The Role of Young Firms as Employers of Marginalized Workers

Kylie Hwang
Northwestern University

Melanie Wallskog*
Duke University

March 26, 2026

Abstract

Young firms are important drivers of job creation, yet little is known about how and who these firms hire. We show that young firms struggle to compete for labor against established firms and subsequently become disproportionate employers of a largely "unemployable" group: individuals with criminal records. Following a criminal conviction, a worker becomes 21% more likely to take a job at a young firm. This pattern is driven by firm-side constraints rather than worker or young firm preferences: young firms conduct fewer background checks and draw from a residual pool of workers that established firms have screened out.

*We thank seminar participants at Duke University, Drexel University, MIT Sloan School, Seoul National University, the Academy of Management Annual Meeting, and Strategic Management Society Annual Meeting for helpful feedback as well as Zoe Cullen, Vera Rocha, and Jonathan Zandberg. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2967. (CBDRB-FY25-P2967-R12288 and CBDRB-FY26-P2967-R12836) This research uses data from the Census Bureau's Longitudinal Employer-Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191, and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. All errors are our own.

1 Introduction

Young firms play a special role in the economy, driving job creation and employment growth (Haltiwanger, Jarmin, and Miranda, 2013; Klenow and Li, 2021). Yet we know little about *for whom* they create jobs, limiting our understanding of how young firms shape labor market inequality. Young firms struggling to compete in the labor market may disproportionately employ workers passed over by established employers — and consequently create job opportunities for marginal workers — or they may simply mimic the hiring patterns of the broader labor market.[1] In this paper, we ask whether young firms expand employment access for workers excluded from traditional labor markets and, if so, why and with what consequences.

We study these questions in the context of workers with criminal records, who are useful and important for three reasons. First, they constitute a large group: nearly one-third of working-age Americans have a record, comparable to the share with four-year college degrees (Friedman, 2015). Second, they face substantial exclusion from the traditional labor market, as employers seeking to manage risk frequently avoid applicants with records (Cullen, Dobbie, and Hoffman, 2023; Mueller-Smith, 2015). Third, the intensity of barriers to employment varies across time and place — e.g., through Ban-the-Box policies and fluctuations in labor market tightness — creating variation we can leverage to understand why young firms might hire these workers. Under the (empirically-supported) premise that employers generally avoid applicants with records when feasible, observing greater employment of this group at young firms is informative about the constraints and practices that differentiate young from established employers.

We leverage integrated administrative data covering the labor market and criminal justice system for individuals in four states (Arizona, Maryland, New Jersey, and Wisconsin) between 2010 and 2017. This data, housed within the Federal Statistical Research Data Centers (FSRDCs), allows us to follow individuals across the labor market before and after they are convicted of crimes. With rich data on both worker and firm characteristics and outcomes, we are able to characterize when workers with criminal convictions find jobs at young firms — i.e., firms in their first five years of employing workers. We supplement this data with national job postings data from Lightcast in which we observe firms' recruiting behavior — including stated reliance on criminal background checks — from 2010 to 2024. Together, these data allow us to document how young firms disproportionately employ workers with criminal records and to evaluate whether young firms' reliance on workers with criminal records reflects different recruiting and screening behavior.

We begin by documenting that, on average, workers with criminal convictions are disproportionately employed by young firms. We show this using worker-level data, which allows us both to control for worker characteristics and to document this pattern *within* a worker's job history. Namely, the sorting of workers with criminal convictions to young firms persists even after accounting for demographics, home county, employment sector, and past earnings. Furthermore, this pattern does not simply reflect ex-ante differences between individuals who ever commit crimes and those who never do. When we add worker fixed effects, we see that the rate of working for a young firm is 13% higher when a worker has a recent conviction, relative to the mean. In an event study framework, we find that a worker sees a 1 percentage point increase in the rate of working at a young firm *after* a conviction, relative to two years before their conviction, a 21%

[1] Two papers that do study characteristics of young firm employees are Ouimet and Zarutskie (2014) and Fackler et al. (2019), who show that young firms hire younger workers in the U.S. and older, foreign, and unemployed individuals in Germany, respectively. We discuss our contribution relative to these papers below.

increase relative to the mean rate. These patterns are consistent with treatment effects of gaining a criminal record, as opposed to preexisting differences in ability or education that might otherwise lead individuals to sort into young firms (Ouimet and Zarutskie, 2014). We find that these patterns appear across demographic groups, sectors, firm sizes, and types of convictions.

Before turning to mechanisms, we establish that the baseline patterns of sorting we document reflects something specific to firm age, beyond simple assortative or quality-based matching. If both young firms and workers with records are "low-quality," they may match together in equilibrium without any specific hiring behavior driving the pattern. A related possibility is that workers with records accept lower wages and sort to low-wage firms, which are over-represented among young firms (Ma, Murfin, and Pratt, 2022). We find evidence against both interpretations. We see similar sorting of workers with criminal records to young firms regardless of the relative quality; for example, sorting to young firms is equally strong among young firms that survive longer and those that are large employers, suggesting that the patterns are not all about the lowest-quality firms employing the lowest-quality workers. Similarly, we see no evidence that workers with criminal records sort to young firms because they are cheaper workers — e.g., even when we compare workers earning very similar pay, those with criminal convictions disproportionately work at young firms. The pattern therefore reflects something about firm age, not merely firm size or quality. Given this, we next consider how labor supply- and demand-side mechanisms can explain *why* young firms provide disproportionately employment for workers with convictions.

We first establish that the sorting is likely driven by labor demand rather than labor supply in the form of worker preferences. While workers with convictions could, in principle, actively seek out employment at young firms — perhaps because young firms offer more flexibility or less regimented work — such preferences would likely reflect stable individual characteristics present before a conviction occurs (Fairlie, 2002). Our within-worker estimates rule this out, as the same worker becomes more likely to work at a young firm after a conviction, inconsistent with stable preferences driving the sorting. The literature further supports this interpretation: workers with records face severe barriers to employment and generally accept whatever jobs they can find, making selective search toward young firms unlikely (Pager, 2003; Western, 2002; Mueller-Smith, 2015; Sugie, 2018). Moreover, if workers with convictions preferred young firms, sorting should not depend on established firms' behavior — but our results below suggest otherwise.

Instead, labor demand — primarily in the form of young firms struggling to compete in the labor market — is pivotal. We observe notable differences in background check rates by young and old firms: in job postings data, young firms mention conducting background checks significantly less often than older firms, even when we account for job and firm characteristics. We consider three potential mechanisms to explain this different screening behavior. First, young firms may face *direct constraints* on their ability to screen and employ clean-record workers: limited HR sophistication or financial resources may make background checks and formal screening costly. Second, young firms may face *indirect constraints* from competition with established firms. With less visibility and weaker applicant flow, they may draw from a residual pool that established firms have already screened out (Moser, Smith, and Jones, 2017; Hurst, Lee, and Frake, 2024; Leung, 2003; Chung and Parker, 2023). Consistent with this view, younger high-growth startups receive fewer applications per vacancy, especially in downturns (Campero and Kacperczyk, 2020; Bernstein,

Townsend, and Xu, 2024). Third, young firms may simply have *preferences* for workers with criminal records, deliberately seeking workers with records whom other firms overlook — perhaps because these workers are loyal, risk-tolerant, or possess undervalued skills that traditional screening misses. Part of this preferences story may also reflect networks: if managers of young firms know — and trust — more individuals with criminal records, they may be more likely to hire them.

Our evidence points strongly toward indirect constraints as the primary mechanism, while direct constraints and young firm preferences play limited roles. We first show that direct constraints cannot fully explain the sorting. In job postings data, young firms' lower background check rates persist even after controlling for measures of HR sophistication, suggesting that young firms do not screen less simply because they lack the capability. Financial constraints matter: young firms in industries with lower startup capital costs and those with greater local access to small business loans are both more likely to conduct background checks, suggesting that financial capital enables more extensive hiring and screening practices. However, this attenuation is partial — the gap in background check rates between young and established firms persists even among young firms with greater financial slack or capital access, suggesting that financial constraints alone cannot account for the sorting we document.

Instead, we find substantial evidence for indirect constraints — young firms employ workers with convictions because they struggle to compete against established firms for workers. Young firms receive fewer applications and fill positions more slowly than established firms, leaving them to hire from a residual pool that established firms have already filtered (Campero and Kacperczyk, 2020; Bernstein, Townsend, and Xu, 2024). The composition of this pool depends on how aggressively established firms screen: when established firms screen intensively, more workers with records accumulate in the residual pool; when they screen less, fewer such workers filter down.

This mechanism generates specific predictions that we confirm in the data. First, sorting should be strongest for crimes that established firms screen against most aggressively. We find that sorting to young firms is strongest for workers with felony and property crime convictions — crimes that employers particularly avoid (Cullen, Dobbie, and Hoffman, 2023) — and weakest for less-stigmatized offenses like DUIs and disorderly conduct. Second, sorting should weaken when established firms' ability to screen is restricted. We find that the sorting of workers with records to young firms is substantially weaker in jurisdictions with Ban-the-Box policies, which prevent employers from inquiring about criminal history early in the hiring process. This pattern is driven by Black men with criminal records, for whom Ban-the-Box policies appear most binding (Agan and Starr, 2018). Third, sorting should weaken when established firms' willingness to screen declines — whether because labor market conditions force them to compete harder for workers or because local labor markets are generally more "friendly" to workers with records. Consistent with both predictions, sorting is weaker in tight labor markets, where employers compete aggressively for workers and lower their hiring standards, and in counties where the wage penalty to having a conviction is lower, indicating that established firms in those markets screen less intensively against this population.

These patterns are difficult to reconcile with young firm preferences driving the sorting. If young firms genuinely valued workers with records, sorting should not depend as much on established firms' screening behavior — yet it weakens precisely when established firms are constrained from screening. Nor is a

young firm preferences story consistent with the distribution of sorting across offense types and demographic groups: if young firms actively sought workers with records, we would expect sorting to be strongest for minor offenses and uniform across demographic groups. Instead, sorting is strongest for felonies and concentrated among minorities. Our evidence thus suggests that young firms hire workers with criminal records not because they intentionally target or accept these workers, but because they draw from whoever remains after established firms have already filtered the applicant pool.

In the final part of our paper, we explore the consequences of these matches. Does working at a young firm benefit workers with convictions? We find that workers with convictions who work at young firms have worse outcomes than those at established firms, showing higher rates of future non-employment and higher recidivism. However, comparing young firm workers to established firm workers may not reflect the relevant counterfactual. Our evidence suggests that workers with records end up at young firms precisely because established firms screened them out. For these workers, the realistic alternative is likely non-employment, not an established firm job. Relative to this benchmark, young firm employment may provide modest but meaningful benefits: workers with records at young firms have somewhat lower recidivism rates and substantially higher future employment rates than non-employed individuals with records. These potential benefits depend on firm stability: workers at young firms that ultimately survive fare better, while those at firms that quickly fail see outcomes that rival the non-employed. Yet, these descriptive patterns reflect a combination of negative selection — workers with the worst prospects are more likely to be selected into young firms or non-employment than established firms — and any treatment effects of the jobs themselves. We cannot fully disentangle these forces with our individual-level data.

To get at the causal effects of young firm employment on outcomes for workers with convictions, we exploit county-level variation in young firm employment using a Bartik instrumental variable strategy based on historical industry composition. We find that counties with exogenously higher young firm employment have higher employment rates for individuals with criminal records, with no much smaller effects on overall county employment rates — confirming that young firms absorb this population specifically, rather than simply reflecting better general economic conditions. Recidivism rates, however, are not meaningfully lower. Together these results suggest that young firms function as a labor market access point for workers whom established firms screen out, but that the jobs they provide are often too unstable to durably reduce criminal behavior.

This paper contributes to the literature on the constraints that young firms face and the role these firms play in the economy. Despite young firms' importance in the economy (Haltiwanger, Jarmin, and Miranda, 2013; Glaeser, Kerr, and Kerr, 2015), most young firms are unprofitable (Robb and Robinson, 2014) and financially constrained (Adelino, Ma, and Robinson, 2017; Guo and Wallskog, 2025). As discussed above, young firms are also constrained in their hiring practices; yet, not much is known about *who* these young firms hire based on their constraints. An exception is Ouimet and Zarutskie (2014), who show that young firms tend to hire young workers, arguably because young workers possess skills and risk tolerance that matches well with young firms' profiles and because they simply are more often looking for jobs. Our work demonstrates how the constraints that young firms face, especially in the face of competition from older firms, can lead them to employ marginalized workers (controlling for worker age and other characteristics).

Our story of young firms providing jobs for disadvantaged workers is broadly in line with Fackler et al. (2019), who document that young German firms generate low-paying jobs for workers that established firms may avoid — namely older, foreign, and recently unemployed individuals. By leveraging variation in when workers with criminal records sort more to young firms, we develop a better understanding of the mechanisms behind this type of job creation.

This paper also contributes to research on criminal justice involvement and labor market inequality, demonstrating young firm employment as a potential route for economic integration for individuals with criminal records.[2] Existing studies on the barriers to employment faced by individuals with criminal records have typically treated employers as homogeneous, rarely exploring *which* employers are more receptive to hiring workers with criminal records (e.g., Pager, 2003; Holzer, Raphael, and Stoll, 2003, 2006; Mueller-Smith and Schnepel, 2021; Western, 2002; Decker et al., 2015; Mueller-Smith, 2015). Our findings address this gap by highlighting young firms as potential providers of job opportunities for this population, challenging the prevailing narrative of uniform exclusion from employers.

More broadly, our findings suggest that competitive market dynamics can generate labor market inclusion as a byproduct, without deliberate policy intervention. Workers with records constitute a population that classical models of statistical discrimination predict will face persistent exclusion — employers cannot easily distinguish productive workers with records from genuinely risky ones, and so screen out the group (Phelps, 1972). Our paper identifies a structural feature of labor markets that partially disrupts this equilibrium: young firms, by drawing from the residual pool that established firms screen out, provide employment to workers who would otherwise be excluded. Whether this employment generates durable improvements in workers' labor market positions — by building work histories that future employers can observe — is a question we take up in the conclusion.

The remainder of the paper is organized as follows. Section 2 provides details of our data construction, as well as basic descriptive statistics. Section 3 presents our baseline results, documenting that workers with convictions sort to younger firms. Sections 4 and 5 discuss the mechanisms underlying this pattern and its implications. Section 6 concludes.

2 Data

In order to study the link between young firm employment and criminal records, we combine several administrative datasets housed by the U.S. Census Bureau. Throughout, we focus on individuals aged 18 to 65 years old, living and working in four states (Arizona, Maryland, New Jersey, and Wisconsin) between 2010 and 2017.[3] We link these datasets using longitudinal person identifiers provided by the Census.

In addition, we draw from online job postings collected by Lightcast (formerly Burning Glass), an employment analytics and labor market information firm. Lightcast aggregates job postings from more than 50,000 online sources — including vacancy aggregators, government job boards, and employer websites — and reports coverage of a "near-universe" of online postings (more than 385 million job postings) in the

[2] A related literature has illustrated that young firm *entrepreneurship* and self-employment is also relatively common for these individuals (Hwang and Phillips, 2024; Finlay, Mueller-Smith, and Street, 2023).

[3] These four states comprise the intersection of states for which we have both criminal justice conviction and employment data for 2010-2017. The New Jersey data covers felonies but not misdemeanors; the three other states cover both (Finlay and Mueller-Smith, 2021).

United States from 2010 to 2025 (Hansen et al., 2023). For each job posting, we observe the plain text of the job advertisement along with metadata such as the posting date, employer name, occupation, location, and industry. We use the job posting data in two ways. First, within the four-state 2010–2017 window that aligns with our administrative data, we link postings to our main analytic setting at the county-level to generate variables on labor market tightness. Second, in job postings-only analyses using the full Lightcast database from January 2010 to December 2024, we compare recruiting and screening practices between young firms and established firms more broadly.

2.1 Data components

We have three key data components that underlie our main analysis: criminal justice records, employment histories, and individual demographic and residence information.

2.1.1 Measuring criminal justice histories

We measure individuals' criminal justice outcomes, namely conviction histories, through the Criminal Justice Administrative Records System (CJARS) program. The CJARS program collects comprehensive data on the U.S. criminal justice system, with data linkages that allow us to identify and track individuals with criminal records before and after their criminal justice experiences in other Census data. Throughout our paper, we consider two measures of criminal justice experience: whether an individual has been convicted of any crime in the past seven years, and the relative timing of their conviction (i.e., in an event study framework). For the former measure, we focus on the past seven years to capture recent criminal justice contact and to align with common lookback horizons used in background screening in many employment contexts.[4] We refer to convictions in the past seven years as "recent" convictions. We also use the CJARS data to study recidivism, by measuring future convictions.

