Job Sorting and the Labor Market Effects of a Criminal Record

Randi Hjalmarsson, University of Gothenburg and CEPR
Louis-Pierre Lepage, Stockholm University
Matthew Lindquist, Stockholm University
Conrad Miller, UC Berkeley and NBER*

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Abstract

We examine the effects of a criminal record on labor market outcomes and the mediating role of job sorting using Swedish register data. Prime-age adults with criminal records earn about 30% less than observably similar adults without records and are concentrated in specific employers and occupations. To estimate the causal effect of a criminal record, we use an event study design that compares outcomes for adults charged with an offense for the first time to matched adults who were suspected of a similar offense but not charged. Acquiring a criminal record reduces months employed by 2% and annual earnings by 5%. These negative effects are: twice as large for more serious or subsequent charges, not driven by job displacement or incapacitation, and not mitigated by automatic record expungement, which typically occurs 5 or 10 years after case disposition. We classify firms by their propensity to hire workers with criminal records, holding suspected offense history fixed. A criminal record reduces employment at firms classified as less likely to hire workers with criminal records, increases employment at other firms, and decreases monthly wages across all firm types. Firm propensity to hire workers with criminal records varies substantially—even within industries—and is linked to firm size and managers' prior exposure to people with records. Leveraging manager moves across small firms, we find that when a firm hires a new manager with greater prior exposure to people with criminal records, it hires more people with records, with no detectable effect on productivity.

Keywords: criminal record, earnings, employment, firms, discrimination, managers
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1 Introduction

Firms often conduct criminal background checks when screening job applicants. Evidence from surveys and audit studies indicates that many firms are reluctant to hire workers with criminal records (WCRs) for at least some roles.[1] However, little is known about the extent to which a criminal record harms labor market prospects and the role of heterogeneity in employer preferences. For example, the labor market consequences of a criminal record may depend on whether WCRs can identify and seek employment with firms more willing to hire them, as well as the wages offered in those jobs (Becker, 1971).

In this paper, we address four questions: (1) How do workers with and without criminal records compare in their labor market outcomes, including their earnings, employer, and occupation? (2) What is the causal effect of a criminal record on employment and earnings? (3) How does worker sorting across employers and jobs mediate this effect? (4) To what extent, and why, do employers vary in their willingness to hire WCRs? The answers to these questions have important implications for policies aimed at improving the labor market status of WCRs.

We explore these questions using comprehensive Swedish register data from 1990 to 2015, including the universe of criminal convictions linked to employer-employee matched data, enabling us to track workers across employers and jobs both before and after conviction. Like much of Europe, Sweden has historically had stronger privacy protections around criminal records than the United States. However, reforms throughout this period have expanded access to criminal record registries, altered which offenses are visible and for how long (through changes in expungement rules), and implemented mandatory background checks for specific occupations. In a 2014 government survey (SOU 2014:48), more than 50% of large firms reported using background checks.

We begin with the first population-wide description of the labor market for WCRs, focusing on earnings and sorting across employers and occupations. Next, we estimate the causal effect of a criminal record by comparing the labor market trajectories of adults charged with an offense to a matched group of adults suspected of a similar offense but not charged. We then decompose these effects into changes in employment by employer type and monthly wages within employers. Finally, we measure and characterize heterogeneity in employers' propensity to hire WCRs.

Averaging across years in the study period, about 9% of prime-age men and 2.5% of prime-age

[1] For survey evidence, see Holzer et al. (2006b,a, 2007); Raphael (2014); Cullen et al. (2023); Bushway and Pickett (2024). For audit study evidence, see Pager (2003); Uggen et al. (2014); Agan and Starr (2018), and Ahmen and Lång (2017) and Baert and Verhofstadt (2015) for evidence from Europe in particular.

women have an active criminal record in a given year, while 28% of prime-age men and 9% of prime-age women have ever had a record. Conditional on education, age, gender, and immigrant status, prime-age WCRs earn 31% less than prime-age adults without records. Conditional on worker demographics, WCRs work about 20% fewer months annually. Among those employed, the (residual) monthly wage gap between WCRs and non-WCRs is 23 log points.

There is also considerable workplace segregation by criminal record status. Compared to demographically similar non-WCRs, WCRs work in establishments where the proportion of coworkers with active records is about 70% higher. Differences in firm pay premiums explain about 17% of the (residual) monthly wage gap, where pay premiums are estimated using a standard worker-firm wage decomposition (Abowd et al., 1999; Card et al., 2016). Conditional on other worker characteristics, WCRs are less likely to work in occupations involving interactions with vulnerable populations, opportunities for theft or financial responsibility, customer exposure, high educational or skill requirements, non-hazardous work conditions, and mandatory background checks.

To isolate the causal effect of a criminal record, we use a novel research design within an event study framework. We compare labor market outcomes for adults charged with a criminal offense for the first time to matched adults deemed by police or prosecutors to be reasonable suspects for a similar offense but not charged, typically due to a lack of evidence. While the latter group, often arrested during the investigation, would typically acquire a criminal record accessible to employers in the United States, this is not the case in most of Europe, including Sweden.[2] The motivation for this design is that charged and non-charged suspects are plausibly similar in their precipitating circumstances and conduct that led police to suspect them. We focus on a sample of adult suspects between the ages of 22 and 40 who have no prior charges. Among those charged, nearly all will acquire a criminal record, with most receiving a fine or probation as punishment. Supporting the validity of our research design, charged suspects and matched non-charged suspects work in similar jobs, have similar coworkers, and exhibit comparable employment and earnings trajectories prior to the suspected offense. They are also suspected of future offenses at similar rates.

We find that a person's first criminal charge decreases their months employed by 2% and their earnings by 5%. These effects are larger for more serious offenses (4% and 8%) and subsequent charges (5% and 10%). The roles of job displacement and incarceration are limited: effects are

[2] In the United States, adults arrested for criminal offenses are typically charged shortly after arrest via a criminal complaint. Prosecutors subsequently decline or dismiss a substantial share of these cases. Nonetheless, the initial arrest and charge usually remain publicly accessible in court dockets and commercial background check databases unless actively sealed or expunged.

proportionally similar for those working and not working at the time of the suspected offense and incarceration sentences are rare (8% of our event study sample). Further supporting our design, we conduct a placebo test leveraging a policy reform that retroactively assigned criminal records for certain minor offenses. We confirm that prior to this reform—when suspects charged with these minor offenses did not yet acquire records—there were no detectable employment or earnings effects. Finally, we find no evidence that expungement mitigates these labor market penalties: (i) the earnings gap between charged and non-charged suspects persists for over a decade, after most offenses are expunged and (ii) labor market outcomes do not improve in the years following expungement.

We then unpack these employment and earnings reductions by investigating how a criminal record affects: (1) where people work and (2) how much they earn at a given employer. We extend the charged versus non-charged suspect research design to classify firms by their propensity to hire WCRs using an empirical Bayes approach (Walters, 2024). Focusing on hired workers with a suspected offense in the recent past, we measure each firm's tendency to hire WCRs conditional on the suspected offense history of their hires. Under this approach, we classify a firm as less willing to hire WCRs if, relative to other firms, they are more likely to hire non-charged suspects than otherwise comparable charged suspects. We find that the overall decrease in months worked is concentrated at firms less likely to hire WCRs, consistent with these firms discriminating on the basis of criminal record status. The employment decrease at these firms is partially offset by an increase in employment at more WCR-"friendly" firms. Among those working, monthly wages decrease by 4%. This mostly reflects a decrease in monthly wages within firms, including "friendly" firms, though a criminal record also leads to a modest shift from higher- to lower-paying firms. Overall, we attribute roughly 40% of the earnings decline to fewer months of employment, 20% to shifts across firms, and the remaining 40% to lower within-firm monthly wages.

Next, we relate firm-level propensities to hire WCRs rather than otherwise comparable non-charged suspects to a set of firm characteristics geared towards testing potential reasons why some firms are more likely to hire WCRs than others. We have four findings. First, while firms in some industries (e.g., construction) are systematically more likely to hire WCRs than firms in other industries (e.g., public administration and defense, health care, and education), industry explains only a small share of variation across firms in their propensity to hire WCRs. Second, small firms are more likely to hire WCRs than large firms. Third, firms that signal criminal background checks in their job ads are less likely to hire WCRs, consistent with these firms using checks to screen out applicants with records. Fourth, firms with operating managers who have a past criminal record themselves or have been exposed to records via family members or past coworkers are more likely to hire WCRs.

Finally, we extend our analysis of exposure using variation from operating manager moves across small firms. When a new operating manager arrives with more (less) prior exposure to WCRs than their predecessor, firms subsequently hire more (fewer) WCRs. In contrast, we find little evidence that these managerial changes affect firm productivity or profitability.

This paper contributes to three areas of research on the labor market outcomes of people with criminal records.[3] The first is an active debate on how various criminal justice system interactions, including pretrial detention, diversion, and prison time, affect labor market outcomes (Dobbie et al., 2018; Mueller-Smith and Schnepel, 2021; Mueller-Smith, 2015; Bhuller et al., 2020; Garin et al., 2023). Such effects may arise through several channels, including the persistent effects of job displacement, depreciation of human and social capital while in prison (Western et al., 2001), or stigma of a resulting criminal record. Our paper advances this literature by isolating the effects of a criminal record *per se*—that is, the extensive margin effect of whether a person has a criminal record visible in a background check. A fundamental challenge in answering this question is finding an appropriate comparison group. Prior studies on the labor market effects of arrest and conviction (Grogger, 1995; Rose, 2021; Agan et al., 2024b) compare those charged at younger versus older ages in an event study framework. While broadly applicable, this design has two limitations: (1) it does not account for the immediate circumstances leading up to an offense (e.g., criminal and non-criminal activities, social interactions, and economic prospects) and (2) the comparison group is mechanically more likely to commit a future offense. Our novel research design addresses both limitations: using non-charged suspects as the comparison group nets out the potential effects of the circumstances leading to an alleged offense—indeed, our matched non-charged suspects also experience a dip in their employment rate growth—and compares two groups with comparable risks of committing future offenses. In our setting, the age-based research design generates concerning pre-trends, and applying the same approach to non-charged suspects also yields apparent declines in employment and earnings.[4]

[3] See Hjalmarsson et al. (2024) for a recent survey of the field.

[4] There is an analogy between the challenges of identifying causal effects of acquiring a criminal record and those in the "child penalty" literature, which examines the effects of childbearing on women's labor market outcomes. This literature typically compares women who have their first child earlier versus later in life (e.g., Kleven et al., 2019). Alternative designs have used variation in vitro fertilization (IVF) success (Lundborg et al., 2017) and intrauterine device (IUD) failures (Gallen et al., 2024) to more credibly estimate the causal effect of having a child.

A second body of literature examines how a criminal record affects firm labor demand and employer discrimination. Measuring the effects of firm discrimination—whether based on criminal record status, race, or other attributes—on employment and earnings is challenging due to the many correlated unobservables present in observational data (Charles and Guryan, 2011). Audit and correspondence studies, which overcome this challenge, generally find lower callback rates for job applicants who disclose or signal a criminal record (Pager, 2003; Uggen et al., 2014; Agan and Starr, 2018; Ahmen and Lång, 2017). Furthermore, surveys indicate that many—though not all—employers are reluctant or unwilling to hire WCRs, particularly for positions with customer contact (Holzer et al., 2006b,a, 2007; Raphael, 2014; Cullen et al., 2023; Bushway and Pickett, 2024).

While this literature suggests that some firms discriminate against WCRs, it has two fundamental limitations. First, it typically relies on a small and selected sample of firms to measure callback rates or firm stated preferences rather than actual employment or earnings outcomes. Evidence of disparate treatment by participant firms does not necessarily imply disparate outcomes for real-world job applicants with criminal records. For instance, if there are enough non-discriminatory employers, the effects of discrimination may be moderated by the workers' ability to sort to those employers (Becker, 1971). On the other hand, in models with search frictions, workers from disfavored groups may experience longer unemployment spells and receive lower wages, even at nondiscriminatory firms, because they have worse outside options (Black, 1995; Bowlus and Eckstein, 2002). Sorting to less discriminatory firms may decrease earnings if the most discriminatory firms also offer higher wages (Card et al., 2016; Gerard et al., 2021). Our population-wide employee-employer matched registers allow us to address this knowledge gap. Consistent with frictional models, we find that a criminal record reduces employment at more discriminatory firms, increases time spent unemployed, and decreases monthly wages within employers.

The second limitation is that much of the literature is based in the United States, an international outlier when it comes to the accessibility, visibility, and breadth of criminal records.[5] In the US, unlike most of Europe, including Sweden, background checks are widespread and often include arrests, records are often public, and automatic expungement is the exception. We provide the first study of how records affect employment and earnings in Europe. Our findings can inform an active US debate about how to mitigate the negative consequences of a criminal record.[6]

[5]Exceptions include two correspondence studies (Ahmen and Lång, 2017; Baert and Verhofstadt, 2015) and papers on the the effects of prison in Norway (Bhuller et al., 2020), record expungement in New Zealand (Dasgupta et al., 2021), and an evaluation of the 2014 introduction of the Swedish online record depository, Lexbase (Forsberg, 2024).

[6]See Rose (2021), Agan and Starr (2018), and Doleac and Hansen (2020) for studies of US ban the box laws and Dasgupta et al. (2021), Agan et al. (2024a), Agan et al. (2024b) for evaluations of expungement and clean slate laws.

A third related literature examines the role of firms and managers in hiring workers from disadvantaged groups. The firm surveys and studies of hypothetical hiring scenarios discussed above show substantial variation in employer preferences and WCR-related hiring policies. But do these differences influence actual hiring decisions? Why are some firms, even within the same industry, more receptive to hiring WCRs? To what extent does manager heterogeneity contribute to these patterns? The existing literature (see Hoffman and Stanton (2024) and Lazear and Shaw (2007) for reviews) has little to say on these questions.[7] We find that both explicit firm-level policies and manager characteristics play a role. While worker-manager concordance in race, immigrant status, and gender have been studied in prior work (Giuliano et al., 2011; Åslund et al., 2014; Benson et al., 2022; Cullen and Perez-Truglia, 2019), criminal record exposure is a novel dimension. Our findings are consistent with experience and exposure shaping employers' hiring of disadvantaged groups (Miller, 2017; Li et al., 2020; Benson and Lepage, 2024; Lepage, 2024; Ronchi and Smith, 2024). Our findings also suggest that WCR hiring does not necessarily harm firm performance, consistent with a small literature on the job performance of WCRs that documents that WCRs have longer tenure and less turnover (Lundquist et al., 2018; Minor et al., 2018).

The remainder of the paper proceeds as follows. Section 2 describes the Swedish criminal justice system, including the rules related to criminal records and background checks. Section 3 describes the data. Section 4 presents new descriptive facts about the labor market for WCRs. Section 5 describes the research design comparing charged and non-charged suspects, presents the results, and examines the effects of expungement. Section 6 measures and characterizes firm heterogeneity in propensity to hire WCRs. Section 7 concludes.

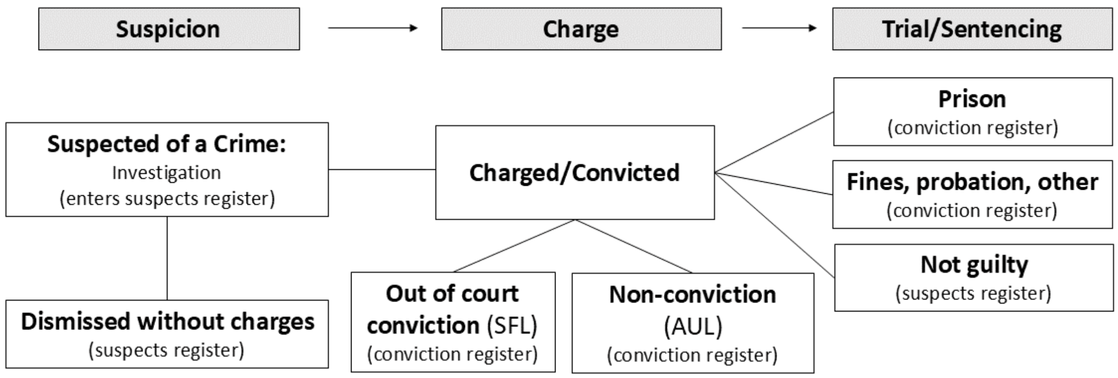
2 Institutional background

2.1 Crime and the criminal justice system

Figure 1 depicts how a suspect progresses through the Swedish criminal justice system. This will inform our charged versus non-charged suspect research design in section 5.1. When a crime is reported, the police are in charge of the initial investigation and identification of suspects. The case is then passed to a prosecutor who decides if there are sufficient and reasonable grounds for an arrest of the suspect(s). Only suspects meeting this threshold are included in the suspect register (*misstankeregistret*). They are informed of their status and typically arrested. The prosecutor (or

[7] One exception is Adams-Prassl et al. (2024), which studies the role of managers after violent coworker conflicts.

Figure 1
Swedish justice system flowchart



Note: This figure demonstrates the possible paths an individual can flow through the Swedish criminal justice system, from being suspected of a crime, the decision to dismiss or charge, disposition decisions, and trial as well as sentencing outcomes. Within each cell, we also label the Swedish data registers in which the case outcomes are recorded.

police for minor offenses like shoplifting, traffic violations, or petty theft) continues the investigation. Depending on the evidence, the prosecutor decides whether to charge the suspect or dismiss the case without charges. The reason for dismissal is often recorded in the suspects register. The most common stated reason is insufficient evidence; prosecutors rarely state that a suspect is innocent.

Charged individuals can have four possible outcomes: (i) non-conviction (AUL or åtalsunderlåtelse), (ii) out-of-court conviction (SFL or strafföreläggande), (iii) in-court conviction by a judge, or (iv) in-court acquittal. Non-convictions and convictions (in and out of court) include a determination of guilt and lead to a criminal record. Convictions in and out of court can result in punishment, though prison is only possible from in-court convictions. We illustrate the frequency of these different outcomes with a snapshot of the more than 174,000 individuals suspected of at least one crime in 2010. 54% were found guilty of their most serious charge: 5% AUL, 15% SFL, and 34% in-court.[8] Only 6% of all suspects are eventually sentenced to prison.

2.2 Background checks

Until the emergence of online companies like Lexbase in 2014, Swedish firms could conduct background checks through two channels. First, some employers (e.g., in criminal justice, public employment, energy, health, transportation, and defense) have direct access to criminal record registries (termed in Swedish *belastningsregistret*). Second, employers without direct access can ask applicants

[8] 37% of charges went to court; 8% of these received an acquittal, which means that 3% of all suspects in the sample where charged, tried in court, and then acquitted.

to obtain an extract (at no cost) from the register maintained by the police. Swedish guidelines recommend that employers (i) only request this extract at the end of the hiring process, (ii) avoid keeping or copying it, and (iii) accept it as valid for one year, allowing applicants to use the same document at multiple employers. Though it is not illegal to discriminate on the basis of criminal records in Sweden, the police website emphasizes that a criminal record is not disqualifying (even if the background check is mandatory).[9]

The information visible in the extract depends on the type and timing of the request. Since 1989, individuals can obtain a "general extract", which includes all offenses and corresponding sentences, regardless of type, that are not yet expunged. As described in Appendix A.3, starting in the early 2000s, mandatory checks were introduced for certain jobs in education (2001), insurance (2006), residential and foster care homes (2007), children with disabilities (2011), and children in non-school activities (optionally, 2014). Such occupation-specific mandatory extracts only show selected offenses deemed relevant (by the government) to such jobs.[10]

Figure 2 shows the annual number of extracts overall and by type (general versus occupation-specific) per 100 people aged 18 to 64. The number of extracts per 100 people increased from around 2.8 in 2001 to 16.4 in 2022, when there were more than one million extracts.[11]

Further insights into the prevalence of background checks can be gained from a novel supplementary data set of Swedish job ads (described in section 3.3), in which we measure whether a firm signals in each ad that the hiring process includes a background check. There is substantial variation in the prevalence of these signals across occupations, ranging from less than 4% of ads for resources/agriculture, construction, and manufacturing jobs to 18% of ads for elementary unskilled jobs. In 2022, about 55% of ads were at firms that at least sometimes signaled background checks, making clear that many jobs are with employers who use background checks. These patterns are consistent with a 2014 survey of Swedish firms (SOU 2014:48) presented in Appendix Figure H.10; 14% of firms, but more than 50% of the largest, report background checks for all or some new hires.

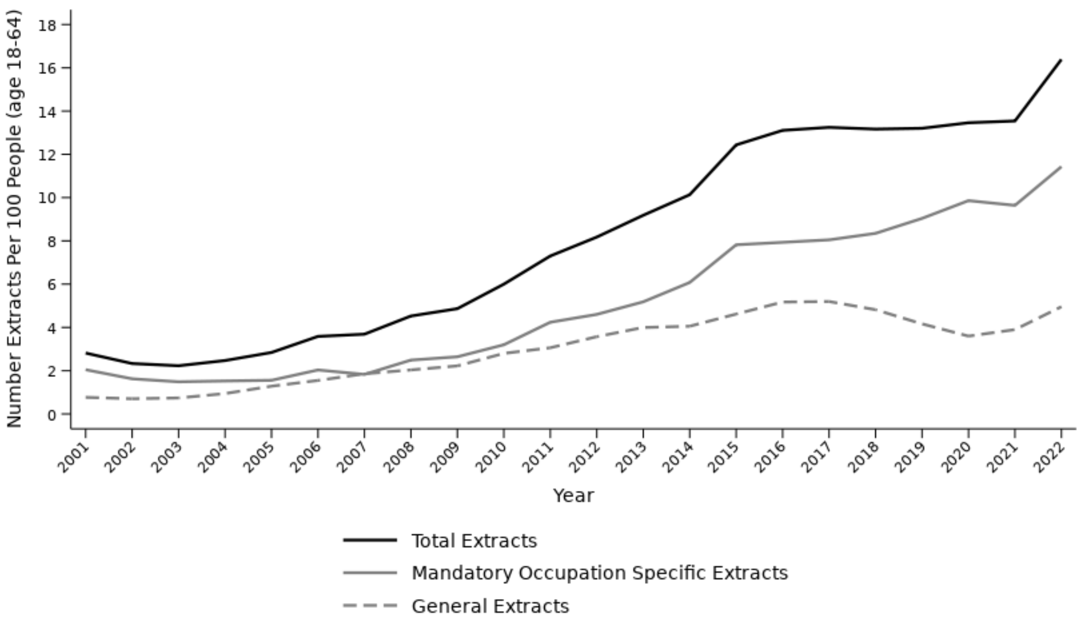
There are some stark differences between Sweden and the United States, where background checks are primarily conducted by specialized companies (Agan et al., 2024b) and information concerning many criminal justice interactions, often including arrests, are accessible to the public.

[9] Though we do not know the extent to which firms adhere to *all* of these recommendations, a 2014 government investigation (SOU 2014:48) found that 78% of surveyed firms that reported conducting background checks did not request the extract until offering employment.

[10] For a list of selected offenses by type of extract, see https://polisen.se/en/services-and-permits/police-record-extracts/police-record-extracts-for-employment-in-a-school-or-a-preschool/.

[11] A media analysis presented in Appendix Figure H.11 further demonstrates the increasing prevalence of criminal record extracts over time. It also shows that they were already salient before the 2014 introduction of Lexbase.

Figure 2
Annual extract requests from police registry



Note: This figure presents the annual number of occupation-specific mandatory extracts (gray solid line) and general extracts (gray dashed line) obtained from the Swedish police, as well as the combined total (black line). Statistics after 2007 were provided by the police while those from earlier years are sourced from Backman (2012). All statistics are scaled by the population size each year from ages 18 to 64.

European privacy laws restrict access to criminal record information, especially arrest records. However, employers (including in Sweden) generally have the right to ask candidates for record extracts or about their criminal history. Finally, hiring WCRs does not expose Swedish employers to negligent hiring liability as it may in the US.

2.3 Record expungement rules and reforms

In Sweden, all criminal offenses are automatically expunged from an individual's criminal record—and hence no longer visible during a background check—regardless of severity. The timing of expungement depends on three factors: (i) age (under 18) at the time of offense, (ii) the sentence received, and (iii) any subsequent offenses. This automatic expungement system is fairly representative of Europe, but in sharp contrast to the US, where clean slate laws remain uncommon.

Today, most offenses are visible for 10 years if committed when 18 or older but only three or five years if committed when younger. For all ages, offenses resulting in a prison sentence are cleared 10 years from the date of release. For minor offenses where fines or tickets are the primary sanction, expungement occurs after five years. In general, an offense is only expunged if the offender has not committed another offense in the interim; recidivism can extend the retention period to a maximum of 20 years. However, once an offense is cleared, it remains permanently cleared.

Appendix A.1 describes the expungement regulations from 1990 to today, which we use to code whether an individual has an 'active' criminal record each year and the nature of that record.

3 Data

3.1 Swedish registers and key variables

For the entire population born between 1923 and 2000, we combine matched employee-employer registers with two core crime data sets from the Swedish National Council for Crime Prevention. The Convictions Register (*lagföringsregistret*) spans 1973 to 2015 and provides a complete history of an individual's convictions, including offense and conviction dates, crime types, and sanctions (e.g., prison, probation, fines). We use these data to calculate whether a person has an *Active Criminal Record* each year, i.e., whether a person has been convicted of an offense (excluding traffic tickets) in the past that is not yet expunged from the background check register.

The Suspects Register (*misstankeregistret*) is available from 1995 onward, and also includes crime types and offense and/or decision dates. Suspects are added to this register as soon as they are arrested or substantively connected to a crime as part of a police or prosecutor investigation. The causal design in section 5 takes advantage of the fact that offenses remain in this register and are observable to researchers (but not employers), even if they do not lead to a conviction.

Our labor market data span 1990 to 2015 and are sourced from Statistic Sweden's Register for Labor Market Statistics (*registerbaserad arbetsmarknadsstatistik, RAMS*). They include unique firm and workplace identifiers, which allow us to trace out monthly employment and earnings histories for each worker in each job (*jobbregisret*). We also observe key firm characteristics, including industry, number of employees, geographic location, as well as performance measures including value-added, profits, and labor costs. These are drawn from Statistics Sweden's Company Register (*företagsregistret*). We also observe in later years detailed 4-digit occupation codes for almost all workers and, starting in 2004, each firm's operational leader for a given year. The firm operator, identified by the Swedish tax authority, is a high-level manager responsible for the firm's daily operations. Our ability to track WCRs across establishments and types of work as well as identify their coworkers and operator (and their criminal history) is a novel aspect of Swedish registers.

Along with occupational and workplace sorting, we study the effects of having a criminal record on annual earnings, months employed during the year, and monthly wages. Annual earnings are measured in real Swedish krona (SEK) using 2015 prices.[12] These are pre-tax earnings reported by employers to the tax authorities. We also know the start and stop month for each job spell, which allows us to calculate an average monthly wage for each job a worker has and the number of months per year that a worker is gainfully employed.

Finally, these data are combined with registers that provide demographic information on each individual and their family members, including gender, immigrant status, birth year, and educational attainment, as well as military enlistment test scores for several older cohorts of men.[13]

3.2 Core analysis sample

We create a person by year level data set that tracks each person's criminal record status and labor market outcomes from 1990 to 2015. The baseline data set includes workers between ages 18 and 55. Our analysis sample is composed of 126.8 million person-year observations from 8.6 million people. These individuals are observed to work in more than 1.4 million firms, but firm

[12]On December 31, 2015, there were about 8.4 SEK per US dollar.

[13]Education is recorded in 7 levels, to which, per the standard in the literature (e.g., Hjalmarsson et al. (2015)), we assign the following years of schooling: 7 for old primary school, 9 for new compulsory school, 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university and 19 for a Ph.D.

composition changes over time; about 330,000 unique firms per year are observed. We use this sample to characterize the labor market for workers with and without criminal records in section 4, but make additional sample restrictions in our event study analysis in section 5. We also use data on newly-hired workers in sections 5 and 6.2 to measure firm heterogeneity in hiring of WCRs. We define a worker as a new hire if that year is the first time the worker is observed at the firm since the beginning of our sample in 1990.

3.3 Supplementary data: occupational characteristics and Swedish job ads

Occupational characteristics: We use the US Department of Labor's Occupational Information Network (O*Net) data to both measure traditional occupational characteristics (e.g., average education, job preparation, and hazardous work conditions) and create custom characteristics that are, according to firm surveys, especially salient to the WCR context, including: (i) extent of customer interactions, (ii) exposure to vulnerable populations (i.e., children, disabled, elderly, mentally ill, and substance use-related), and (iii) opportunities for theft or financial responsibility. Appendix A.2 describes the O*NET data, variable creation, and procedure to match Swedish occupation codes. Second, for each Swedish occupation code and year, we measure whether background checks are required upon hiring or licensing according to the laws described in Appendix A.3.

Job advertisement background checks: We use a data set of over 9 million job advertisements from 2006 to 2023 from the job board maintained by the Swedish Public Employment Service–Platsbanken. We search the text of each ad for keywords—'belastningsregist', 'registerkontroll', 'bakgrundskontroll'—to measure whether the employer signals a background check is part of the hiring process. We linked about 70% of ads to the firms that posted them in our employer-employee register using employer identification numbers. This allows us to investigate how background check signals in ads translate to actual hiring of WCRs—a first in the literature, beyond self-reported measures from firm surveys. Appendix A.4 describes the job ad data and linking procedure.

4 Descriptive facts about the labor market for WCRs

4.1 Prevalence of WCRs

We begin by describing the prevalence of criminal records in Sweden. Figure 3 shows the percentage of working-age adults with an active criminal record from 1990 to 2015 (panel A) and by age in 2015 (panel B). Over the sample period, the average percentage of men and women with a record has been 9.4% and 2.5%, respectively, rising from 8.4% and 1.9% in 1990 to 9.2% and 2.6% in 2015. There is a notable jump in the percentage of the workforce with an active record in 2000; this is not driven by a jump in crime, but rather by a reform, which we analyze in Section 5.6.4, that included more minor offenses in the record registry (see Appendix A.1). Consistent with the typical age profile of criminal behavior, the percentage of adults with an active record initially increases with age, peaks at 24 for men and 20 for women, then decreases and plateaus around 40. Among those ever convicted of an offense, the median age at first conviction is 22. About 25% of men and 9% of women have been found guilty of a crime at least once. These rates are comparable to those documented in the United States and the rest of Europe.[14]

Among workers with a record, the median person has been convicted of one offense in the past 10 years, while the average is four offenses. Approximately 27% were convicted of a violent offense, 46% of a property offense, 32% of a traffic offense, 18% of a narcotics offense, 2% of a sex offense, and 19% of some other type of offense.[15] Nineteen percent of WCRs were sentenced to prison in the last 10 years, for a median combined sentence of 120 days.[16]

4.2 Characteristics of WCRs

Table 1 compares the demographic characteristics, labor market outcomes, and employer characteristics of WCRs and non-WCRs. Compared to non-WCRs, WCRs are more likely to be male (79.53% versus 48.96%), less likely to be Swedish-born (78.46% versus 86.26%), younger (average age of 33.83 versus 36.99) and less educated (average years of schooling of 10.78 versus 11.97).

