Job Sorting and the Labor Market Effects of a Criminal Record

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November 2026

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, largely within industries. To estimate the causal effect of a criminal record, we use an event study design that compares outcomes for adults charged with an offense for the first time to matched adults who were suspected of a similar offense but not charged. Acquiring a criminal record reduces annual earnings by 5%, with larger effects for more serious charges. Using a record registry reform, we show that when charges are not visible to employers, the same design yields no detectable effects. The evidence indicates a central role for the record itself, rather than sanctions or job disruption generated by the adjudication process. We classify firms by their propensity to hire workers with criminal records, holding suspected offense history fixed. A criminal record reduces employment at firms classified as less likely to hire workers with criminal records, increases employment at other firms, and decreases monthly earnings within firms. Firm propensity to hire workers with criminal records varies substantially, including within industry, and is decreasing in firm size and increasing in managers' prior exposure to people with records.

Keywords: criminal record, earnings, employment, firms, discrimination, managers
JEL Classification: J3, J6, J71, K42, M51

*We thank our discussant Jeffrey Grogger, Pat Bayer, Erik Lindqvist, Björn Tyrefors, and seminar participants at the NBER Summer Institute, the University of Gothenburg, the Transatlantic Workshop on the Economics of Crime in Zurich, Duke, Public Policy Institute of California, Cal Poly San Luis Obispo, UC Berkeley, Cornell, UC Santa Barbara, UC Irvine, Stanford, Queen Mary University of London, IIES, SOLE, IFAU, DIW Berlin, the University of Hong Kong, the Università della Repubblica, and the Conference on the Economics of Crime and Justice 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 authority can be held responsible for them. We also thank Tomas Reivinger for his excellent research assistance.

1 Introduction

Firms often conduct criminal background checks when screening job applicants. Surveys and audit studies indicate that many firms are reluctant to hire workers with criminal records (WCRs) for at least some roles.[1] However, important questions remain about the extent to which a criminal record itself harms labor market prospects and the mediating role of heterogeneity in employer preferences. In particular, the labor market consequences of a criminal record may depend on whether WCRs can identify or seek employment with firms more willing to hire them, as well as the wages offered in those jobs (Becker, 1971).

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

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

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

[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.

Averaging across years in the study period, about 9% of prime-age men and 2.5% of prime-age women have an active criminal record in a given year, while 28% of prime-age men and 9% of prime-age women have ever had a record. Conditional on education, age, gender, and immigrant status, prime-age WCRs earn 31% less than prime-age adults without records. Conditional on worker demographics, WCRs work about 20% fewer months annually. Among those employed, the (residual) monthly earnings gap between WCRs and non-WCRs is 14 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. While prior work documents that WCRs are concentrated in specific sectors (e.g., Rucks-Ahidiana et al., 2021), like construction and manufacturing, we find that residual sorting is almost entirely within industry. Differences in firm pay premiums explain about 30% of the (residual) monthly earnings gap, where pay premiums are estimated using a standard worker-firm earnings decomposition (Abowd et al., 1999; Card et al., 2016). WCRs are less likely to work in occupations involving interactions with vulnerable populations, opportunities for theft or financial responsibility, customer exposure, high educational or skill requirements, non-hazardous work conditions, and mandatory background checks.

To estimate the causal effect of a criminal record, we develop a research design within an event study framework. We compare labor market outcomes for adults charged with a criminal offense for the first time to matched adults identified by police or prosecutors as reasonable suspects for a similar offense but not charged, typically due to a lack of evidence. Many in the latter group are arrested during the investigation. Nonetheless, the comparison group does not acquire a criminal record in Sweden, nor would they in much of Europe.[2]

The motivation for this design is that charged and non-charged suspects are plausibly similar in the circumstances that led police to suspect them. Importantly, the validity of our design does not require that charged and non-charged suspects engaged in similar conduct. Instead, it requires that, absent the charge, the two groups would have followed similar labor market trajectories following the suspected offense. We focus on a sample of adult suspects between the ages of 22

[2]By contrast, we speculate that many in the comparison group would acquire a criminal record in the United States. Some would be formally charged and then later have those charges dropped by the prosecutor or dismissed by the judge, relatively common case outcomes. The initial charge, unless sealed or expunged, would often be publicly accessible via court dockets and reported by background check providers. For employment screening, the Fair Credit Reporting Act (FCRA) permits consumer reporting agencies to report non-conviction records for up to seven years and convictions without a time limit. In many states—though not all—commercial background checks may include arrests that did not lead to formal charges. Arrest data can be sourced from police booking logs or other public arrest records.

and 40 who have no prior charges. Among those charged, nearly all acquire a criminal record, with most receiving a fine or probation as punishment. Supporting the validity of our research design, charged suspects and matched non-charged suspects work in similar jobs, have similar coworkers, and exhibit similar employment and earnings trajectories prior to the suspected offense. They are also suspected of subsequent offenses at comparable rates.

We find that a person's first criminal charge reduces months employed by 2% and annual earnings by 5%, with larger effects for more serious offenses (4% and 8%). Additional evidence indicates that these estimates reflect the causal effect of a record. We leverage a reform that made certain minor charges visible on criminal records. Before the reform—when these charges were omitted from records—we find no detectable effects on employment or earnings; after the reform, the same charges reduce earnings, including for cohorts for whom the reform applied retroactively. This pattern argues against sanctions or job disruptions related to the adjudication process as explanations for the effects of these minor charges. Incapacitation and incarceration also play limited roles: incarceration sentences are rare (8% of our event study sample) and often rehabilitative in the Scandinavian context (Bhuller et al., 2020; Hjalmarsson and Lindquist, 2022), and excluding suspects sentenced to prison for the focal offense does not change our conclusions. Finally, consistent with recent U.S. evidence (Agan et al., 2024b), expungement does not appear to mitigate these penalties: (i) the earnings gap between charged and non-charged suspects persists for over a decade, after most offenses are expunged and (ii) labor market outcomes do not improve in the years following expungement.

We unpack these employment and earnings reductions by investigating how a criminal record affects: (1) where people work and (2) how much they earn at a given employer. We first extend the charged versus non-charged suspect research design to classify firms by their propensity to hire WCRs using an empirical Bayes approach (Walters, 2024). Focusing on hired workers with a suspected offense in the recent past, we measure each firm's tendency to hire WCRs conditional on the suspected offense history of their hires. Under this approach, we classify a firm as less willing to hire WCRs if, relative to other firms, they are more likely to hire non-charged suspects than otherwise comparable charged suspects. We then re-estimate our charged versus non-charged event study specification, replacing the outcome with employment or earnings at low-, medium-, and high-propensity firms. We find that the overall decrease in months worked is concentrated at firms less likely to hire WCRs, consistent with these firms discriminating on the basis of criminal record status. The employment decrease at these firms is partially offset by an increase in employment

at more WCR-"friendly" firms. Among those working, monthly earnings decrease by 2%. This mostly reflects a decrease in monthly earnings within firms, though a criminal record also leads to a modest shift from higher- to lower-paying firms.

Overall, we attribute about 40% of the annual earnings decline to fewer months employed and 20% to shifts toward firms with lower average coworker earnings. The remainder captures within-firm changes in monthly earnings and between-firm reallocation along dimensions not summarized by average coworker earnings.

Finally, we relate firms' estimated propensities to hire WCRs relative to otherwise comparable non-charged suspects to firm characteristics that may speak to why some firms are more likely to hire WCRs than others. Five patterns emerge. First, consistent with employer survey evidence on stated willingness to hire WCRs (e.g., Holzer et al., 2006b) and with research on how criminal justice interactions affect where workers sort (e.g., Mueller-Smith and Schnepel, 2021; Rose, 2021; Agan et al., 2024b), some industries (e.g., construction) are more WCR-friendly than firms in other industries (e.g., public administration and defense, health care, and education). Second, however, industry accounts for only a small share of variation across firms in their propensity to hire WCRs. Third, small firms are more likely to hire WCRs than large firms. Fourth, firms that signal criminal background checks in their job ads are less likely to hire WCRs, consistent with these firms using checks to screen out applicants with records. Fifth, firms with operating managers who have a past criminal record themselves or have been exposed to records via family members or past coworkers are more likely to hire WCRs.

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, monetary sanctions and prison time, affect labor market outcomes (Dobbie et al., 2018; Mueller-Smith and Schnepel, 2021; Finlay et al., 2024; Mueller-Smith, 2015; Bhuller et al., 2020; Garin et al., 2023). Such effects may arise through several channels, including the persistent effects of job displacement, depreciation of human and social capital while in prison (Western et al., 2001), or stigma of a resulting criminal record.

Our paper advances this literature by isolating the labor market 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. While prior research has leveraged examiner designs and policy discontinuities

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

to credibly identify the labor market effects of pretrial detention, diversion, and incarceration, applying these approaches to study the effect of a criminal record has been more difficult, in part because researchers typically do not observe suspects who are not charged.[4] We use data on suspects who are not charged in a matched event study design. An examiner design is not feasible in our setting because we do not observe the prosecutor assigned to each case and, to the best of our knowledge, cases are generally not assigned randomly. Instead, we validate our research design using a reform to the record registry.

Prior studies on the labor market effects of arrests and criminal charges (Grogger, 1995; Rose, 2021; Agan et al., 2024b) compare those arrested or charged at younger versus older ages in an event study framework. In our setting, this age-based research design generates clear pre-trends, and applying the same approach to non-charged suspects indicates that they too experience a drop in employment and earnings following their suspected offense. This finding highlights the appeal of using non-charged suspects as the comparison group, which in principle nets out the effects of the (typically unobserved) circumstances leading to an alleged offense (e.g., criminal and non-criminal activities, mental health crises, social interactions, and economic prospects).[5]

A related US-based literature asks whether prosecution and conviction themselves affect later criminal justice contact. Agan et al. (2023) exploit quasi-random assignment of nonviolent misdemeanor complaints to prosecutors and find that prosecution increases subsequent criminal complaints. They highlight the creation of a statewide record as one plausible mechanism: prosecution sharply increases the likelihood that the initial complaint enters the statewide record system available to criminal justice agencies and, under more limited circumstances, employers serving vulnerable populations (e.g., children and the elderly). Mueller-Smith and Schnepel (2021) exploit discontinuities in eligibility for felony diversion and find that avoiding conviction reduces subsequent criminal justice contact and improves employment. Humphries et al. (2025) and Kamat et al. (2024) isolate the effect of noncarceral conviction and find that it increases subsequent criminal charges. Together, these studies show that prosecution and conviction can affect later criminal justice contact, whether through changes in offending or through criminal justice responses conditional on conduct. Our paper complements this literature by examining the labor market consequences

[4] An important exception is Agan et al. (2023), which we discuss below.

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

of a criminal record and how sorting across heterogeneous employers mediates those consequences.

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

While this literature suggests that some firms discriminate against WCRs, it has two fundamental limitations. First, it typically relies on a small and selected sample of firms to measure callback rates or firm stated preferences rather than actual employment or earnings outcomes. Evidence of disparate treatment by participant firms does not necessarily imply disparate outcomes for real-world job applicants with criminal records. For instance, if there are enough non-discriminatory employers, the effects of discrimination may be moderated by workers' ability to sort to those employers (Becker, 1971). On the other hand, in models with search frictions, workers from disfavored groups may experience longer unemployment spells and receive lower wages, even at nondiscriminatory firms, because they have worse outside options (Black, 1995; Bowlus and Eckstein, 2002). Sorting to less discriminatory firms may decrease earnings if the most discriminatory firms also offer higher wages (Card et al., 2016; Gerard et al., 2021). Mueller-Smith and Schnepel (2021) find suggestive evidence that felony diversion increases employment particularly in industries where people with felony convictions are underrepresented. Our population-wide employee-employer matched registers, combined with our charged versus non-charged suspect design, allow us to extend this evidence to employer-level heterogeneity, including sorting across firms within the same industry. Consistent with frictional models, we find that a criminal record reduces employment at more discriminatory firms, increases time spent unemployed, and decreases monthly earnings within employers.[6]

The second limitation is that much of the literature is based in the United States, an interna-

[6] A related literature studies how the economic performance of local industries where WCRs are concentrated (e.g., construction) affects WCR labor market outcomes (Finlay and Mueller-Smith, 2021) and recidivism (Schnepel, 2017; Yang, 2017).

tional outlier when it comes to the accessibility, visibility, and breadth of criminal records.[7] In the US, unlike most of Europe, including Sweden, background checks are widespread and often include arrests, records are often public, and automatic expungement is the exception. We provide the first study of how records affect employment and earnings in Europe. Our findings can inform an active US debate about how to mitigate the negative consequences of a criminal record.[8]

Finally, our descriptive evidence relates to a third literature on managers and the hiring of workers from disadvantaged groups. Prior work studies worker-manager concordance in race, immigrant status, and gender (Giuliano et al., 2011; Åslund et al., 2014; Benson et al., 2022; Cullen and Perez-Truglia, 2019), as well as how managers' experience shapes personnel decisions (Miller, 2017; Li et al., 2020; Benson and Lepage, 2024; Lepage, 2024; Ronchi and Smith, 2024). We contribute evidence linking manager backgrounds to realized firm hiring: firms led by operators with their own criminal records or with family or prior-coworker exposure to WCRs have higher estimated WCR hiring propensities.

The remainder of the paper proceeds as follows. Section 2 describes the Swedish criminal justice system, including the rules related to criminal records and background checks. Section 3 describes the data. Section 4 presents new descriptive facts about the labor market for WCRs. Section 5 describes the research design comparing charged and non-charged suspects, reports the baseline results, and presents evidence that the effects operate through the record itself rather than sanctions, adjudication-related job disruptions, or other contemporaneous shocks. Section 6 measures and characterizes firm heterogeneity in the propensity to hire WCRs and examines how this firm heterogeneity mediates the labor market effects of a record. Section 7 concludes.

2 Institutional background

2.1 Crime and the criminal justice system

Figure 1 depicts how a suspect progresses through the Swedish criminal justice system. This will inform our charged versus non-charged suspect research design in section 5.1. When a crime is reported, the police are in charge of the initial investigation and identification of suspects. The case is then passed to a prosecutor who decides if there are sufficient and reasonable grounds for

[7] Exceptions include two correspondence studies (Ahmen and Lång, 2017; Baert and Verhofstadt, 2015) and papers on the effects of prison in Norway (Bhuller et al., 2020), record expungement in New Zealand (Dasgupta et al., 2025), and an evaluation of the 2014 introduction of the Swedish online record depository, Lexbase (Forsberg, 2024).

[8] See Rose (2021), Agan and Starr (2018), and Doleac and Hansen (2020) for studies of US ban the box laws and Dasgupta et al. (2025), Agan et al. (2024a), Agan et al. (2024b) for evaluations of expungement and clean slate laws.

an arrest of the suspect(s).[9] If suspects meet this threshold, then they are included in the suspect register (*misstankeregistret*), informed of their status, and typically arrested. The prosecutor (or police for minor offenses like shoplifting, traffic violations, or petty theft) continues the investigation. Suspects can only be detained pre-trial if the alleged crime carries a potential prison sentence of at least one year and there is a risk of continued criminal activity, an evasion of trial, or disrupted investigation. Given these restrictions, pre-trial detention is relatively rare in Sweden.[10] Depending on the evidence, the prosecutor decides whether to charge the suspect or dismiss the case without charges. The reason for dismissal is often recorded in the suspects register. The most common stated reason is insufficient evidence; prosecutors rarely state that a suspect is innocent. The evidence threshold used to justify a charge in Sweden is especially high, with nearly all charged cases (around 97%) resulting in a determination of guilt.

Charged suspects can follow four possible paths, which are characterized by whether the case: is processed in or out of court, leads to a determination of guilt, and the possible sentences. The first path is a non-conviction (AUL or åtalsunderlåtelse). This out-of-court decision results in no punishment but, despite its name, does include a determination of guilt. AUL occurs when prosecutors waive their duty to prosecute, typically because the crime is very minor, the suspect is especially young, or the suspect is facing charges for other, more serious crimes. The second path is an out-of-court conviction (SFL or strafföreläggande), in which guilt is determined and a non-prison punishment is possible. The last two paths are for those cases that are decided in court: in-court convictions and in-court acquittals. In the case of convictions, defendants can either admit their guilt or be found guilty by the court, which consists of professional and lay judges. The court then determines the sentence (e.g., fines, probation, prison). Furthermore, because Sweden does not have plea bargaining, prosecutors typically play a minimal role in sentencing outcomes.

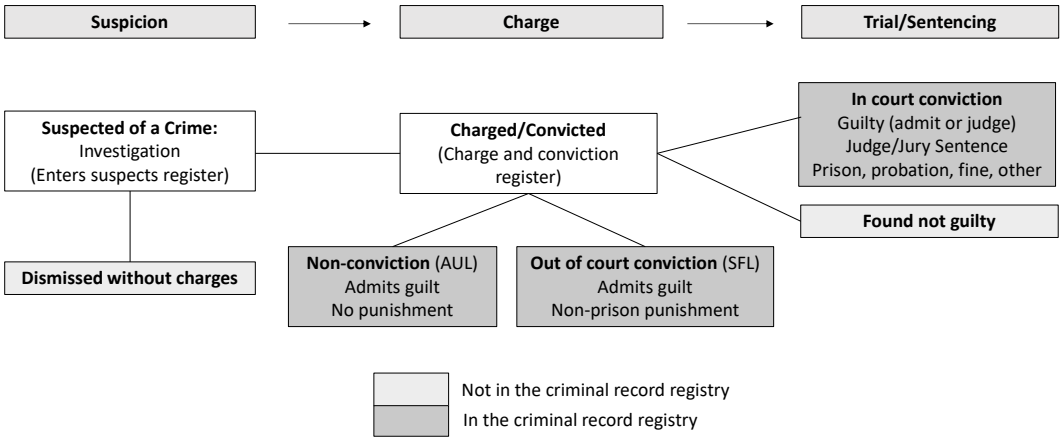
Of central importance to our paper is the fact that any path that leads to a determination of guilt (whether through admission or a court verdict) will result in the offense being recorded in the criminal record registry (termed in Swedish *belastningsregistret*). This is a living registry maintained by the police for the purposes of conducting background checks: new offenses are regularly added while old ones are expunged, according to the rules described below.

We illustrate the frequency of these different outcomes with a snapshot of the more than 174,000

[9] Today there are 31 local prosecution offices in Sweden. Cases are assigned to offices based on the location of the offense. To the best of our knowledge, assignment to prosecutors within these offices is not random but based on factors like caseload, experience, and expertise.

[10] In 2009, for instance, less than 5% of the 244,000 people considered to be suspects by the Swedish public prosecutor's office were detained (Åklagarmyndigheten, 2010).

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. We also indicate, where appropriate, the criminal justice registries in which offenses are recorded. Offenses that result in a determination of guilty will be added to the criminal record registry (dark shaded cells). They remain in the registry until they qualify for automatic expungement, which is typically after 3, 5, 10 or 20 years.

people suspected of at least one crime in 2010. Allocating these suspects to each of the five pathways according to the outcome of their most serious charge, we find 44% were dismissed without charge, 20% charged and determined guilty out-of-court (5% AUL, 15% SFL), while 34% were found guilty in court. Less than 3% of suspected individuals are actually found not guilty in court. In terms of sentences, only 6% of all suspects are eventually sentenced to prison while fines are the most common form of sanction (out-of-court convictions typically result in a fine as do about half of all in court convictions).[11]

2.2 Background checks

For nearly all of our sample period (1990-2015), Swedish firms could conduct background checks through two channels. First, some employers (e.g., in criminal justice, central government administration, energy, health, transportation, and defense) have direct access to criminal record registries, which only include offenses that led to a determination of guilt and that have not yet been expunged. Second, employers without direct access can ask applicants to obtain an extract (at no cost) from this registry. Suspected offenses are thus not visible to Swedish employers. Swedish guidelines

[11] See https://bra.se/english/statistics/statistics-from-the-judicial-system/#h-Processedoffenceslinkedtoasuspect.

recommend that employers (i) only request this extract at the end of the hiring process, (ii) avoid keeping or copying it, and (iii) accept it as valid for one year, allowing applicants to use the same document at multiple employers. Though it is not illegal to discriminate on the basis of criminal records in Sweden, the police website emphasizes that a criminal record is not disqualifying (even if the background check is mandatory).[12]

The information visible in the extract depends on the type and timing of the request. Since 1989, individuals can obtain a "general extract", which includes all offenses and corresponding sentences, regardless of type, that are not yet expunged. As described in Appendix B.4, starting in the early 2000s, mandatory checks were introduced for certain jobs in education (2001), insurance (2006), residential and foster care homes (2007), children with disabilities (2011), and children in non-school activities (optionally, 2014). Such occupation-specific mandatory extracts only show selected offenses deemed relevant (by the government) to such jobs.[13]

Appendix Figure A.1 shows the annual number of extracts overall and by type (general versus occupation-specific) per 100 people aged 18 to 64. The number of extracts per 100 people increased from around 2.8 in 2001 to 16.4 in 2022, when there were more than one million extracts. Extract requests from the registry even continued to grow after the emergence of online companies like Lexbase in 2014, which provide a searchable database of digitized court documents.[14] These statistics understate the number of background checks conducted each year, given that some employers have direct registry access and that extracts are meant to be usable for more than one job application.

Further insights into the prevalence of background checks can be gained from a novel supplementary data set of Swedish job ads (described in section B.1), in which we measure whether a firm signals in each ad that the hiring process includes a background check. There is substantial variation in the prevalence of these signals across occupations, ranging from less than 4% of ads for resources/agriculture, construction, and manufacturing jobs to 18% of ads for elementary unskilled jobs. In 2022, about 55% of ads were at firms that at least sometimes signaled background checks,

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

[13] For a list of selected offenses by type of extract, see the Swedish Police Authority's website.

[14] Some features of Lexbase worth highlighting are: (i) it is not free to use whereas regular extracts are, (ii) documents are still expunged from the database but not according to rules applied to the criminal record registry, (iii) offenses that result in a court acquittal are not visible in the official record registry but would be found in lexbase, and (iv) it can be used for many purposes other than employment, e.g., dating, renting. A media analysis presented in Appendix Figure A.3 further demonstrates the increasing prevalence of criminal record extracts over time. It also shows that they were already salient before the 2014 introduction of Lexbase, which only temporarily shocked the debate due its controversial nature.

making clear that many jobs are with employers who use background checks. These patterns are consistent with a 2014 survey of Swedish firms (SOU 2014:48) presented in Appendix Figure A.2; 14% of firms, but more than 50% of the largest, report background checks for all or some new hires.

There are some stark differences between Sweden and the United States, where background checks are primarily conducted by specialized companies (Agan et al., 2024b) and information concerning many criminal justice interactions, including many arrests, are accessible to the public. European privacy laws restrict access to criminal record information, especially arrest records. However, employers (including in Sweden) generally have the right to ask candidates for record extracts or about their criminal history. Finally, hiring WCRs does not expose Swedish employers to negligent hiring liability as it may in the US.

2.3 Record expungement rules and reforms

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

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

Appendix B.2 describes the expungement regulations from 1990 to today, which we use to code whether an individual has an 'active' criminal record each year and the nature of that record.

3 Data

This section describes the core Swedish register data sets used in our analysis. Supplementary data on occupational characteristics, job ads, record expungement rules, and background check laws are described in Appendix B.

3.1 Swedish registers and key variables

For the entire population born between 1923 and 2000, we combine matched employee-employer registers with two core crime data sets from the Swedish National Council for Crime Prevention. The Convictions Register (*lagföringsregistret*) spans 1973 to 2015 and provides a complete history of an individual's convictions (and acquittals), including offense and conviction dates, crime types, and sanctions (e.g., prison, probation, fines) (Brottsförebyggande rådet, 1973-2015a). We use these data to calculate whether a person has an *Active Criminal Record* each year, i.e., whether a person has been convicted of an offense in the past that is not yet expunged from the background check register. We exclude traffic infractions (e.g., speeding offenses), but include more serious traffic-related offenses, such as driving while intoxicated. In other words, we have all of the information necessary to recreate the state of the criminal record registry (*belastningsregistret*) each year.

The Suspects Register (*misstankeregistret*) is available from 1995 onward, and also includes crime types as well as offense and decision dates (Brottsförebyggande rådet, 1995-2015b). Suspects are added to this register as soon as they are arrested or substantively connected to a crime as part of a police or prosecutor investigation. The causal design in section 5 takes advantage of the fact that offenses remain in this register and are observable to researchers (but not employers), even if they do not lead to a formal admission of guilt and/or conviction.

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

Along with occupational and workplace sorting, we study the effects of having a criminal record on annual earnings, months employed during the year, and monthly earnings (Statistiska central-

byrån, 1990-2015c; Statistiska centralbyrån, 1990-2015d). Annual earnings are measured in real Swedish krona (SEK) using 2015 prices.[15] These are pre-tax earnings reported by employers to the tax authorities. We also know the start and stop month for each job spell, which allows us to calculate average monthly earnings for each job a worker has and the number of months per year that a worker is employed.

Finally, these data are combined with registers that provide demographic information on each individual and their family members, including gender, immigrant status, birth year, and educational attainment, as well as military enlistment test scores for several older cohorts of men (Statistiska centralbyrån, 1990-2015a; Statistiska centralbyrån, 1990-2015e; Plikt- och prövningsverket, 1964-2010; Krigsarkivet, 1950-1988).[16]

3.2 Core analysis sample

We create a person-by-year level data set that tracks each person's criminal record status and labor market outcomes from 1990 to 2015. The baseline data set includes workers between ages 18 and 55. Our analysis sample is composed of 126.8 million person-year observations from 8.6 million people. These individuals are observed to work in more than 1.4 million firms, but firm composition changes over time; about 330,000 unique firms per year are observed. We use this sample to characterize the labor market for workers with and without criminal records in section 4, but make additional sample restrictions in our event study analysis in section 5. We also use data on newly-hired workers in section 6 to measure firm heterogeneity in hiring of WCRs. We define a worker as a new hire if that year is the first time the worker is observed at the firm since the beginning of our sample in 1990.

