Job Sorting and the Labor Market Effects of a Criminal Record

Randi Hjalmarsson*
Louis-Pierre Lepage†
Matthew Lindquist ‡
Conrad Miller§

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PRELIMINARY AND INCOMPLETE

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 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%, with these effects persisting for over a decade. These negative effects are roughly twice as large for more serious or subsequent charges and are not driven by job displacement or incapacitation. A criminal record reduces employment at firms that, conditional on other worker characteristics, are less likely to hire workers with records and decreases monthly wages more broadly. Employer differences in hiring workers with records appear in part shaped by managers and their backgrounds. Managers with a criminal record themselves or more previous exposure to people with records–through former coworkers or family members–are more likely to hire such workers.

*University of Gothenburg and CEPR
†Stockholm University
‡Stockholm University
§Haas School of Business, UC Berkeley and NBER. We thank seminar participants at the University of Gothenburg, the Transatlantic Workshop on the Economics of Crime in Zurich, Duke, Cal Poly SLO, UC Berkeley, Cornell, UC Santa Barbara, and UC Irvine for comments. We are grateful for European Union funding: ERC, Police-Prisons-Firms, 101093345. Views and opinions expressed are however those of the authors only and do not necessarily reflect those of the European Union or the European Research Council. Neither the European Union nor the granting author can be held responsible for them. We also thank Tomas Reivinger for his excellent research assistance.

1 Introduction

In the United States and increasingly in Europe, it is common for firms to conduct background checks when screening job applicants. Evidence from surveys and audit studies indicates that many firms have strong preferences against hiring workers with criminal records (WCRs) for at least some roles.[1] However, we know little about the extent to which having a criminal record harms labor market prospects or the specific role of employer preferences. For example, the labor market effects of a criminal record may depend on whether WCRs can avoid more discriminatory employers and work elsewhere, as well as the pay associated with those jobs (Becker, 1971).

In this paper, we address four questions: (1) How do workers with and without criminal 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? 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. Like much of Europe, Sweden has historically had stronger privacy protections around criminal records than the United States. However, since the 1990s, it has introduced several reforms that 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.[2]

We begin by presenting 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 a criminal 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 provide evidence on the determinants of employers' willingness to hire WCRs, with a specific focus on the role of managers and manager background.

[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.

[2] Consistent with this statistic, our analyses of job ad data indicate that around 30-50% of job ads, depending on the year, are at firms that sometimes signal that criminal background checks will be conducted as part of the hiring process.

During the study period, over 9% of men and 2.5% of women have an active criminal record. Conditional on education, age, gender, and immigrant status, prime-age WCRs earn 31% less than prime-age adults without criminal records. Conditioning further on cognitive test scores only slightly reduces this gap. 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 40% percent 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).

We also match Swedish occupation codes to occupational characteristics from O*NET and annual background check laws to study the kinds of occupations into which WCRs are sorted. 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, 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 to matched adults deemed by police to be reasonable suspects for a similar offense but not charged, typically due to 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. The motivation for this research 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, we find that charged and matched non-charged suspects exhibit similar employment and earnings trajectories before the suspected offense and are suspected of future offenses at comparable 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%). Moreover, these effects persist for over a decade, after most offenses are automatically expunged. The roles of job displacement and incarceration are, at most, limited:

effects are proportionally similar for those working and not working at the time of the suspected offense and incarceration sentences are rare in our context (8% of our event study sample).

We then unpack the overall decrease in employment and earnings that we document and investigate how a criminal record affects: (1) where people work and (2) how much they earn at a given employer. We characterize firms by their out-of-sample likelihood of hiring WCRs, conditional on other worker characteristics. We find that the decrease in months worked reflects a decrease in employment at firms unlikely to hire WCRs, indicating that these firms are likely discriminating on the basis on criminal record status. The decrease in employment at these firms is only partially offset by employment elsewhere. Among those working, monthly wages decrease by 4%. This mostly reflects a decrease in monthly wages within firms, though a criminal record also leads to a modest shift from higher- to lower-paying firms.

Finally, having documented that firm heterogeneity plays a substantial role in explaining labor market disparities for WCRs, we investigate potential mechanisms underlying firms' willingness to hire WCRs. First, we investigate firm-level policies, documenting that firms who signal in their job ads that they will conduct a background check as part of the hiring process are less likely to hire WCRs, consistent with these firms using background checks to screen out applicants with records. Second, we investigate the role of establishment managers' prior exposure to criminal records in shaping their hiring of WCRs. We find that managers who themselves have a criminal record, have a family member with a record, or have been exposed to more coworkers with records at their previous establishment(s) are more likely to hire WCRs, conditional on detailed demographic, industry, and location controls. Moreover, these effects are specific to the hiring of WCRs rather than comparable suspects without a record, indicating that they do not reflect selection in the types of firms WCRs and exposed managers sort into, but rather that exposure shapes managers' willingness to hire WCRs.

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 persistent effects of job displacement, the depreciation of human and social capital while in prison (Western et al., 2001), or the stigma of a resulting criminal record. Our paper advances this literature by isolating the

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

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. Existing studies on the labor market effects of arrest and conviction (Grogger, 1995; Rose, 2021; Agan et al., 2024a) typically compare those charged at younger ages to those charged later at older ages in an event study framework. While broadly applicable, this research design has two limitations: (1) the approach does not account for the immediate circumstances leading up to an offense and (2) the comparison group is mechanically more likely to commit an offense in the future. Our novel research design, which uses individuals suspected of an offense but not charged as the comparison group, addresses both limitations. First, it allows us to net out the potential effects of the circumstances leading to an alleged offense or arrest. Indeed, we find that matched non-charged suspects also experience a dip in their employment rate growth. Second, both groups have comparable risks of committing future offenses.

A second body of literature examines how a criminal record affects firm labor demand and the extent to which firms discriminate against WCRs. 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 that they have 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 involving 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 measures callback rates or firm stated preferences rather than actual employment or earnings outcomes, and it typically relies on a small and selected sample of firms. 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 into 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 discrimi-

natory 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. We also find that more discriminatory firms provide higher wage premiums.

The second limitation is that much of the literature is based in the United States, an outlier compared to the rest of the world when it comes to the accessibility, visibility, and breadth of criminal records.[4] In the US, unlike most of Europe, including Sweden, arrests are often included in background checks, background checks are widespread, records are often public, and automatic expungement rules are the exception. The Swedish context thus allows us to conduct the first study of how records affect employment and earnings in Europe and our findings can inform an active US debate about various policies aimed at mitigating the negative consequences of a criminal record.[5]

A third related literature examines the role of firms and managers in hiring workers from disadvantaged groups. As noted above, firm surveys (Cullen et al., 2023; Bushway and Pickett, 2024) and studies of hypothetical hiring scenarios show substantial variation in employer preferences and policies related to the hiring WCRs. But do these differences influence actual hiring decisions? And why are some firms, even within the same industry, more receptive to hiring WCRs than others? To what extent does manager heterogeneity contribute to these patterns? The existing literature (see Hoffman and Stanton (2024) for a recent survey and Lazear and Shaw (2007) for an older review) has little to say on the answers to these questions.[6] We begin to unpack the sources of firm heterogeneity in WCR hiring by demonstrating that both explicit firm-level policies and manager characteristics play a an important 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. More generally, our findings are consistent with a broader literature on how contact, experience, and exposure shape attitudes towards disadvantaged groups (Paluck et al., 2019; Dahl

[4]See (Ahmen and Lång, 2017) and (Baert and Verhofstadt, 2015) for two exceptions in terms of audit and correspondence studies, (Bhuller et al., 2020) for the effects of prison time in Norway, and (Dasgupta et al., 2021) for record expungement in New Zealand. A contemporaneous study (Forsberg, 2024) in Sweden aims to evaluate the effects of introducing the online record depository, Lexbase, in 2014.

[5]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) and Agan et al. (2024a) for evaluations of clean slate laws.

[6]One recent exception is the work by Adams-Prassl et al. (2024) that looks at the role played by managers after violent conflicts between colleagues at their firm. Manager gender significantly impacts the consequences for the perpetrators of such crimes. Male violence decreases the proportion of women at male-managed firms. Male perpetrators with female victims are more likely to lose their jobs at female-managed firms.

et al., 2021), including in the labor market more specifically (Miller, 2017; Li et al., 2020; Benson and Lepage, 2024; Lepage, 2024).

The remainder of the paper proceeds as follows. Section 2 provides institutional details on the Swedish criminal justice system, including the rules related to criminal records and background checks. Section 3 describes the Swedish crime and employer-employee registries and supplementary data. Section 4 presents new descriptive facts about the labor market for WCRs. Section 5 presents the charged versus non-charged suspects research design and results. Section 6 explores the role of firm and manager heterogeneity in explaining the labor market outcomes for WCRs. Section 7 concludes.

2 Institutional Background

This section explains how a person suspected of a crime proceeds through the Swedish justice system and provides an overview of background checks and automatic expungement rules.

2.1 Crime and the criminal justice system

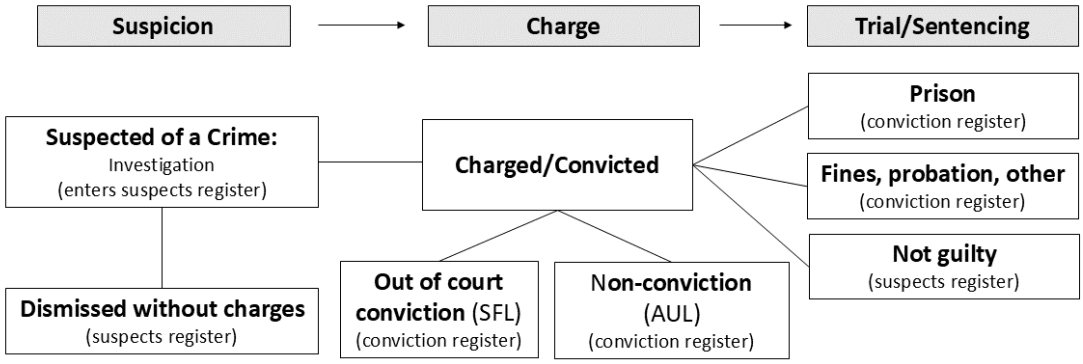
Figure 1 depicts how a suspect progresses through the Swedish criminal justice system. This will inform our charged versus non-charged suspect research design in section 5.1. Upon investigating a crime, police identify and often arrest suspects and pass the case to a prosecutor. Suspects are then informed of the suspicion, including the suspected crimes. The prosecutor (or police for minor offenses) continues the investigation. Depending on the evidence obtained, the prosecutor decides whether to charge the suspect or if the case should be dismissed without charges. The latter is recorded in the suspects register, often including the reason for dismissal. The most common stated reason for dismissal is insufficient evidence; it is rare for prosecutors to conclude 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, and (iv) in-court acquittal[7] Non-convictions and (in and out of court) convictions include a determination of guilt and lead an individual to have 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

[7]In-court acquittals are relatively rare. According to statistics provided by Dobbie et al. (2018), 6% of in-court trials result in acquittal.

least one crime in 2010. 54% were found guilty of their most serious charge: 5% AUL, 15% SFL, and 34% in-court. Only 6% of all suspects are eventually sentenced to prison.

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.

2.2 Background checks

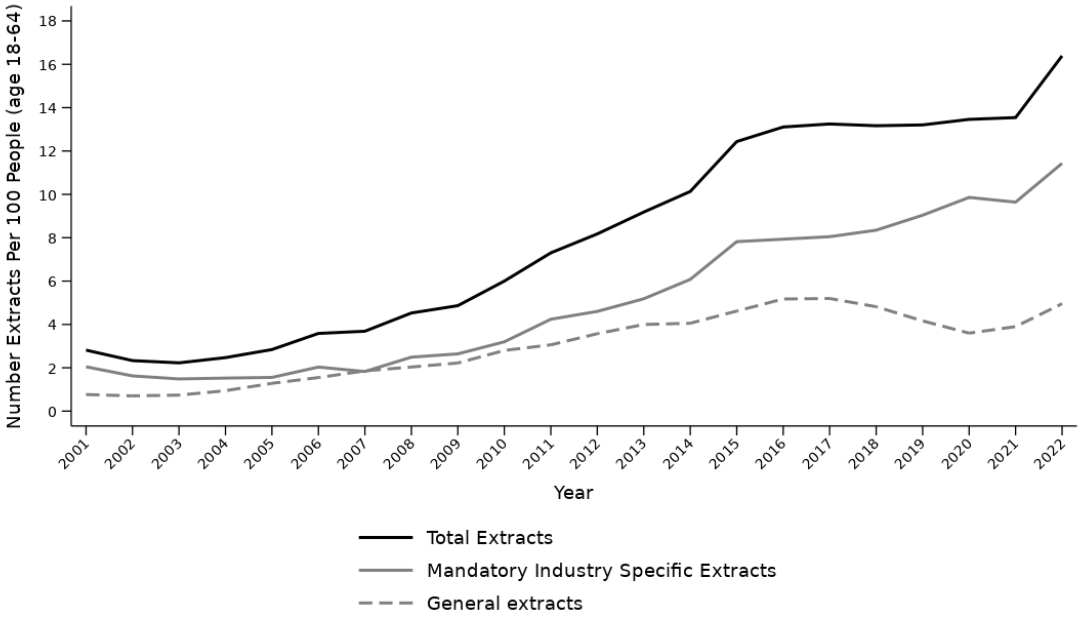
Until the emergence of online companies like Lexbase in 2014, there were two channels through which Swedish firms could conduct background checks. First, since the late 1960s, many employers (e.g., in criminal justice, public employment, energy, health, transportation, and defense) have had direct access to criminal record registries (termed in Swedish *belastningsregistret*). Second, employers without direct access can ask applicants 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. The police website emphasizes that a criminal record is not disqualifying, even if the background check is mandatory.[8]

The information visible in the extract depends on the type and timing of the request. Since 1989, individuals have been allowed to independently obtain what we call a "general extract", which includes all offenses, regardless of type, that are not yet expunged. Starting in the early 2000s,

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

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). See Appendix A.3 for more details on the roll-out of such regulations. Such industry-specific mandatory extracts only show selected offenses that are deemed relevant (by the government) to such jobs.[9]

FIGURE 2
ANNUAL EXTRACT REQUESTS FROM POLICE REGISTRY



Note: This figure presents the annual number of industry-specific mandatory extracts (gray dashed line) and general extracts (gray solid line) obtained from the Swedish police, as well as the combined total (black line). Annual 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.

Figure 2 shows the annual number of registry extracts per 100 people aged 18 to 64 in Sweden, overall and separately for mandatory and self-initiated extracts. 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.[10]

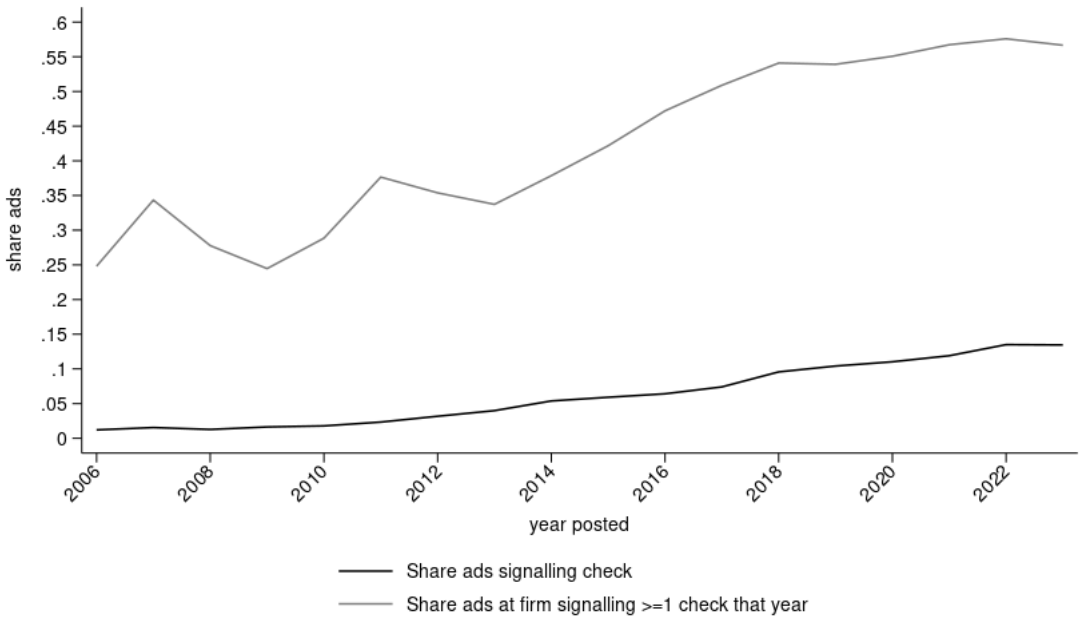
Further insights into the prevalence of background checks–over time and across firms, industries, and occupations–can be gained from a novel supplementary data set of Swedish job ads we

[9]For a list of selected offenses for each 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/ .