2.1.2 Measuring employment

We measure individuals' employment by combining two Census Bureau data sources. First, we measure workers using the Longitudinal Employer-Household Dynamics (LEHD). The LEHD is a quarterly matched employer-employee database that allows researchers to observe the labor earnings of workers at most employment firms within the U.S. The LEHD provides an entry year for each firm, from which we construct firm age; we one-index, such that a new firm is age 1.

Second, to build out our set of "potential" workers, we measure self-employment using the Integrated Longitudinal Business Database (ILBD). The ILBD contains information on all businesses without employees (e.g., sole proprietorships) in the United States, mostly sourced from Schedule C filings; starting in 2007, the ILBD identifies the owner of each firm. While our focus in this paper is on the workers (i.e., those in the LEHD), the ILBD helps us identify individuals who *could* be working. As described below, we fill in our LEHD-ILBD dataset for individuals who are neither working nor self-employed.

In both the LEHD and ILBD, we identify individual's industries (6-digit NAICS codes) and locations (counties).[5]

[4] Record visibility can vary across states, employer policies, and screening vendors. Misdemeanors typically have a lookback horizon of five years.

[5] For LEHD firms with multiple locations, the county we take is the county with the most employment for the firm in a sector-

2.1.3 Measuring demographics and home county

We source individuals' demographics and residence locations from two datasets that are part of the LEHD database. We observe individuals' dates of birth, from which we calculate age, sex, race, and ethnicity. Note that the Census imputes demographics for some individuals; we restrict our entire analysis to individuals with non-imputed birthdates, sex, race, *and* ethnicity.[6]

We additionally identify the county in which each individual resides in a given year using a residence dataset within the LEHD. We use this information both to improve identification (by comparing individuals in the same county) and to study the role of the local labor market in affecting outcomes. This information is not always available for all individuals we consider (in particular, those with no current employment measured in the LEHD). In these cases, we identify the next available county (i.e., looking backwards and forwards in time) for the individual. In case we never observe an individual's residence (e.g., because they never appear in the LEHD and instead only appear in the ILBD), we assume their home county is the same as their firm's county in the ILBD.[7]

2.1.4 Measuring barriers to employment

When we turn to studying mechanisms, we consider three county-year-level proxies for barriers to employment for workers with recent criminal convictions.

First, we consider whether a county enacted a Ban-the-Box policy or has any city with a Ban-the-Box policy in place. A Ban-the-Box policy prohibits employers from conducting criminal background checks or inquiring about criminal histories until later in the hiring process — typically at the time of the conditional job offer. In the 2010 to 2017 window for our sample of states, 15 cities and counties have Ban-the-Box policies. We construct a county-year indicator that takes a value of 1 if Ban-the-Box policy was enacted that year or in any of the prior years, and takes a value of 0 otherwise.[8]

Second, we consider the general "friendliness" of the local labor market to workers with convictions by measuring the pay gap between workers with and without recent convictions at the county-year level.[9] Third, we consider a county's labor market tightness using job postings from the Lightcast data. We estimate the labor market tightness by dividing the number of job postings in a county-year by the total employment

state pair (e.g., the county within Wisconsin with the most workers for a retail firm.). This calculation is performed by Census.

[6]The LEHD data contain non-imputed dates of birth and sex for approximately 95% of individuals and non-imputed race and ethnicity for approximately 80% of individuals. see `https://lehd.ces.census.gov/data/lehd-snapshot-doc/latest` for details.

[7]In rare cases, we observe individuals who have no LEHD residence information but never appear in the ILBD; instead, they do appear in the LEHD but without residence information. In this case, we assume their home county is their LEHD firm county.

[8]When information about the policy's effective date was available, we used that date as the start date of the policy; otherwise we used the date the policy was announced or passed by the legislature. We focus on Ban-the-Box policies that ban public employers from screening criminal records. While Ban-the-Box policies for both private and public employers have a wider reach in the labor market and a larger impact on individuals with criminal records (Rose, 2021), narrowing the focus to private ban-the-box policies within our time frame significantly reduces the size of the treatment group (4 counties or cities within our sample states - Baltimore, MD, Montgomery County, MD, Prince George's County, MD, Newark, NJ). In addition, prior work shows that private employers are likely to follow public employers' Ban-the-Box policies, validating the significance of examining the limited public Ban-the-Box policy (Jacobs, 2005). Given that the public Ban-the-Box policies are weaker policy interventions than private Ban-the-Box policies, we expect to find stronger results when examining the effects of the adoption of Ban-the-Box policies for private employers.

[9]We calculate this pay gap using all workers in a county-year, not just those in our random sample. The pay gap is calculated as the difference between the mean log earnings of clean-record workers and the mean log earnings of workers with recent convictions.

in that county year (calculated from the sample used for the pay gap variable calculation).

2.1.5 Measuring background checks

We measure the reported use of background checks in our job postings data by flagging when job posting ads explicitly mention background checks. Specifically, we generate an indicator variable at the job posting ad level that takes a value of 1 when the job posting text includes the language "criminal background check" or "background check," and 0 otherwise.

2.2 Samples

We combine our various data components in order to study how criminal histories predict and affect young firm employment outcomes. We have two main samples.

2.2.1 Full sample

Our full sample consists of individuals for whom we measure at least one year of traditional work or self-employment in the 2010 to 2017 window. To be clear, we construct our sample in the following way.

First, we take earnings histories for individuals in our states (Arizona, Maryland, New Jersey, and Wisconsin) in the LEHD between 2010 and 2017.[10] For each individual, we take their highest-paying job within the year (and state) and flag this as their main employment; this means that our analyses below study whether an individual's top-paying job in a year is at a young firm. Second, we merge in records from the ILBD in order to flag individuals as self-employed individuals.[11]

The resulting sample from this merge contains "missing" years for individuals who are neither working (i.e., in the LEHD) nor self-employed (i.e., in the ILBD) in a given year. We interpret these missing years as "non-employment" and interpolate these years into our sample (i.e., "fill-in" the missings).[12] To this filled-in sample we merge in demographic and residence information described above.

Finally, we apply three sample restrictions. We restrict to individuals between the ages of 18 and 65; this means that, even though we filled in missing observations, we do not consider the employment outcomes of, e.g., a 12-year-old who works in the later part of our sample as an adult. We additionally restrict to individuals whose home counties (described above) are covered by CJARS in order to increase the likelihood that we accurately measure individuals' criminal histories, under the assumption that crimes happen close to home.[13] Finally, for computational tractability while maintaining statistical power, we take a random sam-

[10] Note that we sometimes control for lagged earnings below. For individuals in our earliest sample years, we measure their lagged earnings before 2010.

[11] We take the union of the LEHD and ILBD sample, such that individuals can be both self-employed or workers.

[12] While adding these observations is not innocuous, we believe it is important to acknowledge the fact that many people with convictions are not consistently working or self-employed; thus it is important to include these observations when making statements about the relative propensity to work and the average outcomes for individuals living in an area. While missing observations could instead reflect individuals having moved (temporarily or permanently) outside of our sample of states, working for a firm not covered by the LEHD (e.g., the federal government, some agricultural firms), or being retired or deceased, we believe some of these misclassifications concerns will be less relevant for the population of convicted individuals; for example, individuals on probation or parole typically must stay within their state. To cast a wide enough net to capture individuals who are persistently non-employed in our 2010-2017 window, we fill in non-employment quarters for individuals who are ever in the LEHD or ILBD in the 2007-2018 time window.

[13] CJARS lacks coverage in some small counties. This sample restriction also means that we drop individuals whose home counties lie outside our sample of states; this predominantly means removing individuals who live in a bordering state but work in one of our sample states (e.g., those who work in Wisconsin but live in Illinois).

ple: we keep a random 10% of individuals with no conviction histories in our data as well as all individuals with convictions in our sample.[14] To adjust for the imbalance (10% vs. 100% samples), we use weights in all analyses below; i.e., individuals with conviction histories receive weights of 1, while our randomly drawn individuals without conviction histories receive weights of 10.

Following this construction, our full sample consists of 16,640,000 individual-year observations, of which 4,375,000 have recent convictions within the past 7 years.

2.2.2 Workers sample

For much of our analysis, we focus on individuals in our full sample who are currently working for a firm. This sample allows us to consider where individuals work, conditional on having any job. Our workers sample consists of 11,260,000 individual-year observations, of which 2,575,000 have recent convictions within the past 7 years. Note that, by definition, this sample does not contain any of our "filled-in" missing individual-year observations.

2.2.3 Job postings sample

For additional analyses, we use the job postings data from Lightcast between January 2010 and December 2024, containing the full text of over 350 million online job postings. Note that we are unable to link the posting-level data to the Census data, and so our postings-level analyses are separate from our individual-level analyses.

We apply two sample restrictions. Unlike in our administrative data, we do not cleanly observe when a firm is established (i.e., when they would be age 1 in our Census data). Instead, we proxy this establishment or entry year with the year in which we first observe a job posting from that firm. Note that, because our job posting data begins in 2010, this proxy is naturally censored; to help address this, we restrict our analysis to job postings between 2015 and 2024 but use the data since 2010 when determining firm "age."

Second, for computational tractability while maintaining statistical power, we analyze all job postings from January 2015 to December 2024 for a random 30% of firms in the dataset. The resulting data covers more than 91 million online job postings, spanning more than 867,000 employers in 3,140 counties.

2.3 Summary statistics

Table 1 presents descriptive statistics for our two main Census samples, with our full sample in Panel A and our workers sample in Panel B, split by whether an individual has a recent conviction, i.e., in the past 7 years. Consistent with prior literature, individuals with convictions are substantially less likely to work, with a 61% employment rate compared to the 73% observed for individuals without convictions. Additionally, these individuals with convictions tend to be male (77% of the full sample, 76% of the workers sample), young, and non-White. In particular, individuals with convictions are disproportionately Black: among all individuals, 25% of individuals with convictions are non-Hispanic Black, compared to only 13% of individuals without convictions. This difference persists in the workers sample, where 22% of workers with convictions are Black, compared to 13% of those without convictions. These patterns align with a vast literature documenting racial disparities in criminal records (Pager, 2003, e.g.).

[14] To match our broader sample from which we fill-in non-employment spells, we take all individuals with a conviction sometime in the 2007-2018 window.

It is worth noting that incarceration rates — namely, the fraction of individuals who were in prison sometime in the past 7 years — are relatively low. Among individuals with recent convictions in our full sample, 10.6% of them have served a prison sentence in the past 7 years. Conditional on working, only 8% of the recently convicted individuals have recently served a prison sentence. This means that our paper is largely focused on individuals who are *not* "removed" from the labor market due a prison sentence; instead, these individuals arguably face barriers to employment due to their *criminal records*.

Figure 1 plots the sectoral distribution for our workers sample, split on recent conviction status. Workers with recent convictions can be found disproportionately in traditionally lower-skill sectors: construction (11% compared to 5% for workers without recent convictions), manufacturing (14% versus 9%), administrative and support and waste management and remediation services (e.g., temp work, security, and janitorial services; 17% versus 7%), and accommodation and food services (15% versus 7%).

3 Baseline fact: Individuals with convictions disproportionately work at young firms

We begin by documenting our baseline fact: workers with convictions disproportionately work at young firms. In Section 4, we discuss the mechanisms underlying this pattern.

3.1 Raw data

We start by considering the raw data: where do individuals with convictions work? Figure 2 presents the firm age distribution, describing where workers with and without convictions work. Panel (a) reports the separate distributions, while Panel (b) presents the relative shares by dividing the share of workers with convictions in a given firm age bin by the share of workers without convictions in that bin. As the figure shows, workers with convictions disproportionately work at young firms, particularly at the expense of not working at mature firms over the age of 30.[15] This pattern is most salient when we consider the very young firms under the age of 5: 12% of workers with convictions work at these new firms, while only 7% of workers without convictions do. By inverting these distributions, these patterns also imply that younger firms employ relatively more individuals with convictions: 5% of workers at firms under the age of 5 have a recent conviction, while only 2% of those at mature firms over the age of 30 do.

These patterns could arise for a variety of reasons. For instance, Figure 1 shows that workers with convictions more often work in sectors like construction and food services, which are more entrepreneurial and younger (Wallskog, 2025); this age pattern could be a reflection of sectoral sorting. Similarly, Table 1 shows that individuals with recent convictions tend to be younger, and younger firms tend to employ younger workers (Ouimet and Zarutskie, 2014). Below, we analyze these patterns systematically.

3.2 Regression analysis

Given that workers with recent convictions may sort to young firms for a variety of spurious reasons, we formalize these patterns in a regression analysis in which we control for demographic and economic characteristics. We begin with cross-sectional comparisons (i.e., comparing workers with and without convictions) before turning to within-worker patterns (i.e., comparing before and after a conviction).

[15] Firm age is censored, so we cannot decompose this oldest group more.

We start by estimating versions of the following model:

$$\text{Work at a young firm}_{it} = \alpha + \beta \text{Recent conviction}_{it} + \mathbf{X}_{it}\delta + \gamma_{c(i,t)t} + \gamma_{n(i,t)} + \gamma_i + \varepsilon_{it}, \quad (1)$$

where we regress an indicator for whether a worker i works at a young firm (under the age of 5) in year t on an indicator for whether they have a recent conviction in the last 7 years, with various controls. $\mathbf{X}_{it}$ is a vector of control variables varying at the individual level (e.g., sex, race, and ethnicity) and individual-year level (e.g., age or lagged earnings). Depending on the specification, we also include county-year ($\gamma_{c(i,t)t}$), sector ($\gamma_n(i,t)$), and individual (γ_i) fixed effects; $c(i,t)$ denotes worker i's county of residence in year t, $n(i,t)$ denotes the sector of i's highest-paying job in year t. Below, we walk through the implications of these controls as we build up to the full model. ε_{it} reflects idiosyncratic noise.

Table 2 presents our results. Across the board, regardless of controls, we find that workers with convictions are more often working at young firms. Column (1) presents a raw correlation (mirroring Figure 2): having a conviction predicts a 5.3 percentage point higher likelihood of working at a young firm. Given that 6.9% of workers work at young firms, this is a large disparity. In column (2), we include demographic controls, namely indicators for sex, race, and ethnicity and age fixed effects, to account for demographic patterns in both conviction records and employment at young firms; for example, to the extent that young workers are both more likely to have recent convictions and (perhaps unrelatedly) work at young firms, these controls account for that. Similarly, column (3) accounts for geographic variation in convictions and employment at young firms (e.g., through entrepreneurship rates) with the inclusion of county-year fixed effects. Column (4) accounts for the sectoral distribution differences highlighted in Figure 1, while column (5) adds as a control a proxy for a worker's skill; namely, we include discretized earnings from 8 years in the past (i.e., before any recent convictions) and include fixed effects for the bins.[16] Collectively, all of these controls reduce the coefficient on recent convictions, consistent with different demographic and economic characteristics correlating with both convictions and working at young firms; nonetheless, the role of recent convictions is still large. Conditional on demographics, county-year, sector, and past earnings, individuals with convictions are 3.1 percentage points more likely to work at young firms, a 45% increase relative to the mean.

The specifications through column (5) are predominantly cross-sectional, comparing individuals with and without convictions. Despite our controls, there may be a myriad of unobserved characteristics that still cause omitted variable bias. For example, perhaps some individuals are more risk-tolerant and consequently are both more likely to commit crime and work for a new firm. In this case, it is not the conviction that leads to working at a young firm. We tackle this issue in two ways. First, in column (6), we add individual fixed effects in order to remove average differences in proclivities to work at young firms.[17] Because there are individual-level characteristics we do not observe, the inclusion of individual fixed effects shrinks the coefficient on having a recent conviction, but the coefficient remains strongly statistically different from zero. Namely, within an individual, we estimate that having a recent conviction predicts a 0.9 percentage

[16] We pool all workers who have no available earnings information for 8 years in the past — e.g., because they were not an adult at that time — and include a separate fixed effect for them.

[17] We omit the fixed effects for past earnings in this specification.

point higher likelihood of working at a young firm, a 13% increase relative to the mean.[18]

Second, we conduct an event study around an individual's first conviction in our sample to explicitly study how working at a young firm changes following a conviction. We estimate the following model:

$$\text{Work at a young firm}_{it} = \alpha + \sum_{-4 \leq d \leq 3, \neq -2} \beta_d \mathbf{1}[t - \text{First conviction}_i = d] + \mathbf{X}_{it}\delta + \gamma_{c(i,t)t} + \gamma_{n(i,t)} + \gamma_i + \varepsilon_{it}, \tag{2}$$

where we estimate coefficients on event time indicators for the years before and after an individual's first conviction in our sample; for individuals without any convictions, these event time indicators are all zero. We omit as our base time two years prior to the first conviction, rather than only one, in order to remove spurious effects from, e.g., an arrest happening the year before the conviction. As in column (7) of Table 2, we include demographic controls (namely age fixed effects), county-year fixed effects, sector fixed effects, and individual fixed effects. We estimate this model using the methodology of Sun and Abraham (2021).