4 Descriptive facts about the labor market for WCRs

4.1 Prevalence of WCRs

We begin by describing the prevalence of criminal records in Sweden. Appendix Figure A.4 shows the percentage of working-age adults with an active criminal record from 1990 to 2015 (panel A) and by age in 2015 (panel B). Over the sample period, the average percentage of men and women

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

[16] 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.

with a record has been 9.4% and 2.5%, respectively, rising from 8.4% and 1.9% in 1990 to 9.2% and 2.6% in 2015. There is a notable jump in the percentage of the workforce with an active record in 2000; this is not driven by a jump in crime, but rather by a reform, which we analyze in Section 5.5, that included more minor offenses in the record registry (see Appendix B.2 for more details about the reform). Consistent with the typical age profile of criminal behavior, the percentage of adults with an active record initially increases with age, peaks at 24 for men and 20 for women, then decreases and plateaus around 40. Among those ever convicted of an offense, the median age at first conviction is 22. About 25% of men and 9% of women have been found guilty of a crime at least once. These rates are comparable to those documented in the United States and the rest of Europe.[17]

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

4.2 Characteristics of WCRs

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

In the labor market, WCRs work on average two and a half fewer months per year (6.99 versus 9.59) and have average earnings that are about 40% lower (122k SEK versus 204k SEK).[20] Among those working, WCRs have shorter job tenure (35 months versus 55 months). WCRs also tend to work in smaller establishments and firms and in establishments where a higher share of

[17] 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.

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

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

[20] Appendix Figure A.5 demonstrates substantial labor market gaps between workers with and without records throughout the sample, with larger gaps 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)
Earnings rank in cohort by gender	51.36	34.31
	(30.16)	(30.25)
Months employed during year	9.588	6.994
	(4.483)	(5.502)
Any months employed during year (%)	85.59	67.58
	(35.11)	(46.81)
Tenure in main job (in months)	54.62	34.87
	(54.09)	(40.55)
Workplace size	451.1	287.2
	(1271.0)	(964.2)
Firm size	6467.2	4413.5
	(13582.2)	(11762.0)
Avg. coworker monthly wage (1000s SEK)	19.83	17.56
	(10.49)	(8.836)
Avg. firm coworker monthly wage (1000s SEK)	18.79	17.22
	(9.945)	(8.430)
Firm pay premium	-0.00730	-0.0535
	(0.237)	(0.268)
% of coworkers with active records	3.906	8.830
	(6.490)	(12.13)
% of firm coworkers with active records	4.000	8.102
	(5.832)	(11.28)
Industry (%):		
Agriculture and resources	3.203	3.608
Care and social services	10.11	9.106
Construction	4.825	9.611
Education	8.405	4.139
Healthcare	6.541	2.426
Hotels and restaurants	2.981	5.767
Manufacturing and industry	14.63	18.73
Public administration and defense	4.745	2.120
Retail and wholesale	11.15	11.45
Services	20.88	20.80
Unknown	0.934	1.757
Missing	11.60	10.49
Observations	107,454,183	6,945,896

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

coworkers also have records (8.8% versus 3.9%). They are more likely to work in construction, hotels and restaurants, and manufacturing and less likely to work in education, health care, and public administration and defense.

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

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

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

Table 2 shows large raw and residual differences in labor market outcomes by criminal record status. WCRs earn about 42% less than non-WCRs before adjusting for demographics and 31% less after adjustment. They also work about two fewer months per year conditional on demographics, roughly 20% less than non-WCRs. Among those employed, the raw gap in monthly earnings is 18 log points and the residual gap is 14 log points. Controlling for industry reduces this gap only slightly, to 13 log points, while firm fixed effects reduce it to 9 log points. Hence, part of the monthly earnings gap reflects sorting toward lower-paying firms, but substantial differences remain within firms. Appendix Table A.1 shows that results are similar when controlling for cognitive test scores measured for males during military conscription. [21]

Table 3 examines this firm sorting directly. Conditional on demographics, WCRs work at firms with pay premiums about 4% lower than those employing otherwise similar non-WCRs; controlling for industry reduces this gap only modestly, to 3%. WCRs also work in establishments with a 2.9 percentage point higher share of coworkers with active records after adjusting for demographics, and adding industry fixed effects has little effect on this difference. Hence, although WCRs are

[21] Specifically, Appendix Table A.1 focuses 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 annual and monthly earnings gaps. This in part reflects the fact that residual test score differences between WCRs and non-WCRs are modest, about 0.17 of a standard deviation.

Table 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	-0.18	-0.14	-0.13	-0.09
	(0.08)	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		✓		✓		✓	✓	✓
Industry FE							✓	
Firm FE								✓
Non-WCR outcome mean	204.35	204.35	9.59	9.59	9.79	9.79	9.79	9.79
Observations	114,400,079				89,949,518			

This table presents the results of estimating equation 1 for our baseline sample of working-age adults from 1990 to 2015. The dependent variable is earnings in columns 1 and 2, number of months worked in columns 3 and 4, and log monthly earnings in columns 5 through 8. In columns 5 through 8, we focus on monthly earnings from an individual's main job, defined as the employer from whom they earned the most in a given year. We restrict the analysis to jobs held for at least two months in that year, with average monthly earnings of at least 2,000 SEK. The columns labeled "Raw" only control for year fixed effects. Demographic controls (fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born) are included in columns 2, 4, and 6 through 8. Industry and firm effects are included in columns 7 and 8, respectively. Only the coefficient on RECORD_{it} is reported; standard errors clustered at the person level are reported in parentheses.

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	-0.04	-0.04	-0.03	4.73	2.89	2.80
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographics		✓	✓		✓	✓
Industry FEs			✓			✓
Non-WCR outcome mean	0.00	0.00	0.00	3.85	3.85	3.85
Observations		88,955,825			82,343,399	

This table presents the results of estimating equation 1 where the dependent variable is the firm pay premium in columns 1 through 3 and percent of coworkers with a criminal record in columns 4 through 6. The sample includes working-age (18–55) adults from 1990 to 2015 with monthly earnings that meet the sample criteria applied in Table 2. 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.

overrepresented in sectors such as construction, manufacturing, and food services (e.g., Rucks-Ahidiana et al., 2021), much of their sorting into lower-paying firms and workplaces with other WCRs occurs within industries.

4.3 Segregation of WCRs across occupations

Appendix D documents occupational differences between WCRs and non-WCRs and how much of these differences are explained by observable characteristics correlated with record status. Conditional on demographics and industry, WCRs are (relative to clerks, the omitted category) more likely to be employed in service and care, resources and agriculture, construction and manufacturing, industry and transport, and elementary unskilled occupations, and less likely to be employed as professionals, technicians, or managers. The adjusted differences can be substantial: WCRs are about 40% less likely to be employed as professionals than in elementary unskilled occupations. To better understand the nature of this sorting, we link occupations to O*NET characteristics and mandatory background check requirements. Conditional on demographics and industry, WCRs are markedly less likely to work in occupations with: (i) high external customer contact, exposure to vulnerable populations, or the opportunity for theft and financial responsibility, (ii) mandatory background checks, (iii) higher education and skill requirements, and (iv) better work conditions. Consistent with our firm-level results, much of this occupational sorting occurs within industry.

Because occupational records are incomplete, particularly earlier in our sample period, we do not focus on occupation-specific outcomes in the analyses that follow. Moreover, we emphasize that even in occupations where background checks are mandatory, criminal records are not disqualifying for employment. It is only mandated that prospective employers check for criminal records and then make their own assessment regarding the suitability of an applicant for the position.

5 Causal effects of a criminal record

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

This section develops and applies a novel research design to estimate the causal effect of a criminal record on employment and earnings. In section 6 we apply the same approach to study the mediating role of firm sorting.

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. Our approach leverages data on people 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.[22][23] People typically enter the suspects register as the result of a formal police investigation and are typically arrested, regardless of whether they are ultimately charged. In Sweden, nearly all of the treatment group acquires a criminal record associated with the offense that employers can access, while the comparison group

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

[23] Event study designs with matched comparisons have been used to study the effects of mass layoffs (e.g., Jacobson et al., 1993; Couch and Placzek, 2010; Gathmann et al., 2020; Britto et al., 2022) and crime victimization (Adams et al., 2024, 2025), among other topics.

does not.[24]

Our main identifying assumption is that, absent the charge, the two groups would have followed similar labor market trajectories after the suspected offense. Critically, our design does not require the decision to charge to be as good as randomly assigned, nor does it require charged and non-charged suspects to have engaged in identical conduct. The comparison group may include both people who committed the alleged offense but were not charged and people who did not commit it. Instead, our design requires that, after matching, any remaining differences between the groups—including differences in underlying conduct—would not have led their labor market trajectories to diverge in the absence of the charge.

This research design offers at least two key strengths. First, the two groups likely share similar circumstances that led police to suspect them of similar offenses. These circumstances include the activities in which they engaged, the people with whom they associated, and their social and economic background and prospects. The design also accounts for potential labor market consequences of being suspected or arrested, which may result from stress, reputational harm, or job disruption.

Second, charging decisions are not determined solely by a suspect's underlying conduct, making charge status a noisy proxy for that 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. For example, prior work shows that idiosyncratic variation in police response times affects the quality of available evidence and, in turn, crime clearance rates (Blanes i Vidal and Kirchmaier, 2018; Facchetti, 2024).

Data from the suspects register support the idea that charge decisions often vary for reasons unrelated to suspect conduct. The reason prosecutors most frequently cite for not pursuing a charge is "insufficient evidence". For dropped charges where a reason is specified, prosecutors cite "insufficient evidence" for 60% of offenses, compared to just 2% for "not guilty" and 14% for "not a crime". These patterns indicate that the quality of the available evidence, rather than underlying conduct alone, is an important determinant of whether prosecutors bring charges.[25]

Consistent with an important role for prosecutor discretion, charging rates vary substantially across prosecutor offices for the same suspected offense and even for the same individual suspect.[26]

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

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

[26] Unfortunately, we cannot identify the specific prosecutor or police officer within an office who made each charging decision. This limitation prevents us from applying an examiner-based research design.

In cases handled by prosecutor offices (accounting for about 80% of suspected offenses),[27] prosecutors pursue charges for 55% of suspected offenses. The standard deviation in charge rates across offices is 12.4%. This variation remains substantial even after conditioning on the specific offense (10.3%) or both the offense and the suspect involved (8.7%).

These institutional features strengthen the case for using matched non-charged suspects as a comparison group for charged suspects. Ultimately, however, we use the data to assess the validity of this research design. Specifically, we will show that the treatment and matched comparison groups (1) hold similar jobs, have similar coworkers, and follow very similar earnings paths before the suspected offense and (2) are suspected of future offenses at similar rates. We also conduct a placebo test leveraging a policy reform that made certain prior minor offenses visible on criminal records; the results provide key evidence that the record itself, rather than the associated sanction, matters.

5.2 Matching charged and non-charged suspects

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

We collapse suspected offenses to the person-year level. For our core sample of events, we limit to person-years with at least one associated suspected offense where the person: (i) does not have a prior charged offense; (ii) is between the ages of 22 and 40 in the event year, and (iii) is present in Sweden (and hence, in our data) at least 4 years before and 5 years after the event. We make the first restriction to focus on the extensive margin of having any criminal record. We examine subsequent offenses separately in section 5.7. We make the second and third restrictions 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

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

refer to either the focal suspected offenses or the suspect's characteristics in the year before the focal event. In 34% of events, a suspected offender is charged with at least one offense.[28] We define these suspects as *charged*.

Charged and non-charged suspects are suspected of different types of crimes. Non-charged suspects are more likely to be suspected of a violent offense (48.81% vs. 25.63%), and less likely to be suspected of property (31.38% vs. 39.45%), traffic (5.57% vs. 28.13%), or narcotics offenses (4.68% vs. 11.86%). These differences likely stem in part from varying challenges in evidence collection. For example, in cases where someone is suspected of driving while intoxicated, evidence 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 (148.8K SEK vs. 135.0K SEK) and months of employment (8.22 vs. 7.99). Interestingly, the two groups work in similar industries and in establishments where about 7% of coworkers have active records. Similar shares of charged and non-charged suspects reside in each of the major metropolitan areas: Stockholm, Göteborg, and Malmö.

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. For events involving offenses in multiple categories, we match on the full combination of

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

Table 4
Characteristics for suspects and suspected offenses

	All			Matched and weighted		
	Not charged	Charged	Std. diff.	Not charged	Charged	Std. diff.
Panel A: Variables used in match						
Year of suspected offense	2003.8	2003.0	-0.188	2002.8	2002.8	0.000
	(4.544)	(4.658)	[< 0.001]	(4.717)	(4.717)	–
Number of prior years with suspected offenses	.3534	.2787	-0.111	.1355	.1355	0.000
	(.7154)	(.6317)	[< 0.001]	(.4036)	(.4036)	–
Type of suspected offense:						
Violent (%)	48.81	25.63	-0.494	23.85	23.85	0.000
			[< 0.001]			–
Property (%)	31.38	39.45	0.169	40.34	40.34	0.000
			[< 0.001]			–
Traffic (%)	5.572	28.13	0.632	24.28	24.28	0.000
			[< 0.001]			–
Narcotics (%)	4.677	11.86	0.263	5.877	5.877	0.000
			[< 0.001]			–
Sex (%)	4.107	2.246	-0.106	1.423	1.423	0.000
			[< 0.001]			–
Other (%)	15.85	12.65	-0.092	8.004	8.004	0.000
			[< 0.001]			–
Any major offense (%)	39.16	26.19	-0.279	20.94	20.94	0.000
			[< 0.001]			–
Age in year of suspected offense	30.54	29.54	-0.177	29.56	29.56	0.000
	(5.628)	(5.709)	[< 0.001]	(5.732)	(5.732)	–
Male (%)	64.99	69.38	0.094	69.67	69.67	0.000
			[< 0.001]			–
Earnings (1000s SEK)	148.8	135	-0.089	142.8	140.8	-0.013
	(166.2)	(142.4)	[< 0.001]	(158.4)	(143.5)	[0.044]
Panel B: Other predetermined characteristics						
Swedish born (%)	69.04	71.77	0.060	73.13	72.82	-0.007
			[< 0.001]			[0.284]
Years of education	11.53	11.35	-0.089	11.53	11.42	-0.056
	(2.023)	(1.913)	[< 0.001]	(1.952)	(1.905)	[< 0.001]
Live in Stockholm (%)	33.54	29.39	-0.089	30.16	28.39	-0.038
			[< 0.001]			[< 0.001]
Live in Göteborg (%)	9.65	12.21	0.082	10.4	12.4	0.064
			[< 0.001]			[< 0.001]
Live in Malmö (%)	11.99	10.86	-0.035	11.99	10.84	-0.036
			[< 0.001]			[< 0.001]
Months employed during year	8.222	7.99	-0.045	8.138	8.128	-0.002
	(5.168)	(5.221)	[< 0.001]	(5.19)	(5.187)	[0.772]
Any months employed during year (%)	76.74	75.67	-0.025	76.16	76.29	0.003
	(42.25)	(42.9)	[< 0.001]	(42.61)	(42.53)	[0.643]
Tenure in main job (months)	37.09	34.77	-0.065	35.72	35.73	0.000
	(36.43)	(34.41)	[< 0.001]	(34.7)	(34.93)	[0.978]
Workplace size	329.4	320	-0.009	303.9	323.1	0.019
	(1008)	(987.8)	[0.071]	(946.3)	(990.4)	[0.009]
Firm size	5024	4624	-0.034	4431	4566	0.012
	(11682)	(11517)	[< 0.001]	(11015)	(11413)	[0.105]
Average firm coworker monthly wage (1000s SEK)	17.23	16.84	-0.045	16.83	17.04	0.025
	(8.833)	(8.247)	[< 0.001]	(8.365)	(8.273)	[0.001]
Firm pay premium at main job	-.05985	-.0582	0.006	-.06081	-.04986	0.040
	(.2768)	(.2733)	[0.230]	(.2764)	(.2686)	[< 0.001]
Firm coworkers with active records (%)	6.511	6.789	0.030	6.437	6.519	0.009
	(9.079)	(9.343)	[< 0.001]	(9.238)	(9.063)	[0.248]
Industry (%):						
Agriculture and resources	3.204	3.653	0.025	3.881	3.854	-0.001
			[< 0.001]			[0.862]
Care and social services	11.24	10.52	-0.023	9.653	10.08	0.014
			[< 0.001]			[0.048]
Construction	6.686	7.66	0.038	7.837	7.769	-0.003
			[< 0.001]			[0.749]
Education	5.528	4.549	-0.045	4.594	4.588	0.000
			[< 0.001]			[0.971]
Health care	3.962	3.474	-0.026	3.511	3.409	-0.005
			[< 0.001]			[0.459]
Hotels and restaurants	6.903	7.231	0.013	6.959	6.799	-0.006
			[0.011]			[0.413]
Manufacturing and industry	18.13	20.12	0.051	19.01	20.74	0.044
			[< 0.001]			[< 0.001]
Public administration and defense	3.773	2.484	-0.074	3.243	2.512	-0.042
			[< 0.001]			[< 0.001]
Retail and wholesale	13.85	14.82	0.028	14.71	15.21	0.015
			[< 0.001]			[0.065]
Services	24.93	23.57	-0.032	24.89	23.36	-0.036
			[< 0.001]			[< 0.001]
Unknown	1.78	1.898	0.009	1.681	1.665	-0.001
			[0.077]			[0.867]
Missing	.01646	.01255	-0.003	.03327	.006274	-0.022
			[0.502]			[0.095]
Panel C: Sanctions						
Fine or less (%)	–	59.25		–	59.14	
Probation and other (%)	–	31.99		–	33.28	
Prison (%)	–	8.767		–	7.581	
Observations	150,438	84,250		95,888	62,682	

This table describes the suspects and suspected offense combinations of people and years ("person-years") satisfying the sample criteria from section 5.2. The first two columns include all non-charged and charged suspects, respectively, and the third column reports the corresponding standardized difference. The fourth and fifth columns include matched non-charged and charged suspects, respectively, re-weighting non-charged suspects across cells to match the distribution among charged suspects. The sixth column reports the corresponding standardized difference. The construction of cells is described in section 5.2. Standardized differences are calculated by dividing the difference between the charged and non-charged means by the square root of the average of the two group variances. The same variances, calculated in the full unmatched sample, are used to standardize both comparisons. p-values for tests of equality between the charged and non-charged means in brackets, allowing observations for the same person to be correlated. The matched-sample p-values are omitted for exact matching variables balanced by construction. Panel A describes the suspected offenses in the event year and the other variables used to construct the matching cells. There can be multiple suspected offenses in an event, so crime types sum to more than 100 percent. The number of prior years with suspected offenses counts the earlier years in which a person was suspected of an offense. Panel B describes other characteristics of suspects in the year before the focal suspected offense. Panel C describes sentence type for charged suspects. Sentence type refers to the most serious suspected offense in the event year. See Appendix C for the construction of firm pay premiums.

offense types. 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.[29] We denote these cases as *major* offenses.[30] We categorize income by dividing suspects into quintiles within each combination of year and age at the time of the suspected offense, including suspects with zero earnings.

Appendix Table A.4 illustrates why our preferred matching procedure conditions on both suspected offense and suspect characteristics. Matching only on suspected offense characteristics produces broadly comparable groups but leaves residual differences in age, gender, and prior earnings. Because these characteristics may influence charging decisions and are strongly predictive of subsequent labor market trajectories,[31] our preferred procedure additionally matches on age, gender, and earnings in the year preceding the suspected offense.

We restrict our analysis to cells that contain both charged and non-charged suspects. While 56% of cells with at least one charged suspect contain both types of suspects, these cells account for 74% of charged suspects.[32] Columns 4 and 5 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. Overall, the matching procedure leads to a modest shift in the distribution of charged suspects included in the analysis, with relatively fewer traffic, narcotics, and major offenses. This shift is consistent with those offenses being more likely to lead to a charge.[33]

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

By construction, matched charged and non-charged suspects have identical suspected offense

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

[30] The five most common major offenses are: threat, fraud, vehicular theft, tax fraud, and fencing (receiving or dealing stolen goods). The five most common minor offenses (and most common offenses more generally) are: theft, non-DUI traffic (typically driving without a valid license), assault, narcotics use, and narcotics possession. For the top 10 major and minor offenses, Appendix Table A.2 shows the distribution of charge and sentencing outcomes. Even for offenses classified as major, defendants with no prior charges are rarely sentenced to prison.

[31] See Frederick and Stemen (2012a) and Starr (2015); for evidence from Sweden and the Netherlands, see Kardell and Bergqvist (2009) and Wermink et al. (2015), respectively.

[32] Appendix Table A.5 provides additional details on common support and retention by offense group.

[33] Appendix Figure A.6 compares labor market trajectories for matched and unmatched charged suspects by realized sanction. Unmatched charged suspects have lower employment and earnings before the suspected offense and experience larger declines afterward, consistent with their offenses being more serious on average.

characteristics. They also have more similar demographic and job characteristics, even on dimensions that we do not match directly; the share Swedish born and average months worked are now statistically indistinguishable.

5.3 Criminal record status and subsequent suspected offenses

Before presenting estimates for employment and earnings, we examine how criminal record status and subsequent suspected offenses evolve for the treatment and comparison groups.

We first look at the percentage of suspects with a criminal record. This is akin to a first stage. Panel A of Figure 2 shows the (weighted) percentage of charged and non-charged suspects with an active record over event time. By construction, no one in either group has an active record before the event. In the event year, the percentage with a record in the treatment group increases to 38%. This is below 100% because some suspects are not convicted in the year of the alleged offense.[34] 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.[35] In the following year, the share with a criminal record increases to 71% and eventually reaches 99% five years post-event. The percentage of people in the comparison group with a record also increases after the event, as some non-charged suspects subsequently face charges for other offenses. Five years post-event, 13% of the comparison group has an active record, 86 percentage points less than the treatment group.

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 criminal record may itself affect subsequent criminal justice contact. Prior US studies find that prosecution and noncarceral conviction increase subsequent complaints or charges (Agan et al., 2023; Humphries et al., 2025; Kamat et al., 2024), while Sweden's expungement rules may create incentives to avoid further offending.[36]

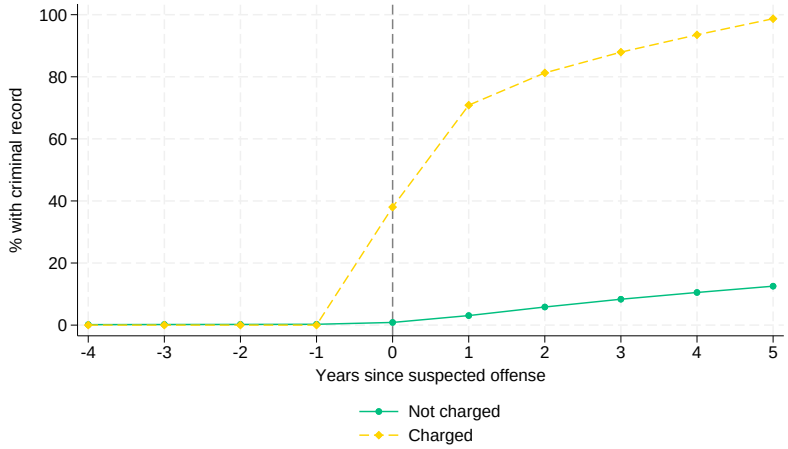
[34] The median and mean time between the suspected offense and conviction is 5 and 8.5 months.

[35] We do not exclude these offenses from the analysis because we cannot determine ex ante which suspected offenses, if charged, would ultimately result in only minor or no punishment.

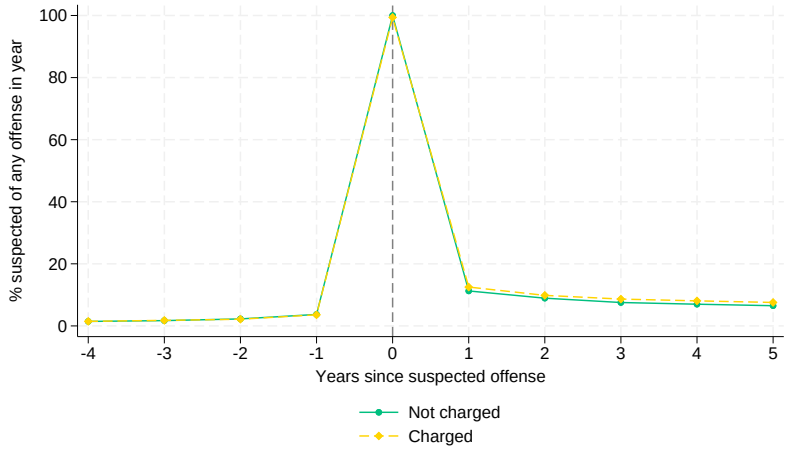
[36] Although incarcerated individuals make up a small fraction of our sample, a literature also studies the effect of prison on recidivism. Bhuller et al. (2020) present quasi-experimental evidence in Norway that incarceration decreases recidivism (i.e., future charges) for individuals who were not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

Figure 2
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.

The results are shown in panel B of Figure 2. By construction, the share suspected of an offense is 100% for both the treatment and comparison groups at event time $t = 0$. The groups are matched on their number of years with suspected offenses before the event, though not on the timing of those suspected offenses. In practice, the two groups are suspected of offenses at nearly identical rates by year before the focal event. For example, in the year before the focal event, 3.9% of both groups are suspected of an offense. Following the focal event, the treatment group is suspected of offenses at slightly higher rates. One year post-event, 12.6% of the treatment group and 11.1% of the comparison group are suspected of another offense; by five years post-event, these rates are 7.7% and 6.5%. This modest difference may reflect either a slightly higher baseline propensity to engage in crime in the treatment group or a modest increase in offending rates—or at least in rates of law enforcement suspicion—due to acquiring a criminal record.

