

The Role of Young Firms as Employers of Marginalized Workers

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Abstract

Young firms are a disproportionate source of job creation, yet little is known about for whom they create jobs and the payoffs to these positions. Using administrative data on U.S. workers, firms, and crime, we show that they disproportionately employ workers with criminal records: following a conviction, workers become 21% more likely to take jobs at young firms. This reflects young firms' competitive disadvantage — drawing from a pool of workers established firms have screened out — rather than preferences. A Bartik instrument shows that growth in young-firm jobs raises employment and earnings for workers with records, but does not reduce recidivism.

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1 Introduction

Young firms play a special role in the economy, driving job creation and employment growth (Haltiwanger, Jarmin, and Miranda, 2013). 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 reproduce the hiring patterns of the broader labor market, including its disparities.¹ Studying young firms specifically is particularly important because the consequences of young firm employment for workers are ambiguous: young firms may offer large career growth opportunities (Ouimet and Zarutskie, 2014), but may expose these workers to greater employment volatility, given the high failure rates of young firms — only about half survive to age five (Sorenson et al., 2021; Headd, 2003). Understanding whether young firms expand employment access for workers excluded from traditional labor markets, why they do so, and what these jobs mean for workers’ outcomes is therefore central to evaluating young firms’ role in labor market inequality.

We study these questions in the context of workers with criminal records, who are useful and important for three reasons. First, they constitute one of the largest groups that face pervasive exclusion from employers in the United States. Nearly one-third of working-age Americans have a record, comparable to the share with four-year college degrees (Friedman, 2015). Yet, employers frequently avoid applicants with records (Pager, 2003; Mueller-Smith and T. Schnepel, 2021; Cullen, Dobbie, and Hoffman, 2023) despite evidence that, when hired, these workers perform as well as their colleagues (Minor, Persico, and Weiss, 2018). Second, the consequences of their access to employment are particularly important, given that employment not only provides a pathway to economic gains but also is one of the strongest predictors of desistance from crime — having implications for public safety (Uggen, 1999). 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. Observing disproportionate employment of this group at young firms and its consequences is informative about both what differentiates young from established employers in their hiring practices and whether the jobs young firms provide serve as meaningful pathways into the labor market for an otherwise excluded population.

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; we observe details of these convictions through the Criminal Justice Administrative Records System (CJARS) (Finlay and Mueller-Smith, 2021). 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 — and to track their subsequent employment and recidivism. 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

¹Two 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.

firms disproportionately employ workers with criminal records, to evaluate whether young firms' reliance on workers with criminal records reflects different recruiting and screening behavior, and to estimate the consequences of young firm employment for this population.

We begin by documenting that, on average, workers with criminal convictions are disproportionately employed by young firms. Although established firms remain the primary employers of workers with records — as they are for the general workforce — young firms play a disproportionate role relative to their share of general employment in absorbing this population. We show this using data on workers, 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 pre-conviction earnings.² 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 — comparing the same worker before and after acquiring a conviction, conditional on being employed — 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% 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; Fackler et al., 2019). We find these patterns persist regardless of firm size and whether young firms ultimately survive to at least age 5, suggesting the sorting reflects something specific to firm age rather than simple assortative or quality-based matching.

We next ask *why* young firms disproportionately employ workers with convictions. Using job postings data, we find that young firms mention conducting background checks significantly less often than older firms, even when we account for job and firm characteristics, providing evidence that young firms behave differently from established firms when hiring. We argue that this reflects young firms' competitive disadvantage in the labor market. Struggling to attract applicants against established employers, young firms draw from a residual pool that established firms have already screened out — and because this pool is thin and disproportionately composed of workers with records, screening on criminal history would simply leave positions unfilled (Campero and Kacperczyk, 2020; Bernstein, Townsend, and Xu, 2024). This competitive disadvantage mechanism generates specific predictions that we observe in the data. Sorting to young firms is strongest for workers with felony, property, and violent crime convictions — the offenses established employers screen against most aggressively (Cullen, Dobbie, and Hoffman, 2023; Holzer, Raphael, and Stoll, 2006) — and weakest for less-stigmatized offenses. Sorting weakens in jurisdictions with Ban-the-Box policies, which restrict firms' ability to screen on criminal history, particularly for Black men (Agan and Starr, 2018; Doleac and Hansen, 2020). And, sorting weakens in tighter labor markets, where established firms compete harder for workers and likely lower their screening thresholds.

We also consider alternative mechanisms. First, young firms may face *direct constraints* on screening,

²While we focus on workers in these analyses, it is notable that the gap in working at young firms appears even in our full sample that includes non-workers; among all individuals, 7% of those with recent convictions are working at young firms, while only 5% of those without recent convictions do so.

such as lack of formal HR infrastructure or financial resources to conduct background checks. However, young firms' lower background check rates persist even after controlling for measures of HR sophistication and for young firms with greater financial slack or capital access, suggesting that direct constraints alone cannot account for the sorting. Second, young firms may have *firm preferences* for workers with records, deliberately seeking workers whom other firms overlook, whether because they value traits associated with this population or because their managers have had prior exposure to workers with criminal records (Hjalmarsson et al., 2025). If young firms genuinely valued these workers, however, sorting should not depend on established firms' screening behavior; yet we find that it weakens precisely when established firms are constrained from screening. Finally, on the labor supply side, *worker preferences* could drive sorting if workers with convictions actively seek out employment at young firms because they prefer the flexibility or less regimented work that young firms offer. Our within-worker estimates suggest otherwise, as the same worker becomes more likely to work at a young firm after a conviction. Moreover, sorting varies systematically with established firms' screening intensity, a pattern difficult to reconcile with worker preferences alone.