In order to ensure that our estimates are not driven by attrition from the workers sample, we require individuals to be working every year in the 2010-2017 window and to have a balanced sample in terms of event time; in practice, this means we compare e.g., workers who have their first conviction in 2014 to workers who never have a conviction. This naturally creates positive selection, as it removes individuals for whom a conviction leads to inconsistent employment. This sample restriction leads to somewhat lower average rates of working at young firms (4.6%), consistent with young firms hiring less consistently employed individuals. Nonetheless, we feel that this restriction is necessary in order to cleanly interpret the event study. It turns out below that we estimate post-conviction coefficients similar in magnitude to those in column (6) of Table 2, which is reassuring.

Figure 3 presents the results: following a conviction, individuals see a 1 percentage point increase in the rate of working at young firms, relative to two years before their conviction, a 21% increase relative to the mean. This increase persists across the following three years, with a slight decay over time (which could be due to their young firm aging out of being "young"). We see limited evidence of any differential pre-trend in working at young firms leading up to an individual's conviction, with no coefficients statistically distinguishable from the omitted year (two years prior to the conviction). As mentioned above, we expect to see some potential increase in the year prior to the conviction, as an individual's crime and arrest may occur in that year; to the degree that these may already appear on an individual's criminal record the year before their conviction, individuals may already move to young firms in this year. Indeed, in that year, we see a coefficient of 0.0035 (compared to the year-of-conviction coefficient of 0.0097) that is not statistically different from zero but suggestive of a partial change in that year.

We consider several robustness analyses to support our findings. We consider alternative measures of working at a young firm; when measuring young firms as newly established firms (i.e., age 1 firms), we find consistent results that workers with convictions work disproportionately for these firms (Table A.1). When

[18] Table A.3 presents an analogous version of Table 2 where we consider how convictions predict working; unsurprisingly given Table 1 and the vast literature on crime and labor markets, we find that individuals with recent convictions have substantially lower employment rates.

splitting out measures of working at a young firm as an employee (i.e., not at the top earner of their firm) versus a likely manager (i.e., the top earner of the firm), we find that our patterns are driven by the former (Table A.1).[19] Furthermore, the sorting patterns are not simply about workers with convictions being cheap labor, as our findings hold after controlling for current earnings (column (1) of Table A.2, where we add fixed effects for current earnings percentile).

Nor are the sorting patterns entirely about workers with convictions sorting to low-quality young firms that exit immediately — i.e., the pattern is not about "low-quality" workers just sorting to "low-quality" firms: when we split young firms on whether they are "successful" (i.e., survive to age 5), we see sorting of workers with convictions to both types of young firms (columns (2) and (3) of Table A.2). Having a recent conviction predicts a 18% higher likelihood of working at an *unsuccessful* young firm, relative to the mean, and a 12% higher likelihood of working at a *successful* young firm, relative to the mean. Furthermore, as we discuss below, the sorting patterns appear across a variety of firm sizes and sectors; if our results were driven purely by assortative matching, we might expect the results to only exist for the lowest-quality firms (e.g., those that do not employ many people.). This is not the case.

Finally, it could be the case that our sorting patterns are simply driven by workers with convictions changing jobs more often (as in the case of young workers in Ouimet and Zarutskie (2014)); i.e., if workers with clean records are not changing jobs as often, they cannot end up at young firms as often. However, when we restrict to workers who are changing jobs, we continue to see our patterns, with having a conviction predicting a 8% higher likelihood of working at a young firm, relative to the mean (column (4) of Table A.2).

3.3 Heterogeneity

How common are these patterns? We conduct several heterogeneity analyses.

First, we consider variation by sex and race/ethnicity. In Table 3, we augment the full model (1) to estimate sorting by demographic group. In column (1), we add interaction terms between having a recent conviction and indicators for sex, race, and ethnicity. In columns (2)-(7), we estimate model (1) on sex-by-race/ethnicity subsamples (e.g., White men, Black women, etc.). Two patterns emerge: this sorting appears across all of these demographic groups, and the sorting is stronger for Black individuals and Hispanic women. White men and women are 12% and 11% more likely to work at young firms, respectively, when they have had a recent conviction, relative to those groups' average rates. Meanwhile, these values are 14% and 20% for Black men and women, respectively, and 7% and 16% for Hispanic men and women, respectively. These findings suggest that while young firms offer disproportionate employment opportunities to individuals with criminal records across the board, the effect is particularly pronounced for racial minorities—and especially for women of color. Given prior research showing that employers are especially reluctant to hire racial minorities with criminal records (Pager, 2003; Holzer, Raphael, and Stoll, 2006), our results imply that young firms may play a unique role in absorbing the individuals who are most stigmatized and screened out by established employers. We directly examine this mechanism in Section 4 below.

Second, we consider variation by firm size. This is important, as our argument is that individuals with convictions disproportionately work at *young* firms. It could be that the pattern is actually about working at *small* firms (which young firms often are), which would have different implications. We address this in

[19] Note that we rank individuals at firms before restricting to their top-paying jobs in order to perform this analysis.

two ways. We begin by estimating model (1) and controlling for firm size, which we do by including fixed effects for employment deciles; with this modification, we estimate a statistically significant coefficient of 0.006. This is a reduction from the coefficient of 0.009 in column (6) of Table 2, suggesting that firm size may indeed be part, but not all, of the story. We then turn to estimating model (1) for different firm size groups; we report results in Table A.4. For each firm size bin, we provide estimates for regressions with and without individual fixed effects. While the version with individual fixed effects remains our preferred specification, note that in this context the fixed effects have a slightly perturbed role, as an individual only has variation in working for a young firm if they work at multiple firms in the firm size bin (or, if they work at a young firm that ages out of our "young" classification). Across all four firm size bins we consider (under 20, 20-49, 50-99, and over 100 employees), we find that workers with convictions more often work at young firms than workers without convictions (odd columns). For instance, conditional on working at a firm with fewer than 20 employees, workers with convictions are 3.6 percentage points more often working at young firms; among workers at firms with at least 100 employees, this gap is 2.1 percentage points. With the exception of the smallest bin, these patterns persist with the inclusion of individual fixed effects (even columns). We conclude that the fact that workers with convictions disproportionately sort to young firms is not wholly reflecting them sorting to small firms.

Third, we consider variation by firm sector. Recall that our main specification (column (6) of Table 2) controls for sector fixed effects, such that our findings are not driven by individuals with convictions sorting to younger sectors. As with firm size, we additionally explore variation by estimating model (1) separately for 10 sector groups.[20] Table A.5 presents results, again with versions with and without individual fixed effects. As in the case of firm size groups, versions with individual fixed effects should be interpreted with some caution, as variation in working at a young firm within an individual here requires working at multiple firms in the same sector (or working at a young firm that ages out of our "young" classification). In our cross-sectional regressions (odd columns, no individual fixed effects), we find evidence of workers with convictions sorting to young firms in every sector. When we consider within-individual variation (even columns, with individual fixed effects), these patterns persist with statistical significance in trade and transportation, professional services, education and health services, and leisure and hospitality. These patterns suggest that the sorting patterns are pervasive across the economy.

4 Mechanisms

Why do workers with criminal records sort to young firms? In this section, we discuss and provide evidence for several mechanisms. We argue that workers with convictions end up at young firms because constrained young firms struggle to compete with established firms and consequently create fewer barriers to employment than their older counterparts. We organize our evidence in four parts. First, we establish that the sorting is driven by labor demand rather than worker preferences (i.e., labor supply). Second, we present and discuss potential labor demand mechanisms. Third, we rule out alternative explanations based on young firms' lack of hiring capability or preferences for workers with criminal records. Fourth, we provide direct evidence for the competition mechanism.

[20] Note that in our specifications, we include fixed effects for sector defined as 2-digit NAICS codes. Here, to keep this exercise parsimonious, we separate on sector groups that pool the 20 2-digit NAICS codes into 10 sector groups.

4.1 Labor demand vs. labor supply

Before examining why young firms hire workers with records, we must establish which side of the market drives the sorting. While the sorting is ultimately an equilibrium outcome, we can consider key labor supply and labor demand factors in turn. Under a *labor supply* story driven by worker preferences, workers with convictions actively prefer young firms — perhaps because these firms offer more flexibility, less bureaucracy, or environments more tolerant of nontraditional backgrounds. Under a *labor demand* story driven by young firm preferences or constraints, young firms' hiring behavior generates the sorting, either because they actively seek workers with records or because their constrained hiring processes lead them to employ these workers.

Several pieces of evidence point toward worker preferences not driving the sorting. First, the extensive literature on criminal records and employment documents that workers with convictions face severe barriers to finding any job and generally accept whatever employment they can obtain (Pager, 2003; Western, 2002; Mueller-Smith, 2015). In other words, this makes it unlikely that these workers are selectively choosing young firms over available alternatives at established employers.

Second, our within-individual estimates (Table 2, column 6) show that the *same worker* is more likely to work at a young firm after acquiring a conviction. Insofar as the characteristics and preferences that incline workers with criminal records towards working for young firms, such as preference for flexible or less-regimented work (e.g., firms with flexible work schedules or less bureaucracy) or higher risk-tolerance (Fairlie, 2002; Sauermann, 2018), are stable worker characteristics that are found in these workers *before* they are convicted of crimes, we would not expect convictions to alter where the *same worker* works.

Third, and most directly, we leverage job posting data to establish that young firms behave differently in their hiring processes in ways that could generate the sorting — young firms' labor demand appears crucial. We analyze job posting data from our sample of 91 million job postings from January 2015 to December 2024. Within these postings, we identify explicit mentions of criminal background checks and use them as a proxy for whether firms screen out individuals with records. Specifically, our goal is to test whether younger firms report conducting such background checks less frequently, compared to older and more established firms. To do this, we estimate job posting-level models of the following form:

$$\begin{aligned}\text{Job posting mentions a background check}_p = \alpha + \beta\,\text{Young firm}_{j(p)ct(p)} \\ + \gamma_{ct(p)} + \gamma_{n(p)} + \gamma_{o(p)} + \gamma_{s(j(p))} + \varepsilon_p,\end{aligned} \tag{3}$$

where p is a job posting for a job at firm j first posted in county-year-month ct; n is the 2-digit NAICS industry in which the firm operates; o denotes the occupation associated with the posting; and s denotes the firm's size bin. The γ's represent county-year-month, industry, occupation, and firm size fixed effects. Our focus is to estimate β, the coefficient capturing any gaps in background checks between positions at young (i.e., age under 5) versus established firms.

In our analyses, we additionally consider a specification in which we separate the young firm indicator into separate indicators for firm ages 1 through 5. We consider several specifications with varying controls, capturing labor market patterns to criminal background checks (with county-year-month fixed effects), job

characteristics (with 3-digit ONET occupation fixed effects), and firm characteristics (with 2- digit NAICS industry and firm size bin — defined by deciles of number of job postings — fixed effects). Because the data are at the job-posting level, and older or larger firms naturally contribute more job postings, we weight all regressions by the inverse of the number of job postings a firm generates in a given year. Doing this allows us to estimate coefficients that provide firm-level interpretations (i.e., whether the average job at a young firm is described differently than that of an older firm) while residualizing against job-level characteristics.

Table 4 presents our results. Columns (1)-(5) estimate the gap in background checks between young and established firms, while column (6) estimates the gaps between age 1 through 5 firms separately, compared to established (age 6 and over) firms. Across specifications, we find that job postings at younger firms mention background checks less frequently than those at older firms. In our preferred specification (column 5), which includes county-year-month, industry, occupation, and firm size fixed effects, we estimate that job postings at young firms mention background checks 0.4 percentage points less often, compared to firms over age 5 — a 5.5% lower rate of mentions of criminal background checks for jobs at young firms, relative to the mean. When separating out the young firm indicator to specific firm age in column (6), we find evidence that this gap is driven predominantly by the youngest firms: job postings at new firms mention background checks 0.9 percentage points less often, compared to firms over age 5, a 12% lower rate relative to the mean.[21,22]

We take these results as indicating that young firms screen out workers with criminal backgrounds less often than older firms, which may allow for our main findings that workers with criminal records disproportionately find jobs at young firms. While workers with criminal backgrounds may internalize this behavior by young firms and subsequently direct their labor supply to them — i.e., the sorting is an equilibrium outcome — we believe the labor demand of young firms is the first order consideration to understanding the sorting.[23] We now turn to understanding *why* young firms screen less.

4.2 Potential labor demand mechanisms

Given that young firms seemingly behave differently when hiring in ways that may lead them to hire more workers with criminal records, we now consider *why* they do so. What is special about young firms when they try to hire workers that leads them to hire more workers with criminal records? We consider three broad categories of explanations: direct constraints on young firms' ability to screen, indirect constraints arising from competition with established firms, and preferences of young firms. In Section 4.3, we provide empirical evidence.

Direct constraints. First, young firms may face direct constraints in their capability to screen out

[21] Mentions of criminal background checks may be driven by firm entry year cohort effects, especially given our data time frame includes firms that entered during and after the COVID-19 pandemic. In order to rule out that these patterns do not simply reflect firm cohort patterns, we confirm (in untabulated results) that our results are consistent when we split our sample into two cohorts of 2015-2019 and 2020-2024.

[22] It is worth noting that young firms may still be conducting background checks, even if they do not mention them in their job postings. However, given our main results that younger firms disproportionately hire workers with criminal background checks, we do not believe this potential difference between job posting behavior and actual practice fully diminishes the estimated gaps in Table 4.

[23] As far as we know, young firms do not have a reputation as begin "second chance" employers. For example, existing policies that encourage hiring of workers with criminal records, such as the Work Opportunity Tax Credit, are disproportionately less often utilized by smaller — and likely younger — firms (Hamersma, 2011).

applicants with criminal records, even if they would prefer to do so. This incapability can manifest in two ways: HR capability and financial capability.

Young firms may lack the *HR capability* to conduct criminal background checks. Screening requires knowing that background checks exist, understanding how to procure them, and integrating them into hiring workflows. Young firms — particularly those founded by first-time entrepreneurs without corporate HR experience — may simply not "know" to screen, or may lack the organizational infrastructure to do so consistently. In this view, young firms hire workers with records "blindly" because they are unsophisticated, not because they are indifferent to criminal histories.

Young firms may also lack the *financial capability* to afford background checks and employing clean-record workers. Criminal background checks cost money, both directly (fees to consumer reporting agencies) and indirectly (staff time, delayed hiring), and workers with clean records tend to be paid more. Young firms tend to have low financial capital access, as they are likely constrained to using the personal savings of the entrepreneurs or bank loans (Robb and Robinson, 2014). This means they likely cannot afford to hire expensive workers, nor can they afford to engage in expensive hiring practices, such as extensive recruiting and heavy vetting of workers. Because criminal records are not automatically observable for firms, they have to pay — e.g., conduct a background check — in order to screen out workers with criminal records. Collectively, this means that young firms facing financial constraints may skip any extensive screening and instead hire relatively cheap workers, and thus end up hiring more workers with criminal records.

Indirect constraints. Second, beyond these direct constraints, young firms may face indirect constraints that lead them to hire workers with records even when they have the HR or financial capability to screen. These indirect constraints arise from competition with established firms for workers, which make screening less practical for young firms.

Young firms face a competitive disadvantage in labor markets. Workers looking for jobs do not randomly apply to firms; instead, they likely apply to firms they know or firms whose job ads they observe. If older firms are more "visible" in the economy — because they have had longer times to build their public brands — then younger firms will receive fewer applications per job position (Leung, 2003; Campero and Kacperczyk, 2020, e.g.,). In addition, even conditional on having interest from applicants, younger firms likely struggle to compete to successfully hire workers, given that young firms may not be able to afford high wages, as mentioned above, or to offer stability, since many young firms go out of business quickly.

The key insight is that screening is only valuable if a firm has good alternatives to the screened-out applicants. When an established firm with a strong employer brand conducts a background check and rejects an applicant with a record, another qualified candidate is likely waiting. The firm simply hires from the remaining pool. But when a young firm screens out an applicant, the alternative may not be a better candidate — it may be no candidate at all.

