-month and county-month unemployment rates from the BLS Local Area Unemployment Statistics (LAUS). The intepretation of the 2SLS coefficient will change slightly under these two measures, as I will discuss. While these measures are more precise estimates of labor market health or tightness than the one constructed from Equation 7, they will be biased if there are significant differences between the re-employment prospects of those in my data (and, particularly, those of young men).

I want to use Z to instrument for individual i's own expected re-employment date (or unemployment duration). The basic idea for using this as an instrument is that being laid off into a worse labor market should impact my own re-employment probability and this is something I have (basically) no control over. The standard 2SLS estimation then proceeds as usual with our first stage and second

stage equations specified as:

$$U_{ij0} = \gamma Z_{ij0} + X_i'\eta + v_{i0}$$
$$Y_{ijt} = \beta_t U_{ij0} + X_i'\theta_t + \epsilon_{ijt}$$

(8)

where X_i is a vector of controls and Y_{ijt} is the crime outcome of interest measured t periods after individual i's initial separation in CZ (or county) j.

Table 5 shows the basic results of this exercise. We can see that OLS estimates are quite large, with 1 additional month of unemployment coinciding with an increase in 1-year crime rates of around 1.67pp (a 16.4% relative increase). Of course, unemployment duration (or re-employment probability) is endogenous and those with longer unemployment durations are likely negatively selected when looking at crime outcomes, as well. Panel B uses average (current) unemployment duration among previously separated workers in the CZ as an instrument. The first stage estimate is fairly strong and the 2SLS estimates are drawn downwards when compared to the OLS estimates, which is not surprising. Though estimates get a bit noisy when breaking down outcomes by crime type, the overall crime effect is still present with 1 additional month of unemployment increasing 1-year crime rates by around 0.68pp (a 6.7% relative increase).

Panels B and C use local unemployment rates from the BLS LAUS at the CZ and county-level, respectively, as instruments. The first stage is particularly strong when looking at the CZ-aggregated instrument with a 1% higher local unemployment rate increasing an individual's own unemployment duration by around .46 months. While this strength is eroded in Panel D, the general second stage estimate remains somewhere between .0078 and .0088. All of these results taken together imply that 1 additional month of unemployment increase 1-year crime rates by between 0.68pp and 0.88pp (a roughly 7-8% relative increase in risk). For reference, the 1-year effect of displacement in Table 3 was found to be around 5.6pp for young men (the same demographic here).

4.2   Layoff Sizes and Local Conditions

4.3   The Long-Run Cost of Separation

Finally, I want to more closely examine how the estimates in section 3 and Appendix D can inform a partial cost-benefit analysis under a counterfactual intervention. When an individual is separated from a job, a few things occur that the state might be concerned with. As shown in this section 3, displace-

ment itself drastically increases criminal behavior over time. In addition to this effect, displacement increases benefits uptake (mechanically and through these other channels), causes long-term employment scarring, increases mortality rates, etc. Roughly, the "cost" of the exogenous unemployment shock imposed on individual i is:

$$\text{Cost} = \Delta\text{Direct Benefits} - \Delta\text{Taxes Collected} + \Delta\text{Crime Cost} + \Delta\text{Other Costs}$$

$$\Delta\text{Direct Benefits} = \sum_t \Delta\text{Unemp Insurance}$$

$$:= \sum_t \Delta b_i$$

$$\Delta\text{Taxes Collected} = \sum_t \Delta\text{Taxes Collected Under Unemp} + \sum_s \Delta\text{Taxes Collected under Re-emp}$$

$$:= \sum_t \Delta\tau_i^u + \sum_s \Delta\tau_i^e$$

$$\Delta\text{Crime Cost} = \sum_t \sum_s \Delta\text{Propensity for Crime}\{\text{Cost of Crime} \quad +$$

$$(\text{Duration of Incarceration} \cdot \text{Cost of Incarceration})\}$$

$$:= \sum_t \sum_s \Delta O_i\{C^O + IC^I\}$$

$$\Delta\text{Other Costs} = \sum_t \sum_s \Delta(\text{Health Costs} + \text{Other Externalities})$$

$$:= \sum_t \sum_s \Delta(H_i + E_i)$$

Here I am assuming 2 possible states following a layoff: unemployment (u) and re-employment (e), where unemployment is indexed by t and re-employment is indexed by s.[28] Though I've obfuscated it above, the "crime effect" should be thought of as recursive, in that committing a crime will have the associated crime cost in the first stage, but will then also increase the duration of unemployment (indexed by t) which will also increase the probability of additional future offense over that horizon. As a simple exercise, we can take most of these parameters directly from section 3 and average across the set of individuals that experience a layoff event:

$$\frac{1}{N}\sum_i \left[\left(\sum_{t_i} \Delta b_i + \Delta\tau_i^u + \Delta O_i^u\{C^O + IC^I\} + \Delta(H_i^u + E_i^u)\right) + \left(\sum_{s_i} \Delta\tau_i^e + \Delta O_i^e\{C^O + IC^I\} + \Delta(H_i^e + E_i^e)\right)\right]$$

---

[28]I use these 2 states because it is clear from the evidence that earnings remain significantly below pre-separation levels upon reattachment to the labor market (and propensity to offend remains slightly above pre-separation levels.

Since I don't have precise re-employment durations (and 17% of people that I see are never re-employed into the payroll firms in my data), a choice parameter also becomes the upper bound on t. I will estimate the average cost with t bounded at 12 months and 24 months. In addition, I will assume that s (the re-employment state) is 36 months.[29] I assume that all displaced individuals claim unemployment insurance for the duration of their unemployment spell (if this does not exceed the cap in Texas). I assume that earnings are solely individual wage earnings, as observed in my data, and that filers are all single. As such, τ_i^u and will simply be the lost income tax revenue (measured precisely), plus the estimated lost revenue from sales and payroll taxes following Piketty, Saez and Zucman (2018). τ_i^e will be measured similarly precisely upon re-employment and can be either positive or negative for each individual (depending upon the relative re-employment wage). ΔO_i^u will be taken as the change in the propensity to commit crime in a given month following displacement, as estimated by Equation 2. ΔO_i^u is taken from Equation 6 and Figure 8. Cost of crime is the estimated average cost of crimes (by crime type) according to the Texas DPS and cost of incarceration is taken from the Texas Comptroller's Office as the average (not marginal) cost of incarceration across all state prisons in Texas.[30] Incarceration duration is measured directly and taken as the average across all charges that result in a conviction among individuals that were laid off.[31] Health costs will only consider mortality with value of a statistical life set at roughly 5 million dollars; estimates therein are taken from Sullivan and Von Wachter (2009) with separation age set at 30. Since I am only considering mortality, and not general health changes, this cost is likely biased significantly downward. Other externalities, such as spillovers from poverty, are not considered and likely further reinforce the sentiment that these estimates are lower bounds on the true underlying cost of separation.[32]

5 Conclusion

This paper aims to estimate the causal effects of negative labor shocks on criminal behavior. I find that displacement from the labor market leads to large reductions in employment and subsequent

---

[29] If anything, this will provide a lower bound on the costs, since it's clear from this paper and prior work that earnings remain depressed (and crime likely remains elevated) for many years beyond this.

[30] See https://www.dps.texas.gov/sites/default/files/documents/crimereports/20/2020cit.pdf for crime costs. Though these estimates likely move, I simply deflate the numbers to 2010 as the reference period.

[31] Though it's not explicitly stated, I also consider outcomes where probation or parole are assigned. Probation fees paid by offenders and their potentially subsequent effect on reoffense are not considered here.

[32] While it's possible to perform a similar exercise with partial separations, it's harder to determine both benefits paid out (since assuming that they claim is nontrivial) and re-employment timing since many remain at their depressed state for longer than 12 (or 24) months.

re-employment earnings. These losses seem to induce a sharp increase in crime, with arrests and convictions nearly doubling in the second and third months following displacement. Though these effects abate somewhat, there is still a $\sim$60%-70% increase in offending risk through the first year and a $\sim$45% increase through the second. When compared to previous findings, these effects are larger and are identified much closer to the layoff event itself. This difference could be an artifact of the data setting, benefits/criminal court structure in Texas, or otherwise. While those without priors exhibit less response, the relative increase in offending risk is about the same when compared to those who have prior offenses. In addition, I see larger effects among "poorer" individuals and among those who are separated into worse labor markets. And while the effects of full separation events are large, crime responses are identified under only partial separation events.

Note 5/10/23: Incomplete

References

Abowd, John M, Francis Kramarz, and David N Margolis. 1999. "High wage workers and high wage firms." *Econometrica*, 67(2): 251–333.

Agan, Amanda, and Sonja Starr. 2018. "Ban the box, criminal records, and racial discrimination: A field experiment." *The Quarterly Journal of Economics*, 133(1): 191–235.

Aizer, Anna, and Joseph J Doyle Jr. 2015. "Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges." *The Quarterly Journal of Economics*, 130(2): 759–803.

Andersson, Fredrik, John C Haltiwanger, Mark J Kutzbach, Henry O Pollakowski, and Daniel H Weinberg. 2018. "Job displacement and the duration of joblessness: The role of spatial mismatch." *Review of Economics and Statistics*, 100(2): 203–218.

Andreoni, James. 1991. "Reasonable doubt and the optimal magnitude of fines: should the penalty fit the crime?" *The RAND Journal of Economics*, 385–395.

Ashenfelter, Orley. 1978. "Estimating the effect of training programs on earnings." *The Review of Economics and Statistics*, 47–57.

Avery, Robert B, Paul S Calem, Glenn B Canner, and Raphael W Bostic. 2003. "An overview of consumer data and credit reporting." *Fed. Res. Bull.*, 89: 47.

Barbarino, Alessandro, and Giovanni Mastrobuoni. 2014. "The incapacitation effect of incarceration: Evidence from several Italian collective pardons." *American Economic Journal: Economic Policy*, 6(1): 1–37.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169–217.

Bennett, Patrick, and Amine Ouazad. 2020. "Job displacement, unemployment, and crime: Evidence from danish microdata and reforms." *Journal of the European Economic Association*, 18(5): 2182–2220.

Bhuller, Manudeep, Gordon Dahl, Katrine Løken, and Magne Mogstad. 2020. "Incarceration, Recidivism, and Employment." *Journal of Political Economy.*

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2021. "Revisiting event study designs: Robust and efficient estimation." *arXiv preprint arXiv:2108.12419.*

Brevoort, Kenneth P, Philipp Grimm, and Michelle Kambara. 2015. "Credit Invisibles." *Consumer Financial Protection Bureau Office of Research Reports Series*, , (15-1).

Card, David, Andrew Johnston, Pauline Leung, Alexandre Mas, and Zhuan Pei. 2015. "The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression kink design in Missouri, 2003-2013." *American Economic Review*, 105(5): 126–30.

Chalfin, Aaron, and Justin McCrary. 2017. "Criminal deterrence: A review of the literature." *Journal of Economic Literature*, 55(1): 5–48.

Chalfin, Aaron, and Justin McCrary. 2018. "Are US cities underpoliced? Theory and evidence." *Review of Economics and Statistics*, 100(1): 167–186.

Deshpande, Manasi, and Michael Mueller-Smith. 2022. "Does welfare prevent crime? the criminal justice outcomes of youth removed from ssi." *The Quarterly Journal of Economics*, 137(4): 2263–2307.

Di Tella, Rafael, and Ernesto Schargrodsky. 2004. "Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack." *American Economic Review*, 94(1): 115–133.

Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review*, 108(2): 201–40.

Dostoevsky, Fyodor. 1866. "Crime and Punishment."

Ehrlich, Isaac. 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy*, 81(3): 521–565.

Enamorado, Ted, Benjamin Fifield, and Kosuke Imai. 2019. "Using a probabilistic model to assist merging of large-scale administrative records." *American Political Science Review*, 113(2): 353–371.

Feigenberg, Benjamin, and Conrad Miller. 2021. "Racial divisions and criminal justice: Evidence from southern state courts." *American Economic Journal: Economic Policy*, 13(2): 207–40.

Franco Buitrago, Catalina, David J Harding, Shawn D Bushway, and Jeffrey D Morenoff. 2022. "Failing to Follow the Rules: Can Imprisonment Lead to More Imprisonment Without More Actual Crime?" *NHH Dept. of Economics Discussion Paper*, , (03).

Glaeser, Edward L, and Bruce Sacerdote. 1999. "Why is there more crime in cities?" *Journal of political economy*, 107(S6): S225–S258.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.

Gould, Eric D, Bruce A Weinberg, and David B Mustard. 2002. "Crime rates and local labor market opportunities in the United States: 1979–1997." *Review of Economics and statistics*, 84(1): 45–61.

Grogger, Jeff. 1998. "Market wages and youth crime." *Journal of labor Economics*, 16(4): 756–791.

Holzer, Harry J, Steven Raphael, and Michael A Stoll. 2006. "Perceived criminality, criminal background checks, and the racial hiring practices of employers." *The Journal of Law and Economics*, 49(2): 451–480.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993. "Earnings Losses of Displaced Workers." *The American Economic Review*, 83(4): 685–709.

Khanna, Gaurav, Carlos Medina, Anant Nyshadham, Christian Posso, and Jorge Tamayo. 2021. "Job Loss, Credit, and Crime in Colombia." *American Economic Review: Insights*, 3(1): 97–114.

Kling, Jeffrey R. 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review*, 96(3): 863–876.

Lachowska, Marta, Alexandre Mas, and Stephen A Woodbury. 2020. "Sources of displaced workers' long-term earnings losses." *American Economic Review*, 110(10): 3231–66.

Lee, David S, and Justin McCrary. 2017. "The deterrence effect of prison: Dynamic theory and evidence." In *Regression discontinuity designs.* Emerald Publishing Limited.

Levitt, Steven D. 1995. "Using electoral cycles in police hiring to estimate the effect of policeon crime."

Lin, Ming-Jen. 2008. "Does unemployment increase crime? Evidence from US data 1974–2000." *Journal of Human resources*, 43(2): 413–436.

Macleod, W. Bentley, and Roman Rivera. 2022. "Deterrence, Income Support and Optimal Crime Policy." *Working Paper.*

Mello, Steven. 2018. "Speed Trap or Poverty Trap? Fines, Fees, and Financial Wellbeing." *Working Paper.*

Morrison, William, and John Wieselthier. 2022. "Legal Financial Obligations and Reoffense." *Working Paper.*

Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration." *American Economic Review.*

Myers Jr, Samuel L. 1983. "Estimating the economic model of crime: Employment versus punishment effects." *The Quarterly Journal of Economics*, 98(1): 157–166.

Nekoei, Arash, and Andrea Weber. 2017. "Does extending unemployment benefits improve job quality?" *American Economic Review*, 107(2): 527–61.

Norris, Samuel, Matthew Pecenco, and Jeffrey Weaver. 2021. "The effects of parental and sibling incarceration: Evidence from ohio." *American Economic Review*, 111(9): 2926–63.

Pager, Devah. 2003. "The mark of a criminal record." *American journal of sociology*, 108(5): 937–975.

Piketty, Thomas, Emmanuel Saez, and Gabriel Zucman. 2018. "Distributional national accounts: methods and estimates for the United States." *The Quarterly Journal of Economics*, 133(2): 553–609.

Polinsky, Mitchell. 2006. "The Optimal Use of Fines and Imprisonment When Wealth is Unobservable." *Journal of Public Economics*, 90(4-5): 823–835.

Raphael, Steven, and Rudolf Winter-Ebmer. 2001. "Identifying the effect of unemployment on crime." *The Journal of Law and Economics*, 44(1): 259–283.

Rege, Mari, Torbjørn Skardhamar, Kjetil Telle, and Mark Votruba. 2009. "The effect of plant closure on crime."