5.4 Baseline results

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

We examine months employed in panel A and earnings in panel B of Figure 3. Although the treatment and comparison groups are only matched to have similar earnings in the year before the focal event, the two groups have virtually identical employment rates in each pre-event year. Average months employed increase from about 7.5 months four years before the event to 8.25 months in the year before the event. Interestingly, among non-charged suspects, the employment rate plateaus in the year following the event, and continues to increase thereafter. This plateau could reflect the effects of whatever circumstances led that group to be suspected of a crime in the first place. Charged suspects see a decrease in employment both in the year of the suspected offense and the following years. A gap between charged and non-charged suspects of about 0.2 months persists for at least four years, which is about 2% of employment in the comparison group. This in part reflects a difference in the share of people with significant unemployment spells. Charged suspects are 1.5 percentage points less likely to work more than 6 months in the year, compared to

a baseline of about 70% among non-charged suspects.

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

The timing of the labor market effects broadly tracks the timing of record acquisition. The treatment–comparison difference in record prevalence grows most sharply between the year before the suspected offense and the first year after. The estimated employment and earnings effects likewise increase in magnitude between event times 0 and 1 and remain broadly stable thereafter, as the gap in record prevalence widens only modestly.

We replicate our findings using event study models of the form

$$Y_{i\tau t} = \alpha_{i\tau} + \gamma_{c(i,\tau),t} + \sum_{j=-4}^{5} \lambda_j D_{i\tau t}^{j} + \epsilon_{i\tau t} \tag{2}$$

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

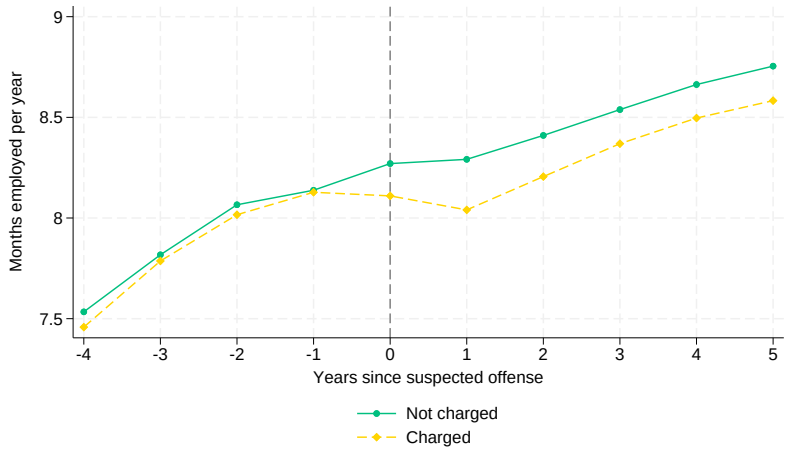
Figure 4 provides coefficient estimates ($\hat{\lambda}_j$) for months employed (panel A) and earnings (panel B). The same patterns emerge: five years post-event, months employed decrease by about 0.2 months and earnings decrease by about 10,000 SEK. Panels C and D shows analogous estimates using the imputation-based estimator of Borusyak et al. (2024), with pre-event coefficients close to zero and jointly insignificant.

5.5 Mechanisms: the role of the criminal record

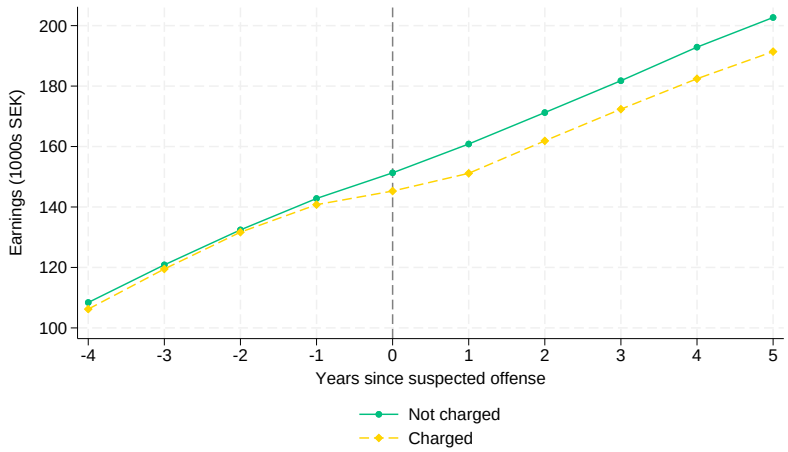
In this section, we examine whether the negative labor market effects of a first criminal charge that we document above are driven by the criminal record itself or from other contemporaneous channels, such as the sanctions imposed, job disruptions, or other contemporaneous shocks.

Figure 3
Acquiring a criminal record reduces employment and earnings

(A) Months employed



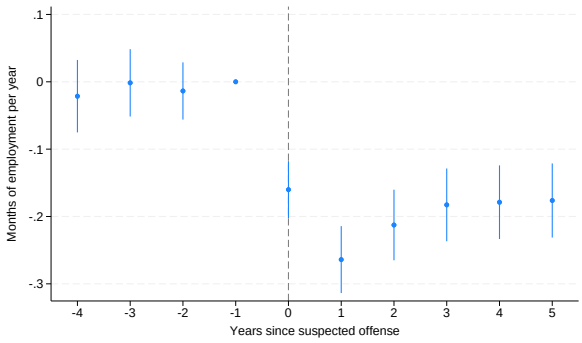
(B) Earnings



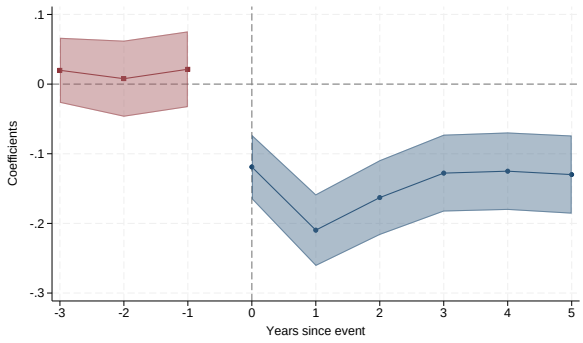
Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not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

Figure 4
Event study estimates for employment and earnings

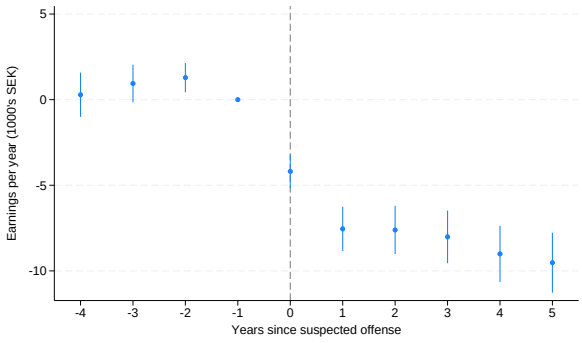
(A) Months employed



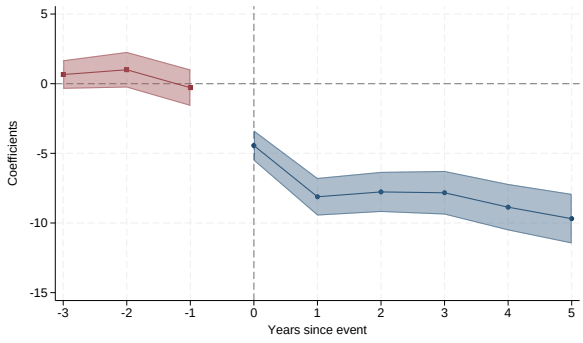
(B) Months employed (imputation approach)



(C) Earnings



(D) Earnings (imputation approach)



Note: This figure plots the event time coefficients (λ_j) from the event study specification (2). In panels A and B the outcome is months employed. In panels C and D the outcome is earnings. In panels B and D, the event time coefficients are plotted using the imputation-based event study methodology of Borusyak et al. (2024). Standard errors are clustered at the person level.

5.5.1 Using the 2000 record reform as a placebo test

We first use the 2000 criminal record reform to test whether the labor market effects we document are driven by the record itself. Before 2000, criminal offenses that received no punishment (AUL, *åtalsunderlåttelse*), a ticket issued by the police (*ordningsbo*t), a fixed-fee fine (*penningbot*), or an income-related fine (*dagsböte*r), did not produce a criminal record. Starting in 2000, these lesser offenses did generate a criminal record that could be expunged after five years. The reform was retroactive: charges from 1995–1999 became visible in 2000. This retroactive change led to a discrete increase in the prevalence of WCRs in 2000; as seen in Figure A.4, the share of men with an active record jumped by more than 35%.

The reform provides a placebo test of our matched charged versus non-charged suspects design. We use this design to estimate the effect of a minor charge and sanction both with and without an ensuing criminal record. We focus our attention on those who are suspected between 1996 - 1998 and are later charged, but received (at most) a ticket or a fine.[37] These pre-reform cohorts had no visible criminal record for the first year after the date of their suspicion (i.e., at event times $\tau = 0$ and 1). They do, however, obtain visible criminal records in 2000, which means that they are either partially or fully treated at event times $\tau = 2$, 3, 4, and 5. By contrast, those who are suspected and charged after the reform receive a visible record as soon as a determination of guilt is made, typically at $\tau = 0$ or 1.

If the record is the operative channel (rather than the sanction or coincident shocks), then we expect: (i) no effects on employment or earnings at event times $\tau = 0$ and 1 for those suspected in 1996 - 1998 of a minor offense, since they have no visible criminal record; (ii) possible negative effects at $\tau = 2$, 3, 4, and 5 after their record appears; (iii) negative effects at all event times for cohorts suspected and charged of a minor offense in 2000 or later, since for these cohorts the record turns on immediately.

To test these hypotheses, we compare the effects for pre-reform cohorts suspected in 1996–1998 to post-reform cohorts suspected in 2000–2010. We also distinguish between those whose first suspicion leads to a charge for a minor offense versus those whose first suspicion leads to a charge for a major offense. Even for pre-reform cohorts, there should be an immediate effect associated with major offenses, since these offense types were not subject to the reform and had records turn

[37] Most of those suspected for the first time in 1995 have records that turn off at some point during 2000 due to expungement rules. Because their records are retroactively turned on for less than a year, we exclude this cohort from the exercise. Many of those suspected for the first time in 1999 are not formally charged until 2000; because they are thus not retroactively treated by the reform, we exclude this cohort from the exercise.

on immediately.

The first row of Figure 5, panels A.1 - D.1, illustrates the first stage effect of the reform by showing the shares of charged and non charged suspects with visible criminal records. For our focal group — the 1996-1998 pre-reform cohorts charged with a minor offense — panel A.1 shows that records only start turning on *two years after* the suspicion (about 30%) and that all individuals have a record by year four. The evolution of this share is strikingly different from those suspected and charged with a minor offense after the reform; as seen in panel C.1, nearly 60% have a record that turns on immediately. Similarly, individuals suspected of major offenses in our pre-reform and post-reform cohorts (panels B.1 and D.1, respectively) have a substantial share (around 30%) of records that turn on immediately.

The second row of Figure 5, panels A.2 - D.2, plots the event time coefficients (λ_j) for earnings from our baseline event study specification (2). The key takeaway from this exercise can be seen in panel A.2. Earnings of the pre-reform cohorts do not immediately respond to being charged with a minor offense. Instead, they only start to react at event times $\tau >= 2$, which is when their criminal record is made visible by the reform (as seen in panel A.1). In sharp contrast to this, panel C.2 shows that the earnings of the post-reform cohorts charged with a minor offense begin to fall immediately, since their criminal records are made visible immediately.

The third row of Figure 5, panels A.3 - D.3, plots the event time coefficients for months employed. Here, we see no meaningful effects for the pre-reform cohorts in panel A.3. These null effects are, however, quite imprecise. These null effects are also in sharp contrast to the post-reform cohorts (panel C.3), for whom there is an immediate effect on months employed.

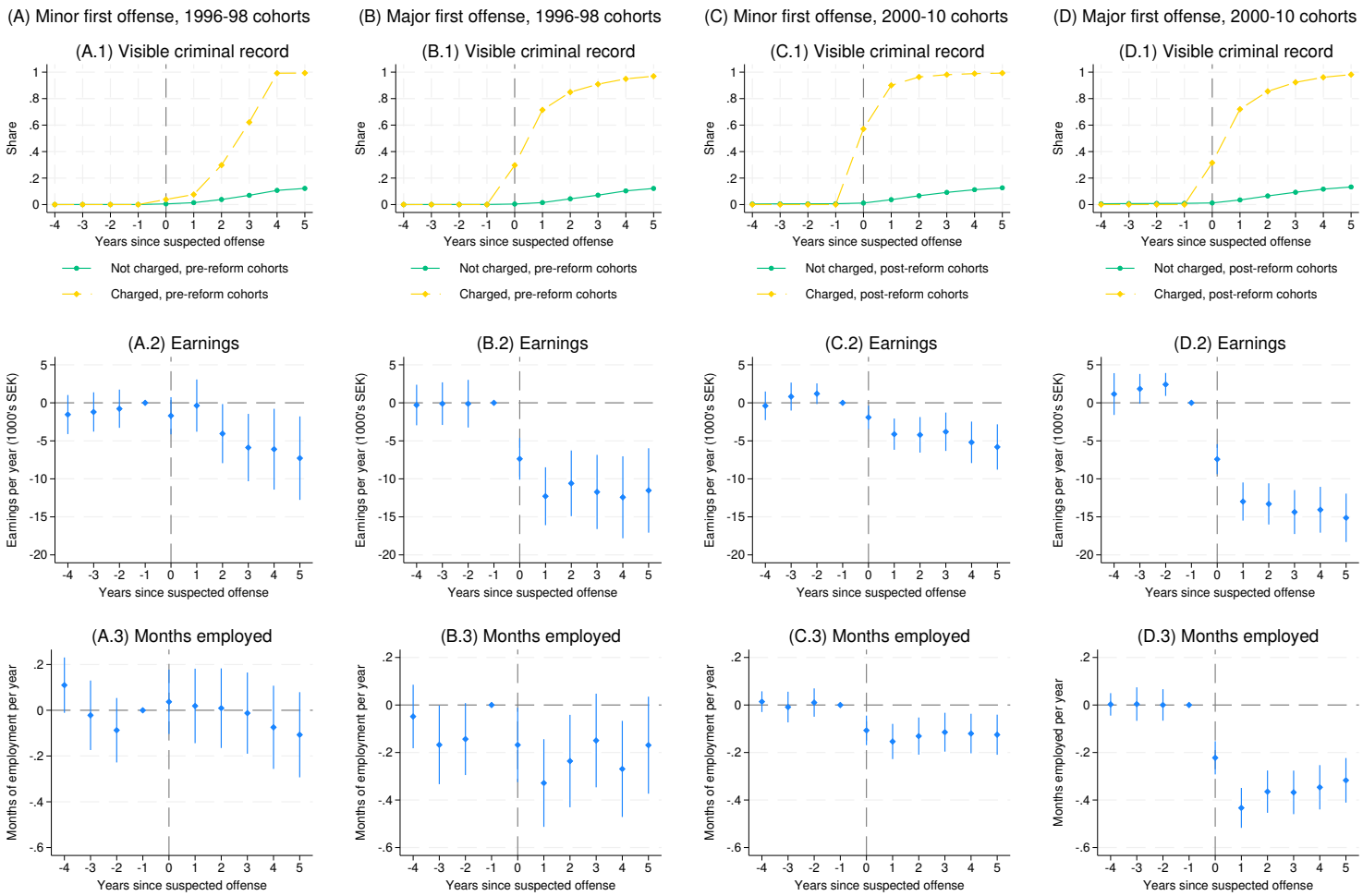
Finally, the effects on earnings and employment of being charged for a major offense do not differ across pre- and post-reform cohorts, as seen in the B and D panels. For both groups, the effects are immediate (consistent with records turning on immediately) and the magnitudes are quite similar.

In sum, these results support the interpretation that the negative effects of a first charge are mediated by the presence of a criminal record rather than by the sanction itself (i.e., the fine) or other contemporaneous channels. The absence of an effect of fines is consistent with recent U.S. evidence that criminal financial sanctions do not have economically meaningful effects on labor market outcomes (Finlay et al., 2024).[38]

[38] In contrast, Mello (2025) finds that speeding fines reduce employment in the U.S.

Figure 5

The 2000 criminal record reform: When criminal records become visible earnings and employment go down



Note: The main results from this exercise are reported in column A. Earnings of pre-reform cohorts only go down after their criminal records become visible to employers. The first row in this figure, panels A.1 - D.1, shows the share of charged and not charged with visible criminal records. Those charged with minor offenses receive no criminal record before the year 2000. Those charged with minor offenses in 2000 or after receive visible criminal records as soon as they are convicted. Those charged with major offenses receive visible criminal records both before and after 2000 as soon as they are convicted. Row 2, panels A.2 - D.2, plots the event time coefficients (λ_j) for earnings from the event study specification (2). Row 3, panels A.3 - D.3, report event time coefficients for months employed. Standard errors are clustered at the person level.

5.5.2 The role of incarceration

The registry reform exercise directly isolates the role of record visibility only for offenses minor enough to be affected by the reform. In our matched sample, nearly 60% of charged suspects receive a fine or less, while fewer than 8% are sentenced to prison. Moreover, prison sentences are relatively short, with a median duration of 60 days. Mechanical incapacitation can therefore affect only a small share of the sample and for a limited period. Existing evidence from Norway (Bhuller et al., 2020) and Sweden (Hjalmarsson and Lindquist, 2022) also indicates that incarceration does not produce persistent labor market scarring and may even improve subsequent outcomes.[39] Together, these facts suggest that incarceration is unlikely to account for much of the pooled and persistent labor market penalty we estimate.[40]

We also repeat the baseline analysis after excluding charged suspects who received a prison sentence for the focal offense. Appendix Figures A.9 and A.10 report the corresponding outcome trajectories and event study estimates. The negative employment and earnings effects persist in this sample, consistent with a limited role for incarceration in explaining the pooled results.

Appendix Figure A.6 provides complementary descriptive evidence. Among charged suspects in the matched sample, the post-event drops in employment and earnings are similar for those sentenced to probation and prison, even though only the latter are incapacitated by imprisonment. Although these comparisons do not isolate causal effects by sentence, they provide little indication that incarceration is the primary source of the pooled employment and earnings effects we document.

A first charge could also reduce earnings by increasing incarceration for subsequent offenses, in part because sentences depend on both current offense severity and criminal history. Appendix Figure A.11 plots cumulative prison sentence days for charged and matched non-charged suspects. Most of the gap emerges by the first year after the focal suspected offense, when it reaches approximately 12 days; by five years after the event, it is approximately 16 days. This modest subsequent widening suggests that additional prison sentences in later years have limited scope to explain the persistent earnings drop.

[39] Recent US evidence similarly finds that relatively long prison sentences do not have persistent labor market scarring effects (Garin et al., 2023).

[40] Mueller-Smith and Schnepel (2021) similarly conclude that incarceration does not play a significant role in explaining the labor market improving effects of diversion from felony charges in the US.

5.5.3 The role of adjudication-related job disruptions

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.[41] This induced job displacement may have negative long-term labor market consequences that are separate from the effects of a record per se.

The registry reform provides direct evidence on adjudication-related job disruptions. For the affected minor offenses, charged suspects experienced the adjudication process and received sanctions both before and after the reform, but the charge produced an immediately visible record only after the reform. The absence of short-run labor market effects before records became visible therefore argues against adjudication-induced job disruption as the explanation for the effects of these minor charges.

There is additional reason to be skeptical that job disruptions play an important role here. While prior research from Sweden finds that job displacement due to mass layoffs or plant closures reduces earnings by about 10% five years after displacement (Bertheau et al., 2023; Athey et al., 2024), both charged and non-charged suspects experience high job turnover: five years after the suspected offense, only 24.6% of non-charged suspects and 23.5% of charged suspects remain with their pre-offense employer. Given this small difference in job turnover between charged and non-charged suspects, there is limited scope for job loss to explain the earnings decline we observe.

We split the sample by employment status at baseline to clarify the role of job disruptions. We estimate the event study specification (2) separately for each subsample. In Table 5 we report the five year post-event coefficients for both months employed (panel A) and earnings (panel B).[42] Sixty-six percent of charged suspects are working in the month of their suspected offense. If suspects with and without jobs both experience significant negative effects when charged, that suggests the role of job disruptions is limited.

Columns 1 and 2 of Table 5 present the results. As expected, those working at baseline have higher earnings. Yet, in proportional terms, the effects of acquiring a record are similar for both groups. We conclude that the effects we measure are not driven primarily by job disruptions.

Finally, we highlight that acquiring a criminal record does not by itself provide Swedish em-

[41] 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.

[42] The remaining coefficients are reported in Appendix Tables A.6 and A.8 and depicted in Appendix Figure A.8.

ployers with grounds to terminate existing employees. During our study period, the Swedish Employment Protection Act (LAS, 1982:80, 7 §) required dismissals to be based on objective grounds (*saklig grund*) and, in circumstances where the offense affects the individual's ability to perform the job or trust to do the position, required employers to consider job reassignment before dismissal.[43]

Section 6 also provides complementary evidence on the underlying mechanism. Acquiring a record shifts employment away from firms independently classified as having a low propensity to hire WCRs and toward firms with a higher propensity to hire them. Incapacitation or adjudication-related disruption could reduce employment overall, but would not naturally generate reallocation in this direction across firms defined by their relative hiring of WCRs.

5.6 Heterogeneous effects by offense type

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

We first split the sample by whether the set of suspected offenses includes any major offense—about 20% of events. Appendix Figure A.7 presents the full event-study estimates using the imputation-based estimator of Borusyak et al. (2024). The pre-event coefficients are close to zero for both groups. After the suspected offense, employment and earnings decline for both groups, with substantially larger effects for major offenses.[44] Columns 3 and 4 of Table 5 report the five-year post-event coefficients. Months employed decrease by 0.13 for minor offenses and 0.30 for major offenses. Relative to the corresponding comparison-group means, these estimates represent declines of approximately 2% and 4%, respectively. Earnings effects follow a similar pattern, with declines of approximately 4% for minor offenses and 8% for major offenses. This gradient is consistent with audit study evidence that firms respond more negatively to more severe offenses (Pager, 2003; Uggen et al., 2014) and survey experimental evidence that show that stated willingness to hire is lower for serious felony offenses than misdemeanors (Agan et al., 2024b; Bushway and Pickett, 2024).

Columns 5 through 8 of Table 5 provide event study estimates for employment and earnings

[43] Swedish Labor Court (Arbetsdomstolen or AD) decisions illustrate how these laws are applied. For example, in AD 2001 nr 60, the Court held that a criminal conviction rendered a security guard at the Swedish central bank unsuitable for his existing position, but nevertheless invalidated his dismissal because the offense was unrelated to his employment and the employer could reasonably reassign him to another available position. Other decisions similarly emphasize that criminal conduct outside work does not automatically constitute objective grounds for dismissal and that the assessment depends on the relationship between the offense and the employment and the possibility of reassignment (see, e.g., AD 1981 nr 51; AD 2001 nr 60; AD 2002 nr 26; AD 2004 nr 30).

[44] This pattern—larger effects for major offenses—was already visible in the reform analysis presented in Figure 5.

Table 5

Heterogeneity in the labor market effects of a charged offense

	By baseline status		By offense severity		By offense type			
	Unemployed (1)	Employed (2)	Minor (3)	Major (4)	Violent (5)	Property (6)	Narcotics (7)	Traffic (8)
Panel A: Months employed								
λ_5	-0.07	-0.18	-0.13	-0.30	-0.17	-0.14	-0.66	-0.22
	(0.06)	(0.03)	(0.03)	(0.06)	(0.05)	(0.05)	(0.13)	(0.07)
Non-charged outcome mean	3.76	10.30	8.55	7.86	8.43	7.82	8.14	9.23
Panel B: Earnings								
λ_5	-4.00	-10.90	-7.83	-14.31	-8.85	-7.64	-24.88	-9.23
	(1.43)	(1.16)	(1.01)	(1.88)	(1.40)	(1.44)	(3.62)	(2.67)
Non-charged outcome mean	50.84	213.16	168.99	152.30	165.23	142.66	139.65	200.00
Observations	449150	1063670	1165480	420220	597120	579170	71550	217680

This table reports event time coefficient estimates ($\hat{\lambda}_j$) from the event study specification (2), where the specification is estimated separately for various subsamples of events. Columns 1 and 2 split the sample by employment status in the month before the suspected offense. Columns 3 and 4 split the sample by whether the set of suspected offenses includes any major offense. Columns 5 through 8 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Non-charged outcome means are calculated over the full event study window.

effects when splitting the sample by offense type. We limit to events that include at least one of the following offense types: violent, property, traffic, and narcotics. Note that events including multiple types of suspected offenses are included in multiple samples.

Coefficient estimates, especially relative to the respective means, are similar for violent, property, and traffic offenses (e.g., driving while intoxicated). This pattern suggests that the effects are driven primarily by the criminal record itself rather than by offense-specific sanctions or restrictions, such as the loss of a driver's license following a major traffic offense. The main exception is narcotics offenses, for which the estimated effects are substantially larger. In this subsample, a charge decreases earnings by 20,000 to 25,000 SEK. One possible explanation is that employers may treat narcotics offenses as particularly salient when evaluating job applicants, perhaps because they view such offenses as more predictive of future misconduct or poor job performance. Consistent with this interpretation, our job ads data (Appendix Table A.11) show that 21% of firms that mention background checks also mention drug or alcohol testing, compared with only 1% of firms that do not mention background checks. Another, not mutually exclusive, potential explanation is emphasis in Swedish policy and public debate during the 1990s and 2000s on achieving a "drug-free society" through a zero-tolerance approach and message that aimed to not only deter drug use but also signal strong societal disapproval. Two selected (author-translated) passages from a 1990s Swedish government drug policy report (Regeringen, 1998, p.27 and 11) illustrate these principles: "The guiding principle of drug-control policy is that society's collective efforts should send a clear and unambiguous message that all non-medical use of drugs is unacceptable" and "The message that drugs are not accepted in society must be widely known and firmly established."[45]

Appendix Tables A.7 and A.9 report results for additional subsamples by suspect age, gender, Swedish born status, and educational attainment, and by time period. Similar proportional effects are seen for all subsamples, particularly for earnings.