The competitive disadvantage that young firms face in labor markets means that young firms may rationally choose not to screen on criminal records, not because they lack the capability, but because screening would leave positions unfilled. In effect, young firms hire from a *residual pool* of applicants that established firms have already filtered. Consider how job search unfolds for a worker with a criminal record: they apply to multiple firms, both established and young. Established firms, with greater resources and larger applicant

pools, screen candidates and reject those with disqualifying records. These rejected workers do not exit the labor market, but they continue searching and applying elsewhere, including to young firms. Young firms, facing thin applicant pools to begin with, end up hiring from whoever remains available.

The composition of this residual pool depends on how aggressively established firms screen. When established firms screen intensively against a particular type of offense or race group, more workers with that offense accumulate in the residual pool that young firms draw from. When established firms screen less—whether due to policy constraints, labor market pressure, or lower stigma attached to certain offenses—fewer such workers filter down to young firms. Under this mechanism, young and established firms may have identical preferences over worker characteristics, yet behave very differently: established firms screen because good alternatives are plentiful, while young firms do not screen because their alternatives are poor.

Firm preferences. Third, young firms may hire workers with criminal records because they actively prefer these workers, seeing something valuable in them that older firms miss or do not appreciate.[24] Young firms may eschew traditional hiring practices if they have different aims when hiring. For instance, established firms may be concerned with risk-management — i.e., avoid hiring workers with convictions out of fears that they will steal from the company or hurt customers — while younger firms may prioritize finding hard-working and loyal employees and subsequently favor demonstrated skills over traditionally attractive resumes (Moser, Smith, and Jones, 2017). Similarly, managers of young firms may simply know — and trust — the individuals with criminal records; these network effects could induce sorting.[25] Under this firm preferences mechanism, we would expect young firms' hiring to not depend on what established firms do.

Collectively, these different demand-side considerations mean that young firms may end up hiring more workers with criminal records, due to direct constraints in ability, indirect constraints from competition, or preferences. These considerations also mean that we should see stronger sorting of workers with convictions to younger firms when these younger firms are less sophisticated, more financially constrained, and face more competition from established firms.

4.3 Evidence of labor demand mechanisms

Given the potential mechanisms laid out above, we next conduct empirical tests to consider the role of direct constraints (HR constraints and financial constraints), indirect constraints (competition with older firms), and firm preferences. Throughout, we blend evidence from our worker-level Census data and our job postings data.

4.3.1 Direct constraints

HR constraints. As discussed above, one reason why young firms may hire more workers with criminal records due to constraints in their HR sophistication or resources. Young firms may screen less simply because they lack the HR sophistication or professionalism to know they "should" screen, or because they

[24] Young firms could even tacitly approve of the risk-taking behavior that led to criminal convictions; as Griffin, Kruger, and Maturana (2019) argue — in the context of financial misconduct leading up to the Global Financial Crisis (GFC) — that bankers who signed residential mortgage-backed security deals that had high loss or misreporting or were the subject of lawsuits faced few penalties in terms of finding jobs after the GFC — arguably because upper-management in the banking sector approved of this risky behavior.

[25] Notably, there is likely limited scope for network effects involving the managers themselves having criminal records, as individuals with criminal records are not particularly likely to be managers (i.e., top earners) at young firms (Table A.1).

cannot afford to do so. If HR constraints drive the sorting, then the young firm gap in hiring workers with records should attenuate when we control for measures of HR sophistication or resources. Young firms with dedicated HR staff or sophistication should behave more like established firms.

We test for this possibility in our job postings data by re-estimating model (3), controlling for various measures of HR constraints and sophistication. We measure HR sophistication by examining whether the firm has previously hired for HR occupations.[26] As we show in Table 5, regardless of our measure of HR sophistication, our estimated coefficients on being a young firm remains largely unchanged. These results are consistent with the fact that we saw sorting of workers with criminal convictions to young firms *regardless of firm size* (Table A.4); if sophistication generally scales with firm size and HR sophistication was a key driver of our sorting patterns, then these patterns would likely be accounted for by firm size rather than age, which is not the case.

We take these patterns as suggestive that young firms do not screen less on criminal backgrounds simply because they are constrained in their HR capability. Instead, something else is at play.

Financial constraints. We next turn to consider the role of financial constraints: if conducting elaborate recruiting and hiring practices — including conducting criminal background checks — is expensive, then cash-constrained young firms may choose to forgo such practices. Similarly, cash-constrained firms may intentionally choose to hire workers with criminal records in order to save on wages, if it is feasible to pay these workers less. If financial constraints drive the sorting, then the young firm gap in hiring workers with records should attenuate when we control for measures of financial access. Young firms with greater access to capital should behave more like established firms.

We explore these possibilities in both our job postings and worker-level data. First, we expand model (3) to explore whether mentions of background checks changes when there is more capital access for young firms, measured in two ways. First, we use a measure of industry-level average start-up cost (Adelino, Schoar, and Severino, 2015), where young firms in industries with higher average start-up costs will face greater capital constraints. As shown in Column (5) and (6) of Table 5, we see that young firms continue to disproportionately mention background checks less often when we control for the industry-level startup capital costs. However, when we estimate heterogeneity by the amount of startup capital costs by industry, we find that the gap in background check mentions between young and old firms reduces in industries where startup capital cost is lower. In other words, when young firms are less financially constrained in terms of less startup capital costs, they conduct more background checks, consistent with these constraints being important in hiring practices.

Second, we measure capital access for young firms by examining the volume of Small Business Association (SBA)-backed loans in a given county-year. These loans are made by private banks to small businesses — while being backed by the SBA, a federal agency — and so may reflect the ease of capital access for young firms in a location.[27] As shown in Column (7) and (8) Table 5, we see that young firms continue to disproportionately mention background checks less often when we control for the local capital supply.

[26] We also use alternative measures of HR sophistication such as whether the job posting itself refers to Human Resources or "looks" more sophisticated (i.e., is longer), and find consistent results.

[27] We pool two SBA loan programs together: 7(a) loans and 504 loans. See details at https://www.sba.gov/funding-programs/loans/.

However, when we estimate heterogeneity by the amount of SBA loans, we find that the gap in background check mentions between young and old firms reduces when SBA lending is higher. In other words, when young firms are less financially constrained, they conduct more background checks, consistent with these constraints being important in hiring practices.

That said, financial constraints do not fully explain why young firms disproportionately hire workers with criminal records. In our worker-level data, we observe workers with criminal convictions sorting to young firms even when controlling very granularly for the annual pay they earn at the firm; when we estimate model (1) controlling for fixed effects for the percentile of pay a worker earns (within our sample), we find similar patterns (Table A.2). In other words, even when we compare two "cheap" workers, the worker with a criminal background is significantly more likely to work at a young firm.

4.3.2 Indirect constraints

Having found that direct constraints do not fully explain the sorting, we now test whether indirect constraints — arising from competition with established firms — play a role.

Regardless of direct constraints involving their HR sophistication and access to capital, young firms ultimately face challenges when hiring because they must compete with older firms who are more visible in the economy. Indeed, we see evidence in our job postings data of this: younger firms appear unable to hire workers as quickly as more established firms. For example, while 23% of jobs posted by older firms are filled within two weeks — as measured as the job ad being taken down — this is true of only 19% of jobs posted by young firms. We posit that this slower job fill rate may reflect that young firms receive fewer applications and/or may struggle to hire applicants in the face of other job opportunities at more visible established employers.

Thus, young firms end up drawing from an applicant pool that established firms may have already filtered. The composition of this residual pool depends on how aggressively established firms screen. For example, when established firms screen intensively against a particular type of offense, more workers with that offense accumulate in the residual pool; when established firms screen less, fewer such workers filter down. Young firms, facing thin applicant pools to begin with, hire from whoever is available — which disproportionately includes workers that established firms have passed over.

This filtering mechanism generates specific predictions. If the sorting reflects young firms passively receiving established firms' rejects, then sorting should be strongest for crimes that established firms most aggressively screen against. More intensive screening by established firms means more workers filtered into the residual pool that young firms draw from. Furthermore, sorting should weaken when established firms' ability to screen is restricted. If policies like Ban-the-Box prevent established firms from filtering out workers with records, fewer such workers end up in the residual pool, and the gap between young and established firms should narrow. Finally, sorting should weaken when established firms' willingness to screen declines. In tight labor markets, established firms compete harder for all workers and may lower their screening standards rather than leave positions unfilled. This reduces the filtering, shrinking the proportion of workers with criminal records in the residual pool and reducing the degree of sorting.

Importantly, these predictions distinguish the indirect constraint mechanism from a story of young firm preferences driving the patterns, in which case sorting should not directly depend on established firms'

behavior. The tests below allow us to distinguish between these stories. In this section, we leverage our worker-level Census data to show that the indirect constraint derived from competition is an important driver of the sorting.

Sorting is stronger for more severe crimes. Our first test examines whether sorting varies by crime type severity. Prior research documents that employers are particularly reluctant to hire individuals with felony convictions, particularly in violent and property crimes, who may bring risks of workplace theft or violence (Cullen, Dobbie, and Hoffman, 2023; Holzer, Raphael, and Stoll, 2006). Meanwhile, firms may be more tolerant of DUI convictions, which society may view with lower stigma. If the indirect constraint mechanism operates as we describe, workers with these heavily-screened offenses should disproportionately end up at young firms — not because young firms prefer them, but because established firms' intensive screening pushes more of them into the residual labor market pool for young firms.

We test this by estimating a modified version of model (1) in which we consider recent convictions for different crimes separately and then horse-raced against each other. We consider both whether the crime is a felony, as opposed to a misdemeanor, as well whether the crime is a violent, property, DUI, drug, public order, or financial crime. All of these are relatively common among the group of workers with recent convictions: 37% of convictions are felonies, 17% are violent crimes, 27% are property crimes, 21% are DUIs, 28% are drug crimes, 35% are public order crimes, and 7% are financial crimes. Note that these are non-mutually exclusive; each of the crime types can be a felony or misdemeanor, and an individual can be convicted of multiple crimes.

Table 6 presents our findings. On their own in columns (1)-(7), all crimes we consider predict higher rates of working at young firms.[28] When horse-raced against each other in column (8), we find the strongest patterns for the severe crimes older firms may more often screen against. Namely, conditional on other offense indicators, workers with felonies and property crime convictions are particularly likely to work at young firms (having 0.8 and 0.9 percentage point higher rates, respectively), followed by violent and drug crimes (both having 0.5 percentage point higher rates). Notably, DUIs, public order crimes (such as disorderly conduct), and financial crimes no longer predict working at younger firms, conditional on the remainder of an individual's conviction history. We interpret these findings as consistent with evidence that the sorting of workers with convictions to young firms is driven by the less "hireable" cases that older firms likely avoid. While the young firms may also prefer avoiding these workers, they may have fewer alternatives than their older counterparts.

These patterns directly contradict what we would expect under a story of young firms having strong preferences for workers with criminal records. If young firms actively valued workers with criminal records — perhaps viewing them as undervalued talent or identifying with nontraditional backgrounds — we might expect them to favor workers with minor offenses, which signal a "one-time mistake," over those with serious crimes. Instead, we observe the opposite: sorting is strongest precisely for the crimes that established employers most want to avoid. This is consistent with young firms passively receiving the workers that established firms filter out, not actively seeking them.

[28] It is worth noting that individuals with recent convictions for particularly egregious crimes are unlikely to be in this workers sample analysis, as they may be incarcerated or unemployed.

Sorting is stronger in less "friendly" labor markets. Second, we consider whether the sorting varies by general "friendliness" of the local labor market to workers with convictions. Regions and local labor markets differ in how "friendly" or discriminatory firms are against people with convictions (Yang, 2017). In local labor markets where firms are less "friendly" or more discriminatory against workers with convictions, established firms will likely screen out more of these workers, resulting in a higher proportion of them ending up in the residual pool. If the indirect constraint mechanism operates as we describe, in regions where the local labor market is more discriminatory against workers with convictions, these workers should disproportionately end up at young firms — because established firms' screening pushes more of them into the residual labor market pool for young firms.

To study this, we consider a proxy for labor market friendliness towards criminal records: the local "wage penalty" to having a conviction, namely the log earnings gap between workers with and without recent convictions. We construct this variable as the mean log earnings of workers without convictions in a county-year minus the mean log earnings of workers with convictions in the county-year, such that a larger positive pay gap between the two groups means that workers without convictions are paid relatively more. This "penalty" may reflect several factors, including overt discrimination (i.e., firms pay workers with convictions less for the same positions), sorting (i.e., workers with convictions sort to worse paying firms), or composition (i.e., workers with convictions have lower human capital). We view our wage gap measure as a proxy for how adverse the local labor market is for workers with convictions, not a diagnostic measure of discrimination, since it may mechanically reflect sorting into lower-paying firms. There is substantial variation in this pay gap across counties and time: on average, workers without convictions earn 29% more than workers with recent convictions, while the standard deviation of this gap is 34 percentage points.

Column (1) of Table 7 presents our results: when the potential penalty to having a recent conviction is higher (namely, regions that appear less friendly towards workers with convictions), workers with convictions are even more likely to work at young firms. In numbers, a one standard deviation (34 percentage point) higher pay gap increases the rate of workers with convictions sorting to young firms by 0.4 percentage points, a nearly 50% increase from the baseline sorting coefficient of 0.009. While this exercise is imperfect, since the pay gaps may reflect factors unrelated to older firms' unfriendliness to workers with convictions, it does provide additional supportive evidence that our findings may reflect young firms being more willing to hire these workers.

Sorting is weaker when firms have lower ability to screen workers. Our third test examines how sorting responds when firms have reduced ability to screen and avoid workers with criminal records: when policies restrict employers' abilities to screen out potential hires with criminal records. Specifically, we consider the role of Ban-the-Box policies. When a location has a Ban-the-Box policy in place, employers are prohibited from inquiring about criminal histories until late in the hiring process, typically at the conditional job offer stage. By delaying when employers learn about criminal records, Ban-the-Box policies make employers marginally constrained in their ability to screen out potential hires with convictions.[29] Under the indirect constraint mechanism, Ban-the-Box policies should *reduce* sorting of workers with convictions

[29]Note that these Ban-the-Box policies are passed at the city or county level; we say a county has a Ban-the-Box policy in place if the county passes the Ban-the-Box policy or if there is a city in the county with policy in place. 12% of worker-year observations in our workers sample are covered by Ban-the-Box under this definition.

to young firms: if established firms cannot easily screen, fewer workers with records get filtered into the residual labor market pool of young firms.

We study the role of Ban-the-Box through estimating expansions of model (1), tabulated in Table 7. In column (2), we estimate whether the role of recent convictions in predicting working at a young firm varies by whether an worker's home county currently has a Ban-the-Box policy in place; recall that we have county-year fixed effects in our specifications, so we are estimating the relative impact of Ban-the-Box for workers with convictions. We find no difference; i.e., in general, workers with convictions are on average equally more likely to sort to young firms, regardless of Ban-the-Box.

However, prior research documents that Ban-the-Box policies' effects are concentrated among Black men, who face the most severe employment penalties from criminal records and for whom the policy binds most strongly (Hwang and Phillips, 2024; Rose, 2021). Columns (3) and (4) therefore estimate the interaction separately for Black men and White men. For Black men (column (3)), we observe a stark contrast: workers with recent convictions living in counties *without* Ban-the-Box — i.e., counties with easier screening on criminal records — are 1.1 percentage points more often working at young firms; but in counties *with* Ban-the-Box, they are only 0.4 percentage points more often working at young firms. For contrast, column (4) presents the same analysis for White men, where we do not see a significant difference in sorting under Ban-the-Box.

We interpret these findings as evidence that when Ban-the-Box restricts established firms from screening, more (Black) workers with criminal records get hired at established firms, and fewer filter down to young firms. In other words, increased barriers to employment for workers with criminal records, such as employers' increased ability to screen out potential hires with criminal backgrounds, contribute to the sorting patterns we observe. These findings also provide additional evidence that the sorting patterns are not entirely driven by labor supply: if workers with criminal records had strong preferences for working at young firms, then these policies should not matter.

Sorting is weaker when firms have lower willingness to screen workers. Our fourth test examines how sorting responds to labor market conditions, specifically labor market tightness. The indirect constraint mechanism predicts that sorting should depend not just on established firms' ability to screen, but also on their willingness to do so. In tight labor markets, established firms compete aggressively for workers and may lower their screening standards rather than leave positions unfilled. This should reduce filtering: more workers with records get hired by established firms, fewer accumulate in the residual pool, and the gap between young and established firms narrows. In slack markets, established firms can afford to be selective, generating more filtering and stronger sorting.