In the labor market, WCRs work on average two and a half fewer months per year (6.99 versus 9.59) and have average earnings that are about 40% lower (122k SEK versus 204k SEK).[17] Among those working, WCRs have shorter job tenure (35 months versus 56 months). WCRs also tend to work in smaller establishments and firms and in establishments where a higher share of coworkers also have records (8.8% versus 3.8%). They are more likely to work in construction,

[14]A commonly cited statistic is that one in three US adults have been arrested by age 23. Brame et al. (2012) estimate for instance using the NLSY97 that 30-40% have been arrested by age 23. Shannon et al. (2017) estimate that 8% of all adults (male and female) in the US have a felony conviction. In Denmark, Dustmann and Landersø (2021) report that 34% of young (age 14-25) fathers have a conviction before pregnancy, while the rate is 17% for other young men in the same neighborhood.

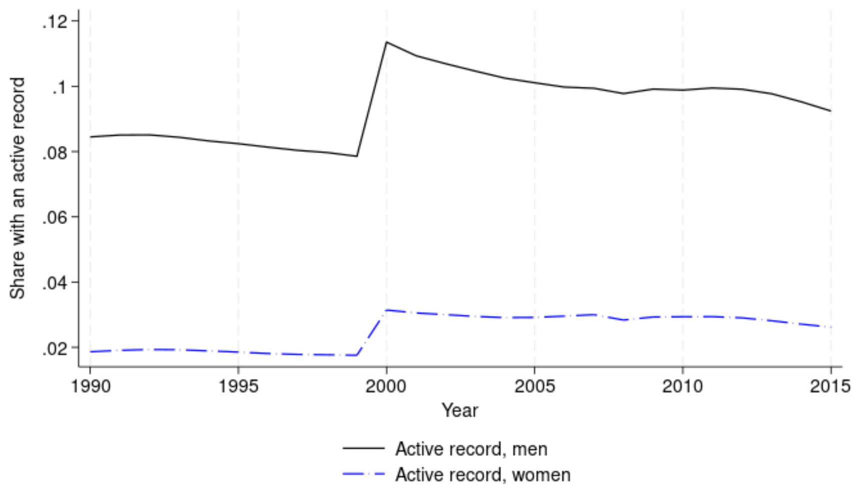
[15]The five most common criminal offenses are: theft, non-DUI traffic (typically driving without a valid license), assault, narcotics use, and narcotics possession.

[16]Swedish prison sentences are historically relatively short; Swedish inmates convicted from 1991 to 2001 served 4.7 months on average, compared to about 8 months in Western Europe in 2001 and more than 30 months in US state and federal prisons (Hjalmarsson and Lindquist, 2022).

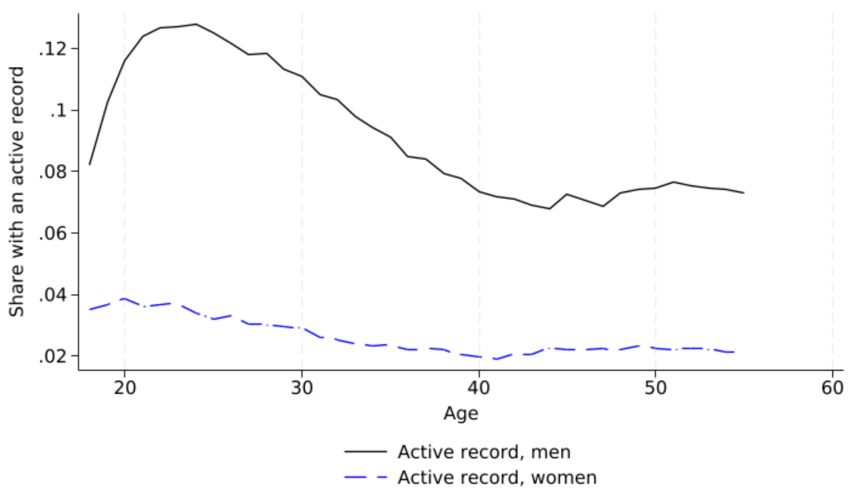
[17]Appendix Figure H.12 demonstrates substantial labor market gaps between workers with and without records throughout the sample, with larger gaps during periods of high unemployment.

Figure 3
Prevalence of workers with criminal records

(A) Share of workforce with a record over time



(B) Share of workforce with a record by age, 2015



Note: Panel A plots the percentage of prime-age adults in each year of our sample with a criminal record while panel B shows the same percentage by age in 2015. In both panels, the solid black line is for men and the dashed blue line is for women.

Table 1
Individual characteristics by WCR status

	No record	Active record
Male (%)	48.96	79.53
Swedish born (%)	86.26	78.46
Age	36.99	33.83
	(10.79)	(10.63)
Years of education	11.97	10.78
	(2.255)	(1.902)
Earnings (1000s SEK)	204.3	122.3
	(197.6)	(149.7)
Months employed during year	9.588	6.994
	(4.483)	(5.502)
Ever employed during year	85.59	67.58
	(35.11)	(46.81)
Tenure in main job (in months)	56.47	35.15
	(56.15)	(40.99)
Workplace size	451.1	287.2
	(1271.0)	(964.2)
Firm size	6467.2	4413.5
	(13582.2)	(11762.0)
Average coworker monthly wage (1000s SEK)	19.83	17.56
	(10.49)	(8.836)
Average firm coworker monthly wage (1000s SEK)	18.79	17.22
	(9.945)	(8.430)
Firm pay premium	0.298	0.242
	(0.730)	(0.757)
% of coworkers with active records	3.906	8.830
	(6.490)	(12.13)
% of firm coworkers with active records	4.000	8.102
	(5.832)	(11.28)
Workplace industry		
Agriculture and resources	3.203	3.608
Care and social services	10.11	9.106
Construction	4.825	9.611
Education	8.405	4.139
Health care	6.541	2.426
Hotels and restaurants	2.981	5.767
Manufacturing and industry	14.63	18.73
Public administration and defense	4.745	2.120
Retail and wholesale	11.15	11.45
Services	20.88	20.80
Unknown	0.934	1.757
Missing	11.60	10.49
Observations	107,454,183	6,945,896

For the baseline sample of working-age (18–55) adults each year, this table presents summary statistics separately for those with and without a record that year. Firm pay premiums are estimated using a standard worker-firm wage decomposition (Abowd et al., 1999; Card et al., 2016). See Appendix B for details.

hotels and restaurants, and manufacturing and less likely to work in education, health care, public administration, and defense.

We next examine the extent to which demographic differences between WCRs and non-WCRs can account for their earning differences. We also measure the extent to which remaining differences in earnings can be explained by differences in months worked, industry, and employer. We estimate linear regression models via OLS of the form

$$\mathrm{Y}_{i,t} = \beta \mathrm{RECORD}_{it} + \tau_t + X_{it}\gamma + \sigma_{s(i,t)} + \epsilon_{it}, \tag{1}$$

where i indexes individuals, t indexes years, τ_t are year fixed effects, and X_{it} is a fully saturated set of interactions between year and demographic characteristics. Demographic characteristics include age, education level, gender, and an indicator for Swedish-born. $\sigma_{s(i,t)}$ are sector fixed effects, where the sector is either defined as industry or firm. We examine three outcomes: total earnings, months worked, and log monthly wages.

The results are shown in Table 2. In columns 1 and 2, the outcome is total earnings. The specification in column 1 includes only RECORD_{it} and year fixed effects as explanatory variables. The $\hat{\beta}$ coefficient is -85.42, meaning that, conditional on year, the difference in earnings between WCRs and non-WCRs is about 85,000 SEK. This is 42% of the average earnings among non-WCRs. Column 2 replaces year fixed effects with fully saturated interactions between year and demographic characteristics. The residual earnings gap is -63.68. Demographic differences explain about one-quarter of the overall earnings gap. The remainder represents an earnings gap of 31%.

One reason that WCRs have lower earnings than non-WCRs is that they are less likely to work. In columns 3 and 4 we replace the outcome with months worked in a given year. Column 3 includes only RECORD_{it} and year fixed effects as explanatory variables. The $\hat{\beta}$ coefficient is -2.59, indicating that WCRs work for about two and a half fewer months than non-WCRs. Column 4 includes the same demographic controls as in column 2. The residual gap in months worked is -1.98. Conditional on individual demographics, WCRs work about 20% fewer months than non-WCRs.

In columns 5 through 8, the outcome is log monthly earnings and the sample is limited to person-years with positive earnings. In column 5, where the only explanatory variables are RECORD_{it} and year fixed effects, the $\hat{\beta}$ coefficient is -0.30, indicating a 30 log point wage gap. This decreases in magnitude to 23 log points with the inclusion of demographic controls in column 6.

The next two columns add different sets of fixed effects for the job sector. Column 7 includes

Table 2

Labor market outcomes by WCR status, 1990–2015

	Earnings		Months worked		Log monthly earnings			
	Raw (1)	+Demo. (2)	Raw (3)	+Demo. (4)	Raw (5)	+Demo. (6)	+Industry (7)	+Firm (8)
Criminal record	-85.42	-63.68	-2.59	-1.98	-0.30	-0.23	-0.20	-0.14
	(0.08)	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		✓		✓		✓	✓	✓
Industry FE							✓	
Firm FE								✓
Non-WCR outcome mean	204.35	204.35	9.59	9.59	2.69	2.69	2.69	2.69
Observations	114,400,079				96,668,650			

This table presents the results of estimating equation 1 for our baseline sample of working-age adults from 1990 to 2015. The dependent variable is earnings in columns 1 and 2, number of months worked in columns 3 and 4, and log monthly earnings in columns 5 through 8. The sample is restricted to those with positive earnings in columns 5 through 8. The columns labeled "Raw" only control for year fixed effects. Demographic controls (fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born) are included in columns 2, 4, and 6 through 8. Industry and firm effects are included in columns 7 and 8, respectively. Only the coefficient on RECORD_{it} is reported; standard errors clustered at the person level are reported in parentheses.

1-digit industry fixed effects. The $\hat{\beta}$ coefficient is reduced to -0.20. WCRs work in industries where average pay is lower. Column 8 replaces industry fixed effects with firm fixed effects, reducing the gap to 14 log points. Even within an industry, WCRs work in firms where pay is lower.

In Appendix Table H.7 we focus on the cohorts of Swedish-born men for whom we have cognitive test score data measured during military conscription. These men were born between 1951 and 1987, excluding a few intervening years where scores are unavailable. We augment the demographic controls with test scores interacted with age and year. Conditional on demographic characteristics, further conditioning on these scores has a limited effect on the estimated earnings and wage gaps.[18]

In Table 3, we examine the extent to which demographic characteristics can account for differences in workplace characteristics between WCRs and non-WCRs. In columns 1 through 3 the outcome is the firm pay premium. We estimate a standard worker-firm decomposition for log monthly wages as in Abowd et al. (1999) and Card et al. (2018a) (details can be found in Appendix B). Column 1 includes only RECORD_{it} and year fixed effects as explanatory variables. The $\hat{\beta}$ coefficient is -0.04, indicating that WCRs work in firms that pay about 4% lower wages. Adding demographic controls in column 2 has no effect. Adding 1-digit industry fixed effects in column 3 reduces the coefficient magnitude to -0.03. Most of the sorting by firm pay premium is within industry. In columns 4 through 6 the outcome is the percentage of establishment coworkers with an active criminal record. Column 4 includes only RECORD_{it} and year fixed effects as explanatory variables. The $\hat{\beta}$ coefficient is 4.82, indicating that WCRs work in establishments with a 4.82pp higher share of coworkers with an active record. Adding demographic controls (column 5) reduces the coefficient to 2.96pp. Adding industry fixed effects (column 6) makes little difference. The residual segregation by WCR status is almost entirely within industry.

4.3 Segregation of WCRs across occupations

The occupational composition of firms varies both across and within industries. This section considers the extent to which WCRs also work in different occupations than non-WCRs. Appendix Table H.8 lists the 20 4-digit occupations in which WCRs are most and least prevalent in 2015, as well as their corresponding share of WCRs. This share is less than 0.5% in the 20 least WCR-prevalent occupations, which includes therapists, judges, prosecutors, chemists, and a number of education and care-related occupations. Background checks are mandatory for employment or

[18] This in part reflects the fact that residual test score differences between WCRs and non-WCRs are modest, about 0.17 of a standard deviation.

TABLE 3
FIRM CHARACTERISTICS BY WCR STATUS, 1990-2015

	Firm pay premium			% of coworkers with record		
	Raw (1)	+Demo. (2)	+Industry (3)	Raw (4)	+Demo. (5)	+Industry (6)
Criminal record	-0.04	-0.04	-0.03	4.82	2.96	2.87
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		✓	✓		✓	✓
Industry FEs			✓			✓
Non-WCR outcome mean				3.90	3.90	3.90
% difference				124	76	74
Observations		94,354,894			86,601,564	

This table presents the results of estimating equation 1 where the dependent variable is the firm pay premium in columns 1 through 3 and percent of coworkers with a criminal record in columns 4 through 6. The sample includes working-age (18-55) adults from 1990 to 2015 with positive earnings in that year. The columns labeled "Raw" only control for year fixed effects, while demographic controls (fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born) are included in columns 2 and 5. Industry fixed effects are included in columns 3 and 6. Only the coefficient on RECORD is reported; standard errors, clustered at the individual level, are reported in parentheses.

occupational licensing in more than half of these occupations. WCRs are most concentrated in construction and manufacturing, industry and transport, and elementary unskilled occupations. Some tangible examples include taxi, car and van drivers (12%), as well as many construction and manual jobs (e.g., painters, 12.3%; floor layers, 12.6%; building frame workers, 14.4%; roofers 15.5%; construction laborers, 18.9%; scaffold builders 26.3%).

Such occupational sorting could simply occur from selection on the basis of observable characteristics correlated with record status. We thus characterize how WCRs are distributed across occupations after conditioning on demographic characteristics and broad industry categories using the following linear regression specification:

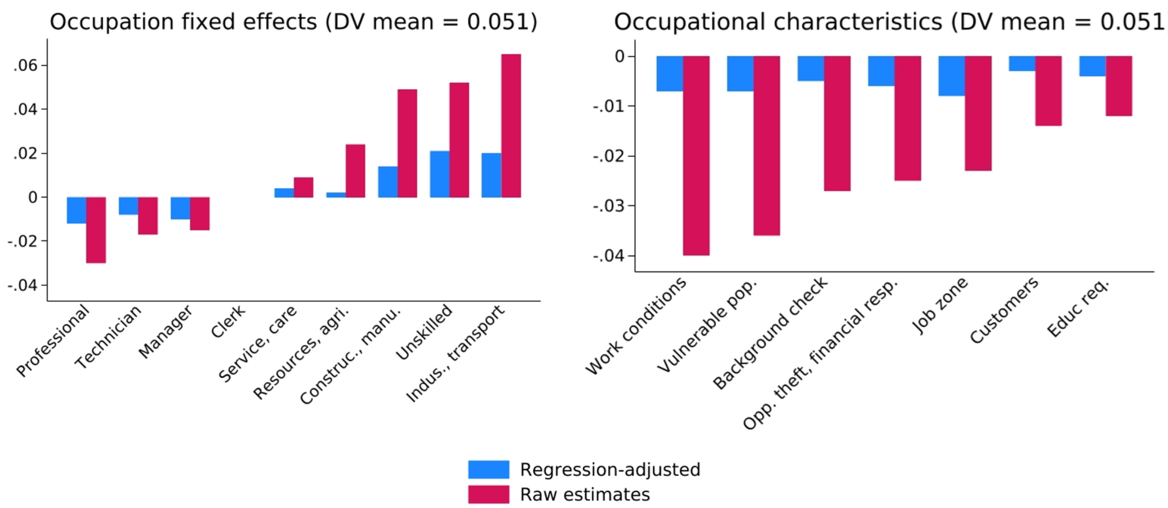
$$\text{RECORD}_{it} = X_{it}\gamma + \mu_{d(i,t)} + \gamma_{k(i,t)} + \sigma_{s(i,t)} + \epsilon_{it}, \tag{2}$$

where t indexes year. We estimate equation (2) using data from 2005-2015.[19] The vector X_{it} includes fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born. $\gamma_{k(i,t)}$ are fixed effects for 1-digit industry categories, and $\sigma_{s(i,t)}$ are fixed effects for 1-digit occupation categories.

The left panel of Figure 4 reports the $\sigma_{s(i,t)}$ coefficient estimates from Equation (2) when the

[19] Occupation information is missing for 17.6% of observations during this time period.

Figure 4
Distribution of WCRs across occupations and occupational characteristics



Note: These figures present the results of estimating equation 2 for 2005 to 2015 without controls (raw estimates, red bars) and with controls (regression-adjusted, blue bars). The panel on the left presents estimates for each 1-digit occupation code in a single regression, where the omitted category is clerks. The panel on the right presents the estimated coefficients on a set of occupation characteristics, which are included one at a time in a series of regressions. All characteristics are measured in the O*NET data, with the exception of background check regulations. Confidence intervals are omitted from the figure since they are too small to visualize. All estimates are statistically significant at the 1% level.

other controls are excluded (*Raw*) and included (*Regression-adjusted*). Confidence intervals are omitted since most are too small to visualize. All estimates are statistically significant at the 1% level. Conditional on demographics and industry, WCRs are (relative to clerks, the omitted category) more likely to be employed in service and care, resources and agriculture, construction and manufacturing, industry and transport, and elementary unskilled occupations, but less likely to be employed as professionals, technicians, or managers. The adjusted differences can be substantial– WCRs are about 40% less likely to be employed as professionals than in elementary unskilled occupations.

To better understand the nature of this sorting, we look at O*NET occupational characteristics and occupation by year mandatory background check requirements. The right panel of Figure 4 reports raw and adjusted coefficient estimates from Equation (2), replacing occupation fixed effects with a series of occupation characteristics. The estimates indicate that WCRs are less likely to work in occupations with: (i) high external customer contact, exposure to vulnerable populations, or the opportunity for theft and financial responsibility (i.e., when there is a perceived risk to the firm), (ii) mandatory background checks, (iii) higher education and skill (job zone) requirements, and (iv) better work conditions (i.e., where risk to the worker is low). Though markedly smaller when adjusting for observables, these relationships remain statistically significant and sometimes substantial. For example, WCRs are over 10% less likely to work in occupations with above-average working conditions, contact with vulnerable populations, or high skill or experience requirements, and 6% less likely to work in occupations that mandate background checks.[20]

5 Causal effects of a criminal record

There are large residual differences in employment and earnings between WCRs and non-WCRs with similar demographic characteristics, including education and cognitive test scores. This gap may reflect the causal effect of a criminal record on labor market outcomes, but it may also reflect selection. WCRs may have low earnings or limited attachment to the labor market even before they acquire criminal records. Causality may also flow in the reverse direction. For example, poorer suspects may be more likely to be charged because they have less access to effective legal representation. Furthermore, limited opportunities in the labor market may increase the chances that someone engages in crime. We use a novel research design to estimate the causal effect of a criminal record on labor outcomes.

5.1 Identification strategy

To isolate the causal effect of acquiring a criminal record, we compare the employment and earnings trends of two groups of workers in an event study framework. Previous studies have compared people convicted at a young age to those convicted later (Grogger, 1995; Rose, 2021; Agan et al., 2024b), using the latter before their conviction to estimate the counterfactual for the former. This approach, though broadly applicable, has at least two limitations. First, it may conflate the effects of the conviction with the circumstances that led to the (alleged) offense and arrest. For example, a person convicted of possession of illicit narcotics may experience a decrease in earnings due to both the conviction itself and the circumstances that initially led to the arrest (e.g., the development of an addiction). Second, by construction, the comparison group is more likely to be convicted in the future than the treatment group. Hence, with foresight, an employer primarily concerned about the risk of future offenses would prefer to hire the past offender over the future offender.

[20]This specification also controls for whether background checks are sometimes but not always mandatory (coef. 0.0013, sd 0.0001); this has little impact on the estimated coefficient for always mandatory background checks.

Our approach leverages data on individuals suspected of an offense but not charged. We define the treatment group as adults who are both suspected of and charged with a criminal offense. We construct a comparison group of matched adults who are suspected of a similar offense but not charged.[21] Typically, people enter the suspects register as the result of a formal police investigation, and both those who are charged or not will be arrested. In Sweden, nearly all of the treatment group acquires a criminal record associated with the offense that employers can access, while the comparison group does not.[22] Our main identifying assumption is that, absent the charge, the two groups would follow similar labor market paths after the suspected offense.

This research design offers at least two key strengths compared to alternative approaches. First, the two groups likely share similar precipitating circumstances that led police to suspect them of similar offenses. These circumstances include the activities in which they engaged, the people with whom they associated, and their social and economic background and prospects.[23]

Second, it is reasonable to believe the two groups have engaged in similar conduct. In particular, prosecutor charge decisions are influenced by important factors unrelated to a suspect's underlying conduct. Local prosecutor offices vary in their resources and practices, as does the assigned prosecutor. Even among comparable offenses, cases vary in the availability and quality of evidence.

Data from the suspects register support the idea that charge decisions often vary for reasons unrelated to suspect conduct. The reason prosecutors most frequently cite for not pursuing a charge is "insufficient evidence". For dropped charges where a reason is specified, prosecutors cite "insufficient evidence" for 60% of offenses, compared to just 2% for "not guilty" and 14% for "not a crime". We interpret this as evidence that, for prosecutors, factors like evidence quality—rather than the nature of the suspected conduct itself–are key determinants in the charge decision.[24]

Consistent with an important role for prosecutor discretion, charging rates vary substantially across prosecutor offices for the same suspected offense and even for the same individual suspect. In cases handled by prosecutor offices (accounting for about 80% of suspected offenses),[25] prosecutors pursue charges for 55% of suspected offenses. The standard deviation in charge rates across offices is 12.4%. This variation remains substantial even after conditioning on the specific offense (10.3%)

[21] In practice, the control group also includes a small number of individuals who are charged but acquitted. These individuals also do not get a record. Excluding them from the analysis has no impact on our results.

[22] The names of individual crime suspects are rarely reported in Swedish media; two uncommon exceptions are for cases in the public interest (e.g., politicians) or when the police are searching for suspects of very serious offenses.

[23] This design also accounts for potential labor market consequences of being suspected or arrested, which may result from stress, reputational harm, or job disruption.

[24] This is also consistent with evidence from the US. See for example Frederick and Stemen (2012).

[25] As discussed in section 2.1, police typically handle less serious offenses. A small percentage of cases are handled by more specialized authorities, e.g. those involving complex financial crimes.

or both the offense and the suspect involved (8.7%).

Ultimately, we use the data to assess the validity of this research design. Specifically, we will show that, once matched, the treatment and comparison groups (1) hold similar jobs, have similar coworkers, and follow very similar earnings paths before the suspected offense and (2) are suspected of future offenses at similar rates.

5.2 Matching charged and non-charged suspects

Our goal is to create a sample of people who are charged with a criminal offense for the first time and match them with a comparison group who are suspected of a similar offense but not charged, under similar circumstances.

We collapse suspected offenses to the person-year level. For our core sample of events, we limit to person-years with at least one associated suspected offense where the person: (i) does not have a prior charged offense; (ii) is between the ages of 22 and 40 in the event year, and (iii) is present in Sweden (and hence, in our data) at least 4 years before and 5 years after the event. We make the first restriction to focus on the extensive margin of having any criminal record. (Later, we look at subsequent offenses.) We make the second restriction so that we can (1) observe pre-trends and compare earnings for charged and non-charged suspects before the event and when they are likely out of school and (2) evaluate long-term effects on earnings before retirement. Limiting to those 22 or older is the more meaningful age constraint: among those ever charged with an offense, 36% are charged between the ages of 15 and 21. Given that the suspect register begins in 1995 and we require five years of post-event data, our core sample of events occur between 1995 and 2010.

Table 4 provides descriptive statistics on person-year events meeting these criteria. Statistics refer to either the focal suspected offenses or the suspect's characteristics in the year before the focal event. In 34% of events, a suspected offender is charged with at least one offense.[26] We define these suspects as *charged*.

Charged and non-charged suspects are suspected of different types of crimes. Non-charged suspects are more likely to be suspected of a violent offense (48.81% vs. 25.63%), and less likely to be suspected of property (31.38% vs. 39.45%), traffic (5.57% vs. 28.13%), or narcotics offenses (4.68% vs. 11.86%). These differences likely stem in part from varying challenges in evidence collection. For example, in cases where someone is suspected of driving while intoxicated, evidence

[26] This is comparable to the share of criminal charges that are pursued in US state courts rather than dropped or dismissed (Feigenberg and Miller, 2021).

Table 4
Characteristics for suspects and suspected offenses

	All		Matched and Weighted	
	Not charged	Charged	Not charged	Charged
Age at time of suspected offense	30.54	29.54	29.56	29.56
	(5.628)	(5.709)	(5.732)	(5.732)
Year of suspected offense	2003.8	2003.0	2002.8	2002.8
	(4.544)	(4.658)	(4.717)	(4.717)
Order of offense	1.353	1.279	1.136	1.136
	(0.715)	(0.632)	(0.404)	(0.404)
Violent (%)	48.81	25.63	23.85	23.85
Property (%)	31.38	39.45	40.34	40.34
Traffic (%)	5.572	28.13	24.28	24.28
Narcotics (%)	4.677	11.86	5.877	5.877
Sex (%)	4.107	2.246	1.423	1.423
Other (%)	15.85	12.65	8.004	8.004
Any major offense (%)	39.2	26.2	20.9	20.9
Fine or less (%)	0	59.25	0	59.14
Probation and other (%)	0	31.98	0	33.28
Prison (%)	0	8.767	0	7.583
Male (%)	64.99	69.38	69.67	69.67
Swedish born (%)	69.04	71.77	73.13	72.82
Age	30.03	29.02	29.03	29.03
	(5.669)	(5.742)	(5.772)	(5.770)
Years of education	11.53	11.35	11.53	11.42
	(2.023)	(1.913)	(1.952)	(1.905)
Earnings (1000s SEK)	173.3	156.0	165.7	161.4
	(169.6)	(145.8)	(161.2)	(146.7)
Months employed year	8.222	7.990	8.138	8.128
	(5.168)	(5.221)	(5.190)	(5.187)
Ever employed during year	76.74	75.67	76.16	76.29
	(42.25)	(42.90)	(42.61)	(42.53)
Tenure in main job (in months)	37.09	34.77	35.72	35.73
	(36.43)	(34.41)	(34.70)	(34.93)
Workplace size	323.2	315.3	297.7	318.1
	(998.7)	(980.6)	(937.0)	(982.6)
Firm size	4934.7	4561.9	4346.3	4503.1
	(11595.2)	(11451.7)	(10926.2)	(11348.5)
Average firm coworker monthly wage (1000s SEK)	17.23	16.84	16.83	17.04
	(8.833)	(8.247)	(8.365)	(8.273)
% of firm coworkers with active records	6.511	6.789	6.438	6.519
	(9.079)	(9.343)	(9.238)	(9.062)
Firm pay premium at main job	-0.0577	-0.0555	-0.0588	-0.0478
	(0.262)	(0.258)	(0.263)	(0.255)
Agriculture and resources	3.204	3.653	3.881	3.854
Care and social services	11.24	10.52	9.652	10.08
Construction	6.686	7.660	7.837	7.769
Education	5.528	4.549	4.594	4.588
Health care	3.962	3.474	3.511	3.409
Hotels and restaurants	6.903	7.231	6.960	6.798
Manufacturing and industry	18.13	20.12	19.01	20.74
Public administration and defense	3.773	2.484	3.243	2.512
Retail and wholesale	13.85	14.82	14.70	15.21
Services	24.93	23.57	24.89	23.36
Unknown	1.780	1.898	1.681	1.665
Missing	0.0165	0.0125	0.0333	0.00627
Observations	158,932	81,261	100,186	60,302

This table describes the suspects and suspected offense combinations of people and years ("person-years") satisfying the sample criteria described in section 5.2. The first column includes all non-charged suspects. The second column includes all charged suspects. The third column includes all non-charged suspects that can be matched to charged suspects, re-weighting across cells to match the distribution among charged suspects. The construction of cells is described in detail in section 5.2. The fourth column includes all charged suspects that can be matched to non-charged suspects. The top panel describes suspected offenses in the event year. Because there can be multiple suspected offenses corresponding to an event, crime types sum to more than one. Order of offense indicates whether this is the first year, second year, or subsequent year that a person has been suspected of an offense. Sentence type refers to the most serious sentence for all suspected offenses in the event year. The bottom panel describes suspects in the year before the focal suspected offense. The construction of firm pay premiums is discussed in Appendix B.

from a breathalyzer test may meet the standard for both suspicion and conviction, making it rare to be suspected without ultimately being charged.

Charged and non-charged suspects are more similar in their demographic and job characteristics, though there are some key differences. Non-charged suspects are about three percentage points less likely to be Swedish-born (69.04% vs. 71.77%) and have higher baseline earnings (173.3K SEK vs. 156.0K SEK) and months of employment (8.222 vs. 7.990). Interestingly, the two groups work in similar industries and in establishments where 6% to 7% of coworkers have active records.

We match charged and non-charged suspects by partitioning events into detailed demographic and offense-based cells, then weighting non-charged cells to align with the distribution of charged cells. We define cells based on the following:

1. Year of suspected offense,
2. Suspect age in the year of suspected offense,
3. Number of prior years with suspected offenses,
4. Type and severity of suspected offenses,
5. Suspect gender, and
6. Suspect income in year before the suspected offense.

Offense types are broadly characterized as violent, property, narcotics, traffic, sex, and other offenses. Severity is categorized based on whether any specific suspected offense code falls in the top quartile of expected prison sentence length, conditional on a charge.[27] We denote these cases as *major* offenses.[28] We categorize income by dividing suspects into quintiles within each combination of year and age at the time of the suspected offense, including suspects with zero earnings.

We restrict our analysis to cells that contain both charged and non-charged suspects. While 56% of cells contain both types of suspects, these cells account for 74% of charged suspects. Columns 3 and 4 of Table 4 show the characteristics of events for our final sample of (weighted) non-charged suspects and charged suspects, which includes about 100,000 events in the comparison group and

[27] This roughly corresponds to the distinction between misdemeanors and felonies in the United States, a classification that is not used in Sweden.