In the final part of our paper, we explore the consequences of these matches for workers. Descriptively, 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 because established firms screened them out, making non-employment, not an established firm job, the realistic alternative. 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 benefits depend on firm stability: workers at young firms that ultimately survive fare better, while those at firms that quickly fail see outcomes comparable to the non-employed. Because 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 treatment effects of the jobs themselves, we turn to a causal framework to assess whether young firm employment generates meaningful improvements for this population.

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. Because a county's exposure to industry-level young firm growth may also reflect exposure to broader industry growth, we control for an analogous Bartik measure of established firm employment, isolating the young firm-specific variation. We find that counties with exogenously higher young firm employment have higher employment rates for individuals with criminal records, with much smaller effects on overall county employment rates. A doubling of the number of jobs at young firms in a county predicts a 2.2% higher overall employment rate, relative to the mean, but a 12.4% (16.3%) higher employment rate for individuals with recent misdemeanor (felony) convictions, relative to the mean; with these employment rates come high average annual earnings for individuals with recent convictions. This confirms 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 crucial labor market access point for workers whom established firms screen out — providing employment opportunities that would not otherwise exist for this population. That these jobs do not substantially reduce recidivism likely reflects the instability of young firms and the jobs they provide.

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, struggling to attract talent in a labor market where established firms increasingly recruit proactively (Black, Hasan, and Koning, 2024); yet, not much is known about *who* these young firms hire based on their constraints, or what these jobs mean for the workers who hold them. 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 match 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 the competition they face from older firms, can lead them to employ marginalized workers (controlling for worker age and other characteristics), complementing 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. And, by estimating the causal effects of young firm employment on workers’ subsequent outcomes, we provide some of the first evidence on whether the jobs young firms create for marginalized workers translate into durable labor market gains — or whether the instability of young firms undermines them.

This paper also contributes to research on criminal justice involvement and labor market inequality. 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., Holzer, Raphael, and Stoll, 2003; Decker et al., 2015; Mueller-Smith, 2015). Most closely related is concurrent work by Hjalmarsson et al. (2025), who use Swedish data to show that workers with criminal records sort to specific firms even within industry and that managers’ prior exposure to this population causally increases hiring. While that work locates employer heterogeneity in the preferences of certain firms or managers, we complement this work by identifying a more structural source, firm age, as a distinct margin of employer heterogeneity in the U.S. Young firms are far from the only firms that employ workers with criminal records, but they play an outsized role relative to their size — which, we argue, arises through their competitive disadvantage in the labor market rather than through manager characteristics or deliberate hiring preferences. Firm age, thus, is a margin through which competition redistributes excluded workers toward young firms. These findings challenge the prevailing narrative of uniform employer exclusion and point to young firms as a partial — though imperfect — source of labor market access for this population.

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. But the same competitive disadvantage that leads young firms to hire these workers also makes these firms less stable, limiting the durability of the inclusion they generate. Young firms expand access to employment for marginalized workers, but whether that access translates into lasting economic integration depends on the stability of the firms providing it.

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 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 (restricting to complete calendar years with full coverage), 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.

³These 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). These states are not uniformly more or less crime-ridden than the US as a whole: we plot burglary and aggravated assault crime rates in the four states versus national patterns in Figure A.1; Maryland crime rates closely track the national average, while Arizona tends to have higher crime rates and New Jersey and Wisconsin lower rates.

2.1.1 Measuring criminal 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; we use the 2022Q4 vintage of this 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 (as opposed to no year cutoff) for three reasons. First, in order to avoid censoring issues, which can arise because CJARS does not cover the entire lifespan of every individual in our sample, it is helpful to impose some cutoff where we can avoid censoring. Second, as we show in our event study framework, recency matters. Third, we pick seven years to match another benchmark in the context of criminal records — namely, the federal Fair Credit Reporting Act prohibits some reporting of criminal charges that do not lead to convictions after seven years. Note that, by definition, the convictions we study are not subject to this seven year rule; criminal convictions can appear on a person’s background check forever. 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. CJARS additionally records the nature of each conviction, including whether it is a felony or misdemeanor and its offense type — violent, property, DUI, drug, public order, or financial — which we use in our heterogeneity analyses in Section 4. Note that the CJARS data includes conviction data before our focal sample — 2010-2017 — which we leverage, such that we have no censoring issues when measuring recent 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 index age starting at 1, such that a new firm is age 1. While we show results with different firm age cutoffs below, our primary focus follows Fort et al. (2013) and compares firms aged 1-5 (“young”) to those 6 or older (“established”).⁴

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 individuals’ industries (6-digit NAICS codes) and locations

⁴Fort et al. (2013) refer to these groups as “young” and “mature”/“older.” Note that their classification calls firms young if they are strictly less than 5 years old; but, their age variable is indexed to start at 0, so this matches our measurement.

(counties).⁵

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, as well as their 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

2.2 Samples

We combine our various data components in order to study how criminal histories predict and affect young firm employment outcomes. We have three 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 2017.⁸ 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.⁹

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).¹⁰ To this filled-in

⁵For LEHD firms with multiple locations, the county we take is the county with the most employment for the firm in a sector-state pair (e.g., the county within Wisconsin with the most workers for a retail firm.). This calculation is performed by Census.