Column (5) of Table 7 investigates this by estimating heterogeneity of sorting by a measure of labor market tightness: the ratio of new job postings to current jobs in the county. This "new job ratio" is higher when relatively more firms are looking to hire workers (or when firms are looking to hire more workers), suggesting a tighter labor market where workers have more options and employers must compete harder. On average, the number of job postings are 11% of the number of current jobs. We find results consistent with our hypothesis: when the county new job ratio is one standard deviation (6 percentage points) higher, workers with convictions are 0.1 percentage points marginally less likely to sort to young firms a reduction

of 14% reduction from the baseline gap of 0.9 percentage points. In other words, when established firms face pressure to fill positions, they screen less aggressively, and fewer workers with records filter down to young firms.

Summary of indirect constraint evidence. Taken together, across four distinct tests, we find consistent evidence for the indirect constraint mechanism. Sorting to young firms is strongest for heavily-stigmatized crimes where established firms screen most intensively and in labor markets that are less friendly towards workers with records; sorting weakens when Ban-the-Box policies restrict established firms' ability to screen (for Black men, where Ban-the-Box binds most); and sorting weakens when tight labor markets reduce established firms' willingness to screen.

These patterns are difficult to reconcile with a story of young firms' preferences driving the sorting. If young firms valued workers with records, sorting should not directly depend on established firms' behavior. Instead, our evidence points to young firms passively receiving the workers that established firms filter out. Young firms hire more workers with criminal records not because they want to, but because they draw from a labor pool that established firms have already considered.

5 Discussion: Outcomes for individuals with criminal records

So far, we have established that workers with recent convictions disproportionately find jobs at younger firms, as these younger firms are more willing to hire them. What does this willingness of young firms mean for individuals with convictions? When workers with convictions sort to young firms, is this a "good" outcome? On the one hand, young firms can be unstable employers, particularly because they have high exit rates, which in turn means high layoff rates (Sorenson et al., 2021). On the other hand, when an individual faces barriers finding jobs, having *any* job may be preferable to having no job, as jobs provide income, structure, and work experience that may facilitate reintegration (Western, 2002; Uggen, 2000). Having *any* job may be particularly important for individuals with criminal records, as employment is one of the strongest predictors of stopping criminal behavior (Uggen, 1999; Yang, 2017), and 11% of non-employed individuals with recent convictions in our full sample recidivate within a year. For workers with convictions, the realistic alternative to young firm employment is likely non-employment, not a job at an established firm, as our evidence from Section 4 suggests that workers with records end up at young firms precisely because established firms have screened them out.

In this section, we argue that young firms' willingness to hire workers with criminal records may mean that promoting firm entry — and survival — may improve economic outcomes for individuals with criminal records, which is beneficial both for the individuals themselves and their communities. We examine these outcomes using two complementary approaches. First, we characterize the outcomes of individuals with criminal records *after* they are hired by young firms, relative to both workers at established firms and non-employed individuals. Second, we consider the causal impact of having more young firms hiring in a county on employment and recidivism for individuals with criminal records.

5.1 Descriptive evidence from individual-level data

What happens to the workers with criminal convictions who find jobs at young firms? To answer this question, we compare the future employment and recidivism rates of these workers to two reference groups: workers with records at established firms and workers with records who are not employed. These com-

parisons serve different purposes. The first comparison tells us whether young firm jobs are "good" jobs compared to jobs at established firms. The second, which may be more policy-relevant, tells us whether young firm jobs are better than the realistic alternative for workers whom established firms have screened out, non-employment.

We draw on two samples: the "working" sample that has been our focus so far, and the "full" sample that adds individuals who "could be" working — namely those we believe live in the states in our analysis (based on residence data) but are not working but are rather non-employed or self-employed — as described in Section 2. The full sample introduces some measurement error, as described in Section 2, and so we interpret these results with some caution; for example, individuals in the full sample we deem to be non-employed could live in our sample of states but be employed in neighboring states for which we do not observe employment records. As we discuss below, measurement error is likely smaller for individuals with criminal records, who may have mobility constraints such as parole and probation requirements.

We emphasize that these analyses are descriptive. The patterns we document reflect both selection — which workers end up at which types of firms — and any treatment effects of the jobs themselves. Given our evidence from Section 4 that young firms hire from a residual pool of workers that established firms filtered out, negative selection is likely substantial. We return to this interpretation after presenting the results.

To formalize how the outcomes for individuals with convictions who find jobs at young firms differ from outcomes of others, we estimate two sets of regressions:

$$\begin{aligned}\text{Future outcome}_{it} = \alpha &+ \beta_1 \text{Recent conviction}_{it} + \beta_2 \text{Work at a young firm}_{it} \\ &+ \beta_3 \text{Recent conviction}_{it} \times \text{Work at a young firm}_{it} + \mathbf{X}_{it}\delta + \gamma_{c(i,t)t} + \gamma_{n(i,t)} + \varepsilon_{it},\end{aligned} \tag{4}$$

and

$$\begin{aligned}\text{Future outcome}_{it} = \alpha^f &+ \beta_1^f \text{Recent conviction}_{it} + \beta_2^f \text{Work at a young firm}_{it} \\ &+ \beta_3^f \text{Recent conviction}_{it} \times \text{Work at a young firm}_{it} \\ &+ \beta_4^f \text{Work at an established firm}_{it} \\ &+ \beta_5^f \text{Recent conviction}_{it} \times \text{Work at an established firm}_{it} + \mathbf{X}_{it}\delta^f + \gamma^f_{c(i,t)t} + \gamma^f_{n(i,t)} + \varepsilon^f_{it},\end{aligned} \tag{5}$$

where we consider future outcomes for individual i as in model (1), as a function of whether they have a recent conviction and whether they currently work at a young firm. In model (4), we consider our workers sample and ask how workers with criminal convictions who work at young firms compare to those at established firms. In model (5), we add in the non-employed individual and study our "full" sample (denoted by the superscript f), where we compare individuals with convictions who work for young firms to both those who are not employed (the omitted group) and those employed at established firms. We consider two outcomes: short-term recidivism — namely, whether the individual has any criminal convictions in the next year — and short-term employment — whether the individual is non-employed two years in the future.[30]

[30] We focus on short-term outcomes in order to avoid censoring due to the limitations of the data; for example, the justice system data we use ends in 2018, one year after the end of our sample.

As in model (1), we include a number of control variables, including fixed effects for the individual's sex, race/ethnicity, age, county c-year t, and sector n.

Future employment. In Table 8, columns (1) and (2), we consider future employment; here, the outcome is an indicator equal to 1 if an individual is not employed two years in the future (i.e., a negative coefficient would indicate higher future employment rates). This is a relevant outcome for our population; 20% of current workers with criminal conviction records are non-employed two years in the future, compared to only 13% of all workers.

In Panel A column (1), we compare workers at young versus established firms. We find that, while workers with convictions and workers at young firms generally have lower future employment rates, workers with convictions who find jobs at young firms have particularly low rates. Compared to an established firm employee with a conviction with a future non-employment rate of approximately 18.9%, a young firm employee with a conviction faces a rate of 23.6% — workers with convictions at young firms are 25% more likely to be non-employed two years in the future, compared to workers with convictions at established firms.

In Panel A column (2), we compare young firm employment to the alternative of non-employment. We find that outcomes for workers at young firm tend to be meaningfully better than for those who have no job. A worker with a conviction at a young firm is approximately 44 percentage points less likely to be non-employed in two years than a non-employed individual with a conviction. Employment in an established firm shows a larger gap, where workers with convictions at an established firm are approximately 50 percentage points less likely to be non-employed in two years than a non-employed individual with a conviction. Thus, young firm employees with convictions "sit between" non-employed individuals with convictions and established firm employees with convictions in terms of their frequency of future non-employment.[31]

Recidivism. In Panel A columns (3) and (4) of Table 8, we consider individuals' recidivism, which we measure as having a conviction in the next year. For individuals with recent convictions, recidivism is relatively common, with 9.4% of workers with recent convictions (and 10.0% of all individuals with recent convictions) having a conviction the next year; meanwhile, in our entire sample of workers (all individuals) only 0.8% (1.0%) have convictions the next year.

Column (3) compares workers at young versus established firms. We find workers with recent convictions have higher future conviction rates (8 percentage points higher), as do workers at young firms (0.1 percentage points higher), and critically these patterns compound, with workers with recent convictions who work at young firms being particularly likely to recidivate. Compared to a worker with a recent conviction working at an established firm, one with a recent conviction working at a young firm is on average 12%

[31] Note that in this specification we see some evidence of the possible mis-measurement of the "full" sample mentioned above, in that, counterintuitively, non-employed individuals with convictions have marginally *lower* future non-employment rates than non-employed individuals with clean records. We believe this is a quirk of the sample's inclusion of non-employed individuals; if some "non-employed" individuals are actually, e.g., employed in a state we do not observe, we will likely persistently call them non-employed. Meanwhile, if we observe an individual with a recent criminal conviction in the state, we are more likely to confidently capture this individual's employment record (since they are less likely to be mobile), in which case they may appear more often employed in the future than their clean-record counterpart.

more often recidivating within a year.[32]

Column (4) compares young firm employment to the alternative of non-employment. We see a smaller but still meaningful difference in recidivism rates: all else equal, an individual with a conviction record who finds a job at a young firm is 3% less likely to recidivate in the short run than an individual with a conviction who finds no job at all. While this is a meaningful difference, the gap between established firm workers with convictions and non-employed workers with convictions is much larger, with an established firm employee with convictions recidivating 17% less often than non-employed individuals with convictions.

Does firm quality matter? Not all young firms are alike in terms of stability and quality. If young firm instability drives the relatively poor outcomes compared to established firms, then workers at young firms that ultimately survive should fare better than those at firms that fail.

In Panel B of Table 8, we explore the role of young firm exits by splitting on whether the young firm is "successful" (i.e., survives to age 5). In column (1), we find that working at a unsuccessful young firm, regardless of criminal record, tends to predict significantly larger future non-employment rates compare to working an established firm. Workers at young firms that will fail are 10.1 percentage points more likely to be non-employed two years later than workers at established firms — compared to just 2.6 percentage points for workers at young firms that survive. This pattern is consistent with the displacement effects documented in the broader literature: workers who lose jobs due to firm exit experience persistent employment difficulties (Jacobson, LaLonde, and Sullivan, 1993). Interestingly, this general pattern dominates any difference by conviction history, suggesting that this displacement penalty does not differ substantially by conviction history. We also find that for workers with convictions, working at a successful young firm has a smaller penalty compared to working at an unsuccessful young firm. Column (2), which compares to non-employed individuals, echoes these patterns: working at an unsuccessful young firm provides smaller employment "benefits" than working at a successful one, and even successful young firm employment carries "penalties" relative to established firms.

In columns (3) and (4) of Panel B, we see that additional recidivism "risk" for workers at an unsuccessful young firm is roughly twice as large for workers without recent convictions and about 50% larger for workers with recent convictions, compared to the analogous gap for working at successful young firms. In column (4) of Panel B, we see that recidivism rates for individuals with convictions working at unsuccessful young firms, all else equal, are, if anything, *higher* than non-employed individuals with convictions. Working at a successful young firm predicts lower recidivism rates, compared to non-employment, for workers with convictions, but in a smaller magnitude than working at an established firm. As discussed below, these patterns likely stem from a combination of selection (hiring at unsuccessful young firms is more negatively selected) and treatment (young firms that exit spur risky behavior like crime).

Taken together, the results in Table 8 suggest three patterns. First, young firm employment is not a panacea for workers with records. Compared to jobs at established firms, young firm jobs are associated

[32]Note that, because the recent conviction may have happened anytime in the past 7 years, the next-year recidivism we measure could reflect recidivating after as little as 2 years or as many as 9. In untabulated results, we estimate column (1) with the addition of fixed effects for the years since the recent conviction, in order to net out differences driven by the recency of the conviction. Results are similar, though the coefficients on the interaction terms are slightly attenuated, suggesting that workers with convictions sort to young firms partially on the basis of the recency of the conviction, consistent with our event study analysis in Figure 3.

with worse outcomes across the board — higher future non-employment and higher recidivism. Second, young firm employment appears modestly better than non-employment. Workers with records who find jobs at young firms have substantially better employment prospects and somewhat lower recidivism rates than those with no job at all. Given that non-employment is the realistic counterfactual for many of these workers, this represents a meaningful —if limited —benefit. Third, firm survival matters. Workers at young firms that ultimately fail face the worst outcomes, consistent with job loss being particularly destabilizing for individuals with criminal records.

There are two potential (non-mutually exclusive) interpretations of these patterns: a selection-based interpretation and a treatment-based interpretation. First, perhaps young firms generally hire "worse" workers, compared to established firms, such that all workers at these young firms have high recidivism rates; this negative selection could be particularly bad for those with convictions, if older firms screen out likely re-offenders. Second, perhaps young firms catalyze crime by generating unstable earnings for individuals; at one extreme, if a worker is on parole and loses their job at a young firm that goes under, they may be charged with a parole violation. Thus we are unable to distinguish whether we these patterns because workers with convictions who find jobs at young firms are likely the most marginally "employable" ones or having a job at a young firms is not that much "better" than having no job at all, at least for recidivism. This begs the question: would outcomes be better if there were more young firms hiring? We turn to this next.

5.2 Bartik IV analysis with the county-level data

If young firms create more jobs, is this good for individuals with criminal records? In this section we study how the presence of more jobs at young firms in a given county affects the outcomes for individuals with criminal convictions. We do this using public county-level data from the CJARS program for 2010-2018, which contains information on recidivism and employment rates for individuals with criminal convictions in different years; the public data additionally has more geographic scope than our main samples (since it is not constrained to the set of Census states we can study in this paper),[33] which allows us to deploy a geography-based identification strategy.

Empirical approach. Simply correlating the number of jobs at young firms with outcomes for individuals with convictions need not return a causal relationship, as there are numerous omitted variables that can bias the results. Most importantly, if firms enter in economic booms, this correlation may reflect the effect of favorable economic conditions on workers with criminal records, rather than the effect of the young firms themselves.

To address this, we construct and deploy an industry-based Bartik instrument in which we instrument a county's log employment in young firms (aged 5 and under) with their predicted employment given (a) national employment in young firm counts in different NAICS3 industries and (b) the county's historic industry composition. Our identification is based on exogeneity of the historical industry composition; namely, identification requires that a county's historical industry mix is uncorrelated with current-day differential trends in outcomes for individuals with convictions. Below, we provide tests that support this assumption,

[33] Our sample with the public CJARS data spans Arizona, Florida, Maryland, Michigan, Minnesota, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Texas, Virginia, and Wisconsin. Note that we require data on both misdemeanors and felonies in our analysis below, which means we omit New Jersey — one of our main sample states, which only has data on felonies.

as suggested by Goldsmith-Pinkham, Sorkin, and Swift (2020).

Specifically, we estimate a ordinary least squares (OLS) model:

$$\text{Outcome}_{ct} = \alpha + \beta \log(\text{Employment at young firms}_{ct}) + \mathbf{X}_{ct}\delta + \gamma_c + \gamma_t + \varepsilon_{ct}, \tag{6}$$

where we consider outcomes, such as the employment rate of recently convicted individuals, for county c in year t. We include county and year fixed effects as well as controls $\mathbf{X}_{ct}$ that capture local economic conditions and crime rates, discussed below.

We then estimate a two-stage least squares (2SLS) model in which we instrument employment in a first stage with a Bartik instrument:

$$B_{ct} = \sum_n z_{nc,t_{2000}} g_{nt}, \tag{7}$$

where n is a NAICS3 industry, g_{nt} is the national employment at young firms in industry n in year t, and $z_{nc,t_{2000}}$ is the share of all employment in industry n that was located in county c in 2000, normalized by the number of people employed in the county in 2000.[34] Note that the shares do not sum up to 1 within a county.

We source county and industry level information on employment by firm age from several public sources. First, we measure the fraction of all employment in an industry located in a county in year 2000 using the Census Bureau's County Business Patterns (CBP) data; we normalize this fraction by the total employment in the county in 2000 using the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) data. Second, we measure employment of young firms nationally in each industry and the employment of all young firms in a county using the Census Bureau's Business Dynamics Statistics (BDS) database.[35] We estimate two versions of each specification: one in which we equally weight all counties (capturing patterns in the *average county*) and one in which we weight all of our analyses using the county's 2000 labor force size, sourced from LAUS (capturing patterns for the *average labor market participant*.[36]

There are two key identification points for our Bartik instrument. The first is that we are assuming that the shares are exogenous; namely, we are assuming that counties that had larger employment shares of particular industries are not on particular trends that would affect individuals with convictions today. The

[34] Namely, $z_{nc,t_{2000}}$ is the employment share for an industry in a county divided by the total employment in that county in 2000. This normalization helps us have an instrument that has a quasi-linear relationship between log employment; i.e., we do not log the Bartik instrument itself, since this would make the instrument no longer additively separably by industry — which we need in order to assess the instrument below. This is similar to how Goldsmith-Pinkham, Sorkin, and Swift (2020) describe the Bartik instrument in Card (2009).