[10]A media analysis presented in Appendix Figure C.5 further demonstrates the increasing role of criminal record extracts over time and that they were already highly salient before the temporary media splash of Lexbase in 2014. See Forsberg (2024) for an analysis of the impact of making records pubicly searchable via Lexbase.

constructed (described in section 3.3), which identifies whether a firm signals in the ad that they will conduct a background check during the hiring process. Figure 3 shows that the percentage of ads (black line) signaling background checks increased from less than 5% in the mid-2000s to around 15% in 2022. These percentages, however, understate the extent to which employees work at firms that (at least sometimes) use background checks. The gray line plots the percentage of ads at firms that signaled a background check in at least one ad that year, increasing from around 30% to about 55% over the sample period. The same data also show substantial variation in the prevalence of background check signals across broad occupation categories, ranging from less the 4% of ads for resources/agriculture and construction and manufacturing jobs to 11% of professional ads and 18% of ads for elementary unskilled jobs.[11]

FIGURE 3
PREVALENCE OF JOB AD BACKGROUND CHECK SIGNALS OVER TIME AND ACROSS FIRMS



Note: This figure presents the share of job ads in the Platsbanken database signaling that a criminal background check or registry extract will be part of the recruitment process, as based on the author's coding of the ad text. It also shows the share of ads at a firm signaling at least one check that year. This figure is based on the sample of nearly 7 million ads for which we could confidently match the job ad to firm identifiers maintained by Statistics Sweden.

There are some stark differences between Sweden and the United States, where background

[11]These patterns are consistent with results of a 2014 survey of Swedish firms (SOU 2014:48), which we present in Appendix Figure C.4. Fourteen percent of firms, but more than 50% of the largest, report background checks for all or some new hires.

checks are primarily conducted by specialized companies (Agan et al., 2024a) and information concerning many criminal justice interactions, often including arrests, are accessible to the public. European privacy laws sharply restrict access to criminal record information, especially arrest records. However, European employers (including those in Sweden) generally have the right to ask candidates for record extracts or about their criminal history. Finally, though background checks are sometimes mandatory and employers may screen records to minimize risks to their reputation, customers, and employees, hiring WCRs does not expose Swedish employers to negligent hiring liability as it may in the U.S.

2.3 Record expungement rules and reforms

In Sweden, all criminal offenses are automatically expunged from an individual's criminal record, regardless of severity. The timing of expungement–and hence the years during which an offense is visible during a background check–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 other European countries but in sharp contrast to the US, where clean slate laws remain uncommon.

According to current regulations, most offenses will be in the registry for 10 years if committed when 18 or older but only 5 years if committed by those younger than 18. 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 details changes in the expungement regulations from 1990 to today, a thorough understanding of which is necessary to code whether an individual has a 'visible' or 'active' criminal record in each year and the nature of that record.

3 Data

3.1 Swedish registers

For the entire population born between 1923 and 2000, we combine matched employee-firm registers with two core crime data sets from the Swedish National Council for Crime Prevention. The Con-

victions Register spans 1973 to 2015 and provides a complete history of an individual's convictions and sentences, including offense and conviction dates, crime types, and sanction type (e.g., prison, probation, fines) and intensity (e.g., sentence length). The Suspects Register is available from 1995 onwards, and also includes crime types and offense and/or decision dates. Suspects are added to this register as soon as they are arrested or connected to a specific crime. 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.

The labor market data primarily span 1990 to 2015 and allow us to trace each offender's employment history, including earnings as well as unique firm and workplace identifiers. We observe key firm characteristics (e.g., sector, industry, number of employees, sales, geographic location) and, for unlisted incorporated and unincorporated firms, the (anonymized) identity of firm owners. For the latter sample years, Statistics Sweden reports detailed 4-digit occupation codes for almost all workers, which we can also use to help identify establishment managers. Our ability to track WCR across establishments and occupations as well as identify their co-workers (including their criminal history) is a novel aspect of the Swedish register context.

Finally, these data are combined with registers that provide basic demographic information on each individual, including gender, citizenship, emigration and immigration years, birth year, educational attainment.[12] Annual earnings are measured in real Swedish krona (SEK) using 2015 prices. For men born between 1951 and 1987, we also use data on cognitive test scores during military enlistment.

3.2 Core analysis sample and key variables

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. We observe about 10 million individuals who are alive and living in Sweden during at least one of these years. The baseline data set includes workers between the ages of 18 and 55. Our analysis sample is composed of 126.8 million person-year observations from 8.6 people. These individuals are observed to work in more than 1.4 million firms, but the composition of firms changes over time; about 330,000 unique firms per year are observed in our baseline analysis sample. We keep each worker's primary employer for the year in terms of total earnings. We use this sample to characterize the labor market for workers with and without criminal

[12]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.

records, but make additional sample restrictions in our event study analysis in section 5. We also use samples of newly-hired workers in section 6 for our analyses of mechanisms.

A central variable in our analyses is whether an individual has an active criminal record each year. Specifically, we measure whether an individual has been convicted of a non-traffic ticket offense in the past that is recorded in the background check register (*belastningsregistret*), but not yet expunged using the detailed expungement rules (described in section 2.3 and Appendix A.1) in place each year.

3.3 Supplementary data: occupational characteristics and swedish job ads

Occupational characteristics: To study the kinds of occupations into which WCRs are sorted, we use the US Department of Labor's Occupational Information Network (O*Net). These data measure a wide range of occupational characteristics for traditional skill requirements like average education (1-12 scale) and the necessary extent of preparation (job zone: 1-5 scale). We also 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. We also measure the extent to which the occupation entails hazardous or risky work conditions. As 4-digit Swedish occupation codes do not correspond directly to US codes, we created US-Swedish occupational crosswalks.[13] The O*NET data, variable creation, crosswalk and matching procedure are described in Appendix A.2. Second, for each 4-digit Swedish occupation code and year, we create variables characterizing the extent to which background checks are required upon hiring or licensing. The underlying laws are described in Appendix A.3.

Job advertisement background Check Signals: Finally, we use a data set of more than 9 million job advertisements from 2006 to 2023 from the largest job board in Sweden, Platsbanken, which is maintained by the Public Employment Service. These data include the text of the ad and a number of ready-tagged variables (e.g., firm name, date, number of vacancies, occupation codes, location). For all ads, we search the text for keywords – 'belastningsregist', 'registerkontroll', 'bakgrundskontroll' – to measure whether the employer signals that a background check is part of the recruitment process. We conduct other text searches to measure other attributes of the process (e.g., drug and alcohol testing). Finally, we were able to link about 70% of ads to the firms that posted them in our employer-employee register using employer identification numbers. This

[13] Occupational codes in Sweden changed in the register data from SSYK96 to SSYK2012 in 2013.

allows us to directly investigate how job ad background check signals translate to actual hiring of WCRs by these firms. Such firm-level measures of willingness to hire WCRs have thus far not been available in the literature, beyond self-reported measures from firm surveys. See Appendix A.4 for details on the job ad data and linking procedure.

4 Descriptive Facts about the Labor Market for WCRs

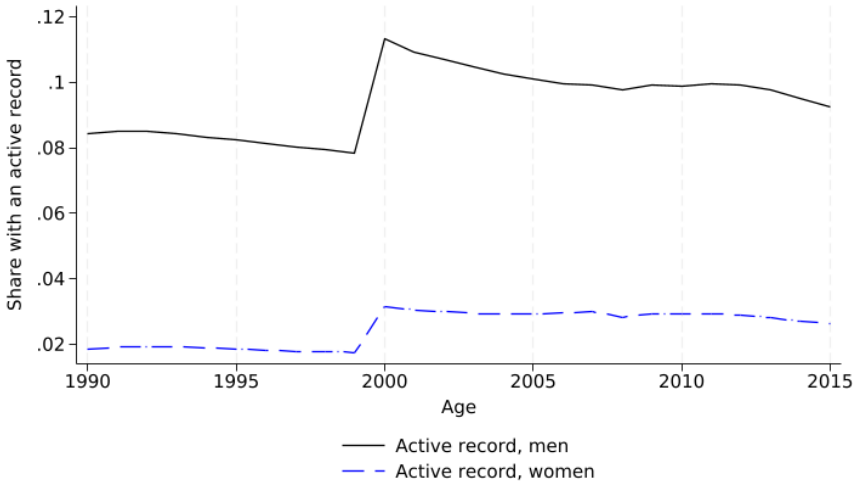
4.1 Prevalence of WCRs

We begin by describing the prevalence of criminal records in our sample. Figure 4 shows the percentage of workers with an active criminal record from 1990 and 2015 (panel A) and by worker age in 2015 (panel B). Over the sample period, the average percentage of men and women with an active 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 with an active record in 2000; this is driven not by a jump in crime but rather by a reform (see section 2.3) that included more minor offenses in the record registry. Consistent with the typical age profile of criminal behavior, the percentage of adults with an active record initially increases in 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 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 Europe.[14]

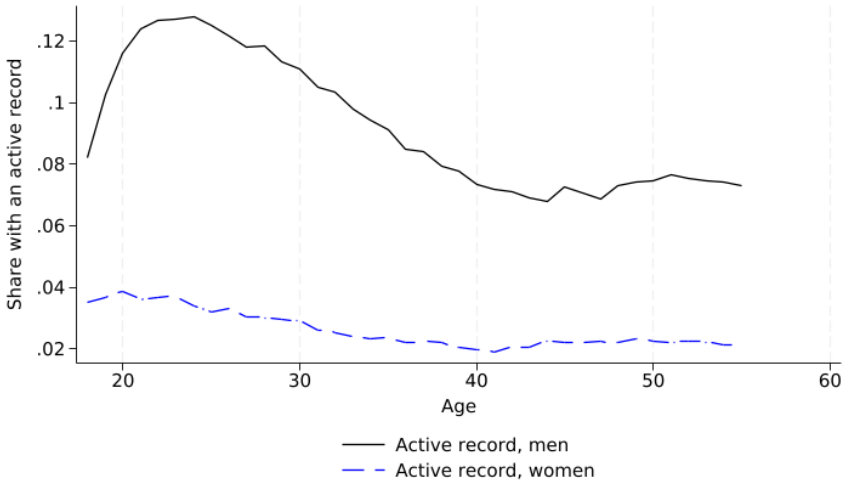
[14] A commonly cited statistic is that one in three US adults have been arrested by age 23. Brame et al. (2012) estimate for instance using the NLSY97 that 30-40% have been arrested by age 23. Shannon et al. (2017) estimate that 8% of all adults (male and female) in the US have a felony conviction. 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.

FIGURE 4
PREVALENCE OF WORKERS WITH CRIMINAL RECORDS

(a) Share of workforce with an active record over time



(b) Share of workforce with an active record by age, 2015



Note: Panel A plots the percentage of prime-age adults in each year of our sample with an active 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.

Among workers with active records, 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. Out of the workers with an active record, 19% were

sentenced to prison in the last 10 years, for a median combined sentence of 120 days.[15]

4.2 Characteristics of WCRs

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

In the labor market, WCRs work on average two and a half fewer months per year (6.994 versus 9.588) and have average earnings that are about 40% lower (122k SEK versus 204k SEK).[16] WCRs also tend to work in establishments and firms that are smaller and in establishments where a higher share of their co-workers also have active records (10.63% versus 4.773%). They are more likely to work in construction, 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 the earnings differences between those groups. 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 year, tau_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 levels, gender, and an indicator for Swedish-born. $\sigma_{s(i,t)}$ are sector fixed effects, where the sector is either defined as broad industry or firm. We examine three outcomes: total earnings, months worked, and log 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_i and year fixed effects as explanatory variables. The β coefficient is -85.42, meaning that, conditional on year, the difference in earnings between

[15] Sweden has historically had shorter prison sentences than the US and Western Europe: the average time spent in prison for Swedish inmates convicted from 1991 to 2001 was 4.7 months, 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).

[16] Appendix Figure C.6 demonstrates that substantial gaps between workers with and without records are seen throughout the sample period, and tend to be larger during periods of high unemployment.

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)
Workplace size	446.1	283.6
	(1264.1)	(957.9)
Firm size	6393.6	4359.2
	(13519.2)	(11698.1)
Average coworker earnings (1000s SEK)	240.6	209.8
	(123.7)	(107.8)
Average firm coworker earnings (1000s SEK)	234.0	209.9
	(115.2)	(102.0)
Firm pay premium	0.0274	0.00325
	(0.220)	(0.258)
% of coworkers with active records	4.773	10.63
	(8.200)	(14.69)
% of firm coworkers with active records	4.905	9.831
	(7.455)	(13.73)
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 an active 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.

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_i and year fixed effects as explanatory variables. The β 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 specification includes only RECORD_i and year fixed effects as explanatory variables, the β 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 1-digit industry fixed effects. The β 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 C.5 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.[17]

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); Card et al. (2018). (Details on estimation can be found in Appendix B.) Column 1 includes only RECORD_i and year fixed effects as explanatory variables. The β coefficient is -0.06, indicating that WCRs work in firms that pay about 6% lower wages.

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

TABLE 2
LABOR MARKET OUTCOMES BY WCR STATUS, 1990-2015

	Earnings		Months Worked		Log Monthly Earnings			
	Raw	+Demo.	Raw	+Demo.	Raw	+Demo.	+Industry	+Firm
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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		9.59		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 total 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 is reported; standard errors clustered at the person level are reported in parentheses.

TABLE 3
FIRM CHARACTERISTICS BY WCR STATUS, 1990-2015

	Firm pay premium			% of coworkers with record		
	Raw	+Demo.	+Industry	Raw	+Demo.	+Industry
	(1)	(2)	(3)	(4)	(5)	(6)
Criminal record	-0.06	-0.10	-0.08	5.66	3.44	3.30
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		✓	✓		✓	✓
Industry FEs			✓			✓
Non-WCR outcome mean					4.77	
% difference				119	72	69
Observations		96,439,914			88,052,526	

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 co-workers 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.