⁶The 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.

⁷In 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.

⁸Note that we sometimes control for lagged earnings below. For individuals in our earliest sample years, we measure their lagged earnings before 2010.

⁹We take the union of the LEHD and ILBD sample, such that individuals can be both self-employed and working.

¹⁰While 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

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.¹¹ Finally, for computational tractability while maintaining statistical power, we take a random sample: we keep a random 10% of individuals with no conviction histories in our data as well as all individuals with convictions in our sample.¹² 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.¹³

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 for jobs across the United States. 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 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

covered by the LEHD (e.g., the federal government, some agricultural firms), or being retired or deceased, we believe some of these misclassification 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.

¹¹CJARS 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).

¹²To 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.

¹³Throughout the paper, observation counts are rounded due to Census disclosure procedure.

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 (e.g., [Pager, 2003](#)).

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 30.¹⁴ 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.¹⁵ While we focus on workers in our analysis below, it is notable that the gap in working at young firms appears even in our full sample that includes non-workers; among all individuals, 7% of those with recent convictions are working

¹⁴Firm age is censored, so we cannot decompose this oldest group more.

¹⁵This is a large disparity, proportionally: as a point of reference, when [Ouimet and Zarutskie \(2014\)](#) document that young firms disproportionately hire young workers, they document that 26.9% workers at firms aged 1-5 are 25-34 years old, while 17.5% of workers at firms over the age of 20 are 25-34 years old.

at young firms, while only 5% of those without recent convictions do so.

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).

We start by estimating versions of the following model:

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

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 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.

¹⁶We pool all workers who have no available earnings information for 8 years in the past — e.g., because they were not adults at that time — and include a separate fixed effect for them. In total, we have 22 bins: one bin is for those with missing values, while others are constructed as bins of ten thousand nominal dollars, with the top group including those lagged earnings weakly exceeding \$200,000.

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 point higher likelihood of working at a young firm, a 13% increase relative to the mean.¹⁸ Note that, because this specification is estimated on the workers sample, the within-worker comparison is conditional on the individual being employed in both the pre- and post-conviction periods; we therefore interpret this estimate as capturing how a conviction shifts where a worker works, not whether they work.

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:

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

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 (6) 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. We find that our estimated post-conviction coefficients are similar in magnitude to those in column (6) of Table 2,

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

¹⁸Table A.1 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.

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. As we argue in Section 4, the presence of a criminal record — rather than having committed a crime — is meaningful, as the sorting dampens when firms cannot as easily observe such records.

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 (column (1) of Table A.2). When splitting out measures of working at a young firm as an employee (i.e., not the top earner of their firm) versus a likely manager or owner (i.e., the top earner of the firm), we find that our patterns are driven by the former (columns (2) and (3) of Table A.2).¹⁹ Since individuals with criminal records are more likely to turn to entrepreneurship (Hwang and Phillips, 2024; Finlay, Mueller-Smith, and Street, 2023), this rules out the possibility that our sorting patterns are driven by workers with convictions founding, owning, or managing these young firms rather than joining them as rank and file employees. Furthermore, the sorting patterns are not simply about workers with convictions being cheap labor — whether because young firms cannot afford to pay more, or because young firms are targeting workers whose limited alternatives allow for lower wages — as our findings hold after controlling for current earnings (column (1) of Table A.3, where we add fixed effects for current earnings percentile). Comparing two workers earning similar wages, the one with a criminal conviction is still meaningfully more likely to work at a young firm, suggesting the driver of sorting operates beyond the wage margin.

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.3). 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

¹⁹Note that we rank individuals at firms before restricting to their top-paying jobs and before taking a random sample in order to perform this analysis.

(e.g., small firms 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.3](#)).

3.3 Heterogeneity

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

First, we consider variation by firm size. A natural concern with our baseline findings is that they reflect sorting to small firms rather than young firms specifically. Firm size and age are correlated but conceptually distinct: a small firm that has operated for decades has had time to build employer brand, hiring infrastructure, and workforce relationships that a young firm of the same size has not. If the pattern we document is actually about working at small firms rather than young firms, it would suggest a different set of mechanisms than those we propose. We address this in 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 [3](#). 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, no individual fixed effects). 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, with individual fixed effects). We conclude that the fact that workers with convictions disproportionately sort to young firms is not wholly reflecting them sorting to small firms.

Second, we consider variation by sex and race/ethnicity. In Table [4](#), 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.

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.4 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 argue that the sorting arises from young firms’ competitive disadvantage in the labor market. Young firms struggle to compete with established firms and consequently create fewer barriers to employment than their older counterparts. We organize our evidence in three parts. First, we establish that young firms behave differently in the hiring process, using job postings data. Second, we develop the competitive disadvantage mechanism and test its predictions. Third, we consider alternative explanations — direct constraints on young firms’ screening capability, firm preferences for workers with records, and worker preferences for young firms — and show that none can fully account for the patterns we document.

4.1 Young firms’ different hiring behavior

Before examining why young firms hire workers with records, we first establish that they behave differently when hiring in ways that could generate the sorting. We leverage 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 (i.e., “criminal background check” or “background check”) and use them as an indicator proxy for whether firms screen out individuals with records. It is worth noting that young firms may still be conducting background checks, even if they do not mention them in their job postings. In this case, one interpretation of our analyses below is that younger firms *advertise* their usage of background checks less; if firms mention conducting background checks in part in hopes of discouraging applications from individuals with records, then younger firms may still be more “welcoming” of such

²⁰Note 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.

applicants if they mention background checks less frequently.