[35] In an ideal analysis, we would be able to consider leave-one-out national employment at young firms in each industry, removing each county's own employment at young firms in the industry; by using the full national value, we risk finite sample bias. This bias may be particularly large for heavily concentrated industries; if an industry is disproportionately located in a single industry both in 2000 and beyond, we may effectively be instrumenting a county's young firm employment with its own actual young firm employment. Unfortunately, the BDS data does not enable to construct leave-one-out measures, as we must use separate county-by-firm age and industry-by-firm age datasets (due to what is available). To reduce the risk of the bias, we instead omit from our analysis industry-county pairs in which the county accounted for more than 0.7% of the industry's total employment in 2000; this 0.7% is the 99th percentile of the industry-county employment share distribution. Our final instrument is a strong predictor of log employment at young firms; in our preferred specification in which we control for economic conditions and overall crime rates, the first stage has an F-statistic of 25.

[36] We weight by a lagged labor force size in order to avoid the weights being endogenous to the number of jobs at young firms due to, e.g., population aging or migration.

second is that we have a vital exclusion restriction concern: a county that is more exposed to an industry's *young firm* employment growth may also be more exposed to the industry's *established firm* employment growth. I.e., we may not be identifying the effect of young firms but rather of just generally more jobs. Below, we have two approaches to assuage this concern. First, we control for the county's actual employment at established firms (over the age of 5) and the county's actual employment rate; both of these values we discretize into ventiles and then include ventile fixed effects. Second, we examine whether our instrumented young firm employment predicts better outcomes for the general population compared to individuals with recent convictions.

Results. We present our OLS and 2SLS regressions in Table 9. Panels A and B present estimates from our unweighted specifications, while Panels C and D present our labor force size-weighted specifications. We start by considering the overall county employment rate and then turn to employment and recidivism rates for individuals with prior misdemeanors and felonies.[37] By looking at outcomes in the same year as the young firm employment, we are considering short-run outcomes; while it is entirely possible that more young firm jobs in a given year will have long run consequences for individuals with criminal records, our instrument does not provide identification for long-term outcomes. For the same reason, we focus on the outcomes of the most recently convicted cohort each year — namely, in this section, an individual is "recently convicted" if they were convicted of a crime in the previous year (as opposed to the previous 7 years).

Consistent with Haltiwanger, Jarmin, and Miranda (2013), more young firm employment in a location predicts higher overall county employment rates, at least in our unweighted specifications (column (1)); i.e., more jobs at young firms is "better" for the general population. However, more young firm jobs matter substantially more for the employment prospects of individuals with recent convictions, consistent with our premise that young firms disproportionately hire workers with criminal records. For example, in our unweighted 2SLS regressions in Panel B, a doubling of number of jobs at young firms predicts a 2.6% higher overall employment rate, relative to the mean, but a 15.2% higher employment rate for individuals with recent misdemeanor convictions (column (2)), relative to the mean, and a 20.7% higher employment rate for those with recent felony convictions (column (4)), relative to the mean.

These large employment effects for individuals with convictions persist when we flexibly control for the overall employment rate (in addition to the already included controls for the number of jobs at established firms) in columns (3) and (5): *conditional on the general employment rate*, individuals with recent misdemeanors see a 9.7% higher employment rate, relative to the mean, while those with recent felony convictions have a 12.6% higher employment rate, relative to the mean. The stronger effect for felonies is consistent with our finding that workers with felony convictions sort disproportionately to young firms (Table 6). We see largely similar patterns in Panel D when we weight our observations by the size of the county's labor force (in 2000); notably the effect of young firm jobs on the overall employment rate disappears, though the unweighted point estimate (column (1) of Panel B) falls within the confidence interval of the weighted point estimate (column (1) of Panel D). Note that the 2SLS estimates are generally one order of magnitude larger

[37] Note that the overall county employment rate is measured as the fraction of labor market participants who are employed, while the employment outcomes for individuals with convictions is not conditional on being an active labor market participant.

than the OLS estimates, which we believe predominantly stems from the 2SLS yielding a local average treatment effect (LATE); we return to this gap while discussing which industries drive our results below. We see largely similar patterns

When we turn to recidivism rates in columns (6) and (7), we see limited patterns. In column (6), the dependent variable is the fraction of individuals with misdemeanors in the previous year who have another misdemeanor in the current year; column (7) considers the felony rates of individuals with felony convictions in the previous year. Consistent with our analysis in Table 8, recidivism rates are high: on average, 16% (11%) of individuals with recent misdemeanor (felony) convictions have another in the following year. In both our OLS and 2SLS models, regardless of weighting, we largely find no statistically meaningful relationship between the employment at young firms and short-term recidivism rates, though there is a statistically insignificant negative coefficient in our second stage in for misdemeanor recidivism in our weighted specifications. The lack of a meaningful relationship is consistent with our findings in Table 8, where we observe only marginally lower recidivism rates for young firm employees with criminal records, compared to those who are non-employed. That is, even if young firms provide jobs for workers with convictions, they may not dramatically change crime rates, perhaps because these jobs are often unstable due to firm exits.[38]

Instrument diagnostics. Following Goldsmith-Pinkham, Sorkin, and Swift (2020), we construct Rotemberg weights to unpack the Bartik instrument; these weights convey which industries have the most power in the first stage and consequently are weighted more in determining the final second stage estimate (which, as Goldsmith-Pinkham, Sorkin, and Swift (2020) show, can be constructed as the sum of all of the industry-specific estimates, weighted by the Rotemberg weights). In our context, a higher Rotemberg weight tells us that counties' young firm employment is better predicted by national trends and historical industry distributions. Tables A.6 and A.7 present a number of a statistics describing these weights, for our unweighted and weighted specifications. Most importantly, we have one industry that plays an outsized role in our first stage, regardless of weighting: NAICS 722, the food services and drinking places sector — i.e., restaurants. The industry has a Rotemberg weight (in our full specification of Table 9) of 0.396 in the unweighted specification and 0.0398 in the weighted specification. The next-largest weight industry in our unweighted specification (NAICS 213 Prime contracting in construction) has a weight of 0.266; in our weighted specification, the next-largest weight industry (NAICS 561 administrative and support services) has a weight of only 0.157. We correlate the normalized employment shares in the year 2000 (i.e., the share of each industry's employment in a county, normalized by the county's total employment in 2000) with several of our control variables to assess how different our high-Rotemberg weight industries are. While there are some patterns — e.g., in our weighted specification, counties that have higher normalized shares of restaurants had more felonies but fewer misdemeanors in year 2001 — they are economically small; for instance, a one standard deviation higher felony rate in 2001 predicts only a 0.013 standard deviation higher restaurant share (column (1) of Table A.7). In other words, we argue that the counties that have, e.g., more restaurants in the year 2000 (normalized by employment) are unlikely to be on a trend that makes future employment opportunities more plentiful for individuals with convictions in a way not dictated by the appearance of

[38]Note that by controlling for the overall misdemeanor and felony conviction rates, we may be restricting the variation to observations where re-offenders amount to a smaller fraction of the overall crime rate. In untabulated results, we confirm that we do not estimate statistically different coefficients if we omit these crime rate controls.

young firms themselves.

Looking at the high-Rotemberg weight industries also provides insights on why our 2SLS estimates are larger than the OLS ones. Namely, in the notes of Tables A.6 and A.7, we report the industry-specific coefficients from our regression of employment rates for individuals with recent misdemeanors on log employment at young firms. Here, we see, e.g., that the presence of more young restaurants has a higher coefficient (0.065) in the unweighted specification than the pooled 2SLS coefficient (0.059) in column (3) of Table 9 Panel B. This is consistent with the fact that restaurants are large employers of individuals with criminal records (Figure 1).

Taken together, the county-level analysis provides complementary evidence that young firms serve as important employers for individuals with criminal records. Counties with exogenously higher young firm employment have substantially higher employment rates among this population. The null effects on recidivism are consistent with our individual-level findings: young firm jobs provide labor market access but may be too unstable to dramatically reduce re-offending. Together with the descriptive evidence from Section 5.1, these results suggest that young firms function as a labor market safety valve — providing employment opportunities for workers whom established firms screen out, with modest but meaningful benefits relative to non-employment.

6 Conclusion

Individuals with criminal records face numerous barriers to employment. This is costly not only to the individuals themselves but also to society at large, as non-employment among this population contributes to recidivism and other negative outcomes that impose public costs. Policy efforts aimed at generating economic inclusion for individuals with criminal records have primarily focused on reducing the ability of firms to screen on criminal records or subsidizing the hiring of individuals with records.

We posit that promoting local entrepreneurship may generate an additional, complementary pathway to inclusion. Young firms disproportionately hire workers with criminal convictions, arguably because they engage in less screening on criminal records than more established firms. While these jobs do not necessarily generate large economic gains, since young firms often fail, the provision of *any* job may be better than no job. In this way, our findings underscore the potential of young firms to address labor market inequalities, while calling for policies that enhance the quality and stability of these opportunities.

More broadly, our findings that young firms can be crucial partners in "second chance" hiring initiatives suggest that workforce development programs and reentry services might explicitly connect job-seekers with criminal records with the young firm ecosystem, recognizing that these firms may be relatively receptive to hiring candidates with nontraditional resumes. Furthermore, if young firms are indeed offering a foothold to individuals with criminal histories, supporting these pathways is critical, through measures such as extended access for small business loans (via programs from the Small Business Administration) or the proposed New Start Act (which provides microloans for people with criminal records).

References

Adelino, Manuel, Song Ma, and David Robinson. 2017. "Firm age, investment opportunities, and job creation." *The Journal of Finance* 72 (3):999–1038.

Adelino, Manuel, Antoinette Schoar, and Felipe Severino. 2015. "House prices, collateral, and self-employment." *Journal of Financial Economics* 117 (2):288–306.

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

Bernstein, Shai, Richard R Townsend, and Ting Xu. 2024. "Flight to safety: How economic downturns affect talent flows to startups." *The Review of Financial Studies* 37 (3):837–881.

Campero, Santiago and Aleksandra Kacperczyk. 2020. "Asymmetric gender homophily in the startup labor market." In *Employee inter-and intra-firm mobility: Taking stock of what we know, identifying novel insights and setting a theoretical and empirical agenda*. Emerald Publishing Limited, 329–359.

Card, David. 2009. "Immigration and inequality." *American Economic Review* 99 (2):1–21.

Chung, Andrew and Robert Parker. 2023. "Visibility Constraints and Hiring in Startups." *Journal of Business Venturing* 38 (2):123–145.

Cullen, Zoë, Will Dobbie, and Mitchell Hoffman. 2023. "Increasing the demand for workers with a criminal record." *The Quarterly Journal of Economics* 138 (1):103–150.

Decker, Scott H, Natalie Ortiz, Cassia Spohn, and Eric Hedberg. 2015. "Criminal stigma, race, and ethnicity: The consequences of imprisonment for employment." *Journal of Criminal Justice* 43 (2):108–121.

Fackler, Daniel, Michaela Fuchs, Lisa Hölscher, and Claus Schnabel. 2019. "Do start-ups provide employment opportunities for disadvantaged workers?" *ILR Review* 72 (5):1123–1148.

Fairlie, Robert W. 2002. "Drug dealing and legitimate self-employment." *Journal of Labor Economics* 20 (3):538–567.

Finlay, Keith and Michael Mueller-Smith. 2021. "Criminal justice administrative records system (cjars)." *Ann Arbor: University of Michigan, Institute for Social Research* .

Finlay, Keith, Michael Mueller-Smith, and Brittany Street. 2023. "Criminal Justice Involvement, Self-Employment, and Barriers in Recent Public Policy." *Journal of Policy Analysis and Management* 42 (1):11–34.

Friedman, Matthew. 2015. "Just Facts: As Many Americans Have Criminal Records as College Diplomas." Brennan Center for Justice.

Glaeser, Edward L, Sari Pekkala Kerr, and William R Kerr. 2015. "Entrepreneurship and urban growth: An empirical assessment with historical mines." *Review of Economics and Statistics* 97 (2):498–520.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik instruments: What, when, why, and how." *American Economic Review* 110 (8):2586–2624.

Griffin, John M, Samuel Kruger, and Gonzalo Maturana. 2019. "Do labor markets discipline? Evidence from RMBS bankers." *Journal of Financial Economics* 133 (3):726–750.

Guo, Audrey and Melanie Wallskog. 2025. "New employer payroll taxes and entrepreneurship." *Journal of Public Economics* 250:105469.

Haltiwanger, John, Ron Jarmin, and Javier Miranda. 2013. "Who Creates Jobs? Small versus Large versus

Young." *Review of Economics and Statistics* 95 (2):347–361.

Hamersma, Sarah. 2011. "Why don't eligible firms claim hiring subsidies? The role of job duration." *Economic inquiry* 49 (3):916–934.

Hansen, Stephen, Peter John Lambert, Nicholas Bloom, Steven J Davis, Raffaella Sadun, and Bledi Taska. 2023. "Remote work across jobs, companies, and space." Tech. rep., National Bureau of Economic Research.

Holzer, Harry J, Steven Raphael, and Michael A Stoll. 2003. *Employer demand for ex-offenders: Recent evidence from Los Angeles*. Institute for Research on Poverty, University of Wisconsin-Madison Madison, WI.

———. 2006. "Perceived criminality, criminal background checks, and the racial hiring practices of employers." *The Journal of Law and Economics* 49 (2):451–480.

Hurst, Reuben, Saerom Lee, and Justin Frake. 2024. "The effect of flatter hierarchy on applicant pool gender diversity: Evidence from experiments." *Strategic Management Journal* 45 (8):1446–1484.

Hwang, Kylie Jiwon and Damon J Phillips. 2024. "Entrepreneurship as a response to labor market discrimination for formerly incarcerated people." *American Journal of Sociology* 130 (1):88–146.

Jacobs, James B. 2005. "Mass incarceration and the proliferation of criminal records." *U. St. Thomas LJ* 3:387.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan. 1993. "Earnings losses of displaced workers." *The American economic review* :685–709.

Klenow, Peter J and Huiyu Li. 2021. "Innovative growth accounting." *NBER Macroeconomics Annual* 35 (1):245–295.

Leung, Mike. 2003. *Organizational Legitimacy and Recruitment in Startups*. University Press.

Ma, Song, Justin Murfin, and Ryan Pratt. 2022. "Young firms, old capital." *Journal of Financial Economics* 146 (1):331–356.

Moser, Nolan, Rebecca Smith, and David Jones. 2017. "Hiring Challenges in Startups: An Organizational Perspective." *Entrepreneurship Theory and Practice* 41 (1):153–178.

Mueller-Smith, Michael. 2015. "The criminal and labor market impacts of incarceration." *Unpublished working paper* 18.

Mueller-Smith, Michael and Kevin T Schnepel. 2021. "Diversion in the criminal justice system." *The Review of Economic Studies* 88 (2):883–936.

Ouimet, Paige and Rebecca Zarutskie. 2014. "Who works for startups? The relation between firm age, employee age, and growth." *Journal of financial Economics* 112 (3):386–407.

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

Phelps, Edmund S. 1972. "The statistical theory of racism and sexism." *The american economic review* 62 (4):659–661.

Robb, Alicia M and David T Robinson. 2014. "The capital structure decisions of new firms." *The Review of Financial Studies* 27 (1):153–179.

Rose, Evan K. 2021. "Does banning the box help ex-offenders get jobs? Evaluating the effects of a prominent example." *Journal of Labor Economics* 39 (1):79–113.

Sauermann, Henry. 2018. "Fire in the belly? Employee motives and innovative performance in start-ups versus established firms." *Strategic Entrepreneurship Journal* 12 (4):423–454.

Sorenson, Olav, Michael S Dahl, Rodrigo Canales, and M Diane Burton. 2021. "Do startup employees earn more in the long run?" *Organization Science* 32 (3):587–604.

Sugie, Naomi F. 2018. "Work as Foraging: A Smartphone Study of Job Search and Employment after Prison." *American Journal of Sociology* 123 (5):1453–1491. URL https://www.journals.uchicago.edu/doi/10.1086/696209.

Sun, Liyang and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of econometrics* 225 (2):175–199.

Uggen, Christopher. 1999. "Ex-offenders and the conformist alternative: A job quality model of work and crime." *Social problems* 46 (1):127–151.

———. 2000. "Work as a Turning Point in the Life Course of Criminals: A Duration Model of Age, Employment, and Recidivism." *American Sociological Review* 65 (4):529. URL http://www.jstor.org/stable/2657381?origin=crossref.