5.7 Effects of a second charge

To this point, we have focused on the first time a person is charged with an offense, when that person is at the margin of having any criminal record. We also apply the same research design to estimate the effect of being charged a second time. We provide further details on this exercise in

[45]Past surveys of US employers find that stated willingness to hire applicants with criminal records varies by offense type, with greater aversion to violent offenses than to drug or property offenses (Holzer et al., 2007; Bushway and Pickett, 2024). Because our estimates capture the total labor market effects of acquiring a record rather than employer preferences alone, the pattern across offense types need not mirror these findings on stated preferences. Moreover, employer views may differ in Sweden given the country's drug-free ambitions.

Appendix E. As shown in Appendix Table E.18, second charged offenses in the matched sample are similar to first charged offenses, including in the share classified as major. Match support is substantially thinner, however: cells containing both charged and non-charged suspects account for only 28% of charged suspects in the second-charge analysis, compared with 74% in the first-charge analysis. Within this more selected sample, we find effects on months employed (5%) and earnings (10%) that are about twice as large as those following a first charge. Caution is warranted in comparing the two sets of estimates because the analyses concern different populations and treatment margins, and the second-charge estimates apply to a more limited subset of charged suspects. The larger estimated effects of a second charge could reflect the addition of another offense to the criminal record, an extension of the record's visibility under Sweden's expungement rules, or other differences between the samples.

5.8 Expungement

The results thus far show a labor market penalty of a criminal record that persists for five years. But, as described in section 2.3, records in Sweden are automatically expunged after a pre-specified number of years, often five and ten. In Appendix F we conduct two exercises to examine whether expungement mitigates the earnings and employment penalties associated with a record. First, we see when using the charged versus non-charged suspects design with an extended horizon that the earnings gap between charged and non-charged suspects persists for over a decade, after most offenses are expunged. Second, for a wide range of subsamples defined by demographics, record length, and offense types, we show descriptively that labor market outcomes do not improve in the years following expungement. Thus, consistent with analyses of Clean Slate laws in the US (Agan et al., 2024a) and New Zealand (Dasgupta et al., 2025), we find minimal effects of record expungement. One leading explanation for the limited effects of expungement is that the labor market consequences of a record are *scarring* (Agan et al., 2024a): earlier employment losses and sorting into lower-quality jobs create résumé gaps, limit human capital accumulation, and place workers on persistently worse career trajectories even after the record itself is no longer visible.

5.9 Alternative within-group research design

Our research design leverages data on non-charged suspects as a comparison group. While we believe this approach has several appealing features, one limitation is that data on non-charged suspects are typically unavailable, making the design less broadly applicable. We explore an al-

ternative event study specification used in prior research that relies only on data for people who are eventually charged (Grogger, 1995; Rose, 2021; Agan et al., 2024b). This alternative design effectively compares outcomes for those charged at younger versus older ages.

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

There are two main takeaways. First, the alternative designs yields clear pre-trends, especially in employment, prior to the charge. This pattern cautions against a causal interpretation.

Second, non-charged suspects experience similar pre-trends and also experience decreases in employment and earnings following the suspicion, though smaller in magnitude. This highlights the advantages of incorporating non-charged suspects as a comparison group, which accounts for the circumstances that prompted police suspicion.

6 The role of firm heterogeneity

A central goal of this paper is to document whether and how the negative employment and earnings effects of a criminal record are mediated by employer heterogeneity. In this section, we ask whether acquiring a record causes WCRs to reallocate across employers—for example, from more to less discriminatory firms or non-employment—and how any decreases in monthly earnings are distributed across employers. Specifically, we allow the suspect versus charge event-study design estimates from the previous section to depend on the firm's 'type'.

We do this in two steps. First we characterize firms by how a candidate's criminal record affects the chances that the firm hires them. We interpret this as a measure of how discriminatory or "WCR-friendly" an employer is. Second, we examine how the overall effects of acquiring a record on months employed, annual earnings, and monthly earnings are distributed across employer types.

6.1 Classifying firms

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

More concretely, we use data on new hires between 1995 and 2015.[46] We divide workers randomly into two groups. We use the first group to classify firms and the second group later to measure the heterogeneous effects of a criminal record by firm type. For each hire i at time t, we calculate their propensity score for having a criminal record that year based on their suspected event history. We denote this propensity by $P(\text{RECORD})_{it}$. To construct $P(\text{RECORD})_{it}$, we estimate a probit model for whether a suspect is charged in each year they were suspected of a crime, focusing on the most serious suspected offense each year. We predict charges based on age, gender, Swedish born status, suspected offense history, and suspected crime type. We then carry this probability of a charge forward for the number of years that a suspect charged of said offense would have had a record (according to the expungement rules). We refer to the set of hires where $P(\text{RECORD})_{it} > 0$ as *at-risk hires*. Among all at-risk hires, 56% have a criminal record and the average value of $P(\text{RECORD})_{it}$ is 0.56.[47]

Our goal is to measure firm-level tendencies to hire WCRs conditional on their hires' risk of having a criminal record. For each at-risk hire (i, t) we measure $\Delta_{it} = \text{RECORD}_{it} - P(\text{RECORD})_{it}$, the difference between the hire's realized record status and predicted probability of having a record. We restrict the analysis to at-risk hires so that the firm measure parallels our charged-versus-non-charged suspect design.[48] We demean the Δ_{it} values to have mean zero in each year. We define the firm measure as

$$\theta_j = E[\Delta_{it}|J(i,t) = j] = E[\text{RECORD}_{it} - P(\text{RECORD})_{it}|J(i,t) = j; P(\text{RECORD})_{it} > 0]. \quad (3)$$

where the expectation is taken over workers hired by firm j ($J(i,t) = j$). Most firms make few at-risk hires, so statistical noise is an important concern when classifying firms.[49] A firm that makes only a handful of at-risk hires may hire few WCRs by chance alone, even if they do not put weight on criminal records per se when hiring. To address this issue, we use an empirical Bayes (EB) approach (Walters, 2024). We construct a prior for each firm using a random forest trained with five features: 1-digit industry, 2-digit industry, number of employees, number of at-risk hires, and the proportion of their employees with criminal records. We estimate the sample analog of θ_j, $\hat{\theta}_j$, and take a weighted average of the same estimate and the prior to construct our EB measure,

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

[47] See Appendix H for more details on how $P(RECORD)_{it}$ is calculated.

[48] By construction, $P(\text{RECORD})_{it} = 0$ implies $\text{RECORD}_{it} = 0$, and therefore $\Delta_{it} = 0$. Including these hires would add only non-suspects who provide no variation in record status.

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

$\hat{\theta}_j^{EB}$. We put more weight on the prior when the sample estimate $\hat{\theta}_j$ is less precise. More details on our empirical Bayes procedure are provided in Appendix I.

We then categorize firms as follows. We take the non-charged suspects in the matched sample from section 5.2, limiting to the half of the sample *not* used to construct $\hat{\theta}_j^{EB}$, and look at the firms where they work during the event study window. We divide these firms into terciles based on their value of $\hat{\theta}_j^{EB}$, using the event-level weights used for matching described in section 5.2.[50] We label these firm terciles as "low propensity", "medium propensity", and "high propensity", where propensity refers to how often a firm hires WCRs relative to other at-risk hires. Table 6 describes the workplace characteristics of non-charged suspects at these three categories of firms, weighted by person-years.

The average values of adjusted firm WCR hiring propensities ($\hat{\theta}_j^{EB}$), reported in percentage points, are -5.09, -0.69, and 3.33 at low, medium, and high propensity firms, indicating that when medium and high propensity firms hire at-risk workers, they are 4.4 and 8.4 percentage points more likely to hire WCRs than low propensity firms, conditional on worker risk. The percentage of coworkers with criminal records is 3.1% at low propensity firms, 5.4% at medium propensity firms, and 11.7% at high propensity firms. Firm pay premiums decrease monotonically from low- to medium- to high-propensity firms. Compared to about 40% of low and medium propensity firms, only 3% of high propensity firms are in the public sector. Table 6 also shows that firms with a relatively high propensity to hire workers with criminal records tend to be smaller and disproportionately in the manufacturing and industry, construction, and hotels and restaurants sectors. These sectoral differences are consistent with Swedish survey evidence on which firms conduct background checks (Arbetsmarknadsdepartementet, 2019) and US surveys on stated willingness to hire WCRs (e.g., Holzer et al., 2002). Importantly, however, we do see low, medium, and high propensity firms within almost all industries.

These differences in firm hiring propensities should be interpreted descriptively. Our measure is not intended to capture what share of workers with criminal records a firm should hire, nor does it identify the specific sources of firm-level differences in hiring patterns. Such variation could reflect differences in screening ability, risk assessment, the organization of production (e.g., the degree of monitoring), or bias. Our aim is to document that this firm-level heterogeneity exists and to show that it plays an important role in shaping the labor market consequences of a criminal record.

[50] Firms with no such hires are left unclassified.

TABLE 6
FIRM DESCRIPTIVE STATISTICS BY TERCILE

	Firm WCR hiring propensity		
	Low	Medium	High
Firm size	6084.1	7441.5	861.7
	(11457.9)	(14486.1)	(2918.0)
Avg. firm coworker monthly wage (1000s SEK)	18163.4	15061.6	15188.7
	(10487.5)	(7674.2)	(8822.0)
% of firm coworkers with active records	3.070	5.380	11.65
	(3.762)	(3.672)	(11.24)
Adjusted firm propensity ($100 \times \hat{\theta}_j^{EB}$)	-5.089	-0.694	3.328
	(5.314)	(0.555)	(2.991)
Firm pay premium	-0.0284	-0.0867	-0.103
	(0.261)	(0.232)	(0.309)
Log value added per worker	12.44	12.16	12.25
	(1.091)	(1.222)	(1.002)
Tenure (in months)	43.09	39.49	38.18
	(40.09)	(37.64)	(36.84)
Annual separation rate	21.26	23.90	25.70
	(40.91)	(42.65)	(43.70)
Public sector (%)	38.87	37.79	3.356
Firm industry:			
Agriculture and resources	1.341	1.829	4.545
Manufacturing and industry	9.770	14.89	35.06
Construction	2.067	4.384	12.18
Retail and wholesale	13.36	10.74	10.00
Hotels and restaurants	4.588	8.136	11.10
Education	5.754	16.62	1.360
Health care	21.34	23.22	1.305
Public administration and defense	7.497	2.129	0.289
Services	30.84	16.19	22.42
N firms	16,725	9,240	31,814

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

6.2 Decomposing the effect of a record by firm type

6.2.1 Employment and earnings changes by firm type

To examine how the overall employment and earnings effects are distributed across firm types, we retain the comparison of matched charged and non-charged suspects and re-estimate the same event study specification (2), replacing the outcome with MONTHS EMPLOYED$_{i\tau t}$ or EARNINGS$_{i\tau t}$ *measured at firms in a given propensity tercile*. The coefficients can be interpreted as the employment or earnings losses (or gains) at a specific set of firms, unconditional on where people are working. For example, a criminal record could reduce earnings at a set of firms either because it decreases the chances of working at those firms, or decreases earnings conditional on working there. We estimate this model using the randomly selected half of workers that were *not* used to estimate $\hat{\theta}_j^{EB}$.

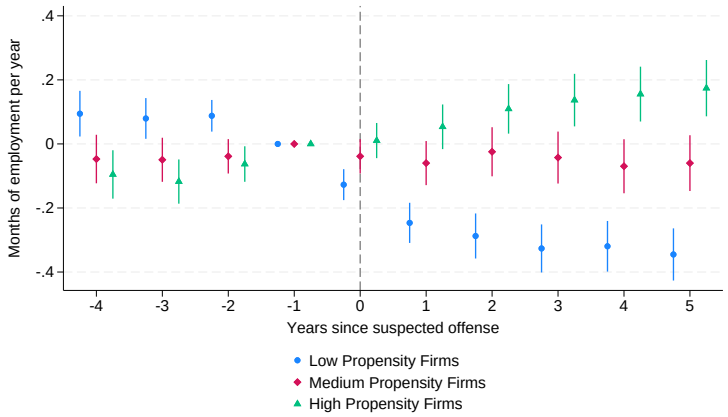
Event study estimates for each tercile are shown in Figure 6. The coefficients are additive: their sum across terciles roughly equals the total earnings effect, except for the fact that some small firms are not classified.

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

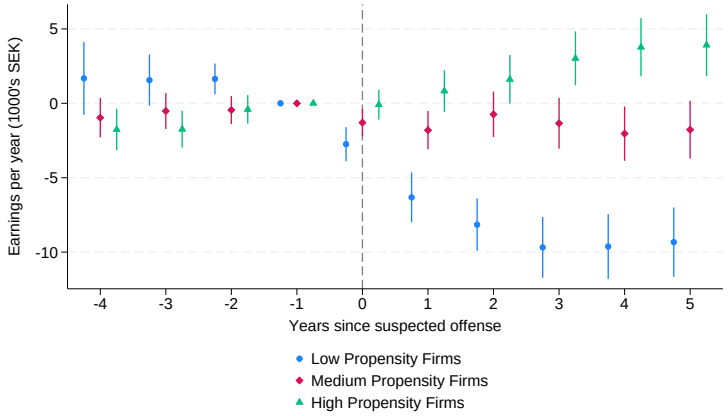
In the year immediately preceding the suspected offense, months employed and earnings decrease modestly at low-propensity firms and increase modestly at high-propensity firms. The post-event reallocation is substantially larger and persists throughout the follow-up period. Moreover, the pre-event pattern is not evident among major offenses, even though the subsequent shifts away from low-propensity firms and toward high-propensity firms are more pronounced (see Appendix Figure A.12). Taken together, these patterns support interpreting the post-event reallocation as a causal effect of acquiring a criminal record rather than as a continuation of differential sorting that predates the focal offense.

Figure 6
Employment and earnings losses are concentrated at low-propensity firms

(A) Months employed



(B) Earnings



Note: This figure plots the event time coefficients ($\hat{\lambda}_j$) from the event study specification (2) for months employed (panel A) and earnings (panel B) interacted with indicators for three firm types: low propensity, medium propensity, and high propensity. See section 6.2 for a description of how firm types are constructed.

6.2.2 Monthly earnings changes by firm type

We find that a criminal record decreases both months employed and annual earnings, and reallocates workers from more to less discriminatory firms. Figure 6 examines employment and annual earnings at firms of each type, combining changes in where suspects work with changes in earnings conditional on employment. In this section, we focus instead on monthly earnings among employed workers. This outcome depends on both wages and hours worked. We are particularly interested in the extent to which the effect on monthly earnings (1) reflects between-firm reallocation versus within-firm pay changes and (2) vary by firm type.

We begin with the event study sample and focus on each person's main job in a given year.[51] We simplify our research design by comparing suspects' observed monthly earnings before and after their suspected offense. We estimate regressions of the form

$$\log(\text{MONTHLY EARNINGS})_{i\tau t} = \beta \text{POST}_{i\tau t} + \gamma_{tg(i,\tau)} + \alpha_{i\tau} + \psi_{J(i,t)} + \epsilon_{i\tau t} \tag{4}$$

where $\text{POST}_{i\tau t}$ is an indicator for whether the suspect is charged and $t \geq \tau$, $\alpha_{i\tau}$ are person-event fixed effects, $\psi_{J(i,t)}$ are firm fixed effects, $g(i,\tau)$ groups events by the year of the offense (τ) and the suspect's age at the time of the suspected offense, and $\gamma_{tg(i,\tau)}$ are group by year fixed effects. We estimate versions of equation (4) with and without both firm fixed effects and interactions between $\text{POST}_{i\tau t}$ and the firm type. We limit the estimation sample to the half of the sample *not* used to estimate $\hat{\theta}_j^{EB}$.

The results are shown in Table 7. Column 1 excludes firm fixed effects. The coefficient on $\text{POST}_{i\tau t}$, -0.018, indicates that a criminal record reduces monthly earnings by 1.8 log points. Including firm fixed effects in column 2 reduces the coefficient to -0.012, indicating that about half a log point of the decrease in monthly earnings reflects a reallocation to lower-paying firms. The remaining decrease represents within-firm pay reductions. In column 3, we interact $\text{POST}_{i\tau t}$ with indicators for our three firm types: low, medium, and high propensity firms. Monthly earnings decrease by 3% at high propensity firms, while changes at low and medium propensity firms are smaller in magnitude and not statistically significant. The negligible effect at low propensity firms in part reflects the fact that those who are charged but manage to find employment at these firms are suspected of less serious offenses. If we restrict the analysis to those suspected of major offenses,

[51] As in section 4.2, we restrict the analysis to jobs held for at least two months in that year, with monthly average earnings of at least 2,000 SEK

Table 7
Effects on monthly earnings and firm heterogeneity

	All offenses			Major offenses		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-0.018	-0.012		-0.035	-0.014	
	(0.004)	(0.004)		(0.009)	(0.010)	
Low Propensity Firm × POST			0.004			-0.012
			(0.006)			(0.016)
Medium Propensity Firm × POST			-0.006			-0.001
			(0.006)			(0.014)
High Propensity Firm × POST			-0.031			-0.030
			(0.005)			(0.013)
Firm FEs		✓	✓		✓	✓
Observations	457,190	457,190	457,190	108,430	108,430	108,430

This table shows estimates for the specification (4), where the outcome is log monthly earnings. We focus on monthly earnings from an individual's main job, defined as the employer from whom they earned the most in a given year. We restrict the analysis to jobs held for at least two months in that year, with average monthly earnings of at least 2,000 SEK. All columns include person-event fixed effects and fixed effects for the combination of the year, the year of the suspected offense, and the suspect's age at the time of the suspected offense. Columns 1–3 include all offenses, while columns 4–6 restrict to major offenses. Columns 2, 3, 5, and 6 include firm fixed effects. Columns 3 and 6 interact $\text{POST}_{i\tau t}$ with fixed effects for the three firm categories defined in section 6.2: low, medium, and high propensity firms.

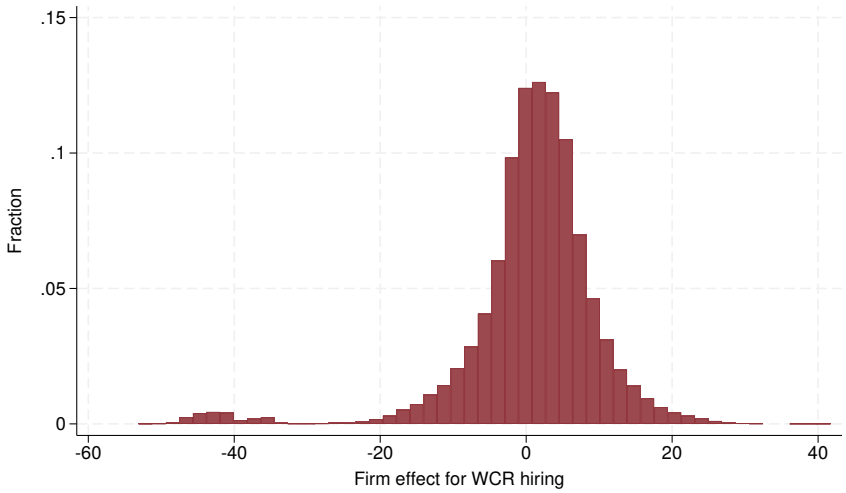
effects on monthly earnings are larger at low propensity firms (see columns 4–6).

In Appendix J, we decompose the earnings decline caused by acquiring a record into three components: (1) reduced months of employment; (2) shifts toward firms with lower average coworker earnings; and (3) lower earnings conditional on employment duration and this firm measure. We attribute roughly 40% of the earnings decline to fewer months employed and 20% to shifts toward firms with lower average coworker earnings. The remaining 40% may reflect within-firm changes in monthly earnings as well as between-firm reallocation along dimensions not summarized by average coworker earnings.

6.3 Characterizing firm heterogeneity

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

FIGURE 7
DISTRIBUTION OF ADJUSTED FIRM WCR HIRING PROPENSITIES ($\hat{\theta}_j^{EB}$)



Note: This figure plots the distribution of firm effects for WCR hiring ($\hat{\theta}_j^{EB}$). See section 6.2 for a description of how these firm effects are constructed.

We first describe the distribution of $\hat{\theta}_j^{EB}$ across firms.[52] Figure 7 plots the distribution of $\hat{\theta}_j^{EB}$ across all firms with at least one at-risk hire. There is substantial dispersion; the inter-quartile range is 8.0pp and the standard deviation is 9.8pp. One-digit industry explains only 2% of the variation in $\hat{\theta}_j^{EB}$, increasing to 7% when weighting by each firm's number of at-risk hires. The implied standard deviation of θ_j is 12.7pp. Firms vary in their propensity to hire WCRs, suggesting that they differ in how they treat criminal records in hiring.

Table 8 correlates estimated firm WCR hiring propensities $\hat{\theta}_j$ (scaled by 100 to express in percentage points) with firm characteristics.[53] All columns include fixed effects for industry and firm size categories, where size is based on a firm's average number of employees per month.[54] Descriptive statistics for the firms included in the analysis are provided in Appendix Table A.10.

Column 1 highlights significant differences by industry. For example, compared to at-risk hires at services firms (the omitted category), hires at manufacturing and construction firms are about 2.4 and 3.4 percentage points more likely to have criminal records, respectively. For reference, recall that 56% of at-risk hires have a record. Among at-risk hires, larger firms are less likely to

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

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

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

hire WCRs. Compared to firms with 1 to 10 employees, at-risk hires at firms with 101 to 250 employees and firms with 1001+ employees are 2.1 and about 2.8 percentage points less likely to have a record.

Firm heterogeneity in WCR hiring may in part reflect differences in explicit policy, including how background checks are used in hiring. Firms may vary in whether they conduct background checks, whether they signal background checks in job ads, and what they do when a candidate's background check is not clean.[55] Column 2 adds an indicator for whether a firm mentions background checks in at least one of their job ads. We limit the sample to firms that we can match to at least one job ad.[56] At-risk hires at firms that mention background checks in their ads are about 1.3pp less likely to have a record.

In column 3 we test whether WCR hiring varies with whether the firm operator has past exposure to people with criminal records. Previous exposure could affect a manager's willingness to hire WCRs by, for instance, providing better information about their performance or decreasing prejudice (Paluck et al., 2019; Lepage, 2024). We study three sources of exposure: (i) whether operators themselves have had a criminal record in the past, (ii) whether an operator was exposed to an above or below median share of coworkers with criminal records at previous establishments in which they worked (regardless of whether they operated those establishments or not),[57] and (iii) whether an operator has a family member (parent, child, or sibling) with a criminal record. Because a firm's operator exposure can vary from year to year, we construct a firm-specific exposure measure by averaging annual operator exposure indicators across the firm's "at-risk" hires from 2005 to 2015–the period during which we observe operator identities.

For each exposure measure, we find that at-risk hires at firms with more exposed operators are 1.4 to 1.9 percentage points more likely to have criminal records. These associations should not be interpreted as causal effects of operator exposure. Firms more willing to hire WCRs may be more likely to select or promote more exposed operators, while more exposed operators may themselves sort toward such firms. Distinguishing these selection channels from the role of managers' discretion, beliefs, and prior experience in shaping hiring is an important direction for future research.

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

[56] See Appendix Table A.11 for firm-level summary statistics characterizing job ad signaling behavior.

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

Table 8
Firm propensity to hire WCRs and firm characteristics

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

This table presents coefficient estimates from least squares regressions of estimated firm WCR hiring propensities $\hat{\theta}_j$, described in section 6.2, on firm characteristics. Each observation is a firm with at least one "at-risk" hire as defined in section 6.3 between 1995 and 2015. "Firm ever mentions background checks in ads" is an indicator variable for whether the firm mentions that they will conduct a background check in at least one of their job ads. "Operator has past record" is an average of an indicator variable for whether the firm's operator in a given year has ever had a criminal record in the past. "Operator has family with record" is an average of an indicator variable for whether the firm's operator in a given year has at least one family member with a record. "Operator has high exposure to co-workers with records" is an average of an indicator variable for whether the firm's operator in a given year has an above-median share of coworkers at previous establishments they worked at who had a record. For each operator exposure measure, the average is taken over the firm's "at-risk" hires from 2005 to 2015, the years where we observe the identity of a firm's operator.

7 Conclusion

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

We provide the first nationwide and register-based estimates of the prevalence of criminal records in the workforce and demonstrate that demographically similar adults with and without records have markedly different labor market outcomes. These gaps are seen at multiple margins: WCRs earn less (31%), work less (20% fewer months per year), have lower monthly earnings, are sorted into lower-paying firms with disproportionately more coworkers with records, and are sorted out of occupations with mandatory background checks or where records may be perceived as risky to a firm's customers, employees, or finances. These estimates indicate that WCRs are among the most disadvantaged groups in the Swedish labor market, with gaps larger than those observed, for instance, with respect to gender, education, and immigrant status (Åslund et al., 2021; Karimi et al., 2024).[58] Interestingly, this firm sorting largely occurs within industries rather than across industries.

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

Our estimates are smaller than those reported in comparable US studies, including a 30%

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

earnings reduction for people with felony convictions in Washington State (Rose, 2021) and employment declines of 7–11% for misdemeanor charges and 8–26% for felony charges across several states (Agan et al., 2024b).[59] These differences could reflect differences between the Swedish and US labor markets and criminal justice systems. For example, the judicial process in the US may be more disruptive to defendants' existing work arrangements, and US employers may exhibit stronger or more widespread aversion to hiring WCRs.[60] Although we show that the research design applied in prior US-based studies is not suitable in our context, differences in design appear unlikely to account for much of the gap in estimated magnitudes.