Rege, Mari, Torbjørn Skardhamar, Kjetil Telle, and Mark Votruba. 2019. "Job displacement and crime: Evidence from Norwegian register data." *Labour Economics*, 61: 101761.

Rose, Evan K. 2018. "The Effects of Job Loss on Crime: Evidence From Administrative Data." *Working Paper.*

Rose, Evan K, and Yotam Shem-Tov. 2021. "How does incarceration affect reoffending? estimating the dose-response function." *Journal of Political Economy*, 129(12): 3302–3356.

Schmieder, Johannes F, Till von Wachter, and Stefan Bender. 2016. "The effect of unemployment benefits and nonemployment durations on wages." *American Economic Review*, 106(3): 739–77.

Shem-Tov, Yotam. 2020. "Make-or-Buy? The Provision of Indigent Defense Services in the U.S." *Working Paper*.

Sullivan, Daniel, and Till Von Wachter. 2009. "Job displacement and mortality: An analysis using administrative data." *The Quarterly Journal of Economics*, 124(3): 1265–1306.

Von Wachter, Till, Jae Song, and Joyce Manchester. 2009. "Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using US administrative data from 1974 to 2004." *unpublished paper, Columbia University*.

Witte, Ann Dryden. 1980. "Estimating the economic model of crime with individual data." *The quarterly journal of economics*, 94(1): 57–84.

Yagan, Danny. 2019. "Employment hysteresis from the great recession." *Journal of Political Economy*, 127(5): 2505–2558.

Table 1: Summary Statistics

	All			Separators			Stayers			Young Male Separators			Young Male Stayers		
	Mean	SD	P50	Mean	SD	P50	Mean	SD	P50	Mean	SD	P50	Mean	SD	P50
Demographics															
Age	36.5	14.4	35.2	36.3	14.2	34.9	36.6	14.4	35.4	29.1	5.7	29.0	29.2	5.5	29.0
Male	0.58			0.59			0.58			1.00			1.00		
White	0.50			0.47			0.51			0.46			0.52		
Black	0.13			0.16			0.12			0.15			0.13		
Hispanic	0.34			0.34			0.34			0.36			0.36		
Employment															
Tenure (years)	3.2	2.0	3.0	2.6	1.9	2.3	3.3	2.1	3.0	2.3	1.5	2.0	2.4	2.0	1.7
Rough Earnings (monthly)	3530.3	3032.9	2923.0	3620.2	3433.7	3278.1	3492.3	2990.6	3350.0	3550.1	3373.1	3380.8	3417.5	3070.6	3291.5
Criminal History															
Prior Felony	0.023			0.025			0.023			0.041			0.040		
Prior Misdemeanor	0.073			0.075			0.073			0.122			0.119		
Crime in Year Prior	0.033			0.034			0.029			0.073			0.067		
Individuals	3,356,805			240,067			616,738			99,201			210,860		

NOTES: Test

Table 2: JLS: Basic Results

	Employment			**Crime**[*]		

Panel A: Separators vs Stayers

	Employment	Earnings ($)	Hours[*]	Misdemeanors	Felonies	Economic
$t=-6$	−0.013	135.6	0.6*	0.002	0.000	0.003
	(0.030)	(110.0)	(0.3)	(0.004)	(0.005)	(0.004)
$t=-5$	−0.006	128.7	0.4	0.003	0.000	0.002
	(0.015)	(105.2)	(0.4)	(0.005)	(0.006)	(0.005)
$t=-4$	−0.002	100.1	0.2	−0.001	−0.001	−0.002
	(0.012)	(89.3)	(0.3)	(0.004)	(0.006)	(0.005)
$t=-3$	−0.001	52.6	0.2	−0.002	−0.001	0.002
	(0.009)	(75.3)	(0.3)	(0.006)	(0.006)	(0.005)
$t=-2$	−0.001	58.0	0.0	0.002	0.000	0.000
	(0.010)	(101.9)	(0.3)	(0.004)	(0.005)	(0.005)
$t=-1$	0.005	22.8	0.0	0.003	0.002	0.002
	(0.008)	(91.2)	(0.3)	(0.004)	(0.004)	(0.004)
$t=0$	−0.001	−1745.3***	−17.4***	0.011**	0.009*	0.012**
	(0.014)	(130.0)	(0.4)	(0.005)	(0.005)	(0.006)
$t=1$	−0.898***	−2856.3***	−30.5***	0.024***	0.020***	0.025***
	(0.013)	(156.6)	(0.5)	(0.004)	(0.005)	(0.006)
$t=2$	−0.776***	−2598.2***	−29.4***	0.028***	0.023***	0.029***
	(0.013)	(172.5)	(0.5)	(0.004)	(0.005)	(0.005)
$t=3$	−0.717***	−2156.0***	−27.2***	0.021***	0.025***	0.025***
	(0.012)	(170.6)	(0.5)	(0.004)	(0.006)	(0.006)
$t=4$	−0.614***	−2087.9***	−25.4***	0.017***	0.019***	0.020***
	(0.017)	(160.9)	(0.5)	(0.003)	(0.005)	(0.005)
$t=5$	−0.555***	−1920.7***	−24.9***	0.017***	0.014**	0.018***
	(0.027)	(156.4)	(0.4)	(0.004)	(0.006)	(0.006)
$t=6$	−0.529***	−1807.7***	−22.7***	0.016***	0.013**	0.018***
	(0.019)	(189.9)	(0.5)	(0.004)	(0.006)	(0.006)

Panel B: High-Risk Stayers vs High-Risk Separators

	Employment	Earnings ($)	Hours[*]	Misdemeanors	Felonies	Economic
$t=-6$	−0.017	152.2	0.9*	0.004	0.002	0.003
	(0.050)	(153.2)	(0.6)	(0.007)	(0.006)	(0.006)
$t=-5$	−0.011	141.1	0.9	0.004	0.001	0.002
	(0.023)	(145.7)	(0.6)	(0.007)	(0.006)	(0.006)
$t=-4$	−0.005	133.3	0.9*	−0.004	0.001	−0.002
	(0.027)	(148.3)	(0.5)	(0.006)	(0.006)	(0.005)
$t=-3$	−0.001	87.0	0.6	−0.003	−0.001	0.002
	(0.019)	(159.0)	(0.6)	(0.006)	(0.006)	(0.005)
$t=-2$	−0.002	60.1	0.3	0.002	0.000	0.003
	(0.019)	(153.8)	(0.5)	(0.006)	(0.007)	(0.006)
$t=-1$	−0.003	33.1	0.4	0.003	0.002	0.003
	(0.017)	(145.2)	(0.5)	(0.005)	(0.007)	(0.006)
$t=0$	−0.002	−1830.9***	−17.4***	0.023***	0.019**	0.020***
	(0.018)	(255.1)	(0.6)	(0.005)	(0.005)	(0.006)
$t=1$	−0.871***	−2896.3***	−30.7***	0.044***	0.041***	0.045***
	(0.019)	(270.7)	(0.6)	(0.006)	(0.007)	(0.006)
$t=2$	−0.756***	−2540.2***	−28.4***	0.058***	0.049***	0.059***
	(0.021)	(283.1)	(0.6)	(0.006)	(0.006)	(0.005)
$t=3$	−0.668***	−2206.2***	−27.0***	0.053***	0.050***	0.058***
	(0.020)	(260.0)	(0.7)	(0.005)	(0.006)	(0.007)
$t=4$	−0.625***	−2030.0***	−24.9***	0.050**	0.049***	0.053***
	(0.025)	(251.9)	(0.6)	(0.005)	(0.007)	(0.007)
$t=5$	−0.578***	−1850.1***	−22.0***	0.043***	0.040**	0.049***
	(0.036)	(273.0)	(0.6)	(0.006)	(0.007)	(0.007)
$t=6$	−0.542***	−1758.7***	−21.1***	0.040***	0.038**	0.047***
	(0.031)	(260.1)	(0.7)	(0.007)	(0.008)	(0.007)

NOTES: Test

Table 3: Relative Earnings (Non-Zeroes) and Cumulative Crime Effects

	All		Men, 20-35		Men, 36-60		Women, 20-35		Women, 36-60	
	Earnings	Any Charge	Earnings	Any Charge	Earnings	Any Charge	Earnings	Any Charge	Earnings	Any Charge
3 months	−0.073***	0.013***	−0.095***	0.029***	−0.133***	0.016	−0.061	0.006	−0.112***	0.003
	(0.020)	(0.004)	(0.033)	(0.009)	(0.037)	(0.010)	(0.043)	(0.012)	(0.040)	(0.017)
6 months	−0.107***	0.021**	−0.121***	0.044***	−0.164***	0.025**	−0.087**	0.009	−0.129***	0.005
	(0.017)	(0.004)	(0.027)	(0.009)	(0.031)	(0.012)	(0.039)	(0.014)	(0.039)	(0.020)
12 months	−0.137***	0.028***	−0.142***	0.056***	−0.161***	0.031**	−0.081***	0.011	−0.120***	0.006
	(0.015)	(0.006)	(0.024)	(0.010)	(0.025)	(0.017)	(0.033)	(0.017)	(0.035)	(0.023)
18 months	−0.128***	0.032***	−0.145***	0.064***	−0.153***	0.034**	−0.095***	0.014	−0.113***	0.006
	(0.016)	(0.006)	(0.024)	(0.011)	(0.027)	(0.017)	(0.030)	(0.018)	(0.038)	(0.025)
24 months	−0.133***	0.036***	−0.151***	0.071***	−0.147***	0.036**	−0.103***	0.015	−0.117***	0.006
	(0.016)	(0.007)	(0.025)	(0.011)	(0.027)	(0.017)	(0.029)	(0.020)	(0.040)	(0.025)
Individuals	835,955		310,061		183,338		215,516		127,040	

NOTES: Earnings are relative to baseline earnings (average of an individual's earnings in the 6 months preceding a layoff) and do not include zeroes (those who remain unemployed or leave my sample). These can likely be thought of as re-employment earnings. I bound ages at 60 at time of initial separation so that estimates of re-employment earnings can still be reasonably obtained. If this bound is instead set at 65, for instance, the probability of re-employment within my data drops by 40%, which isn't surprising.

Table 4: JLS: Local Labor Market Conditions

	6 Months	12 months
Panel A: Firm County		
Subsample: Bottom 25% Income/Capita		
Coefficient	0.061***	0.087***
Standard Error	(0.017)	(0.019)
Subsample: Top 75% Income/Capita		
Coefficient	0.037***	0.050***
Standard Error	(0.014)	(0.015)
Subsample: Bottom 25% Employment Rate		
Coefficient	0.063***	0.091***
Standard Error	(0.017)	(0.020)
Subsample: Top 75% Employment Rate		
Coefficient	0.036**	0.050***
Standard Error	(0.015)	(0.015)
Panel B: Separation Timing		
Subsample: Pre-Recession		
Coefficient	0.039***	0.052***
Standard Error	(0.015)	(0.015)
Subsample: Recession		
Coefficient	0.056***	0.077***
Standard Error	(0.022)	(0.024)
Subsample: Post-Recession		
Coefficient	0.050	0.067
Standard Error	(0.039)	(0.051)
Panel C: Recession Affected CZs		
Subsample: Most Affected (25%)		
Coefficient	0.051**	0.065***
Standard Error	(0.022)	(0.024)
Subsample: Least Affected (25%)		
Coefficient	0.042*	0.055**
Standard Error	(0.025)	(0.025)
Subsample: Least Affected (75%)		
Coefficient	0.041**	0.053***
Standard Error	(0.020)	(0.020)

NOTES: Test

Table 5: The Effect of Local Unemployment on 1-Year Crime Outcomes

	All Crime	Economic Crime	Violent Crime
OLS: Unemployment	0.0167***	0.0091***	0.0113***
	(0.0023)	(0.0029)	(0.0029)

Panel A: 1 Additional Month of Unemp for Prior Separators (CZ)

Unemployment (Months)

	All Crime	Economic Crime	Violent Crime
First Stage	0.3601***		
	(0.0397)		
IV: Unemployment	0.0068**	0.0031	0.0026
	(0.0033)	(0.0042)	(0.0046)

Panel B: 1% Increase in Local Unemployment at Separation (CZ)

Unemployment (Months)

	All Crime	Economic Crime	Violent Crime
First Stage	0.4601***		
	(0.0237)		
IV: Unemployment	0.0088***	0.0053*	0.0026
	(0.0027)	(0.0036)	(0.0041)

Panel C: 1% Increase in Local Unemployment at Separation (County)

Unemployment (Months)

	All Crime	Economic Crime	Violent Crime
First Stage	0.2501***		
	(0.0401)		
IV: Unemployment	0.0078*	0.0033	0.0036
	(0.0047)	(0.0067)	(0.0079)
Dependent Mean	0.1013	0.0673	0.0448
Observations	83,297	83,297	83,297