Interestingly, adding demographic controls in column 2 increases the magnitude of the coefficient to -0.10. Column 3 adds fixed effects for 1-digit industry, reducing the coefficient magnitude to -0.08. Most of the sorting by firm pay premium is within industry. In columns 4 through 6 the outcome is the percentage of establishment coworkers that have an active criminal record. Column 4 includes only RECORD_i and year fixed effects as explanatory variables. The β coefficient is 5.66, indicating that WCRs work in establishments with a 5.66pp higher share of coworkers with an active record. Adding demographic controls (column 5) reduces the coefficient to 3.44pp. 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 even within, industries. This section, thus, considers the extent to which WCRs work in different occupations than non-WCRs. Appendix Table C.6 lists the 20 4-digit occupations in which WCRs are most and least prevalent in 2015, as well as their corresponding share of workers with records. This is less than 0.5% in the 20 least WCR prevalent occupations, which includes therapists, judges, prosecutors, and chemists, and a number of education and care-related occupations for children or the elderly. Background checks

are mandatory for employment or occupational licensing in more than half of these occupations. One can also see from the 1st digit of the occupation code that WCRs are least prevalent in the broad occupation categories of senior officials and managers (1st digit = 1) and professionals (1st digit = 2). In contrast, the occupations in which WCR are most concentrated are primarily classified as crafts and trade, plants and machines, and elementary unskilled occupations (1st digit = 7, 8, and 9, respectively). Some tangible examples include taxi, car and van drivers (12%) and many construction and manual labor 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%). The one service occupation is telemarketers (12.2%)–an occupation where customer interactions are not in-person.

Such occupational sorting can of course simply occur as a result of selection on the basis of many observable characteristics (e.g., gender, age, education) that are highly correlated with WCR status. We thus document how WCRs are distributed across occupations, focusing on residual variation after conditioning on worker demographic characteristics and broad industry categories. We first characterize the occupation-specific concentration of WCRs using the following linear regression model:

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

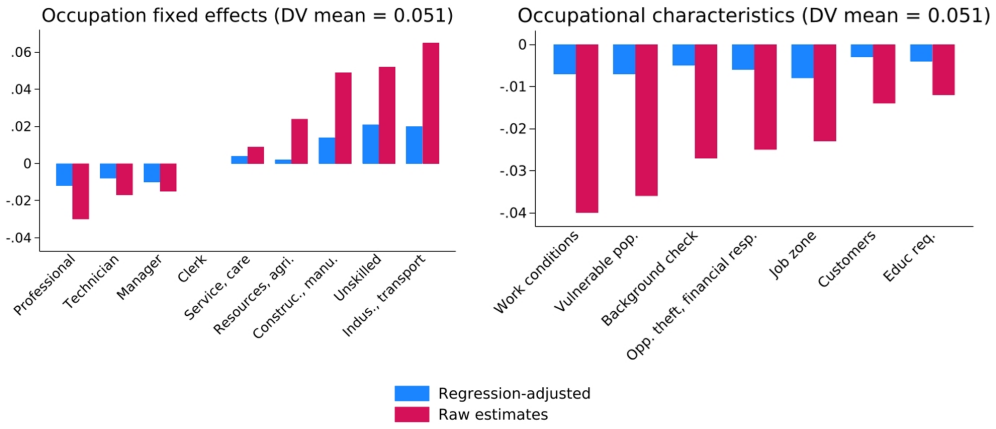
where t indexes year. We estimate equation (2) using data from 2005-2015.[18] The vector X_{it} includes fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born. $\gamma_{k(i,t)}$ are fixed effects for 1-digit industry categories, and $\sigma_{s(i,t)}$ are fixed effects for broad occupation categories (i.e., first digit of the occupation code).

Panel A of Figure 5 reports the $\sigma_{s(i,t)}$ coefficient estimates from Equation (2) when the other controls are excluded (*Raw*) and included (*Regression-adjusted*). Confidence intervals are omitted from the figure since most are too small to visualize distinctively - all estimates are statistically significant at the 1% level. It indicates that, even conditional on demographics and industry, WCRs are not evenly distributed across broad occupation categories. They 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. Even the adjusted differences can be substantial–WCRs are about 40% less likely to be employed as professionals than in

[18] Occupation information is missing for 17.6% of observations during this time period. We eventually plan to update this section using data from more recent years where the missing rate of occupation is much lower.

elementary unskilled occupations.

FIGURE 5
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 the 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 coefficient 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 most are too small to visualize distinctively - all estimates are statistically significant at the 1% level.

To better understand this broad occupational sorting, we look at the characteristics of occupations using our supplementary O*NET data and occupation by year specific mandatory background check requirements. See section 3.3 and Appendices A.2 and A.3 for detailed descriptions of these data. Panel B of Figure 5 reports raw and adjusted coefficient estimates from Equation (2), replacing occupation fixed effects with a series of occupation characteristics. The raw estimates indicate that WCR are less likely to work in occupations: (i) with 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) in which background checks are mandatory, (iii) with higher education and skill (job zone) requirements, and (iv) with better work conditions (i.e., when 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 more than 10% less likely to work in occupations with better-than-average work conditions, involve contact with vulnerable populations, or with higher-than-average skill/experience requirements, and 6% less likely to work in occupations that mandate background checks during the hiring process.[19]

[19]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.

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. Among those who commit or are suspected of an offense, people with low income may be more likely to be charged and end up with a criminal record. Causality may also flow in the reverse direction: 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 both due to the conviction itself and to the circumstances that led the person to be arrested in the first place (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.

Our approach levers 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. Typically, both groups 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.[20] Our main identifying assumption is that, absent the charge, the two

[20] Individual names are rarely reported in Swedish media for individuals who are suspected of a crime; two uncommon exceptions are for cases in the public interest (e.g., politicians) or when the police are searching for suspects of

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 they engaged in, the people they associated with, and their social and economic background and prospects.

Second, it is reasonable to believe the two groups have engaged in similar conduct. In particular, prosecutor charge decisions are influenced by important factors that are 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 suspect 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 decision to pursue charges.

Consistent with an important role for prosecutor discretion–or systematic variation in evidence quality–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, prosecutors pursue charges for 55% of suspect 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%) or both the offense and the suspect involved (8.7%).

Ultimately, we will use the data to assess the validity of this research design. Specifically, we will show that, once properly matched, the treatment and comparison groups (1) 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

This section describes our core sample, the differences between charged and non-charged suspects, and the matching procedure.

Our goal is to create a sample of people who are charged with a criminal offense for the first

very serious offenses.

time and match them with a similar set of people who are suspected but not charged with a similar offense 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:

1. does not have a prior charged offense,
2. is between the ages of 22 and 40 in the event year, and
3. 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 for at least one offense.[21] We define these suspects as *charged*.

Charged and non-charged suspects are suspected of different types of offenses. 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 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.

[21] This is comparable to the share of criminal charges that are pursued in US state courts rather than dropped or dismissed.

Table 4

Characteristics for suspects and suspected offenses

	All		Matched and Weighted	
	Suspects	Convicted	Suspects	Convicted
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	0.392	0.262	0.209	0.209
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)
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 coworker earnings (1000s SEK)	212.1	205.7	205.0	208.1
	(114.5)	(104.5)	(110.1)	(105.2)
% of coworkers with active records	8.634	8.833	8.247	8.396
	(12.68)	(12.63)	(12.34)	(12.16)
Firm pay premium at main job	0.182	0.206	0.192	0.229
	(0.794)	(0.798)	(0.800)	(0.786)
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 offenses 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. Sentence type refers to the more 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.

156.0K SEK) and months of employment (8.222 vs. 7.990). Interestingly, the two groups work in similar industries and in establishments where about 9% 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. We denote these cases as *major* offenses. We categorize income by dividing suspects into quintiles within each combination of year and age at the time of the suspected offense.[22]

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 60,000 events in the treatment group.

Note that the same person can be included 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 events at ages 22 and 26 to the comparison group and one event at age 30 to the treatment group. We cluster standard errors at the person level.

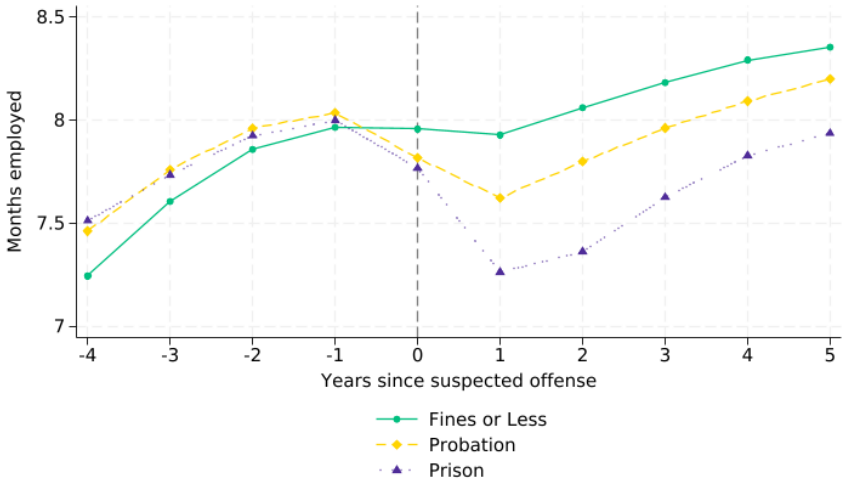
By construction, matched charged and non-charged suspects have identical suspected offense characteristics. They also have more similar demographic and job characteristics, even on dimensions that we do not match directly; the share Swedish born and average months worked are now statistically indistinguishable.

[22]Suspects with zero earnings are included in this categorization.

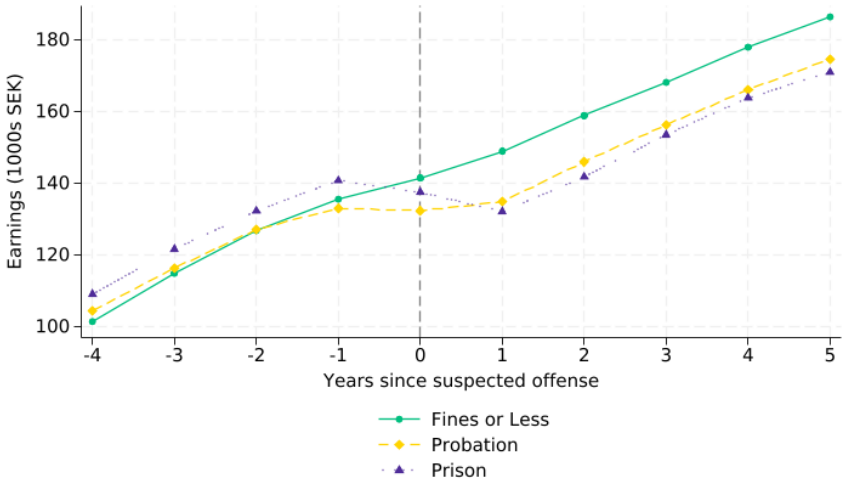
Before discussing the matched event study design results in the next section, we first describe the labor market trends for the treatment group. We group charged suspects in our final matched sample by their sentence: fine or less (59.14%), probation (33.28%), and prison (7.58%).

FIGURE 6
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.

Figure 6 plots months employed (panel A) and earnings (panel B) 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 decreases significantly, and earnings decrease or plateau for those receiving probation and prison sentences. The fact that prison sentences are uncommon and earnings decrease significantly for more common 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, though earnings continue to increase on a similar linear trend.

While the patterns in Figure 6 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 these trajectories for matched charged and non-charged suspects, where the latter group will stand in for the counterfactual of the former.

5.3 Results

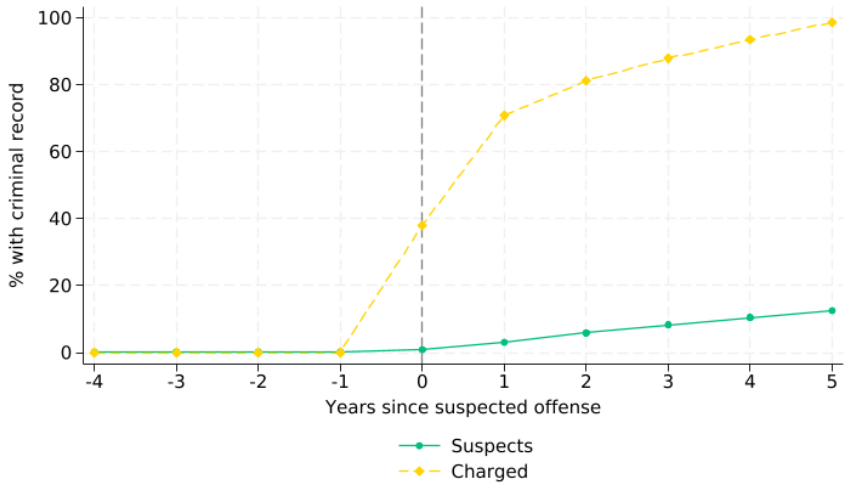
We present results in three ways. First, we plot (weighted) means for charged and non-charged suspects in event time. Second, we estimate traditional event study models using two-way fixed effects and report coefficient estimates. Our matching strategy circumvents recent concerns with these models by 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 first look at the percentage of suspects with a criminal record. This is akin to a first stage. Panel A of Figure 7 shows the 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. 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

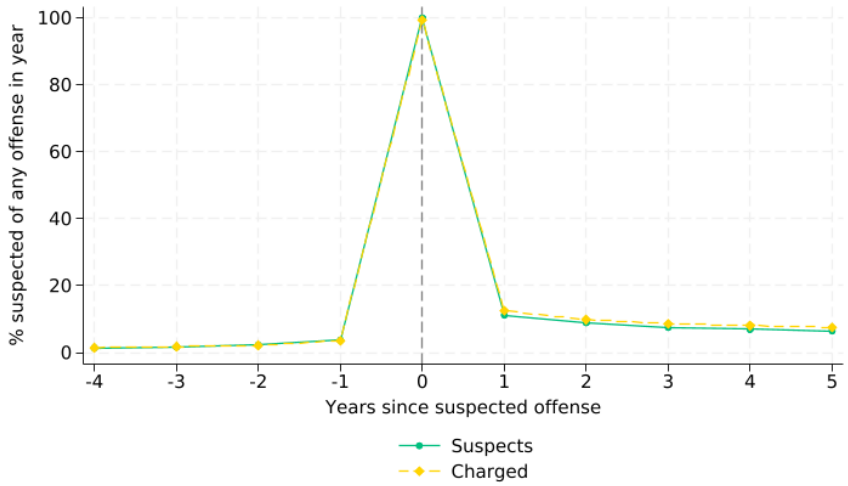
has an active record, an 86 percentage point difference between the treatment and comparison groups.

FIGURE 7
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.

Naturally, the treatment group is also subject to more significant punishments. On average, the

treatment group faces an additional 15 days in cumulative confinement sentences and 3,500 SEK in daily fines by five years post-event. About 8% of the treatment group and 2% of the comparison group have received any jail or prison sentence.

Next, 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, specific deterrence effects.

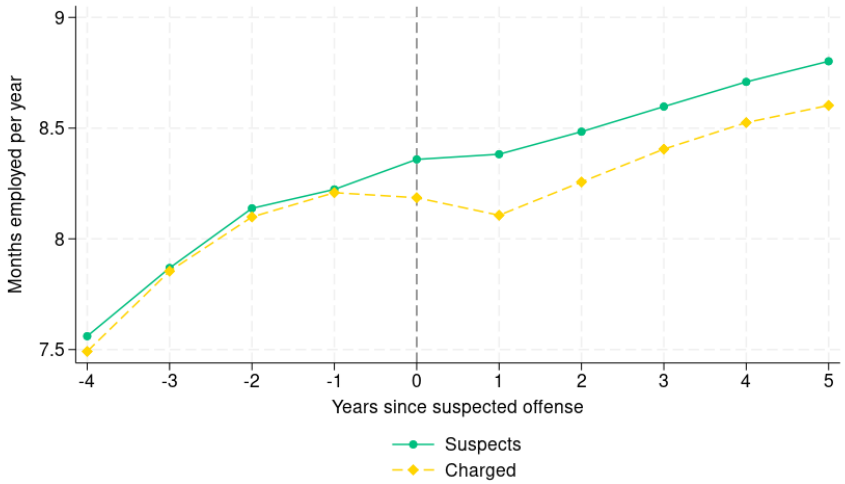
The results are shown in panel B of Figure 7. By construction, the share suspected of an offense is 100% for both the treatment and comparison groups at event time $t = 0$. The groups are matched on their number of years with suspected offenses before the event, though they were not matched on the timing 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 that (1) the treatment group has a slightly higher baseline propensity to engage in criminal activity; (2) acquiring a criminal record slightly increases rates of offending; (3) having a criminal record slightly increases the likelihood that law enforcement suspects someone of an offense, given similar conduct.