We also document substantial heterogeneity in hiring of WCRs across firms, even within industry and controlling for worker demographics. While a record decreases monthly earnings within firms, we find that it also leads workers to sort away from more discriminatory employers (i.e., those with the lowest propensity to hire WCRs) and that these effects are partially offset by an increase in employment at less discriminatory firms.

[59] Earlier work by Grogger (1995) using data from the early 1980s finds small and temporary employment and earnings losses after arrests and no detectable effects of convictions. Criminal background checks, even in the US, were much less common in this pre-Internet period.

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

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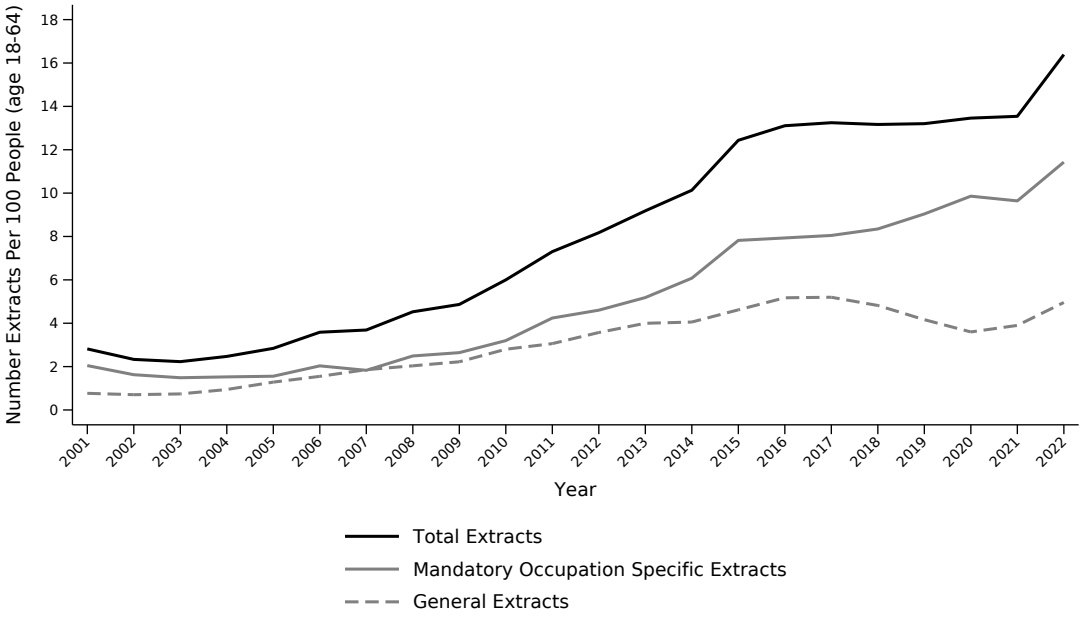
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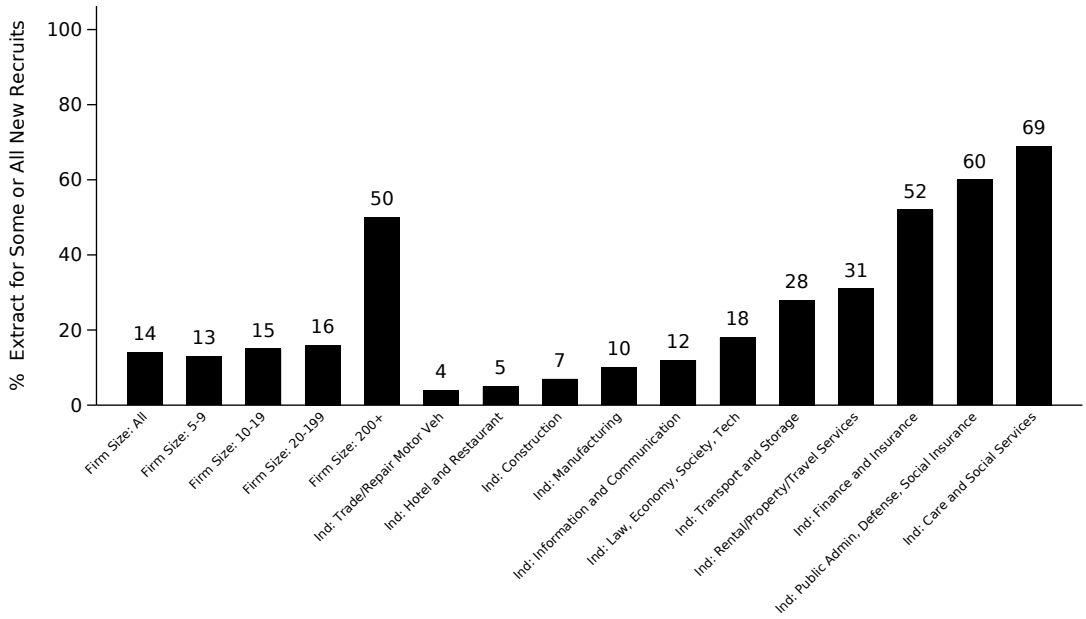
Figure A.1
Annual extract requests from police registry



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

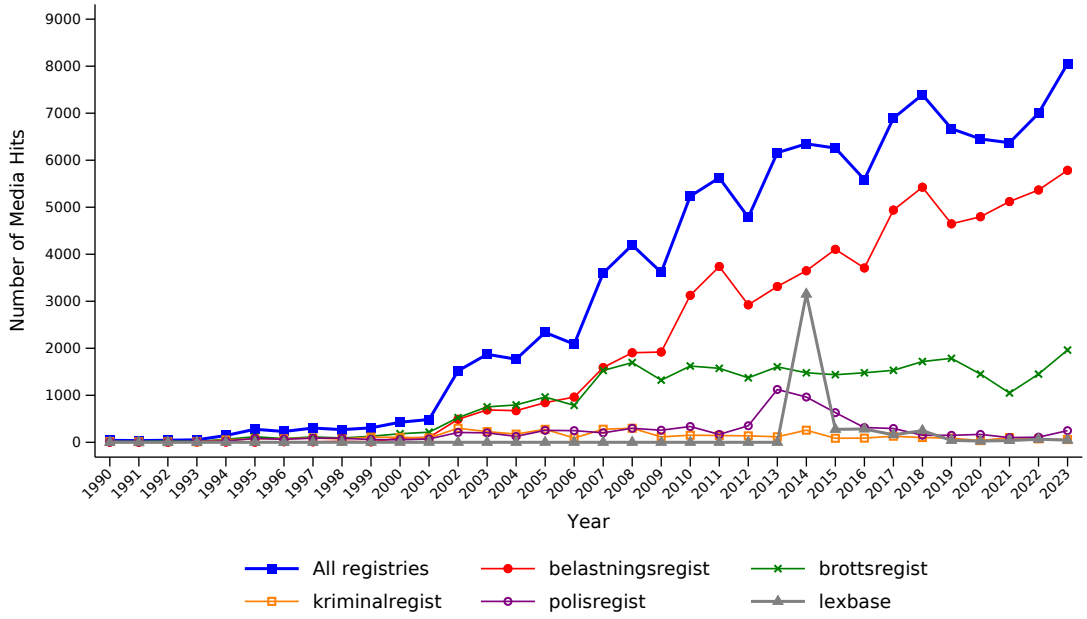
A Additional exhibits

FIGURE A.2
GOVERNMENT SURVEY OF FIRMS ABOUT BACKGROUND CHECKS



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

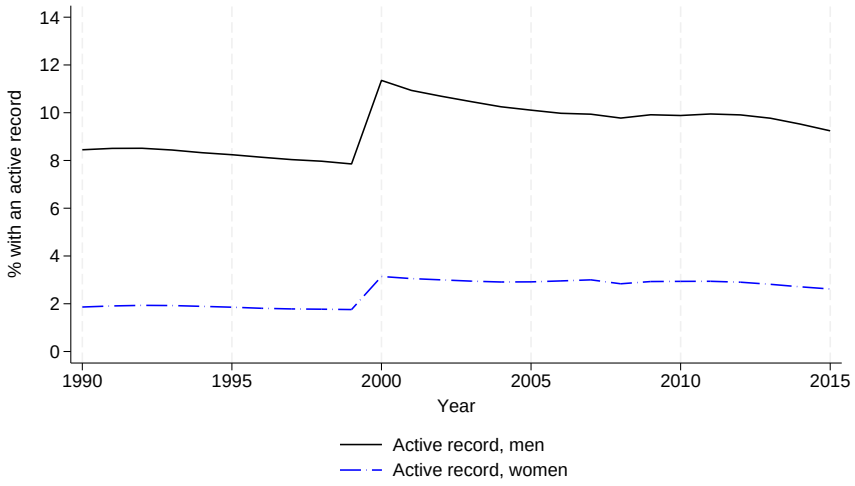
FIGURE A.3
MEDIA COVERAGE OF CRIME REGISTERS AND LEXBASE



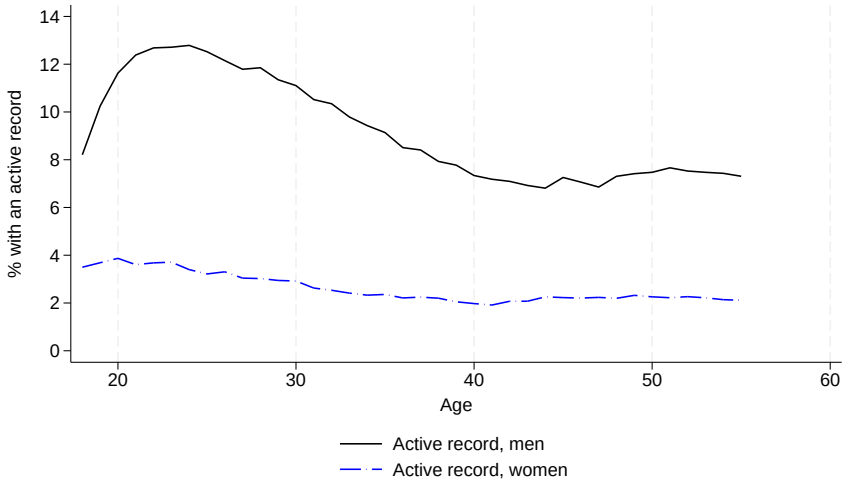
Note: This figure presents the number of hits per year in a Swedish media archive (newspapers, TV, and radio) for the following keyword searches in *Retriever*: belastningsregist, brottsregist, kriminalregist, polisregist. The latter two capture the two registries that existed prior to 2000: Kriminalregistret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

Figure A.4
Prevalence of workers with criminal records

(A) Share of workforce with a record over time



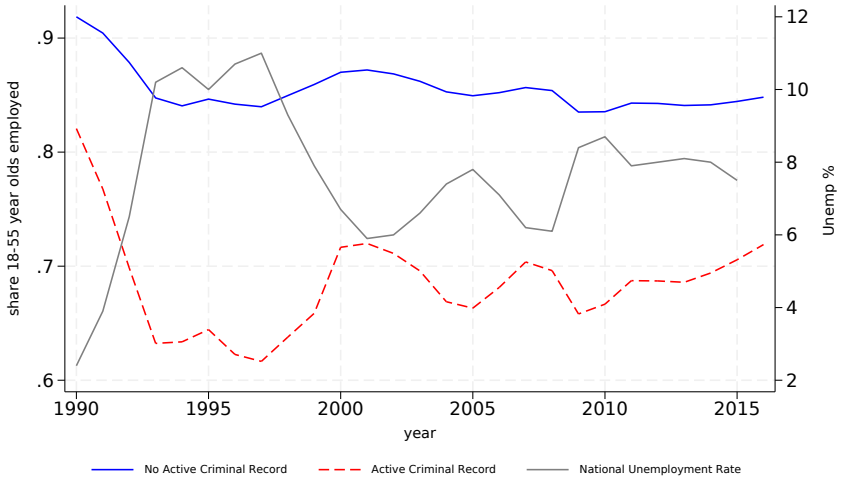
(B) Share of workforce with a record by age, 2015



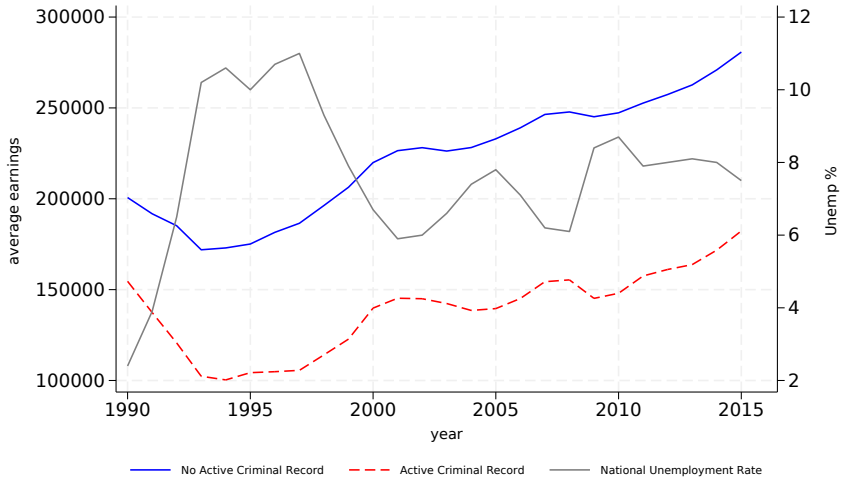
Note: Panel A plots the percentage of prime-age adults in each year of our sample with a criminal record while panel B shows the same percentage by age in 2015. In both panels, the solid black line is for men and the dashed blue line is for women.

FIGURE A.5
RAW EMPLOYMENT AND EARNINGS GAPS OVER TIME

(A) Employment



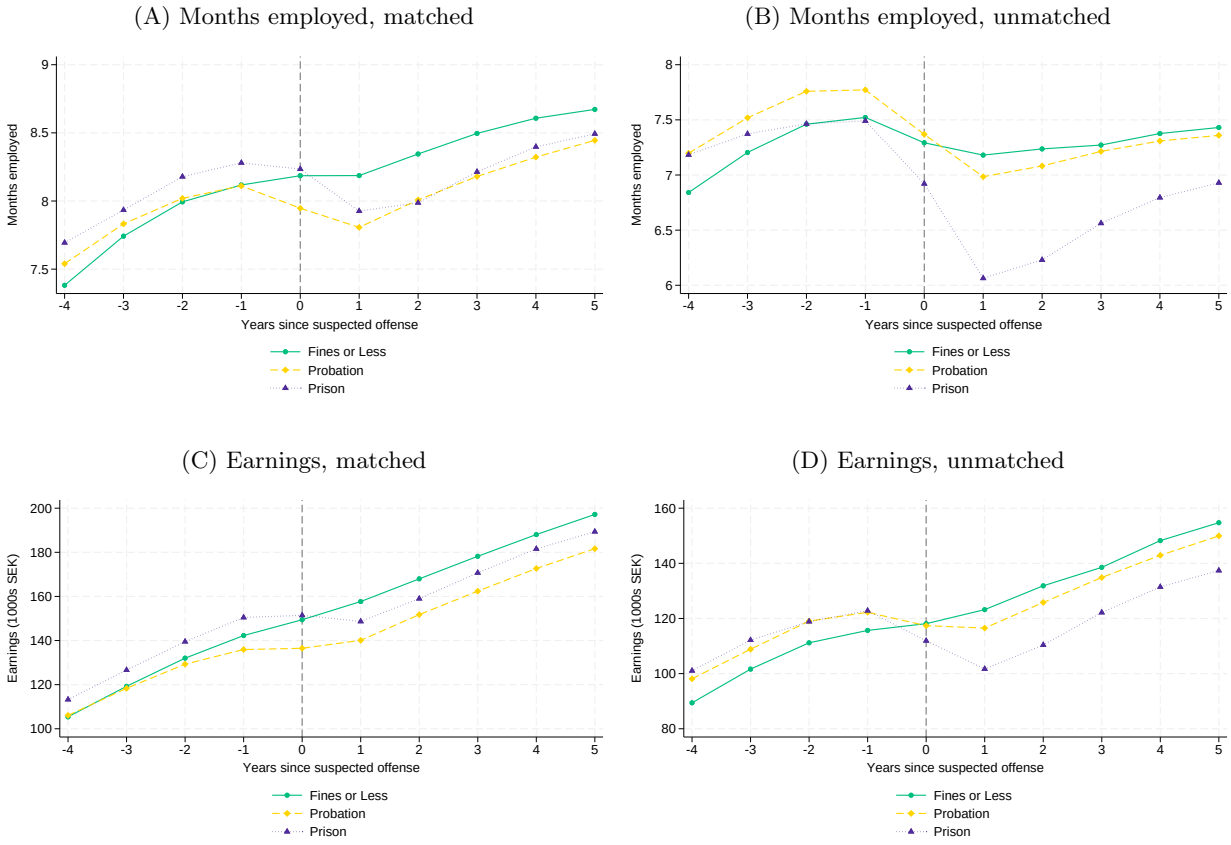
(B) Earnings



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

Figure A.6

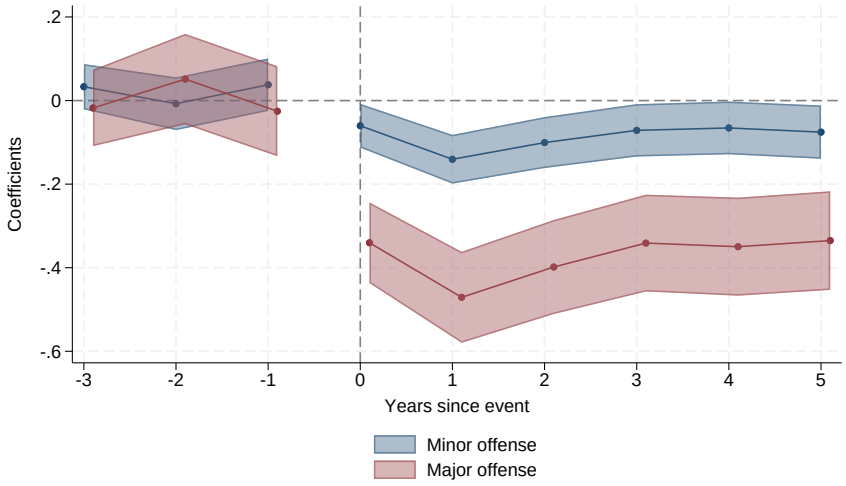
Employment and earnings after the first charged offense, by sentence and match status



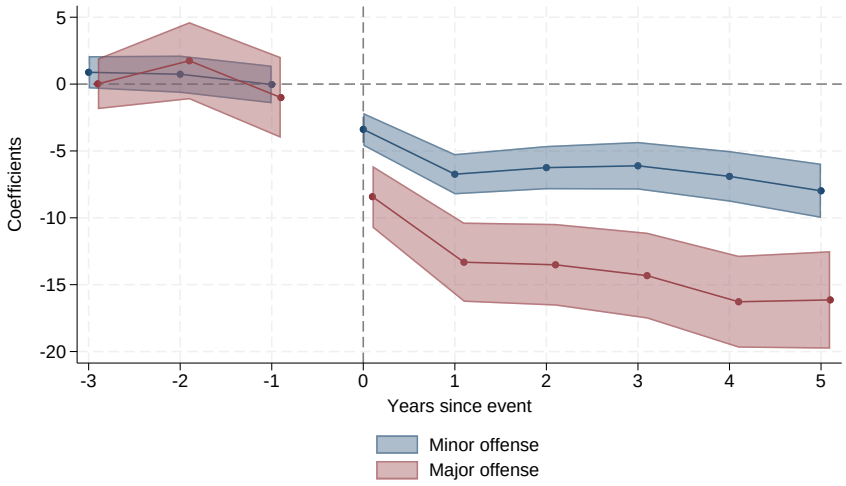
Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) 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 A.7
Employment and earnings effects by offense severity, imputation-based method

(A) Months employed



(B) Earnings

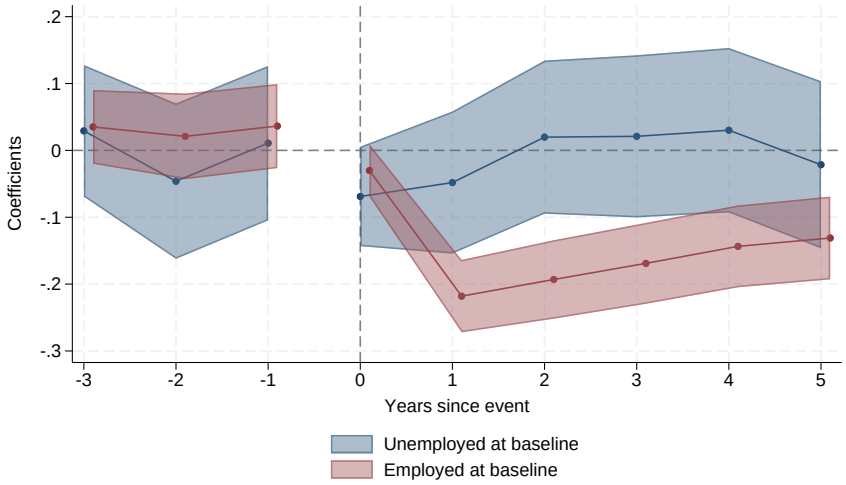


Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024), where the specification is estimated separately for events corresponding to minor and major offenses. An event includes a major offense if any suspected offense code falls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

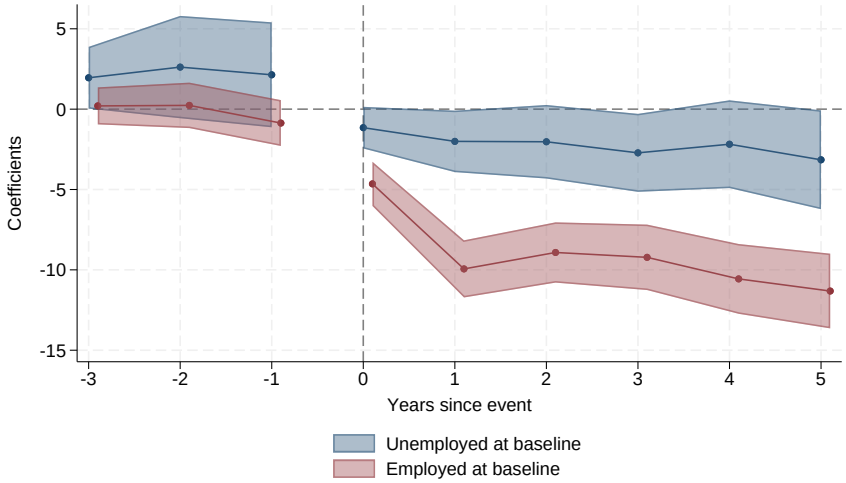
FIGURE A.8

EMPLOYMENT AND EARNINGS EFFECTS BY BASELINE EMPLOYMENT STATUS, IMPUTATION-BASED METHOD

(A) Months employed



(B) Earnings

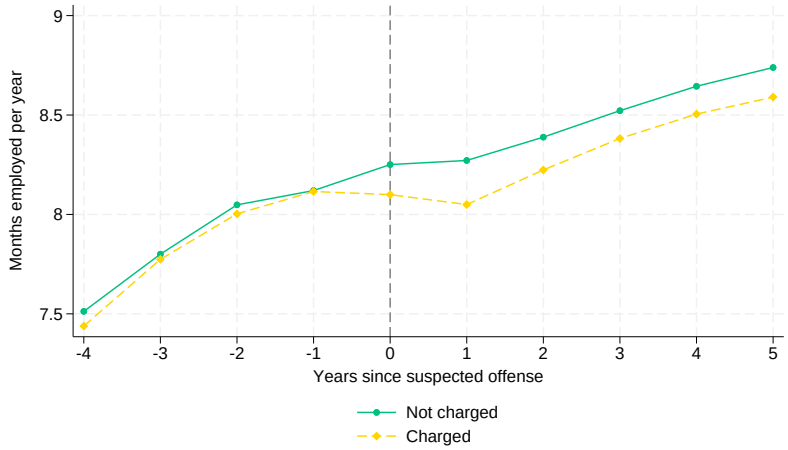


Note: This figure plots the event time coefficients using the imputation-based event study methodology of Borusyak et al. (2024), where the specification is estimated separately for events where the suspect is working in the month of the suspected offense and events where the suspect is not. In panel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

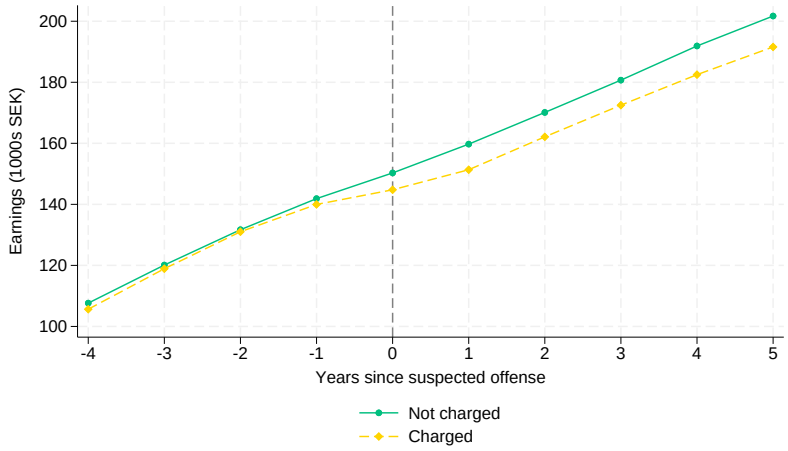
Figure A.9

Acquiring a criminal record reduces employment and earnings, omitting prison sentences

(A) Months employed



(B) Earnings

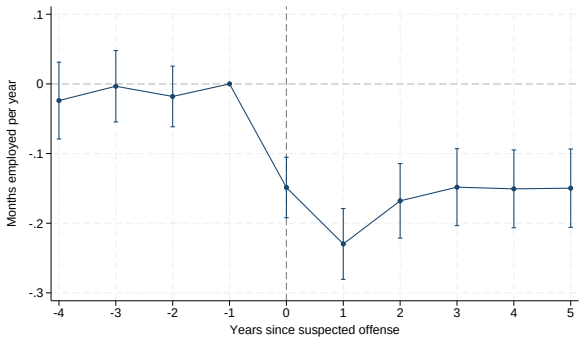


Note: This figure plots months employed (panel A) and earnings (panel B) before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. We limit events to person-years with at least one associated suspected offense where the person (1) does not 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. We exclude charged suspects who received a prison sentence for the focal offense. We recalculate the weights for non-charged suspects after this exclusion, retaining the original matching variables. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects.