Wallskog, Melanie. 2025. "Entrepreneurial spillovers across coworkers." *Journal of Financial Economics* 170:104077.

Western, Bruce. 2002. "The impact of incarceration on wage mobility and inequality." *American sociological review* 67 (4):526–546.

Yang, Crystal S. 2017. "Local labor markets and criminal recidivism." *Journal of Public Economics* 147:16–29.

Table 1: Summary statistics

Sample:	All		Without recent convictions		With recent convictions	
	Mean (1)	Std Dev (2)	Mean (3)	Std Dev (4)	Mean (5)	Std Dev (6)
Panel A: All individuals (working, self-employed, and not working)						
Recent conviction (7 years)	0.046	0.209	0	0	1	0
Future conviction (1 year)	0.010	0.097	0.005	0.072	0.100	0.300
Recent incarceration (7 years)	0.006	0.076	0.001	0.033	0.106	0.307
Working	0.723	0.448	0.729	0.445	0.605	0.489
Male	0.500	0.500	0.487	0.500	0.767	0.423
Age	41.26	13.52	41.50	13.59	36.21	10.82
Non-Hispanic White	0.668	0.471	0.673	0.469	0.569	0.495
Non-Hispanic Black	0.140	0.347	0.134	0.341	0.249	0.433
Non-Hispanic Other Race	0.079	0.269	0.079	0.270	0.062	0.241
Hispanic	0.114	0.318	0.114	0.317	0.120	0.325
N	16,640,000		12,260,000		4,375,000	
Panel B: Workers						
Recent conviction (7 years)	0.038	0.191	0	0	1	0
Future conviction (1 year)	0.008	0.090	0.005	0.069	0.094	0.292
Recent incarceration (7 years)	0.004	0.061	0.001	0.029	0.077	0.266
Working at young firm	0.069	0.254	0.067	0.250	0.120	0.325
Male	0.492	0.500	0.482	0.500	0.760	0.427
Age	40.84	13.24	41.07	13.30	35.03	10.30
Non-Hispanic White	0.686	0.464	0.689	0.463	0.601	0.490
Non-Hispanic Black	0.129	0.336	0.126	0.332	0.215	0.411
Non-Hispanic Other Race	0.072	0.258	0.072	0.259	0.061	0.239
Hispanic	0.113	0.317	0.113	0.316	0.124	0.329
N	11,260,000		8,689,000		2,575,000	

Note: This table presents summary statistics for all individuals as well as workers, split on whether an individual has a recent conviction in the past 7 years. Note that our subsample without recent convictions is a random 10% sample, so we up-weight these individuals when taking averages for the full sample in column (1).

Table 2: Workers with recent convictions disproportionately work at younger firms

	Dependent Variable: Work at a young firm					
	(1)	(2)	(3)	(4)	(5)	(6)
Recent conviction	0.053***	0.047***	0.051***	0.034***	0.031***	0.009***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Male		-0.001***	-0.001***	-0.002***	0.000**	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Black		-0.004***	-0.006***	-0.005***	-0.007***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Other Race		0.021***	0.016***	0.012***	0.007***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Hispanic		0.016***	0.006***	0.002***	0.001*	
		(0.000)	(0.000)	(0.000)	(0.000)	
Constant	0.067***	0.065***	0.067***	0.068***	0.068***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Age FE		x	x	x	x	x
County-year FE			x	x	x	x
Sector FE				x	x	x
Past earnings bin FE					x	
Individual FE						x

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working. The seven columns gradually build up model (1), slowly adding controls as demonstrated in the table and table footer.

N = 11,260,000. Mean of dependent variable: 0.069.

Table 3: Workers with recent convictions disproportionately work at younger firms, by sex and race

Sample:	Dependent Variable: Work at a young firm						
	All	White men	White women	Black men	Black women	Hispanic men	Hispanic women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Recent conviction	0.008***	0.008***	0.007***	0.010***	0.013***	0.006**	0.014**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.005)
Recent conviction × Male	0.000						
	(0.001)						
Recent conviction × Non-Hispanic Black	0.004***						
	(0.000)						
Recent conviction × Non-Hispanic Other	-0.001						
	(0.003)						
Recent conviction × Hispanic	0.001						
	(0.002)						
Constant	0.069***	0.065***	0.064***	0.068***	0.064***	0.135***	0.086***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean(work at a young firm)	0.069	0.065	0.064	0.069	0.064	0.089	0.086
Mean(recent conviction)	0.038	0.050	0.017	0.109	0.026	0.067	0.028
N	11,260,000	4,195,000	3,168,000	1,022,000	725,000	815,000	554,000

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working, by the sex and race.

All columns include as controls age, county-year, sector, and individual fixed effects.

Table 4: Younger firms mention background checks less frequently in job postings

	Dependent Variable: Job posting mentions a background check					
	(1)	(2)	(3)	(4)	(5)	(6)
Young firm	-0.004***	-0.006***	-0.006***	-0.008***	-0.004***	
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	
Firm age = 1						-0.010***
						(0.001)
Firm age = 2						-0.002**
						(0.001)
Firm age = 3						-0.001
						(0.001)
Firm age = 4						-0.001
						(0.001)
Firm age = 5						0.000
						(0.001)
Constant	0.083***	0.085***	0.084***	0.085***	0.083***	0.084***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
County-Year-Month FE		x	x	x	x	x
Industry FE			x	x	x	x
Occupation FE				x	x	x
Firm Size FE					x	x

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the relationship between firm age and the likelihood of mentioning background checks in job postings. Firm age is measured as years since the firm's first observed job posting; a young firm has an age weakly less than 5. In all columns, firm age 6+ is the omitted reference category. All regressions are weighted by the inverse of the number of job postings a firm generates in a given year, so the estimated coefficients have firm-level interpretations. Standard errors are clustered at the firm level.

N = 91,099,295. Mean of dependent variable: 0.073.

Table 5: When do younger firms mention background checks less frequently?

	Dependent Variable: Job posting mentions a background check							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young firm	-0.006***	-0.006***	-0.005***	-0.005***	-0.005***	0.021	-0.006***	-0.016***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.005)
% HR $jobs_{t-1}$	0.014***	0.012**						
	(0.003)	(0.005)						
Young firm		0.004						
$\times$ % HR $jobs_{t-1}$		(0.006)						
% HR $language_{t-1}$			0.032***	0.039***				
			(0.002)	(0.003)				
Young firm				-0.017***				
$\times$ % HR $language_{t-1}$				(0.004)				
Log(Startup capital)					-0.009***	-0.008***		
					(0.001)	(0.001)		
Young firm						-0.002+		
$\times$ Log(Startup capital)						(0.001)		
Log (SBA loan)							0.001*	0.001
							(0.000)	(0.000)
Young firm								0.001*
$\times$ Log(SBA loan)								(0.000)
Constant	0.084***	0.084***	0.083***	0.083***	0.196***	0.181***	0.069***	0.075***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.008)	(0.008)
County-Year-Month FE	x	x	x	x	x	x		
County FE							x	x
Year-Month FE							x	x
Industry FE	x	x	x	x			x	x
Occupation FE	x	x	x	x	x	x	x	x
Firm Size FE	x	x	x	x	x	x	x	x

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows interaction effects of various firm-, industry-, and county-level characteristics on the relationship between firm age and the likelihood of mentioning background checks in job postings. These characteristics include the share of jobs the firm posted in the previous year that were for HR roles (columsn (1) and (2)) or mentioned HR (but were not for HR roles) (columns (3) and (4)); the logged dollar value of firms' NAICS2 sector startup capital costs (taken from Adelino, Schoar, and Severino (2015) Table A5) (columns (5) and (6)); and the log dollar value of SBA loans in the job's county in the current year (columns (7) and (8)). All regressions are weighted by the inverse of the number of job postings a firm generates in a given year and standard errors are clustered at the firm level.

Columns (1)-(4): N = 91,099,295. Columns (5)-(6): N = 72,367,269. Columns (7)-(8): N = 87,604,395. (Samples shrink due to data availability).

Table 6: Workers with recent convictions disproportionately work at younger firms, by crime type

	Dependent Variable: Work at a young firm							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recent conviction:								
Felony	0.014***							0.008***
	(0.001)							(0.001)
Violent		0.011***						0.005***
		(0.001)						(0.002)
Property			0.021***					0.009***
			(0.001)					(0.001)
DUI				0.003*				0.001
				(0.001)				(0.001)
Drug					0.010***			0.005***
					(0.001)			(0.001)
Public order						0.006***		0.002
						(0.001)		(0.001)
Financial							0.009***	-0.002
							(0.002)	(0.002)
Constant	0.069***	0.069***	0.069***	0.069***	0.069***	0.069***	0.069***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working, by the type of conviction.

N = 11,260,000. Mean of dependent variable: 0.069.

Conviction rates, conditional on any recent conviction: felony = 0.368; violent = 0.168; property = 0.265; DUI = 0.210; drug = 0.279; public order = 0.348; financial = 0.067. Note that these categories are not mutually exclusive, as individuals can be convicted of multiple crimes.

All columns include as controls age, county-year, sector, and individual fixed effects.

Table 7: Workers with recent convictions disproportionately work at younger firms when older firms can "afford" to not hire them

	Dependent Variable: Work at a young firm				
Sample:	All	All	Black men	White men	All
	(1)	(2)	(3)	(4)	(5)
Recent conviction	0.007***	0.009***	0.011***	0.008***	0.011***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Recent conviction × Pay gap	0.011***				
	(0.002)				
Recent conviction × Ban-the-Box		0.000	-0.007***	-0.003	
		(0.001)	(0.002)	(0.002)	
Recent conviction × New job ratio					-0.021**
					(0.007)
Constant	0.069***	0.069***	0.068***	0.065***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean(work at a young firm)	0.069	0.069	0.069	0.065	0.069
Mean(recent conviction)	0.038	0.038	0.109	0.050	0.038
N	11,260,000		11,260,000	1,022,000	4,195,000
					11,260,000

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working, by various frictions.

All columns include as controls age, county-year, sector, and individual fixed effects. Note that the pay gap, Ban-the-Box indicator, and new job ratio are collinear with county-year fixed effects, and so do not appear on their own in the regressions.

-

Table 8: Individuals with recent convictions who work at young firms tend to have poor outcomes

Dependent Variable:	Not working in 2 years		Have a conviction next year	
Sample:	Workers	All individuals	Workers	All individuals
Omitted group:	*Work at old firm*	*Not working*	*Work at old firm*	*Not working*
	(1)	(2)	(3)	(4)
Panel A: Outcomes by age of firm				
Recent conviction	0.055***	-0.037***	0.080***	0.095***
	(0.001)	(0.001)	(0.000)	(0.000)
Work at young firm	0.037***	-0.545***	0.001***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Recent conviction	0.010***	0.102***	0.011***	-0.003***
× Work at young firm	(0.002)	(0.002)	(0.000)	(0.000)
Work at established firm		-0.601***		-0.001**
		(0.000)		(0.000)
Recent conviction		0.101***		-0.015***
× Work at est. firm		(0.001)		(0.000)
Panel B: Outcomes by age and success of firm				
Recent conviction	0.055***	-0.037***	0.080***	0.095***
	(0.001)	(0.001)	(0.000)	(0.000)
Work at unsuccessful young firm	0.101***	-0.472***	0.002***	0.002***
	(0.001)	(0.001)	(0.000)	(0.000)
Recent conviction	-0.003	0.084***	0.015***	0.000
× Work at unsuccessful young firm	(0.004)	(0.004)	(0.001)	(0.000)
Work at successful young firm	0.026***	-0.558***	0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Recent conviction	0.010***	0.103***	0.010***	-0.004***
× Work at successful young firm	(0.002)	(0.002)	(0.000)	(0.000)
Work at established firm		-0.601***		-0.001**
		(0.000)		(0.000)
Recent conviction		0.101***		-0.015***
× Work at est. firm		(0.001)		(0.000)
Mean(dep var)	0.134	0.321	0.008	0.010
Mean(dep var | recent conviction)	0.200	0.422	0.094	0.100

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions and working at a young firm (under the age of 5) predict short-term outcomes, including employment and recidivism.

Columns (1) and (3): N = 11,260,000. Rate of working at a young firm: 0.069; an unsuccessful young firm: 0.011; a successful young firm: 0.058.

Columns (2) and (4): N = 16,640,000. Rate of working at a young firm: 0.051; an unsuccessful young firm: 0.008; a successful young firm: 0.043; an old firm: 0.672.

All columns include indicators for sex, race/ethnicity, age, county-year, and sector. Note that individual fixed effects are not included; instead, we include controls for past earnings. Constants are omitted for space. A "successful" young firm is one in which the firm survives to at least age 5. In columns (1) and (3), the omitted category is firms age 6 and older; in columns (2) and (4), the omitted category is individuals who are not currently working for a firm.

Table 9: Counties with more jobs at young firms have better employment outcomes for individuals with recent convictions

Dependent Variable:	Employment rate					Recidivism rate	
Sample:	All	Misdemeanor previous year		Felony previous year		Misd. prev. year	Fel. prev. year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS, Unweighted							
Log employment	0.003**	0.008***	0.005**	0.009***	0.005**	0.003**	0.002
at young firms	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Panel B: 2SLS, Unweighted							
Log employment	0.024*	0.091***	0.059**	0.092***	0.057**	0.022	0.008
at young firms	(0.010)	(0.026)	(0.023)	(0.025)	(0.022)	(0.013)	(0.013)
Panel C: OLS, Weighted by Labor Force Size							
Log employment	0.007**	0.022**	0.014***	0.024***	0.017***	0.003	0.001
at young firms	(0.002)	(0.004)	(0.004)	(0.005)	(0.004)	(0.002)	(0.003)
Panel D: 2SLS, Weighted by Labor Force Size							
Log employment	-0.018	0.086	0.106*	0.200*	0.218**	-0.035	-0.021
at young firms	(0.022)	(0.005)	(0.047)	(0.078)	(0.078)	(0.051)	(0.030)
FEs in Notes	x	x	x	x	x	x	x
Emp. Rate Ventile FE			x		x	x	x
Unweighted mean(dep var)	0.935	0.626	0.626	0.465	0.465	0.155	0.112
Weighted mean(dep var)	0.936	0.603	0.603	0.449	0.449	0.158	0.112

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

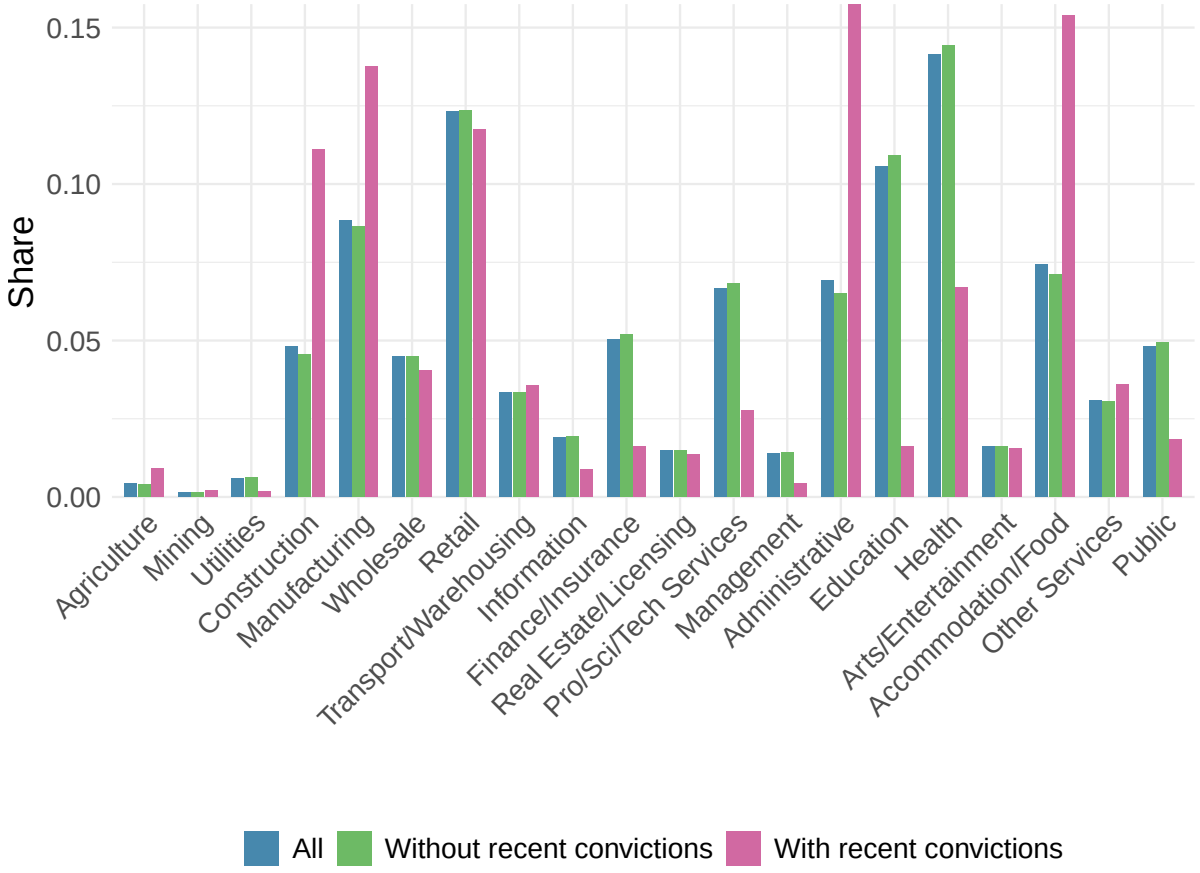
Note: This table demonstrates how local employment by young firms may improve outcomes for individuals with criminal records by regressing county-year level outcomes on the log employment by firms age 5 and under.
All columns include fixed effects for county, year, established firm employment ventile, misdemeanor rate ventile, and felony rate ventile. Columns (3), (5), (6), and (7) additionally include fixed effects for county overall employment rate ventiles.
In column (1) we consider the overall employment rate (based on individuals in the labor force). In columns (2)-(5) we consider whether individuals with misdemeanor and felony convictions in the previous year are currently working. In column (6), the dependent variable is the fraction of individuals convicted of a misdemeanor in the previous year who are convicted of another misdemeanor this year. In column (7), the dependent variable is the comparable measure for individuals with felonies.