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:

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

where p is a job posting for a job at firm $j(p)$ first posted in county-year-month $ct(p)$; $n(j(p))$ is the 2-digit NAICS industry in which the firm operates; $o(p)$ 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 reported 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 5 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

We take these results as indicating that young firms screen out workers with criminal backgrounds less often than established firms, which may allow for our main findings that workers with criminal records disproportionately find jobs at young firms. We now turn to understanding *why* young firms screen less.

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. We show in Table A.5 that our results are qualitatively similar when we split our sample into two cohorts of 2015-2019 and 2020-2024. We also show that our results are consistent within the four states that we examine in our main Census data in Table A.6.

4.2 Competitive disadvantage in hiring for young firms

We argue that young firms screen less because they face a competitive disadvantage in the labor market that makes screening impractical. Young firms face well-documented challenges in attracting workers. Workers looking for jobs do not randomly apply to firms; they are more likely to apply to firms they recognize, and established firms have had longer time to build visibility and reputation (e.g., [Campero and Kacperczyk, 2020](#)). 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 ([Headd, 2003](#)). Evidence from our job postings data is consistent with this disadvantage, where 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.

Screening is only valuable if a firm has good alternatives to the screened-out applicants. Established firms, with deep applicant pools, can afford to filter on criminal history and still fill positions with clean-record workers. Young firms, on the other hand, facing a competitive disadvantage in attracting applicants, find themselves drawing from a *residual pool* — workers that established firms have already screened out and rejected. Because this pool is both thin and composed disproportionately of workers with records, raising the barriers to hiring by screening on criminal history would simply leave positions unfilled.

Critically, the composition of this residual pool — and therefore how many workers with records filter down to young firms — depends on how aggressively established firms screen. When established firms screen intensively against a particular type of offense or demographic group, more such workers 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 applicant pool is already the residual of what established firms left behind.

This mechanism generates three specific predictions. First, sorting should be strongest for crimes that established firms most aggressively screen against. Second, 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. Third, sorting should weaken when established firms' willingness to screen declines — namely, in tighter labor markets. In tight labor markets, established firms compete harder for all workers and may lower their screening standards rather than leave positions unfilled.

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 for 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 competitive disadvantage 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.²² 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 established 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.

Sorting is weaker when firms’ ability to screen is restricted. Our second 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 (Avery and Lu, 2021). By delaying when employers learn about criminal records, Ban-the-Box poli-

²²It 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 non-employed.

cies make employers marginally constrained in their ability to screen out potential hires with convictions.²³ Under the competitive disadvantage mechanism, Ban-the-Box policies should *reduce* sorting of workers with convictions 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 (1), we estimate whether the role of recent convictions in predicting working at a young firm varies by whether a 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 (2) and (3) therefore estimate the interaction separately for Black men and White men. For Black men (column (2)), 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 (3) 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.

These findings also speak to a broader puzzle in the Ban-the-Box literature. Studies of aggregate employment effects of Ban-the-Box have found mixed and often muted impacts on whether workers with records get hired at all (Doleac and Hansen, 2020; Agan and Starr, 2017; Rose, 2021). Our heterogeneity by firm age offers one candidate explanation for these muted aggregate effects: when establishments differ in their

²³Note 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. 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 a way that makes disclosure of Census results impossible. 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. 12% of worker-year observations in our workers sample are covered by Ban-the-Box under this definition.

screening intensity, restricting screening at one type of employer (here, established firms that are most able to screen) primarily reshuffles who hires workers with records — away from young firms and toward established firms — rather than changing whether these workers are hired at all. This echoes Becker’s (1971) insight that employer heterogeneity can dampen the aggregate effect of restricting employer discrimination.

Sorting is weaker when established firms’ willingness to screen declines. Our third test examines how sorting responds to labor market conditions, specifically labor market tightness. The competitive disadvantage mechanism predicts that sorting should depend not just on established firms’ ability to screen, but also on their willingness to do so. In tighter labor markets, established firms likely compete more 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 (4) 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 is 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 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 competitive disadvantage evidence. Taken together, across three distinct tests, we find consistent evidence for the competitive disadvantage mechanism. Sorting to young firms is strongest for heavily stigmatized crimes where established firms screen most intensively; sorting weakens when Ban-the-Box policies restrict established firms’ ability to screen, particularly for Black men where the policy binds most strongly; and sorting weakens when tight labor markets reduce established firms’ willingness to screen. In each case, the sorting of workers with records to young firms depends on established firms’ screening behavior—a pattern that the competitive disadvantage mechanism predicts but that alternative explanations struggle to account for. We turn to these alternatives next.

4.3 Alternative mechanisms

We consider three broad categories of alternative explanations: direct constraints on young firms’ ability to screen, preferences of young firms, and preferences of workers.

Direct constraints. Young firms may screen less not because of competitive pressure but simply because they lack the capability to do so. First, 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, and young firms may simply not “know” to screen, or lack the organizational infrastructure to do so consistently. Second, 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. Given young firms’ limited financial capital access (Robb and Robinson, 2014), young firms may skip any extensive screening and instead hire relatively cheap workers, and thus end up hiring more workers with criminal records. If direct constraints drive the sorting, the gap between young and established firms should attenuate when we account for HR sophistication and financial access.