NOTES: When compared to Table 1, I lose some observations due to my inability to pinpoint their commuting zone. I maintain this same sample in Panel D (when the instrument is constructed by county) so that the composition is not shifting and comparisons can be made more easily.
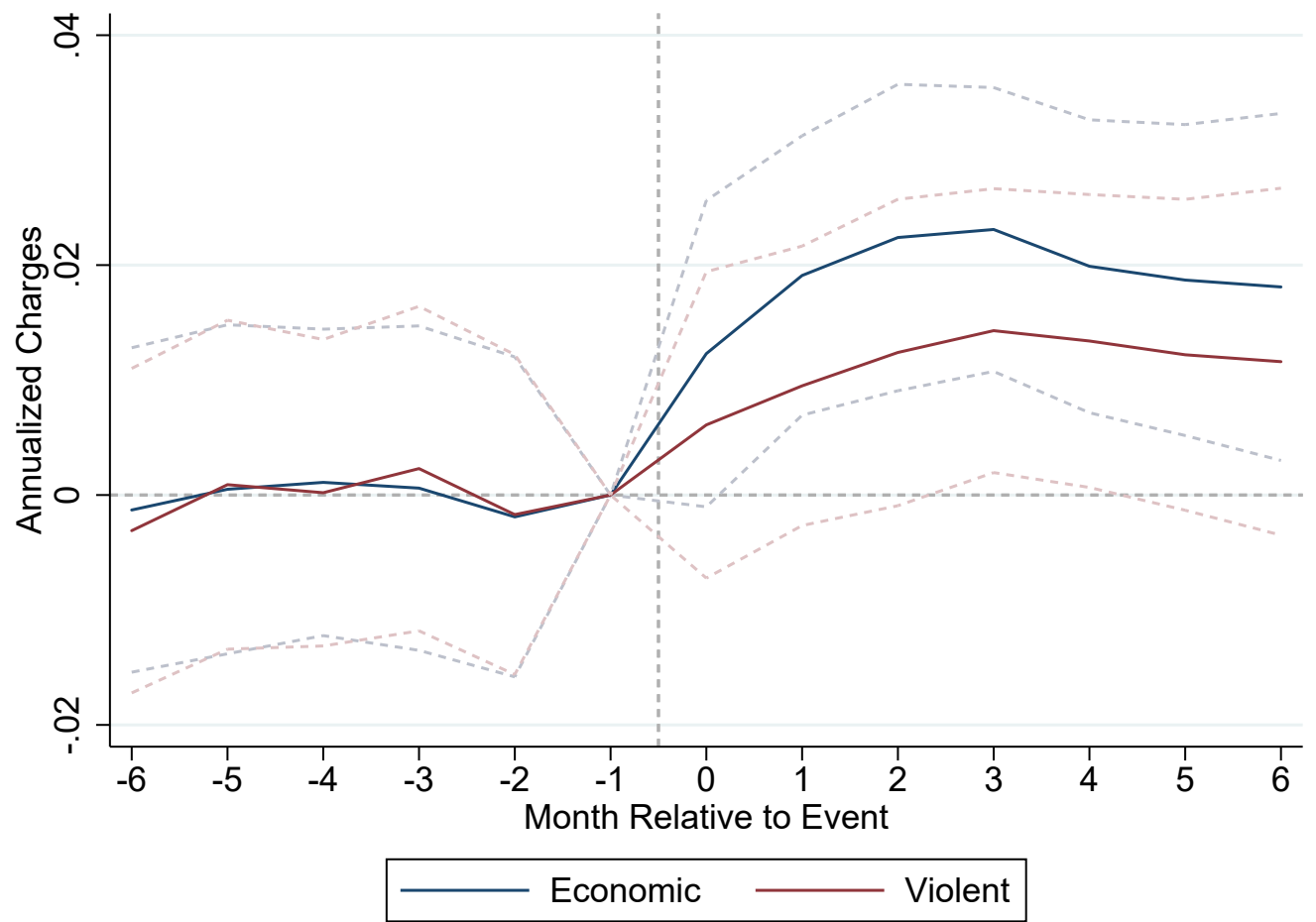
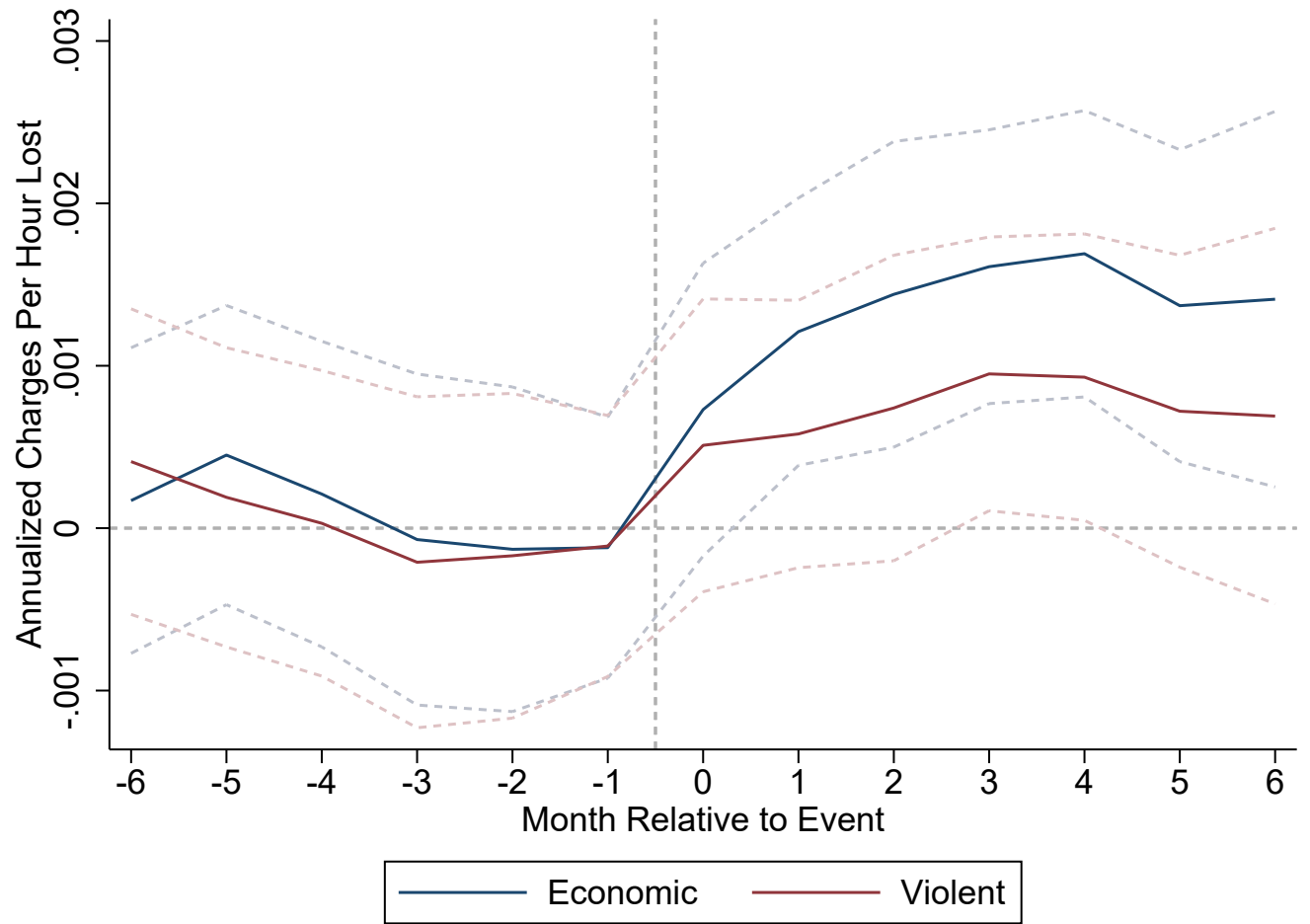Figure 1: JLS: Economic and Violent Crimes
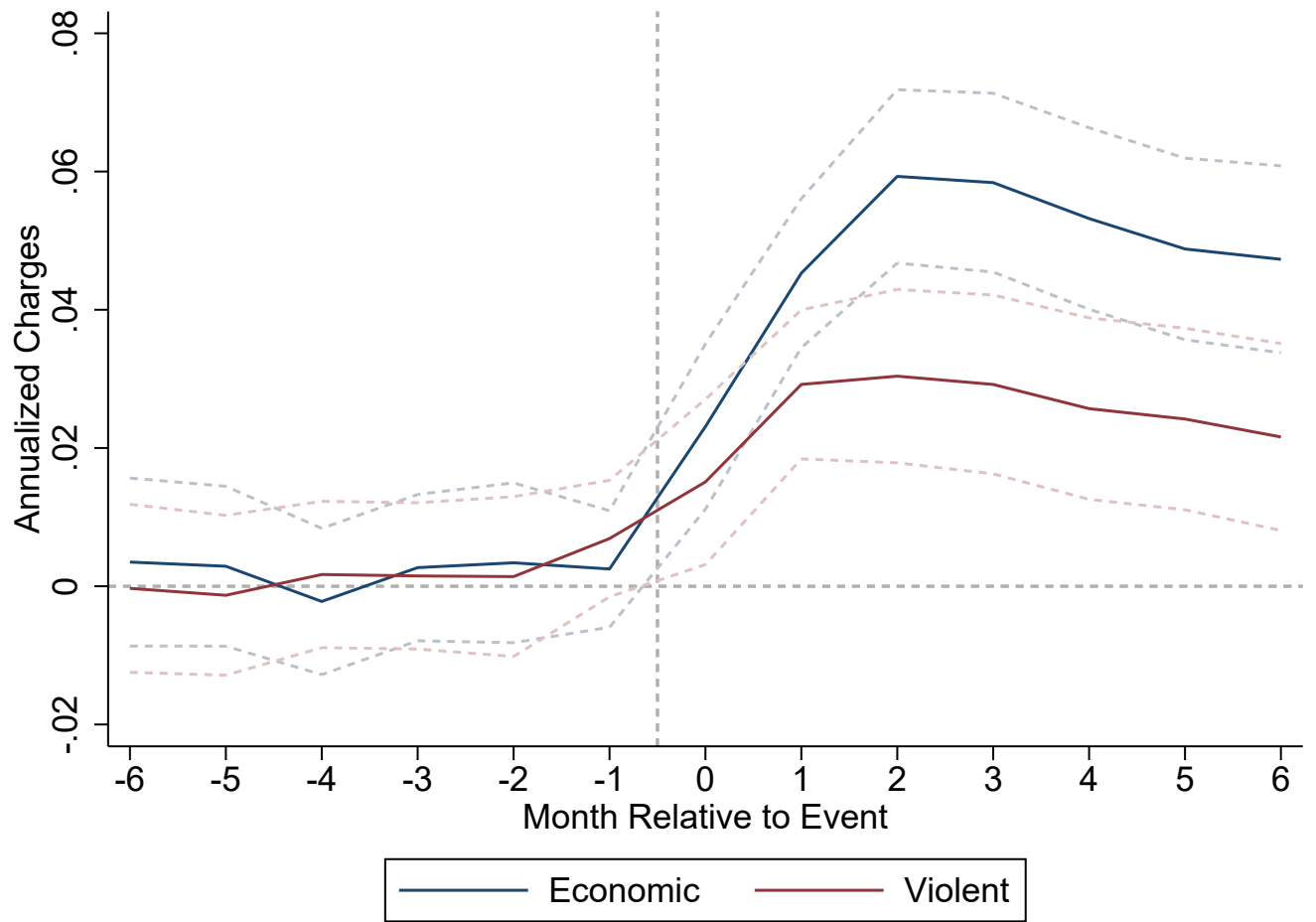
Figure 2: JLS: Prior vs No Prior
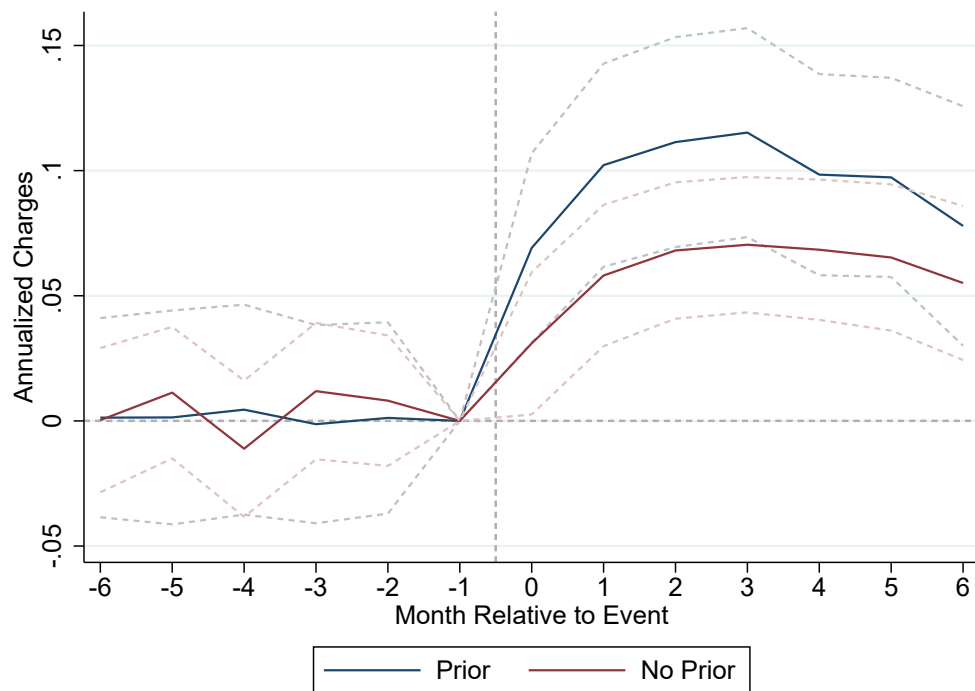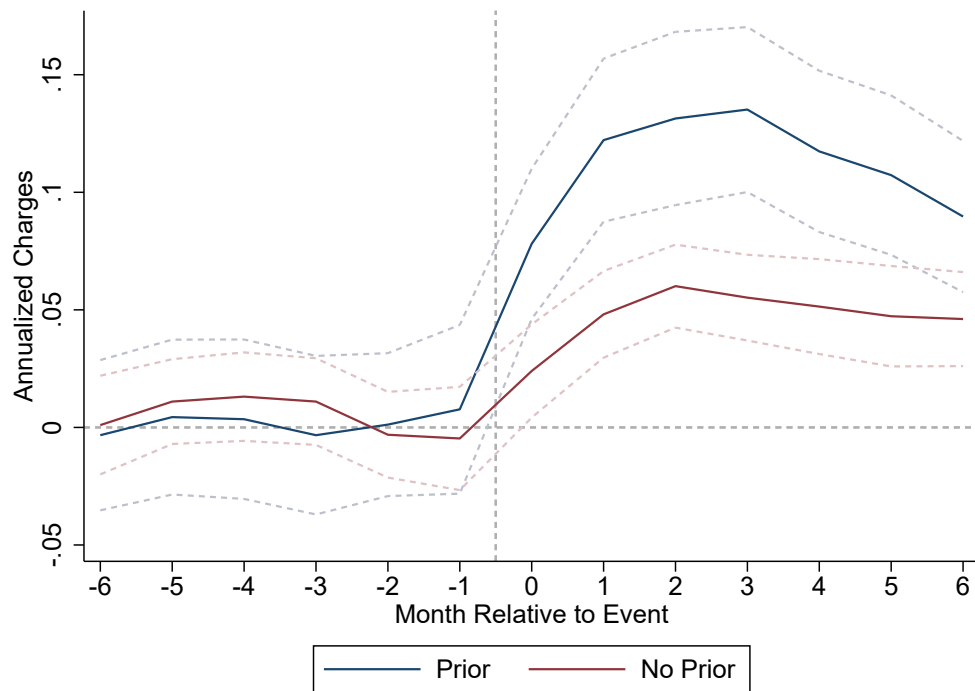
NOTES: test
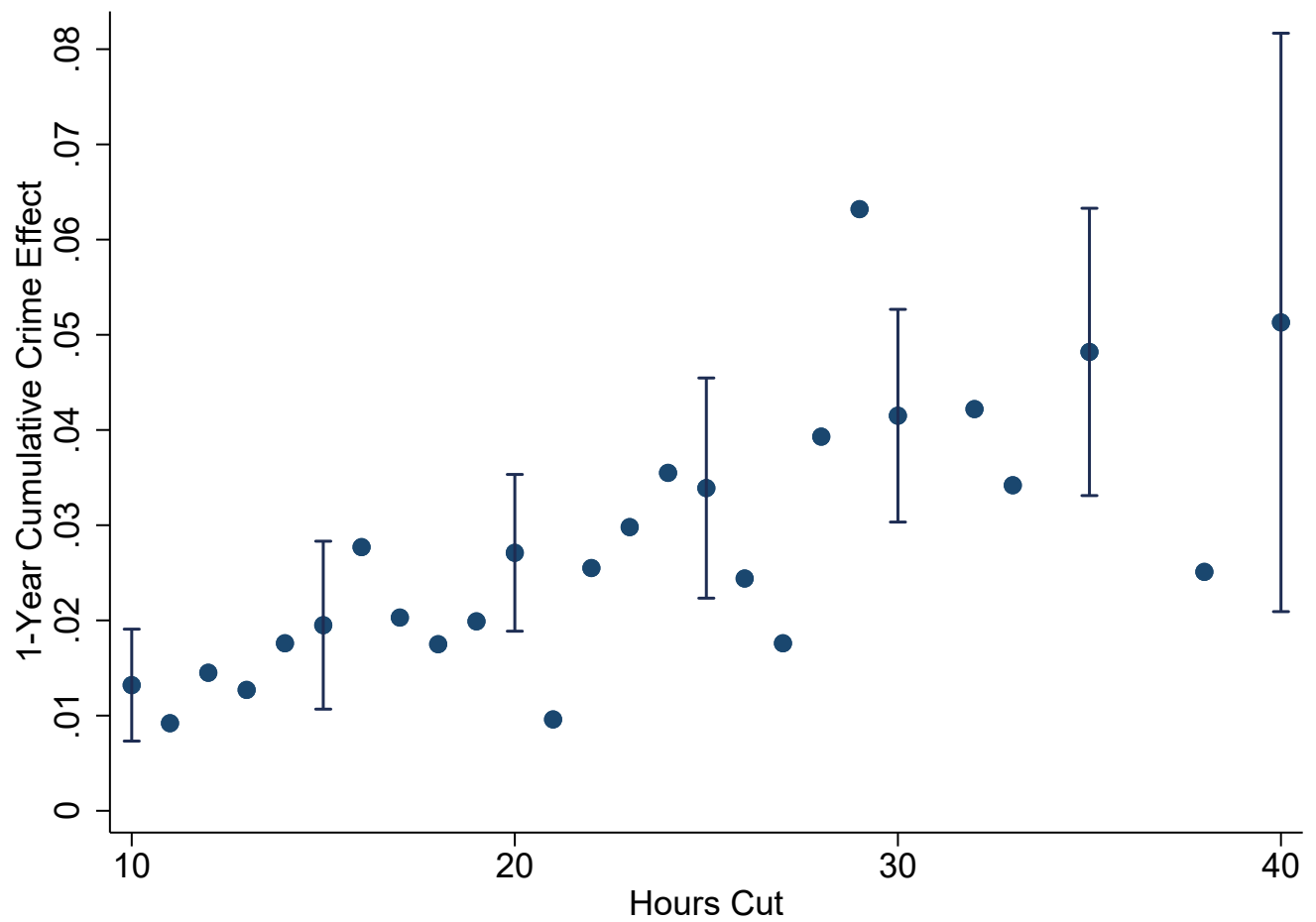
Figure 3: Partial Separation: Economic and Violent Crimes

Figure 4: Partial Separation: Economic and Violent Crimes (Per Hour)

Figure 5: Partial Separations: 1-Year Cumulative JLS Coefficient by Hour Cut

NOTES: This does not include those that were part of a full layoff event; that is, 40 "hours cut" here is still those who lost 40 hours of work following a partial separation event. Coefficients for some of the individual hours cut bins were unable to be estimated due to small sample size within that bin; for instance, there were only 12 individuals (5 young men) in my sample who had 39 hours cut in a partial separation event out of over 130,000 people who experience some such event.