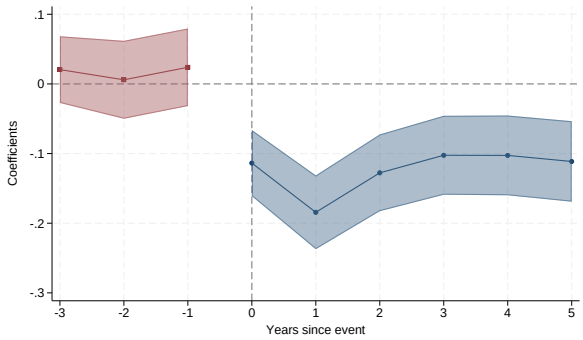
Figure A.10

Event study estimates for employment and earnings, omitting prison sentences

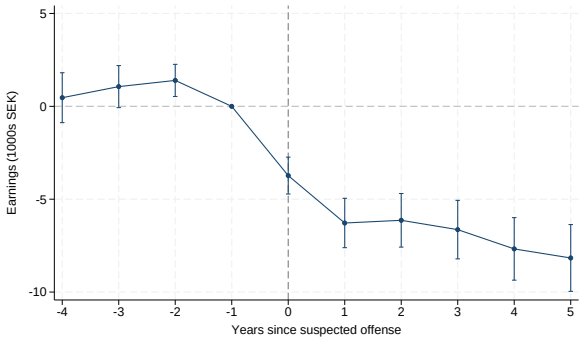
(A) Months employed



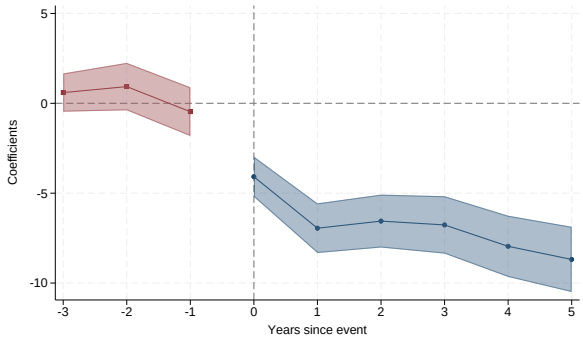
(B) Months employed (imputation approach)



(C) Earnings

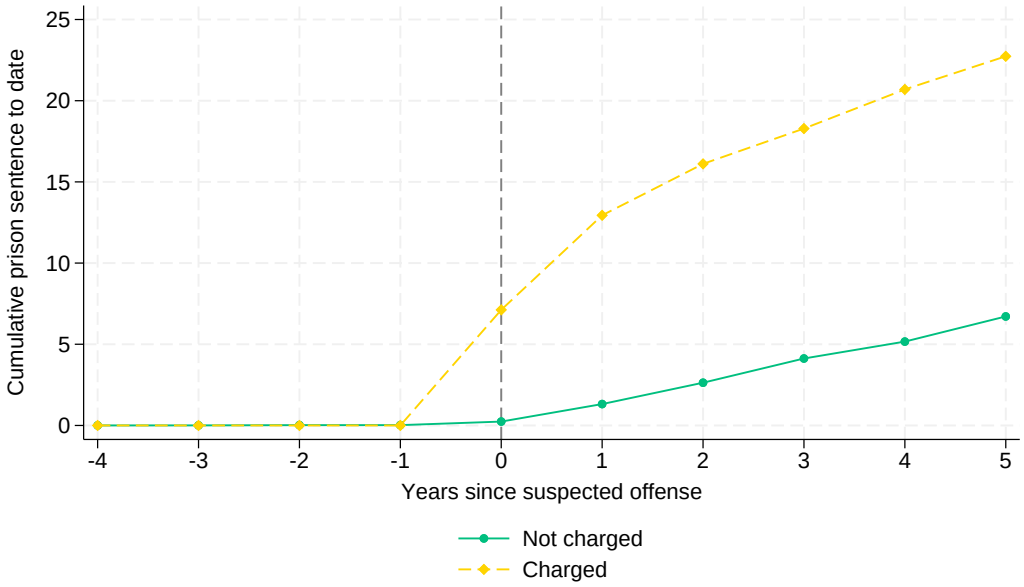


(D) Earnings (imputation approach)



Note: This figure plots the event time coefficients (λ_j) from the event study specification (2). We exclude charged suspects who received a prison sentence for the focal offense. We recalculate the weights for non-charged suspects after this exclusion, retaining the original matching variables. In panels A and B the outcome is months employed. In panels C and D the outcome is earnings. In panels B and D, the event time coefficients are plotted using the imputation-based event study methodology of Borusyak et al. (2024). Standard errors are clustered at the person level.

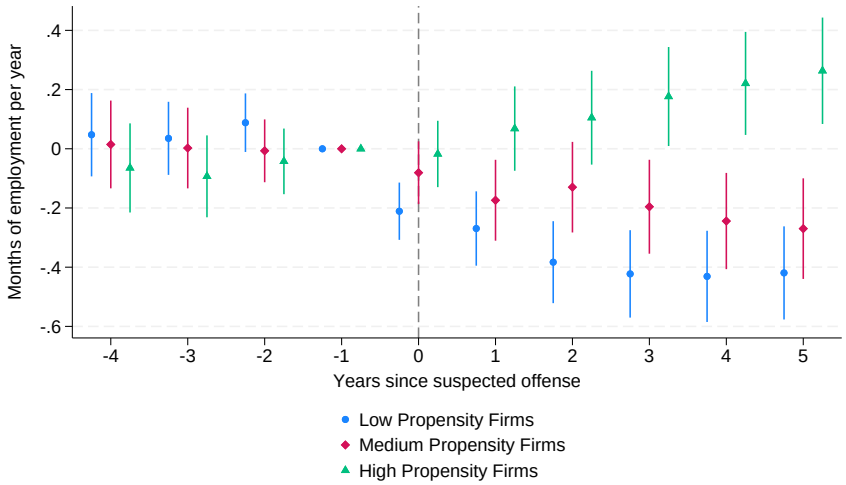
Figure A.11
Cumulative prison sentence days by event time



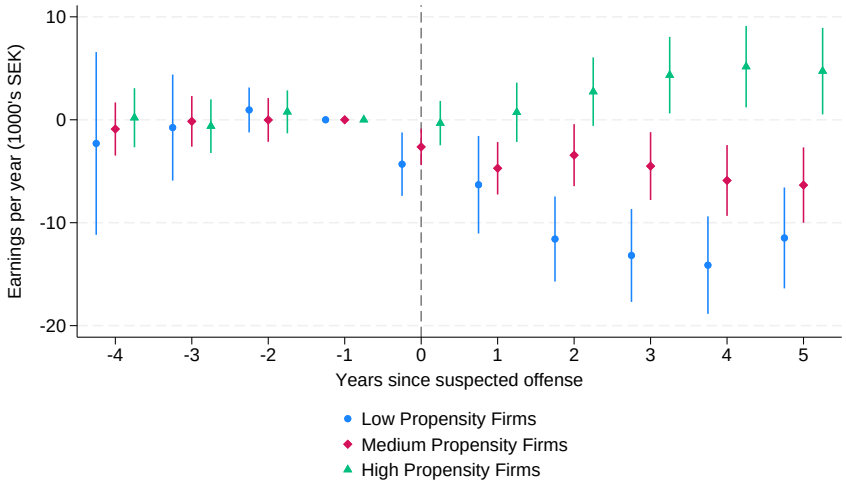
Note: This figure plots weighted mean cumulative prison sentence days before and after suspected offenses for the sample of charged and matched non-charged suspects described in section 5.2. Prison sentence days are cumulated from four years before the suspected offense through the indicated year and include sentences for the focal offense and subsequent offenses. The measure records days sentenced rather than days actually spent in prison. Outcomes are plotted separately for non-charged (green solid line) and charged (orange dashed line) suspects.

Figure A.12
Employment and earnings effects by firm type, major offenses

(A) Months employed



(B) Earnings



Note: This figure plots the event time coefficients ($\hat{\lambda}_j$) from the event study specification (2) for months employed (panel A) and earnings (panel B) interacted with indicators for three firm types: low propensity, medium propensity, and high propensity to hire WCRs. See section 6.2 for a description of how firm types are constructed. We limit to suspected offenses that include a major offense.

Table A.1
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	-0.26	-0.15	-0.15	-0.14	-0.08
	(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 outcome mean	270.58	270.58	270.58	10.18	10.18	10.18	10.02	10.02	10.02	10.02	10.02
Observations			31,324,035						26,379,960		

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

TABLE A.2
MOST COMMON SUSPECTED OFFENSES AND OUTCOMES

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

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

TABLE A.3
MOST COMMON SUSPECTED OFFENSES AND OUTCOMES, NO PRIOR CHARGE

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

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

Table A.4
Event characteristics under different matching schemes

	Unadjusted		Case-based match		Full match	
	Charged	Non-charged	Charged	Weighted non-charged	Charged	Weighted non-charged
Panel A: Offense characteristics						
Year of suspected offense	2003.0	2003.8	2002.9	2002.9	2002.8	2002.8
Number of prior years with suspected offenses	.2787	.3534	.2572	.2572	.1355	.1355
Type of suspected offense:						
Violent (%)	25.63	48.81	25.01	25.01	23.85	23.85
Property (%)	39.45	31.38	39.03	39.03	40.34	40.34
Traffic (%)	28.13	5.572	27.19	27.19	24.28	24.28
Narcotics (%)	11.86	4.677	10.69	10.69	5.877	5.877
Sex (%)	2.246	4.107	2.056	2.056	1.423	1.423
Other (%)	12.65	15.85	11.9	11.9	8.004	8.004
Any major offense (%)	26.19	39.16	25.2	25.2	20.94	20.94
Panel B: Suspect characteristics						
Age in year of suspected offense	29.54	30.54	29.56	30.27	29.56	29.56
Male (%)	69.38	64.99	69.17	65.17	69.67	69.67
Swedish born (%)	71.77	69.04	71.87	71.7	72.82	73.13
Years of education	11.35	11.53	11.37	11.53	11.42	11.53
Earnings (1000s SEK)	135	148.8	135.8	147.1	140.8	142.8
Months employed during year	7.99	8.222	8.018	8.241	8.128	8.138
Any months employed during year (%)	75.67	76.74	75.87	77.02	76.29	76.16
Tenure in main job (months)	34.77	37.09	34.89	36.69	35.73	35.72
Live in Stockholm (%)	29.39	33.54	29.34	31.93	28.39	30.16
Live in Göteborg (%)	12.21	9.65	12.22	10.11	12.4	10.4
Live in Malmö (%)	10.86	11.99	10.87	11.95	10.84	11.99
Workplace size	320	329.4	321.3	321.4	323.1	303.9
Firm size	4624	5024	4632	4817	4566	4431
Average firm coworker monthly wage (1000s SEK)	16.84	17.23	16.86	16.94	17.04	16.83
Firm pay premium at main job	-.0582	-.05985	-.05756	-.05981	-.04986	-.06081
Firm coworkers with active records (%)	6.789	6.511	6.736	6.407	6.519	6.437
Industry (%):						
Agriculture and resources	3.653	3.204	3.656	3.62	3.854	3.881
Care and social services	10.52	11.24	10.53	10.67	10.08	9.653
Construction	7.66	6.686	7.634	7.267	7.769	7.837
Education	4.549	5.528	4.571	5.076	4.588	4.594
Health care	3.474	3.962	3.499	3.985	3.409	3.511
Hotels and restaurants	7.231	6.903	7.195	6.803	6.799	6.959
Manufacturing and industry	20.12	18.13	20.1	18.31	20.74	19.01
Public administration and defense	2.484	3.773	2.49	3.419	2.512	3.243
Retail and wholesale	14.82	13.85	14.9	14.03	15.21	14.71
Services	23.57	24.93	23.53	24.9	23.36	24.89
Unknown	1.898	1.78	1.889	1.897	1.665	1.681
Missing	.01255	.01646	.01282	.02204	.006274	.03327
Panel C: Sanctions						
Fine or less (%)	59.25	–	59.46	–	59.14	–
Probation and other (%)	31.99	–	32.03	–	33.28	–
Prison (%)	8.767	–	8.51	–	7.581	–
Charged events retained	84,250		82,276		62,682	
Non-charged events retained		150,438		149,576		95,888

This table describes the suspects and suspected offense combinations of people and years ("person-years") satisfying the sample criteria described in section 5.2. The first two columns include all charged and non-charged suspects, respectively. The third and fourth columns include charged and non-charged suspects who can be matched using suspected offense characteristics, respectively, re-weighting non-charged suspects across cells to match the distribution among charged suspects. The fifth and sixth columns repeat this procedure using the preferred matching procedure, which additionally matches on age, gender, and earnings in the year before the suspected offense. The construction of cells is described in detail in section 5.2. 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 100 percent. The number of prior years with suspected offenses counts the earlier years in which a person was suspected of an offense. The middle panel describes characteristics of suspects, including labor market outcomes in the year before the focal suspected offense. The bottom panel describes sentence type for charged suspects. Sentence type refers to the most serious sentence for all suspected offenses in the event year. The final rows report the numbers of charged and non-charged events retained under each matching procedure. The construction of firm pay premiums is discussed in Appendix C.

Table A.5
Common support under matching

Panel A: Overall support

Measure	Value
Eligible charged events	84,250
Charged events retained	62,682
Share of charged events retained	74.4%
Eligible non-charged events	150,438
Non-charged events retained	95,888
Total exact matching cells	69,760
Exact matching cells containing both groups	21,524
Share of exact matching cells containing both groups	30.9%

Cell-level support among retained charged events

Percentile	N_{0c}	N_{0c}/N_{1c}
10th	1	0.250
25th	2	0.500
50th	4	1.000
75th	7	2.000
90th	11	3.500

Panel B: Retention by offense group

Charged-event group	Eligible charged	Retained charged	Share retained	Median N_{0c}/N_{1c}
All	84,250	62,682	74.4%	1.000
Minor	62,187	49,556	79.7%	0.800
Major	22,063	13,126	59.5%	1.667
Violent	21,597	14,952	69.2%	2.333
Property	33,240	25,286	76.1%	1.000
Traffic	23,700	15,217	64.2%	0.333
Narcotics	9,992	3,684	36.9%	0.833
Sex	1,892	892	47.1%	1.000
Other	10,659	5,017	47.1%	1.000

This table describes common support under the preferred matching procedure described in section 5.2. The top panel reports the numbers of eligible and retained charged and non-charged events and the numbers of exact matching cells before and after imposing common support. An event is retained if its exact matching cell contains at least one charged and one non-charged event. N_{0c} and N_{1c} denote the numbers of non-charged and charged events, respectively, in exact matching cell c. The percentiles in the top panel and medians in the bottom panel are calculated across common-support cells, weighting each cell by N_{1c}. Hence, the reported statistics describe the cell-level support available for a randomly selected retained charged event. The bottom panel reports the number and share of charged events retained and the median ratio of non-charged to charged events within matching cells, separately by offense group. Offense-type rows overlap because an event may involve suspected offenses in multiple categories.

Table A.6
Heterogeneity in the employment effect of a charged offense

	By offense severity		By baseline status		By offense type			
	Minor	Major	Unemployed	Employed	Violent	Property	Narcotics	Traffic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
λ_{-4}	-0.04	0.03	-0.01	-0.04	0.06	-0.04	-0.00	-0.23
	(0.03)	(0.05)	(0.06)	(0.03)	(0.05)	(0.04)	(0.13)	(0.07)
λ_{-3}	-0.00	0.01	0.02	-0.00	0.06	-0.03	0.04	-0.13
	(0.03)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.12)	(0.06)
λ_{-2}	-0.05	0.08	-0.06	-0.02	0.03	-0.05	0.06	-0.14
	(0.02)	(0.04)	(0.05)	(0.02)	(0.04)	(0.04)	(0.10)	(0.05)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_0	-0.11	-0.30	-0.10	-0.07	-0.11	-0.21	-0.42	-0.14
	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)	(0.10)	(0.05)
λ_1	-0.20	-0.45	-0.11	-0.25	-0.27	-0.26	-0.54	-0.23
	(0.03)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)	(0.12)	(0.06)
λ_2	-0.16	-0.37	-0.03	-0.24	-0.26	-0.16	-0.63	-0.16
	(0.03)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.12)	(0.07)
λ_3	-0.13	-0.33	-0.05	-0.22	-0.18	-0.16	-0.60	-0.17
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)
λ_4	-0.13	-0.33	-0.02	-0.20	-0.20	-0.15	-0.54	-0.22
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)
λ_5	-0.13	-0.30	-0.07	-0.18	-0.17	-0.14	-0.66	-0.22
	(0.03)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)	(0.13)	(0.07)
Non-charged outcome mean	8.55	7.86	3.76	10.30	8.43	7.82	8.14	9.23
Observations	1165480	420220	449150	1063670	597120	579170	71550	217680

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (2), where the specification is estimated separately for various subsamples of events. The outcome is months employed. Columns 1 through 4 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Columns 5 and 6 split the sample in half by suspect age: 22–29 in column 5 and 30–40 in column 6. Columns 7 and 8 split the sample period in half: 1995–2003 in column 7 and 2004–2010 in column 8.

Table A.7

Heterogeneity in the employment effect of a charged offense: demographics and time period

	By age		By time period		By gender		By immigrant status		By education	
	22–29	30–40	'95-'03	'04-'10	Men	Women	Swedish born	Immigrant	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
λ_{-4}	-0.04	-0.01	-0.06	0.02	-0.03	-0.01	-0.04	-0.08	-0.01	-0.10
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.07)
λ_{-3}	0.00	-0.01	-0.03	0.03	0.03	-0.07	0.00	-0.09	0.02	-0.07
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.06)	(0.03)	(0.07)
λ_{-2}	-0.02	-0.01	-0.05	0.02	-0.01	-0.03	-0.00	-0.08	-0.01	-0.06
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.02)	(0.06)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_0	-0.14	-0.18	-0.15	-0.17	-0.17	-0.14	-0.18	-0.10	-0.15	-0.12
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.02)	(0.05)
λ_1	-0.27	-0.26	-0.23	-0.30	-0.26	-0.27	-0.31	-0.10	-0.28	-0.15
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.06)
λ_2	-0.24	-0.19	-0.15	-0.27	-0.19	-0.26	-0.25	-0.05	-0.22	-0.12
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.07)
λ_3	-0.19	-0.17	-0.14	-0.22	-0.15	-0.25	-0.22	-0.05	-0.19	-0.15
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.07)
λ_4	-0.18	-0.18	-0.15	-0.21	-0.15	-0.24	-0.16	-0.12	-0.19	-0.08
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.07)
λ_5	-0.19	-0.16	-0.14	-0.21	-0.15	-0.24	-0.17	-0.09	-0.17	-0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.07)	(0.03)	(0.07)
Non-charged outcome mean	8.40	8.29	7.99	8.65	8.75	7.42	9.09	6.38	8.26	8.74
Observations	801610	784090	773550	812150	1104600	481100	1116320	386510	1201710	326072

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (2), where the specification is estimated separately for various subsamples of events. The outcome is months employed. Columns 1 and 2 split the sample by suspect age. Columns 3 and 4 split the data by the time period of the suspected offense. Columns 5 and6 split the sample by suspect gender. Columns 7 and 8 split the sample by suspect Swedish born status. Columns 9 and 10 split the sample by suspect educational attainment. The 'Low' group has 12 years or less of education while the 'High' group has more than 12 years of education.

Table A.8
Heterogeneity in the earnings effect of a charged offense

	By offense severity		By baseline status		By offense type			
	Minor (1)	Major (2)	Unemployed (3)	Employed (4)	Violent (5)	Property (6)	Narcotics (7)	Traffic (8)
λ_{-4}	0.03	1.01	-2.14	0.87	1.24	-0.70	3.86	-2.09
	(0.71)	(1.54)	(1.65)	(0.72)	(0.88)	(1.28)	(1.90)	(1.60)
λ_{-3}	0.91	1.01	-0.18	1.06	1.14	0.98	3.45	-1.21
	(0.63)	(1.18)	(1.06)	(0.73)	(0.87)	(0.88)	(2.04)	(1.90)
λ_{-2}	0.77	2.73	0.48	1.09	1.03	1.65	3.08	0.77
	(0.50)	(0.91)	(0.60)	(0.59)	(0.70)	(0.70)	(1.73)	(1.33)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_0	-3.14	-7.16	-1.96	-3.95	-3.44	-4.52	-7.99	-5.44
	(0.53)	(1.17)	(0.47)	(0.60)	(0.74)	(0.85)	(1.92)	(1.38)
λ_1	-5.91	-12.17	-2.99	-8.35	-6.03	-6.85	-14.91	-11.22
	(0.70)	(1.58)	(0.76)	(0.86)	(0.98)	(1.15)	(2.52)	(1.88)
λ_2	-6.05	-12.03	-3.00	-8.59	-7.34	-6.65	-19.92	-6.80
	(0.79)	(1.59)	(1.02)	(0.93)	(1.12)	(1.19)	(2.90)	(2.09)
λ_3	-6.38	-12.64	-3.76	-9.24	-7.97	-7.36	-20.97	-5.94
	(0.88)	(1.67)	(1.23)	(1.01)	(1.23)	(1.28)	(3.11)	(2.30)
λ_4	-6.93	-14.89	-3.52	-10.46	-7.66	-8.83	-23.18	-8.58
	(0.94)	(1.78)	(1.33)	(1.08)	(1.32)	(1.34)	(3.38)	(2.51)
λ_5	-7.83	-14.31	-4.00	-10.90	-8.85	-7.64	-24.88	-9.23
	(1.01)	(1.88)	(1.43)	(1.16)	(1.40)	(1.44)	(3.62)	(2.67)
Non-charged outcome mean	168.99	152.30	50.84	213.16	165.23	142.66	139.65	200.00
Observations	1165480	420220	449150	1063670	597120	579170	71550	217680

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (2), where the specification is estimated separately for various subsamples of events. The outcome is earnings. Columns 1 through 4 split the sample by the type of offense (violent, property, narcotics, traffic). Events that include multiple types of suspected offenses are included in multiple samples. Columns 5 and 6 split the sample in half by suspect age: 22–29 in column 5 and 30–40 in column 6. Columns 7 and 8 split the sample period in half: 1995–2003 in column 7 and 2004–2010 in column 8.

Table A.9

Heterogeneity in the earnings effect of a charged offense: demographics and time period

	By age		By time period		By gender		By immigrant status		By education	
	22–29	30–40	'95-'03	'04-'10	Men	Women	Swedish born	Immigrant	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
λ_{-4}	0.31	0.26	0.26	0.32	-0.01	0.96	-0.41	0.43	0.42	-1.51
	(0.61)	(1.21)	(0.70)	(1.12)	(0.88)	(0.80)	(0.89)	(1.08)	(0.70)	(2.68)
λ_{-3}	0.68	1.22	0.57	1.31	1.10	0.59	0.41	0.95	0.84	1.96
	(0.63)	(0.95)	(0.70)	(0.87)	(0.73)	(0.78)	(0.68)	(1.13)	(0.59)	(2.24)
λ_{-2}	0.96	1.62	1.61	0.95	1.55	0.69	0.94	0.80	0.99	4.26
	(0.54)	(0.70)	(0.59)	(0.64)	(0.56)	(0.65)	(0.55)	(0.82)	(0.48)	(1.50)
λ_{-1}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
λ_0	-4.04	-4.34	-3.32	-5.05	-5.16	-1.97	-5.11	-2.12	-3.83	-2.71
	(0.59)	(0.80)	(0.62)	(0.77)	(0.64)	(0.70)	(0.64)	(0.90)	(0.50)	(1.90)
λ_1	-7.17	-7.95	-5.60	-9.50	-8.78	-4.75	-9.17	-2.89	-6.15	-8.22
	(0.78)	(1.08)	(0.88)	(0.99)	(0.86)	(0.93)	(0.84)	(1.23)	(0.65)	(2.68)
λ_2	-8.07	-7.12	-5.89	-9.34	-8.55	-5.47	-9.65	-2.54	-6.32	-7.73
	(0.89)	(1.14)	(0.92)	(1.11)	(0.92)	(1.05)	(0.89)	(1.43)	(0.73)	(2.72)
λ_3	-8.21	-7.80	-6.07	-9.97	-9.01	-5.73	-9.49	-4.38	-6.03	-9.43
	(0.97)	(1.25)	(1.02)	(1.19)	(1.01)	(1.14)	(0.99)	(1.53)	(0.80)	(2.90)
λ_4	-9.55	-8.43	-7.92	-10.10	-10.55	-5.49	-10.48	-4.35	-6.35	-11.99
	(1.04)	(1.33)	(1.12)	(1.25)	(1.08)	(1.21)	(1.05)	(1.67)	(0.84)	(3.08)
λ_5	-9.96	-9.06	-8.77	-10.29	-11.12	-5.89	-11.05	-4.48	-6.07	-12.49
	(1.11)	(1.42)	(1.20)	(1.32)	(1.15)	(1.30)	(1.12)	(1.79)	(0.90)	(3.28)
Non-charged outcome mean	145.50	181.59	144.48	180.68	187.79	109.27	186.00	107.08	152.10	207.63
Observations	801610	784090	773550	812150	1104600	481100	1116320	386510	1201710	326072

This table shows event time coefficients ($\hat{\lambda}_j$) from the event study specification (2), where the specification is estimated separately for various subsamples of events. The outcome is earnings. Columns 1 and 2 split the sample by suspect age. Columns 3 and 4 split the data by the time period of the suspected offense. Columns 5 and6 split the sample by suspect gender. Columns 7 and 8 split the sample by suspect Swedish born status. Columns 9 and 10 split the sample by suspect educational attainment. The 'Low' group has 12 years or less of education while the 'High' group has more than 12 years of education.