We test for HR constraints in our job postings data by re-estimating model (3), controlling for two measures of HR constraints and sophistication. First, we examine whether the firm has hired for HR occupations in the previous year, excluding observations of brand new (age 0) firms that mechanically have no job postings from the previous year. Second, we consider the fraction of the firm’s non-HR job postings in the previous year that explicitly referred to “Human Resources.” As we show in Table 8, regardless of our measure of HR sophistication, our estimated coefficients on being a young firm remains largely unchanged. While these measures of HR sophistication are coarse and capture primarily *formal* HR infrastructure, these results are consistent with the fact that we saw sorting of workers with criminal convictions to young firms *regardless of firm size* (Table 3); if HR sophistication generally scales with firm size and is 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.

We next test the role of financial constraints in both our job postings and worker-level data. With our job postings data, we expand model (3) to explore whether mentions of background checks changes when there is more capital access for young firms. We measure capital access for young firms in two ways: industry-level average start-up cost, where young firms in industries with higher average start-up costs will face greater capital constraints (Adelino, Schoar, and Severino, 2015), and the volume of Small Business Administration (SBA)-backed loans in a county-year, which may reflect the ease of capital access for young firms in a location (Granja et al. 2022).²⁴ As shown in Column (5) to (8) of Table 8, the gap in background check mentions between young and established firms is smaller both in industries with lower startup costs and in counties with higher SBA lending—consistent with young firms screening more when they are less financially constrained. However, financial constraints do not fully explain the sorting, as the gap in background check rates between young and established firms persists even among young firms with greater financial access.

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.3). 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. Direct constraints thus play a contributing role—particularly financial constraints—but cannot fully account for the patterns we document.

Firm preferences. 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.²⁵ Young firms

²⁴These loans are made by private banks to small businesses — while being backed by the SBA. We pool two SBA loan programs together: 7(a) loans and 504 loans <https://www.sba.gov/funding-programs/loans/>.

²⁵Young firms could even tacitly approve of the risk-taking behavior that led to criminal convictions; as Griffin, Kruger, and

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 (Kacperczyk and Campero, 2025). Similarly, managers of young firms may simply know — and trust — individuals with criminal records; these network effects could induce sorting (Hjalmarsson et al., 2025).²⁶ Under a firm preferences mechanism, we would expect young firms’ hiring to not depend on what established firms do.²⁷

The evidence from Section 4.2 is difficult to reconcile with this account. If young firms actively valued workers with criminal records, we would expect them to favor workers with minor offenses — which signal a one-time mistake — over those with serious crimes. Instead, sorting is strongest precisely for the offenses that established employers most aggressively avoid. Furthermore, if firm preferences drove the sorting, it should not weaken when established firms are constrained from screening. Yet we find sorting weakens both under Ban-the-Box and in tight labor markets. These patterns are more consistent with young firms passively receiving the workers that established firms filter out than with young firms deliberately seeking them.

Worker preferences. Under a *labor supply* story driven by worker preferences, workers with convictions may actively prefer young firms. This could reflect stable preferences for flexibility, less bureaucracy, or environments more tolerant of nontraditional backgrounds, or it could reflect changes in search behavior after conviction — as workers narrow the scope and methods of their search, face geographic constraints from parole or probation, or lower their reservation wages (Smith and Broege, 2020; Sugie, 2018). An extensive literature on criminal records and employment establishes 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), making it unlikely that these workers are selectively choosing young firms over available alternatives at established employers. Our within-individual estimates (Table 2, column (6)) reinforce this point, showing 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.

Yet, this does not fully rule out workers’ post-conviction changes in search behavior or preferences. One

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.

²⁶Notably, 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.2).

²⁷As far as we know, young firms do not have a reputation as being “second chance” employers. For example, existing policies that encourage hiring of workers with criminal records, such as the Work Opportunity Tax Credit (WOTC), are disproportionately less often utilized by smaller — and likely younger — firms (Hamersma, 2011; Jain, Mommaerts, and Weaver, 2026). For instance, Jain, Mommaerts, and Weaver (2026) report a median quarterly employment at WOTC-subsidized firms of 1,304 workers.

specific supply-side channel we can test is whether the sorting operates through wages — if workers with records sort to young firms because their reservation wages fall after conviction, then comparing workers with similar current earnings should eliminate the pattern. We find that even among workers earning similar wages, those with convictions are significantly more likely to work at a young firm (Table A.3), suggesting the sorting is not driven by workers accepting lower pay at young firms.

In addition, a labor-supply driven account — whether based on stable or changed preferences — struggles to explain why the sorting varies systematically with established firms’ screening behavior. If workers with records gravitate toward young firms because of their own preferences or constraints, the rate at which they do so should not depend on whether established firms are restricted from screening (as under Ban-the-Box) or are competing harder for workers (as in tight labor markets). Yet we find precisely these patterns, suggesting that, while supply-side forces likely play some role in the sorting, the systematic dependence on established firms’ screening behavior points to competitive disadvantage as the primary driver.

5 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 of consideration.

Beyond providing immediate employment, young firm jobs may play a more fundamental role in disrupting the statistical discrimination that drives persistent exclusion of workers with records (Phelps, 1972). Employers cannot easily distinguish productive workers with records from genuinely risky ones, and so screen out the group. Young firm employment generates observable work histories for workers who would otherwise remain unknown to the labor market — potentially allowing future employers to update their beliefs about individual productivity rather than relying on group-level screening. Whether this translates into persistent improvements in workers’ labor market outcomes is a question we take up below.