Figure 6: Collections Balance and Major Derogatory Events
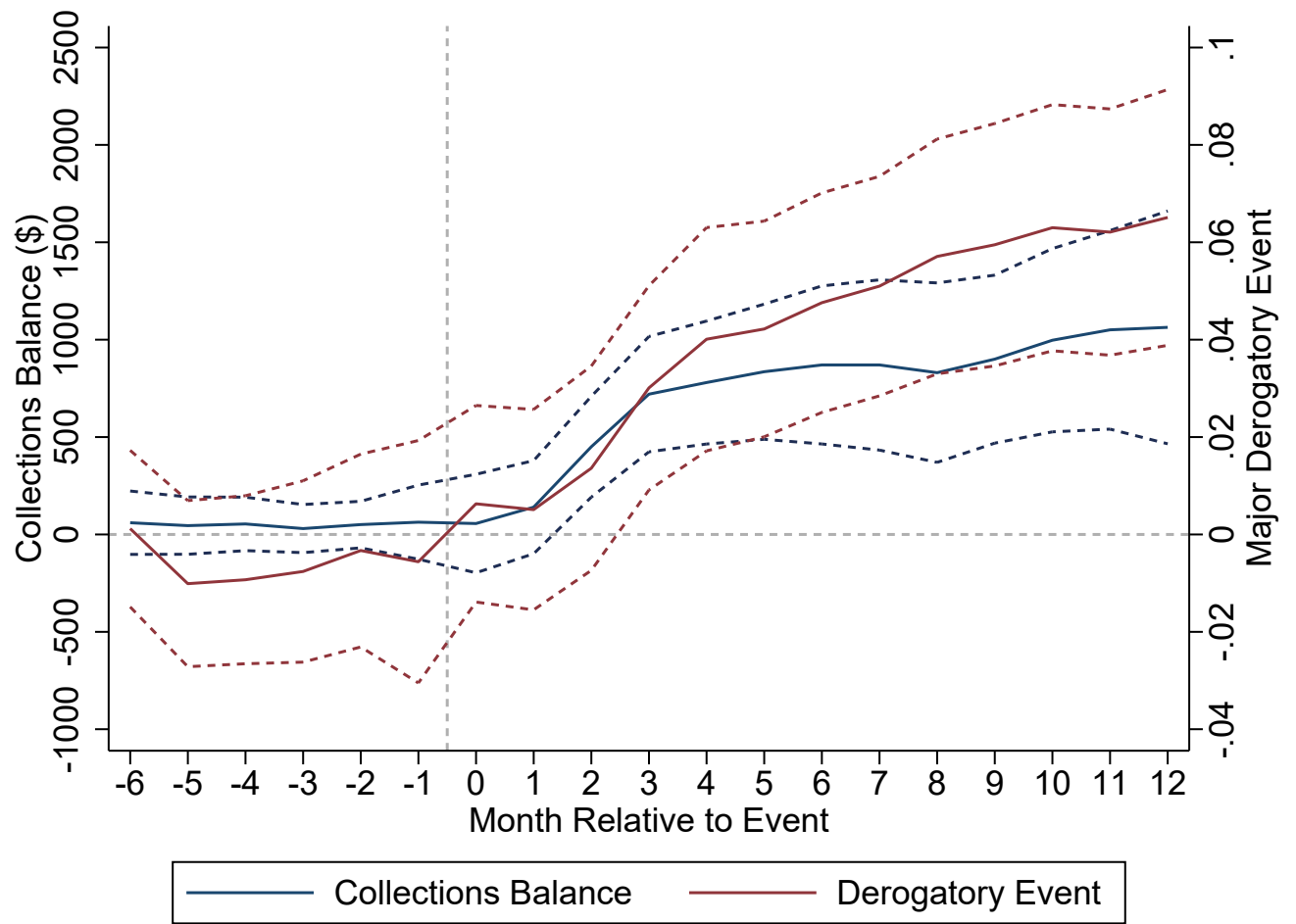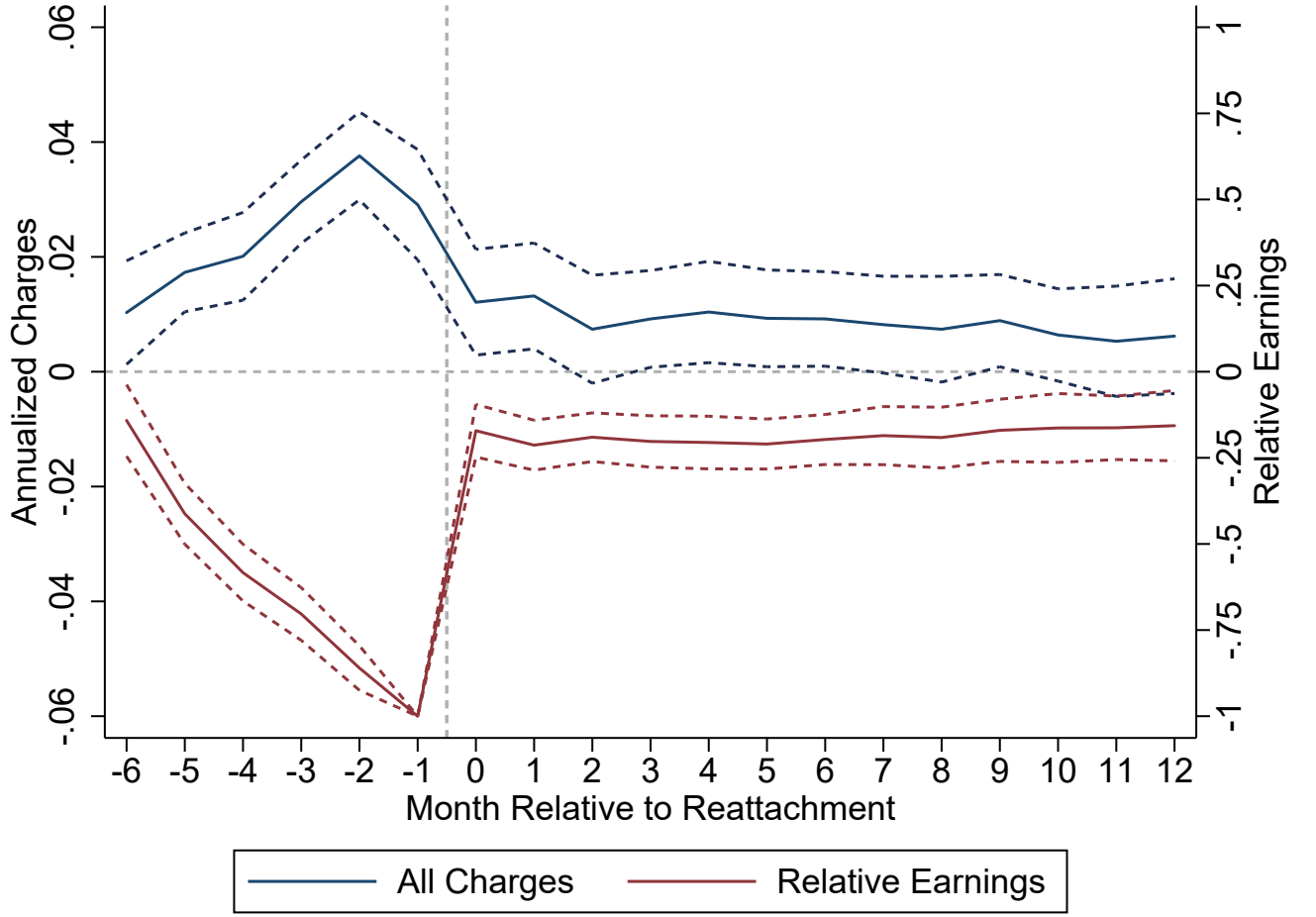
NOTES:

Figure 7: Income, Credit, and Crime
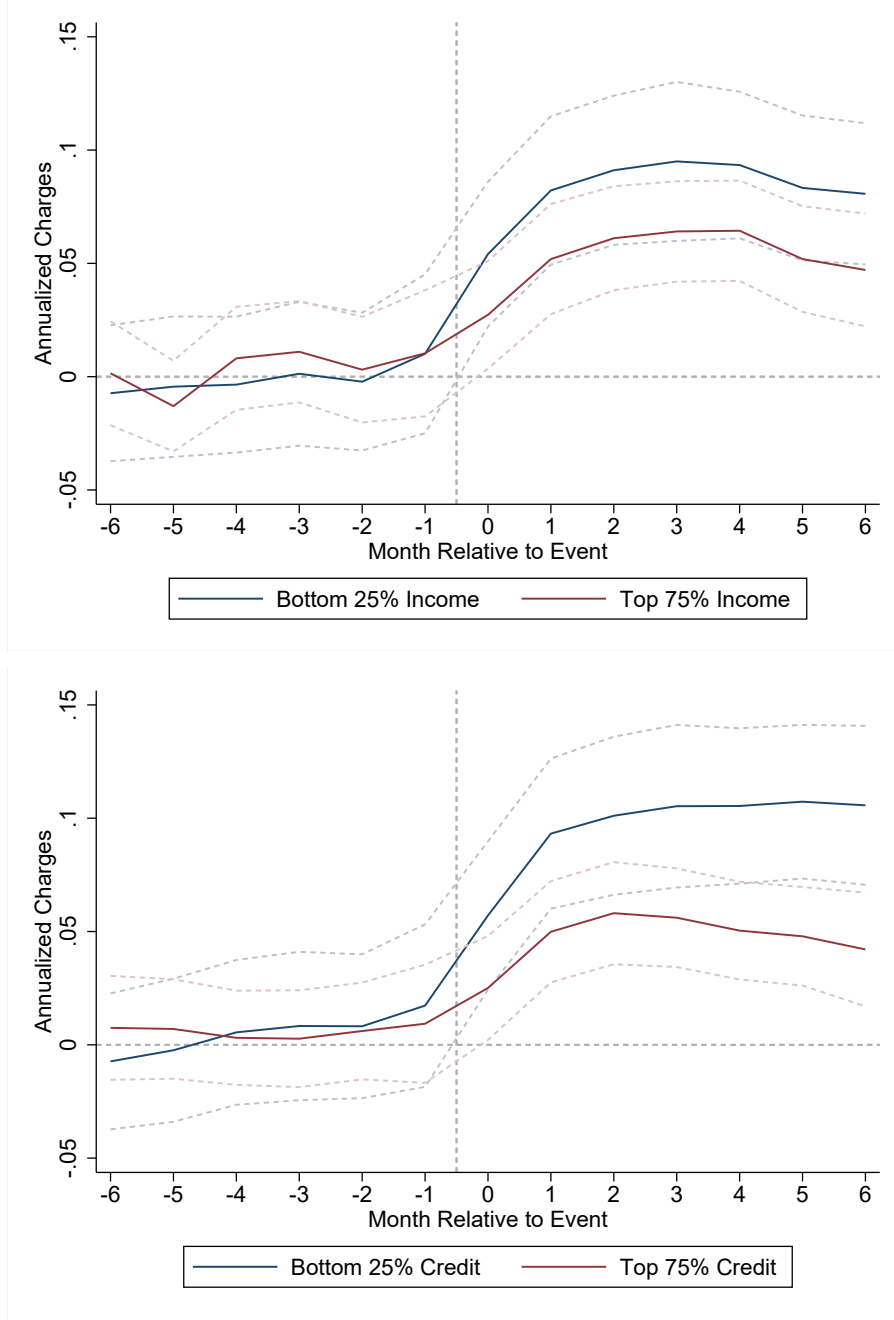
NOTES: These figures are estimated by:

$$y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \in K} D_{it}^k \cdot \delta_k + \varepsilon_{it}$$

separately by bottom 25% of income, top 75% of income, bottom 25% of credit, and top 75% of credit. Income is defined by the average of an individual's income in the 6 months prior to a layoff event. Credit is defined by outstanding collections accounts 2 periods prior to the layoff event ($t = -2$). In both figures, estimates of δ_k are plotted for $kin\{-6, \ldots, -2, 0, 1, \ldots, 6\}$. Standard errors are clustered at the employee-firm level and 95% confidence intervals are shown as dotted lines.

Figure 8: Reattachment, Crime, and Earnings

NOTES: This figure is estimated by:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{j \neq -7} \beta_j \mathbb{1}\{Reattachment_{it} = j\} + \varepsilon_{it}$$

among young men who were displaced (in full separation events) and then reattached to the labor market within at most 6 months and who experienced at least 1 month of unemployment. On the left axis, $Y_{it} \coloneqq \mathbb{1}\{i$ charged with crime at $t\}$ scaled so that it's comparable to annual charges (estimate and standard errors multiplied by 12). On the right axis, $Y_{it} \coloneqq \frac{\text{Earnings}_t}{\text{Earnings}_{t=-7}}$. Though the procedure estimates each β_j for $j \in \{-13, \ldots, -8\} \cup \{-7, \ldots, 13\}$, I've excluded $\{\beta_{-6}, \ldots, \beta_{-1}\}$ from the figure for relative earnings (right axis) because the interpretation of the coefficients relies on knowledge of the underlying proportion of individuals that are laid off by each time period; that is, not everyone in this event study is unemployed for those 6 pre-period months, as some have yet to be laid off. The same concern is true of the estimates for annualized chages in the pre-period, but the pattern here can be simply considered a mirror of the pattern we see in other figures (weighted by the proportion that have experienced their layoff by j). Standard errors are clustered at the employee-firm level and 95% confidence intervals are shown as dotted lines.

39

A   Data Appendix

In this section, I describe the matching procedure more precisely and include some examples of how the match treats individuals with similar information. I also include a brief data dictionary that more precisely identifies variables that I refer to throughout the text, as well as the structure of the panel that I've constructed.

A.1   Matching Procedure

The credit and payroll data are already matched, so the exercise left in this paper is to match the crime data to those sets. The combined Department of Public Safety (DPS) data includes demographic information, such as name, date of birth, sex, address, race, etc. And while the payroll and credit data do not include sex and race, they can be constructed reasonably (though, I will show this is not really necessary for a good match).