Table A.10
Firm descriptive statistics for Table 8

	(1) Baseline	(2) Matched to job ads	(3) With exposure measures
EB firm effect for WCR hiring (θ) times 100	0.862	-0.868	0.371
	(9.309)	(7.420)	(8.897)
Unadjusted firm effect for WCR hiring (θ) times 100	0.813	0.0177	0.636
	(19.85)	(15.92)	(18.55)
Number of at-risk hires	20.86	60.21	22.66
	(145.7)	(330.4)	(134.1)
Firm pay premium	-0.179	-0.0680	-0.153
	(0.350)	(0.286)	(0.331)
Public sector	2.166	6.176	1.779
	(14.56)	(24.07)	(13.22)
Industry			
Agriculture and resources	3.40	2.93	3.32
Manufacturing and industry	18.39	17.06	18.21
Construction	14.69	7.814	16.42
Retail and wholesale	16.29	15.04	16.47
Hotels and restaurants	14.27	14.24	14.07
Education	2.53	6.80	2.74
Health care	3.11	7.02	2.97
Care and social services	3.08	3.16	3.11
Public administration and defense	0.26	0.72	0.13
Services	23.98	25.22	22.55
Firm size			
Average firm size	59.18	177.0	48.08
	(519.4)	(1157.8)	(300.7)
1-10	39.76	18.10	38.02
11-25	30.70	30.33	31.80
26-100	22.17	33.21	22.87
101-250	4.528	9.841	4.684
251-1000	2.140	5.697	2.155
1001-2500	0.442	1.613	0.330
2501+	0.265	1.216	0.142
Job ads and exposure measures			
% firms ever mentions background checks in ads	13.49	13.49	12.33
	(34.17)	(34.17)	(32.88)
Operator has past record	24.64	18.91	24.68
	(41.38)	(36.84)	(41.40)
Operator has family with past record	15.27	13.13	15.27
	(32.84)	(29.51)	(32.84)
Operator has high exposure to co-workers with records	36.26	19.81	36.27
	(46.20)	(36.26)	(46.19)
Observations	130633	23188	78436

This table reports descriptive statistics for firms used to estimate specifications described in Table 8 (columns 1-3). Firm pay premiums are estimated using a standard worker-firm earnings decomposition (Abowd et al., 1999; Card et al., 2016). See Appendix C for details.

TABLE A.11
FIRM-LEVEL SUMMARY STATISTICS IN THE JOB ADVERTISEMENT DATA

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

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

B Data appendix

B.1 Supplementary data: occupational characteristics and Swedish job ads

Occupational characteristics: We use the US Department of Labor's Occupational Information Network (O*Net) data to both measure traditional occupational characteristics (e.g., average education, job preparation, and hazardous work conditions) and create custom characteristics that are, according to firm surveys, especially salient to the WCR context, including: (i) extent of customer interactions, (ii) exposure to vulnerable populations (i.e., children, disabled, elderly, mentally ill, and substance use-related), and (iii) opportunities for theft or financial responsibility. Appendix B.3 describes the O*NET data, variable creation, and procedure to match Swedish occupation codes. Second, for each Swedish occupation code and year, we measure whether background checks are required upon hiring or licensing according to the laws described in Appendix B.4.

Job advertisement background checks: We use a data set of over 9 million job advertisements from 2006 to 2023 from the job board maintained by the Swedish Public Employment Service–Platsbanken. We search the text of each ad for keywords—'belastningsregist', 'registerkontroll', 'bakgrundskontroll'—to measure whether the employer signals a background check is part of the hiring process.[61] We linked about 70% of ads to the firms that posted them in our employer-employee register using employer identification numbers. This allows us to investigate how background check signals in ads translate to actual hiring of WCRs—a first in the literature, beyond self-reported measures from firm surveys. Appendix B.5 describes the job ad data and linking procedure.

B.2 Criminal record expungement rules over time

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

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

Between 1990 and 1999, the expungement rules were the same for all persons 15 years or older.

[61] All of these terms are regularly used to signal criminal background checks; research assistants also manually validated this for a large sample of ads.

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

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

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

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

Appendix Tables B.12, B.13, and B.14 give a detailed description of the changes to Sweden's criminal record expungement laws over time.

Table B.12
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 B.13
Swedish law (1998:620) concerning expungement of criminal records (2000-2024)

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

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

Table B.14
Continued ... Swedish law (1998:620) concerning expungement of criminal records (2000-2024)

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

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

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

B.3 O*NET data appendix

B.3.1 SOC to SSYK occupation code crosswalk

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

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

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

B.3.2 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**.[62] 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

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

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

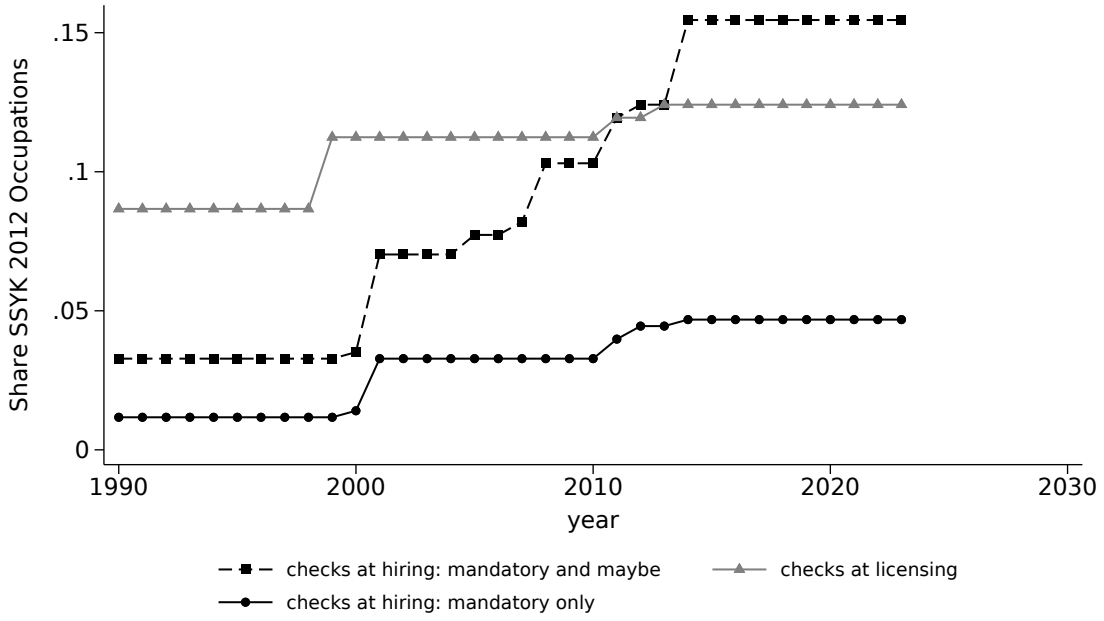
Our analyses use the average of all nine O*NET work environment conditions: exposure to contaminants; disease/infection; hazardous conditions; hazardous equipment; high places; minor burns, cuts, bites and stings; radiation; whole body vibration; and extremely bright or inadequate lighting. We proxy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

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

B.4 Mandatory background check laws and reforms

As described in section B.1, we characterize jobs according to whether criminal background checks were required upon hiring or to obtain an occupational license. The latter are generally only conducted at the time the license is obtained, and not required retroactively upon reforms. Reforms have been rolled out that made background checks required for selected jobs and occupations. The table below lists the reforms we used (as well as the date of implementation) to code whether background checks would be required in each 4-digit occupation code in each year. We classify occupation codes as having mandatory checks (for all possible jobs), maybe background checks (for either some jobs or optional), and no background check requirements. Figure B.13 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 B.13
Roll-out of background check laws at hiring/licensing by occupation



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

Table B.15
List of background check laws for hiring and occupational licensing

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

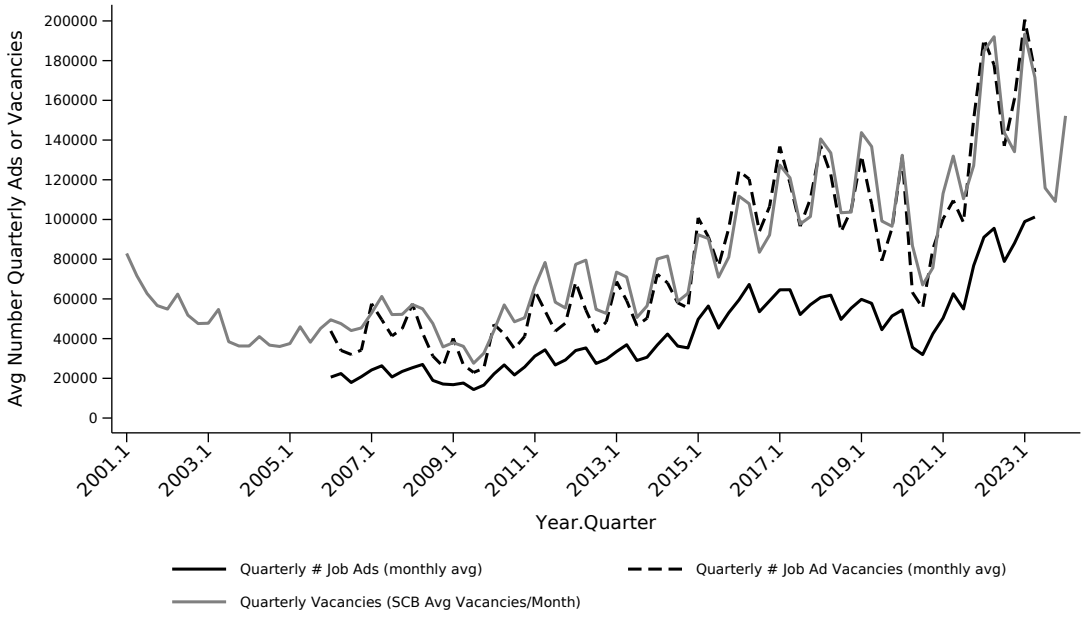
B.5 Job ad data appendix

We measure the extent to which Swedish firms signal that background checks will be conducted during hiring using a dataset of Swedish job advertisements from Platsbanken, the largest job board in Sweden.[63] The dataset spans 2006-2023 and includes over 9 million job ads; a total of close to 17 million vacancies (due to multiple jobs per ad). We create a unique ID variable for each ad and analyze the text to capture whether it signals that a background check will be conducted during the hiring process.

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

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

FIGURE B.14
NUMBER OF PLATSBANKEN JOB ADS/VACANCIES VS. NATIONAL VACANCY ESTIMATES



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

To get a better sense of how representative Platsbanken is of the labor market, we compare in Appendix Figure B.14 the number of vacancies in Platsbanken to those reported in Statistic Sweden's monthly survey of firms asking for the number of vacancies (lediga jobb, which refers to job vacancies for which the firm has started external recruitment). As Statistics Sweden reports the average number of monthly vacancies per quarter, we collapse the job ad data down to the quarterly level and report the average number of monthly ads and average number of monthly vacancies. Two phenomena stand out. First, the trends in the these two time series are very similar, suggesting the Platsbanken captures business cycle variations over time in the labor market. Second, these two time series are very close together.

Key Variables: A key step of our analysis is to identify employers who signal - either implicitly or explicitly - that they will require a background check. We do this by searching the text for the following key words and creating a dummy for each hit: "belastningsregist", "registerkontroll", "bakgrundskontroll". We drew random samples of ads and manually looked at the text to confirm

that these hits were indeed capturing background checks. We flag the ad as signaling a background check if any one of these key words are contained in the ad. We created a number of other variables, i.e., indicators for whether the ad includes other keywords related to drug- and alcohol-free work environments. The dataset also includes occupation codes, dates of ad posting, number of vacancies, position type (e.g., regular versus summer), position length, salary type (e.g., fixed versus flexible), and job location (of which municipality is the most complete). The main occupation code variable is one used internally by the team that maintains the ads database, but a mapping to ssyk96 or ssyk12 codes is available at https://gitlab.com/davidnoirman/legacy-id-to-ssyk. Finally, all ads include a firm name, which we use to code the employer type. We classify jobs as 'public' if the employer name contains 'municipality', 'region', 'county', or 'city'.

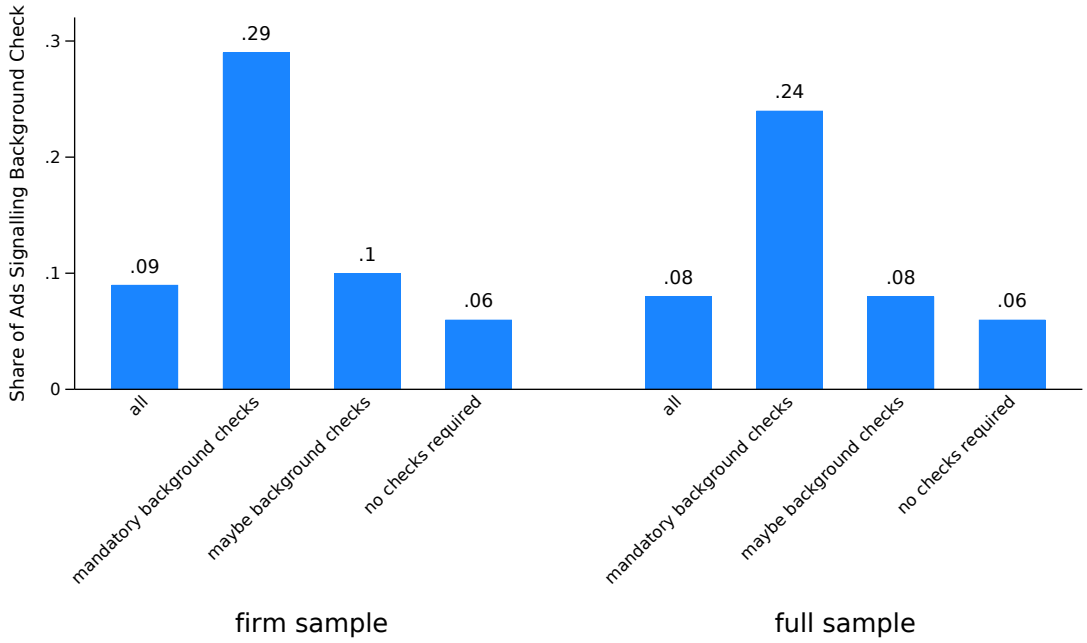
Firm Organization Numbers and Matching to SCB Registers: The final step is to match these job ads data to firms in the employer-employee register. This is straightforward for the 2021-2023 job ads since almost all of these ads included the firm's organization number. But, only firm names were included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

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

Figure B.15 demonstrates that the firm sample is fairly representative of the full sample of job ads in terms of background check signals: 9% of the firm sample and 8% of the full sample signal

background checks. This is consistent with the firm sample containing firms that post more ads and survey evidence suggesting that larger firms conduct more background checks. Moreover, firms are more likely to signal background checks for 4-digit occupation codes that have mandatory checks (29%) than those that only maybe have checks (10%) or do not require checks (6%).

FIGURE B.15
SHARE OF JOB ADS SIGNALING BACKGROUND CHECKS BY OCCUPATION TYPE



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

C Estimating firm pay premiums

We model log monthly earnings as a function of additive worker and firm fixed effects Abowd et al. (1999). Each observation corresponds to a worker-firm-year combination. For each worker and year, we restrict to the firm where the worker earned the most that year. We drop observations where monthly earnings are below 2,000 SEK. Log monthly earnings w_{it} for worker i in year t are given by:

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

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

We estimate approximately 7 million worker effects and 1 million firm effects. The standard deviation of worker effects is 0.428, and the standard deviation of firm effects is 0.224. The raw correlation between estimated worker effects and firm effects is 0.09. Appendix Table C.16 provides more descriptive statistics on the model estimates.

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

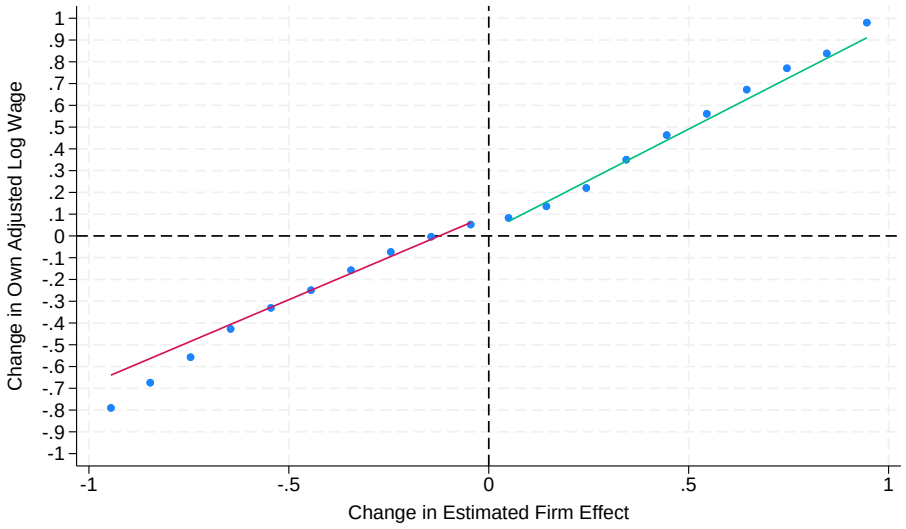
$$E[(\epsilon_{it} - \bar{\epsilon})(D^j_{it} - \bar{D}^j_i)] = 0 \; \forall j,$$

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

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

Figure C.16 plots the relationship between earnings changes for firm switchers and changes in

FIGURE C.16
EARNINGS CHANGES FOR MOVERS BY FIRM EFFECT

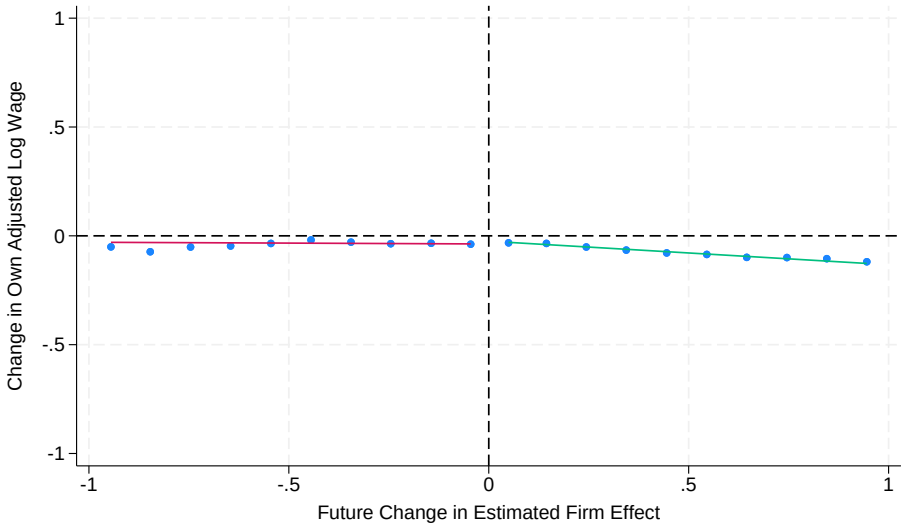


Note: This figure plots earnings changes for firm switchers (vertical axis) against the difference in estimated firm fixed effects between their old and new firms (horizontal axis). Each point represents a decile of changes in firm effects.

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

Next, we perform a placebo test. We select a sample of workers who switch firms in the future but have not yet moved. If workers anticipate or select future firms based on pre-move earnings trajectories, we would expect current earnings changes to correlate with the eventual change in the firm effect at the new firm. Figure C.17 shows no evidence of this relationship, consistent with exogenous mobility.

Figure C.17
Earnings changes for future movers by firm effect



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

Table C.16
Summary of AKM Estimates

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

We limit to each worker's firm with the highest earnings in a given year and drop observations where average monthly earnings are below 2,000 SEK. Limited to largest connected set, which includes 99% of employment. Sample restrictions are described in further detail in section 3.2.

D Segregation of WCRs across occupations

The occupational composition of firms varies both across and within industries. This section considers the extent to which WCRs also work in different occupations than non-WCRs. Table D.17 lists the 20 4-digit occupations in which WCRs are most and least prevalent in 2015, as well as their corresponding share of WCRs. This share is less than 0.5% in the 20 least WCR-prevalent occupations, which includes therapists, judges, prosecutors, chemists, and a number of education and care-related occupations. Background checks are mandatory for employment or occupational licensing in more than half of these occupations. WCRs are most concentrated in construction and manufacturing, industry and transport, and elementary unskilled occupations. Some tangible examples include taxi, car and van drivers (12%), as well as many construction and manual jobs (e.g., painters, 12.3%; floor layers, 12.6%; building frame workers, 14.4%; roofers 15.5%; construction laborers, 18.9%; scaffold builders 26.3%).

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

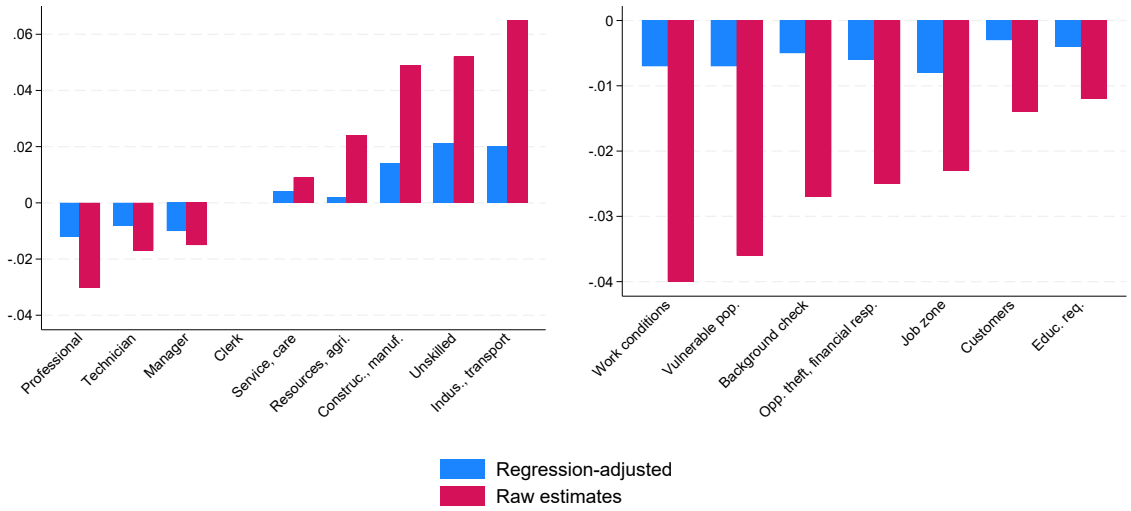
$$\text{RECORD}_{it} = X_{it}\gamma + \gamma_{s(i,t)} + \sigma_{o(i,t)} + \epsilon_{it}, \tag{D.1}$$

where t indexes year. We estimate equation (D.1) using data from 2005–2015.[64] The vector X_{it} includes fully saturated interactions between year, age, education level, gender, and an indicator for Swedish-born. $\gamma_{s(i,t)}$ are fixed effects for 1-digit industry categories, and $\sigma_{o(i,t)}$ are fixed effects for 1-digit occupation categories.

The left panel of Figure D.18 reports the $\sigma_{o(i,t)}$ coefficient estimates from Equation (D.1) when the other controls are excluded (*Raw*) and included (*Regression-adjusted*). Confidence intervals are omitted since most are too small to visualize. All estimates are statistically significant at the 1% level. Conditional on demographics and industry, WCRs are (relative to clerks, the omitted category) more likely to be employed in service and care, resources and agriculture, construction and manufacturing, industry and transport, and elementary unskilled occupations, but less likely to be employed as professionals, technicians, or managers. The adjusted differences can be substantial– WCRs are about 40% less likely to be employed as professionals than in elementary unskilled

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

FIGURE D.18
DISTRIBUTION OF WCRS ACROSS OCCUPATIONS AND OCCUPATIONAL CHARACTERISTICS



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

occupations.

To better understand the nature of this sorting, we look at O*NET occupational characteristics and occupation by year mandatory background check requirements. The right panel of Figure D.18 reports raw and adjusted coefficient estimates from Equation (D.1), replacing occupation fixed effects with a series of occupation characteristics. The estimates indicate that WCRs are less likely to work in occupations with: (i) high external customer contact, exposure to vulnerable populations, or the opportunity for theft and financial responsibility (i.e., when there is a perceived risk to the firm), (ii) mandatory background checks, (iii) higher education and skill (job zone) requirements, and (iv) better work conditions (i.e., where risk to the worker is low). Though markedly smaller when adjusting for observables, these relationships remain statistically significant and sometimes substantial. For example, WCRs are over 10% less likely to work in occupations with above-average working conditions, contact with vulnerable populations, or high skill or experience requirements, and 6% less likely to work in occupations that mandate background checks.[65]

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

Table D.17
Top/bottom 20 occupations by prevalence of WCRs in 2015

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

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

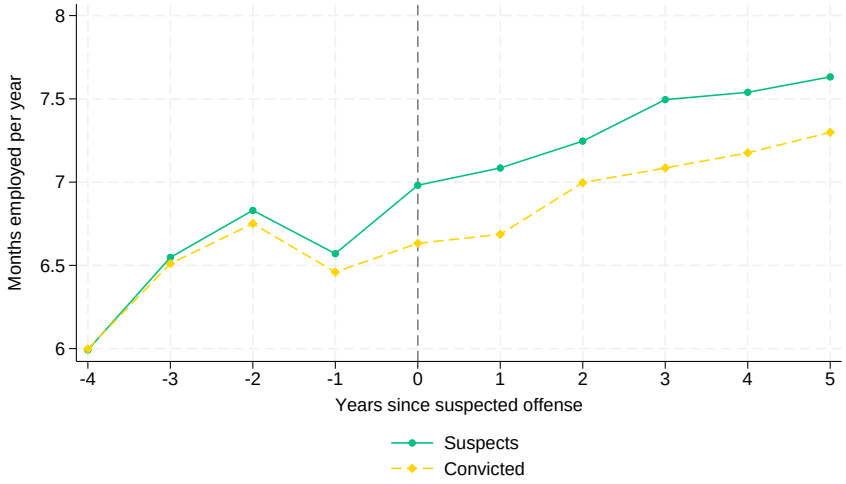
E Subsequent offenses

We construct our estimation sample and weights as in 5.2, except we limit to suspected offenses where the suspect has been charged exactly once in the past, and in the past five years. Again we restrict our analysis to cells that contain both charged and non-charged suspects and weight non-charged suspects to match the distribution of charged suspects. The number of events meeting our sample criteria is smaller and hence it is more difficult to find matches. Cells containing both types of suspects account for 28% of charged suspects. Appendix Table E.18 shows the characteristics of events. Our final example includes 14,233 events in the treatment group and 15,246 events in the comparison group.