[28] The five most common major offenses are: threat, fraud, vehicular theft, tax fraud, and fencing (receiving or dealing stolen goods). The five most common minor offenses (and most common offenses more generally) are: theft, non-DUI traffic (typically driving without a valid license), assault, narcotics use, and narcotics possession. For the top 10 major and minor offenses, Appendix Table H.9 shows the distribution of charge and sentencing outcomes.

60,000 events in the treatment group. Overall, the matching procedure leads to a modest shift in the distribution of charged suspects included in the analysis, with relatively fewer traffic, narcotics, and major offenses. This shift is consistent with those offenses being more likely to lead to a charge.

The same person may appear multiple times in the analysis. For example, a person suspected of an offense at ages 22, 26, and 30, but only charged at age 30 would contribute three events: two at ages 22 and 26 to the comparison group and one at age 30 to the treatment group. The final sample includes 158,570 events and 147,487 people. We cluster standard errors at the person level.

By construction, matched charged and non-charged suspects have identical suspected offense characteristics. They also have more similar demographic and job characteristics, even on dimensions that we do not match directly; the share Swedish born and average months worked are now statistically indistinguishable.

5.3 Motivating descriptive evidence

Before discussing the matched event study design results in the next subsection, we first describe how labor market outcomes evolve in the treatment group and compare criminal record status and subsequent offending for the treatment and comparison groups over time.

Figure 5 describes the labor market trends for the treatment group of charged suspects in our matched sample separately for those sentenced to a fine or less (59.14%), probation (33.28%), and prison (7.58%). Panels A and B plot months employed and earnings, respectively, in event time for charged suspects, where the event is the year of the suspected offense. For both outcomes, charged suspects are on upward trends before their suspected offense. In the year of the suspected offense and the following year, months employed decrease significantly and earnings decrease or plateau for those receiving probation and prison sentences. The fact that prison sentences are uncommon and brief (median length of 60 days in the matched sample) and earnings decrease significantly for probation sentences suggests a limited role for incapacitation effects in explaining the pooled effects we document below. For more minor offenses where the punishment is a fine or less, months employed plateau, while the change in earnings growth is more muted.

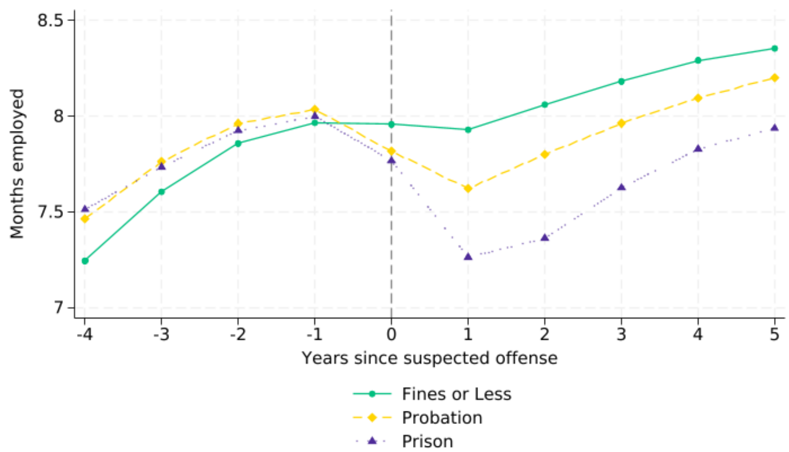
While the patterns in Figure 5 suggest that a charged offense has negative effects on labor market outcomes, they do not tell us what counterfactual employment and earnings would look like. In the next section, we compare trajectories like these for matched charged and non-charged suspects, where the latter group stands in for the counterfactual of the former.

We next look at the percentage of suspects with a criminal record. This is akin to a first stage.

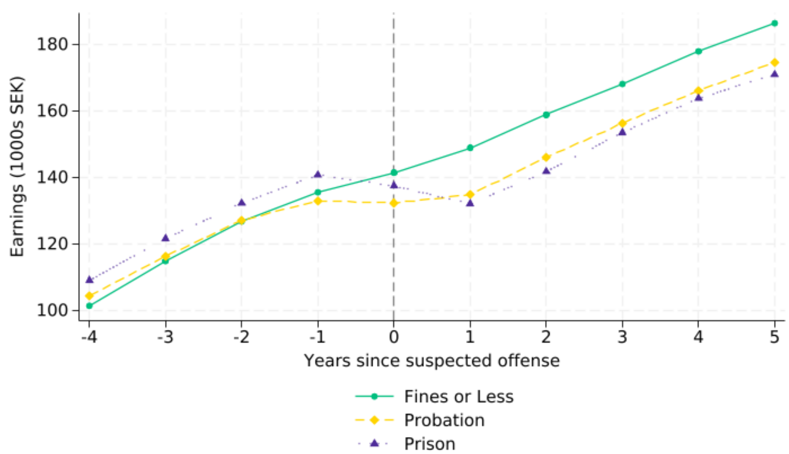
Figure 5

Employment and earnings after the first charged offense

(A) Months employed



(B) Earnings



Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between the ages of 22 and 40 at the time of the suspected offense; and (3) is present in Sweden at least 4 years before and 5 years after the suspected offense. Outcomes are plotted separately by the most serious sentence associated with suspected offenses in the event year.

Panel A of Figure 6 shows the (weighted) percentage of charged and non-charged suspects with an active record over event time. By construction, no one in either group has an active record before the event. In the event year, the percentage with a record in the treatment group increases to 38%. This is below 100% because some suspects are not convicted in the year of the alleged offense.[29] In addition, as discussed in section 2.3, some minor offenses were not included in background checks before 2000 and hence did not generate a criminal record. In the following year, the share with a criminal record increases to 71% and eventually reaches 99% five years post-event. The percentage of people in the comparison group with a record also increases after the event, as some non-charged suspects subsequently face charges for other offenses. Five years post-event, 13% of the comparison group has an active record, 86 percentage points less than the treatment group.

Finally, we examine the percentage of people who are suspected of an offense in a given year. A key concern with our research design is that the treatment group may be more likely at baseline to engage in criminal activity, independent of having a record. For example, they may be more likely to be guilty of the suspected focal offense. In that case, income differences between the treatment and comparison group that emerge after the event may reflect differences in future criminal behavior rather than the effects of a criminal record per se. On the other hand, acquiring a record may have criminogenic or, given expungement rules, deterrence effects.[30]

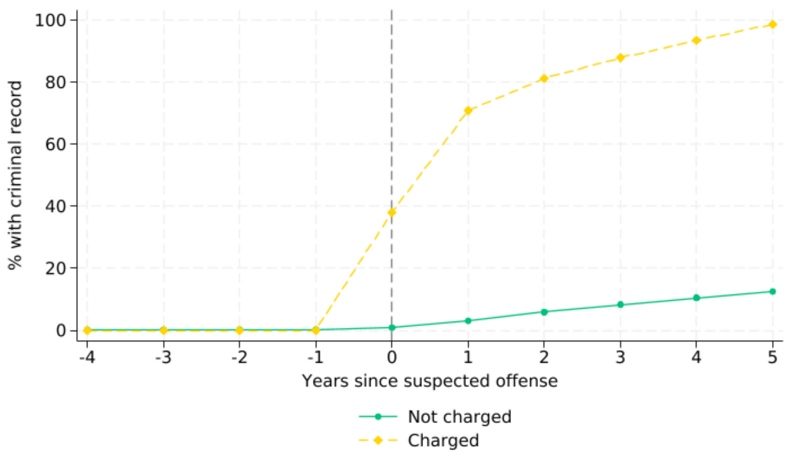
The results are shown in panel B of Figure 6. By construction, the share suspected of an offense is 100% for both the treatment and comparison groups at event time $t = 0$. The groups are matched on their number of years with suspected offenses before the event, though not on the timing of those suspected offenses. In practice, the two groups are suspected of offenses at nearly identical rates by year before the focal event. For example, in the year before the focal event, 3.9% of both groups are suspected of an offense. Following the focal event, the treatment group is suspected of offenses at slightly higher rates. One year post-event, 12.6% of the treatment group and 11.1% of the comparison group are suspected of another offense; by five years post-event, these rates are 7.7% and 6.5%. This modest difference may reflect slightly higher baseline propensities in the treatment group to engage in crime and/or slight increases in offending rates (or at least law enforcement suspicion rates) as a result of acquiring a criminal record.

[29] The median and mean time between the suspected offense and conviction is 5 and 8.5 months.

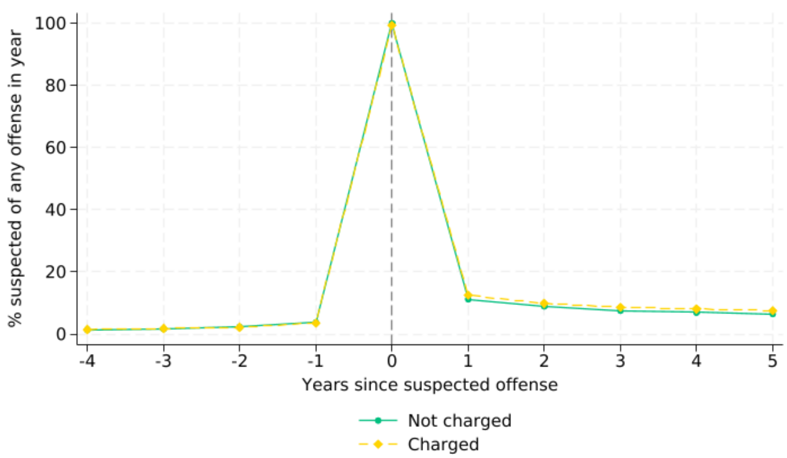
[30] Although incarcerated individuals make up a small fraction of our sample, a literature also studies the impact of prison on recidivism. Bhuller et al. (2020) present quasi-experimental evidence in Norway that incarceration decreases recidivism (i.e., future charges) for individuals who were not working prior to incarceration. Using the same judge design in the Swedish context, Dobbie et al. (2018) find no effect of incarceration on future convictions.

FIGURE 6
CRIMINAL RECORD STATUS AND SUSPECTED OFFENSES IN EVENT TIME

(A) Criminal record status



(B) Suspected offenses



Note: This figure plots the percentage of people with an active criminal record (panel A) and a suspected offense by year (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between the ages of 22 and 40 at the time of the suspected offense; and (3) is present in Sweden at least 4 years before and 5 years after the suspected offense. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects.

5.4 Results

We present results in three ways. First, we plot (weighted) means for charged and non-charged suspects in event time. Second, we estimate traditional event study models using two-way fixed effects and report coefficient estimates. Our matching strategy circumvents recent concerns with these models by restricting the estimation to "clean" comparisons between charged and non-charged suspects within cohorts (Roth et al., 2023). Third, in the Appendix, we nonetheless construct and report event study estimates using the imputation-based methodology of Borusyak et al. (2024). These estimates are similar to those derived from traditional event study models.

We examine months employed in panel A and earnings in panel B of Figure 7. Although the treatment and comparison groups are only matched to have similar earnings in the year before the focal event, the two groups have virtually identical employment rates in each pre-event year. Average months employed increase from about 7.5 months four years before the event to 8.25 months in the year before the event. Interestingly, among non-charged suspects, the employment rate plateaus in the year following the event, and continues to increase thereafter. This plateau could reflect the effects of whatever circumstances led that group to be suspected of a crime in the first place. Charged suspects see a decrease in employment both in the year of the suspected offense and the following years. A gap between charged and non-charged suspects of about 0.2 months persists for at least four years, which is about 2% of employment in the comparison group. This in part reflects a difference in the share of people with significant unemployment spells. Charged suspects are 1.5 percentage points less likely to work more than 6 months in the year, compared to a baseline of about 70% among non-charged suspects.

The two groups also follow very similar earnings profiles before the focal event, with earnings increasing by about 13,000 SEK each year. Non-charged suspects continue on this approximately linear trend after their focal suspected offense. By contrast, charged suspects experience a relative drop in earnings in the year of the focal suspected offense, and the gap between the treatment and comparison group grows to about 10,000 SEK by the year after the event. The gap in levels remains roughly constant for at least four years.

We replicate our findings using event study models of the form

$$Y_{i\tau t} = \alpha_{i\tau} + \gamma_{c(i,\tau),t} + \sum_{j=-4}^{5} \lambda_j D_{i\tau t}^{j} + \epsilon_{i\tau t} \tag{3}$$

where i indexes people, τ indexes the year of the focal suspected offense, and t indexes years. $\alpha_{i,\tau}$

are person by focal event effects, $\gamma_{c(i,\tau),t}$ are year effects that are specific to a given cell of suspects, $c(\cdot,\cdot)$, where cells are defined as above. The $D^j_{i\tau t}$ terms are event time indicators for the treatment group such that $D^j_{i\tau t} = D_{i\tau}\mathbb{1}\{t = \tau + j\}$, where $D_{i\tau} = 1$ if (i,τ) corresponds to a suspected offense that leads to a criminal charge. We cluster standard errors at the person level.

Figure 8 provides coefficient estimates ($\hat{\lambda}_j$) for months employed (panel A) and earnings (panel B). The same patterns emerge: five years post-event, months employed decrease by about 0.2 months and earnings decrease by about 10,000 SEK.

5.5 Heterogeneous effects

In this section, we measure the heterogeneous effects of acquiring a criminal record as a function of suspected offense characteristics, suspect characteristics, and contextual factors.

We estimate the event study specification (3) separately for each subsample. In Table 5 we report the five year post-event coefficients for both months employed (panel A) and earnings (panel B). The remaining coefficients are reported in Appendix Table H.13.

We first split the sample by whether the set of suspected offenses includes any major offense—about 20% of events. Estimates are reported in Columns 1 and 2. Effects are larger in absolute terms for more serious offenses. Months employed decrease by 0.15 for less serious offenses and 0.4 for more serious offenses. Differences in proportional effects are even larger because those suspected of more serious offenses have fewer months of employment at baseline. Months employed decrease by about 2% for less serious offenses and 4% for more serious offenses. Earnings effects follow a similar pattern, decreasing by about 4% and 8%, respectively.

We next look at whether a suspect is employed at the time of their suspected offense. Acquiring a criminal record could worsen labor market outcomes because employers care about whether a job applicant or employee has a record. But acquiring a record could also worsen labor market outcomes because the adjudication process itself is disruptive and makes it difficult for suspects to work at pre-existing jobs.[31] This induced job displacement may have negative long-term labor market consequences that are separate from the effects of a record per se.

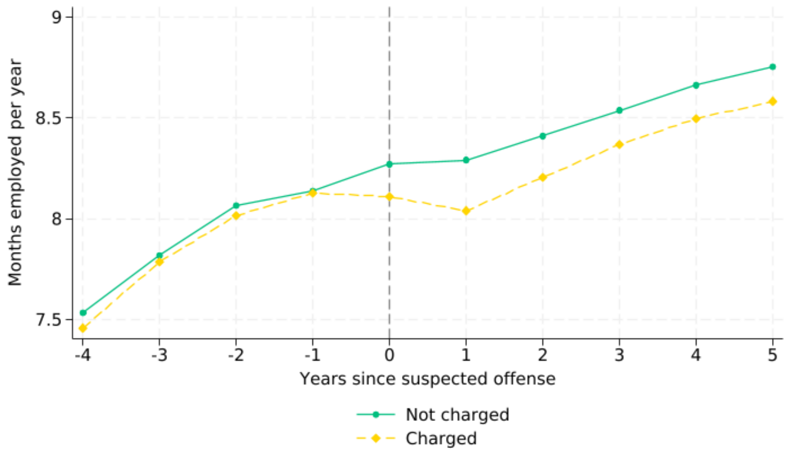
There is reason to be skeptical that job displacement plays an important role here. While prior research from Sweden finds that job displacement due to mass layoffs or plant closures reduces earnings by about 10% five years after displacement (Bertheau et al., 2023; Athey et al., 2024),

[31]For example, a suspect may have to attend court hearings at times when they would typically work, potentially leading them to separate from their job.

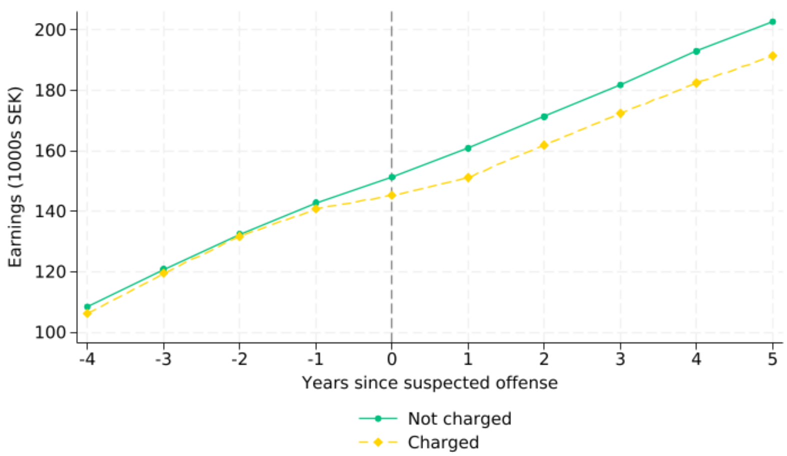
Figure 7

Acquiring a criminal record reduces employment and earnings

(A) Months employed



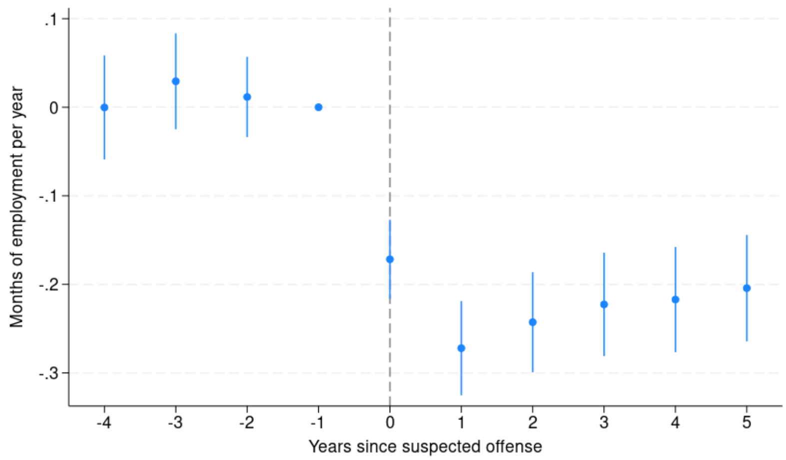
(B) Earnings



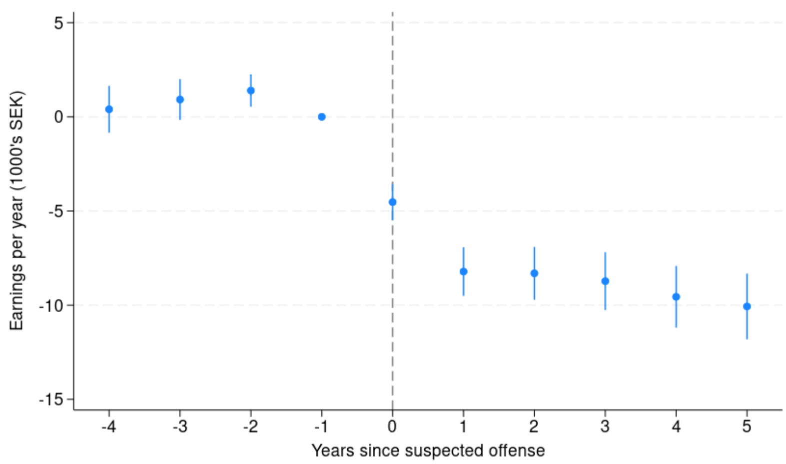
Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between ages 22 and 40 at the time of suspicion; (3) is present in Sweden at least 4 years before and 5 years after the suspected offense. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects.

FIGURE 8
EVENT STUDY ESTIMATES FOR EMPLOYMENT AND EARNINGS

(A) Months employed



(B) Earnings



Note: This figure plots the event time coefficients (λ_j) from the event study specification (3). In panel A the outcome is months employed. In panel B the outcome is earnings. Standard errors are clustered at the person level.

both charged and non-charged suspects experience high job turnover: five years after the suspected offense, only 24.6% of non-charged suspects and 23.5% of charged suspects remain with their pre-offense employer. Given this small difference in job turnover between charged and non-charged suspects, there is limited scope for job loss to explain the earnings decline we observe.

Nonetheless, we split the sample by employment status at baseline to clarify the role of job displacement. Sixty-six percent of charged suspects are working in the month of their suspected offense. If suspects with and without jobs both experience significant negative effects when charged, that suggests the role of job displacement is limited.

Columns 3 and 4 of Table 5 present results. As expected, those working at baseline have higher earnings. Yet, in proportional terms, the effects of acquiring a record are similar for both groups. We conclude that the effects we measure are not driven primarily by job displacement.

The remaining columns of Table 5 provide event study estimates for earnings effects by various additional splits. Columns 5 through 8 split the sample by offense type. We limit to events that include at least one of the following offense types: violent, property, traffic, and narcotics. Note that events including multiple types of suspected offenses are included in multiple samples. Columns 9 and 10 split the sample in half by suspect age. Columns 11 and 12 split the sample period in half.

Coefficient estimates are similar across crime categories, which suggests that the effects are driven by the record itself and not other potential penalties associated with selected crimes (e.g., the loss of a driver's license). The one exception is that effects are much larger for those suspected of a narcotics offense. In this subsample, a charge decreases earnings by 20,000 SEK to 25,000 SEK. This larger effect could be driven by firms having additional policies that target drug offenders. As seen in our job ads data (Appendix Table H.17), 21% of firms that signal background checks also signal drug and alcohol testing, compared to just 1% that do not signal background checks.

5.6 Additional analyses and extensions

5.6.1 Subsequent offenses

To this point, we have focused on the first time a person is charged with an offense, where that person is at the margin of having any criminal record. We also apply the same research design to look at the effect of being charged with an offense for the *second time*. We provide further details on this exercise in Appendix C. We find effects on months employed (5%) and earnings (10%) that are about twice as large as those associated with a first charged offense.

Table 5

Heterogeneity in the earnings effect of a charged offense

	By offense severity		By baseline status		By offense type				By age		By time period	
	Minor (1)	Major (2)	Unemployed (3)	Employed (4)	Violent (5)	Property (6)	Narcotics (7)	Traffic (8)	22–29 (9)	30–40 (10)	'95-'03 (11)	'04-'10 (12)
Panel A: Months employed												
λ_5	-0.13	-0.30	-0.07	-0.18	-0.17	-0.14	-0.66	-0.22	-0.19	-0.16	-0.14	-0.21
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
Non-charged outcome mean	8.55	7.86	3.76	10.30	8.43	7.82	8.14	9.23	8.40	8.29	7.99	8.65
Panel B: Earnings												
λ_5	-7.83	-14.31	-4.00	-10.90	-8.85	-7.64	-24.88	-9.23	-9.96	-9.06	-8.77	-10.29
	(1.01)	(1.88)	(1.43)	(1.16)	(1.40)	(1.44)	(3.62)	(2.67)	(1.11)	(1.42)	(1.20)	(1.32)
Non-charged outcome mean	168.99	152.30	50.84	213.16	165.23	142.66	139.65	200.00	145.50	181.59	144.48	180.68
Observations	1165480	420220	449150	1063670	597120	579170	71550	217680	801610	784090	773550	812150

This table shows event time coefficient estimates ($\hat{\lambda_j}$) from the event study specification (3), where the specification is estimated separately for various subsamples of events. Columns 1 through 4 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Columns 5 and 6 split the sample in half by suspect age: 22–29 in column 5 and 30–40 in column 6. Columns 7 and 8 split the sample period in half: 1995–2003 in column 7 and 2004–2010 in column 8.

5.6.2 Alternative within-group research design

Our research design leverages data on non-charged suspects as a comparison group. While we believe this approach has several appealing features, one limitation is that data on non-charged suspects are typically unavailable, making the design less broadly applicable. We explore an alternative event study specification used in prior research that relies only on data for people who are eventually charged (Grogger, 1995; Rose, 2021; Agan et al., 2024b). This alternative design effectively compares outcomes for those charged at younger versus older ages.

Appendix E presents the details and results for both months employed and earnings. We show results for the sample that is eventually charged and, as a placebo test, for the sample of suspects who are never charged.

There are three main takeaways. First, the alternative designs yields clear pre-trends, especially in employment, prior to the charge. Second, non-charged suspects experience similar pre-trends and also experience decreases in employment and earnings following the suspicion, though smaller in magnitude. Third, the design still suggests changes in earnings and employment around a charge that are of a similar order of magnitude as our main estimates.

This analysis highlights the limitations of the alternative within-group event study design as well as the advantages of incorporating non-charged suspects as a comparison group.

5.6.3 Referral networks in job finding

A criminal record could impact labor outcomes through both labor demand (e.g., firm preferences) and supply (e.g., job search) side channels. The focus of this paper, as much of the literature, is on the former. To the best of our knowledge, only one paper considers how job search changes upon interacting with the justice system, finding suggestive evidence of decreased job search and a shift from more active to passive methods (Smith and Broege, 2020).

We test this hypothesis in our charged versus non-charged suspects design by assessing whether a record impacts whether a worker appears to have been hired via referrals. Appendix Figure H.17 presents the results, where the dependent variables are the fractions of newly-hired workers who work at firms where either a family member or previous coworker works. There is little evidence of changes in job finding via networks.

5.6.4 Using the 2000 criminal record reform as a placebo test

Before 2000, offenses that received no punishment, a ticket, or a fine did not result in a criminal record. Starting in 2000, these minor offenses began to generate a criminal record that could be expunged after 5 years. The reform applied retroactively, affecting those charged with these minor offenses from 1995 to 1999. This retroactive change led to a sudden increase in the prevalence of workers with criminal records in 2000 (see Figure 3.)

The pre-reform period provides a placebo test for the suspects design. The 1995–1997 cohorts of charged suspects who received at most a ticket or a fine did *not* acquire a criminal record in the year they were charged or in the two subsequent years. If the suspects design isolates the effect of a visible criminal record, we should therefore *not* observe any effect on earnings or months employed at event times 0, 1, or 2 for these cohorts.

In Appendix F, we use the suspects design to compare earnings and employment effects for pre-reform cohorts (who did *not* acquire records) to post-reform cohorts (who did acquire records). We find no detectable effect on earnings and employment for the placebo pre-reform cohorts, but clear negative effects for the post-reform cohorts, supporting the validity of our research design.

5.7 Expungement

As described in section 2.3, records in Sweden are automatically expunged after a pre-specified number of years. Does expungement mitigate the earnings and employment penalties associated with a record? Studying the causal effect of expungement is complicated by its endogenous nature (i.e., expungement timing is a function of not recidivating).

We first study this question by applying the charged versus non-charged suspects research design to a restricted sample of events for which we observe labor market trajectories for over a decade. Panel B of Figure 9 demonstrates that the charged versus non-charged suspects earnings gap persists, and even expands, for up to 14 years after the record; yet, Panel A shows that only about 20% of the charged sample still has a record at this point.

Our second exercise re-centers the analysis around the expungement event for a sample of 233,049 individuals who: (i) were in the baseline sample of 18-55 year olds from 1990 to 2015, (ii) had an active record during this period, and (iii) were observed five years prior and subsequent to expungement. By construction, the share with a record decreases from 100 to 0% in the first year after the event (gray dots, panel C). Yet, average total earnings increase smoothly over time (black squares, panel C) and there are no visible discrete changes in annual months employed or share working at a new primary employer (gray dots and black squares, panel D).[32]

Analyses of Clean Slate laws in the US (Agan et al., 2024a) and New Zealand (Dasgupta et al., 2021) also find minimal effects of record expungement. The absence of an expungement effect in Sweden may reflect several factors. People may not adjust their job search behavior after expungement, either because they are unaware of the expungement (though this channel is ruled out by Agan et al. (2024a)), feel loyalty to their current employer, or lack the motivation or networks needed to find a higher-paying job. Alternatively, a criminal record may have scarring effects: reduced employment or sorting into lower-quality jobs could produce resume gaps and limit human capital accumulation, effects that employers may observe even after expungement.

6 The Role of firm heterogeneity

6.1 Decomposition by firm type

A central goal of this paper is to document whether and how the negative employment and earnings effects of a criminal record are mediated by employer heterogeneity. In this section, we ask whether acquiring a record causes WCRs to reallocate across employers—for example, from more to less discriminatory firms or non-employment—and how any decreases in wages are distributed across employers.

We first characterize firms by how a candidate's criminal record affects the chances that the firm hires them. We interpret this as a measure of how discriminatory or "WCR-friendly" an employer is. We then examine how the overall effects of acquiring a record on months employed, earnings, and wages are distributed across employer types.

We follow the intuition of our suspect research design in a more flexible way. Broadly, we ask: among recently hired workers with past suspected offenses, which firms are less likely to hire WCRs? If a firm's proportion of hires with records is unusually low compared to what we would expect based on the suspected offense history of its hires, we classify it as less willing to hire WCRs.