N = 6,686. Observations are at the county-year level and are unweighted in Panels A and B but weighted by county's labor force size in 2000 in Panels C and D. Standard errors are clustered at the county level.

In 2SLS regressions (Panels B and D), log employment at young firms is instrumented by the predicted young firm employment based on industry trends and a county's historical industry composition (see text for details.)

First stage F-stats: Unweighted, without controlling for the overall employment rate: 35.2; with controlling: 27.6. Weighted, without controlling: 26.5; with controlling: 24.5.

Figure 1: Sectoral distributions for workers, by conviction history



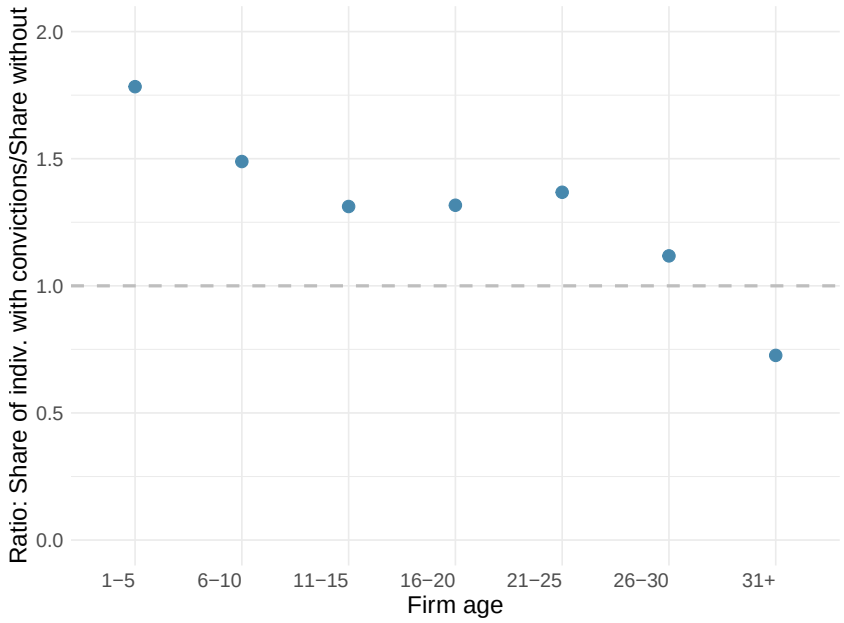
Note: This figure plots the sectoral distribution for workers, both in total ("All") as well as split on whether an individual has recent convictions in the past 7 years. (Note that the "All" values are weighted averages of the split series.)

Figure 2: Firm age distributions for workers, by conviction history

(a) Distributions

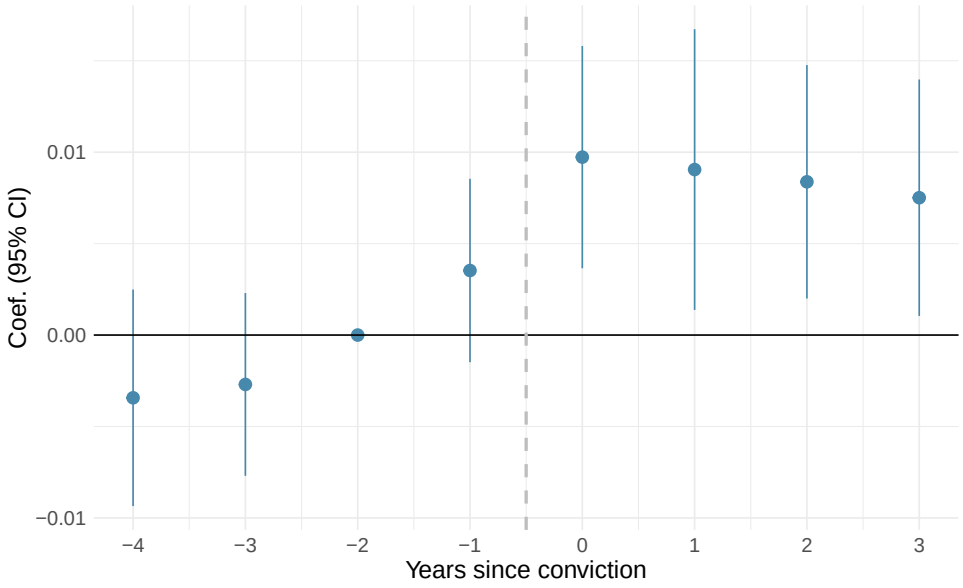


(b) Relative shares



Note: Panel a of this figure plots the firm age distribution for workers, both in total ("All") as well as split on whether an individual has had any convictions in the past 7 years. (Note that the "All" values are weighted averages of the split series.) Panel b plots the ratio of shares, i.e., the share of individuals with convictions working at firms of a certain age divided by the share of individuals without convictions in that same age bin.

Figure 3: Event study of working at a young firm, conditional on working



Note: This figure plots event study coefficients from a regression of working at a young firm (under the age of 5), around the time of an individual's first criminal conviction, controlling for age, county-year, sector, and individual fixed effects. Sample restricts to individuals who are working every year 2010-2017.
N = 4,238,000. Mean of dependent variable: 0.046.

Table A.1: Workers with recent convictions disproportionately work at younger firms, particularly as non-top employees

Dependent variable: work at firm that is…	Age 1	Age under 5, as the top earner	Age under 5, as top 3 earner
	(1)	(2)	(3)
Recent conviction	0.002***	0.000	0.015***
	(0.000)	(0.000)	(0.000)
Constant	0.010***	0.011***	0.021***
	(0.000)	(0.000)	(0.000)
Mean(dep var)	0.010	0.011	0.021
Mean(recent conviction)	0.038	0.038	0.038

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at new firms (age 1) or young firms in leadership positions (i.e., top earners).

N = 11,260,000.

All columns include as controls age, county-year, sector, and individual fixed effects.

Table A.2: Robustness: Workers with recent convictions disproportionately work at younger firms

Dependent variable: work at a...	Young firm	Unsuccessful young firm	Successful young firm	Young firm
Sample:	All	All	All	Job changers
	(1)	(2)	(3)	(4)
Recent conviction	0.007***	0.002***	0.007***	0.011***
	(0.001)	(0.000)	(0.001)	(0.001)
Constant	0.069***	0.011***	0.058***	0.138***
	(0.000)	(0.000)	(0.000)	(0.000)
Earnings percentile FE	x			
Mean(dep var)	0.069	0.011	0.058	0.139
Mean(recent conviction)	0.038	0.038	0.038	0.077
N	11,260,000	11,260,000	11,260,000	3,452,000

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms. Column (1) adds controls for the worker's current earnings. Columns (2) and (3) consider separately working a unsuccessful versus successful young firms, where a "successful" young firm is one that survives to at least age 5 and an "unsuccessful" young firm is one that does not. Column (4) restricts the sample to workers who are changing jobs, i.e., those whose top-paying firm in the current year is different from their top-paying firm in the previous year.

All columns include as controls age, county-year, sector, and individual fixed effects; column (1) adds fixed effects for the within-sample percentile of current annual earnings.

Table A.3: Individuals with recent convictions are less likely to work

	Dependent Variable: Work at any firm					
	(1)	(2)	(3)	(4)	(5)	(6)
Recent conviction	-0.139***	-0.147***	-0.161***	-0.135***	-0.118***	-0.021***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Male		-0.005***	-0.004***	0.007***	-0.003**	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Black		-0.075***	-0.035***	-0.032***	-0.020***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Other		-0.075***	-0.035***	-0.032***	-0.037***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Hispanic		-0.034***	-0.010***	-0.001***	0.006***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Constant	0.768***	0.792***	0.782***	0.774***	0.774***	0.763***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Age FE		x	x	x	x	x
County-year FE			x	x	x	x
Sector FE				x	x	x
Past earnings bin FE					x	
Individual FE						x

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working. The columns gradually build up model (1), slowly adding controls as demonstrated in the table footer.

N = 16,640,000. Mean of dependent variable: 0.723.

Table A.4: Workers with recent convictions disproportionately work at younger firms, regardless of firm size

	Dependent Variable: Work at a young firm							
Sample:	Emp 1-19 [N = 1,383,000]		Emp 20-49 [N = 918,000]		Emp 50-99 [N = 800,000]		Emp 100+ [N =7,389,000]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recent conviction	0.036***	0.002	0.037***	0.007**	0.032***	0.009***	0.021***	0.004***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.000)	(0.001)
Constant	0.179***	0.187***	0.113***	0.115***	0.086***	0.090***	0.026***	0.028***
	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)
Individual FE		x		x		x		x
Mean(work at a young firm)	0.187	0.187	0.115	0.115	0.091	0.091	0.028	0.028
Mean(recent conviction)	0.038	0.038	0.047	0.047	0.046	0.046	0.034	0.034

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working at firms of different employment sizes.

All columns include as controls age, county-year, and sector fixed effects, as well as indicators for sex and race/ethnicity (which are collinear with individual fixed effects in even columns).

Table A.5: Workers with recent convictions disproportionately work at younger firms, across sectors

Sample:	Dependent Variable: Work at a young firm									
	Natural Resources [N = 71,000]		Construction [N = 674,000]		Manufacturing [N = 1,043,000]		Trade and Transportation [N = 2,142,000]		Information [N = 155,000]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Recent conviction	0.016***	0.008	0.032***	0.001	0.011***	0.000	0.031***	0.007***	0.015***	0.003
	(0.005)	(0.008)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.005)
Constant	0.127***	0.120***	0.101***	0.098***	0.030***	0.029***	0.044***	0.048***	0.041***	0.038***
	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Individual FE		x		x		x		x		x
Mean(work at a young firm)	0.120	0.120	0.098	0.098	0.029	0.029	0.048	0.048	0.038	0.038
Mean(recent conviction)	0.067	0.067	0.084	0.084	0.056	0.056	0.034	0.034	0.016	0.016
Sample:	Financial Services [N = 563,000]		Professional Services [N = 1,625,000]		Education and Health Services [N = 2,109,000]		Leisure and Hospitality [N = 1,190,000]		Other Services [N = 307,000]	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Recent conviction	0.035***	-0.001	0.044***	0.012***	0.055***	0.008***	0.031***	0.008***	0.041***	0.003
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.005)
Constant	0.046***	0.045***	0.083***	0.087***	0.046***	0.048***	0.158***	0.155***	0.154***	0.147***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Individual FE		x		x		x		x		x
Mean(work at a young firm)	0.045	0.045	0.088	0.088	0.048	0.048	0.156	0.156	0.147	0.147
Mean(recent conviction)	0.015	0.015	0.047	0.047	0.012	0.012	0.071	0.071	0.040	0.040

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working in different sectors.

All columns include as controls age, county-year, and sector fixed effects, as well as indicators for sex and race/ethnicity (which are collinear with individual fixed effects in even columns).

Table A.6: Rotemberg weights for Bartik instrument, unweighted version

Dependent Variable:	2000 Normalized share of industry in county (Z-score)				
Industry:	722	213	713	561	812
	(1)	(2)	(3)	(4)	(5)
Misdemeanor rate (2001, Z-score)	0.022**	-0.803	0.004	-0.003	0.019**
	(0.007)	(0.437)	(0.010)	(0.008)	(0.007)
Felony rate (2001, Z-score)	0.015*	-0.508	0.030**	0.028***	-0.009
	(0.007)	(0.433)	(0.010)	(0.008)	(0.007)
Employment rate (2000, Z-score)	-0.000	-0.344	0.013	0.019**	0.015*
	(0.006)	(0.357)	(0.009)	(0.006)	(0.006)
N (county-industry pairs)	392	255	368	376	387

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table correlates industry 2000 shares of the industries with top 5 highest Rotemberg weights with 2001 misdemeanor and felony rates (for individuals convicted in 2000) and the 2000 employment rate, for the industry-county pairs underlying column (3) of Table 9 Panel B. All variables are converted to Z-scores (in this table only) for easier interpretation.

Observations are unweighted. Constants are omitted. Observation counts vary by column because not all counties have all industries.

NAICS 722: Food services and drinking places. Rotemberg weight: 0.396. Industry-specific coefficient of the effect of log young firm employment on the employment rate of individuals with misdemeanors: 0.065.

NAICS 213: Prime contracting (construction). Rotemberg weight: 0.266. Industry-specific coefficient: 0.028.

NAICS 713: Amusement, gambling, and recreation. Rotemberg weight: 0.084. Industry-specific coefficient: 0.027.

NAICS 561: Administrative and support services. Rotemberg weight: 0.065. Industry-specific coefficient: 0.167.

NAICS 812: Personal and laundry services. Rotemberg weight: 0.046. Industry-specific coefficient: 0.133.]

Sum (mean) of positive Rotemberg weights: 1.15 (0.03). Sum (mean) of negative Rotemberg weights: -0.15 (-0.01).

Correlation of Rotemberg weights and industry-specific coefficient: 0.0455.

Table A.7: Rotemberg weights for Bartik instrument, weighted version

Dependent Variable:	2000 Normalized share of industry in county (Z-score)				
Industry:	722	561	213	541	713
	(1)	(2)	(3)	(4)	(5)
Misdemeanor rate (2001, Z-score)	-0.012*	-0.020	-0.087	-0.020**	-0.024**
	(0.005)	(0.011)	(0.158)	(0.007)	(0.009)
Felony rate (2001, Z-score)	0.013***	0.046***	-0.167	0.035***	0.026***
	(0.003)	(0.009)	(0.106)	(0.006)	(0.006)
Employment rate (2000, Z-score)	0.001	0.046***	-0.369*	0.046***	0.028***
	(0.004)	(0.010)	(0.182)	(0.007)	(0.008)
N (county-industry pairs)	392	376	255	388	368

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table correlates industry 2000 shares of the industries with top 5 highest Rotemberg weights with 2001 misdemeanor and felony rates (for individuals convicted in 2000) and the 2000 employment rate, for the industry-county pairs underlying column (3) of Table 9 Panel D. All variables are converted to Z-scores (in this table only) for easier interpretation.

Observations are weighted by county's labor force size in 2000. Constants are omitted. Observation counts vary by column because not all counties have all industries.

NAICS 722: Food services and drinking places. Rotemberg weight: 0.398. Industry-specific coefficient of the effect of log young firm employment on the employment rate of individuals with misdemeanors: 0.079.

NAICS 561: Administrative and support services. Rotemberg weight: 0.157. Industry-specific coefficient: 0.283.

NAICS 213: Prime contracting (construction). Rotemberg weight: 0.124. Industry-specific coefficient: 0.086.

NAICS 541: Professional, scientific, and technical services. Rotemberg weight: 0.104. Industry-specific coefficient: 0.031.

NAICS 713: Amusement, gambling, and recreation. Rotemberg weight: 0.082. Industry-specific coefficient: 0.087.

Sum (mean) of positive Rotemberg weights: 1.30 (0.03). Sum (mean) of negative Rotembergweights: -0.30 (-0.01).

Correlation of Rotemberg weights and industry-specific coefficient: -0.011.