I begin by standardizing all names. Names are stored in unicode format in the credit and payroll data, so I first map any foreign characters into their respective English phonemes and recast all characters to lowercase. I standardize suffixes (junior → jr) in this data and then perform a similar standardization in the DPS data across a number of variables (addresses, in particular). At thiis point, I have a set of individuals in the credit/payroll data and a set in the DPS data and the only choices remaining are which variables to match across and which criterion to choose.

I follow Enamorado, Fifield and Imai (2019) for the basic procedure and iterate over a few combinations of potential information that can be used for matching. In particular, name and date of birth are always selected. Other reasonable variables to choose from are address history, sex, and race. I mandate that name and month of birth match exactly in order for a probabalistic match to be made and set the threshold for the (probabalastic) match at 0.93. If only name and month of birth are specified, close to 17% of observations that are matched are many-to-many. When adding address history, this drops to 0.4% out of the 79% of individuals that appear in the credit panel.[33] Adding sex and race cause the match rate to drop by 2% and 6%, respectively. This likely reflects poor imputation of both of these variables within the credit/payroll data, more than anything else. Consistent with Brevoort, Grimm and Kambara (2015) and Mello (2018), I find that the match rate is positively correlated with income per capita in the zip code of the registered address (in the DPS data):

---

[33]This match rate is fairly typical–good, even–considering the negative selection of individuals in the DPS data. That is, many likely never hold long-term full-time employment and have trouble establishing any credit.

Figure A.1: Match Rate by Zip Code Per Capita Income
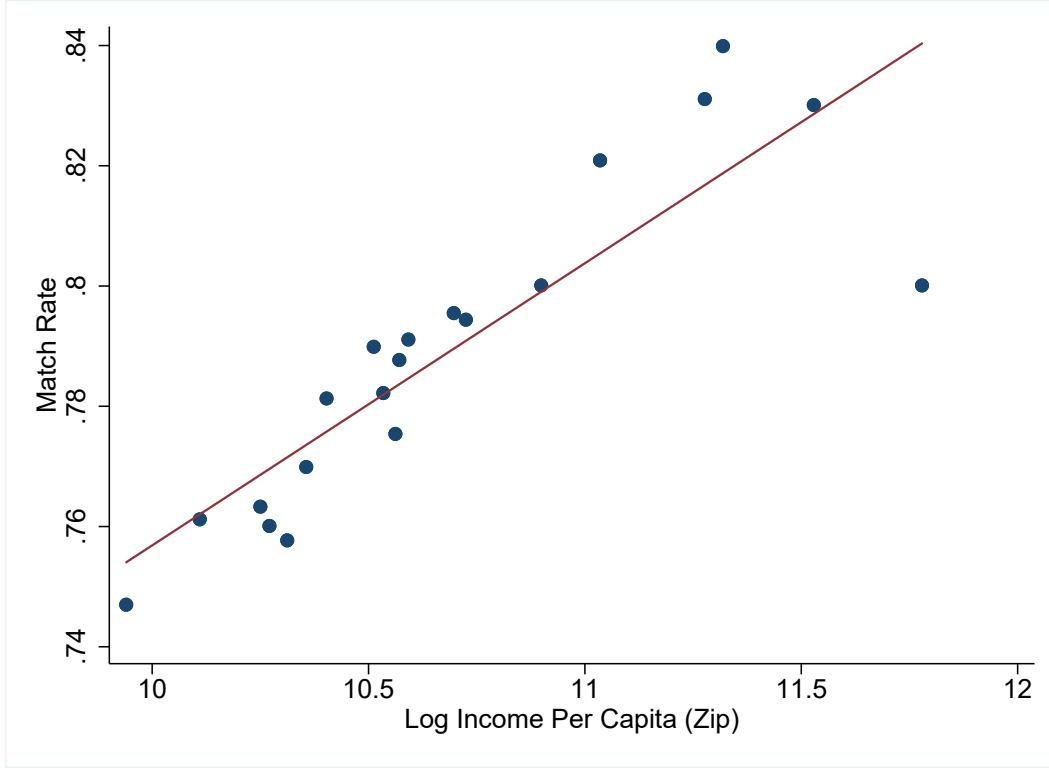
NOTES: This figure is simply binned match rates by zip code plotted against the log per capita income of the individual's zip code. The fitted line has a coefficient (standard error) of .046 (.006).

Similar to settings in other similar papers, individuals committing crimes are more disadvantaged on average (poorer and from more impoverished areas, as shown in Figure 7 and Table 4), but there is positive selection in terms of being matched to the payroll and credit data. If the treatment effects are larger for poorer individuals, the selection into this data should pull the estimates back towards zero.

A.2 Data Dictionary

This section briefly describes of a number of variables used in the analysis, as well as their construction if necessary. This is an attempt to make clear the construction or definition of any variable referenced in this paper.

1. **Month (Event Time):** The panel upon which most of the data is analyzed is structured as a month x year x firm panel of individuals (each uniquely identified). The "month" is not a calendar month, but instead a consistent month-time (roughly 30 days) relative to a layoff event *at that firm*. In this way, for the purpose of most regressions, all time within an ID relative to this event and is inconsistent across different individuals and across different firm-spells for any individual.

   Important to note is that an individual can serve as a stayer (control) for 1 layoff event and be

separated in a different layoff event, as long as it's at another firm. If a firm has multiple layoff events between 2000 and 2011, only the first event is considered. There are very few instances of this happening. In the same way, an individual can be observed being laid off in an earlier event and then serve as a stayer (control) at a later event. What is restricted, however, is if an individual is laid off at firm A and then later laid off at firm B; only the first instance of a layoff for that individual will be considered (that is, they are not in the regression; either as part of the control or treated group). Experiencing multiple layoff events across different firms is fairly rare and being part of the laid off group in both (or more) instances is even less common. Individuals that meet this criteria are, however, counted when I'm trying to determine whether a mass layoff has occurred. See definitions below for those restrictions.

2. **Separation (Layoff):** A separation, or layoff, event is defined with the following restrictions at baseline:

    (a) Employees at a firm are considered only those with at least 6 months of tenure prior to a layoff event.

    (b) Firm has at least 25 employees (with ≥ 6 months of tenure) at time of layoff event.

    (c) 20%-80% of employees that meet the tenure threshold are separated in a given month, where separation is identified by zero earnings in the *following* month.

    (d) Firm employment remains below pre-separation levels for at least one year and firm employment remains stable ($< 25\%$ variation in employment in either direction for 1 year). This is so that I don't misclassify firms that routinely hire for holiday seasons, or otherwise, as experiencing mass layoff events. This will also naturally omit firms where turnover is particularly high, if any are missed by the aforementioned tenure restrictions.

In practice, this is identified as follows. For each individual within a firm with 6 or more months of tenure, identify potential separation event by non-zero earnings followed by zero earnings (at that firm) for at least 1 year. If 20-80% of of individuals that meet the tenure threshold are identified as being separated within a month, then that firm is flagged as experiencing a mass layoff in that month. Note that a layoff event is not based on calendar time and so the "event time" is then coded as the date of the earliest of those individual separations. In this way, layoffs can still be staggered across a month. In this data, where a layoff event is identified, around 80% of events have 90% or more of individuals laid off within 2 weeks of one another. In addition, when I test alternative specifications that aggregate to the quarterly-level in Appendix B, results remain broadly similar. At a higher-level, events that are identified also only change slightly (16% more events are identified at the quarterly-level). While I chose these restrictions based

primarily on those in other papers, I do diverge slightly from prior work. In particular, I have much more precise earnings information than most prior work and so I choose to identify events at the month-level for this reason, while most other papers choose either the quarterly or annual-level to match their data structure. Because of this, I also choose a slightly wider range for proportion of total employees that must be laid off in order for an event to be identified. Most

prior work chooses a lower bound, for instance, that is closer to 30%. In other papers sutdying layoff events, tenure requirements are also typically set at 1-3 years and minimum employees (in a firm prior to an event) is typically closer to 100. Tenure requirements, in particular, are set lower than is typical because I can only measure tenure indirectly through an individual showing up in my data; that is, I don't have an individual's employment history prior to the first month of my data, or the first month that a firm begins appearing in my data. I show in Appendix B that flexing these restrictions does not substantively change my results, though I do lose a reasonable amount of power (particularly when increasing the tenure threshold given the nature of my data).

3. **Partial Separation:** A partial separation event is defined with the following restrictions at baseline:

   (a) Employees at a firm are considered only those with at least 6 months of tenure prior to a partial separation event.

   (b) Firm has at least 25 employees (with ≥ 6 months of tenure) at time of partial separation event.

   (c) Firm does *not* experience a layoff (full separation) event within 1 year of this event (in either direction).

   (d) $\geq 20\%$ of employees that meet the tenure threshold have their hours cut by 10 or more, but do not experience zero earnings in the subsequent month.

   This is identified similarly to layoff (full separation) events and the structure of the panel is the same. Figure F.4 shows what the distribution of these cuts look like, among individuals in firms that experience partial separation events.

4. **Hours:** For the most part, this is not constructed by me but is instead present in the payroll data. In cases where hours are not present, but earnings are cut (in reference to partial separations), I assume an individual's wage remained the same and that their hours worked prior to the earnings cut was 40 as long as the individual only appeared to be working for 1 firm. For instance, if an individual earned \$3000 in March and is only working for 1 firm, and is then earning \$1500 in April while still being employed at that same firm (alone), I assume that they were working 40 hours in March and 20 hours in April. Again, this type of construction is only necessary for a subset of the data and is only relevant to the partial separation results.

5. **Offense:** Indicator for being charged or arrested. Throughout the paper, and in the construction of the panel, this is tied to a period (a month, based on event time and relative to the date of the separation event). In most cases, multiple offenses within the same month will simply be identified as an indicator for offense in that month. Source: DPS.

6. **Reoffense:** Indicator for being charged or arrested again within t days of either release from prison or, if no incarceration was assigned, the date of sentencing. Source: DPS.

7. **Violent Crime:** These are mapped directly from the FBI's Uniform Crime Reporting (UCR) Data. These are defined as crimes that "involve force or threat of force."

8. **Economic Crime:** Also referred to as an "economically-motivated crime" in the text. Crime of this type are broadly property crimes, drug sales, intent to sell, fraud, bribery, counterfeiting, etc. While the mapping is not directly from the UCR for this type of crime, the litmus test for the charges is generally, "is there a potential economic benefit to committing the crime?" A full map between Texas charge codes and this indicator will be provided separately.

9. **Prior:** Indicator for existence of a prior conviction. Source: DPS.

10. **Indigence:** Indicator for whether an defendant's counsel is flagged as either a public defender or court-appointed attorney. While this is a useful proxy for poverty, the court data is not as complete as one might want and information like this is sparse. See Shem-Tov (2020) to see why type of counsel may matter considerably for court outcomes. Source: DPS.

11. **Collections Accounts Balance:** This balance mostly reflects unpaid bills that were sent to third-party collection agencies. The majority of these bills are associated with medical and utility bills (Avery et al. (2003)). Balances are topcoded, though the topcoding decisions are relatively ambiguous, as the topcoding appears to be $10,000 for the majority of the data but is topcoded at lower thresholds in some instances (made clear by the fact that an individual's instances of derogatory events continue evolving, but their accounts balance remains at, e.g., $9990.). Source: Credit Panel.

12. **Major Derogatory Events:** Instances of reposessions, foreclosures, bankruptcies, charge-offs, etc. Number of accounts with major derogatory events to date is used as the measure of "major derogatory events". I primarily follow Mello (2018) in this definition, as one could conceive of alternative ways of aggregating the events or events across accounts. Source: Credit Panel.

13. **Delinquency:** Number of accounts ever past 90 days due. Source: Credit Panel.

14. **Estimated Income (Credit):** Within the credit data, there exists an estimated income measure, which is highly correlated with earnings in the payroll data ($\rho \approx .89$). While I could conceivably use this to estimate changes for those outside my sample, the actual construction of the earnings estimate is a blackbox. I include this here just to make clear that any estimates on changes in income (and even identifying those experiencing layoff events) are conducted solely using the payroll panel. Source: Credit Panel.

15. **Local (Un)employment:** Throughout the paper, local unemployment (or something similar to it) is used in 3 different ways, which are sourced from 3 locations:

    (a) Yagan (2019) provides measures of how exposed different commuting zones (CZs) are to the great recession. These are essentially constructed from changes local unemployment rates in these areas. The measure is time-invariant (measured one time as the difference) and aggregated to the CZ-level. This measure is used to construct the subsamples in panel C of Table 4.

(b) Table 5 uses two measures for local unemployment at time of separation to instrument for an individual's own unemployment duration. Description of these measures is describes more completely in subsection 4.1, but they are constructed as:

    i. $D_{k0} :=$ the current amount of time (in months) that k has been unemployed at the time of i's layoff, where each k is someone within CZ j who has previously experienced a layoff event. This is constructed entirely from the payroll data and could alternatively be defined across smaller markets (say, counties or MSAs). The domain for k could also be expanded to individuals who were previously separated; even outside of layoff events. Neither choice significantly affects the results in Table 5.

    ii. subsection 4.1 also uses local unemployment at time of separation as an instrument, where local unemployment is drawn from the BLS Local Area Unemployment Statistics (LAUS). See https://www.bls.gov/lau/lauov.htm.

B Robustness

In the procedure outlined in section 3, there are a number of choice parameters that might influence estimation. Below, I broadly list the full scope of possible choices that could be made in this regard (with actual choices made listed as the first of the possible choices):

Individual Restrictions:

1. Length of tenure prior to separation event:

    (a) ≥ 6 months of tenure prior to separation event

    (b) ≥ 12 months " "

    (c) ≥ 24 months " "

2. Age at event:

    (a) 20-60

    (b) 18-65

3. Credit Information:

    (a) Matched to credit panel

    (b) All, regardless of match

Firm Restrictions:

1. Number of employees at event:

    (a) ≥ 25

    (b) ≥ 10

    (c) ≥ 100

2. Number of employees at firms that don't experience displacement (count as:)

    (a) Average across horizon

    (b) Maximum number of employees across horizon

3. Firm Deaths:

    (a) Firms are allowed to die (close or leave sample)

    (b) Firms that die, or otherwise leave the sample, are excluded from control

Layoff Event Definition:

1. Proportion laid off (zero earnings):

    (a) 20%-80%

    (b) 30%-90%

  2. Post-event employment variation

    (a) $< 25\%$ employment variation in the year following possible event

    (b) $< 30\%$ " "

    (c) $< 50\%$ " "

Control Group:

1. All individuals not displaced at the same time

2. All individuals not displaced at the same time within the same firm (stayers)

3. Matched sample of those not displaced at the same time

4. Separated individual prior to their displacement (standard event study)

Horizon (months):

1. $[-6, 6]$

2. $[-6, 12]$

3. $[-12, 12]$

4. $[-12, 24]$

Time Aggregation:

1. Monthly

2. Quarterly

3. Annually

Interacting all possible choices with all outcomes of interest would generate far too many estimates to list succinctly. As such, Table B.1 takes the 1 year cumulative crime effect ($Y_{it} := \mathbb{1}\{i \text{ offends by period } t\}$) as the estimate of interest and iterates across many of the possible choices made above. For clarity, the estimates are the same as in Table 3 when ≥ 6 months tenure and either ≥ 25 employees or 20%-80% separated are specified. The number of separation events identified drops from 6,356 under my preferred specifcation to 922 under the strictest combination of choices (≥ 100 employees, 30%-90% separated, and ≥ 24 months of tenure). A large number of lost events are due to the fairly short horizon in my data (2000-2011) and how I construct tenure (see Appendix A). The number of young male separators similarly drops from 99,201 to 41,224. Regardless, estimates in Table B.1 don't seem to move very much across the different combinations of these choices, which is reassuring.