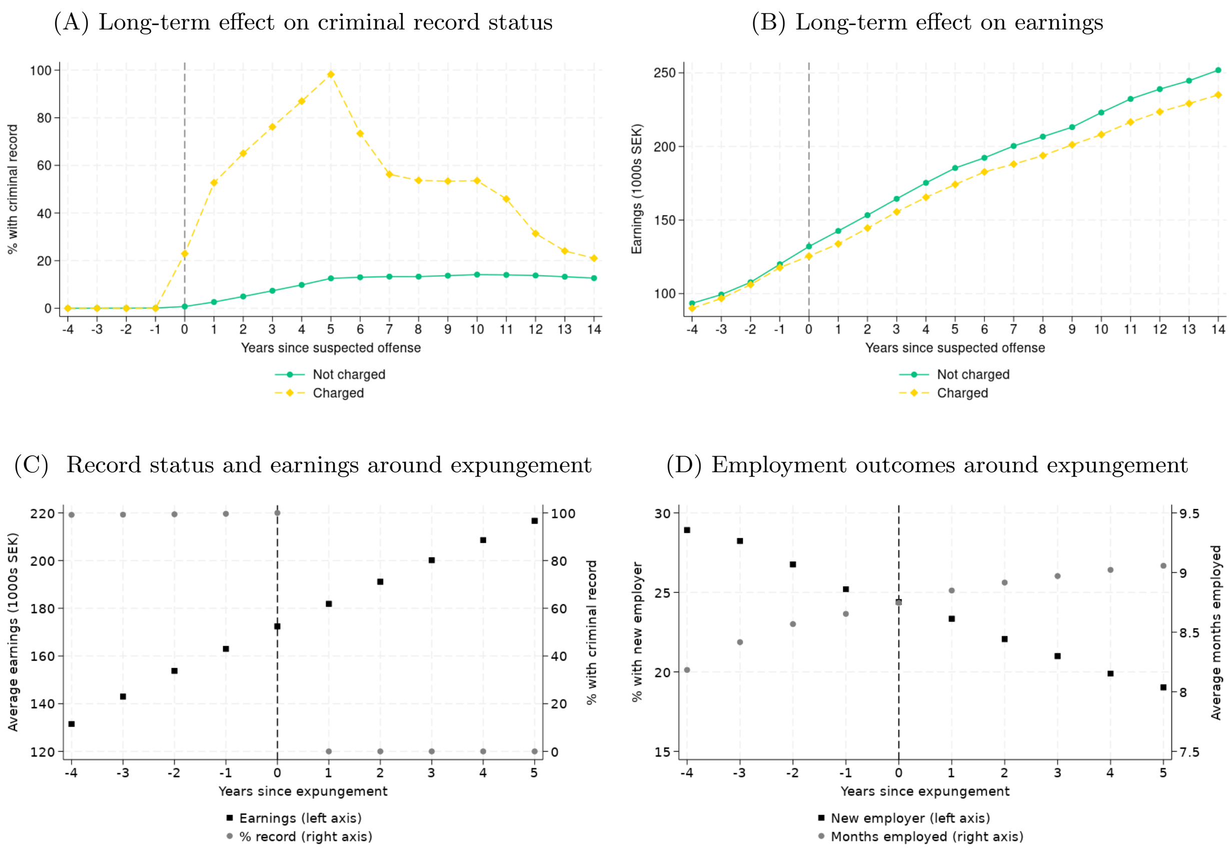
More concretely, we use data on new hires between 1995 and 2015.[33] We divide workers randomly into two groups. We use the first group to classify firms and the second group later to

[32]Appendix Figure H.16 shows that expungement occurs 5 to 21 years after the record, especially at six and eleven years, and that about 60% (20%) of expunged records were for property (violent) crimes. Appendix Figure H.18 demonstrates that the lack of an expungement effect in earnings is seen for a wide range of subsamples.

[33]We explicitly exclude firm operators from the sample because we later use operator characteristics to describe firms.

Figure 9

Expungement has no discernible effects on labor market outcomes



Note: Panel A presents the record status and panel B presents earnings of charged and non-charged suspects over a 14 year period after the focal suspected offense. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between ages 22 and 40 at the time of suspicion; (3) is present in Sweden at least 4 years before and 14 years after the suspected offense. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects. Panels C and D treat expungement as the event and are based on a sample of individuals for whom five years is observed before and after expungement. Panel C plots total earnings and record status in the years surrounding expungement while the annual average number of months employed and share with a new primary employer each year are traced out in panel D.

measure the heterogeneous effects of a criminal record by firm type. For each hire i at time t, we calculate their propensity score for having a criminal record that year based on their suspected event history. We denote this propensity by $P(\text{RECORD})_{it}$. To construct $P(\text{RECORD})_{it}$, we estimate a probit model for conviction for each suspect in each year they were suspected of a crime, focusing on the most serious suspected offense each year. We predict conviction based on age, gender, Swedish born status, suspected offense history, and suspected crime type. We then carry this probability of conviction forward for the number of years that a suspect convicted of said offense would have had a record (according to the expungement rules). We refer to the set of hires where $P(\text{RECORD})_{it} > 0$ as *at-risk hires.* Among all at-risk hires, 56% have a criminal record and the average value of $P(\text{RECORD})_{it}$ is 0.56.

Our goal is to measure firm-level tendencies to hire WCRs conditional on their hires' risk of having a criminal record. For each at-risk hire (i, t) we measure $\Delta_{it} = \text{RECORD}_{it} - P(\text{RECORD})_{it}$, the difference between whether that hire has a criminal record and their propensity score. We demean these values to have mean zero in each year. We define the firm measure as

$$\theta_j = E[\Delta_{it}|J(i,t) = j] = E[\text{RECORD}_{it} - P(\text{RECORD})_{it}|J(i,t) = j; P(\text{RECORD})_{it} > 0]. \quad (4)$$

where the expectation is taken over workers hired by firm j ($J(i,t) = j$). Most firms make few at-risk hires, so statistical noise is an important concern when classifying firms.[34] A firm that makes only a handful of at-risk hires may hire few WCRs by chance alone, even if they do not put weight on criminal records per se when hiring. To address this issue, we use an empirical Bayes (EB) approach (Walters, 2024). We construct a prior for each firm using a random forest trained with five features: 1-digit industry, 2-digit industry, number of employees, number of at-risk hires, and the proportion of their employees with criminal records. We estimate the sample analog of θ_j, $\hat{\theta}_j$, and take a weighted average of the same estimate and the prior to construct our EB measure, $\hat{\theta}_j^{EB}$. We put more weight on the prior when the sample estimate $\hat{\theta}_j$ is less precise. More details on our empirical Bayes procedure are provided in Appendix D.

We then categorize firms as follows. We take the non-charged suspects in the matched sample from section 5.2, limiting to the half of the sample *not* used to construct $\hat{\theta}_j^{EB}$, and look at the firms where they work during the event study window. We divide these firms into terciles based on their value of $\hat{\theta}_j^{EB}$, using the event-level weights used for matching described in section 5.2. We

[34]For the set of firms with any at-risk hires, the median firm has four at-risk hires.

label these firm terciles as “low propensity”, “medium propensity”, and “high propensity”, where propensity refers to how often a firm hires WCRs relative to other at-risk hires. Table 6 describes the workplace characteristics of non-charged suspects at these three categories of firms, weighted by person-years.

The average values of firm fixed effects, $\hat{\theta}_j^{EB}$, are -5.07, -0.69, and 3.35 at low, medium, and high propensity firms, indicating that when medium and high propensity firms hire at-risk workers, they are 4.4 and 8.4pp more likely to hire WCRs than low propensity firms, conditional on worker risk. The percentage of coworkers with criminal records is 3.1% at low propensity firms, 5.4% at medium propensity firms, and 11.7% at high propensity firms. Firm pay premiums decrease monotonically from low- to medium- to high-propensity firms. Compared to about 40% of low and medium propensity firms, only 3% of high propensity firms are in the public sector.

6.1.1 Employment and earnings changes by firm type

Next, we estimate event study specifications where the outcome is MONTHS EMPLOYED$_{i\tau t}$ or EARNINGS$_{i\tau t}$ interacted with an indicator for whether the firm where person i at time t is working is in a particular tercile. The coefficients can be interpreted as the employment or earnings losses (or gains) at a specific set of firms, unconditional on where people are working. For example, a criminal record could reduce earnings at a set of firms either because it decreases the chances of working at those firms, or decreases earnings conditional on working there. We estimate this model using the randomly selected half of workers that were *not* used to estimate $\hat{\theta}_j^{EB}$.

Event study estimates for each tercile are shown in Figure 10. The coefficients are additive: their sum across terciles roughly equals the total earnings effect, except for the fact that some small firms are not classified.

In describing the coefficients, we focus on the effects five years post-event. Months employed decrease by about 0.4 at low propensity firms. These decreases are partially offset by a 0.2 month *increase* in employment at high propensity firms. Earnings follow a similar pattern, decreasing most at low propensity firms (by about 9,800 SEK), less at medium propensity firms (by about 1,900 SEK), and increasing (by about 4,000 SEK) at high propensity firms.

Months employed at low-propensity firms decline slightly in the year preceding the suspected offense. However, this pre-trend does not appear for major offenses, even though the subsequent reallocation patterns are otherwise more pronounced (see Appendix Figure H.19).

Table 6
Firm descriptive statistics by tercile

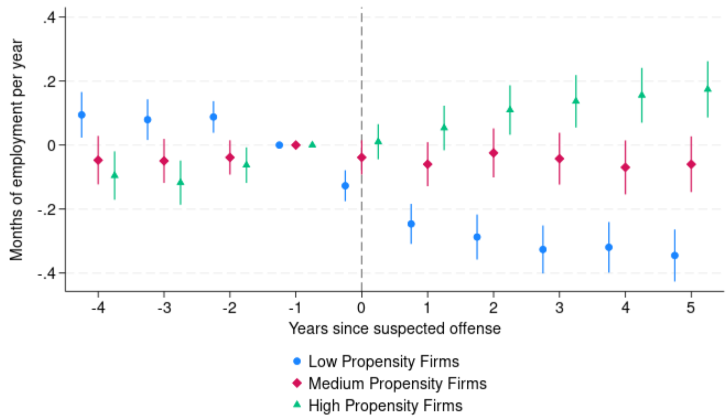
	Firm WCR hiring propensity		
	Low	Medium	High
Firm size	6126.7	7373.9	884.4
	(11430.3)	(14524.1)	(2934.3)
Average firm coworker monthly wage (1000s SEK)	18.23	15.04	15.13
	(10.44)	(7.735)	(8.798)
% of firm coworkers with active records	3.051	5.395	11.67
	(3.745)	(3.721)	(11.23)
CR firm effect ($\hat{\theta}_j^{EB}$)	-5.069	-0.689	3.350
	(5.333)	(0.558)	(3.037)
Firm pay premium	-0.0284	-0.0867	-0.103
	(0.261)	(0.232)	(0.309)
Log value added per worker	12.45	12.17	12.24
	(1.083)	(1.221)	(1.005)
Public sector	0.392	0.373	0.0346
	(0.488)	(0.484)	(0.183)
Firm industry:			
Agriculture and resources	1.313	1.888	4.514
	(11.38)	(13.61)	(20.76)
Manufacturing and industry	9.842	14.31	35.59
	(29.79)	(35.01)	(47.88)
Construction	2.491	4.599	11.55
	(15.58)	(20.95)	(31.97)
Retail and wholesale	13.10	10.92	10.06
	(33.74)	(31.19)	(30.09)
Hotels and restaurants	4.663	8.066	11.11
	(21.08)	(27.23)	(31.43)
Education	5.927	16.51	1.330
	(23.61)	(37.13)	(11.45)
Health care	21.45	23.05	1.345
	(41.05)	(42.12)	(11.52)
Public administration and defense	7.417	2.199	0.276
	(26.20)	(14.67)	(5.244)
Services	30.51	16.52	22.38
	(46.04)	(37.14)	(41.68)
N firms	16,725	9,240	31,814

This table reports descriptive statistics for three categories of firms employing non-charged suspects in our matched event study sample, weighted by non-charged suspect person-years. We take the non-charged suspects in the matched sample, limiting to the half of the sample *not* used to estimate $\hat{\theta}_j^{EB}$, and look at the firms where they work during the event study window. We divide these firms into terciles based on their value of $\hat{\theta}_j^{EB}$, using the event-level weights described in section 5.2. We label these firm terciles as "low propensity", "medium propensity", and "high propensity". Firm pay premiums are estimated using a standard worker-firm wage decomposition (Abowd et al., 1999; Card et al., 2016). See Appendix B for details.

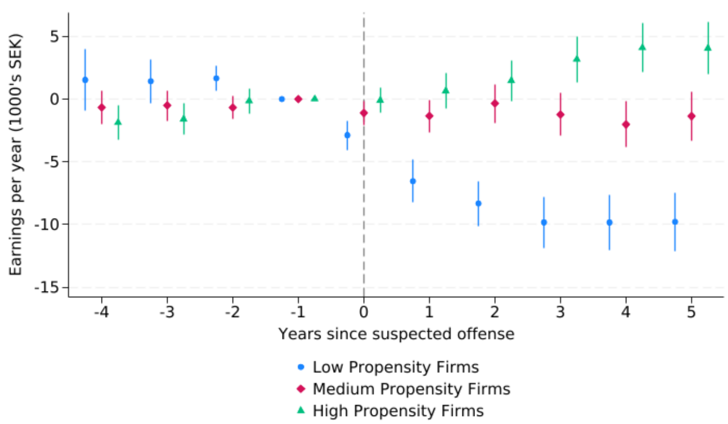
Figure 10

Employment and earnings losses are concentrated at more discriminatory firms

(A) Months employed



(B) Earnings



Note: This figure plots the event time coefficients ($\hat{\lambda}_j$) from the event study specification (3) for months employed (panel A) and earnings (panel B) interacted with indicators for three firm types: low propensity, medium propensity, and high propensity. See section 6.1 for a description of how firm types are constructed.

6.1.2 Wage changes by firm type

We find that a criminal record decreases both months employed and earnings, and reallocates workers from more to less discriminatory firms. In this section, we examine the effects of a record on monthly wages. We are particularly interested in the extent to which wage effects (1) reflect between-firm reallocation versus within-firm wage changes and (2) vary by firm type.

Studying wages is complicated by the fact that wages are only observed for realized worker-firm matches. To address this challenge, we simplify our research design by comparing suspects' observed wages before and after their suspected offense. We estimate regressions of the form

$$\log(\text{MONTHLY WAGE})_{i\tau t} = \beta\text{POST}_{i\tau t} + \gamma_{tg(i,\tau)} + \alpha_{i\tau} + \psi_{J(i,t)} + \epsilon_{i\tau t} \tag{5}$$

where $\text{POST}_{i\tau t}$ is an indicator for whether the suspect is charged and $t \geq \tau$, $\alpha_{i\tau}$ are person-event fixed effects, $\psi_{J(i,t)}$ are firm fixed effects, $g(i,\tau)$ groups events by the year of the offense (τ) and the suspect's age at the time of the suspected offense, and $\gamma_{tg(i,\tau)}$ are group by year fixed effects. We estimate versions of equation (5) with and without both firm fixed effects and interactions between $\text{POST}_{i\tau t}$ and the firm type. We limit the estimation sample to the half of the sample *not* used to estimate $\hat{\theta}_j^{EB}$.

The results are shown in Table 7. Column 1 excludes firm fixed effects. The coefficient on $\text{POST}_{i\tau t}$, -0.032, indicates that a criminal record reduces wages by 3.2 log points. Including firm fixed effects in column 2 reduces the coefficient to -0.024, indicating that nearly one log point of the wage decrease reflects a reallocation to lower-paying firms. The remaining wage decrease represents within-firm wage reductions. In column 3, we interact $\text{POST}_{i\tau t}$ with indicators for our three firm types: low, medium, and high propensity firms. Wages decrease by 3% at medium and high propensity firms, and by a (statistically insignificant) 1% at low propensity firms. The negligible effect at low propensity firms reflects the fact that those who are charged but manage to find employment at these firms are suspected of less serious offenses. If we restrict the analysis to those suspected of major offenses, wage effects are larger, especially at low propensity firms (see columns 4–6).

In Appendix G, we decompose the earnings decline caused by acquiring a record into three components: (1) reduced months of employment; (2) shifts from higher- to lower-paying firms; and (3) reduced monthly wages within firms. We attribute roughly 40% of the earnings decline to fewer months employed, 20% to shifts across firms, and the remaining 40% to lower wages within firms.

Table 7
Wage effects and firm heterogeneity

	All offenses			Major offenses		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-0.032	-0.024		-0.051	-0.041	
	(0.007)	(0.007)		(0.016)	(0.016)	
Low Propensity Firm × POST			-0.010			-0.051
			(0.010)			(0.025)
Medium Propensity Firm × POST			-0.029			-0.040
			(0.010)			(0.024)
High Propensity Firm × POST			-0.032			-0.033
			(0.009)			(0.021)
Firm FEs		✓	✓		✓	✓
Observations	503,551	503,551	503,551	120,242	120,242	120,242

This table shows estimates for the specification (5), where the outcome is log monthly wages. All columns include person-event fixed effects and fixed effects for the combination of the year, the year of the suspected offense, and the suspect's age at the time of the suspected offense. Columns 1–3 include all offenses, while columns 4–6 restrict to major offenses. Columns 2, 3, 5, and 6 include firm fixed effects. Columns 3 and 6 interact $\text{POST}_{i\tau t}$ with fixed effects for the three firm categories defined in section 6.1: low, medium, and high propensity firms.

6.2 Characterizing firm heterogeneity

Firm heterogeneity plays an important role in explaining the labor market outcomes of WCRs. Some firms are more likely than others to hire WCRs, and acquiring a criminal record shifts workers away from what we label "low" and "medium" propensity firms and toward "high" propensity firms. This section explores firm heterogeneity in hiring WCRs, focusing on four key characteristics: (1) industry, (2) size, (3) background check policies, and (4) operator exposure to WCRs.

We first describe the distribution of $\hat{\theta}_j^{EB}$ across firms.[35] Figure 11 plots the distribution of $\hat{\theta}_j^{EB}$ across all firms with at least one at-risk hire. There is substantial dispersion; the inter-quartile range is 8.0pp and the standard deviation is 9.8pp. One-digit industry explains only 2% of the variation in $\hat{\theta}_j^{EB}$, increasing to 7% when weighting by each firm's number of at-risk hires. The implied standard deviation of $\hat{\theta}_j$ is 12.7pp. Firms vary in their propensity to hire WCRs, suggesting that they differ in how they treat criminal records in hiring.

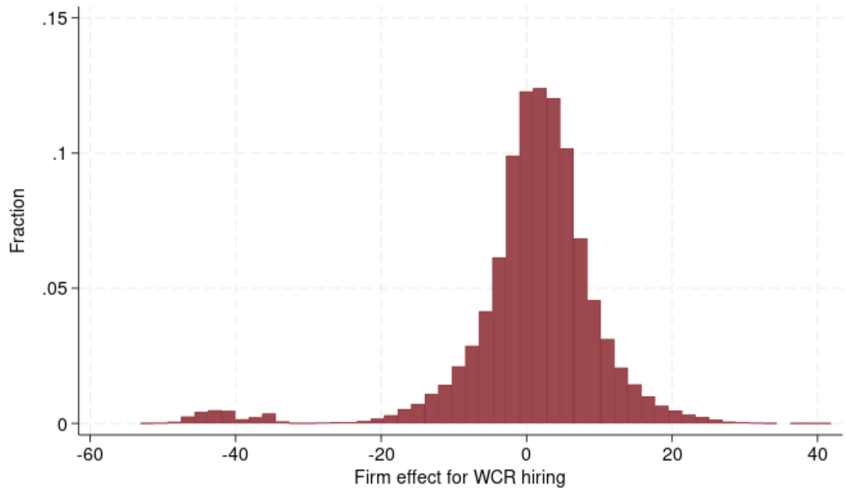
Table 8 correlates estimated raw firm effect estimates $\hat{\theta}_j$ (scaled by 100 to express in percentage points) with firm characteristics.[36] All columns include fixed effects for industry and firm size

[35]In this section we use $\hat{\theta}_j^{EB}$ and $\hat{\theta}_j$ estimates derived using all at-risk hires from 1995–2015 rather than a split sample.

[36]We use the raw firm effect instead of $\hat{\theta}_j^{EB}$ because empirical Bayes shrinkage is inappropriate when the variable of interest serves as the dependent variable in a regression (Walters, 2024).

Figure 11

Distribution of WCR hiring firm effects ($\hat{\theta}_j^{EB}$)



Note: This figure plots the distribution of firm effects for WCR hiring ($\hat{\theta}_j^{EB}$). See section 6.1 for a description of how these firm effects are constructed.

categories, where size is based on a firm's average number of employees per month.[37]

Column 1 highlights significant differences by industry. For example, compared to at-risk hires at services firms (the omitted category), hires at manufacturing and construction firms are about 2.4pp and 3.4pp more likely to have criminal records, respectively. For reference, recall that 56% of at-risk hires have a record. By contrast, hires in health care, education, and public administration and defense (three industries where mandatory background checks are often required at licensing or hiring) are about 2pp less likely to have a record. Among at-risk hires, larger firms are less likely to hire WCRs. Compared to firms with 1 to 10 employees, at-risk hires at firms with 101 to 250 employees and firms with 1001+ employees are 2.1pp and about 2.8pp less likely to have a record.

Firm heterogeneity in WCR hiring may in part reflect differences in explicit policy, including how background checks are used in hiring. Firms may vary in whether they conduct background checks, whether they signal background checks in job ads, and what they do when a candidate's background check is not clean.[38] Column 2 adds an indicator for whether a firm mentions background checks in at least one of their job ads. We limit the sample to firms that we can match to at least one job ad.[39] At-risk hires at firms that mention background checks in their ads are about 1.3pp less likely

[37]Firm size categories are defined as 1-10, 11-25, 26-100, 101-250, 251-1000, 1001-2500, and 2501+ employees. We categorize firms based on their average size and modal industry across at-risk hires.

[38]Measuring and linking such firm policies to real-world hiring outcomes (beyond a small survey or single firm) is rare in the personnel economics literature, especially for WCRs (Hoffman and Stanton, 2024).

[39]See Appendix Table H.17 for firm-level summary statistics characterizing job ad signaling behavior.

Table 8
Firm propensity to hire WCRs and firm characteristics

	(1)	(2)	(3)
Firm ever mentions background checks in ads		-1.295	
		(0.317)	
Operator has past record			1.642
			(0.171)
Operator has family with record			1.436
			(0.217)
Operator has high exposure to co-workers with records			1.867
			(0.156)
Industry			
Agriculture and resources	1.638	2.339	2.069
	(0.340)	(0.671)	(0.450)
Manufacturing and industry	2.354	2.681	1.759
	(0.168)	(0.321)	(0.218)
Construction	3.378	3.318	3.051
	(0.182)	(0.437)	(0.237)
Retail and wholesale	0.448	0.531	0.336
	(0.183)	(0.362)	(0.238)
Hotels and restaurants	1.642	1.887	1.653
	(0.180)	(0.357)	(0.239)
Education	-0.134	0.462	0.010
	(0.351)	(0.469)	(0.407)
Health care	-0.113	0.764	0.063
	(0.298)	(0.417)	(0.338)
Care and social services	0.407	0.200	0.553
	(0.320)	(0.639)	(0.427)
Public administration and defense	-1.258	-1.461	-2.558
	(0.851)	(0.978)	(0.802)
Services	0.000	0.000	0.000
	(.)	(.)	(.)
Firm size			
1-10	0.000	0.000	0.000
	(.)	(.)	(.)
11-25	-0.616	-0.397	-0.171
	(0.138)	(0.392)	(0.186)
26-100	-1.651	-1.219	-1.035
	(0.137)	(0.368)	(0.183)
101-250	-2.094	-1.485	-1.273
	(0.188)	(0.395)	(0.220)
251-1000	-2.759	-1.973	-1.794
	(0.217)	(0.394)	(0.223)
1001-2500	-2.730	-1.814	-2.031
	(0.325)	(0.454)	(0.291)
2501+	-2.810	-1.572	-2.842
	(0.302)	(0.476)	(0.311)
Observations	138,334	23,501	67,786

This table presents coefficient estimates from least squares regressions of estimated WCR hiring firm effects $\hat{\theta}_j$, described in section 6.1, on firm characteristics. Each observation is a private sector firm with at least one "at-risk" hire as defined in section 6.2 between 1995 and 2015. "Firm ever mentions background checks in ads" is an indicator variable for whether the firm mentions that they will conduct a background check in at least one of their job ads. "Operator has past record" is an average of an indicator variable for whether the firm's operator in a given year has ever had a criminal record in the past. "Operator has family with record" is an average of an indicator variable for whether the firm's operator in a given year as at least one family member with a record. "Operator has high exposure to co-workers with records" is an average of an indicator variable for whether the firm's operator in a given year has an above-median share of coworkers at previous establishments they worked at who had a record. For each operator exposure measure, the average is taken over the firm's "at-risk" hires from 2005 to 2015, the years where we observe the identity of a firm's operator.

to have a record.

In column 3 we test whether WCR hiring varies with whether the firm operator has past exposure to people with criminal records. Previous exposure could affect a manager's willingness to hire WCRs by, for instance, providing better information about their performance or decreasing prejudice (Paluck et al., 2019; Lepage, 2024). We study three sources of exposure: (i) whether operators themselves have had a criminal record in the past, (ii) whether an operator was exposed to an above or below median share of coworkers with criminal records at previous establishments in which they worked (regardless of whether they operated those establishments or not),[40] and (iii) whether an operator has a family member (parent, child, or sibling) with a criminal record. Because a firm's operator exposure can vary from year to year, we construct a firm-specific exposure measure by averaging annual operator exposure indicators across the firm's "at-risk" hires from 2005 to 2015–the period during which we observe operator identities.

For each exposure measure, we find that at-risk hires at firms with more exposed operators are 1.5pp to 2pp more likely to have criminal records.

6.3 Evidence on exposure from operator moves

The correlations between operator exposure and WCR hiring documented above may not reflect the causal effect of operators. For instance, firms more willing to hire WCRs may also be more likely to hire operators with records or promote WCRs into operator roles.

To further investigate whether operators influence WCR hiring, we leverage variation from new operator arrivals at firms in a differences-in-differences framework. We restrict our sample to 313,860 firms with 2 to 50 employees that hired at least one new worker between 2005 and 2015.[41] The firm size restriction focuses the analysis on workplaces where operators are more likely to influence hiring decisions (Åslund et al., 2014; Ronchi and Smith, 2024). We consider cases where a newly hired worker becomes the firm's operator, excluding the firm's first and last observed years in the sample. Each firm is assigned a unique operator per year. Eight percent of firms have at least one new operator arrival.[42]

For each operator arrival, we construct a measure of the change in WCR exposure between the incoming and outgoing operators. Exposure is again measured along three dimensions: (1)

[40]For each operator, we restrict to previous establishments with less than 100 workers to increase the likelihood that coworkers interacted in the workplace.

[41]We begin our analysis in 2005 since that is when information on firm operators was first made available.

[42]In rare cases where a firm has multiple new operator arrivals between 2005 and 2015, we consider only the first.

whether the operator has a criminal history, (2) whether a family member has a record, and (3) the average share of past coworkers with records at the operator's previous firms. For the binary exposure measures (own and family), changes take values of -1, 0, or 1. Exposure increases and decreases each make up 12–13% (own) and 9–10% (family) of transitions. The coworker measure is continuous, ranging from -1 to 1 (mean: 0.002; SD: 0.07). Because operator moves are uncommon and often do not change exposure, this exercise is data-intensive.

We estimate regression specifications of the form

$$RECORD_{ijt} = \gamma \text{POST}_{jt} + \beta \text{EXPOSURE}_{jt} * \text{POST}_{jt} + \zeta_{s(j)t} + \xi_{c(i)t} + \psi_{J(i,t)} + \epsilon_{ijt} \tag{6}$$

where $RECORD_{ijt}$ denotes whether hire i by firm j at time t has a criminal record, $POST_{jt}$ denotes the period after a new operator arrives at firm j, $\zeta_{s(j)t}$ denotes year-by-industry fixed effects, $\xi_{c(i)t}$ denotes year-by-commuting zone fixed effects, and $\psi_{J(i,t)}$ denotes firm fixed effects.[43] The coefficient of interest, β, captures the impact of the change in criminal record exposure between the previous and the new operator on the probability that a new hire has a criminal record. When estimating these regressions, we exclude (i) operators themselves from the pool of new hires and (ii) operators who are themselves convicted of a crime during our sample period. Although β is identified using firms with exposure changes from new operator arrivals, we include all firms (including those without operator changes) to improve precision.

Table 9 presents results for each of the three exposure types. Columns 1–3 focus on the operator's own criminal record history. Column 1 includes firm fixed effects but no other controls. The estimated $\hat{\beta}$ coefficient is 0.010, indicating that WCR hiring increases by 10% after the arrival of an operator with their own criminal record history, relative to an operator without one. Column 2 adds industry-by-year and commuting zone-by-year fixed effects. The coefficient is unchanged. Column 3 replaces the outcome with $P(\text{RECORD})_{it}$, the predicted probability that a new hire has a criminal record given their suspected offense history. The coefficient is positive though half as large in magnitude and statistically insignificant. This suggests that a hire's criminal record itself, not just correlated characteristics, influences hiring decisions.

Columns 4–6 consider exposure via past coworkers and otherwise follow the same structure as columns 1–3. Columns 4–5 show that WCR hiring increases by 5% following the arrival of an operator whose former coworkers included a one standard deviation higher share of WCRs.

[43] Commuting zones are local labor markets defined by Statistics Sweden. They are constructed using employee commuting patterns within geographic areas in Sweden in 2015. There are 69 commuting zones in our data.

Table 9

Changes in firm exposure from operator moves and WCR hiring

	Own record			Coworkers record			Family record		
			DV: P(rec)			DV: P(rec)			DV: P(rec)
Hire has a record	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
After arrival	-0.002	0.001	0.001	-0.001	0.001	0.001	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Exposure X After arr.	0.010	0.010	0.005	0.502	0.489	0.232	0.009	0.009	0.008
	(0.005)	(0.005)	(0.003)	(0.078)	(0.080)	(0.063)	(0.005)	(0.005)	(0.004)
Ind. X Year FEs		✓	✓		✓	✓		✓	✓
CZ X Year FEs		✓	✓		✓	✓		✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Outcome mean	0.102	0.102	0.076	0.104	0.104	0.076	0.103	0.103	0.076
% difference	10	10	7	5 (1 SD)	5 (1 SD)	3 (1 SD)	9	9	11
Observations	2,053,819	2,048,589	2,392,105	2,302,379	2,296,430	2,296,430	2,399,320	2,392,105	2,392,105

This table presents coefficient estimates from least squares regressions of the probability that a hire has a criminal record on an indicator for the period after the arrival of a new operator and its interaction with the increase in operator exposure at the firm after the arrival of the new operator. Measures of own and family exposure take a value of 0 or 1, so changes in exposure can correspond to -1 (exposure decrease), 0 (constant exposure), or 1 (exposure increase). Measures of exposure to past co-WCRs are continuous, so values for changes in exposure are also continuous and range from -1 to 1. Firms without a new operator are included in the regression with a value of 0 for change in exposure. Each observation is a worker hired for the first time at a firm between 2005 and 2015.

In column 6, the change in $P(\text{RECORD})_{it}$ is again half as large. Estimates remain virtually unchanged when restricting the sample to hires with whom the operator has never worked (90% of hires), indicating that the observed increases are not driven by operators simply rehiring the workers with records they were previously exposed to, but instead reflect broader changes in hiring behavior toward these workers.

Columns 7–9 examine family exposure. Columns 7–8 show that WCR hiring increases by 9% when the new operator has a family member with a record, though the effect is only marginally significant at the 10% level. In column 9, the change in $P(\text{RECORD})_{it}$ is similar in magnitude.

Finally, we ask whether the arrival of operators with more WCR exposure affects firm performance. For value added per worker, labor costs per worker, and profits, we find little evidence of substantive effects. As shown in Appendix Table H.18, most estimates are small, statistically insignificant, and often positive. These findings suggest that firms can increase WCR hiring without harming productivity, consistent with prior research (Lundquist et al., 2018; Minor et al., 2018).[44]

[44] Appendix Table H.19 uses balance sheet data on firms of all sizes between 1997 and 2015 and shows that increases in the share of employees with an active criminal record do not correlate with firm performance in any meaningful way.