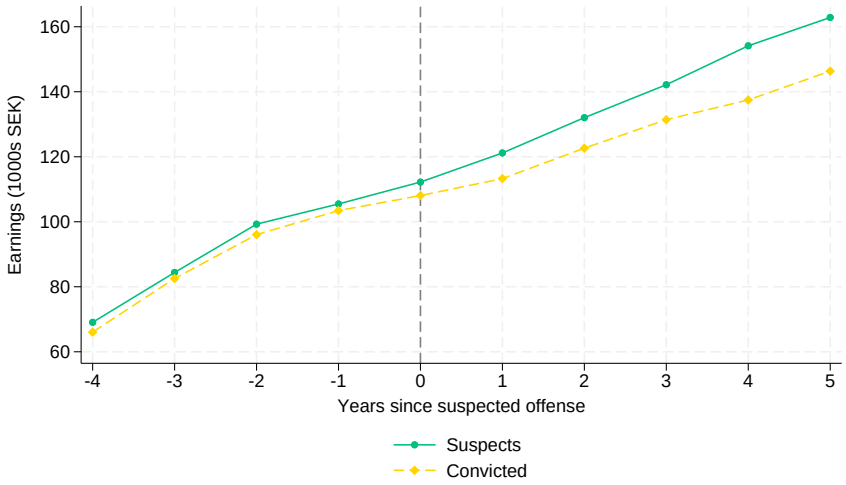
Figure E.19 plots months employed in panel A and earnings in panel B for charged suspects and (weighted) non-charged suspects. Months employed are generally increasing for non-charged suspects, except for a dip in the year before the focal event. A gap of about 0.4 months between charged and non-charged 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 E.19
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 5.7. We limit events to person-years with at least one associated suspected offense where the person (1) has been charged in exactly one prior year; (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.

Table E.18

Characteristics for suspects and suspected offenses, second offense

	All		Matched and Weighted	
	Not charged	Charged	Not charged	Charged
Age at time of suspected offense	29.02	28.09	26.79	26.79
	(5.697)	(5.541)	(5.294)	(5.294)
Year of suspected offense	2004.6	2004.2	2004.4	2004.4
	(4.015)	(4.107)	(4.202)	(4.202)
Order of offense	2.414	2.376	2.155	2.155
	(0.711)	(0.677)	(0.410)	(0.410)
Violent (%)	48.54	28.60	30.50	30.50
Property (%)	34.62	41.16	39.28	39.28
Traffic (%)	6.133	29.99	13.31	13.31
Narcotics (%)	10.94	27.58	17.44	17.44
Sex (%)	4.217	2.073	0.510	0.510
Other (%)	12.13	14.45	3.392	3.392
Any major offense (%)	42.86	31.27	19.77	19.77
Fine or less (%)	0	60.01	0	58.02
Probation and other (%)	0	27.19	0	31.40
Prison (%)	0	12.80	0	10.58
Male (%)	76.13	74.97	79.39	79.39
	(42.63)	(43.32)	(40.45)	(40.45)
Swedish born (%)	62.86	66.23	68.53	68.73
	(48.32)	(47.29)	(46.45)	(46.36)
Age	28.55	27.61	26.30	26.25
	(5.752)	(5.578)	(5.366)	(5.335)
Years of education	11.07	10.93	11.06	11.01
	(1.944)	(1.887)	(1.804)	(1.835)
Earnings (1000s SEK)	108.6	96.42	105.5	103.4
	(134.0)	(117.2)	(126.1)	(120.6)
Months employed year	6.924	6.544	6.570	6.460
	(5.457)	(5.467)	(5.528)	(5.528)
Ever employed during year	68.12	65.80	63.96	63.81
	(46.60)	(47.44)	(48.02)	(48.06)
Tenure in main job (in months)	29.09	27.19	26.01	26.01
	(31.61)	(29.17)	(27.11)	(26.87)
Workplace size	269.7	272.3	245.7	273.0
	(879.0)	(893.7)	(833.6)	(869.5)
Firm size	4219.6	3869.8	3432.9	3756.9
	(10981.9)	(10433.9)	(9794.7)	(10487.2)
Avg. firm coworker monthly wage (1000s SEK)	16.45	16.23	16.78	17.15
	(8.268)	(7.866)	(8.197)	(8.165)
Firm pay premium at main job	-0.0886	-0.0833	-0.0774	-0.0502
	(0.296)	(0.280)	(0.278)	(0.269)
% of firm coworkers with active records	8.510	8.935	8.594	8.620
	(10.80)	(11.52)	(10.87)	(10.26)
Agriculture and resources	3.274	3.577	3.888	3.517
Care and social services	10.00	10.25	7.384	8.034
Construction	9.128	9.461	10.70	12.43
Education	4.044	3.844	3.444	3.557
Health care	2.580	2.488	1.914	1.958
Hotels and restaurants	8.723	9.012	7.621	7.514
Manufacturing and industry	19.32	19.54	20.56	21.58
Public administration and defense	2.080	1.431	1.533	1.199
Retail and wholesale	12.96	13.65	15.17	14.79
Services	25.28	23.93	25.45	23.26
Unknown	2.580	2.787	2.338	2.158
Missing	0.0193	0.0320	0	0
Observations	15,246	14,233	4,058	3,921

This table describes the suspects and suspected offense combinations of people and years ("person-years") satisfying the sample criteria described in section 5.7. 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. There can be multiple suspected offenses corresponding to an event, so crime types sum to more than 100 percent. Order of offense indicates whether this is the first, second, or subsequent year that a person has been suspected. Sentence type refers to the most serious sentence for all suspected offenses in the event year. The bottom panel describes suspects in the year before the focal suspected offense. See Appendix C for the construction of firm pay premiums.

F Expungement

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

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

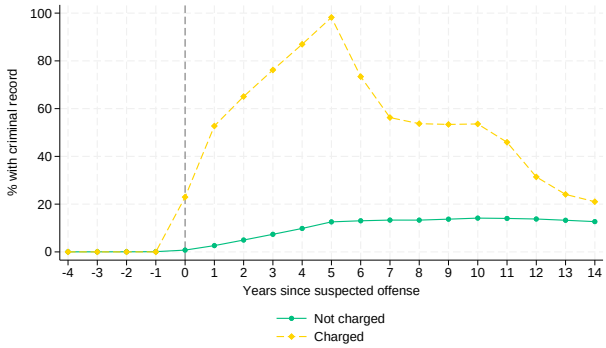
Our second exercise re-centers the analysis around the expungement event for a sample of 233,049 people who: (i) were in the baseline sample of 18–55 year olds from 1990 to 2015, (ii) had an active record during this period, and (iii) were observed five years prior and subsequent to expungement. By construction, the share with a record decreases from 100 to 0% in the first year after the event (gray dots, panel C). Yet, average total earnings increase smoothly over time (black squares, panel C) and there are no visible discrete changes in annual months employed or share working at a new primary employer (gray dots and black squares, panel D).[66]

Analyses of Clean Slate laws in the US (Agan et al., 2024a) and New Zealand (Dasgupta et al., 2025) also find minimal effects of record expungement. The absence of an expungement effect in Sweden may reflect several factors. People may not adjust their job search behavior after expungement, either because they are unaware of the expungement (though this channel is ruled out by Agan et al. (2024a)), feel loyalty to their current employer, or lack the motivation or networks needed to find a higher-paying job. Alternatively, a criminal record may have scarring effects: reduced employment or sorting into lower-quality jobs could produce resume gaps and limit human capital accumulation, effects that employers may observe even after expungement.

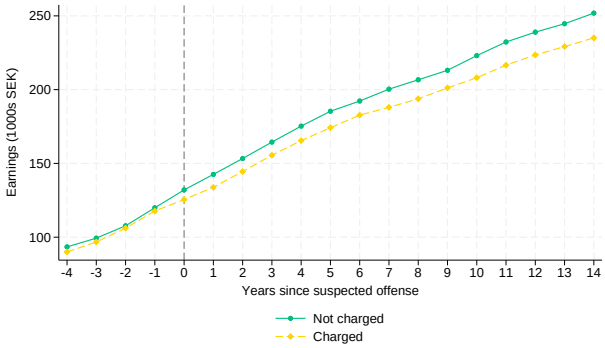
[66] Appendix Figure F.21 shows that expungement occurs 5 to 21 years after the record, especially at six and eleven years, and that about 60% (20%) of expunged records were for property (violent) crimes. Appendix Figure F.22 demonstrates that the lack of an expungement effect in earnings is seen for a wide range of subsamples.

Figure F.20
Expungement has no discernible effects on labor market outcomes

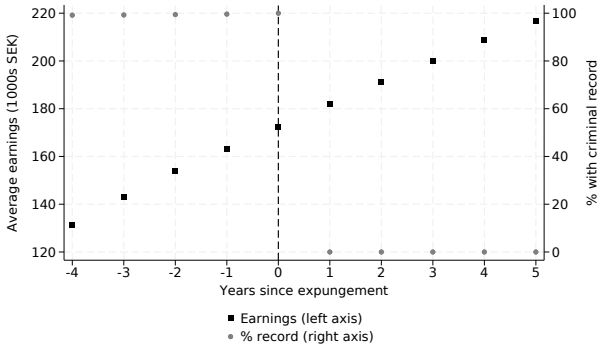
(A) Long-term effect on criminal record status



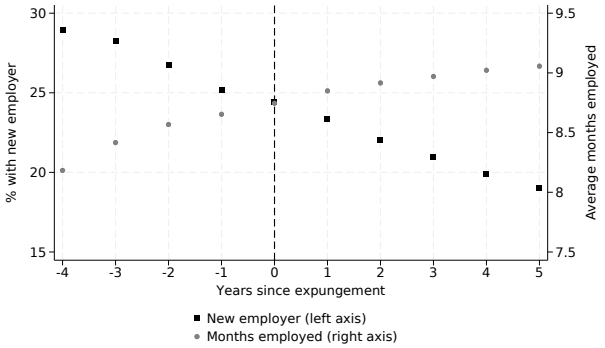
(B) Long-term effect on earnings



(C) Record status and earnings around expungement



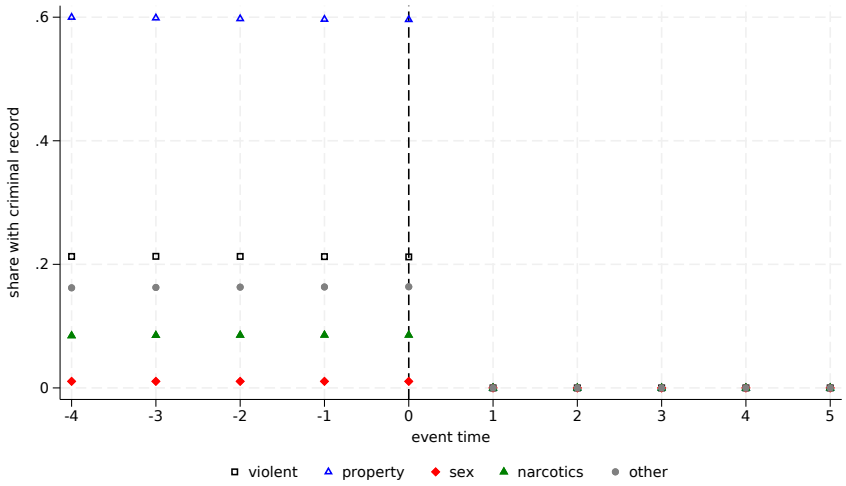
(D) Employment outcomes around expungement



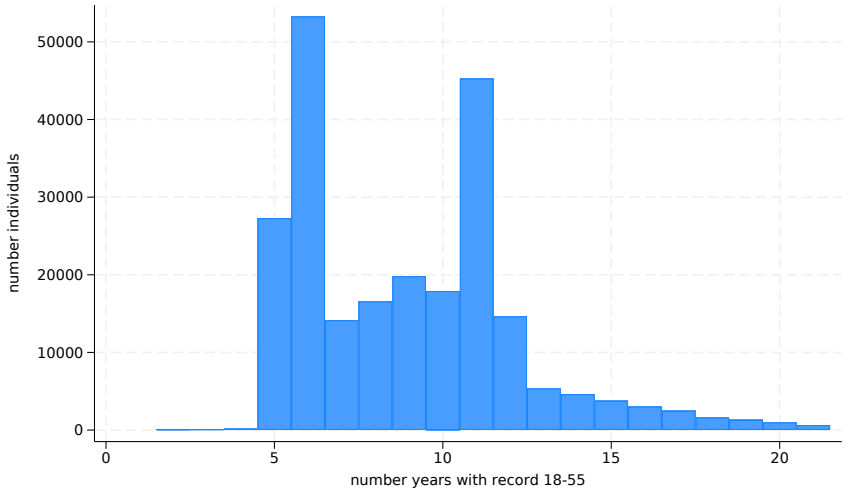
Note: Panel A presents the record status and panel B presents earnings of charged and non-charged suspects over a 14-year period after the focal suspected offense. We limit events to person-years with at least one associated suspected offense where the person (1) does not have a prior charge; (2) is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

Figure F.21
Record characteristics of the expungement analysis sample

(A) Types of expunged records



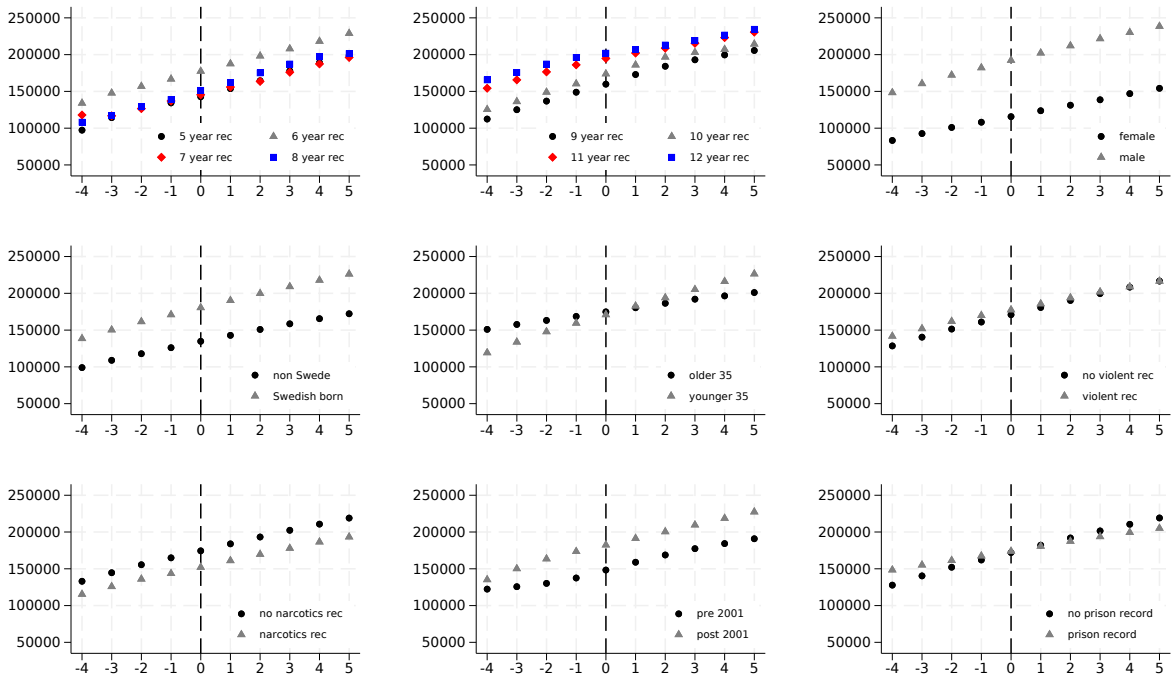
(B) Lengths of expunged records



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

Figure F.22

Average annual total earnings before and after expungement: by subsample



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

G Alternative research design

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

Specifically, using our sample of charged suspects, we estimate

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

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

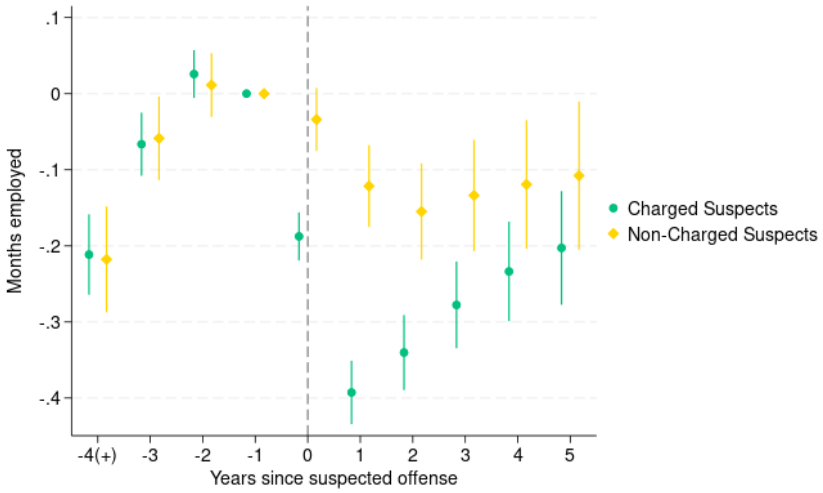
We estimate specifications for both months employed and earnings. We also estimate (G.1) using suspects who are never charged, using the first suspected offense as the focal event. This serves as a placebo test, assessing what the alternative research design would suggest in a group that does not ultimately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

Appendix Figure G.23 plots the estimated coefficients $\hat{\lambda}_j$ for months employed (panel A) and earnings (panel B). In panel A, for charged suspects, there is a clear upward pre-trend leading up to the event. Months employed drop by 0.4 months after the event and recover by about 0.2 months by five years post-event. This estimated decline is similar in magnitude to our main estimate shown in Figure 4. Interestingly, non-charged suspects exhibit a similar pre-trend, followed by a smaller post-event decrease.

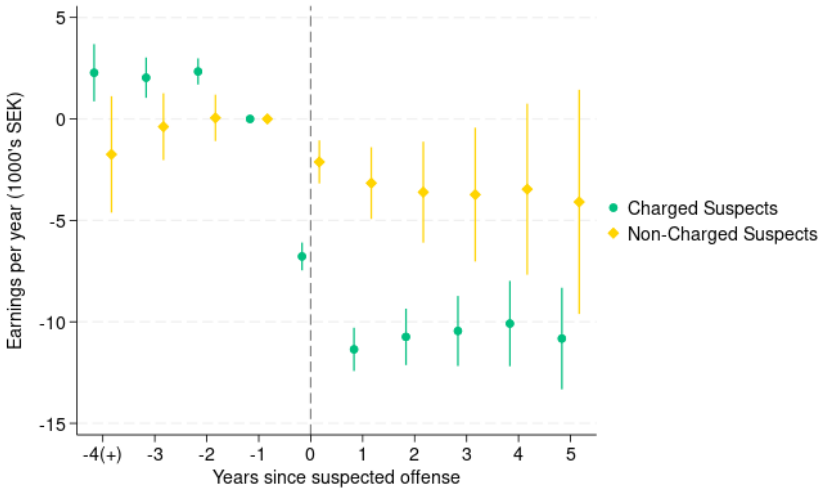
For earnings (Panel B), pre-trends are subtler and diverge between groups—slightly upward for non-charged suspects and slightly downward for charged suspects. Charged suspects show a post-event earnings decline similar in magnitude to our main estimate from Figure 4. Again, non-charged suspects experience a decrease in earnings as well, though somewhat smaller.

Figure G.23
Employment and earnings effects using alternative within-group design

(A) Employment



(B) Earnings



Note: This figure plots the event time estimates ($\hat{\lambda}_j$) from the alternative event study specification (G.1) described in section 5.9. In panel A the outcome is months employed. In panel B the outcome is earnings. For each outcome, the model is estimated separately using charged suspects and suspects who are never charged.

H Calculating $P(RECORD)_{it}$

To calculate $P(\text{RECORD})_{it}$, we take all individuals who are suspected of a crime in year t and label them as charged if they are charged (and receive a criminal record) in year t or in year $t+1$. We estimate a probit model using this charge (1/0) as the dependent variable. We use age at the time of the suspected offense, gender, Swedish born status, suspected offense history, and suspected crime type as explanatory variables. Using this probit model, we predict the probability of being charged and of having a criminal record, $P(\text{RECORD})_{it}$, for each suspected individual, even those who are not actually charged.

We then use age at offense and crime type to estimate how many years this predicted criminal record would remain visible before being automatically expunged (if a suspect had actually been convicted); 3 years, 5 years, 10 years, or 10 years + prison time. For example, if you are 25 years old and commit an aggravated assault, your criminal record would likely be expunged after 12 years (2 years in prison + 10 years post-release). If you are 15 and commit an act of vandalism, then your criminal record would be expunged after 3 years.

We assign each individual their own specific value of $P(\text{RECORD})_{it}$ for each year between year t and the expected expungement year. If they are suspected of a new crime in year $s > t$, then we calculate a new $P(\text{RECORD})_{is}$. But we only replace the original $P(\text{RECORD})_{it}$ with the new $P(\text{RECORD})_{is}$ if $P(\text{RECORD})_{is} > P(\text{RECORD})_{it}$. Being suspected of a new crime also pushes the expected expungement date for all crimes forward in time.

A typical $P(\text{RECORD})_{it}$ for a person suspected of a crime only once will turn from zero to a probability between zero and one in year t and then turns back to zero again after the expected expungement year. For those suspected of multiple offenses, $P(\text{RECORD})_{it}$ may be turned on for many years or turn on and off more than once.

I Empirical Bayes estimation

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

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

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

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

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

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

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

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

We estimate the hyperparameter σ_r^2 with the estimator:

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

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

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

Our empirical Bayes (EB) shrinkage estimate is given by

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

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

J Decomposing the earnings effect

We find that acquiring a criminal record reduces earnings by approximately 5%, with larger declines following more serious offenses. In this appendix, we decompose this earnings decline into three components: (1) reduced months of employment, (2) shifts from higher- to lower-paying firms, and (3) lower monthly earnings within firms. Sections 5.4 and 6.2.2 document the reductions in employment duration, shifts to lower-paying firms, and within-firm pay reductions. Here, we quantify the relative contributions of these factors.

A decomposition based directly on the AKM premium of the worker's employer would require restricting the sample to employed person-years. Because employment is itself affected by a criminal record, this would condition on a post-treatment outcome. We instead use a prediction-based decomposition that remains defined for all person-years, including those with no employment. Average coworker earnings summarizes the earnings environment of the worker's employer and reflects both firm pay-setting and workforce composition. We therefore interpret this component as reallocation toward firms with lower average pay, rather than as a pure change in firms' wage premiums.

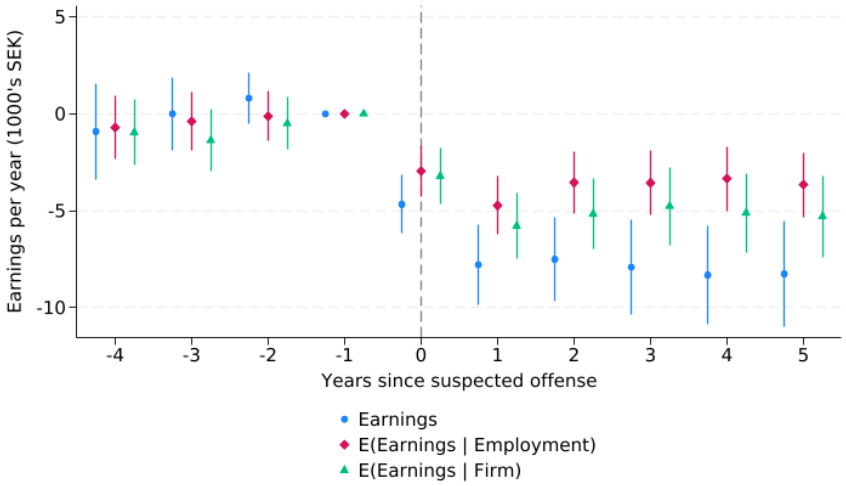
Our decomposition compares estimated treatment effects using three different earnings measures: (a) total earnings, (b) expected earnings conditional on months employed (E[earnings | months employed]), and (c) expected earnings conditional on months employed and firm characteristics (E[earnings | firm]).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

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

Coefficient estimates from this decomposition are shown in Figure J.24. We find that the

treatment effects for E[earnings | months employed] and E[earnings — firm] are approximately 44% and 64% of the total earnings treatment effect, respectively. Thus, we attribute 44% of the earnings decline to fewer months employed, 20% to shifts toward lower-paying firms, and the remaining 36% to reductions in monthly earnings within firms.

FIGURE J.24
DECOMPOSITION OF EARNINGS EFFECT



Note: This figure plots the event time coefficients ($\hat{\lambda}_j$) from the event study specification (2) for three outcomes: a) total earnings, (b) expected earnings conditional on months employed (E[earnings | months employed]), and (c) expected earnings conditional on months employed and firm characteristics (E[earnings | firm]). Sample and variable construction are described in Appendix J. Standard errors are clustered at the person level.