In my mind, the only "very" important choice missing from the Table B.1 is choice of time aggregation since, beyond slight changes in point estimation, it could significantly change which layoff events are flagged. That is, it's possible that if layoffs tend to be staggered in nature (say, over the course of a couple of months), I am missing many potential layoff events by aggregating at the month-level. Importantly, without flexing any of the other restrictions, every event that is identified under the monthly time aggregation will be similarly identified under the quarterly and annual aggregations. From these tables, one significant thing of note does arise: broader aggregation and stronger firm restrictions (on number of employees) seems to pull estimates closer to those of prior works. From the change in number of separators and stayers relative to events in Table B.2, it's clear that both choices are causing me to miss layoff events at large firms, which appear to be more staggered in nature. The fact that estimates are pulled downward under these new restrictions This is consistent with the idea that firm size is a good proxy for firm (and job) quality (see Abowd, Kramarz and Margolis (1999)). If separators that I am looking at have worse outside options due solely to their current job (firm), then their unemployment durations are likely exacerbated which would further drive crime responses above and beyond the displacement itself.

Table B.1: Robustness: 1-Year Cumulative Crime Effect

	Control: All Non-Separators Within Separated Firm				
	≥ 10 Emp	≥ 25 Emp	≥ 100 Emp	20%-80% Separated	30%-90% Separated
≥ 6 Months Tenure	0.051***	0.056***	0.046***	0.056***	0.051***
	(0.010)	(0.010)	(0.015)	(0.010)	(0.014)
≥ 12 Months Tenure	0.053***	0.054***	0.042***	0.056***	0.049***
	(0.011)	(0.011)	(0.013)	(0.011)	(0.013)
≥ 24 Months Tenure	0.051***	0.049***	0.043***	0.051***	0.048***
	(0.016)	(0.015)	(0.016)	(0.013)	(0.015)
	Control: All Non-Separators				
	≥ 10 Emp	≥ 25 Emp	≥ 100 Emp	20%-80% Separated	30%-90% Separated
≥ 6 Months Tenure	0.046***	0.046***	0.039***	0.049***	0.049***
	(0.011)	(0.009)	(0.009)	(0.011)	(0.012)
≥ 12 Months Tenure	0.047***	0.047***	0.037***	0.039***	0.053***
	(0.011)	(0.010)	(0.010)	(0.011)	(0.014)
≥ 24 Months Tenure	0.046***	0.044***	0.037***	0.042***	0.051***
	(0.013)	(0.013)	(0.014)	(0.015)	(0.015)
	Control: Matched Sample				
	≥ 10 Emp	≥ 25 Emp	≥ 100 Emp	20%-80% Separated	30%-90% Separated
≥ 6 Months Tenure	0.039***	0.039***	0.041***	0.044***	0.047***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)
≥ 12 Months Tenure	0.036***	0.033***	0.039***	0.039***	0.046***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)
≥ 24 Months Tenure	0.033***	0.032***	0.036***	0.040***	0.040***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
	Control: Separator (Event Study)				
	≥ 10 Emp	≥ 25 Emp	≥ 100 Emp	20%-80% Separated	30%-90% Separated
≥ 6 Months Tenure	0.053***	0.058***	0.043***	0.056***	0.054***
	(0.012)	(0.012)	(0.013)	(0.012)	(0.014)
≥ 12 Months Tenure	0.053***	0.058***	0.041***	0.054***	0.052***
	(0.012)	(0.013)	(0.015)	(0.014)	(0.017)
≥ 24 Months Tenure	0.048***	0.046***	0.040***	0.048***	0.044**
	(0.013)	(0.013)	(0.015)	(0.016)	(0.019)

NOTES: Each coefficient (and standard error) in panel A is estimated by Equation 3. Estimation in panels B & C is estimated separately by:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \in K} D_{it}^k \cdot \delta_k + \varepsilon_{it}$$

where $Y_{it} = \mathbb{1}\{i$ charged with crime by $t\}$ and $D_{it}^k \coloneqq \mathbb{1}\{i$ displaced k periods before $t\}$. The definition of "displaced," however, is changing across each estimate with each panel based on the discussion of choice parameters above. Estimates of δ_k are being reported with $k = 12$. Standard errors are clustered at the employee-firm level and are shown in parentheses (*$p < .1$, **$p < .05$, ***$p < .01$).

Table B.2: Time Aggregation

	Month	Quarter	Year
Count			
Number of Events	6,356	6,985	7,205
Number of Separators	99,201	118,733	150,229
Number of Stayers	210,860	247,751	290,256
1-Year Crime Effects			
All Charges	0.056***	0.046***	0.043***
	(0.010)	(0.009)	(0.009)
Economic Crime	0.035***	0.029***	0.028***
	(0.008)	(0.008)	(0.008)
Violent Crime	0.017**	0.013*	0.013*
	(0.008)	(0.007)	(0.007)

NOTES: This first part of this table shows how number of events, separators, and stayers (within firm that experiences a layoff) changes as the definition of a layoff event gets broader. The same restrictions are held across the 3 columns, with the only thing changing being the horizon across which I search in order to define a layoff event (see subsection A.2). The crime effects are estimated by:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \in K} D_{it}^k \cdot \delta_k + \varepsilon_{it}$$

where $Y_{it} = \mathbb{1}\{i$ charged with (any/economic/violent) crime by $t\}$ and $D_{it}^k := \mathbb{1}\{i$ displaced k periods before $t\}$. t is changing across the 3 columns, with the panel being unique at the individual-month level in the first column, the individual-quarter level in the second column, and the individual-year column in the third column. Estimates of δ_k are reported with $k = 12$ in the first column, $k = 4$ in the second column, and $k = 1$ in the third column (each representing effects roughly 1-year post-layoff event). Standard errors are clustered at the employee-firm level and are shown in parentheses ($^*p < .1$, $^{**}p < .05$, $^{***}p < .01$).

C   Analogous Results for Low-Risk Population

Before showing all of the analogous results, I begin with the 1-year cumulative crime effect across the age profile for young men. Figure C.2 shows coefficients from 16 separate regresssions that are comparing separators that are men aged X to stayers that are men aged 20-35, inclusive. I present this result simply as additional motivation for the "high-risk" demographic presented in the majority of the paper.

Figure C.2: Separators vs Stayers: 1-Year Crime Outcome by Age (Young Men)
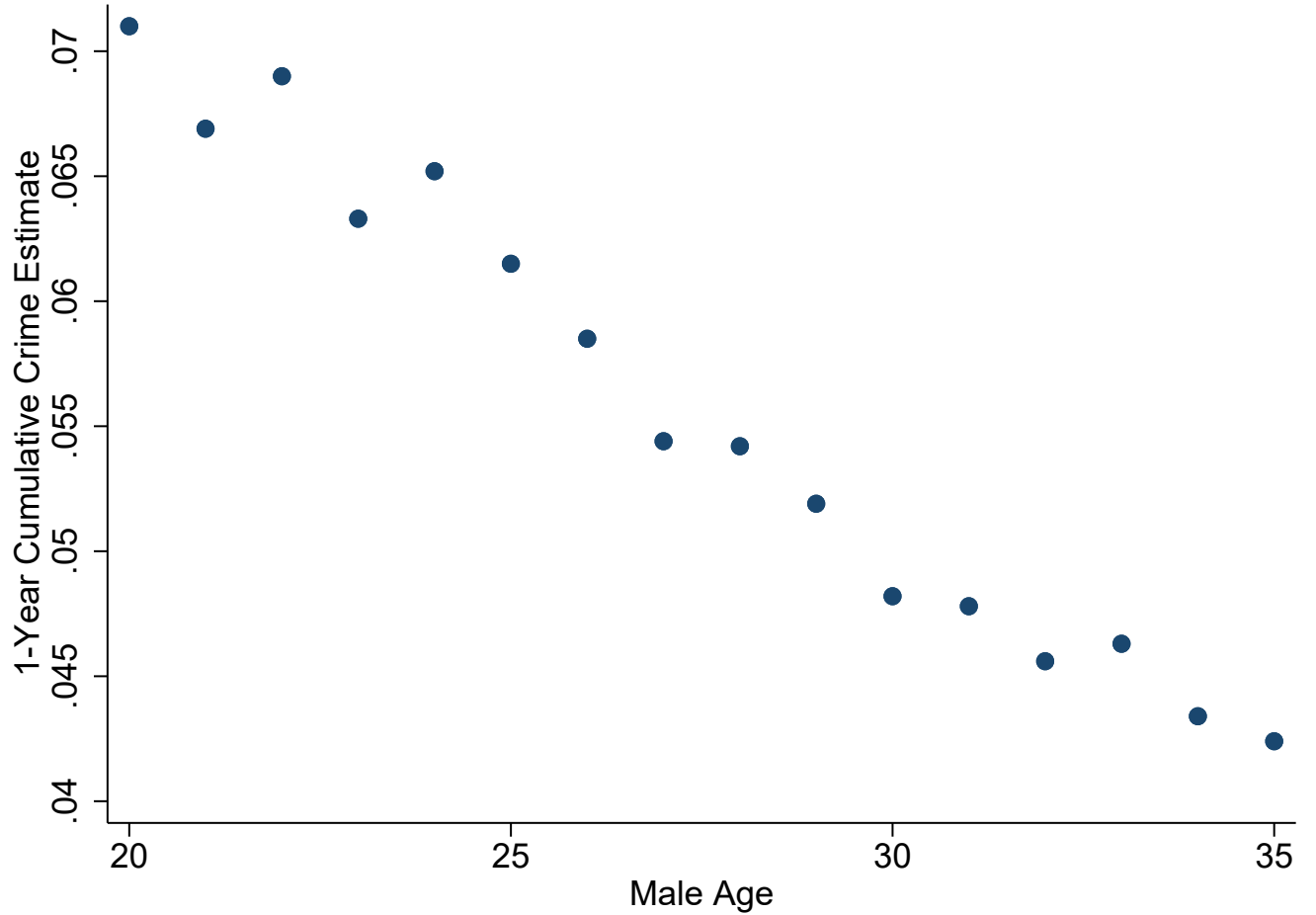
NOTES: This figure is estimated by:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{a \in A}[\sum_{k \in K} \cdot D_{it}^k \cdot \delta_{a,k}] + u_{it}$$

where $A = \{20, 21, \ldots, 34, 35\}$ is the age of i at time of full separation and $D_{it}^k \coloneqq \mathbb{1}\{i \text{ displaced } k \text{ periods before } t\}$. The 16 coefficients shown are the set $\{\delta_{a,k}\}$ from this estimating equation at $k = 12$.

We can also look at the 1-year cumulative effect across the age profile for both men and women, which I present below. Important to note regarding Figure C.3 is that the comparison group changes when compared to the previous figure. Specifically, the comparison group for men, across the age profile, is constructed as all men that are not displaced at time of separation, which amplifies the impact seen among young men when comparing coefficients to those in Figure C.2. For women, the control group is all women not displaced at time of separation. Negative JLS coefficients can be rationalized by noise, poor control selection as I iterate across the age profile, deaths caused by separation, or some combination of these things.

Figure C.3: Separators vs Stayers: 1-Year Crime Outcome by Age
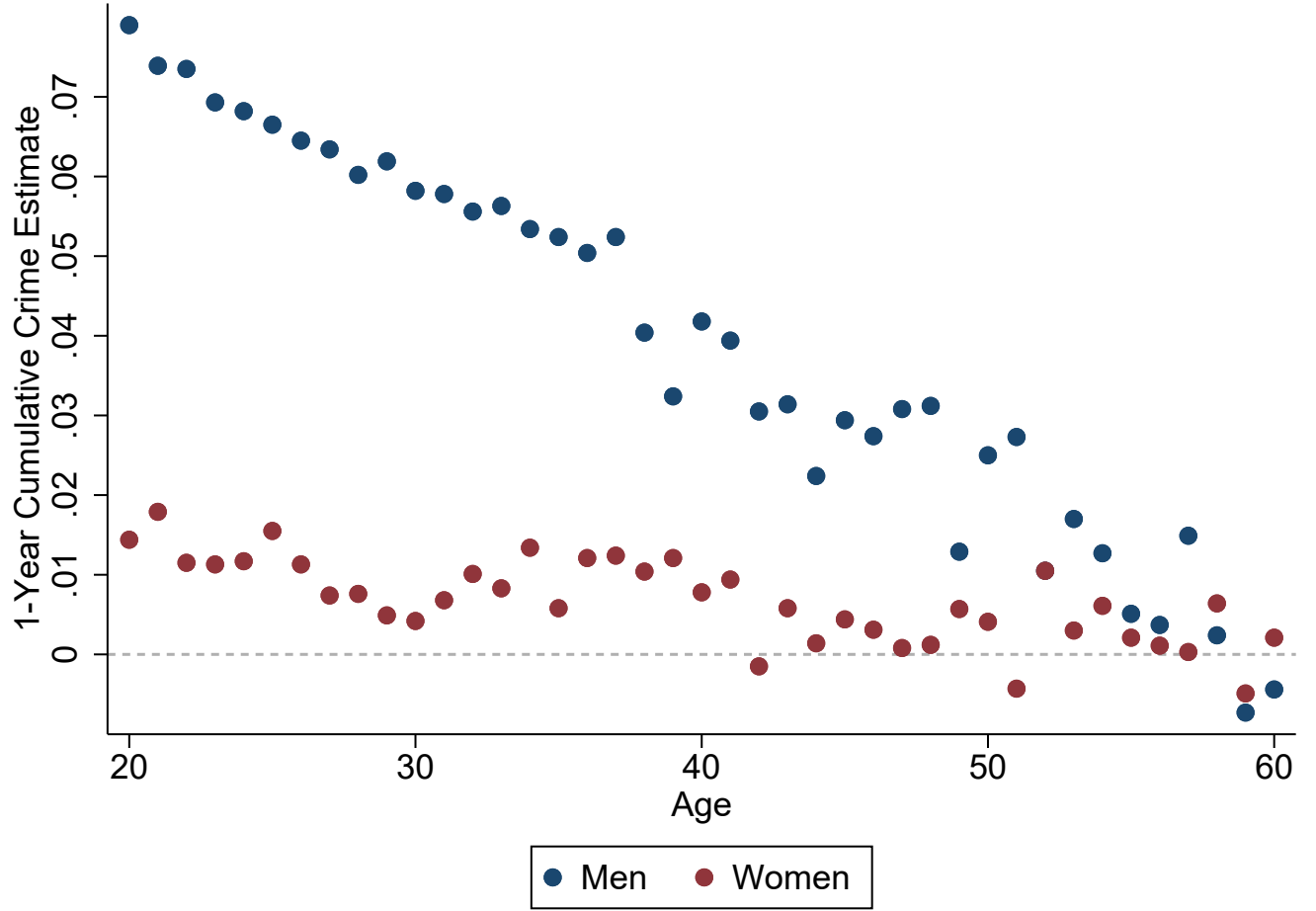
NOTES: This figure is estimated by:

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{a \in A}[\sum_{k \in K} \cdot D_{it}^k \cdot \delta_{a,k}] + u_{it}$$

where $A = \{(20, \text{male}), (21, \text{male}), \dots, (60, \text{male}), (20, \text{female}), (21, \text{female}), \dots, (60, \text{female})\}$ is the age and sex of i at time of full separation, and $D_{it}^k := \mathbb{1}\{i \text{ displaced } k \text{ periods before } t\}$. The 82 coefficients shown are the set $\{\delta_{a,k}\}$ from this estimating equation at $k = 12$.

D   A Model of Criminal Choice

Analysis in this paper focused primarily on studying the effects of job loss on reoffending identified by variation induced by mass layoff events. Here, I present a model similar in nature to the one proposed by Becker (1968) and extended by Ehrlich (1973). Extension of the standard Becker-Ehrlich framework is made in attempt to determine the extent to which a model of this kind can rationalize the results that are borne out in the data.