7 Conclusion

This paper addresses four questions concerning the labor market outcomes of workers with criminal records: (1) How do workers with and without records differ in their labor market outcomes, including their earnings, employer, and occupation? (2) What is the causal effect of a criminal record on employment and earnings? (3) How does worker sorting across employers and jobs mediate this effect? (4) To what extent, and why, do employers vary in their hiring of WCRs? We conclude by summarizing and critically discussing our answers to these questions.

We provide the first nationwide and register-based estimates of the prevalence of criminal records in the workforce and demonstrate that demographically similar adults with and without records have markedly different labor market outcomes. These gaps are seen at multiple margins: WCRs earn less (31%), work less (20% fewer months per year), have lower wages, are sorted into lower-paying firms with disproportionately more coworkers with records, and are sorted out of occupations with mandatory background checks or records may be perceived as risky to a firm's customers, employees, or finances. These estimates indicate that WCRs are among the most disadvantaged groups in the Swedish labor market, with gaps larger than those observed, for instance, with respect to gender, education, and immigrant status (Åslund et al., 2021; Karimi and Palme, 2024).[45]

But are these labor market disparities caused by the record or are they attributable to selection on unobservables or simultaneity? Using our charged versus non-charged suspects research design, we find that a criminal record indeed has a negative causal effect on months employed and earnings, which persists for several years. The effect sizes are 2% and 5%, respectively, for the first such charge but about twice as large for more serious or subsequent charges. These penalties exist even in Sweden, a society that historically emphasizes the rights of WCRs, and we do not see a rebound in labor market outcomes after records are expunged. Moreover, these effects are not small; the earnings estimates are similar in size to the long-run effects of direct job displacement due to plant closure (e.g., 8% in Sweden (Athey et al., 2024) and 3% in Norway (Huttunen et al., 2011)) or estimates of the labor market costs to crime victims (Bindler and Ketel, 2022).

Our estimates are smaller than those reported in comparable U.S. studies, including a 30% earnings reduction for people with felony convictions in Washington State (Rose, 2021) and employment declines of 7–11% for misdemeanor charges and 8–26% for felony charges across several states (Agan

[45]These disparities are especially sizable given that a record includes all non-traffic ticket offenses, and not just offenses serious enough to result in incarceration. Recent estimates of the earnings penalty (using an occupation-based proxy for earnings) associated with US incarceration from 1880 to 1940 range from 5 to 15% (Ang et al., 2024).

et al., 2024b). [46] These differences could reflect variation in research design or differences between the Swedish and US labor markets and criminal justice systems. For example, the judicial process in the US may be more disruptive to defendants' existing work arrangements, and US employers may exhibit stronger or more widespread aversion to hiring WCRs.[47] Although we show that the research design applied in prior US-based studies is not suitable in our context, differences in design appear unlikely to account for much of the gap in estimated magnitudes. Consistent with recent US evidence (Agan et al., 2024a), we also find limited effects of automatic record expungement.

We also document substantial heterogeneity in hiring of WCRs across firms, even within industry and controlling for worker demographics. While a record decreases wages broadly across firms, we find that it also leads workers to sort away from more discriminatory employers (i.e., those with the lowest propensity to hire WCRs) and that these effects are partially offset by an increase in employment at less discriminatory firms. Finally, we present evidence regarding one specific mechanism underlying heterogeneity in WCR hiring across firms: exposure of firm management to criminal records. Using variation from firm operators moving across firms. we find that operators with a past criminal record themselves, or with more exposure to individuals with records through former coworkers or family members, are more likely to hire such workers. Moreover, we find no evidence that firm performance is harmed upon increases in a firm's WCR hiring.

The importance of firm heterogeneity, of the off-setting effects of WCR-friendly firms, and of the impacts of exposure, suggest a set of policies to increase the proportion of firms willing to hire WCRs. These include general policies incentivizing the employment of such workers (e.g., subsidies, quotas, employment programs after prison release) as well as firm-level policies to reduce the costs of hiring WCRs and increasing exposure (e.g., trial or probation periods, manager rotations).

[46] In contrast, Grogger (1995) found small and temporary employment and earnings impacts of arrest (and not conviction) in data from the early 1980s. Criminal background checks, even in the US, were much less common in this pre-Internet period.

[47] Bertheau et al. (2023) show that the earnings consequences of a work-related shock—job displacement—differ markedly across countries.

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A Data appendix

A.1 Criminal record expungement rules over time

This appendix summarizes changes to the criminal record expungement rules in Sweden between 1990 and 2020. The age of criminal majority in Sweden is 15, so no crimes committed before age 15 appear on a record.

All offenses, regardless of severity, are automatically expunged from one's criminal record. Once expunged, an offense disappears permanently from a record. The timing of expungement is a function of four factors: (i) age at the time of the offense (under 18 years versus 18 years or older), (ii) the sentence received, (iii) recidivism, and (iv) regulations in place in a given year.

Between 1990 and 1999, the expungement rules were the same for all persons 15 years or older. Crimes that did not receive punishment, a ticket issued by the police (*ordningsbo*t), a fixed fee fine (*penningbot*), or an income related fine (*dagsböte*r), did not result in a criminal record. All other crimes that led to a conviction generated a record that was expunged after 10 years. For those receiving a prison sentence or institutional placement, offenses were expunged 10 years after release. New offenses generated an extension of a criminal record such that an offense was not expunged until all offenses were expunged. Offenses that led to tickets or fines did not extend the expungement time.

Starting in 2000, all convictions that included lesser punishments (no punishment, tickets, and fines) now appeared on a criminal record and could be expunged after 5 years. This also meant that these crimes could suddenly appear on a person's criminal record extract in the year 2000. That is, this change to the expungement rules also affected those with fines received between 1995 and 1999, since these past offenses were also subject to the new expungement rules. As before, other crimes were expunged after 10 years, or 10 years after release from prison or institution. Committing a new offense extended the expungement time until the last crime was expunged. This extension rule now included those who received an income-related fine, but still excluded those who received a ticket or a fixed-fee fine. However, starting in 2000, no offense was allowed to stay on a record for more than 20 years (or 20 years after release from a prison or institution).

Starting in 2008, expungement rules began to differ by age. Offenses committed by someone under the age of 18 who admitted their guilt and was then given a decision of non-prosecution (AUL, *åtalsunderlåtelse*) were now expunged after 3 years. All other expungement rules remained the same.

In 2010, the expungement rules were changed in order to hasten the expungement of offenses committed before age 18. The automatic expungement time for non-prosecuted crimes remained 3 years, while most other crimes committed before age 18 were now expunged after 5 years. The exception to this rule was for crimes that led to an institutional placement (e.g., a locked youth facility). As before, these crimes were still expunged 10 years after release. Furthermore, recidivism continued to extend the expungement times of all previous offenses, including those committed before age 18. These rules remained in place until the end of 2020.

Appendix Tables A.1, A.2, and A.3 give a detailed description of the changes to Sweden's criminal record expungement laws over time.

TABLE A.1
SWEDISH LAW (1963:197) CONCERNING EXPUNGEMENT OF CRIMINAL RECORDS (1980 - 2000)

Paragraph	(2) Jan 1980	(3) July 1981	(4) July 1983	(5) Jan 1989	(6) July 1997
7 §					
Accused acquitted of the act by the court of appeal	w.d	w.d	w.d	w.d	w.d
Accused found guilty but not sentenced by the court of appeal	w.d	w.d	w.d	w.d	w.d
(Retrial) Accused acquitted from the charged act	w.d	w.d	w.d	w.d	w.d
(Retrial) Accused found guilty but not sentenced	w.d	w.d	w.d	w.d	w.d
The registered person has died	w.d	w.d	w.d	w.d	w.d
When 80 years have passed from his year of birth	w.d	w.d	w.d	w.d	w.d
The Court of Appeal has overturned the District Court's decision on conversion punishment for fines	–	–	w.d	w.d	w.d
(Retrial) The Court of Appeal has overturned the District Court's decision on conversion punishment for fines	–	–	w.d	w.d	w.d
Penal order removed	–	–	–	–	w.d
10 §					
Conditional sentence (1)	10	10	10	10	10
Probation (1)	10	10	10	10	10
Imprisonment according to ch. 28, sec. 3 of the Penal Code (Skyddstillsyn) (1)	10	10	10	10	10
Handover to special care (1)	10	10	10	10	10
Stay of execution of conversion punishment (1)	10	10	–	–	–
Imprisonment in other cases than referred to in 1	10	10	10	10	10
Internment	10	–	–	–	–
Conversion punishment for fines	–	–	10	10	10
New sentence before expungement of older record	Keep old records	k.o.r.	k.o.r.	k.o.r.	k.o.r.
11 §					
Imprisonment for no more than 1 year	5	5	5	–	–
Conditional sentence	5	5	5	–	–
Probation	5	5	5	–	–
Imprisonment as sentenced according to chapter 28, section 3 of the Penal Code	5	5	5	–	–
Stay of execution of conversion punishment	5	5	–	–	–
Under 18: Nothing is released	Yes	Yes	Yes	No	No
Conversion punishment for fines	–	–	5	–	–
Other					
Twenty year ceiling	No	No	No	No	No
Restrictive access 9 §	Yes	Yes	Yes	No	No

Numbers in columns (2) - (6) refer to years until automatic expungement. The letters w.d. stand for expungement "without delay". Entries in red signal changes made in that year.

Table A.2
Swedish law (1998:620) concerning expungement of criminal records (2000-2024)

Paragraph	(2) Jan 2000	(3) Jan 04	(4) July 05	(5) Jan 07	(6) April 08	(7) Sept 10	(8) Oct 11	(9) June 15	(10) Jan 16	(11) Jan 21
16 §										
Acquitted by the court of appeal	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Acquitted (after reopening of the case)	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Judgment, decision, penalty order, or order of summary punishment has been revoked	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Appellate court has revoked conversion punishment for fines	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Prosecutor's decision not to prosecute has been recalled	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Prosecutor's decision on warning punishment has been recalled	–	–	–	–	–	–	–	w.d	w.d	w.d
Restraining order has been lifted	–	–	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
European protection order has been lifted	–	–	–	–	–	–	–	–	w.d	w.d
Visitation ban has been lifted	w.d	w.d	w.d	w.d	w.d	–	–	–	–	–
Registered person has deceased	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
Contact ban according to the law on European protection orders has been lifted	–	–	–	–	–	–	–	–	w.d	w.d
17 §										
Released from prison (alt. conversion punishment for fines)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)
Prison sentence enforced through earlier deprivation of liberty	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)
Liberated by decision of clemency	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)
Prison or conversion punishment for fines has been dropped	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)
Sentenced to probation	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Conditional sentence (years after the sentence)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Closed juvenile care has been enforced	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (5)
Juvenile care, after sentence or decision	–	–	–	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Juvenile service, after sentence or decision	–	–	–	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Juvenile supervision, after sentence or decision	–	–	–	–	–	–	–	–	–	10 (5)
Handover to care within the social services (years after decision)	10 (10)	10 (10)	10 (10)	–	–	–	–	–	–	–
Care according to the law on care of abusers (years after decision)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Handover to forensic psychiatric care (after discharge)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)
Fines	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)
Declared free from sanctions	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (5)	10 (5)	10 (5)	10 (5)	10 (5)
Prosecutor's decision not to prosecute for crime	10 (10)	10 (10)	10 (10)	10 (10)	10 (3)	10 (3)	10 (3)	10 (3)	10 (3)	10 (3)
Decision on restraining order	–	–	–	–	–	–	10 (10)	10 (10)	10 (10)	10 (10)
Ban according to the law on European protection orders (after the decision)	–	–	–	–	–	–	–	–	10 (10)	10 (10)
Decision on entry ban	–	–	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)	5 (5)
Decision on visitation ban	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	10 (10)	–	–	–	–
Decision on extradition for crime	10 (10)	–	–	–	–	–	–	–	–	–

The table shows number of years until expungement (under 18 in parenthesis). Changes are highlighted in red. For 16 §, expungement is always "without delay" (w.d). Otherwise, expungement rules are stated as years after the prosecutor decision or court judgment, or years after release from prison or institution.

Table A.3

Continued ... Swedish law (1998:620) concerning expungement of criminal records (2000-2024)

Paragraph	(2) Jan 2000	(3) Jan 04	(4) July 05	(5) Jan 07	(6) April 08	(7) Sept 10	(8) Oct 11	(9) June 15	(10) Jan 16	(11) Jan 21
17a §										
Another state has notified expungement	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d	w.d
18 §										
New crime committed before the expiration of the time in section 17 (or 17a)[a]	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.	k.o.r.
Twenty-year ceiling	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The table shows the time until expungement (under 18 in parenthesis). For 16 §, expungement is always *without delay* (w.d). Otherwise expungement rules are years after the prosecutor decision or court judgment, or years after release form prison or institution. The letters k.o.r. stand for *keep old records*

[a] Does not apply if the new entry relates to a ticket (*ordningsbot*) or a fixed fee fine (*penningbot*).

A.2 O*NET data appendix

A.2.1 SOC to SSYK occupation code crosswalk

To merge O*NET occupational characteristics to Swedish employment registers, crosswalks are needed between the US and Swedish occupation codes. The O*Net data version we use contains the 2000 Standard Occupational Classification by the Bureau of Labor Statistics (referred to as SOC2000), while the Swedish register data use the Standard for Svensk Yrkesklassificering (Standard for Swedish Occupation Classifications). The Swedish occupation codes were updated in 2012, such that there are two versions: SSYK96 and SSYK2012. To the best of our knowledge there is no official crosswalk between the SOC2000 codes and the SSYK codes (96 or 2012). But, given that both the US and Swedish standards are based on the International Standard Classification of Occupations (ISCO), we use the ISCO-88 occupation codes to create a crosswalk between SOC2000 and SSYK96 codes.

To create the SSYK96-SOC2000 crosswalk, we used (i) an SSYK96 to ISCO-88 crosswalk obtained from Statistics Sweden, and (ii) an ISCO-88 to SOC2000 crosswalk obtained from the Bureau of Labor Statistics. The crosswalk from Statistics Sweden is, however, incomplete; thus, a research assistant manually completed the SSYK to ISCO crosswalk by using the SSYK and ISCO-88 look up functions. In a handful of cases, too much guesswork was required to confidently assign corresponding ISCO and SOC codes to the SSYK code, and thus the crosswalk is missing for these SSYK codes. We could also merge SOC2000 data onto the SSYK2012 codes given the existence of an official SSYK96 and SSYK2012 crosswalk from Statistics Sweden.

There are more than twice as many 6-digit US occupation codes as 4-digit Swedish codes. There is therefore not always a one-to-one mapping of US and Swedish codes in the crosswalk, but also many-to-one mappings of SSYK to SOC codes and one-to-many mappings of SSYK to SOC codes.

A.2.2 O*NET variables

We obtained the data dictionary and raw data for version 14 of the Occupational Information Network (O*Net) data from the **O*NET® Database Releases Archive**.[48] We used six raw data files: (i) *Occupation Data.txt*, from which we obtained occupation titles and SOC codes (at the 8-digit level), (ii) *Scales Reference.txt*, which indicates the scale of each survey question, (iii) Education, *Training, and Experience.txt*, from which we extracted the required level of education

[48] See https://www.onetcenter.org/db_releases.html.

for each occupation (1-12 scale), (iv) *Job Zones.txt*, which provides a job zone measure of how much preparation is needed for each occupation (1-5 scale, where 5 is extensive and 1 is little or none), (v) *Work Context.txt*, which signals (on a 1-5 scale) the extent of customer interaction and exposure to poor work environment characteristics, and (vi) *Task Statements.txt*, which includes a detailed task description, in which we searched for key words to measure exposure to vulnerable populations and opportunities for theft and financial responsibility.

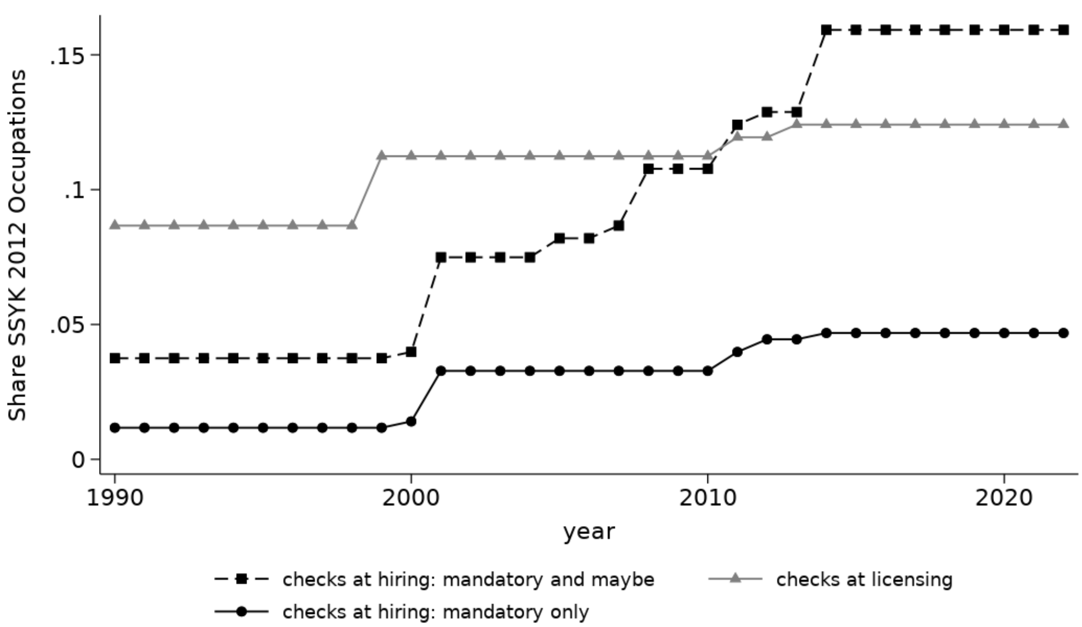
Our analyses use the average of all nine O*NET work environment conditions: exposure to contaminants; disease/infection; hazardous conditions; hazardous equipment; high places; minor burns, cuts, bites and stings; radiation; whole body vibration; and extremely bright or inadequate lighting. We proxy for exposure to vulnerable populations by searching for whether the core occupation tasks include the following key words: child, youth, student, handicap, disable, disability, group home, elderly, geriatric, old age, addict, alcohol, mentally ill, mental ill, and substance abuse. We proxy for exposure to opportunities for theft or financial responsibility by averaging whether there are any tasks (i) with opportunity for theft (key words: cash, payment, money, sell, sales, valuable) and (ii) signaling financial responsibility (key words: financial, budget, accounting).

We restrict our dataset to occupations with SOC codes of the form YY-ZZZZ.00, which allows us to use a 6-digit SOC code to perform a many-to-one merge match to the Swedish SSYK 4-digit codes in the crosswalk. Since some SSYK codes correspond to more than one SOC code, we collapse the data to the SSYK96 (or SSYK2012, when appropriate) occupation code level and use the mean O*NET characteristic. The final data set has 342 SSYK96 occupation codes and corresponding occupational characteristics. Finally, we transform the poor work environment variable, such that 0 is a poor condition and 5 is a good condition.

A.3 Mandatory background check laws and reforms

As described in section 3.3, we characterize jobs according to whether criminal background checks were required upon hiring or to obtain an occupational license. The latter are generally only conducted at the time the license is obtained, and not required retroactively upon reforms. Reforms have been rolled out that made background checks required for selected jobs and occupations. The table below lists the reforms we used (as well as the date of implementation) to code whether background checks would be required in each 4-digit occupation code in each year. We classify occupation codes as having mandatory checks (for all possible jobs), maybe background checks (for either some jobs or optional), and no background check requirements. Figure A.1 depicts the share of 4-digit occupation categories impacted by these laws over time. We assign the reform year such that it is the year of implementation for reforms in January to September and the following year for reforms in October to December.

Figure A.1
Roll-Out of Background Check Laws at Hiring/Licensing by Occupation



Note: This figure demonstrates the roll-out of mandatory and optional background check laws upon hiring and for occupational licensing by plotting the share off SSYK 2012 4-digit occupation codes covered each year. As each SSYK code includes multiple jobs, some occupation codes are only partially affected. These are captured in the maybe category, as are laws that say a background check is optional. An occupation falls into the mandatory only category in a given year if a background check is mandatory for all jobs in that occupation.

Table A.4
List of background check laws for hiring and occupational licensing

Year (Month)	Law and Description
Background Check Hiring Laws with Non-General Extracts from the Belastningsregister	
2001 (01)	Law (2000:873) on MANDATORY registry control of personnel in preschool activities, schools and after-school care
2007 (07)	Law (2007:171) on MANDATORY registry control of personnel at certain accommodations that receive children
2011 (01)	Law (2010:479) on MANDATORY registry control of personnel performing certain services for children with disabilities
2012 (08)	Education Act (2010:800) reinforces MANDATORY registry controls in list of different types of educational institutions
2013 (12)	Law (2013:852) on OPTIONAL background checks for persons working with children in other contexts
2018 (07)	Law (2018:1219) on MANDATORY registry controls for those working in insurance distribution
Occupational Licensing Laws	
1984 (07)	Law (1984:542) on the Eligibility to Practice Professions in Healthcare and Related Fields (included list of 10 occupations)
1999 (01)	Law (1998:531) on professional practice in the field of healthcare (expanded from 10 to 21 occupations in list)
2011 (01)	Patient Safety Act (2010:659) (minor changes, 22 occupations)
National Security and Ordinance Laws	
1969 (06)	Personnel Control Ordinance Law (1969:446) that gives 74 agencies the right do employee background checks. It also essentially makes it MANDATORY for the military to do background checks.
1981 (01)	Law (1980:578) that makes it MANDATORY to background checks for security guards.
1996 (07)	This Security Measures (1996:627) and Security Protection Ordinance (1996:633) replaces 1996:455, and expanded the list to 81 agencies.

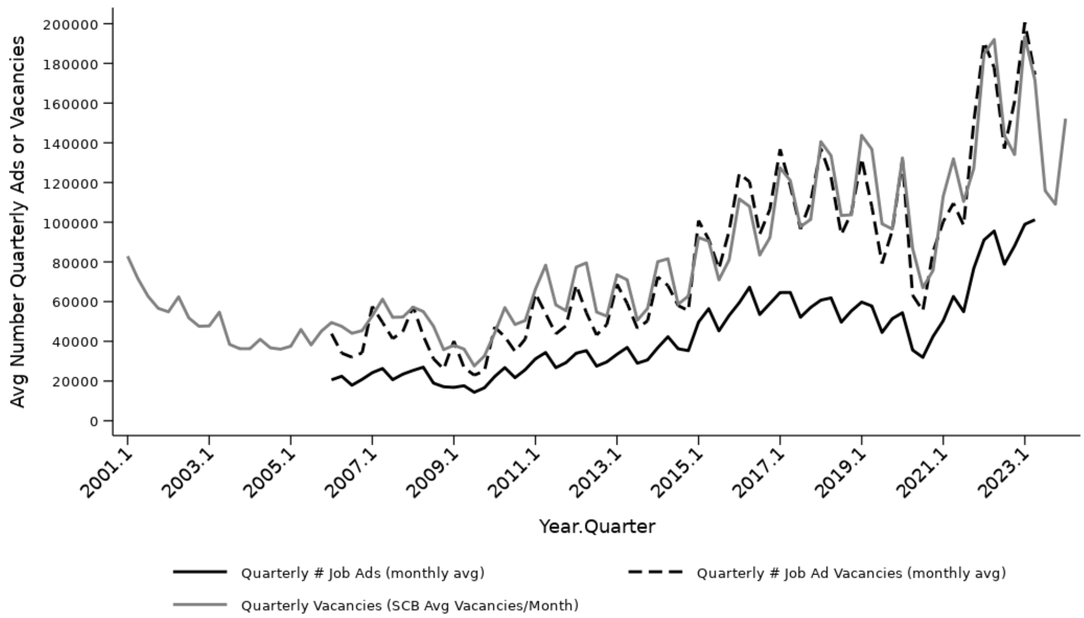
A.4 Job ad data appendix

We measure the extent to which Swedish firms signal that background checks will be conducted during hiring using a dataset of Swedish job advertisements from Platsbanken, the largest job board in Sweden.[49] The dataset spans 2006-2023 and includes over 9 million job ads; a total of close to 17 million vacancies (due to multiple jobs per ad). We create a unique ID variable for each ad and analyze the text to capture whether it signals that a background check will be conducted during the hiring process.

Platsbanken and the Job Ad Dataset: Platsbanken is maintained by the Swedish Public Employment Service (PES). Employers can post directly on the job board after registering an account or by special recruitment software using the job board's API. This job board is a central part of the Swedish labor market, given that unemployment insurance recipients have to register as unemployed at PES to receive benefits and that beneficiaries can be required to fulfill tasks, like active job search. Public sector jobs are required to post jobs on Platsbanken, while other vacancies were mandated prior to 2007. The number of ads on Platsbanken has significantly expanded over time, with the expansion of the labor market but also potentially changes to the platform to include new ad recruitment technologies in 2013 and an employer 'self-service' component added in 2017. Ads from other private job boards, e.g., Blocket, Monster.se and LinkedIn, were added starting in 2021.

[49] https://data.jobtechdev.se/annonser/historiska/monthly/

Figure A.2
Number of Platsbanken job ads/vacancies vs. national vacancy estimates



Note: This figure compares estimates of the quarterly number of job vacancies in Sweden from Statistics Sweden with the quarterly number of job ads on Platsbanken, and the corresponding number of vacancies. All data series present quarterly averages (across all months in the quarter) of the monthly number of ads or vacancies. The national vacancy data are sourced from: https://www.scb.se/hitta-statistik/statistik-efter-amne/arbetsmarknad/vakanser-och-arbetsloshet/konjunkturstatistik-over-vakanser-kv/.

To get a better sense of how representative Platsbanken is of the labor market, we compare in Appendix Figure A.2 the number of vacancies in Platsbanken to those reported in Statistic Sweden's monthly survey of firms asking for the number of vacancies (lediga jobb, which refers to job vacancies for which the firm has started external recruitment). As Statistics Sweden reports the average number of monthly vacancies per quarter, we collapse the job ad data down to the quarterly level and report the average number of monthly ads and average number of monthly vacancies. Two phenomena stand out. First, the trends in the these two time series are very similar, suggesting the Platsbanken captures business cycle variations over time in the labor market. Second, these two time series are very close together.

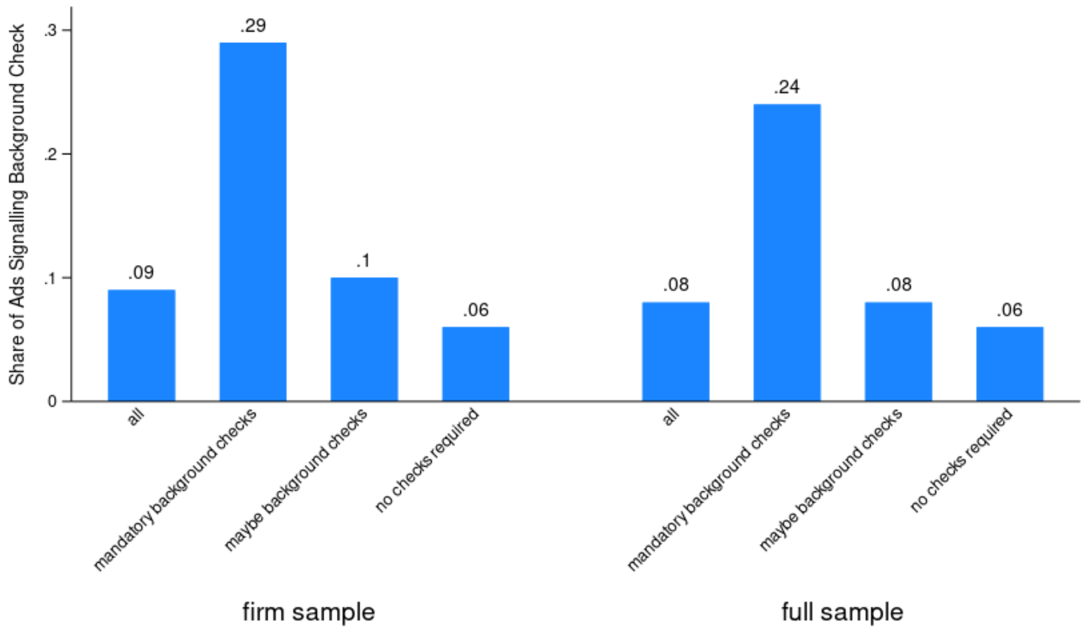
Key Variables: A key step of our analysis is to identify employers who signal - either implicitly or explicitly - that they will require a background check. We do this by searching the text for the following key words and creating a dummy for each hit: "belastningsregist", "registerkontroll", "bakgrundskontroll". We drew random samples of ads and manually looked at the text to confirm that these hits were indeed capturing background checks. We flag the ad as signaling a background check if any one of these key words are contained in the ad. We created a number of other variables, i.e., indicators for whether the ad includes other keywords related to drug- and alcohol-free work environments. The dataset also includes occupation codes, dates of ad posting, number of vacancies, position type (e.g., regular versus summer), position length, salary type (e.g., fixed versus flexible), and job location (of which municipality is the most complete). The main occupation code variable is one used internally by the team that maintains the ads database, but a mapping to ssyk96 or ssyk12 codes is available at https://gitlab.com/davidnoirman/legacy-id-to-ssyk. Finally, all ads include a firm name, which we use to code the employer type. We classify jobs as 'public' if the employer name contains 'municipality', 'region', 'county', or 'city'.

Firm Organization Numbers and Matching to SCB Registers: The final step is to match these job ads data to firms in the employer-employee register. This is straightforward for the 2021-2023 job ads since almost all of these ads included the firm's organization number. But, only firm names were included in earlier sample periods. We therefore took a number of steps to assign organization numbers to these ads. First, a *within-firm* crosswalk was created from the existing ids and mapped onto firm names in the earlier years. The next step was to scrape firm ids for the remaining firms. We chose to only use firm names with 10 or more ads in the 2006-2023 period. The first round of scraping was done on proff.se, a website that lists firm information such as revenue, phone number address and, firm ids. We scrape both the firm id and the firm name so that we can compare the scraped name to the name in the data frame. The second round used a search engine (duckduckgo.com) to retrieve even more firm ids from online searches. We consider a match between firm name and firm id as valid if (i) the source is the original id or from the within dataset crosswalk, (ii) the source was manually inspected by a research assistant, (iii) the scraped name is exactly the same as the firm name in the ads data set, and (iv) the first two or three words in the firm's name are also in the scraped firm's name.