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, earnings, 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 comparisons 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:

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

and

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

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 employment — whether the individual is non-employed two years in the future — and short-term recidivism — namely, whether the individual has any criminal convictions in the next year.²⁸ 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 .²⁹

Future employment. In Table 9, 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% (the mean plus the coefficient on having a recent conviction), 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.³⁰

These patterns admit three interpretations. First, the higher future employment rates of young firm workers with convictions relative to the non-employed may reflect selection, if the individuals who find jobs at young firms are persistently more “employable” than those who find no job. Second, these patterns could reflect employment persistence — workers who find any job are more likely to remain employed,

²⁸We 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.

²⁹We present estimates with individual fixed effects in Table A.7. For studying future employment, the estimated interactions between having a recent conviction and working a young firm are qualitatively similar to the estimates without individual fixed effects. We do not estimate these specifications for recidivism as the outcome, as there is a mechanical relationship: within a worker’s trajectory, they mechanically have a future conviction in the years before they have a “recent conviction.”

³⁰Note 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 due to movement restrictions for individuals on parole, e.g.), in which case they may appear more often employed in the future than their clean-record counterpart.

and young firm employment mechanically increases the likelihood of continued employment in subsequent years. Under this interpretation, young firm jobs are valuable primarily because they keep workers attached to the labor market. Third, young firm employment may generate information about worker quality that facilitates future hiring. “Good” workers with records, who would otherwise be unobservable to employers, acquire a performance record at young firms that signals their productivity to future employers. Under this interpretation, young firm employment serves not merely as a bridge to continued employment, but as a mechanism for resolving the statistical discrimination that drives persistent exclusion of this population. While we cannot distinguish between these interpretations, the latter suggests that the benefits of young firm employment for workers with records may extend beyond the jobs themselves.

Recidivism. In Panel A columns (3) and (4) of Table 9, 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% more often recidivating within a year.³¹

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 young firms that fail.

In Panel B of Table 9, 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 an unsuccessful young firm, regardless of criminal record, tends to predict significantly larger future non-employment rates compared 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

³¹Note 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.

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 9 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 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 but only marginally 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.

Again, 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 see 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 firm 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 Justice Outcomes Explorer (JOE) program for 2010-2018 (Finlay, Mueller-Smith, and the CJARS team, 2024), 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),³² 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 young 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, as suggested by Goldsmith-Pinkham, Sorkin, and Swift (2020).

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

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

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:

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

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 2000.³³ Note that the shares do not sum up to 1 within a county. In addition to the Bartik instrument, we construct a similar measure for employment at *established*

³²Our 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.

³³Namely, $z_{nc,t_{2000}}$ 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 separable 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).

firms (i.e., those over age 5), which we use as a control below to account for general economic trends across industries; this amounts to replacing the g_{nt} with the national employment at *established* firms in industry n in year t .

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 and established 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.³⁴ 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*).³⁵

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 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 three approaches to assuage this concern.

First, as mentioned above, we control for the predicted measure of jobs at established firms, given by our second Bartik variable. This second Bartik variable predicts the number of jobs at established firms in the county, based on the national employment at *established* in that industry and the industry distribution in the county in 2000. We discretize this Bartik variable into ventiles and include fixed effects for the ventiles; this allows us to separately estimate the role of jobs specifically at young firms.

Second, instead of using the established firm Bartik variable, in appendix results 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.

Third, we examine whether our instrumented young firm employment predicts better outcomes for the general population compared to individuals with recent convictions to show that young firms are of particular relevance to individuals with recent convictions.

³⁴In 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.

³⁵We 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.

Results. We present our OLS and 2SLS regressions in Table 10. 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, earnings, and recidivism rates for individuals with prior misdemeanors and felonies.³⁶ 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; note that these specifications include granular controls for our Bartik variable for established firms, such that we are specifically considering the marginal effect more jobs at *young* firms. 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.³⁷ For example, in our unweighted 2SLS regressions in Panel B, a doubling of number of jobs at young firms predicts a 2.2% higher overall employment rate, relative to the mean, but a 12.4% higher employment rate for individuals with recent misdemeanor convictions (column (2)), relative to the mean, and a 16.3% higher employment rate for those with recent felony convictions (column (3)), 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).

These patterns extend to earnings as well: more jobs at young firms predicts higher average W-2 income for individuals with criminal convictions and a larger fraction of individuals earning above the poverty line (columns (4)-(5)). Note that these variables are based on W-2 income for *all* individuals with convictions, i.e., unconditional on working; for individuals who are not working at all in a year, they have W-2 income of zero, which is included when the CJARS program calculates the averages. This means that an increase in average income is consistent with higher wages, more hours worked, or longer employment spells.

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 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.

³⁶Note 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.

³⁷It is possible that the stronger effects for workers with recent convictions arise if young firms are particularly prevalent in industries that disproportionately hire workers with criminal records. In Table A.8, we restrict the instrumental variable calculations to exclude sectors where more than 10% of individuals with recent convictions work — construction, manufacturing, administrative, and accommodation and food (Figure 1) — and find similar results.

These large employment effects for individuals with convictions persist when we flexibly control for the actual overall employment rate and employment at established (instead of the industry-predicted employment at established firms) in Table A.9: *conditional on actual general employment*, 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.