We next turn to labor market outcomes. We examine months employed in panel A and earnings in panel B of Figure 8. 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 increases 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 year. A gap between charged and non-charged suspects of about 0.2 months persists for at least four years, which is

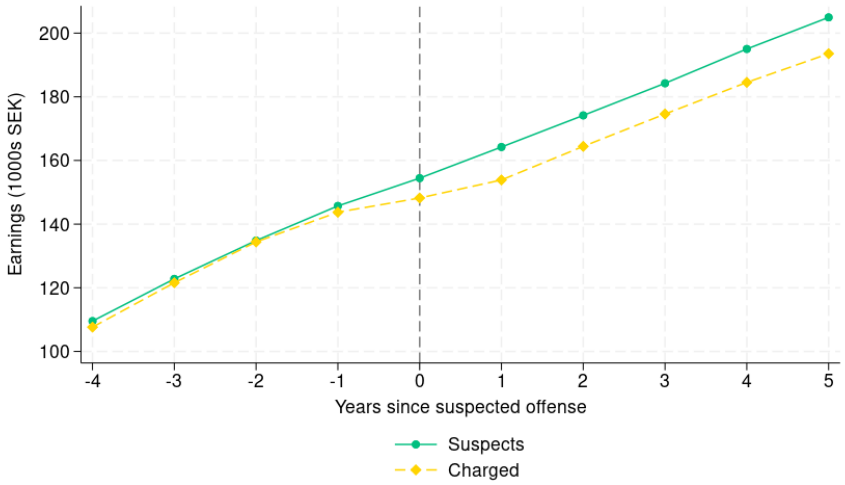
about 2% of employment in the comparison group. This largely reflects a difference in the share of people with significant unemployment spells. Charged suspects are 1.5 percentage points less likely to work more than 6 months in the year, compared to a baseline of about 70% among non-charged suspects.

Figure 8
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.

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. For a smaller sample of events from 1995 to 2001, this gap in earnings persists for over decade, after most records are expunged (see Appendix Figure C.10).

We replicate our findings using event study models of the form

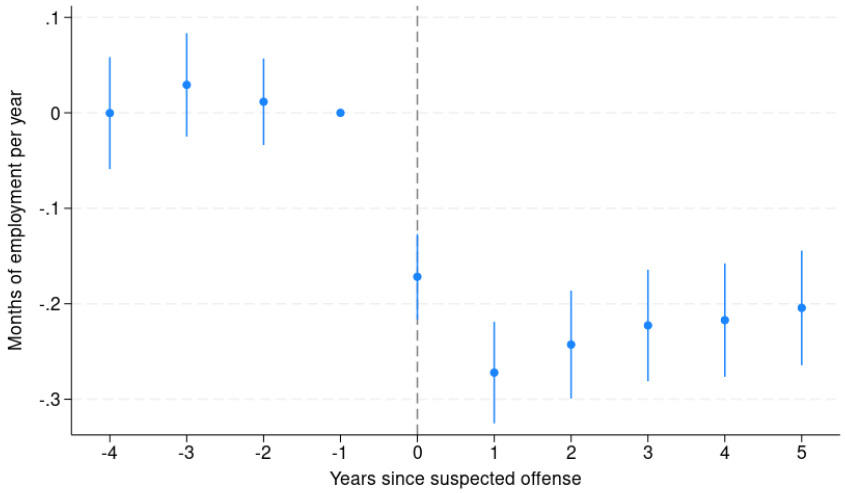
$$Y_{i\tau t} = \gamma_{c(i,\tau),t} + \sum_{j=-4}^{5} \theta_j D^j_{i\tau t} + \epsilon_{i\tau t} \tag{3}$$

where i indexes people, τ indexes the year of focal suspected offense, and t indexes years. $\gamma_{c(i,\tau),t}$ 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 $D^j_{i\tau t} = D_{i\tau} 1_{t=\tau+k}$, where $D_{i\tau} = 1$ if (i,τ) corresponds to a suspected offense that leads to a criminal charge.

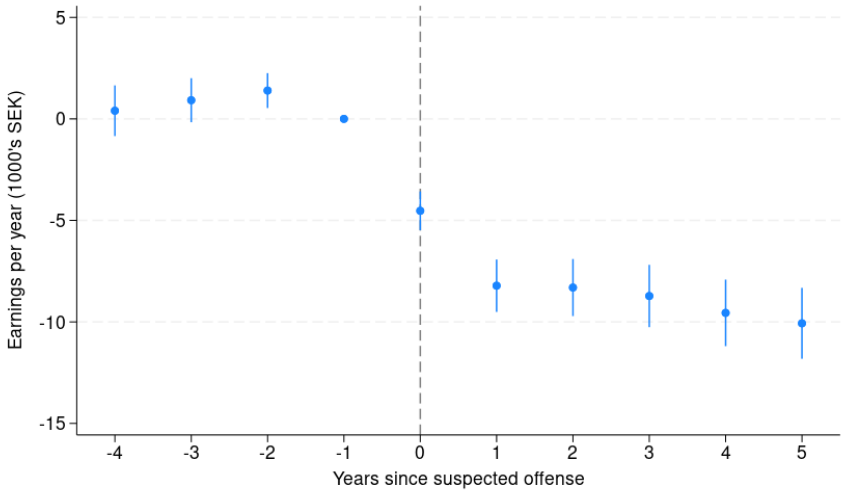
Figure 9 provides coefficient estimates (θ_j) for months employed (panel A) and earnings (panel B). The same patterns emerge: five years post-event, months employed decreases by about 0.2 months and earnings decrease by about 10,000 SEK.

FIGURE 9
EVENT STUDY ESTIMATES FOR EMPLOYMENT AND EARNINGS

(a) Months employed



(b) Earnings



Note: This figure plots the event time coefficients (θ_j) from the event study specification (3). In panel A the outcome is months employed. In panel B the outcome is earnings.

5.4 Heterogeneous effects

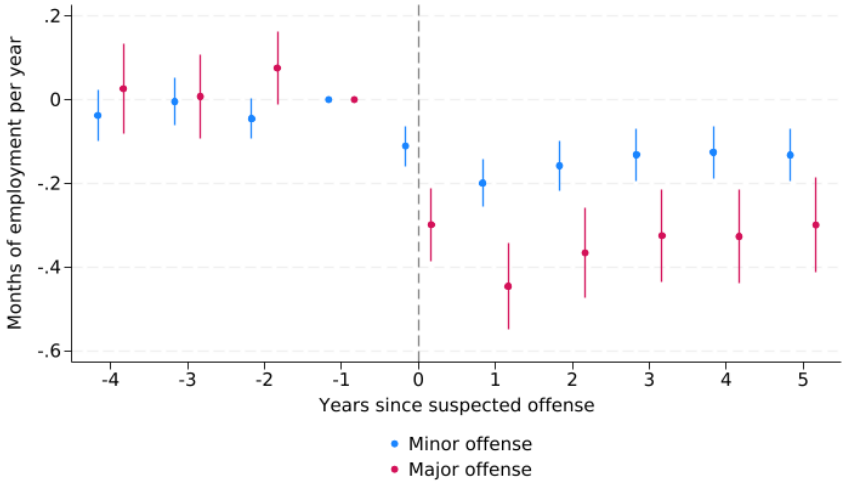
In this section, we measure the heterogeneous effects of acquiring a criminal record as a function of suspected offense characteristics, suspect characteristics, and contextual factors. Given our matched design, to measure heterogeneous effects we estimate our event study models using subsamples of

data and then compare coefficient estimates across subsamples.

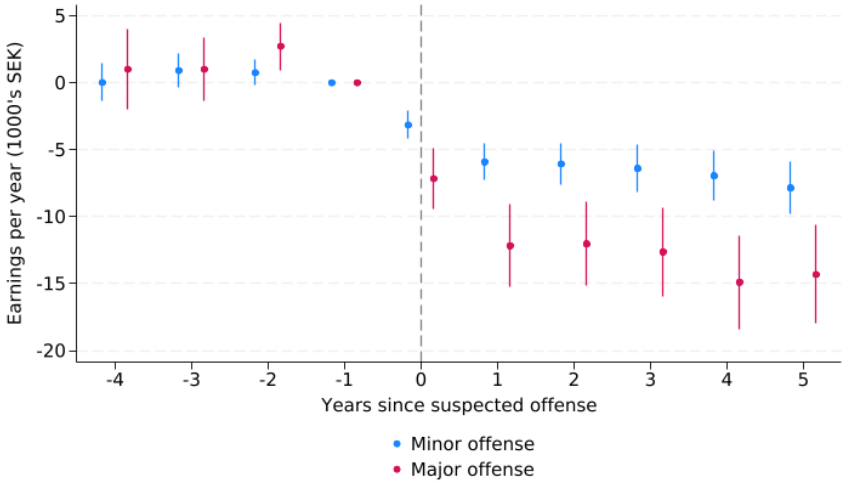
We first look at offense severity. We split the sample by whether the set of suspected offenses includes any major offense. About 20% of events are associated with a major offense. Figure 10 shows event study estimates for months employed (panel A) and earnings (panel B).

FIGURE 10
EMPLOYMENT AND EARNINGS EFFECTS BY OFFENSE SEVERITY

(a) Months employed



(b) Earnings

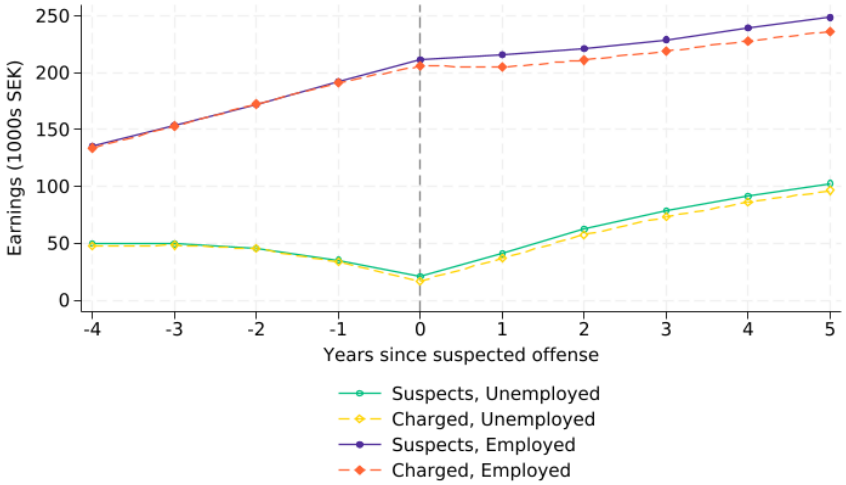


Note: This figure plots the event time coefficients (θ_j) from the event study specification (3), where the specification is estimated separately for events corresponding to minor and major offenses. A event includes a major offense if any specific suspected offense code falls in the top quartile of 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.

Effects on both outcomes are larger in absolute terms for more serious offenses. Months employed decreases by about 0.15 for less serious offenses and about 0.3 to 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 decreases by about 2% for less serious offenses and 4% for more serious offenses. Earnings effects follow a similar pattern, decreasing by about 4% for less serious offenses and 8% for more serious offenses.

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. 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. This induced job displacement may have negative long-term labor market consequences that are separate from the effects of a record per se. We split the sample by employment status at baseline to clarify the role of job displacement for the effects we document. The motivation for this exercise is that if suspects with and without jobs both experience significant negative effects when charged, that suggests the role of job displacement is limited.

FIGURE 11
EARNINGS EFFECTS BY BASELINE EMPLOYMENT STATUS



Note: This figure plots the event time coefficients (θ_j) from the event study specification (3), 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.

TABLE 5
HETEROGENEITY IN THE EARNINGS EFFECT OF A CHARGED OFFENSE

	By offense type				By age		By time period	
	Violent	Property	Narcotics	Traffic	22–29	30–40	'95-'03	'04-'10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
θ_{-4}	1.24	-0.70	3.86	-2.09	0.31	0.26	0.26	0.32
	(0.88)	(1.28)	(1.90)	(1.60)	(0.61)	(1.21)	(0.70)	(1.12)
θ_{-3}	1.14	0.98	3.45	-1.21	0.68	1.22	0.57	1.31
	(0.87)	(0.88)	(2.04)	(1.90)	(0.63)	(0.95)	(0.70)	(0.87)
θ_{-2}	1.03	1.65	3.08	0.77	0.96	1.62	1.61	0.95
	(0.70)	(0.70)	(1.73)	(1.33)	(0.54)	(0.70)	(0.59)	(0.64)
θ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
θ_0	-3.44	-4.52	-7.99	-5.44	-4.04	-4.34	-3.32	-5.05
	(0.74)	(0.85)	(1.92)	(1.38)	(0.59)	(0.80)	(0.62)	(0.77)
θ_1	-6.03	-6.85	-14.91	-11.22	-7.17	-7.95	-5.60	-9.50
	(0.98)	(1.15)	(2.52)	(1.88)	(0.78)	(1.08)	(0.88)	(0.99)
θ_2	-7.34	-6.65	-19.92	-6.80	-8.07	-7.12	-5.89	-9.34
	(1.12)	(1.19)	(2.90)	(2.09)	(0.89)	(1.14)	(0.92)	(1.11)
θ_3	-7.97	-7.36	-20.97	-5.94	-8.21	-7.80	-6.07	-9.97
	(1.23)	(1.28)	(3.11)	(2.30)	(0.97)	(1.25)	(1.02)	(1.19)
θ_4	-7.66	-8.83	-23.18	-8.58	-9.55	-8.43	-7.92	-10.10
	(1.32)	(1.34)	(3.38)	(2.51)	(1.04)	(1.33)	(1.12)	(1.25)
θ_5	-8.85	-7.64	-24.88	-9.23	-9.96	-9.06	-8.77	-10.29
	(1.40)	(1.44)	(3.62)	(2.67)	(1.11)	(1.42)	(1.20)	(1.32)
Non-charged outcome mean	...	...	...	...	...	...	...	...
Observations	597120	579170	71550	217680	801610	784090	773550	812150

This table shows event time coefficients (θ_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 includes multiple types of suspected offenses will be 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.

Figure 11 plots earnings effects separately by baseline employment status. ??% of charged suspects are working in the month of their suspected offense. As expected, those working at baseline have higher earnings at baseline. 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.

Table 5 provides event study estimates for earnings effects by various additional splits. Columns 1 through 4 split the sample by the type of offense. We limit to events that include at least one of the following types of offenses: violent, property, traffic, and narcotics. Note events that includes multiple types of suspected offenses will be included in multiple samples. Columns 5 and 6 split the sample in half by suspect age. Columns 7 and 8 split the sample period in half.

In general, coefficient estimates are similar across samples. 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.

5.5 Subsequent offenses

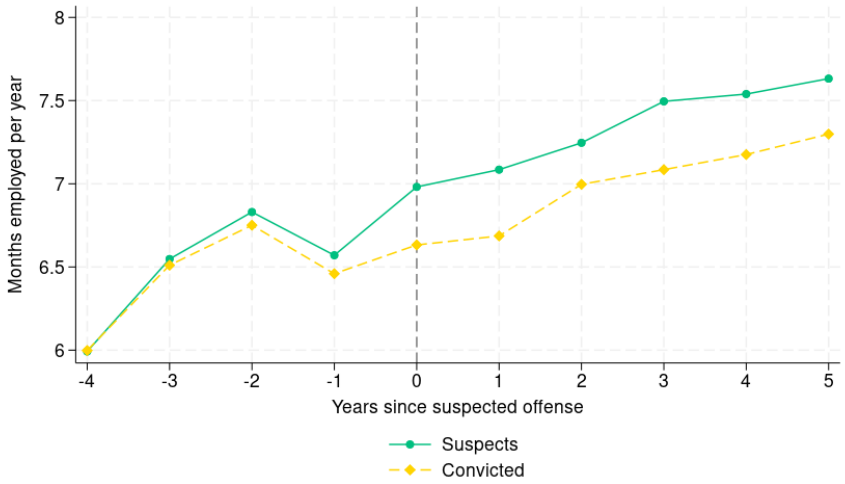
To this point, we have focused on the first time a person is charged with an offense, where that person is at the margin of having any criminal record. In this section, we apply the same research design to look at the effect of being charged with an offense for the *second time*.