Sample and Selected Summary Statistics: Our final data set includes 9,124,783 job ads; for 6,517,523 job ads, we have a firm identification number that could be matched by Statistics Sweden to our employer-employee registers. This includes 54,370 firms. For each firm, we calculate the share of job ads that signal a background check and whether the firm ever signaled a background check in one of its ads.

Figure A.3 demonstrates that the firm sample is fairly representative of the full sample of job ads in terms of background check signals: 9% of the firm sample and 8% of the full sample signal background checks. This is consistent with the firm sample containing firms that post more ads and survey evidence suggesting that larger firms conduct more background checks. Moreover, firms are more likely to signal background checks for 4-digit occupation codes that have mandatory checks (29%) than those that only maybe have checks (10%) or do not require checks (6%).

Figure A.3
Share of job ads signaling background checks by occupation type



Note: This figure presents the share of job ads signaling background checks in the full sample as well as in the sample of ads matched to firm IDs in the employer-employee register. It also reports the share of ads signaling checks for three categories of occupations: those that require mandatory background checks, those that may require checks, and those do not require checks.

B Estimating firm pay premiums

We model log monthly earnings as a function of additive worker and firm fixed effects Abowd et al. (1999). Each observation corresponds to a worker-firm-year combination. For each worker and year, we restrict to the firm where the worker earned the most that year. We drop observations where the monthly wage is below 2,500 SEK. Log monthly earnings w_{it} for worker i in year t are given by:

$$\log w_{it} = \alpha_i + \psi_{J(i,t)} + X_{it}'\beta + \epsilon_{it}, \tag{B.1}$$

where α_i are worker fixed effects, $\psi_{J(i,t)}$ are firm fixed effects, and $J(i,t)$ worker i's employer in year t. X_{it} is a vector of time-varying controls, including year effects and controls for worker age. The residual ϵ_{it} captures time-varying shocks to wages, including worker-job specific match effects, shocks to human capital, and other factors. Following Card et al. (2018a), in X_{it} we include a third-order polynomial in age and restrict the age profile to be flat at age 40 by omitting the linear age term and re-centering age at 40. We estimate equation (B.1) via OLS within the largest 'connected set' of firms, i.e., the largest set of firms that can be linked by a path of worker firm-to-firm movements. This connected set includes 99% of all monthly earnings observations.

We estimate approximately 7 million worker effects and 1 million firm effects. The standard deviation of worker effects is 0.428, and the standard deviation of firm effects is 0.224. The raw correlation between estimated worker effects and firm effects is 0.09. Appendix Table B.5 provides more descriptive statistics on the model estimates.

For the AKM approach to yield unbiased estimates of worker and firm effects, the following *exogenous mobility* condition must hold (Card et al., 2018b):

$$E[(\epsilon_{it} - \bar{\epsilon})(D_{it}^{j} - \bar{D}_{i}^{j})] = 0 \ \forall j,$$

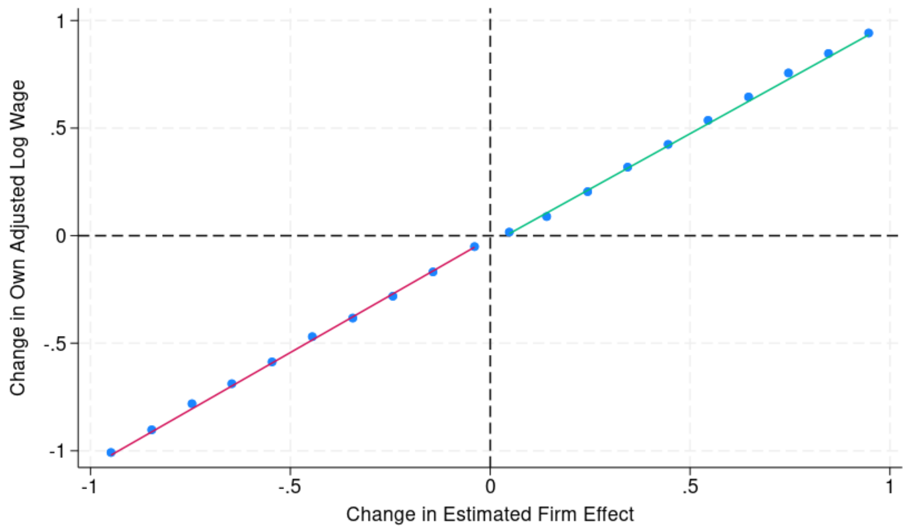
where $D_{it}^{j} = \mathbb{1}\{J(i,t) = j\}$ is an indicator for employment at firm j in period t. This condition states that workers must not systematically sort across firms based on transitory wage shocks or match-specific earnings effects.

To validate the AKM wage model, we follow specification checks similar to those proposed in Card et al. (2013) and Card et al. (2016). Specifically, we test whether firm-to-firm transitions produce abrupt earnings changes consistent with the exogenous mobility assumption.

Figure B.4 plots the relationship between earnings changes for firm switchers and changes in

Figure B.4

Earnings changes for movers by firm effect



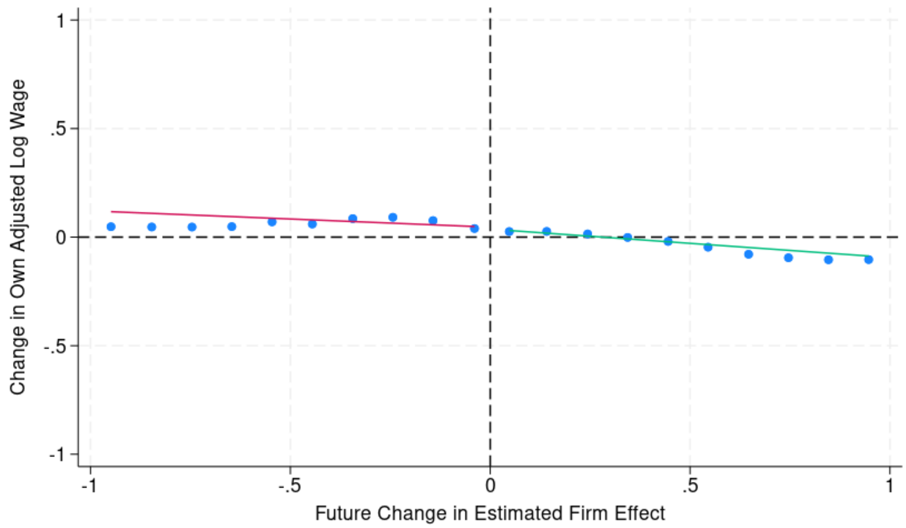
Note: This figure plots earnings changes for firm switchers (vertical axis) against the difference in estimated firm fixed effects between their old and new firms (horizontal axis). Each point represents a decile of changes in firm effects.

firm effects associated with these moves. Each point represents a decile of the distribution of firm effect changes. We observe a linear and symmetric relationship around zero, with an estimated slope close to one, consistent with the additive earnings structure in equation (B.1).

Next, we perform a placebo test. We select a sample of workers who switch firms in the future but have not yet moved. If workers anticipate or select future firms based on pre-move earnings trajectories, we would expect current earnings changes to correlate with the eventual change in the firm effect at the new firm. Figure B.5 shows no evidence of this relationship, consistent with exogenous mobility.

Figure B.5

Earnings changes for future movers by firm effect



Note: This figure plots earnings changes for future firm switchers (vertical axis) against the eventual difference in firm fixed effects between their current and future firms (horizontal axis). Each point represents a decile of changes in future firm effects.

Table B.5

Summary of AKM Estimates

	All
Worker and firm parameters	
# of worker effects	7,309,805
# of firm effects	1,011,294
Summary of parameter estimates	
Std. dev. of worker effects	0.428
Std. dev. of firm effects	0.224
Correlation of worker/firm effects	0.09
Adjusted R^2	0.621
Comparison match model	
Adjusted R^2	0.719
Addendum	
Std. dev. log wages	0.653
Sample size	90,154,064

We limit to each worker's firm with the highest earnings in a given year and drop wage observations where the monthly wage is below 2,500 SEK. Limited to largest connected set, which includes 99% of employment. Sample restrictions are described in further detail in section 3.2.

C Subsequent offenses

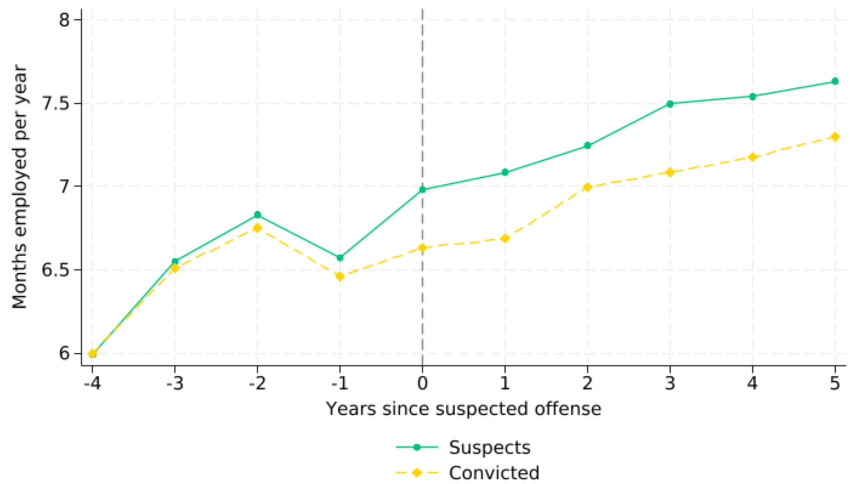
We construct our estimation sample and weights as in 5.2, except we limit to suspected offenses where the suspect has been charged exactly once in the past, and in the past five years. Again we restrict our analysis to cells that contain both charged and non-charged suspects and weight non-charged suspects to match the distribution of charged suspects. The number of events meeting our sample criteria is smaller and hence it is more difficult to find matches. Cells containing both types of suspects account for 28% of charged suspects. Appendix Table H.15 shows the characteristics of events. Our final example includes 14,233 events in the treatment group and 15,246 events in the comparison group.

Figure C.6 plots months employed in panel A and earnings in panel B for charged suspects and (weighted) non-charged suspects. Months employed are generally increasing for non-charged suspects, except for a dip in the year before the focal event. A gap of about 0.4 months between charged and non-charged suspects emerges in the event year and the following year. The earnings gap emerges more slowly, with a difference of about 8,000 SEK (7% of mean among non-charged suspects) one year post-event and 16,000 SEK (10%) two years post-event. The larger effects of a second charge could arise because second offenses are more likely to result in incarceration (10.6% versus 7.6%).

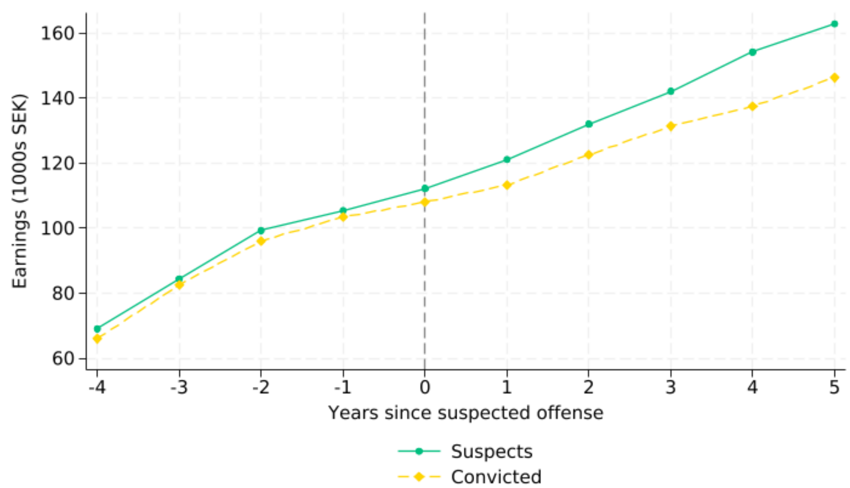
Figure C.6

Subsequent charges also reduce employment and earnings

(A) Months employed



(B) Earnings



Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.6.1. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between ages 22 and 40 at the time of suspicion; (3) is present in Sweden at least 4 years before and 5 years after the suspected offense. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects.

D Empirical Bayes estimation

We use an empirical Bayes approach to estimate firm effects for WCR hiring. Our goal is to measure firm-level tendencies to hire WCRs conditional on their hires' risk of having a criminal record, defined as

$$\theta_j = E[\Delta_{it}|J(i,t) = j] = E[\text{RECORD}_{it} - P(\text{RECORD})_{it}|J(i,t) = j; P(\text{RECORD})_{it} > 0]. \quad \text{(D.1)}$$

where the average is taken over workers hired by firm J. We often observe few at-risk hires for a given firm, so the sample mean of Δ_{it} for a firm's observed hires is often a noisy estimate for θ_j. We take an empirical Bayes approach to appropriately shrink our θ_j estimates and to make statements about the distribution θ_j across firms, accounting for estimation noise.

Our approach closely follows Walters (2024). The first step is to construct estimates of θ_j for each firm, $\hat{\theta}_j$. Our estimate $\hat{\theta}_j$ is the mean Δ_{it} for all (i,t) such that $J(i,t) = j$. The standard error of this mean is s_j. We also observe firm characteristics Z_j (industry, size, proportion of employees with criminal records). We assume the $\hat{\theta}_j$ estimates are unbiased, normally distributed, and mutually independent, with sampling variances equal to their squared standard errors:

$$\hat{\theta}_j|\theta_j, s_j, Z_j \sim \mathcal{N}(\theta_j, s_j^2). \quad \text{(D.2)}$$

We assume that θ_j follows a normal mixing distribution where the mean depends on characteristics Z_j such that

$$\theta_j|s_j \sim \mathcal{N}(\mu(Z_j'), \sigma_r^2). \quad \text{(D.3)}$$

We estimate the function $\mu(\cdot)$ using a random forest. We train the model using five features: 1-digit industry, 2-digit industry, number of employees, number of at-risk hires, and the proportion of their employees with criminal records. We use the R function `ranger`.

We estimate the hyperparameter σ_r^2 with the estimator:

$$\hat{\sigma}_r^2 = \frac{1}{K}\sum_{j=1}^{K}\left[(\hat{\theta}_j - \hat{\mu}(Z_j)^2 - s_j^2\right]. \quad \text{(D.4)}$$

In estimating $\hat{\sigma}_r^2$, we account for the statistical noise associated with our θ_j estimates by subtracting away s_j^2.

Finally, we combine our noisy estimates $\hat{\theta}_j$ and our estimates of μ_θ and σ_θ^2 to form posteriors for each θ_j.

Our empirical Bayes (EB) shrinkage estimate is given by

$$\hat{\theta}_j^{EB} = \left(\frac{\hat{\sigma_\theta^2}}{\hat{\sigma_\theta^2} + s_j^2} \right) \hat{\theta}_j + \left(\frac{s_j^2}{\hat{\sigma_\theta^2} + s_j^2} \right) \hat{\mu}_\theta. \tag{D.5}$$

The EB estimate shrinks the unbiased estimate $\hat{\theta}_j$ for firm j towards the mean for all firms with similar characteristics.

E Alternative research design

In this section, we provide more details on the alternative event study specification relying only on data for people who are eventually charged. It effectively compares outcomes for those charged at younger versus older ages.

Specifically, using our sample of charged suspects, we estimate

$$Y_{i\tau t} = \alpha_i + \gamma_t + \delta_{a(i,t)} + \sum_{j=-4}^{5} \lambda_j D_{it}^{j} + \epsilon_{i\tau t} \tag{E.1}$$

where α_i are person fixed effects, γ_t are year fixed effects, and $\gamma_{a(i,t)}$ are age fixed effects. The $D_{i\tau t}^{j}$ terms are event time indicators such that $D_{i\tau t}^{j} = \mathbb{1}\{t = \tau + j\}$. We pool observations at the left endpoint $D_{i\tau t}^{-4}$ such that $D_{i\tau t}^{-4} = 1$ if $t \leq \tau - 4$. For each person, the sample includes all years from age 18 through five years after the first charged offense.

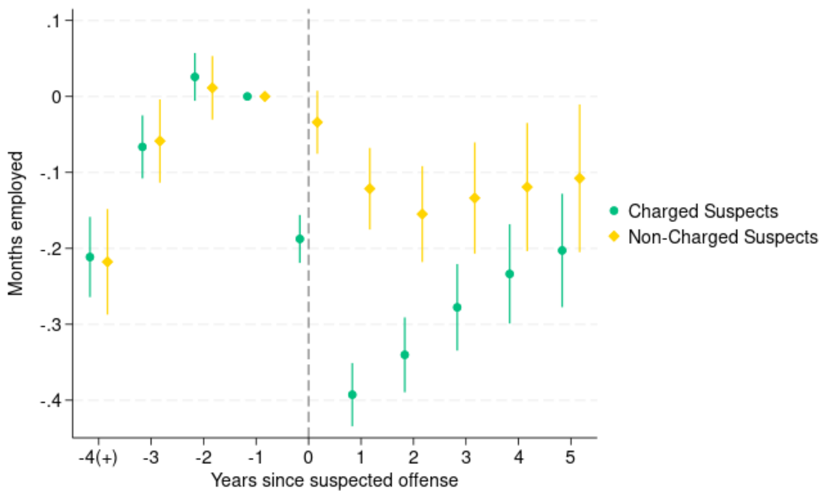
We estimate specifications for both months employed and earnings. We also estimate (E.1) using suspects who are never charged, using the first suspected offense as the focal event. This serves as a placebo test, assessing what the alternative research design would suggest in a group that does not ultimately acquire a criminal record.

Appendix Figure E.7 plots the estimated coefficients $\hat{\lambda}_j$ for months employed (panel A) and earnings (panel B). In panel A, for charged suspects, there is a clear upward pre-trend leading up to the event. Months employed drop by 0.4 months after the event and recover by about 0.2 months by five years post-event. This estimated decline is similar in magnitude to our main estimate shown in Figure 8. Interestingly, non-charged suspects exhibit a similar pre-trend, followed by a smaller post-event decrease.

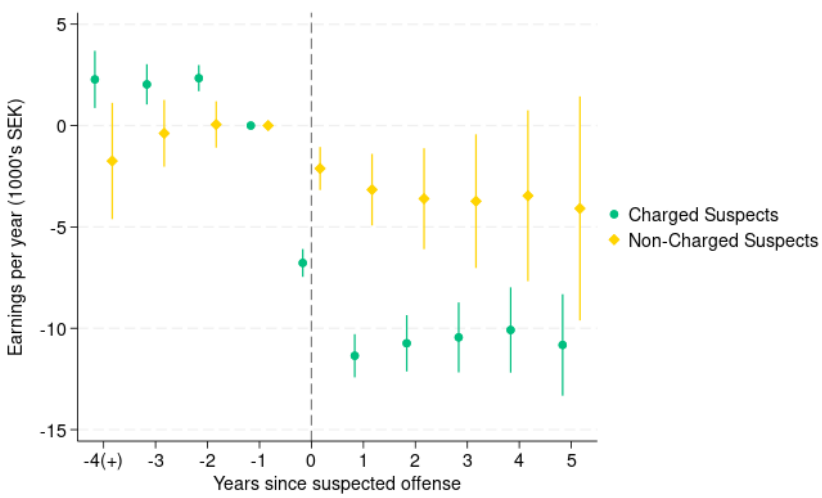
For earnings (Panel B), pre-trends are subtler and diverge between groups—slightly upward for non-charged suspects and slightly downward for charged suspects. Charged suspects show a post-event earnings decline similar in magnitude to our main estimate from Figure 8. Again, non-charged suspects experience a decrease in earnings as well, though somewhat smaller.

Figure E.7
Employment and earnings effects using alternative within-group design

(A) Employment



(B) Earnings



Note: This figure plots the event time estimates ($\hat{\lambda_j}$) from the alternative event study specification (E.1) described in section 5.6.2. In panel A the outcome is months employed. In panel B the outcome is earnings. For each outcome, the model is estimated separately using charged suspects and suspects who are never charged.

F The 2000 criminal record reform

Before 2000, criminal offenses that received no punishment (AUL, *åtalsunderlåttelse*), a ticket issued by the police (*ordningsbot*), a fixed fee fine (*penningbot*), or an income related fine (*dagsböter*), did not result in a criminal record. Starting in 2000, these types of lesser offenses did generate a criminal record that could be expunged after 5 years. Past offenses were subject to these new rules, so that they also affected those with convictions between 1995 and 1999.

The pre-reform period provides us with a placebo test for our suspects design. The 1995-1997 cohorts of charged suspects who received (at most) a ticket or a fine had no criminal record in the year that they were charged nor in the two years after the charge (i.e., at event times $\tau = 0$, 1, and 2). They obtain criminal records in 2000, which means that these cohorts are partially or fully treated at event times $\tau = 3$, 4, and 5.

Our expectations concerning the effects of the 2000 reform are as follows. First, we should not see an effect on earnings and months employed for the 1995-1997 cohorts of charged suspects at event times 0, 1, or 2. This is our placebo test. Second, we may see detrimental effects for the 1995-1997 cohorts at event times 3, 4, and 5. Third, we expect to see detrimental effects on cohorts who receive tickets or fines after 2000.

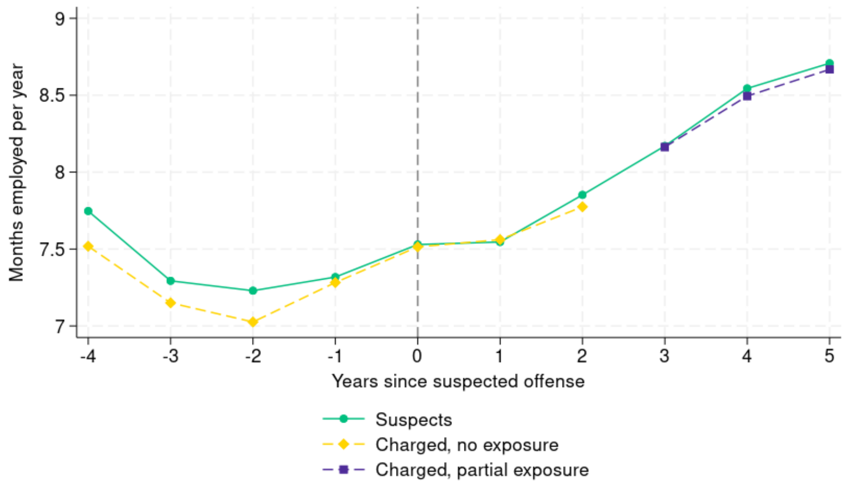
We focus on a comparison of effects between the pre-reform cohorts suspected in 1995-1997 to post-reform cohorts suspected in 2002-2010. We drop charged suspects in all cohorts who received more consequential punishments (e.g. probation or prison). Among those charged in the pre-reform cohorts, we keep those who have no visible criminal record before 2000, but have records that suddenly appear in 2000 due the reform.

In Figure F.8, we observe no apparent differential changes for the 1995-1997 cohorts in earnings and employment between charged and non-charged suspects at event times 0, 1, or 2. This result is in line with our expectations and acts as our first placebo test. We observe lower earnings for charged suspects at event times 3, 4 and 5, but not lower employment.

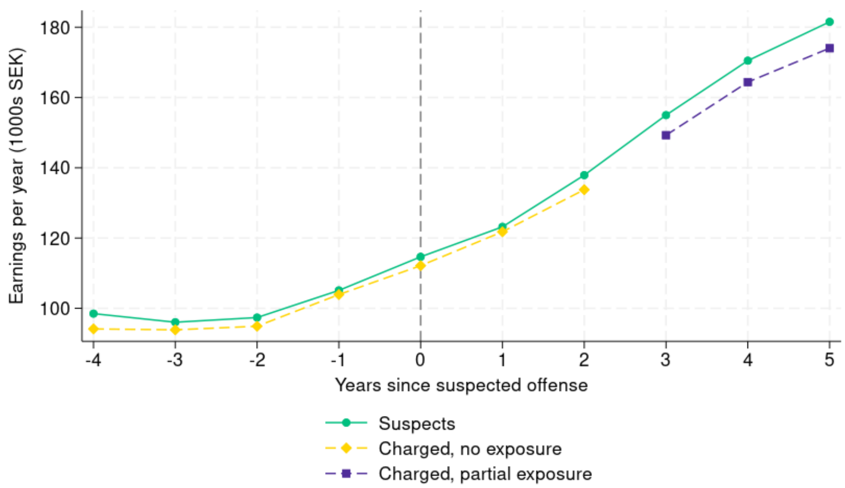
Figure F.8

Not acquiring a record has no impact on employment and earnings of 1995-1997 suspicion year cohorts

(A) Employment



(B) Earnings



Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects who have their first suspicion in 1995-1997. We drop charged suspects with sentences that include more than just a ticket or fine. We keep those charged who have no criminal record before 2000 that later gets turned on by the 2000 reform. The "no exposure" years act as a placebo test for our suspects design. We see no evidence of a differential change in employment or earnings during the "no exposure" period, which includes event times 0, 1, and 2.

We estimate the size and statistical significance of these effects using our baseline event study model

$$Y_{i\tau t} = \alpha_{i\tau} + \gamma_{c(i,\tau),t} + \sum_{j=-4}^{5} \lambda_j D^j_{i\tau t} + \epsilon_{i\tau t} \tag{3}$$

where i indexes people, τ indexes the year of the focal suspected offense, and t indexes years. $\alpha_{i,\tau}$ are person by focal event effects, $\gamma_{c(i,\tau),t}$ are year effects that are specific to a given cell of suspects, $c(\cdot,\cdot)$, where cells are defined as in section 5.2. The $D^j_{i\tau t}$ terms are event time indicators for the treatment group such that $D^j_{i\tau t} = D_{i\tau} 1\{t = \tau + j\}$, where $D_{i\tau} = 1$ if (i, τ) corresponds to a suspected offense that leads to a criminal charge. We cluster standard errors at the person level.

Table F.6

No effect on employment and earnings of charged and convicted suspects who Receive no record

	(1)	(2)	(3)	(4)
Cohort	1995-1997	2002-2010	1995-1997	2002-2010
Punishment	ticket or fine	ticket or fine	All	All
Panel A: Employment				
λ_1	0.08	-0.13	-0.23	-0.30
	(0.08)	(0.04)	(0.06)	(0.03)
λ_5	0.01	-0.13	-0.18	-0.21
	(0.08)	(0.04)	(0.07)	(0.04)
Panel B: Earnings				
λ_1	0.54	-4.15	-6.10	-9.14
	(1.56)	(1.02)	(1.25)	(0.87)
λ_5	-5.01	-6.67	-11.33	-10.12
	(2.54)	(1.47)	(1.93)	(1.17)
Observations	187,287	838,810	265,260	999,750

In column (1), the 1995-1997 cohorts have no visible criminal record at $\tau = 1$. We, therefore, hypothesize that λ_1 should equal zero. The (shaded) λ_1s in column (1) act as placebo tests of our suspects design. All other cohorts and punishment types have visible criminal records at $\tau = 1$. We, therefore, hypothesize that their λ_1s will be < 0. All cohorts and punishment types have visible criminal records at $\tau = 5$. We, therefor, hypothesize that all λ_5s should be < 0.

Estimates of the effect of a criminal record on months employed are shown in Panel A of Table F.6. The estimate of λ_1 is 0.08 (0.08) for the untreated 1995-1997 cohorts and -0.13 (0.04) for the treated 2002-2010 cohorts. At even time $\tau = 5$, when all cohorts have obtained visible criminal records, the estimates for λ_5 are 0.01 (0.10) and -0.13 (0.04) for the 1995-1997 and 2002-2010 cohorts, respectively.

Estimates of the effect of a criminal record on earnings are shown in Panel B of Table F.6. The estimate of λ_1 is 0.54 (1.56) for the untreated 1995-1997 cohorts and -4.15 (1.02) for the treated 2002-2010 cohorts. At event time $\tau = 5$, when all cohorts have obtained visible criminal records, the estimates for λ_5 are -5.01 (2.54) and -6.67 (1.47) for the 1995-1997 and 2002-2010 cohorts, respectively. Thus, we conclude that the 2000 criminal record reform generated wage penalties for those whose lesser crimes now appeared on their records. This earnings penalty, however, is smaller than the baseline penalty found in our full sample.[50]

In summary, all estimates of λ_1 for the 1995-1997s cohorts of charged suspects support our hypothesis that lesser crimes have no effect on wages or employment when they do not generate a criminal record. As records become visible, most of our estimates of λ_5 become negative; especially for earnings.

[50] The fact that all of our estimates of λ_5 are essentially equal in columns (3) and (4) implies that there is nothing special *per se* about the 1995-1997 cohorts that would generate a set of λ_1s equal to zero; other than the fact they do not have a criminal record at $\tau = 1$.

G Decomposing the earnings effect

We find that acquiring a criminal record reduces earnings by approximately 5%, with larger declines following more serious offenses. In this appendix, we decompose this earnings decline into three components: (1) reduced months of employment, (2) shifts from higher- to lower-paying firms, and (3) lower monthly wages within firms. Sections 5.4 and 6.1.2 document the reductions in employment duration, shifts to lower-paying firms, and within-firm wage declines. Here, we quantify the relative contributions of these factors.

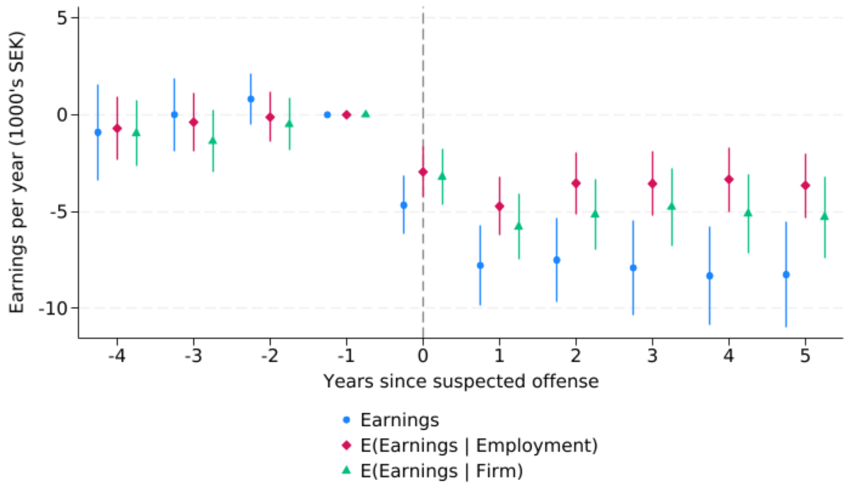
Our decomposition compares estimated treatment effects using three different earnings measures: (a) total earnings, (b) expected earnings conditional on months employed (E[earnings | months employed]), and (c) expected earnings conditional on months employed and firm characteristics (E[earnings | firm]). The treatment effect for E[earnings | months employed] captures the impact due solely to changes in employment duration, while the effect for E[earnings | firm] captures both changes in employment duration and shifts in the quality of employers. The difference between the total earnings effect and the effect on E[earnings | firm] therefore isolates the contribution of within-firm wage declines.