D.1   Basic Model Setup

The basic setup of this model and estimation follows recent work, such as Chalfin and McCrary (2018) and Macleod and Rivera (2022). Consider individual i, with general skill type $\theta_i \in \Theta$, who is choosing how much labor to provide to either legitimate activity (L_i), illegitimate activity (l_i). The legitimate activity provides wage $W(\theta_i)$, while the illegitimate one provides w.[34] Let $t_i = t$ be a public transfer to individual i if they are not currently working in the formal labor market. Then individual i's level of consumption will be given by:

$$c_i = W_i L_i + w_i l_i + t_i \tag{9}$$

Individual i chooses how much labor to supply to each activity, in order to maximize utility of the form:

$$U_i(c_i, L_i, l_i) = u_i(c_i) - V_i(L_i, l_i) \tag{10}$$

where u_i is assumed to be twice differentiable, $u' > 0$, $u'' \leq 0$ and $c \in [0, \infty)$. $V \geq 0$ is our disutility of labor function and is also assumed to be twice differentiable with $V' > 0$, $V'' > 0$, and $V''' \leq 0$ for all $L, l \geq 0$. Further assume that $V(\vec{0}) = V'(\vec{0}) = 0$. If $l_i > 0$, define the probability of detection is p and the cost is d. In order to pin down choices, assume that $l_i + L_i \leq H$ for some $H > 0$ in every period and that $\mathbf{E}[w(p,d)] < W_i \ \forall i$.[35] With this setup, an individual is choosing $\{l_i, L_i\}$ subject to their wage draw (W_i), the value of committing crime (w, p, and d), transfers (t_i), and their own disutility of labor (V_i).

To illustrate how this might operate in a simple setting, consider a two-period model where, in period 0, each individual initially draws from skill distribution Θ which dictates their formal market wage W_i, and provides formal market labor corresponding to their draw. For further simplicity, assume that saving (and borrowing) is not possible and that consumption must meet some minimal level of acceptable consumption $\underline{c}_i$. Before period 1, these individuals are shocked such that their formal market labor in period 1 is 0 (and restricted, as such). They now maximize c_i by choosing l_i subject to w, t_i, and V_i. In this simple form, $l_i > 0$ will be the optimal choice if $w > 0$, $V_i(l_i)$ sufficiently small, and $t_i < \underline{c}_i$. That is, individuals will choose to commit crime following a layoff if they cannot immediately resecure employment and their safety net (t_i) does not allow them to meet some minimum threshold. This is, of course, under a number of simplifying assumptions; particularly, that borrowing and saving are restricted and that re-employment is not something that can be influenced (e.g. through search effort).

D.2   Parametric Assumptions and Estimation

In order to make estimation feasible, I need to impose more parametric assumptions and a few other modifications to match what I can observe in the data. First, I simplify l_i such that the choice to commit crime is binary in each period: $l_i \in \{0, 1\}$. I assume effort required to commit this crime (e_l) instead represents the number of hours put forth if $l_i = 1$ (with the same restriction on time; $e_i + L_i \leq H$ for some $H > 0$) and that w is now just the payoff if $l_i = 1$ (rather than the per-unit wage). Further assume that $L_i \in \{0, 1\}$ is the choice to work in the formal market for earnings (rather than wage) W_i. Under the current structure, there is uncertainty in an individual's utility in each period only if they

---

[34]One could make the illegitimate wage individual-specific, but it then obscures differences between skilled and unskilled (formal market) laborers in their motivation to commit crime. One could also add search effort as a choice parameter, but draws from stable distributions produce the same general results (and I can't actually observe anything like search effort that would distinguish outcomes from random draws).

[35]The specific bound on (formal and informal) labor supply also doesn't particularly matter as long as one is set.

are choosing to commit crime:

$$\mathbf{E}[U_i(c_i, L_i, l_i)] = \underbrace{U_i(c_i, L_i, 0)}_{\text{No Crime}} + \underbrace{pU_i(c_i, L_i, 1)}_{\text{Crime detected}} + \underbrace{(1-p)U_i(c_i, L_i, 1)}_{\text{Crime not detected}} \tag{11}$$

Following Lee and McCrary (2017), I also assume that individuals have a per-month discount rate equal to δ, their cost of detection is captured by the risk of incarceration (I discrete and distributed with probability function $\{\pi\}$) which inhibits their ability to draw a formal market wage, and that utility takes Stone-Geary form:

$$U_i(c) = log(c - \underline{c})$$

Such that this does not predict that every individual that loses employment *must* commit crime, I allow individuals to borrow and save intertemporally to insure themselves against negative shocks. Let s_{it} represent the amount saved (not consumed) *by* period t. With discrete time, each individual i is then maximizing:

$$\sum_{t \in T} \mathbf{E}_t[U_{it}(c_{it}, L_{it}, l_{it})] \tag{12}$$

with respect to $\{c_{it}, l_{it}\}$, subject to:

1. $c_{it} \leq W_{it}L_{it} + wl_{it} + t_{it} + s_{i,t-1}$ (consumption constraint)

2. L_{it} (whether they have an offer in period t; this is taken as exogenous)

3. W_i (wage based on draw from Θ)

4. $c_{it} > \underline{c} \;\forall t$ (minimum consumption level)

5. $s_{it} = \sum_{j=1}^{t} W_{ij}L_{ij} + wl_{ij} + t_{ij} - c_{ij}$ and $s_{i0} = 0$ (evolution of savings/borrowing)

6. $t_{it} = t \cdot \mathbb{1}\{L_{it} = 0\}$ (transfer if unemployed)

7. $s_{it} \geq \underline{s} \;\forall t$ (cumulative borrowing constraint for some $\underline{s} \leq 0$)

8. $e_{l,it} + L_{it} \leq H \;\forall t$ for some $H > 0$ (time constraint)

9. $w + t \geq \underline{c}$ (crime under unemployment will sustain an individual)

Given the lack of labor market interactions (everyone is offered a wage W_i) and the restraint imposed on w ($\mathbf{E}[w(p,d)] < W_i \;\forall i$), it should be unsurprising that *nobody* that is employed should commit crime under the current formulation of the model. I propose modifications to match the experiment in this paper. Let W_i' be redrawn from a different distribution (Ω) if an individual is displaced with poisson arrival rate λ in each period and λ_{pr} if an individual has a prior. Relax the assumption that $E[w(p,d)] < W_i \;\forall i$ and instead assume that $w = P_x(F(W))$; that is w (not considering the cost or probability of detection) takes value relative to some point in the formal labor market wage distribution (where $x \in (0,1)$ gives the value it takes from the CDF of W ($F(W)$)).

Under this formulation, the individual decision is governed by their ability to borrow, their minimum consumption level, their expected future earnings, and their expected payoff from crime (including the potential cost of delaying their formal market earnings if they are incarcerated). Many of the same comparative statics arise from this extension as under the standard Becker-Ehrlich framework: crime is decreasing in the probability of detection (p), decreasing in the cost of detection (I), decreasing in income (W_i), etc. Under this extension, however, crime is also increasing in the minimum level of consumption ($\underline{c}$), decreasing in savings (s_{it}), decreasing in labor market reattachment rates (λ and λ_{pr}), etc.

Estimation of an individual's "rational" choice following separation under this model proceeds as follows. W_i will be taken as i's earnings in period -1 (the period prior to separation). For individual i, in each subsequent period t:

1. If not incarcerated:

(a) Calculate the expected value of abstaining from crime in period t determined by $E[W_i']$, λ (or λ_{pr}), δ, t, c_{it}, and $\underline{c}$

(b) Calculate the expected value of commiting a crime determined by $E[w]$, p, $E[I]$, δ, t, $\lambda - \lambda_{pr}$, c_{it}, s_{it} and $\underline{c}$

   - c_{it} under each of these choices is taken as total earnings plus the change in credit extended (if any) and benefits. This entirely dictates per-period utility (given $\underline{c}$).

(c) If (a) provides a consumption level less than $\underline{c}$, then crime is predicted in period t. This occurs if an individual has no established credit (rare), if they experience a major derogatory event, or if they have collections accounts exceeding a derogatory event prediction rate of r based on $\vec{X}$.

   - Here, r will be a choice parameter and $\vec{X}$ is a characteristic vector including age, gender, prior formal labor market tenure duration, and earnings.

(d) If (a) < (b), then crime is predicted in period t.

(e) If (c) or (d) (crime predicted) and not detected, then proceed to the next period under no incarceration restriction or change of parameters.

(f) If (c) or (d) (crime predicted) and detected (individual charged in the data), then:

   i. Take actual realization of I from the data; that is, is an individual incarcerated and for how long.

   ii. $\lambda \to \lambda_{pr}$ upon release if individual didn't previously have a prior

      - If $I = 0$ (no incarceration was ordered), then $lambda_{pr}$ is the only realized cost of being detected.

   iii. For subsequent periods, look to condition 2. below.

2. If incarcerated:

   - $\theta = (c_{it} - \underline{c}|\text{incarceration})$ is another choice parameter that is capturing the cost of detection (in conjunction with I)

   - No other parameters are necessary for estimation in combination with the conditions in 1.

This procedure will predict whether an individual commits a crime in period t using realized information from period $t-1$ subject to a number of parameters. Choice parameters that impact estimation above are $[p, \delta, w, E[W_i'], \lambda, \lambda_{pr}, E[I], \underline{c}, t_i, r, \theta]$. The data informs choice of parameter values for some within this set ($E[W_i']$, λ, λ_{pr}, $E[I]$), but not others (p, δ, w, $\underline{c}$, t, r, θ). Choosing values as:

- $p = .08$
- $\delta = .95^{1/12}$
- $w = P_{.1}(F(W))$
- $E[W_i'] = .83W_i$
- $\lambda = .2$
- $\lambda_{pr} = .13$
- $E[I] = 7$
- $\underline{c}$ tied to moving poverty line in Texas
- t_i calculated as maximum claimable unemployment benefits (based on $W_{i,-1}$)
- $r = .5$
- $\theta = 100$

Looking 1-year out from separation, 51% of individuals who commited an economic crime following separation is also predicted to have made this choice rationally under this model. Of the 99,201 young male separators that this procedure was run on, 4357 additional separators were predicted to offend in the year following separation but did not in the data. While both types of errors (under-predicting and over-predicting) could be due to model structure or parameter tuning, there are unobservables that can also explain these mistakes. If we think this model is close to the true decision process that dictates economic crime, under-prediction could be due to unobserved benefits claims (wherein I assume

that everyone is eligible). Over-prediction, on the other hand, could be driven by unobserved personal endowments or secondary income. Alternatively, these predictions could be "corrrect," in that the individual did commit a crime, but it was not detected.

Finally, one might ask what set of parameters maximize the probability of a correct prediction or minimize the probability of incorrect prediction. It's easy to see that some parameter choices optimize along one dimension without regard to the other. For instance, rationalizing more of the realized economic crime response is easy to do if I increase $\underline{c}$. If the minimum consumption level is set close to consumption prior to separation, rather than anchored to some outside point (the poverty level, in this case), I will (correctly and incorrectly) predict that many more separators will commit crime. Increasing wage under crime (w) or decreasing the probability of detection (p) both have similar effects. As such, we might seek to select parameters under a criterion that simultaneously maximizes the probability of detection and minimizes the probability of a type I error. To that aim, I iterate across the set of parameters that are not informed directly by the data: $[p, \delta, w, \underline{c}, \theta]$. Other parameter values are fixed, as above. Using grid search, one set that (locally) optimizes the criterion (set as the difference between correct predictions and incorrect predictions) is $[.131, .963^{1/12}, P_{.03(F(W))}, .73 \cdot (\text{Pov Line}), 22]$, which correctly predicts 47% of the realized crimes, but only mispredicts an additional $\sim 16\%$.

E   Separations vs Closures

The primary reason for using separations as the natural experiment in this setting is that closures can be misidentified in my data. In particular, since I am identifying employee or firm changes using the payroll data, it's possible (and not terribly uncommon) for firms to cease using the software from these management firms. It's not possible to disentangle closure from changes in that firm's payroll management procedure (if that change dictates thta they leave my data). Regardless, it's possible to show that events identified as "firm closures" in this data are not distinct from separation events in in their effect on re-employment earnings, credit, and crime.

As with layoff events, there are a few ways to construct the comparison: separated employees compared to those in a stable firm, separators compared to themselves post-event, and separators compared to a matched sample of individuals in stable firms.[36] As a simple approach, I will just use a two-way fixed effects estimation procedure that is comparing differences between employees at a "closed firm" and those at a matched "stable firm," where the match is made based on firm characteristics one year prior to closure (for the closed firm). These firms will be matched based on firm age (within my data), firm size, commuting zone, average earnings of employees, and broad industry code.[37] Call

---

[36]Unfortunately, the comparison of separators to stayers within firm cannot be made since the event itself is separating all individuals within a firm.

[37]These are not NAICS codes (or any standardized set of codes) in my data, but they are similar to 2- and 4-digit NAICS codes in terms of aggregation.

F Miscellaneous Figures and Tables

Table F.3: Test of Random Separation Within Firm

| | *Dependent Variable:* | | | |
Explanatory Variable:	$\mathbb{1}${**Crime within 1 Year**}		$\mathbb{1}${**Laid Off**}	
	Coefficient	Standard Error	Coefficient	Standard Error
Demographics				
Age at Event	-0.007***	(0.001)	-0.027	(0.063)
Male	0.019***	(0.003)	0.006	(0.006)
White	-0.004**	(0.002)	-0.003	(0.007)
Other				
Bottom 25% Credit	0.010***	(0.004)	0.018*	(0.012)
Bottom 25% Income	0.005	(0.004)	-0.006	(0.012)
Prior Conviction	0.023***	(0.005)	0.007	(0.017)
Tenure (months)	-0.003	(0.004)	-0.001	(0.009)
F-Stat	122.13		1.37	

NOTES: This shows a simple test of random assignment of separation within firm. All estimations include controls for firm x county x year effects. Standard errors are two-way clustered at the individual and firm-level and are shown in parentheses ($^*p < .1$, $^{**}p < .05$, $^{***}p < .01$).