We construct our estimation sample and weights in the same manner as above, 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 ??% of charged suspects. Appendix Table C.7 shows the characteristics of events. Our final example includes ?? events in the treatment group and ?? events in the comparison group.

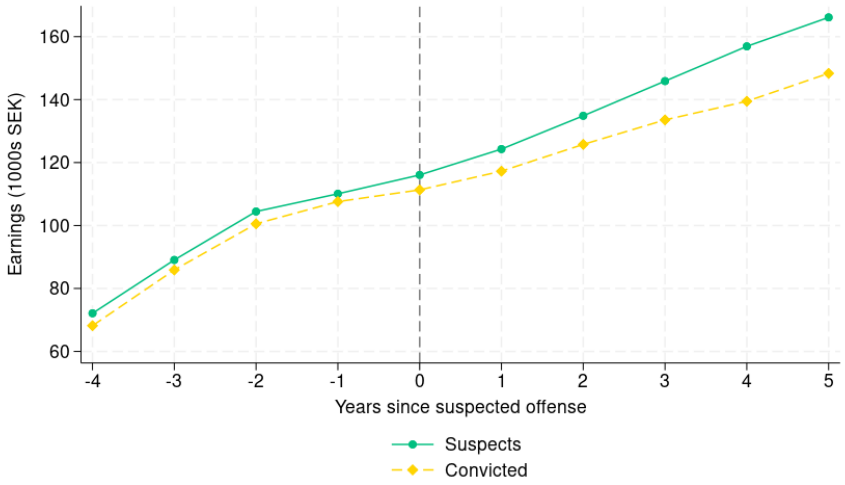
Figure 12 plots months employed in panel A and earnings in panel B for charged suspects and (weighted) non-charged suspects. Months employed are generally increasing for non-charged suspects, except for a dip in the year before the focal event. A gap of about 0.4 months between charged and non-charged 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.

Figure 12
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 5.5. 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.

5.6 Decomposition by Firm Type

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

We first characterize employers by their tendency to hire WCRs, conditional on other worker characteristics. We interpret this as a proxy for how discriminatory or "WCR-friendly" an employer is. We later refine this measure by also conditioning on a propensity score for whether a new hire has a record based on their suspected offense history. We then examine how the overall effects of acquiring a record on months employed, earnings, and wages are distributed across employer types.

To estimate firm tendencies to hire WCRs, we estimate the following specification:

$$\text{RECORD}_{it} = X_{it}\gamma + \psi_{J(i,t)} + \epsilon_{it}, \tag{4}$$

using a sample of hires from 1990 to 2015 for a randomly selected half of workers. $\psi_{J(i,t)}$ are firm fixed effects, which capture a firm's tendency to hire WCRs above and beyond the demographic characteristics of their hires. We interpret this as a proxy for how much a criminal record per se affects a firm's hiring decisions.

We then categorize firms as follows. We take the non-charged suspects in the final matched sample, limiting to the half of the sample *not* used to estimate equation (4), and look at the firms where they work during the event study window. We divide these firms into terciles based on their value of ψ_J, using the event-level weights described in section 5.2. We label these firm terciles as "Discriminatory", "Typical", and "WCR-Friendly". Table 6 describes the workplace characteristics of suspects at these three categories of firms.

The average values of firm fixed effects, ψ_J, are -3.149, -0.253, and 4.449 at Discriminatory, Typical, and WCR-friendly firms. Interestingly, the percentage of coworkers with criminal records is similar at Discriminatory and Typical firms at about 5 to 6%. At WCR-friendly firms, nearly 14% of coworkers are WCRs. Discriminatory firms are higher-paying than Typical firms, which are in turn higher-paying than WCR-Friendly firms, both as measured by average earnings and firm pay premiums.

Next, we estimate event study specifications where the outcome is MONTHS EMPLOYED$_{i\tau t}$ or

TABLE 6
FIRM DESCRIPTIVE STATISTICS BY TERCILE

	Discriminatory	Typical	WCR-Friendly
Workplace size	296.7	554.5	101.1
	(810.4)	(1468.4)	(247.2)
Firm size	3154.0	5120.7	5352.6
	(8545.7)	(9016.3)	(14599.7)
Average coworker earnings (1000s SEK)	261.4	224.6	185.7
	(134.9)	(92.92)	(90.08)
Average firm coworker earnings (1000s SEK)	262.5	221.6	186.0
	(128.9)	(84.05)	(84.59)
% of coworkers with active records	5.480	5.952	13.64
	(9.556)	(7.151)	(15.25)
% of firm coworkers with active records	5.373	5.564	13.03
	(9.178)	(5.894)	(14.45)
CR firm effect	-3.149	-0.253	4.449
	(3.381)	(1.457)	(5.935)
Firm pay premium	0.344	0.228	0.0482
	(0.837)	(0.764)	(0.751)
Agriculture and resources	5.868	2.056	3.171
Care and social services	0.692	16.96	11.21
Construction	9.383	4.032	9.723
Education	1.863	8.652	4.898
Health care	0.546	8.088	1.232
Hotels and restaurants	3.810	3.343	12.10
Manufacturing and industry	22.10	16.90	16.87
Public administration and defense	5.304	3.558	1.313
Retail and wholesale	15.58	13.76	11.61
Services	31.55	18.36	24.32
Unknown	1.078	1.854	1.647
Missing	2.231	2.437	1.909
Observations	175,088	196,790	188,530
N firms	27,505	11,140	26,845

This table reports descriptive statistics for three categories of firms employing non-charged suspects in our final matched event study sample. We take the non-charged suspects in the final matched sample, limiting to the half of the sample *not* used to estimate equation (4), and look at the firms where they work during the event study window. We divide these firms into terciles based on their value of ψ_J, using the event-level weights described in section 5.2. We label these firm terciles as "Discriminatory", "Typical", and "WCR-Friendly". 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.

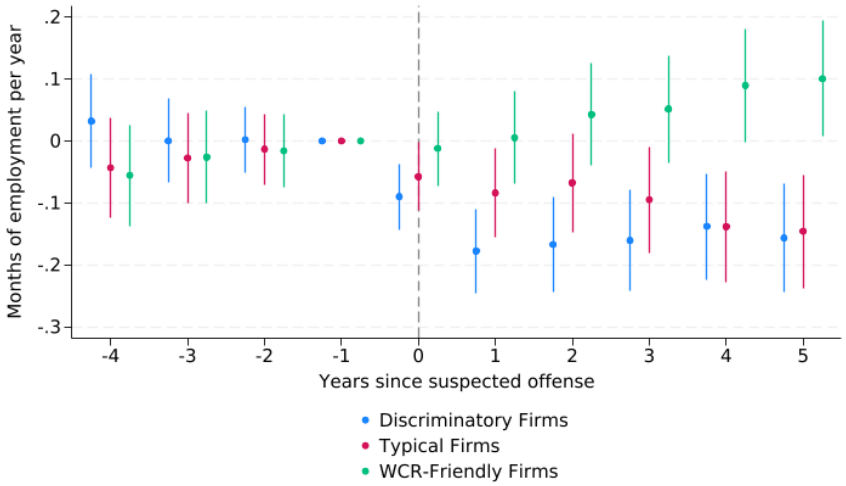
$\text{EARNINGS}_{i\tau t}$ multiplied by an indicator for whether the firm where person i at time t is working is in a particular tercile. The coefficients can be interpreted as the employment or earnings losses (or gains) at a specific set of firms, unconditional on where people are working. For example, a criminal record could reduce earnings at a set of firms either because it decreases the chances of working in that set, or it decreases earnings conditional on working in that set. We estimate this model using the randomly selected half of workers that were *not* used to estimate equation (4).

Event study estimates for each tercile are shown in Figure 13. Note that the coefficients are additive: the sum of coefficients across terciles will roughly equal the total earnings effect, except for the fact that some small firms are not classified.

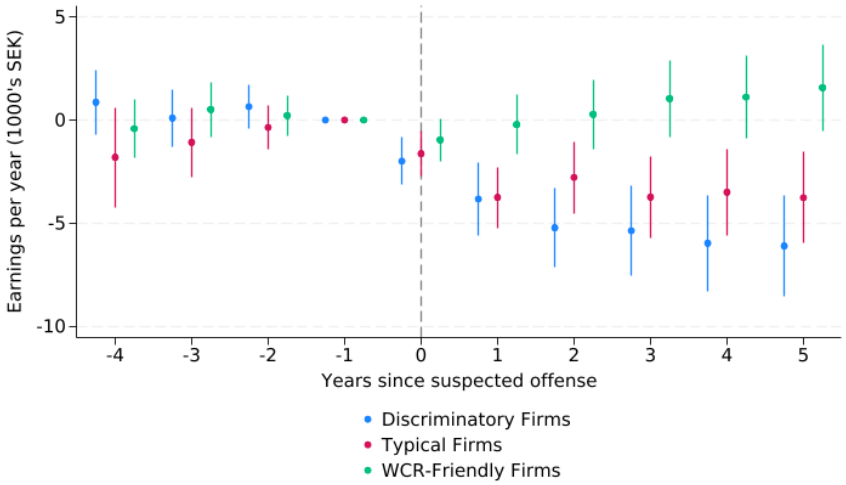
FIGURE 13

EMPLOYMENT AND EARNINGS LOSSES ARE CONCENTRATED AT MORE DISCRIMINATORY FIRMS

(a) Months employed



(b) Earnings



Note: This figure plots the event time coefficients (θ_j) from the event study specification (3) for months employed (panel A) and earnings (panel B) interacted with indicators for three firm types: Discriminatory, Typical, and WCR-Friendly. See section 5.6 for a description of how firm types are constructed.

In describing the coefficients, we focus on the effects five years post-event. Months employed decrease by about 0.2 at Discriminatory firms and 0.1 at Typical firms. These decreases are partially offset by a 0.1 month *increase* in employment at WCR-friendly firms. Earnings follow a similar pattern, decreasing most at Discriminatory firms (about 7,000 SEK), decreasing by less at Typical firms (about 3,000 SEK), and remaining constant or increasing slightly (by a statistically

insignificant 1,000 SEK) at WCR-friendly firms.

5.6.1 Wage Changes by Firm Type

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

Compared to months employed and earnings, studying wages is complicated by the fact that we can only measure them for people who are employed and we want to distinguish between overall and within-firm effects. Given these considerations, we simplify our research design and compare suspects before and after their suspected offense. We estimate regression specifications 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 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 firm fixed effects and with and without 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 equation (4).

The results are shown in Table 7. Column 1 excludes firm fixed effects. The coefficient on $\text{POST}_{i\tau t}$, -0.036, indicates that a criminal record reduces wages by 3.6 log points. Column 2 includes firm fixed effects. The coefficient is reduced to -0.026, indicating that 1 log point of the wage decrease reflects a reallocation to lower-paying firms. The remaining decrease in wages represents within-firm wage reductions. In column 3 we interact $\text{POST}_{i\tau t}$ with indicators for our three firm categories: discriminatory, typical, and WCR-friendly firms. Interestingly, the within-firm wage decrease is similar across all firm types.

6 Mechanisms and Sources of Firm Heterogeneity

The findings thus far suggest that firm heterogeneity plays a significant role in explaining the labor market outcomes of WCR. Even conditioning on observable characteristics, WCR are more likely

Table 7
Wage effects and firm heterogeneity

	(1)	(2)	(3)
POST	-0.036	-0.026	
	(0.007)	(0.006)	
Discriminatory Firm × POST			-0.020
			(0.008)
Typical Firm × POST			-0.031
			(0.009)
WCR-Friendly Firm × POST			-0.025
			(0.009)
Firm FEs		✓	✓
Observations	570286	570286	570286

This table shows estimates of the specification (5), where the outcome is log monthly wages. All columns included 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. Column 2 includes firm fixed effects. Column 3 interacts $\text{POST}_{i\tau t}$ with fixed effects for the three firm categories defined in section 5.6: discriminatory, typical, and WCR-friendly firms.

to be employed in some firms than others and the acquisition of a record shifts workers away from what we label the 'Discriminatory' and 'Typical' firms. This section aims to provide insights into the (not necessarily mutually exclusive) mechanisms underlying this redistribution of WCR.

One channel is differential preferences towards and willingness to hire WCR across firms. Such hetereogeneity can be reflected in a wide range of firm policies that actually impact hiring outcomes, including whether to conduct background checks for any or all potential employees, whether to signal such background checks in job ads, when in the hiring process to conduct checks, and whether and when to hire a candidate whose background check is not completely clean. Measuring such firm policies and linking them to real-world hiring outcomes (at a national level as opposed to a small survey or single firm RCT) is rare in the personnel economics literature in general, and with regards to WCR especially (Hoffman and Stanton, 2024). Section 6.1 does exactly this for one particular policy: signaling background checks in job ads.[23]

But *why* is there heterogeneity in firm preferences and policies? Why are some firms friendly

[23]Though job ads have been increasingly used by researchers to study questions related, for instance, to gender discrimination (e.g., Kuhn and Shen (2012)) or how firm demand for occupations and tasks has evolved over time (e.g., Atalay et al. (2020)), these data sets have rarely been linked to register data on actual firm outcomes. The one exception of which we are aware is Card et al. (2024), who study the effect of stated gender preferences in ads.

and others discriminatory towards WCR? Section 6.2.1 considers whether firm managers drive differential hiring practices and the extent to which the manager's preferences are informed by their exposure to individuals with records in the past (including their own records as well as those of family members and former co-workers).

Finally, the sorting of WCR across different types of firms may not only arise through the policies and decisions of the firm, but can also be driven (or at least amplified) by criminal records leading to workers searching for and finding jobs through different channels, including more informal channels like referrals.[24]

In this section, we focus our analyses on new hires from 2005 to 2015 and whether they have a criminal record in the year in which they are hired. 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. To differentiate these new hires from the founders of firms, we also exclude workers present in the first year of new firms and establishments that appear in our data for the first time during our sample period. Similarly, we explicitly exclude managers, owners, and firm operators from our definition of new hires to focus on employees unlikely to have influence over the hiring process.

We consider the general specification

$$\text{RECORD}_{iet} = X_{it}\gamma + f(P(\text{RECORD})_{it}) + Z_{iet} + \epsilon_{it}, \tag{6}$$

where $RECORD_{iet}$ denotes whether hire i by firm or establishment e in year t has a criminal record, $f(P(\text{RECORD})_{it})$ denotes a new hire's propensity score for having a criminal record in a given year based on their suspicion history, and Z_{iet} is a firm or establishment-level characteristic of interest, including namely whether the firm announced conducting background checks in their job ads or characteristics of the establishment's manager.[25]

6.1 Job advertisements

The analysis in this section is based on our data set of job ads with known firm ids. Appendix Table C.8 provides firm level (n=54370) summary statistics characterizing job ad signaling behavior for

[24] There is little evidence on how having a record affects job search (Hjalmarsson et al., 2024). One exception is Smith and Broege (2019), who finds in the NLSY97 that interacting with the justice system decreases both overall job search and shifts search from more active to pass methods.

[25] We estimate a probit model for the probability of 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, nationality, suspected offense history, and suspected crime type. We then carry this probability of conviction forward across a certain number of years based on how many years the suspect would have had a criminal record for, had they in fact been convicted of this given offense.