To construct these measures, we first randomly split the non-charged suspect sample into two subsets. Using one subset, we estimate two predictive regression models: (1) a regression of total earnings on months employed, which we use to predict E[earnings | months employed]; and (2) a regression of total earnings on months employed, average coworker earnings at the firm (including an indicator for missing coworker earnings), and their interactions, which we use to predict E[earnings | firm]. We then apply these predictive models to the other subset to generate predictions. Finally, we estimate separate event studies in this second subset for total earnings and the two conditional earnings measures.

Coefficient estimates from this decomposition are shown in Figure G.9. We find that the treatment effects for E[earnings | months employed] and E[earnings — firm] are approximately 44% and 64% of the total earnings treatment effect, respectively. Thus, we attribute 44% of the earnings decline to fewer months employed, 20% to shifts toward lower-paying firms, and the remaining 36% to reductions in monthly wages within firms.

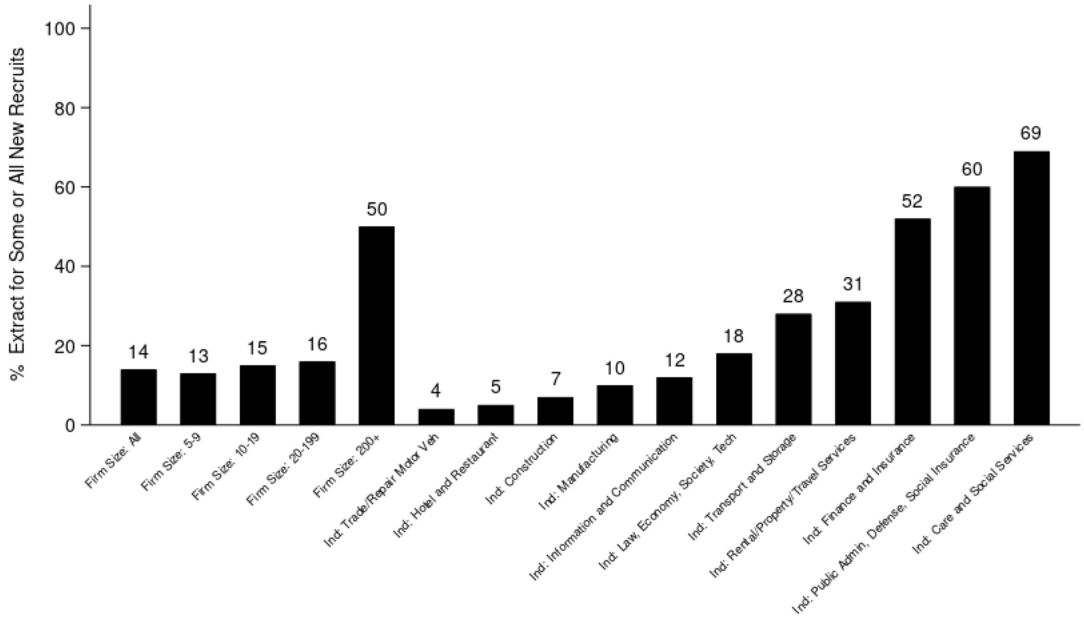
Figure G.9

Decomposition of earnings effect



Note: This figure plots the event time coefficients ($\hat{\lambda}_j$) from the event study specification (3) for three outcomes: a) total earnings, (b) expected earnings conditional on months employed (E[earnings | months employed]), and (c) expected earnings conditional on months employed and firm characteristics (E[earnings | firm]). Sample and variable construction are described in Appendix G. Standard errors are clustered at the person level.

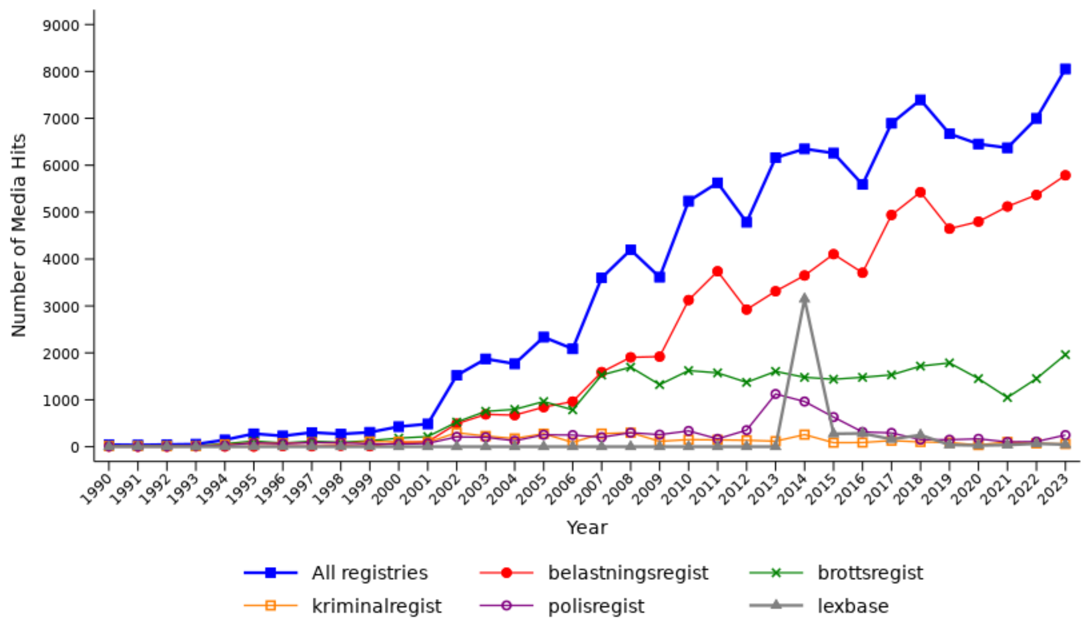
Figure H.10
Government survey of firms about background checks



Note: This figures presents statistics from a 2014 government investigation into the prevalence of background checks, for which more than 1600 firms were surveyed. See SOU 2014:48, Registerutdrag i arbetslivet, Stockholm, 2014.

H Additional exhibits

Figure H.11
Media coverage of crime registers and Lexbase

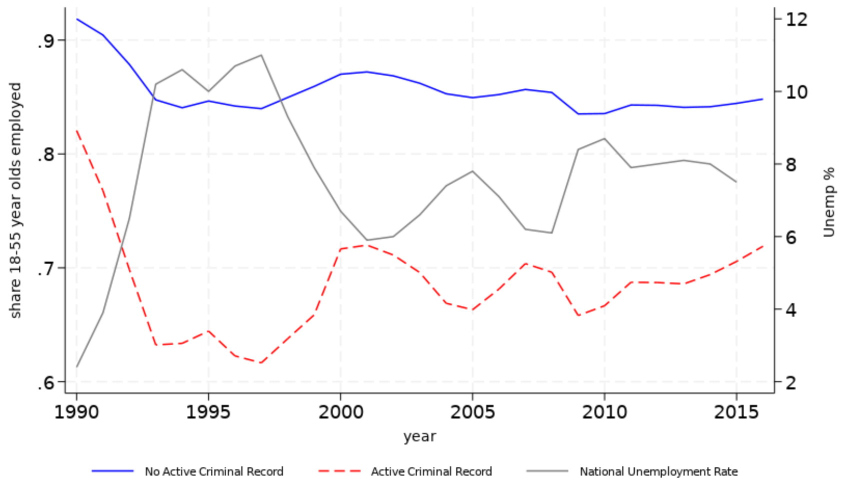


Note: This figure presents the number of hits per year in a Swedish media archive (newspapers, TV, and radio) for the following keyword searches in *Retriever*: belastningsregist, brottsregist, kriminalregist, polisregist. The latter two capture the two registries that existed prior to 2000: Kriminalregistret and Polisregistret, while the Brottsregistret is a commonly used synonym. The figure also displays the number of hits for Lexbase.

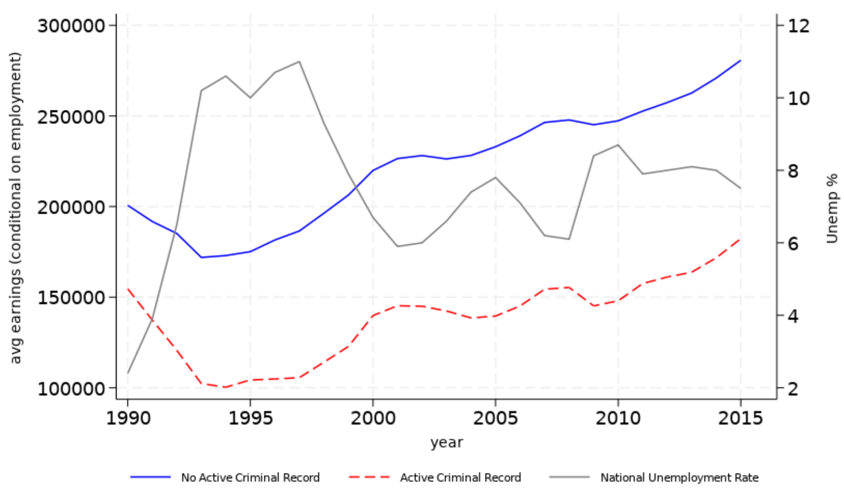
Figure H.12

Raw employment and earnings gaps over time

(A) Employment



(B) Earnings

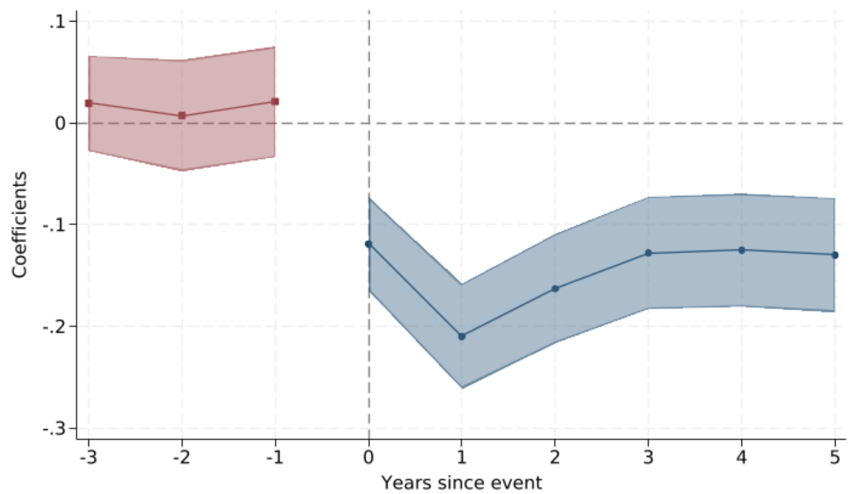


Note: This figure demonstrates how the employment and earnings gaps between workers with and without active records vary over time and with the national unemployment rate, which is plotted on the right axis.

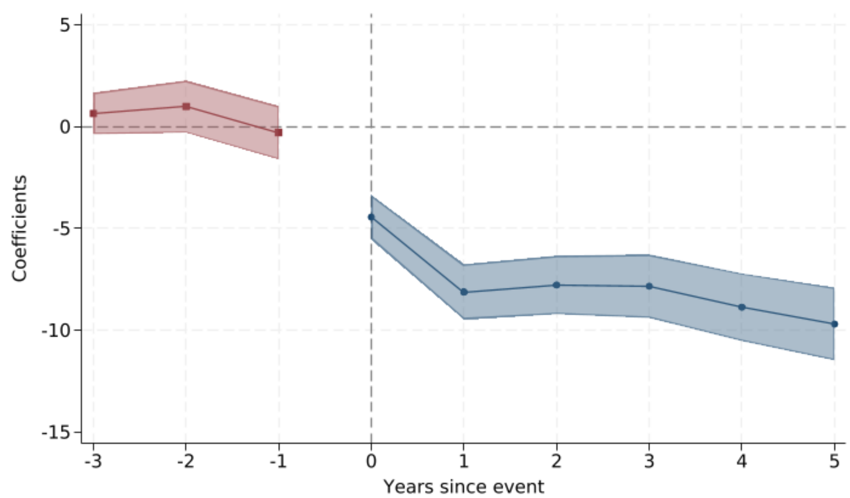
Figure H.13

Event study estimates for employment and earnings, imputation-based method

(A) Months employed



(B) Earnings

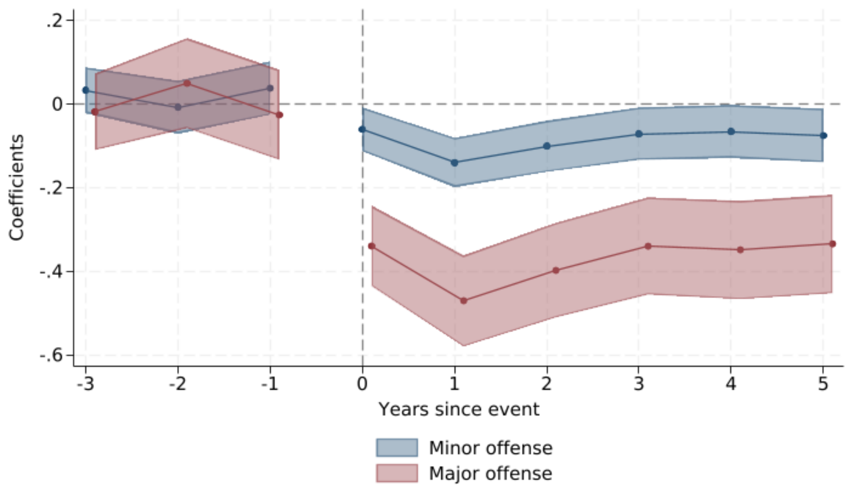


Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024). In panel A the outcome is months employed. In panel B the outcome is earnings.

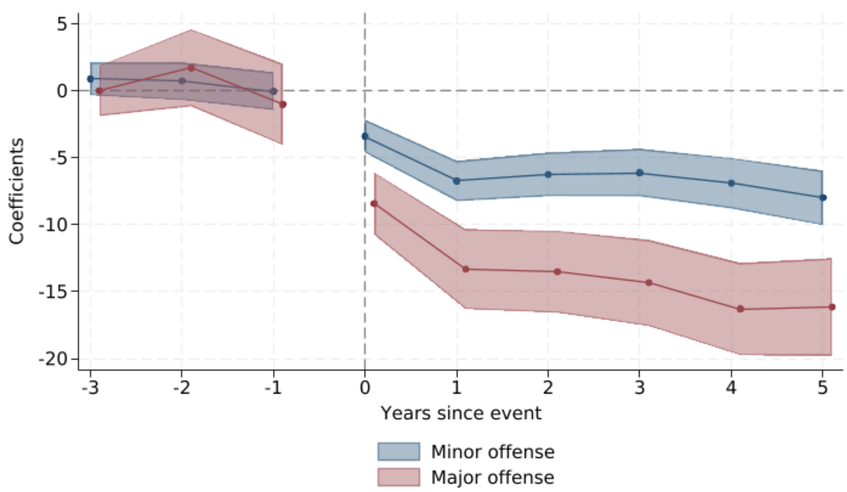
Figure H.14

Employment and earnings effects by offense severity, imputation-based method

(A) Months employed

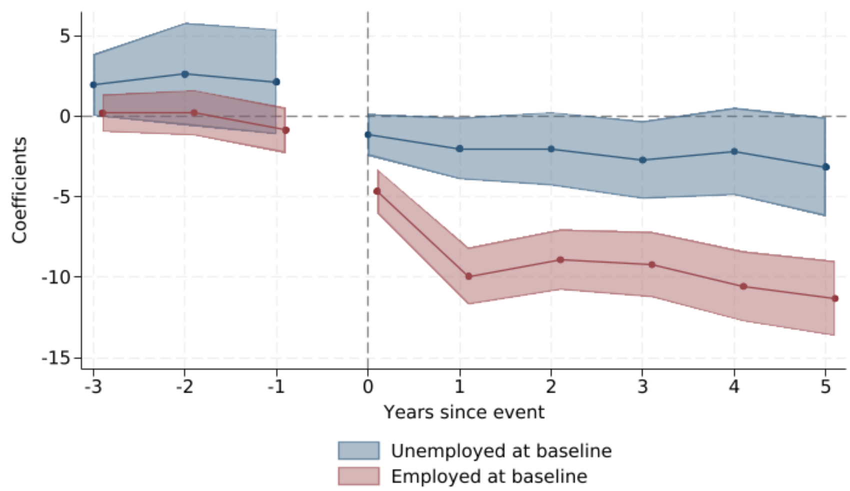


(B) Earnings



Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024), where the specification is estimated separately for events corresponding to minor and major offenses. An event includes a major offense if any suspected offense code falls in the top quartile of expected prison sentence length, conditional on a charge. Remaining events are denoted as minor offenses. In panel A the outcome is months employed. In panel B the outcome is earnings.

Figure H.15
Earnings effects by baseline employment status, imputation-based method

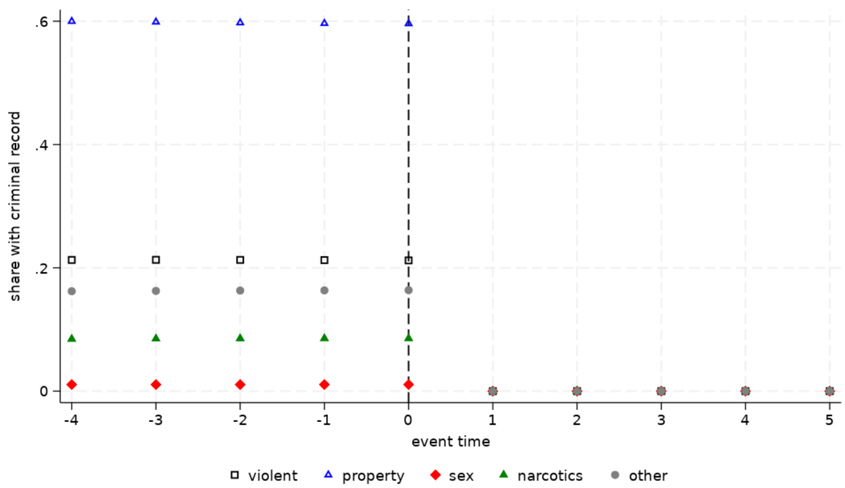


Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024), where the specification is estimated separately for events where the suspect is working in the month of the suspected offense and events where the suspect is not. In panel A the outcome is months employed. In panel B the outcome is earnings.

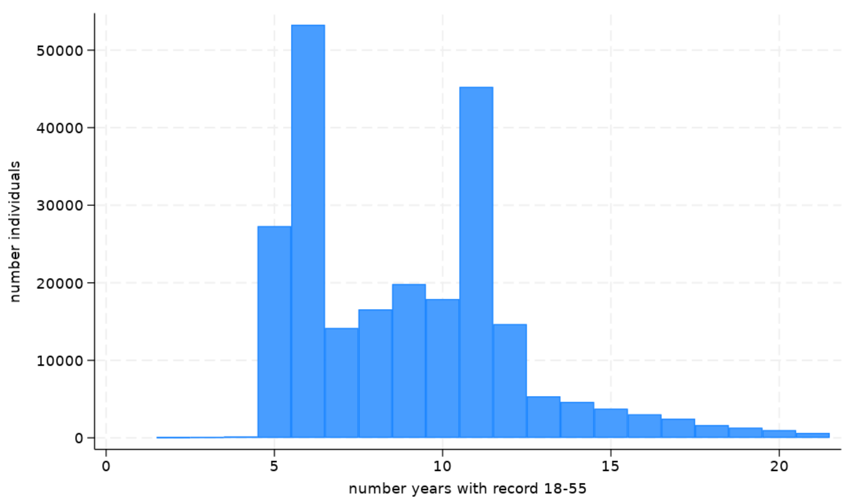
Figure H.16

Record characteristics of the expungement analysis sample

(A) Types of expunged records



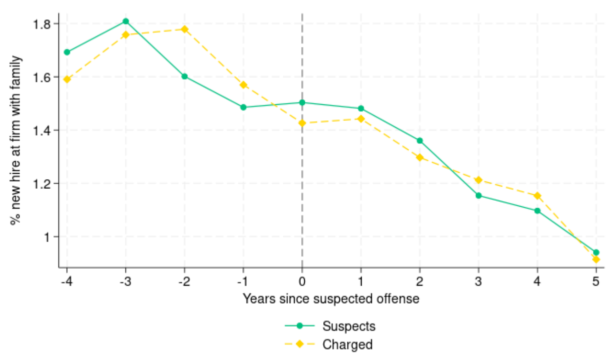
(B) Lengths of expunged records



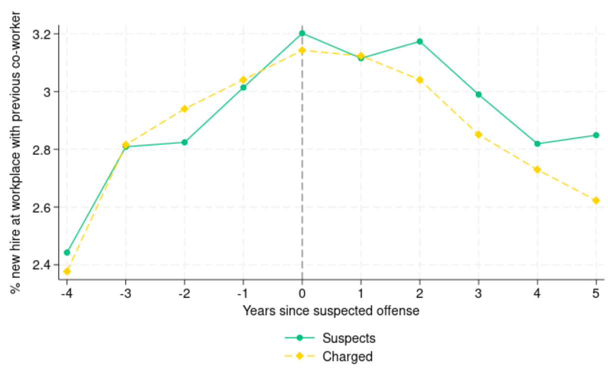
Note: These figures are based on the sample of individuals who have a record expunged and are observed for five years before and after expungement. Panel A shows the types of offenses on the expunged record while panel B shows the length of time individuals had the record before expungement.

Figure H.17
Effects on job finding through networks

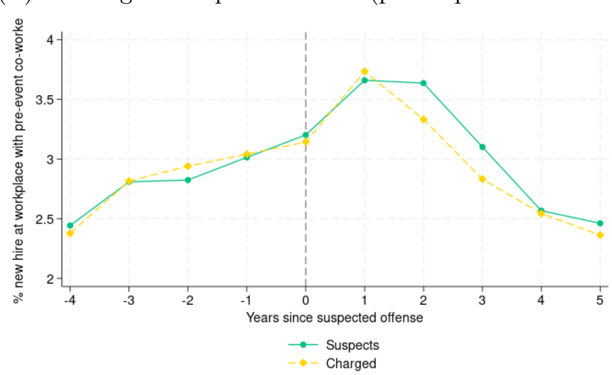
(A) Working with a family member



(B) Working with a past coworker



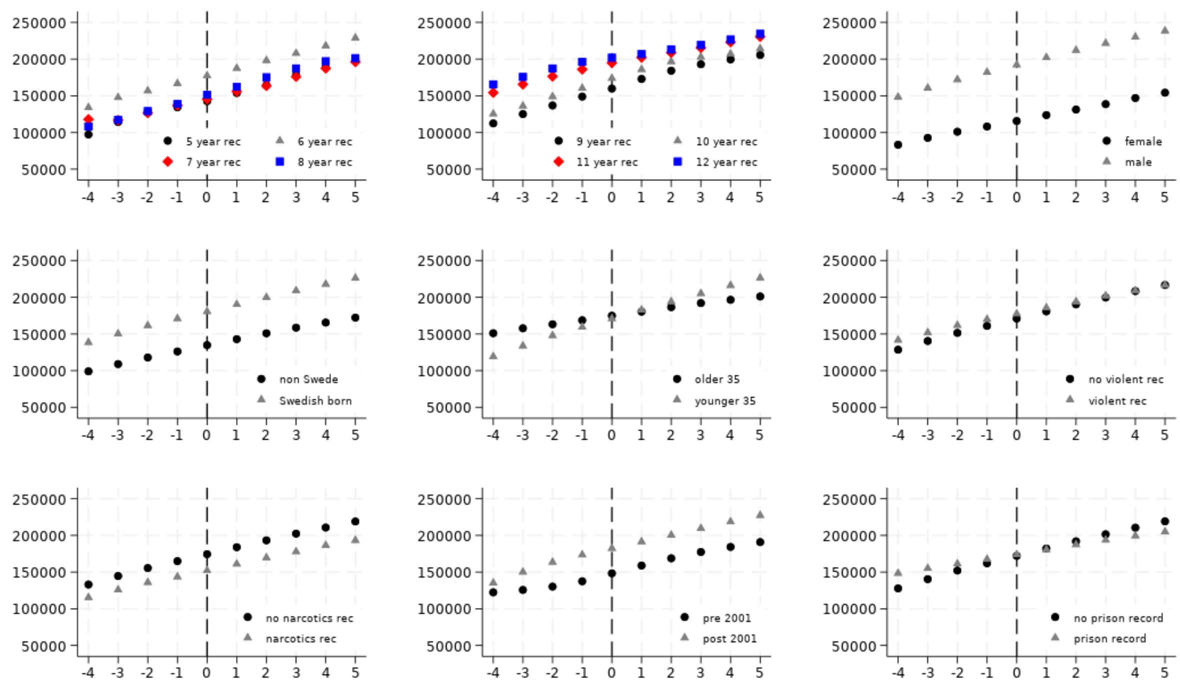
(C) Working with a past coworker (pre-suspicion network)



Note: This figure plots measures of job finding through networks before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is between ages 22 and 40 at the time of suspicion; (3) is present in Sweden at least 4 years before and 5 years after the suspected offense. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects. In panel A, we plot the share of newly-hired workers from each group who work at a firm with a family member. In panel B, we plot the share of newly-hired workers from each group who work at a firm with a coworker whom with they worked at another firm at some point during the past five years. In panel C, we plot the same share as in panel B, but holding constant the network of past coworkers during the five year period before the suspicion. Both coworker network measures exclude firms with more than 500 employees.

Figure H.18

Average annual total earnings before and after expungement: by subsample

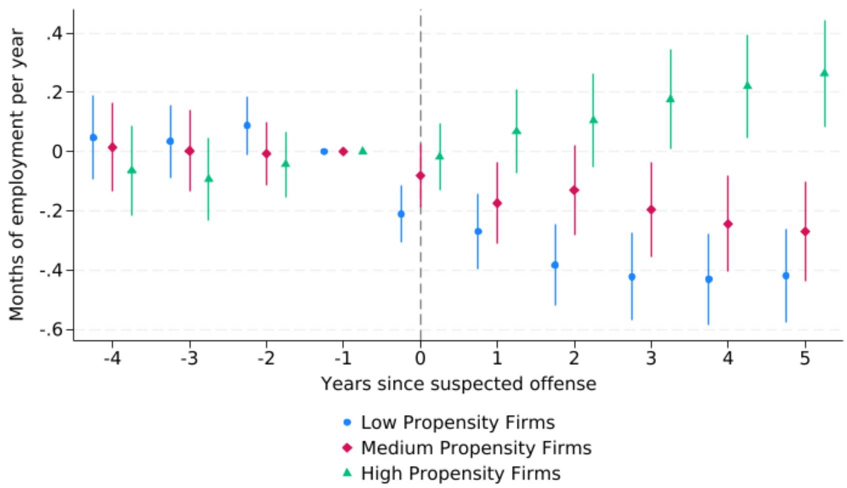


Note: This figure traces out the total annual earnings for the sample of individuals with an expunged record who are observed for five years before and after expungement. Each panel splits the sample into different subsamples: by length of time before expungement, gender, Swedish born, age at expungement, and type of record.

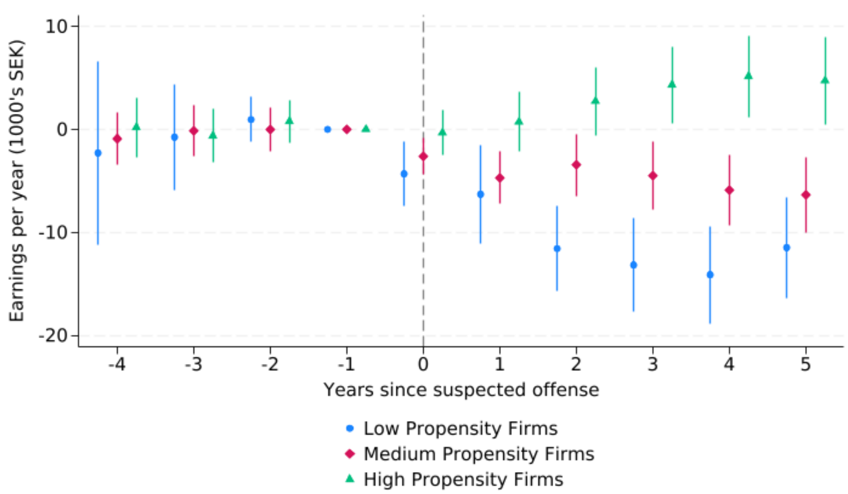
Figure H.19

Employment and earnings effects by firm type, major offenses

(A) Months employed



(B) Earnings



Note: This figure plots the event time coefficients ($\hat{\lambda_j}$) from the event study specification (3) for months employed (panel A) and earnings (panel B) interacted with indicators for three firm types: low propensity, medium propensity, and high propensity to hire WCRs. See section 6.1 for a description of how firm types are constructed. We limit to suspected offenses that include a major offense.

Table H.7
Earnings by WCR status with exam scores, 2015

	Earnings			Months worked			Log monthly earnings				
	Raw (1)	+Demo. (2)	+Test Scores (3)	Raw (4)	+Demo. (5)	+Test Scores (6)	Raw (7)	+Demo. (8)	+Test Scores (9)	+Industry (10)	+Firm (11)
Criminal record	-113.53 (0.15)	-78.82 (0.14)	-75.47 (0.14)	-2.43 (0.00)	-2.19 (0.00)	-2.18 (0.00)	-0.36 (0.00)	-0.25 (0.00)	-0.24 (0.00)	-0.21 (0.00)	-0.13 (0.00)
Demographics		✓	✓		✓	✓		✓	✓	✓	✓
Scores × Demo			✓			✓			✓	✓	✓
Industry FE										✓	
Firm FE											✓
Observations			31,324,035						27,740,240		

This table presents the results of estimating equation 1 using cohorts of working-age men for whom we have cognitive test score data measured during military conscription. These men were born between 1951 and 1987, excluding a few intervening years where scores are unavailable. Earnings and employment data are from 1990 to 2015. The dependent variable is earnings in columns 1 through 3, number of months worked in columns 4 through and 6, and log monthly earnings in columns 7 through 11. The sample is restricted to those with positive earnings in columns 7 through 11. The columns labeled "Raw" only control for year fixed effects. Demographic controls (fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born) are included in columns 2, 3, 5, 6, and 8 through 11. Standardized exam scores interacted with age and year are included in columns 3, 6, and 9 through 11. Industry and firm effects are included in columns 10 and 11, respectively. Only the coefficient on RECORD_{it} is reported; standard errors clustered at the person level are reported in parentheses.