When we turn to recidivism rates in columns (8)-(9), we see limited evidence of jobs at young firms leading to lower crime. In column (8), the dependent variable is the fraction of individuals with misdemeanors in the previous year who have another misdemeanor in the current year; column (9) considers the felony rates of individuals with felony convictions in the previous year. Consistent with our analysis in Table 9, 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 little 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 specification. The lack of a meaningful relationship is consistent with our findings in Table 9, 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.10 and A.11 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 column (2) specification of Table 10³⁹) of 0.378 in the unweighted specification and 0.397 in the weighted specification. The next-largest weight industry in our unweighted specification (NAICS 213 prime contracting in construction) has a weight of 0.264; in our weighted specification, the next-largest weight industry (NAICS 561 administrative and support services) has a weight of only 0.154. 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

³⁸Note 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.

³⁹Results are very similar if we use other columns. For instance, under the Table A.9 column (2) specification, NAICS 722 has a Rotemberg weight of 0.396 in the unweighted specification and 0.398 in the weighted specification.

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.11). 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 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.10 and A.11, 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 prime contracting construction firms has a higher coefficient (0.102) in the unweighted specification than the pooled 2SLS coefficient (0.075) in column (2) of Table 10 Panel B. This is consistent with the fact that construction firms 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 imperfect employment opportunities for workers whom established firms screen out.

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.

This paper documents a complementary, market-driven pathway to inclusion — one that operates without deliberate policy intervention. Young firms, struggling to compete against established employers for workers, draw from a residual pool of applicants that established firms have already screened out. This competitive disadvantage leads young firms to conduct less screening and consequently become disproportionate employers of individuals with criminal convictions, with this pattern strengthening precisely when established firms screen most intensively, and weakens when their ability or willingness to screen declines. In this way, competitive market dynamics partially disrupt the statistical discrimination equilibrium that classical models predict will generate persistent exclusion of workers with records (Phelps, 1972): young firms, by drawing from the residual pool, provide employment to workers who would otherwise be excluded.

The jobs young firms provide are meaningful but imperfect. Relative to non-employment — the realistic counterfactual for workers whom established firms screen out — young firm employment substantially improves future employment prospects and modestly reduces recidivism. However, these benefits depend critically on firm survival. These findings underscore the potential role of young firms in providing labor

market access for individuals with criminal records. At the same time, the instability of young firm employment — and our null effects on recidivism — caution against viewing young firm entry as a substitute for existing reentry and labor market policies. Whether policies that promote young firm entry would improve aggregate outcomes for individuals with records depends critically on the stability of the jobs created, a margin our evidence suggests matters considerably. Our findings are perhaps most useful in understanding how labor markets reabsorb workers excluded by established employers, and in informing the design of complementary policies that improve the quality and stability of the matches young firms generate.

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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
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
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*** (0.000)	0.047*** (0.000)	0.051*** (0.000)	0.034*** (0.000)	0.031*** (0.000)	0.009*** (0.001)
Male		-0.001*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)	0.000** (0.000)	
Non-Hispanic Black		-0.004*** (0.000)	-0.006*** (0.000)	-0.005*** (0.000)	-0.007*** (0.000)	
Non-Hispanic Other Race		0.021*** (0.000)	0.016*** (0.000)	0.012*** (0.000)	0.007*** (0.000)	
Hispanic		0.016*** (0.000)	0.006*** (0.000)	0.002*** (0.000)	0.001* (0.000)	
Constant	0.067*** (0.000)	0.065*** (0.000)	0.067*** (0.000)	0.068*** (0.000)	0.068*** (0.000)	0.069*** (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, regardless of firm size

Sample:	Dependent Variable: Work at a young firm							
	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)	0.002 (0.003)	0.037*** (0.002)	0.007** (0.003)	0.032*** (0.002)	0.009*** (0.003)	0.021*** (0.000)	0.004*** (0.001)
Constant	0.179*** (0.001)	0.187*** (0.000)	0.113*** (0.001)	0.115*** (0.002)	0.086*** (0.001)	0.090*** (0.000)	0.026*** (0.001)	0.028*** (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 4: 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*** (0.001)	0.008*** (0.001)	0.007*** (0.002)	0.010*** (0.001)	0.013*** (0.003)	0.006** (0.002)	0.014** (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*** (0.000)	0.065*** (0.000)	0.064*** (0.000)	0.068*** (0.000)	0.064*** (0.000)	0.135*** (0.000)	0.086*** (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 5: 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*** (0.000)	-0.006*** (0.001)	-0.006*** (0.001)	-0.008*** (0.001)	-0.004*** (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*** (0.000)	0.085*** (0.000)	0.084*** (0.000)	0.085*** (0.000)	0.083*** (0.000)	0.084*** (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 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*** (0.001)							0.008*** (0.001)
Violent		0.011*** (0.001)						0.005*** (0.002)
Property			0.021*** (0.001)					0.009*** (0.001)
DUI				0.003* (0.001)				0.001 (0.001)
Drug					0.010*** (0.001)			0.005*** (0.001)
Public order						0.006*** (0.001)		0.002 (0.001)
Financial							0.009*** (0.002)	-0.002 (0.002)
Constant	0.069*** (0.000)	0.069*** (0.000)	0.069*** (0.000)	0.069*** (0.000)	0.069*** (0.000)	0.069*** (0.000)	0.069*** (0.000)	0.069*** (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