Table 8
Firms that signal background checks are less likely to hire WCRs

	All firms			Non-Mandatory Jobs	Private Sector
	Raw	+Demo.	+ Industry and Municipality		
	(1)	(2)	(3)	(4)	(5)
Any ads mentioning check	-0.021	-0.003	-0.007	-0.006	-0.008
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	6047725	5963957	5778205	3722907	3033163
Mean	0.064	0.065	0.065	0.060	0.079

This table presents the results of regressing whether a new hire has a criminal record in the year of hire on whether the firm ever signals in the job ads that they will do a criminal background check. Columns 1 through 3 are for all new hires from 2005 to 2015, while column 4 restricts to new hires in jobs that do not mandate criminal background checks and column 5 restricts to jobs in non-public (e.g., municipal, county) firms. Column 1 includes no controls and column 2 controls for year fully interacted with demographic variables of gender, age, education level, and Sweden born. Columns 3 through 5 further control for interactions of year with municipality and five digit workplace industry codes. Standard errors clustered on firm are in parentheses.

both criminal background checks and drug and alcohol testing, the types of occupations advertised, and other firm or job characteristics that can be measured in the ads. Job ad characteristics substantially differ between firms that ever signal a criminal background check and those that do not. More than 20% of firms that signal checks (versus just 1% of firms that do not) also signal drug and alcohol testing. In other words, firms that signal checks have other observed (and likely unobserved) policies related to concerns about a safe work environment. Ads at firms that signal checks are for jobs that on average require more education and job skills and have five times more exposure to vulnerable populations (O*NET measures) and relatively more manager, professional and technician positions and fewer unskilled labor, plant and machine, and crafts and trades positions.

We then turn to the relationship between a firm's job ad signaling and its propensity to hire WCRs by matching the job ad data to the more than six million new hires at these firms from 2005 to 2015 meeting our original sample criteria. Binned scatter plots in Appendix Figure C.11 demonstrate that firms that signal checks are less likely to hire WCRs. Of course, this raw negative relationship does not adjust for other individual characteristics that may be correlated with record status and the types of jobs advertised. Table 8 thus presents the results of regressing whether each new hire has a record on a dummy for whether the firm ever signals background checks. The unadjusted estimate is presented in column 1 while columns 2 and 3 add controls for demographics and industry by municipality, respectively. Columns 4 and 5 estimate this full specification for two restricted samples: occupations without mandatory background checks and non-public firms. Even with the full set of controls, WCRs are 10% less likely to be hired at firms that signal checks. This is true for jobs at all firms as well as in non-mandatory check jobs and non-public firms.

6.2 Managers

6.2.1 Identifying managers

For each establishment, we identify the worker likely to be in the top managerial role based on a classification system proposed in Åslund et al. (2014), who study the role of a manager's nationality on the hiring of immigrant workers. First, when occupation codes are available, we use the first digit to identify managers and, when there is more than one, rank them using subsequent digits to distinguish between top versus middle management. Second, when an establishment has no identifiable manager from the occupation codes or has more than one manager in the same managerial level, we use the worker with the highest yearly earnings at the firm to identify the "top manager".[26] To focus on hiring decisions in which the identified managers may have played an influential role, we exclude managers in their first year at an establishment (and therefore avoiding studying workers potentially hired at the same time as the manager). For the same reason, we restrict the sample to establishments with between 2 and 50 workers since managers may be more involved in hiring any given worker in smaller establishments. Table 9 presents summary statistics for the resulting sample of managers and establishments.[27]

6.2.2 The role of manager exposure to criminal records

We test whether a manager's exposure to criminal records in the past shapes the hiring of WCRs at the establishments they manage. Previous exposure could affect a manager's willingness to hire WCRs for several reasons, including having better information about their performance for certain jobs or decreasing prejudice towards these workers (Paluck et al., 2019; **?**).

We study three sources of exposure. First, we consider whether managers themselves have a criminal record. Second, we consider whether a manager was exposed to more or less coworkers with criminal records at previous establishments they worked at (whether they managed those establishments or not).[28] Third, we consider whether a manager has a family member (parent, child, sibling) with a criminal record.

[26] If a worker is identified as the manager in two non-consecutive years, we also assign them to be the manager for the years in between if they are also present at the establishment in those years. Once a worker is identified as the manager, we assign them to be the manager in the years before and after as long as they are the highest-earning worker each year and are working in the establishment for those years too.

[27] Two statistics support the quality of our 'manager' classification: 79% of managers identified using the occupation code also have the top earnings in an establishment and 67% of managers in single establishment firms are also owners or primary operators. We are identifying influential individuals in the firms.

[28] We restrict these previous establishments to have had less than 100 workers to increase the likelihood that the two coworkers interacted in the workplace.

TABLE 9
SUMMARY STATISTICS FOR MANAGER EXPOSURE ANALYSIS

	All establishments	Establishments with 2-50 workers			
		Manager exposure type			
		No exposure	Record	Coworker records	Family record
New hire					
Male	0.48	0.5	0.65	0.57	0.55
Years of education	12.6	12.5	11.6	11.9	12
Native born	0.83	0.86	0.76	0.82	0.83
Age	31.4	30.8	30.3	29.8	30.5
Earnings	211082	205177	169378	172663	185477
Criminal record	0.07	0.07	0.15	0.12	0.11
Firm/Establishment					
Hires per year	6	2.7	2.8	3	2.8
Establishment size	32	15	13	14	14
Firm size	2506	1638	665	1512	1382
Firm pay premium					
Manager has record		0	1	0.07	0.08
Manager share past co-WCRs		0.01	0.09	0.08	0.06
Manager family has record		0	0.2	0.13	1
Industry					
Services	16	20	15	17	17
Retail	15	26	21	26	23
Care and social services	15	6	5	7	8
Education	13	11	2	5	9
Manufacturing and industry	12	10	15	12	11
Hotel and restaurant	7	8	16	15	11
Construction	7	11	20	12	15
Number of observations	1025091	252080	83709	432240	165202
Number of establishments	323696	41217	17287	71,753	30116

This table reports descriptive statistics for different subsets of establishments. The first column describes all establishments included in our main analysis sample. Columns 2-5 restrict to establishments between 2 and 50 workers, for which we identify managers. Column 2 restricts to establishments with a manager that has no record, has below-median exposure to coworkers with records at their past establishments, and has no family member. Column 3 restricts to establishments with managers who have a record, column 4 to establishments with managers who have above-median exposure to coworkers with records at their past establishments, an column 5 to managers with a family member (parent, child, or sibling) who has a record.

The results of regressing whether the new hire has a criminal record on each of these exposure variables are shown in each panel of Table 10, respectively. The unadjusted relationships are presented in column 1, while columns 2 and 3 respectively adding controls for worker demographics and $Industry$ X $Year$ X $Municipality$ fixed effects. Column 4 also conditions on a propensity score for whether a new hire has a record based on their suspected offense history. Intuitively, we ask whether manager exposure influences WCR hiring comparing individuals with the same probability of currently having a criminal record based on their suspected offense history, but only some of whom have actually been convicted and have a criminal record. This accounts for the likely possibility that managers with more exposure to previous records are sorted into firms that are more likely to hire WCRs due to reasons unrelated to the manager, as suggested by substantial differences in establishment characteristics between those with managers who have more or less exposure to records. Across columns, estimates indicate positive, statistically-significant impacts of manager exposure to criminal records on their establishment's hiring of WCRs. These estimates range from 26-43% in column 3 and 9-12% in column 4.

7 Conclusion

This paper examines the labor market outcomes for workers with criminal records (WCRs). WCRs earn significantly less than demographically similar workers without records, are employed in lower-paying firms with a higher concentration of coworkers who also have criminal records, and tend to be concentrated in specific occupations. We find that a criminal record has a negative causal effect on months employed and earnings; this effect is larger for more serious or repeated offenses and persists over many years. A criminal record also leads workers to sort away from more discriminatory employers and toward less discriminatory ones, and broadly lowers wages across employers. Finally, we document substantial variation across firms in their likelihood of hiring WCRs, even after accounting for worker demographics and offense history. We find that managers play a key role in explaining this variation: managers with a criminal record themselves, or with more exposure to individuals with records (through former coworkers or family members), are more likely to hire such workers.

Table 10
Regressions of New Hire Criminal Record Status on Manager Exposure

	(1)	(2)	(3)	(4)
	Manager has a record			
Criminal record	0.076	0.046	0.035	0.009
	(0.002)	(0.002)	(0.002)	(0.001)
Outcome mean	0.080	0.081	0.081	0.081
Observations	1,826,023	1,796,254	1,617,452	1,617,452
	Manager has above-median exposure to past WCRs			
Criminal record	0.047	0.027	0.021	0.009
	(0.001)	(0.001)	(0.001)	(0.001)
Outcome mean	0.080	0.081	0.081	0.081
Observations	1,826,023	1,796,254	1,617,452	1,617,452
	Manager has family member with a record			
Criminal record	0.036	0.026	0.021	0.007
	(0.001)	(0.001)	(0.001)	(0.001)
Outcome mean	0.081	0.082	0.082	0.082
Observations	1,526,295	1,501,203	1,334,442	1,334,442
Demographics		✓	✓	✓
Industry X Year X Municipality FE			✓	✓
P(criminal record)				✓

This table presents the results of regressing whether a new hire has a criminal record in the year of hire on measures of an establishment's manager's exposure to criminal records. The measure of exposure in the first panel is whether the manager has a criminal record or not, in the second panel whether the manager has above-median exposure to coworkers who had a record when they worked together at the manager's previous establishments, and in the third column whether the manager has a family member (parent, child, or sibling) with a record. For the second measure, we consider a manager's previous establishments going back to 1990 and restrict previous establishments to those with less than 100 workers. Column 1 includes no controls and column 2 controls for year fully interacted with demographic variables of gender, age, education level, and Sweden born. Column 3 controls for interactions of year with municipality and five digit workplace industry codes. Column 4 further controls for a hire's probability of having a criminal record in a given year based on their history of suspected offenses. Standard errors clustered on firm are in parentheses.

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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 that occurred 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 criminal record.

All offenses, regardless of severity, are automatically expunged from one's criminal record. Once expunged, an offense disappears from a criminal record permanently. The precise timing of expungement is a function of four factors: (i) age at the time of the offense (under 18 versus 18 or older), (ii) the sentence received, (iii) whether or not one recidivates, and (iv) the regulations in place in a given year (new rules supersede old rules).

Between 1990 and 1999, the expungement rules were the same for all persons age 15 or older. Crimes that received no 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 an in-court or out-of-court conviction generated a criminal record that was expunged after 10 years. For those receiving a prison sentence or institutional placement, their offense was automatically expunged 10 years after their 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.

Between 2000 and 2007, the expungement rules were still the same for all persons age 15 or older. If the main punishment was a fixed fee fine (*penningbot*) or an income related fine (*dagsböter*), then this offense now showed up on a criminal record and was expunged after 5 years. This also meant that these crimes could suddenly appeared 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 1996 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 offenses 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 the year 2000, no offense was allowed to stay on a criminal record for more than 20 years (or 20 years after release from a prison or institution).

Starting in 2008, the expungement rules began to discriminate by age. Offenses that were

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 once again 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 crime were still expunged 10 years after release. Furthermore, recidivism continued to extend expungement times of all previous offenses, even those crimes committed before age 18. These rules stayed 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 the time (in years) until expungement for individuals over and under 18 (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 judgement, 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 for individuals over and under 18 (under 18 in parenthesis). For 16 §, expungement is always *without delay* (w.d). Otherwise expungement rules are years after the prosecutor decision or court judgement, 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 below as SOC2000) while the Swedish register data uses the Standard for Svensk Yrkesklassificering (the 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. Both crosswalks contain the ISCO-88 code and occupation name, as well as the corresponding SSYK or SOC codes and names. These files were merged using the ISCO-88 code, yielding a SSYK96 to SOC2000 crosswalk. The crosswalk from Statistics Sweden is, however, only semi-official and incomplete; thus, a research assistant manually completed the SSYK to ISCO crosswalk by using the SSYK and ISCO-88 look up functions.[29] 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 can also merge SOC2000 data onto the SSYK2012 codes, given the existence of an official SSYK96 and SSYK2012 crosswalk from Statistics Sweden.

We highlight here that there are more than twice as many 6-digit US occupation codes as 4-digit Swedish codes. It is not surprising that there is not always a one-to-one mapping of US and Swedish codes in the crosswalk, but rather also many-to-one mappings of SSYK to SOC codes and one-to-many mapping of SSYK to SOC codes.

[29] In some cases, the link between occupation codes may not be obviously correct since there are multiple occupations included in the Swedish occupation categories. Using the look-up function, however, provides a list of all occupations included, and can clarify why the link made. https://www.h5.scb.se/yreg/ssyk.asp; https://ilostat.ilo.org/methods/concepts-and-definitions/classification-occupation/

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**.[30] For the purposes of our analyses, 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 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 (one 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 any 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).

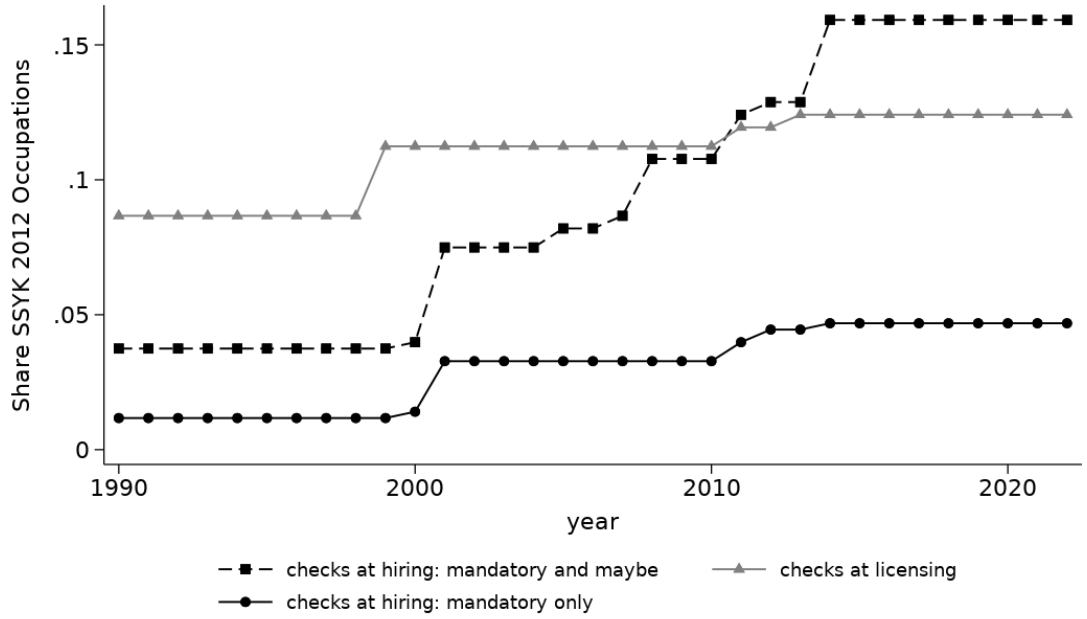
We restrict our data set to occupations with SOC codes of the form YY-ZZZZ.00, which allows us to use a 6-digit SOC code to use a many to one merge match to the Swedish SSYK 4-digit codes in the crosswalk. Since some SSYK codes correspond to more than one SOC code, we collapse the data to the SSYK96 (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.

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

A.3 Mandatory Background Check Laws and Reforms

As described in section 3.3, we characterize the types of jobs in which individuals work 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 main 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 has 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 affected each year. As each SSYK code includes multiple jobs, which are sometimes even in different industries (e.g., a cook in a school versus a restaurant), some occupation codes are only partially affected. These are captured in the maybe category, as is laws that say a background check is optional. An occupation falls into the mandatory only category if a background check is mandatory in year t for all jobs under that occupation.