Figure F.4: Partial Separations: Hours Cuts
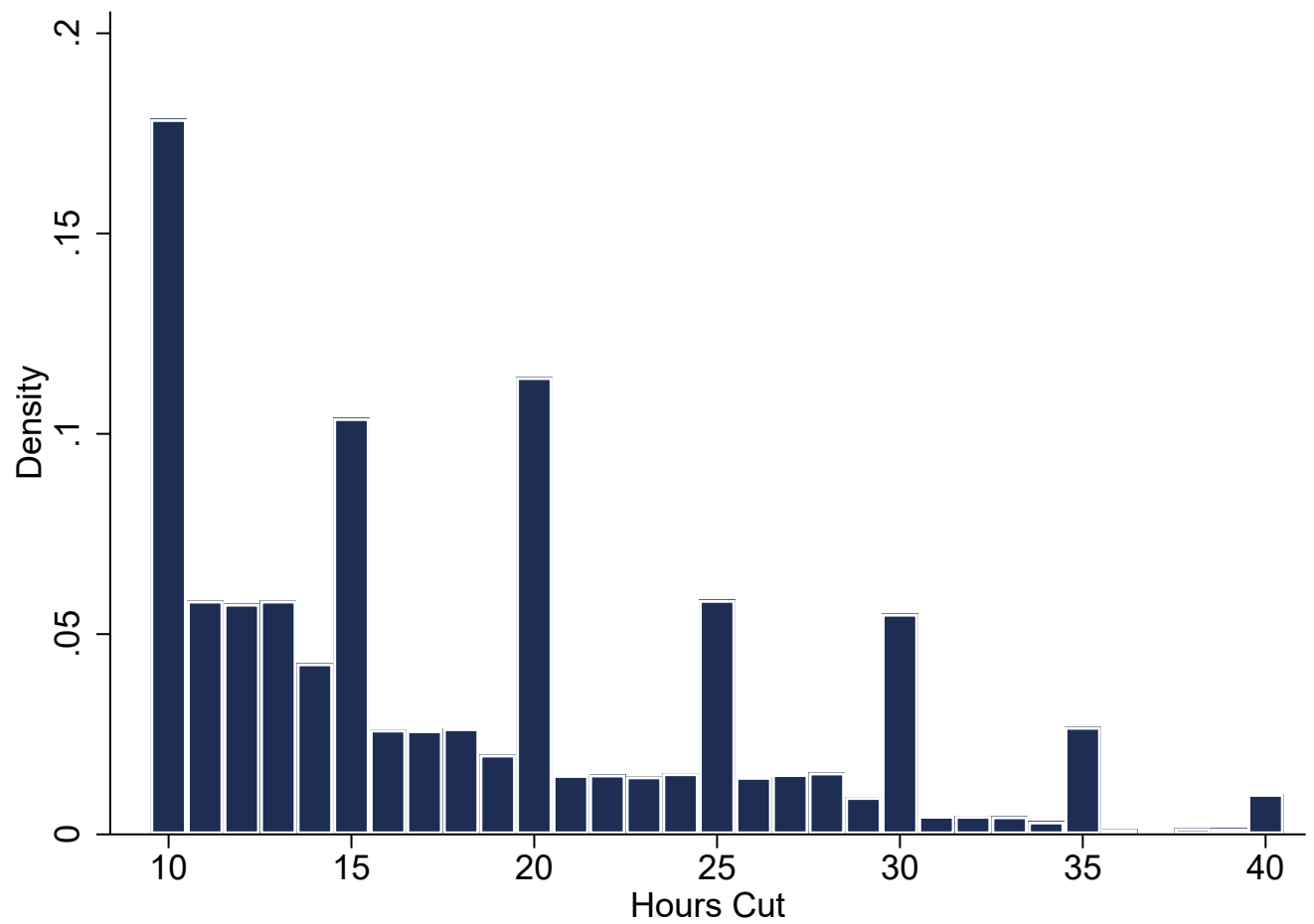
NOTES: This figure is a simple histogram of average hours cut among those with hours cut within a firm that experiences a partial separation event (as defined in subsection 3.3).

Figure F.5: Separations over Time
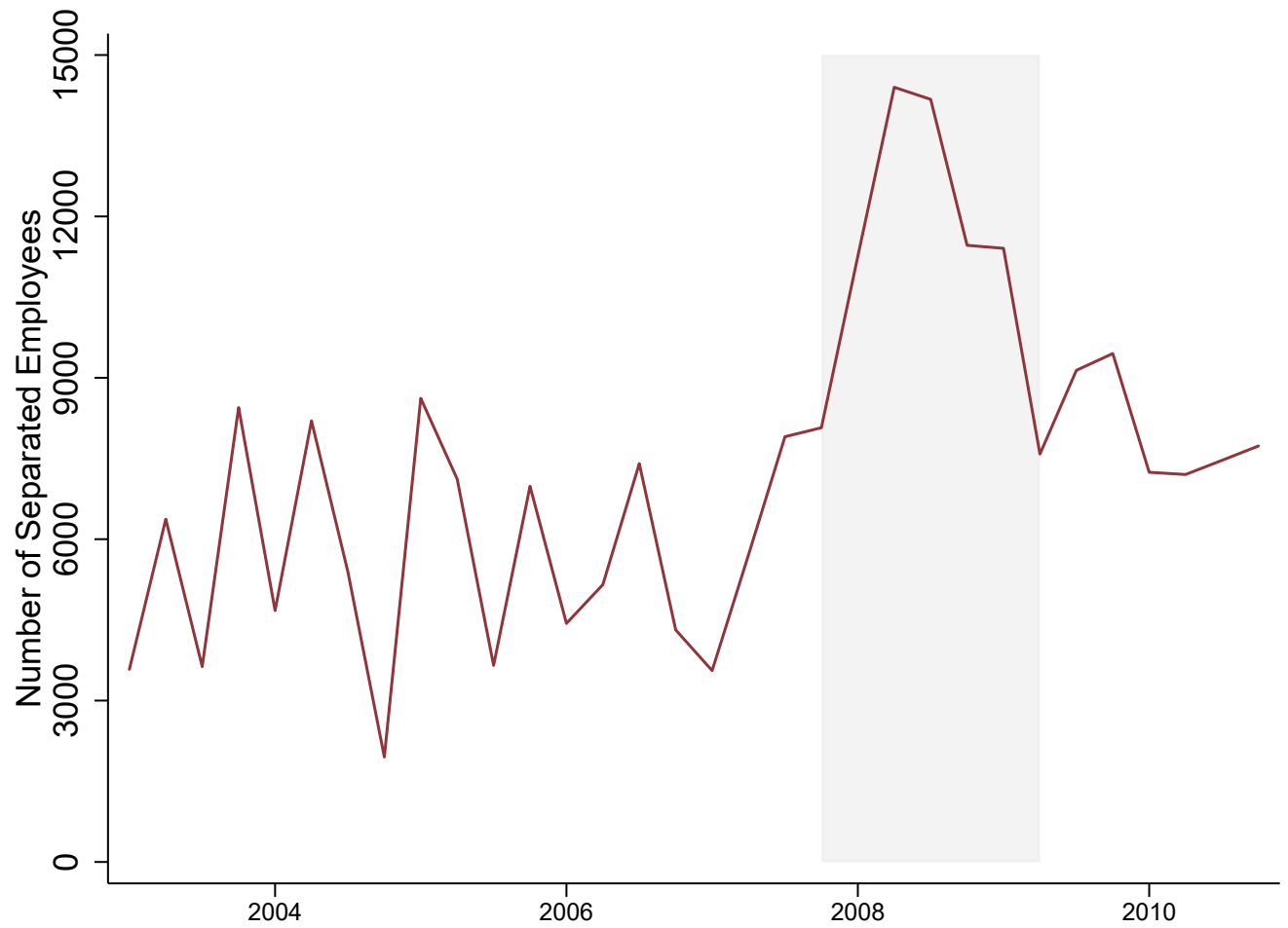
NOTES: This figure plots number of full separations by quarter. The shaded region is (roughly) the duration of the 2007-2009 recession, which naturally resulted in a higher number of separations (and events).

Figure F.6: Goodman-Bacon Decomposition - Separation

NOTES: This figure plots results from the Goodman-Bacon Decomposition (see Goodman-Bacon (2021)) for Equation 2.

Figure F.7: Goodman-Bacon Decomposition - Partial Separation

NOTES: This figure plots results from the Goodman-Bacon Decomposition (see Goodman-Bacon (2021)) for Equation 4.

Figure F.8: Goodman-Bacon Decomposition - Reattachment

NOTES: This figure plots results from the Goodman-Bacon Decomposition (see Goodman-Bacon (2021)) for Equation 6.

Figure F.9: Goodman-Bacon Decomposition - Firm Closures

Figure F.10: Earnings (Payroll) and Estimated Earnings (Credit)
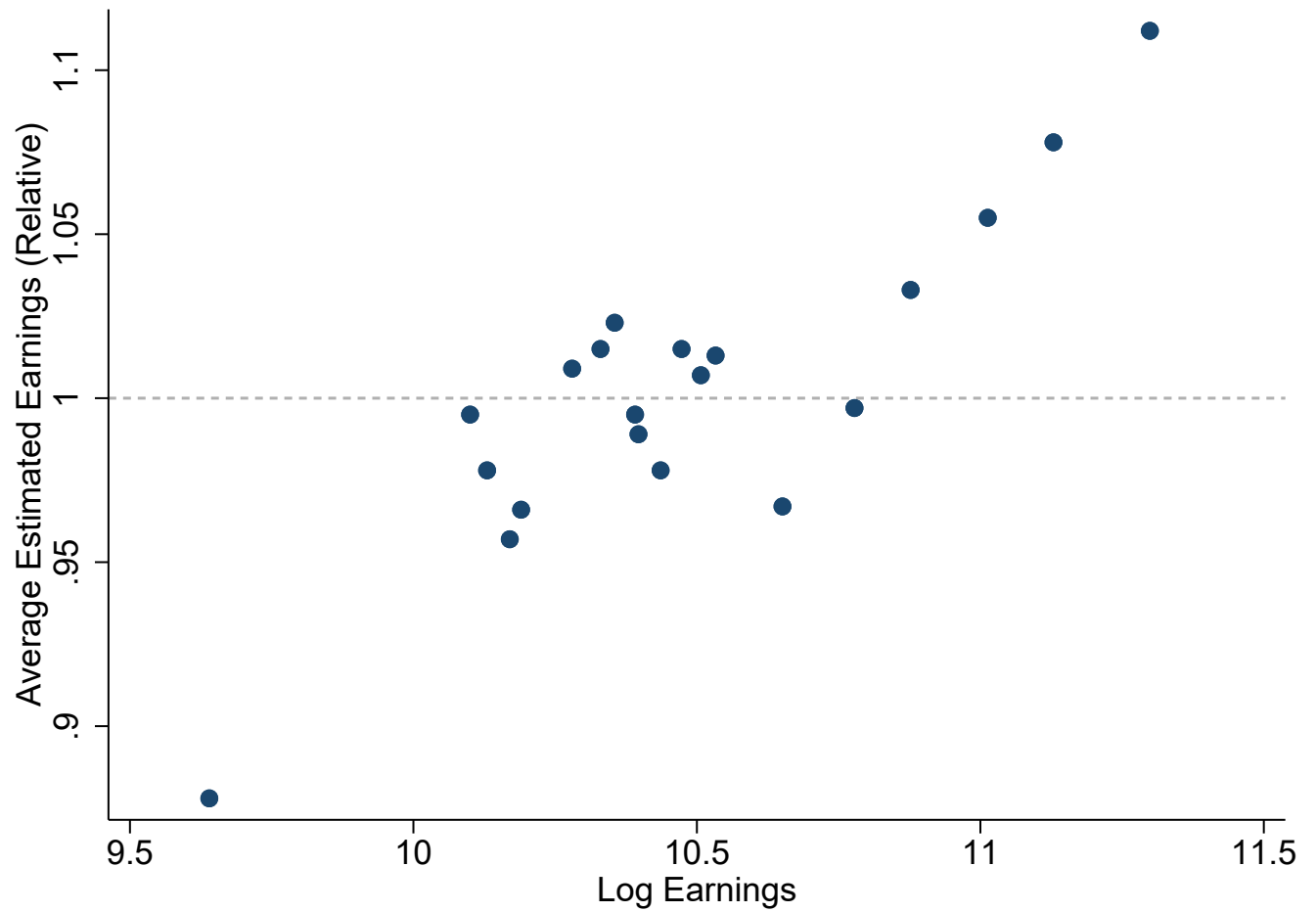
NOTES: This shows binned scatters for log earnings plotted against the estimated earnings in the credit panel. The estimated earnings construction seems fairly precise, but it remains a blackbox. In addition, it appears to either underestimate low earners and overestimate high earners; or it is reflective of differences between earnings I see in the payroll data and true earnings. Regardless, this estimated income measure is not used for any analysis in the paper and is only included here since it is referenced as a possible proxy for those outside the payroll sample.

Figure F.11: Relative Re-Employment Earnings
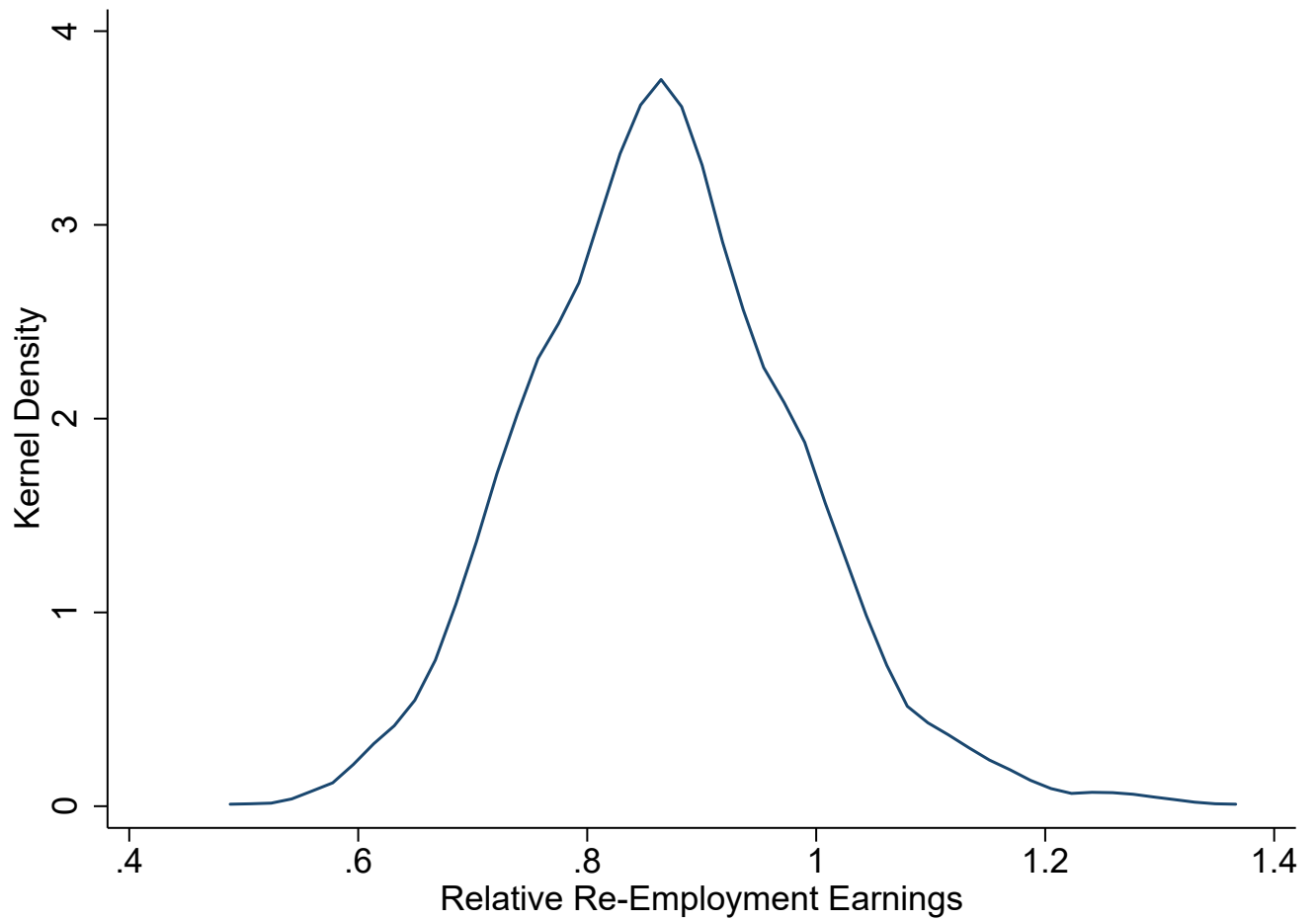
NOTES: This figure is a simple kernel density plot of earnings upon reattachment relative to earnings in the month before layoff for individuals who were laid off in full separation events and then were re-employed into my data within 2 years. The mean earnings upon reattachment are around 12% lower than earnings prior to layoff. Part-time re-employment observations are discarded, as are reattachment earnings that are more than 50% higher than pre-separation (though this happens very rarely, the right tail was rather skewed with sufficiently small choice of bandwidth).