Table H.8
Top/bottom 20 occupations by prevalence of WCRs in 2015

SSYK12 code	*English Name*	*N*	*shrecord*	*MH*	*ML*
2134	Agricultural professionals	616	.002		
2146	Engineering prof: mining, metallurgy...	571	.002		
2242	Psychotherapists	415	.002		+
2232	Nurses - children	2577	.003		+
1351	Real estate and Head of Admin	344	.003		
1540	Religious body manager/leader	340	.003		
2672	Deacons	622	.003		
1111	Legislators	301	.003		
1531	Dept manager elderly care	586	.003		+
2623	Archaelologist and related	827	.004		
1422	Pre-school managers, level 2	2152	.004	X	
2612	Judges	1333	.004	X	
2111	Physicists and astronomers	1542	.004		+
2613	Prosecutors	761	.004		+
2234	Company nurses	494	.004		+
2415	Economists and macro analyst	1118	.004		
2250	Veterinarians	1702	.005		
2283	Audiologists and speech therapists	1859	.005		+
2113	Chemists	2265	.005		
2224	District nurses	4501	.005		+
8321	Taxi, car, and van drivers	6768	.12		+
7212	Welders and flame cutters	9552	.121		
5242	Telemarketers	7147	.122		
9629	Other unclassified service workers	15086	.122		
7132	Painters and industrial painters	3712	.123		
7123	Insulation workers	1238	.123		
7122	Floor layers	2870	.126		
9120	Car reconditioning, window/hand cleaners	3715	.131		
7113	Concrete placers, finishers, related	6389	.134		
9520	Street and market vendors	666	.135		
8113	Well drillers, borers, and related	368	.136		
9332	Ground personnel, movers and stockers	3952	.137		
7215	Steel structure erectors and heavy plate workers	1268	.139		
7119	Unclassified building frame and related	13710	.144		
6222	Fishermen	481	.152		
7121	Roofers	2644	.155		
7221	Blacksmiths, hammersmiths, forging press	2088	.157		
7134	Building structure cleaners and related	2931	.172		
9310	Construction labourers	4502	.189		
7116	Scaffold builders	2358	.263		

For workers 18 to 55 year old with non-missing occupation codes in 2015, we list the top and bottom 20 occupations in terms of the prevalence of workers with non-traffic ticket criminal records. X indicates whether there are mandatory checks for hiring (ML) and + for occupational licenses (ML). We exclude the bottom 5 percent of occupations in terms of number of workers.

Table H.9

Most common suspected offenses and outcomes

				if charged:			if prison
Rank	Offense	% of offenses	% not charged	% fines or less	% probation	% prison	Med. sentence (days)
Minor							
1	theft	20.0	40.1	52.6	25.2	22.2	120
2	traffic	13.1	26.4	58.3	10.6	31.1	90
3	assault	12.1	70.9	22.4	54.4	23.2	120
4	narcotics use	10.5	44.9	64.4	15.9	19.7	120
5	narcotics possession	6.4	43.1	60.3	17.5	22.2	150
6	dui alcohol	5.0	16.6	50.1	22.9	27.1	30
7	property damage	4.8	54.7	55.4	28.4	16.2	150
8	resisting arrest	3.7	51.6	34.4	40.6	25.1	120
9	harassment	3.7	78.4	44.4	37.1	18.4	120
10	other	3.1	82.5	68.0	13.0	19.0	120
Major							
1	threat	22.7	72.3	22.5	45.1	32.5	180
2	fraud	15.2	67.0	20.1	48.7	31.2	300
3	vehicular theft	9.4	43.2	22.1	38.5	39.4	150
4	tax fraud	6.8	80.6	9.3	62.4	28.2	365
5	fencing	5.4	56.9	23.7	38.1	38.3	180
6	sex crime	4.8	77.0	25.8	32.6	41.6	730
7	narcotics selling	4.3	42.4	30.5	25.7	43.8	300
8	smuggling narcotics	4.1	35.8	47.1	19.2	33.7	150
9	guns	4.0	51.6	41.3	22.5	36.2	240
10	aggravated assault	3.5	57.7	5.9	42.8	51.3	365

This table reports the most common minor and major suspected offenses in the suspect register and the distribution of outcomes associated with those suspected offenses. Severity is categorized based on whether any specific suspected offense code falls in the top quartile of expected prison sentence length, conditional on a charge.

TABLE H.10

MOST COMMON SUSPECTED OFFENSES AND OUTCOMES, NO PRIOR CHARGE

				if charged:			if prison
Rank	Offense	% of offenses	% not charged	% fines or less	% probation	% prison	Med. sentence (days)
Minor							
1	theft	19.1	36.0	74.7	23.9	1.4	240
2	assault	17.8	74.4	31.8	61.6	6.5	120
3	traffic	9.0	40.2	86.6	10.1	3.4	30
4	narcotics use	6.9	51.6	83.4	14.1	2.5	240
5	property damage	6.0	55.2	70.8	27.4	1.8	120
6	dui alcohol	5.7	15.2	61.0	23.8	15.2	30
7	harassment	5.3	82.0	58.1	37.0	4.9	120
8	other	4.5	84.8	83.7	9.5	6.8	14
9	resisting arrest	3.4	55.8	47.5	46.4	6.1	90
10	narcotics possession	3.2	41.2	80.1	16.2	3.7	240
Major							
1	threat	24.8	78.0	33.0	56.0	11.0	180
2	fraud	15.1	74.1	21.5	68.9	9.6	545
3	tax fraud	8.8	80.6	11.7	71.1	17.2	365
4	sex crime	7.6	78.9	26.6	38.3	35.0	730
5	vehicular theft	6.2	42.8	43.4	53.8	2.8	210
6	fencing	3.5	56.7	45.7	49.4	4.9	300
7	aggravated assault	3.4	62.7	8.8	66.2	25.0	300
8	guns	2.9	53.2	69.9	21.8	8.3	240
9	narcotics selling	2.3	44.9	40.8	34.7	24.5	545
10	robbery	2.0	54.9	8.3	74.5	17.2	270

This table reports the most common minor and major suspected offenses in the suspect register and the distribution of outcomes associated with those suspected offenses, limiting to suspects with no prior criminal charge. Severity is categorized based on whether any specific suspected offense code falls in the top quartile of expected prison sentence length, conditional on a charge.

Table H.11
Heterogeneity in the employment effect of a charged offense

	By offense severity		By baseline status		By offense type				By age		By time period	
	Minor	Major	Unemployed	Employed	Violent	Property	Narcotics	Traffic	22–29	30–40	‘95-‘03	‘04-‘10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
λ_{-4}	-0.04	0.03	-0.01	-0.04	0.06	-0.04	-0.00	-0.23	-0.04	-0.01	-0.06	0.02
	(0.03)	(0.05)	(0.06)	(0.03)	(0.05)	(0.04)	(0.13)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
λ_{-3}	-0.00	0.01	0.02	-0.00	0.06	-0.03	0.04	-0.13	0.00	-0.01	-0.03	0.03
	(0.03)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.12)	(0.06)	(0.04)	(0.03)	(0.04)	(0.04)
λ_{-2}	-0.05	0.08	-0.06	-0.02	0.03	-0.05	0.06	-0.14	-0.02	-0.01	-0.05	0.02
	(0.02)	(0.04)	(0.05)	(0.02)	(0.04)	(0.04)	(0.10)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_0	-0.11	-0.30	-0.10	-0.07	-0.11	-0.21	-0.42	-0.14	-0.14	-0.18	-0.15	-0.17
	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)	(0.10)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
λ_1	-0.20	-0.45	-0.11	-0.25	-0.27	-0.26	-0.54	-0.23	-0.27	-0.26	-0.23	-0.30
	(0.03)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)	(0.12)	(0.06)	(0.04)	(0.03)	(0.04)	(0.04)
λ_2	-0.16	-0.37	-0.03	-0.24	-0.26	-0.16	-0.63	-0.16	-0.24	-0.19	-0.15	-0.27
	(0.03)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.12)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
λ_3	-0.13	-0.33	-0.05	-0.22	-0.18	-0.16	-0.60	-0.17	-0.19	-0.17	-0.14	-0.22
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
λ_4	-0.13	-0.33	-0.02	-0.20	-0.20	-0.15	-0.54	-0.22	-0.18	-0.18	-0.15	-0.21
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
λ_5	-0.13	-0.30	-0.07	-0.18	-0.17	-0.14	-0.66	-0.22	-0.19	-0.16	-0.14	-0.21
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)
Non-charged outcome mean	8.55	7.86	3.76	10.30	8.43	7.82	8.14	9.23	8.40	8.29	7.99	8.65
Observations	1165480	420220	449150	1063670	597120	579170	71550	217680	801610	784090	773550	812150

This table shows event time coefficients ($\hat{\lambda_j}$) from the event study specification (3), where the specification is estimated separately for various subsamples of events. The outcome is months employed. Columns 1 through 4 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Columns 5 and 6 split the sample in half by suspect age: 22–29 in column 5 and 30–40 in column 6. Columns 7 and 8 split the sample period in half: 1995–2003 in column 7 and 2004–2010 in column 8.

Table H.12
Heterogeneity in the employment effect of a charged offense, gender and Swedish born status

	By gender		By immigrant status	
	Men	Women	Swedish born	Immigrant
	(1)	(2)	(3)	(4)
λ_{-4}	-0.03	-0.01	-0.04	-0.08
	(0.03)	(0.05)	(0.03)	(0.07)
λ_{-3}	0.03	-0.07	0.00	-0.09
	(0.03)	(0.05)	(0.03)	(0.06)
λ_{-2}	-0.01	-0.03	-0.00	-0.08
	(0.03)	(0.04)	(0.03)	(0.05)
λ_{-1}	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)
λ_{0}	-0.17	-0.14	-0.18	-0.10
	(0.03)	(0.04)	(0.03)	(0.05)
λ_{1}	-0.26	-0.27	-0.31	-0.10
	(0.03)	(0.05)	(0.03)	(0.07)
λ_{2}	-0.19	-0.26	-0.25	-0.05
	(0.03)	(0.05)	(0.03)	(0.07)
λ_{3}	-0.15	-0.25	-0.22	-0.05
	(0.03)	(0.05)	(0.03)	(0.07)
λ_{4}	-0.15	-0.24	-0.16	-0.12
	(0.03)	(0.05)	(0.03)	(0.07)
λ_{5}	-0.15	-0.24	-0.17	-0.09
	(0.03)	(0.05)	(0.03)	(0.07)
Non-charged outcome mean	8.75	7.42	9.09	6.38

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (3), where the specification is estimated separately for various subsamples of events. The outcome is months employed. Columns 1 and 2 split the sample by suspect gender. Columns 3 and 4 split the sample by suspect Swedish born status.

Table H.13
Heterogeneity in the earnings effect of a charged offense

	By offense severity		By baseline status		By offense type				By age		By time period	
	Minor	Major	Unemployed	Employed	Violent	Property	Narcotics	Traffic	22–29	30–40	'95-'03	'04-'10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
λ_{-4}	0.03	1.01	-2.14	0.87	1.24	-0.70	3.86	-2.09	0.31	0.26	0.26	0.32
	(0.71)	(1.54)	(1.65)	(0.72)	(0.88)	(1.28)	(1.90)	(1.60)	(0.61)	(1.21)	(0.70)	(1.12)
λ_{-3}	0.91	1.01	-0.18	1.06	1.14	0.98	3.45	-1.21	0.68	1.22	0.57	1.31
	(0.63)	(1.18)	(1.06)	(0.73)	(0.87)	(0.88)	(2.04)	(1.90)	(0.63)	(0.95)	(0.70)	(0.87)
λ_{-2}	0.77	2.73	0.48	1.09	1.03	1.65	3.08	0.77	0.96	1.62	1.61	0.95
	(0.50)	(0.91)	(0.60)	(0.59)	(0.70)	(0.70)	(1.73)	(1.33)	(0.54)	(0.70)	(0.59)	(0.64)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_{0}	-3.14	-7.16	-1.96	-3.95	-3.44	-4.52	-7.99	-5.44	-4.04	-4.34	-3.32	-5.05
	(0.53)	(1.17)	(0.47)	(0.60)	(0.74)	(0.85)	(1.92)	(1.38)	(0.59)	(0.80)	(0.62)	(0.77)
λ_{1}	-5.91	-12.17	-2.99	-8.35	-6.03	-6.85	-14.91	-11.22	-7.17	-7.95	-5.60	-9.50
	(0.70)	(1.58)	(0.76)	(0.86)	(0.98)	(1.15)	(2.52)	(1.88)	(0.78)	(1.08)	(0.88)	(0.99)
λ_{2}	-6.05	-12.03	-3.00	-8.59	-7.34	-6.65	-19.92	-6.80	-8.07	-7.12	-5.89	-9.34
	(0.79)	(1.59)	(1.02)	(0.93)	(1.12)	(1.19)	(2.90)	(2.09)	(0.89)	(1.14)	(0.92)	(1.11)
λ_{3}	-6.38	-12.64	-3.76	-9.24	-7.97	-7.36	-20.97	-5.94	-8.21	-7.80	-6.07	-9.97
	(0.88)	(1.67)	(1.23)	(1.01)	(1.23)	(1.28)	(3.11)	(2.30)	(0.97)	(1.25)	(1.02)	(1.19)
λ_{4}	-6.93	-14.89	-3.52	-10.46	-7.66	-8.83	-23.18	-8.58	-9.55	-8.43	-7.92	-10.10
	(0.94)	(1.78)	(1.33)	(1.08)	(1.32)	(1.34)	(3.38)	(2.51)	(1.04)	(1.33)	(1.12)	(1.25)
λ_{5}	-7.83	-14.31	-4.00	-10.90	-8.85	-7.64	-24.88	-9.23	-9.96	-9.06	-8.77	-10.29
	(1.01)	(1.88)	(1.43)	(1.16)	(1.40)	(1.44)	(3.62)	(2.67)	(1.11)	(1.42)	(1.20)	(1.32)
Non-charged outcome mean	168.99	152.30	50.84	213.16	165.23	142.66	139.65	200.00	145.50	181.59	144.48	180.68
Observations	1165480	420220	449150	1063670	597120	579170	71550	217680	801610	784090	773550	812150

This table shows event time coefficients ($\hat{\lambda_j}$) from the event study specification (3), where the specification is estimated separately for various subsamples of events. The outcome is earnings. Columns 1 through 4 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Columns 5 and 6 split the sample in half by suspect age: 22–29 in column 5 and 30–40 in column 6. Columns 7 and 8 split the sample period in half: 1995–2003 in column 7 and 2004–2010 in column 8.

Table H.14
Heterogeneity in the earnings effect of a charged offense, gender and Swedish born status

	By gender		By immigrant status	
	Men	Women	Swedish born	Immigrant
	(1)	(2)	(3)	(4)
λ_{-4}	-0.01	0.96	-0.41	0.43
	(0.88)	(0.80)	(0.89)	(1.08)
λ_{-3}	1.09	0.59	0.41	0.94
	(0.73)	(0.78)	(0.68)	(1.13)
λ_{-2}	1.54	0.69	0.94	0.78
	(0.56)	(0.65)	(0.55)	(0.82)
λ_{-1}	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)
λ_{0}	-5.16	-1.97	-5.11	-2.11
	(0.64)	(0.70)	(0.64)	(0.90)
λ_{1}	-8.77	-4.75	-9.17	-2.88
	(0.86)	(0.93)	(0.84)	(1.23)
λ_{2}	-8.55	-5.47	-9.65	-2.54
	(0.92)	(1.05)	(0.89)	(1.43)
λ_{3}	-9.02	-5.73	-9.49	-4.39
	(1.01)	(1.14)	(0.99)	(1.53)
λ_{4}	-10.56	-5.49	-10.48	-4.36
	(1.08)	(1.21)	(1.05)	(1.67)
λ_{5}	-11.12	-5.89	-11.05	-4.49
	(1.15)	(1.30)	(1.12)	(1.79)
Non-charged outcome mean	187.79	109.27	186.00	107.08

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (3), where the specification is estimated separately for various subsamples of events. The outcome is earnings. Columns 1 and 2 split the sample by suspect gender. Columns 3 and 4 split the sample by suspect Swedish born status.

Table H.15

Characteristics for suspects and suspected offenses, second offense

	All		Matched and Weighted	
	Not charged	Charged	Not charged	Charged
Age at time of suspected offense	29.02	28.09	26.79	26.79
	(5.697)	(5.541)	(5.294)	(5.294)
Year of suspected offense	2004.6	2004.2	2004.4	2004.4
	(4.015)	(4.107)	(4.202)	(4.202)
Order of offense	2.414	2.376	2.155	2.155
	(0.711)	(0.677)	(0.410)	(0.410)
Violent (%)	48.54	28.60	30.50	30.50
Property (%)	34.62	41.16	39.28	39.28
Traffic (%)	6.133	29.99	13.31	13.31
Narcotics (%)	10.94	27.58	17.44	17.44
Sex (%)	4.217	2.073	0.510	0.510
Other (%)	12.13	14.45	3.392	3.392
Any major offense (%)	42.86	31.27	19.77	19.77
Fine or less (%)	0	60.01	0	58.02
Probation and other (%)	0	27.19	0	31.40
Prison (%)	0	12.80	0	10.58
Male (%)	76.13	74.97	79.39	79.39
	(42.63)	(43.32)	(40.45)	(40.45)
Swedish born (%)	62.86	66.23	68.53	68.73
	(48.32)	(47.29)	(46.45)	(46.36)
Age	28.55	27.61	26.30	26.25
	(5.752)	(5.578)	(5.366)	(5.335)
Years of education	11.07	10.93	11.06	11.01
	(1.944)	(1.887)	(1.804)	(1.835)
Earnings (1000s SEK)	108.6	96.42	105.5	103.4
	(134.0)	(117.2)	(126.1)	(120.6)
Months employed year	6.924	6.544	6.570	6.460
	(5.457)	(5.467)	(5.528)	(5.528)
Ever employed during year	68.12	65.80	63.96	63.81
	(46.60)	(47.44)	(48.02)	(48.06)
Tenure in main job (in months)	29.09	27.19	26.01	26.01
	(31.61)	(29.17)	(27.11)	(26.87)
Workplace size	269.7	272.3	245.7	273.0
	(879.0)	(893.7)	(833.6)	(869.5)
Firm size	4219.6	3869.8	3432.9	3756.9
	(10981.9)	(10433.9)	(9794.7)	(10487.2)
Average firm coworker monthly wage (1000s SEK)	16.45	16.23	16.78	17.15
	(8.268)	(7.866)	(8.197)	(8.165)
Firm pay premium at main job	-0.0833	-0.0784	-0.0724	-0.0459
	(0.276)	(0.263)	(0.260)	(0.253)
% of firm coworkers with active records	8.510	8.935	8.594	8.620
	(10.80)	(11.52)	(10.87)	(10.26)
Agriculture and resources	3.274	3.577	3.888	3.517
Care and social services	10.00	10.25	7.384	8.034
Construction	9.128	9.461	10.70	12.43
Education	4.044	3.844	3.444	3.557
Health care	2.580	2.488	1.914	1.958
Hotels and restaurants	8.723	9.012	7.621	7.514
Manufacturing and industry	19.32	19.54	20.56	21.58
Public administration and defense	2.080	1.431	1.533	1.199
Retail and wholesale	12.96	13.65	15.17	14.79
Services	25.28	23.93	25.45	23.26
Unknown	2.580	2.787	2.338	2.158
Missing	0.0193	0.0320	0	0
Observations	15,246	14,233	4,058	3,921

This table describes the suspects and suspected offense combinations of people and years ("person-years") satisfying the sample criteria described in section 5.6.1. The first column includes all non-charged suspects. The second column includes all charged suspects. The third column includes all non-charged suspects that can be matched to charged suspects, re-weighting across cells to match the distribution among charged suspects. The construction of cells is described in detail in section 5.2. The fourth column includes all charged suspects that can be matched to non-charged suspects. The top panel describes suspected offenses in the event year. Because there can be multiple suspected offenses corresponding to an event, crime types sum to more than one. Order of offense indicates whether this is the first year, second year, or subsequent year that a person has been suspected of an offense. Sentence type refers to the most serious sentence for all suspected offenses in the event year. The bottom panel describes suspects in the year before the focal suspected offense. The construction of firm pay premiums is discussed in Appendix B.

Table H.16
Firm descriptive statistics for Table 8

	Baseline	Matched to job ads	With exposure measures
Male (%)	63.01	57.33	63.78
	(27.71)	(27.07)	(27.63)
Swedish born (%)	79.99	79.17	80.98
	(24.53)	(22.95)	(24.03)
Avg. age	30.42	30.58	30.17
	(4.981)	(4.544)	(4.717)
Avg. years of education	11.68	11.94	11.68
	(0.970)	(0.999)	(0.975)
Monthly wage (1000s SEK)	10.76	12.26	11.29
	(8.197)	(7.653)	(7.644)
Log monthly wage	1.492	1.718	1.591
	(1.114)	(0.962)	(1.024)
Avg. earnings rank	53.04	53.63	53.58
	(11.26)	(10.49)	(11.10)
Criminal record (%)	13.13	10.43	12.73
	(10.64)	(8.512)	(10.26)
Suspect without record (%)	7.557	6.762	7.194
	(6.571)	(5.563)	(6.252)
Workplace size	114.4	131.3	107.8
	(209.1)	(223.1)	(188.1)
Firm size	1461.7	1627.2	1364.1
	(2047.2)	(2100.0)	(1848.3)
Firm effect for WCR hiring (θ) $\times$ 100	0.629	-0.0246	0.572
	(22.28)	(19.43)	(21.85)
Avg. criminal propensity of hires X 100	10.33	8.702	9.933
	(8.111)	(6.958)	(7.773)
Firm pay premium	-0.257	-0.00557	-0.107
	(1.084)	(0.872)	(0.941)
Log value added per worker	11.27	11.65	11.46
	(1.384)	(1.236)	(1.247)
% firms ever mentions background checks in ads	11.10	11.10	11.40
	(31.42)	(31.42)	(31.78)
operator has past record	0.233	0.201	0.234
	(0.405)	(0.375)	(0.405)
operator has family with past record	0.142	0.135	0.142
	(0.312)	(0.294)	(0.312)
operator has high exposure to co-workers with records	0.269	0.168	0.269
	(0.425)	(0.338)	(0.424)
Agriculture and resources	3.917	3.149	4.094
	(12.98)	(11.64)	(13.29)
Care and social services	5.101	6.867	4.777
	(11.63)	(15.29)	(11.07)
Construction	12.74	7.718	13.69
	(25.85)	(20.13)	(26.81)
Education	4.225	6.621	4.154
	(10.26)	(15.26)	(10.36)
Health care	1.502	2.080	1.354
	(7.032)	(9.280)	(6.518)
Hotels and restaurants	12.65	12.92	11.67
	(24.81)	(25.34)	(23.97)
Manufacturing and industry	14.98	14.51	15.37
	(25.77)	(25.73)	(26.06)
Public administration and defense	1.129	1.088	1.011
	(3.263)	(2.743)	(2.669)
Retail and wholesale	15.05	14.83	15.57
	(23.69)	(23.40)	(24.24)
Services	27.63	29.45	27.33
	(28.86)	(29.96)	(29.08)
Unknown	1.066	0.768	0.969
	(3.876)	(1.896)	(3.606)
Observations	104697	23497	71946

This table reports descriptive statistics for firms used to estimate specifications described in Table 8. Firm pay premiums are estimated using a standard worker-firm wage decomposition (Abowd et al., 1999; Card et al., 2016). See Appendix B for details.

Table H.17

Firm-level summary statistics in the job advertisement data

	(1) All Firms		(2) All: Signal=1		(3) All: Signal = 0		(4) Private: Signal=1		(5) Private: Signal=0	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
any_ads_crimrec	0.09	0.28	1.00	0.00	0.00	0.00	1.00	0.00	0.00	0.00
any_ads_drugalc	0.03	0.17	0.21	0.41	0.01	0.11	0.17	0.38	0.01	0.11
onet_avg_educ	3.35	1.35	3.82	1.20	3.30	1.36	3.77	1.23	3.30	1.36
onet_jobzone	2.48	0.79	2.71	0.73	2.46	0.80	2.68	0.75	2.46	0.80
onet_customers	3.42	0.58	3.42	0.48	3.42	0.59	3.41	0.49	3.42	0.59
onet_vulnerable_pop	0.10	0.24	0.35	0.37	0.07	0.21	0.33	0.38	0.07	0.21
onet_theft_fin_opp	0.19	0.19	0.15	0.15	0.20	0.19	0.16	0.15	0.20	0.19
onet_work_conditions	3.14	0.46	3.27	0.31	3.13	0.47	3.26	0.32	3.13	0.47
share_military	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01
share_manager	0.03	0.10	0.05	0.08	0.03	0.10	0.04	0.08	0.03	0.10
share_professionals	0.13	0.27	0.20	0.26	0.12	0.27	0.18	0.26	0.12	0.26
share_technicians	0.18	0.28	0.26	0.26	0.18	0.28	0.27	0.26	0.18	0.28
share_clerks	0.07	0.18	0.05	0.12	0.07	0.18	0.06	0.13	0.07	0.18
share_service_shop_sales	0.22	0.34	0.24	0.32	0.22	0.35	0.25	0.33	0.22	0.35
share_agric_fishery	0.02	0.11	0.01	0.05	0.02	0.12	0.01	0.05	0.02	0.12
share_craft_trades	0.15	0.30	0.07	0.17	0.16	0.31	0.07	0.18	0.16	0.31
share_plant_machine	0.08	0.22	0.06	0.18	0.08	0.23	0.06	0.19	0.08	0.23
share_elem_unskilled	0.11	0.25	0.06	0.19	0.12	0.26	0.07	0.19	0.12	0.26
sh_mancheck_hire	0.05	0.17	0.23	0.33	0.03	0.14	0.22	0.34	0.03	0.14
sh_mancheck_license	0.09	0.25	0.17	0.27	0.09	0.24	0.17	0.27	0.09	0.24
recruiting_firm	0.01	0.09	0.04	0.19	0.01	0.07	0.04	0.20	0.01	0.07
likely_public_sector	0.01	0.10	0.07	0.26	0.00	0.06	0.00	0.00	0.00	0.00
avg number vacancies	1.64	5.68	1.92	4.16	1.61	5.81	1.91	4.32	1.62	5.82
number of ads	119.87	1214.99	929.76	3839.44	41.83	358.92	774.89	3735.95	41.90	359.56
share_shortterm_job	0.12	0.22	0.12	0.16	0.12	0.22	0.12	0.16	0.12	0.22
share_regular_emp	0.86	0.28	0.86	0.23	0.86	0.28	0.85	0.24	0.86	0.28
share_fixed_salary	0.92	0.23	0.95	0.16	0.92	0.23	0.95	0.17	0.92	0.23
share_fulltime	0.81	0.29	0.75	0.28	0.82	0.29	0.75	0.29	0.82	0.29
share_license_req	0.19	0.30	0.27	0.29	0.18	0.30	0.27	0.30	0.18	0.30
adtext_result_detail	0.02	0.08	0.02	0.06	0.02	0.09	0.02	0.06	0.02	0.09
adtext_independent	0.18	0.27	0.25	0.24	0.17	0.28	0.24	0.25	0.17	0.28
adtext_adaptable	0.13	0.26	0.14	0.19	0.13	0.26	0.14	0.20	0.13	0.26
adtext_responsible	0.07	0.19	0.10	0.18	0.07	0.19	0.11	0.18	0.07	0.19
N	54370		4779		49591		4426		49412	

This table presents firm-level summary statistics for all firms with known ID numbers in the job ads data. Column (1) includes all firms, while column (2) and (3) separate firms according to whether they ever signal background checks. Columns (4) and (5) present the same split for firms that are likely non-public.

Table H.18
Changes in firm exposure from operator moves and firm performance

	Own record			Coworkers record (SD 0.009)			Family record		
	VA (1)	LC (2)	Profits (3)	VA (4)	LC (5)	Profits (6)	VA (7)	LC (8)	Profits (9)
After arrival	0.010	0.017	0.022	0.008	0.018	0.014	0.008	0.018	0.015
	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.004)
Exposure X After arr.	-0.009	0.011	0.022	0.320	0.126	0.199	0.028	-0.009	0.053
	(0.178)	(0.017)	(0.040)	(0.196)	(0.157)	(0.431)	(0.019)	(0.019)	(0.043)
Ind. X Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
CZ X Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,099,711	1,118,030	861,263	1,330,340	1,353,891	1,043,175	1,360,310	1,384,512	1,066,921

This table presents coefficient estimates from least squares regressions of the firm's log value added per worker, log of total labor costs per worker, or log profits on an indicator for the period after the arrival of a new operator and its interaction with the increase in operator exposure at the firm after the arrival of the new operator. Measures of own and family exposure take a value of 0 or 1, so changes in exposure can correspond to -1 (exposure decrease), 0 (constant exposure), or 1 (exposure increase). Measures of exposure to past co-WCRs are continuous, so values for changes in exposure are also continuous and range from -1 to 1. Firms without a new operator are included in the regression with a value of 0 for change in exposure. Each observation is a firm-year between 2005 and 2015.

Table H.19
Changes in the WCR share and firm performance

	Firm size 2-50 employees			Firm size >= 2 employees		
	VA (1)	LC (2)	Profits (3)	VA (4)	LC (5)	Profits (6)
Share WCR	0.003	-0.003	0.059	0.002	-0.004	0.061
	(0.003)	(0.003)	(0.009)	(0.003)	(0.003)	(0.009)
Ind. X Year FEs	✓	✓	✓	✓	✓	✓
CZ X Year FEs	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓
Outcome mean	12.96	12.61	12.55	12.97	12.62	12.69
% change from +1 SD	< 0.1	< 0.1	< 0.1	< 0.1	< 0.1	< 0.1
Observations	1,826,925	1,826,925	1,826,925	1,905,148	1,905,148	1,905,148
Number firms	269886	269886	269886	275186	275186	275186

This table presents coefficient estimates from least squares regressions of the firm's log value added per worker, log of total labor costs per worker, or log profits on the share of workers at the firm with an active criminal record. Each observation is a firm-year between 1997 and 2015. For the sample of firms used in columns (1)-(3), share WCR has mean of 0.065 and a standard deviation equal to 0.134. For the sample of firms used in columns (4)-(6), share WCR has a mean of 0.064 and a standard deviation equal to 0.132.