Table A.4
List of the Main Background Check Laws for Hiring and Occupational Licensing

Year (Month)	Law and Description
Main 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
Main 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

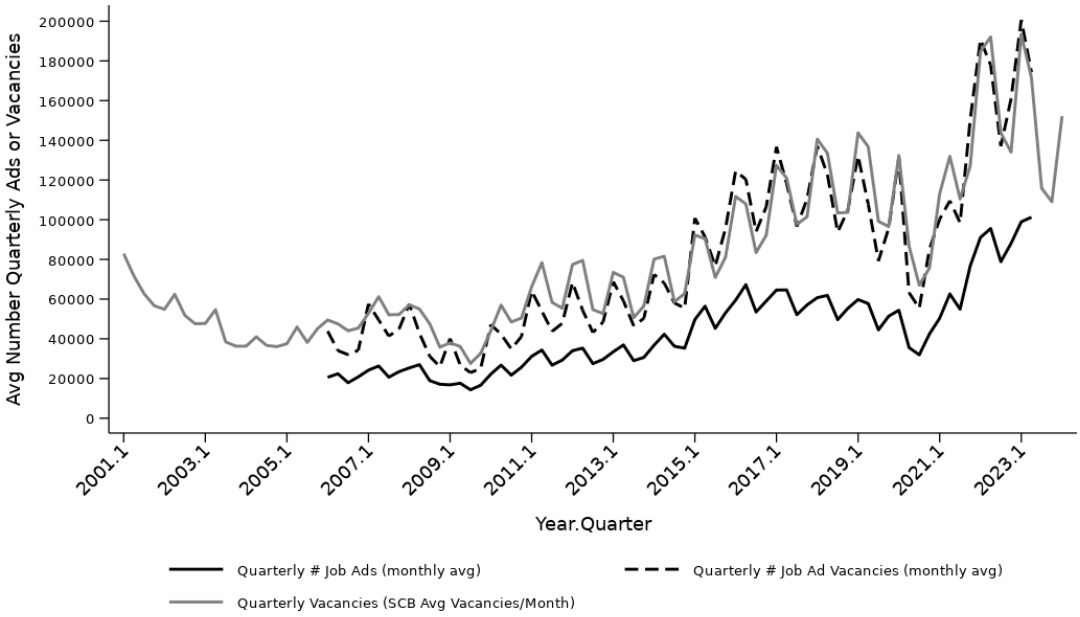
We measure the extent to which Swedish firms signal that they care about the criminal records of workers using a data set of Swedish job advertisements from Platsbanken, the largest job board in Sweden. A team at Platsbanken maintains a historical database and has made it available online.[31] The dataset spans the 2006-2023 period and includes over 9 million job ads; a total of close to 17 million vacancies (due to multiple jobs per ad). Ads are grouped by the month and year they are first published and is stored in the jsonl format. After downloading the files and converting the

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

files to the csv-format, we create a unique ID variable for each ad. The data is then separated into a two parts – one file for the main ad text and one file for the variables – to speed up further computations. Specifically, we analyze the text to capture whether the firm signals in the ad that a background check will be conducted during the hiring process. This section provides some key details about the data source, the cleaning and creation of the main variables, and the procedure via which we matched on the job ads to our employer-employee registers.

Platsbanken and the Job Ad Dataset: Platsbanken is maintained by the Swedish Public Employment Service (PES) and had an average of about 7.7 million visits in 2022-2023. Employers can post directly on the job board after registering an account or by special recruitment software using the job boards 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 both 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 in 2021.

Figure A.2
Number of Platsbanken Job Ads/Vacancies Versus 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 average (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/

These background features strongly suggest that Platsbanken is a good measure of the Swedish labor market. To get a better sense of how representative Platsbanken is of the 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 as well 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 signalling a background check if any one of these variables is equal to one. We have created a number of other naive dummy variables, i.e., indicators if the ad includes other keywords, related to drug and alcohol free work environments, required skills (svenska or swedish, körkort or driving license), personal character traits, personal cognitive traits, personal traits related to customer service, personal traits related to management ability, and personal social traits. The data set includes a large number of other variables, many of which were tagged (though the coverage of these variables is far from complete in the earlier sample years). These include 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. This variable was once connected to an official ssyk96 or ssyk12 codes; but as that connection was lost at a prior date, the team at Arbetsförmedlingen was able to recreate these mappings by combining different data sources at their disposal and by letting experts at Arbetsförmedlingen map the codes manually.[32] This translation key was then joined to the ads database. Finally, all ads include a firm name (of various cleanness), 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: We thus have a large data set of job ads and variables capturing job characteristics, including whether a background check will be conducted. The final step is to match these data to the firm's in the employer-employee register, for whom we have a pseudonymized firm id. In order to do this match, we needed to provide our job ad data set to Statistics Sweden, so that they could replace the firm organization number in the job ad data set with the corresponding pseudonymized id. This is straight forward for the 2021-2023 job ads since almost all of these ads included in the firm organization number as a variable. But firm names were only included in earlier sample periods. We therefore took a number of steps to assign organization numbers to the ads for which this was missing in earlier periods. First, a *within* crosswalk is created from the existing ids and mapped onto the firm names in the earlier years. The next step was to attempt to scrape the firm ids for the remaining firms. We chose to only use firm names with 10 or more ads in the 2006-2023 period to make this manageable. The first round of scraping is done on proff.se, a website that lists firm information such as revenue, phone

[32] https://gitlab.com/davidnoirman/legacy-id-to-ssyk

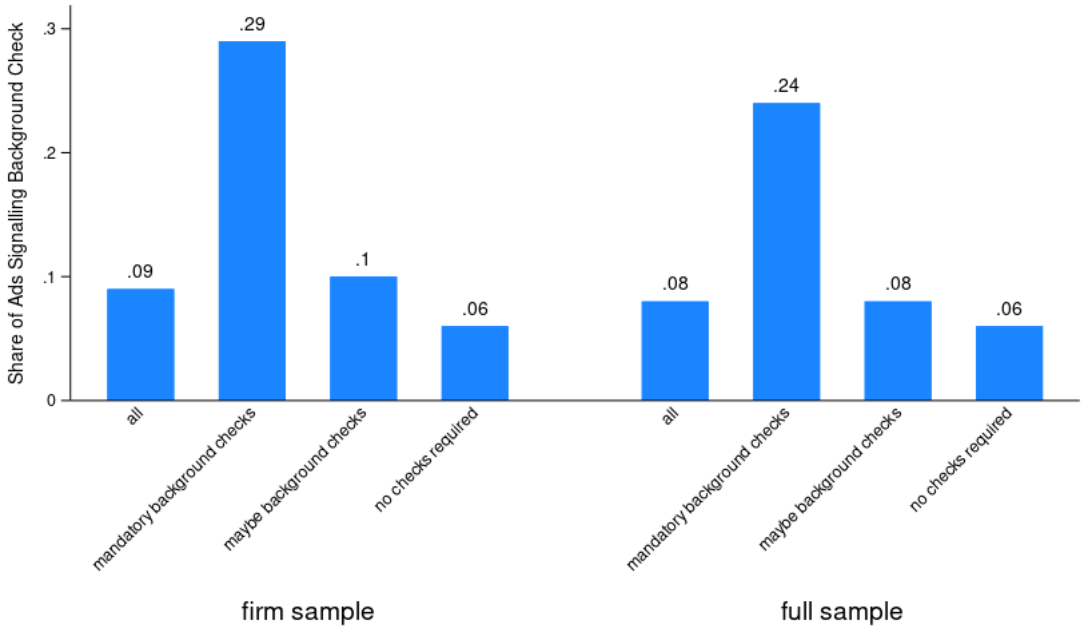
number address and, of course, firm ids. We scrape both the firm id and the firm name so that we can compare the scraped name to the name in the data frame. The second and third round use the search engine duckduckgo.com to retrieve even more firm ids. The result is a data frame that contain the first two search hits[33]. Because a search engine was used, these results are not as clean as round 1. The firm id needs to be extracted (10 digits) and the firm name will contain more information than we want (e.g. the name of the website). For each firm id, we thus create an indicator of how confident we are that it corresponds to the correct firm; we label the id 'robust' 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 three or two words in the firm name are also in the scraped firm 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 robust 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 signalled a background check.

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. Given that the firm sample is conditioned firms that post more ads, i.e., firm size, and survey evidence that larger firms conduct more background checks, these statistics make sense. Moreover, as one would expect, we see that 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%).

[33]Duckduckgo has an option to restrict results to a named list of urls. We used duckduckgo twice; The first round use the search formula "[firm-name] organisationsnummer inurl:bolag inurl:hitta inurl:118100 inurl:ratsit inurl:merinfo"; The second round only used "[firm-name] organisationsnummer".

FIGURE A.3
SHARE OF JOB ADS SIGNALLING BACKGROUND CHECKS BY TYPE OF OCCUPATION



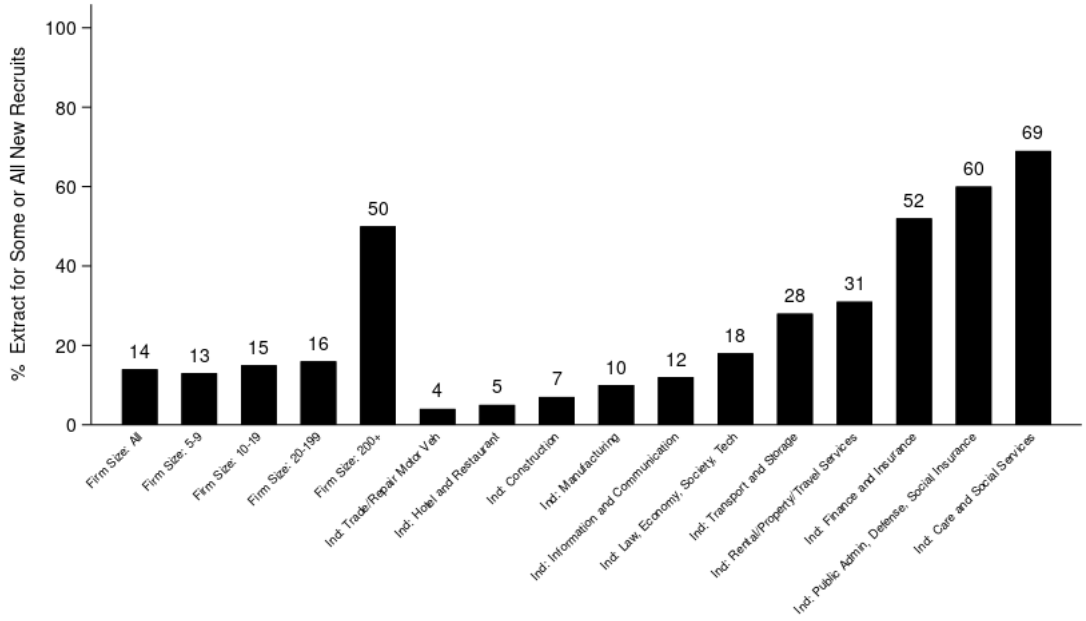
Note: This figure presents the share of job ads signalling background checks in the full sample as well as in the sample of ads matched to firm ids in the employer-employee register. It also reports the share of ads signalling checks for three categories of occupations: those that require mandatory background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

B Estimating Firm Pay Premiums

(To be added.)

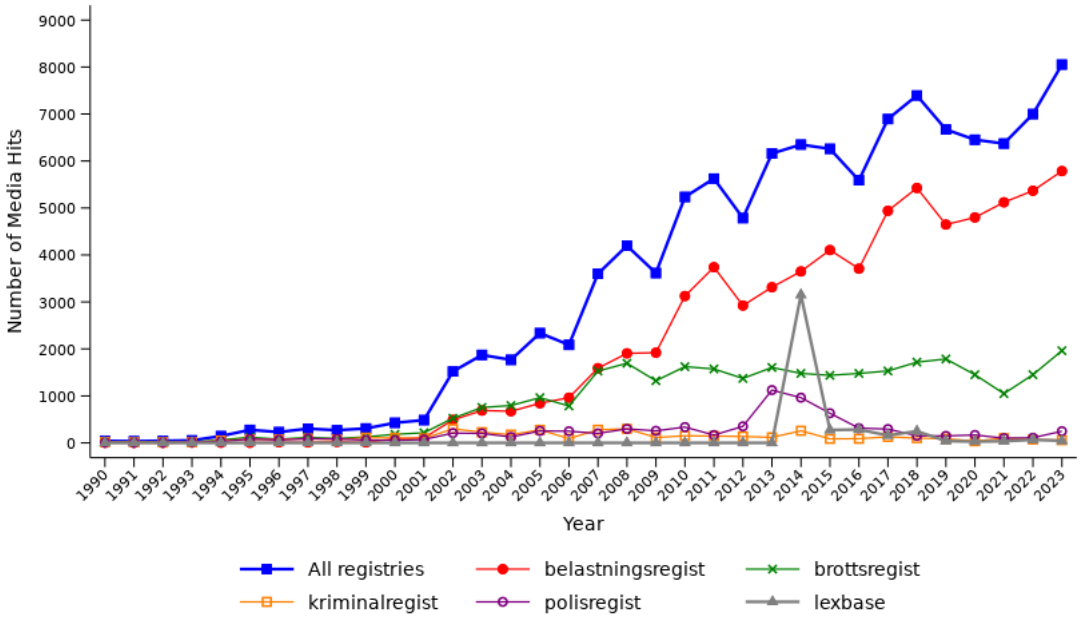
C Additional Exhibits

FIGURE C.4
GOVERNMENT SURVEY OF FIRMS ABOUT BACKGROUND CHECKS



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

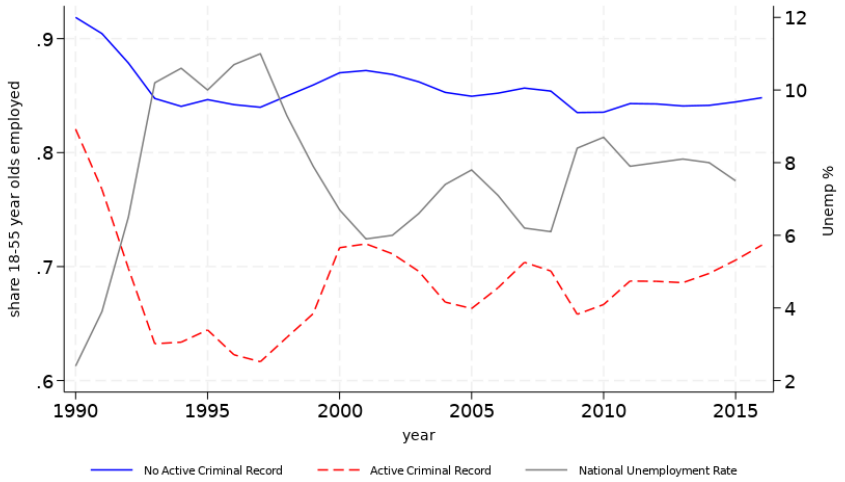
Figure C.5
Media Coverage of Crime Registers and Lexbase



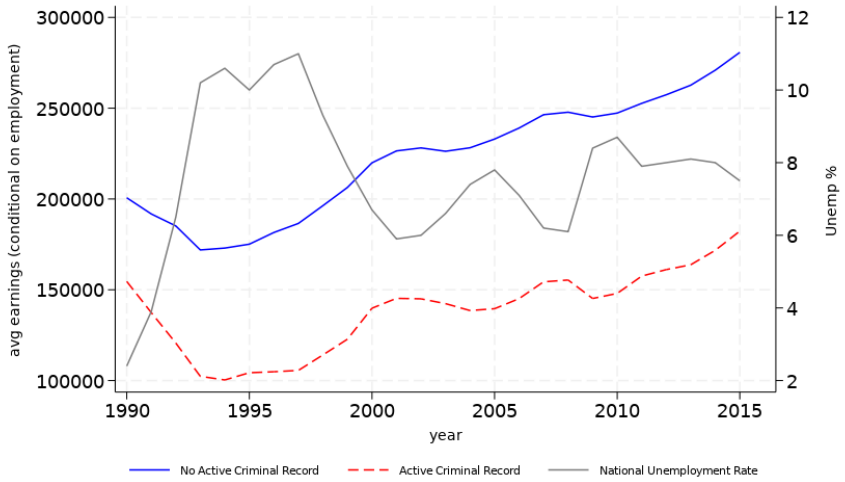
Note: This figures presents the number of hits per year in a Swedish media archive (newspapers, TV, and radio) for the following keyword searches in *Retriever*: belastningsregist, brottsregist, kriminalregist, polisregist. The latter two capture the two registries that existed prior to 2000: Kriminalregistret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

Figure C.6
Raw employment and earnings gaps over time

(a) Employment



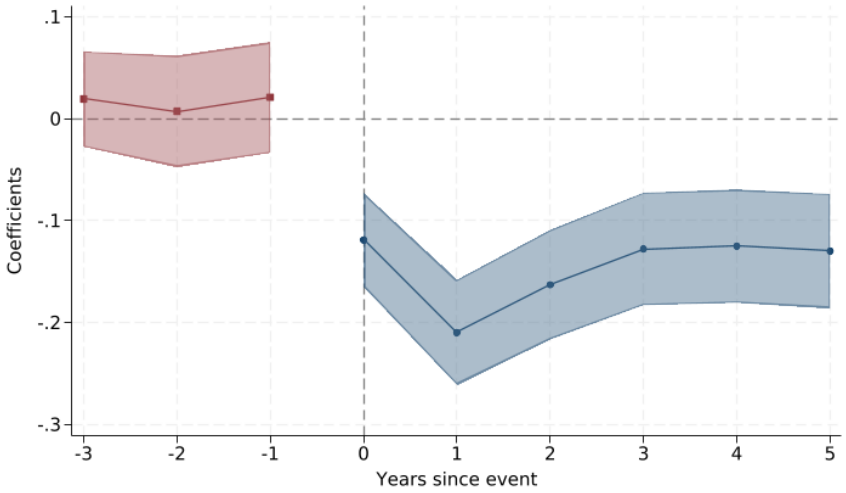
(b) Earnings



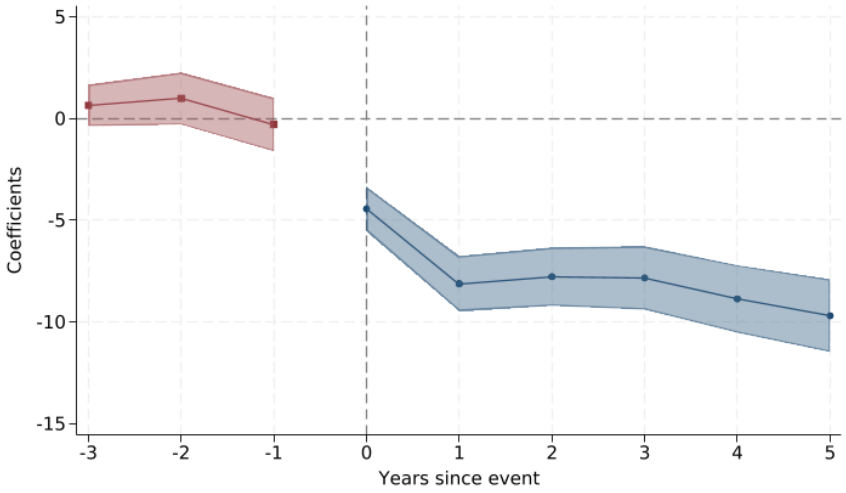
Note: This figure demonstrates how the employment and earnings gaps beteen workers with and without active records vary over time and with the national unemployment rate, which is plotted on the right axis.

Figure C.7
Event study estimates for employment and earnings, imputation-based method

(a) Months employed



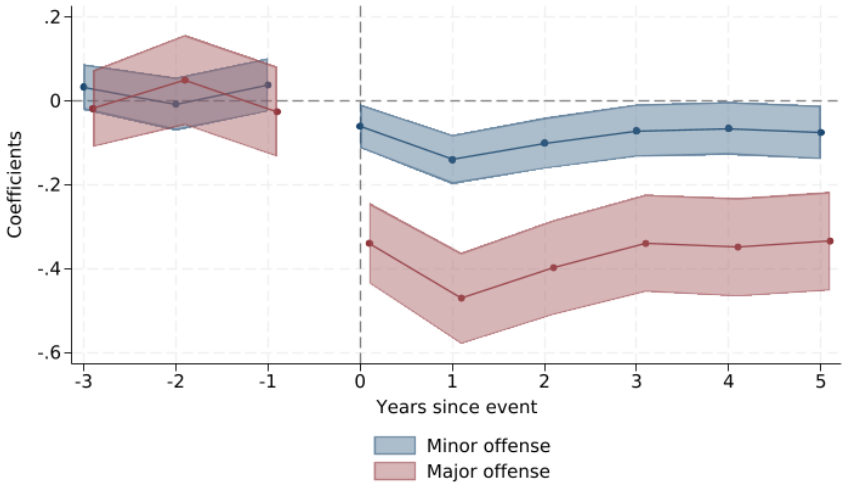
(b) Earnings



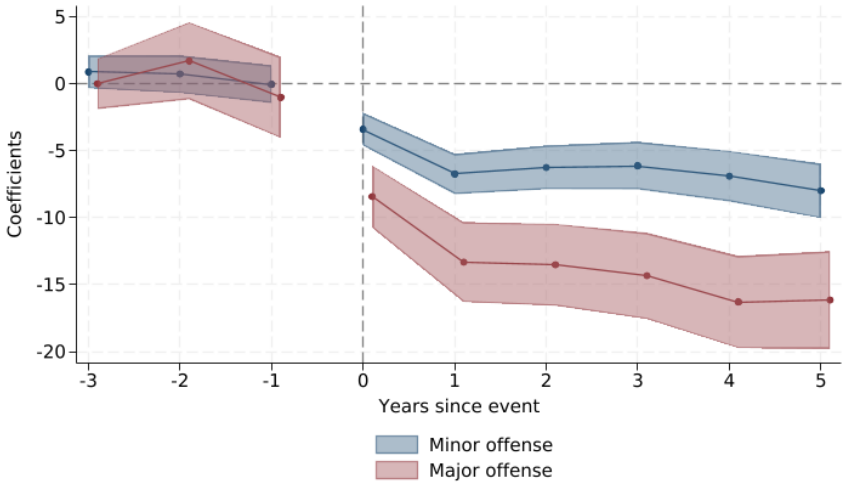
Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024). In panel A the outcome is months employed. In panel B the outcome is earnings.

Figure C.8
Employment and earnings effects by offense severity, imputation-based method

(a) Months employed



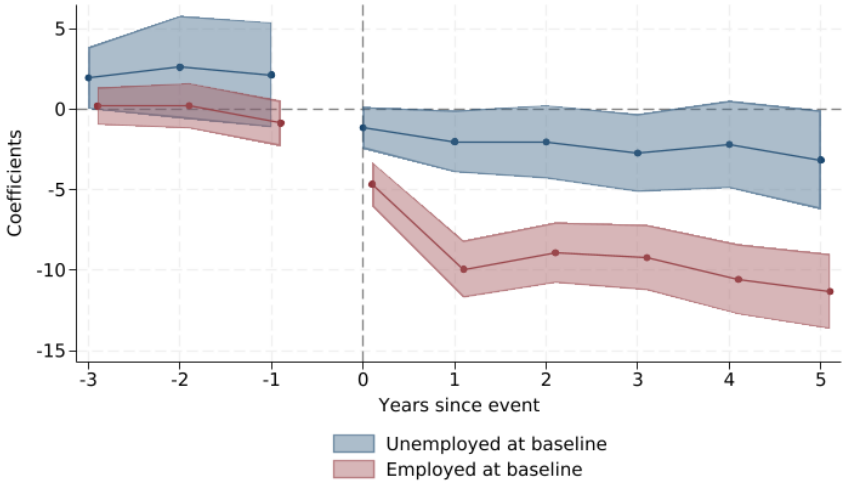
(b) Earnings



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. A event includes a major offense if any specific 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 C.9

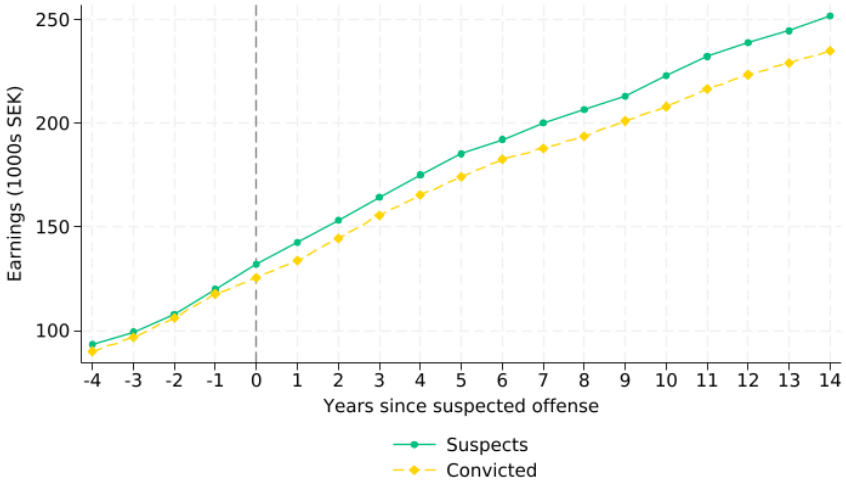
Earnings effects by baseline employment status, imputation-based method



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 C.10

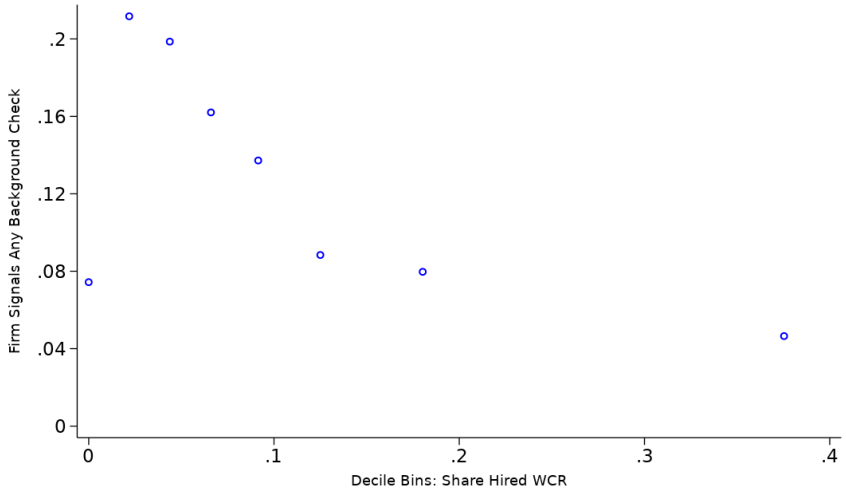
Acquiring a criminal record reduces earnings for over an decade



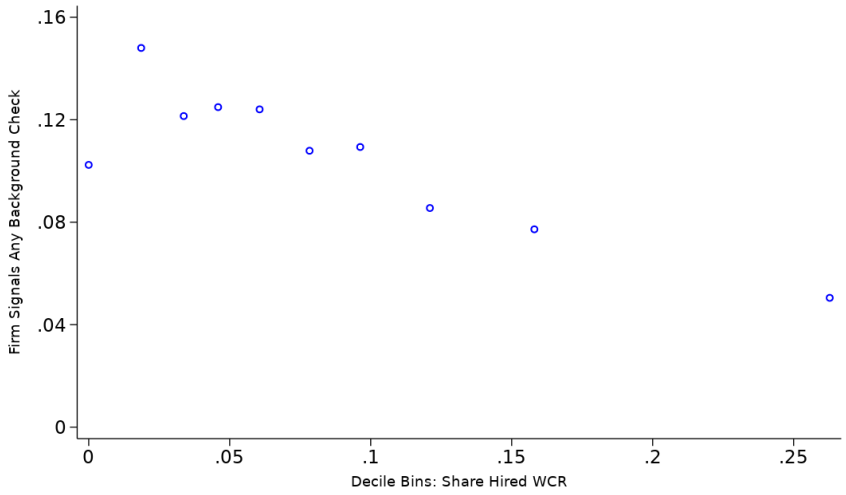
Note: This figure plots earnings before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2, extending the time window to 14 years post-event. We limit events to person-years with at least one associated suspected offense where the person (1) 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.

Figure C.11
Bin Scatter Plots: Background Check Job Ad Signals versus Share of Hires with Criminal Record

(a) All Firms



(b) Trimmed Sample of Firms



Note: This figure demonstrates that there is a negative relationship between the share of workers with a criminal record hired at a firm and whether firms signal any criminal background checks in the job ad data. Panel A includes all firms, while Panel B trims the sample of firms to exclude those with less than 20 hires or more than 346 job ads (which corresponds to the top 5% of firms, and disproportionately includes recruiting firms and public employers).

TABLE C.5
EARNINGS BY WCR STATUS WITH EXAM SCORES, 2015

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

...

Table C.6
Top/Bottom 20 Occupations: Prevalence of WCR 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 the sample 18 to 55 year old employees with 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 if 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 employees.

TABLE C.7
CHARACTERISTICS FOR SUSPECTS AND SUSPECTED OFFENSES, SECOND OFFENSE

	All		Matched and Weighted	
	Suspects	Convicted	Suspects	Convicted
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 (%)	42.9	31.3	19.8	19.8
Fine or less (%)	0	59.95	0	57.97
Probation and other (%)	0	27.25	0	31.45
Prison (%)	0	12.80	0	10.58
Male (%)	76.13	74.97	79.39	79.39
Swedish born (%)	62.86	66.23	68.53	68.73
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)	133.1	115.1	123.3	118.2
	(151.3)	(121.8)	(128.7)	(121.8)
Months employed year	6.924	6.544	6.570	6.460
	(5.457)	(5.467)	(5.528)	(5.528)
Workplace size	263.8	269.8	240.5	272.6
	(869.6)	(890.0)	(824.9)	(871.5)
Firm size	4136.2	3832.3	3366.5	3726.0
	(10891.7)	(10385.7)	(9722.2)	(10438.6)
Average coworker earnings (1000s SEK)	197.0	194.4	204.4	205.0
	(105.6)	(95.74)	(98.93)	(93.10)
% of coworkers with active records	11.30	11.73	11.11	11.14
	(14.44)	(14.84)	(13.91)	(13.70)
Firm pay premium at main job	0.144	0.145	0.193	0.244
	(0.830)	(0.822)	(0.809)	(0.755)
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	29479			

...

Table C.8
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 the firms according to whether they ever signal background checks. Columns (4) and (5) present the same splits for firms that are likely non-public.

Table C.9
Correlations between firm FE for P(Suspect) and other firm characteristics

		Firm FE for P(suspect)				Firm FE for P(WCR)	
		Non-mandated	Mandated				
Value-added per worker (100k SEK)	0.000004						
	(0.000003)						
Firm pay premium (100k SEK)		-0.0034					
		(0.0001)					
Background check in job ad			0.004	0.002			
			(0.002)	(0.002)			
Manager with active record					-0.015		
					(0.001)		
Manager with ever record						0.009	
						(0.001)	
Placebo man. with active record							0.023
							(0.001)
Placebo man. with ever record							
SD of DV	0.010		0.087	0.056	0.138	0.138	0.184
Observations	349,631		32,115	4,671	385,631	385,631	385,625

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