Sample:	Dependent Variable: Work at a young firm			
	All	Black men	White men	All
	(1)	(2)	(3)	(4)
Recent conviction	0.009*** (0.001)	0.011*** (0.002)	0.008*** (0.001)	0.011*** (0.001)
Recent conviction $\times$ Ban-the-Box	0.000 (0.001)	-0.007*** (0.002)	-0.003 (0.002)	
Recent conviction $\times$ New job ratio				-0.021** (0.007)
Constant	0.069*** (0.000)	0.068*** (0.000)	0.065*** (0.000)	0.069*** (0.000)
Mean(work at a young firm)	0.069	0.069	0.065	0.069
Mean(recent conviction)	0.038	0.109	0.050	0.038
N	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 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: 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*** (0.000)	-0.006*** (0.000)	-0.005*** (0.000)	-0.005*** (0.000)	-0.005*** (0.001)	0.021 (0.001)	-0.006*** (0.000)	-0.016*** (0.005)
% HR jobs _{t-1}	0.014*** (0.003)	0.012** (0.005)						
Young firm × % HR jobs _{t-1}		0.004 (0.006)						
% HR language _{t-1}			0.032*** (0.002)	0.039*** (0.003)				
Young firm × % HR language _{t-1}				-0.017*** (0.004)				
Log(Startup capital)					-0.009*** (0.001)	-0.008*** (0.001)		
Young firm × Log(Startup capital)						-0.002 (0.001)		
Log (SBA loan)							0.001* (0.000)	0.001 (0.000)
Young firm × Log(SBA loan)								0.001* (0.000)
Constant	0.084*** (0.000)	0.084*** (0.000)	0.083*** (0.000)	0.083*** (0.000)	0.196*** (0.001)	0.181*** (0.001)	0.069*** (0.008)	0.075*** (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 (columns (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 vary due to data availability).

Table 9: 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:	<i>Work at old firm</i>	<i>Not working</i>	<i>Work at old firm</i>	<i>Not working</i>
	(1)	(2)	(3)	(4)
Panel A: Outcomes by age of firm				
Recent conviction	0.055*** (0.001)	-0.037*** (0.001)	0.080*** (0.000)	0.095*** (0.000)
Work at young firm	0.037*** (0.000)	-0.545*** (0.000)	0.001*** (0.000)	0.000** (0.000)
Recent conviction × Work at young firm	0.010*** (0.002)	0.102*** (0.002)	0.011*** (0.000)	-0.003*** (0.000)
Work at established firm		-0.601*** (0.000)		-0.001** (0.000)
Recent conviction × Work at est. firm		0.101*** (0.001)		-0.015*** (0.000)
Panel B: Outcomes by age and success of firm				
Recent conviction	0.055*** (0.001)	-0.037*** (0.001)	0.080*** (0.000)	0.095*** (0.000)
Work at unsuccessful young firm	0.101*** (0.001)	-0.472*** (0.001)	0.002*** (0.000)	0.002*** (0.000)
Recent conviction × Work at unsuccessful young firm	-0.003 (0.004)	0.084*** (0.004)	0.015*** (0.001)	0.000 (0.000)
Work at successful young firm	0.026*** (0.000)	-0.558*** (0.000)	0.001*** (0.000)	0.000 (0.000)
Recent conviction × Work at successful young firm	0.010*** (0.002)	0.103*** (0.002)	0.010*** (0.000)	-0.004*** (0.000)
Work at established firm		-0.601*** (0.000)		-0.001** (0.000)
Recent conviction × Work at est. firm		0.101*** (0.001)		-0.015*** (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. (Table A.7 presents versions of columns (1) and (2) with individual fixed effects.) 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 10: Counties with more jobs at young firms have better employment outcomes for individuals with recent convictions

Dependent Variable:	Employment rate			Log mean earnings		Share above poverty line		Recidivism rate	
Sample:	All	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: OLS, Unweighted									
Log employment at young firms	0.003** (0.001)	0.008*** (0.002)	0.009*** (0.002)	0.029*** (0.006)	0.046*** (0.009)	0.007*** (0.002)	0.007*** (0.002)	0.003*** (0.001)	0.002* (0.001)
Panel B: 2SLS, Unweighted									
Log employment at young firms	0.020* (0.009)	0.075*** (0.022)	0.073*** (0.021)	0.334*** (0.080)	0.401*** (0.110)	0.087*** (0.021)	0.067*** (0.018)	0.018 (0.010)	0.020 (0.012)
Panel C: OLS, Weighted by Labor Force Size									
Log employment at young firms	0.006** (0.002)	0.021*** (0.004)	0.024*** (0.005)	0.044*** (0.013)	0.092*** (0.020)	0.008* (0.004)	0.008* (0.003)	0.003 (0.003)	0.002 (0.003)
Panel D: 2SLS, Weighted by Labor Force Size									
Log employment at young firms	-0.011 (0.020)	0.066 (0.048)	0.156* (0.068)	0.118 (0.177)	0.673* (0.262)	0.032 (0.050)	0.091* (0.041)	-0.039 (0.043)	0.012 (0.027)
Unweighted mean(dep var)	0.935	0.626	0.465	9.426	8.731	0.319	0.166	0.155	0.112
Weighted mean(dep var)	0.936	0.603	0.449	9.380	8.693	0.303	0.159	0.158	0.112

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

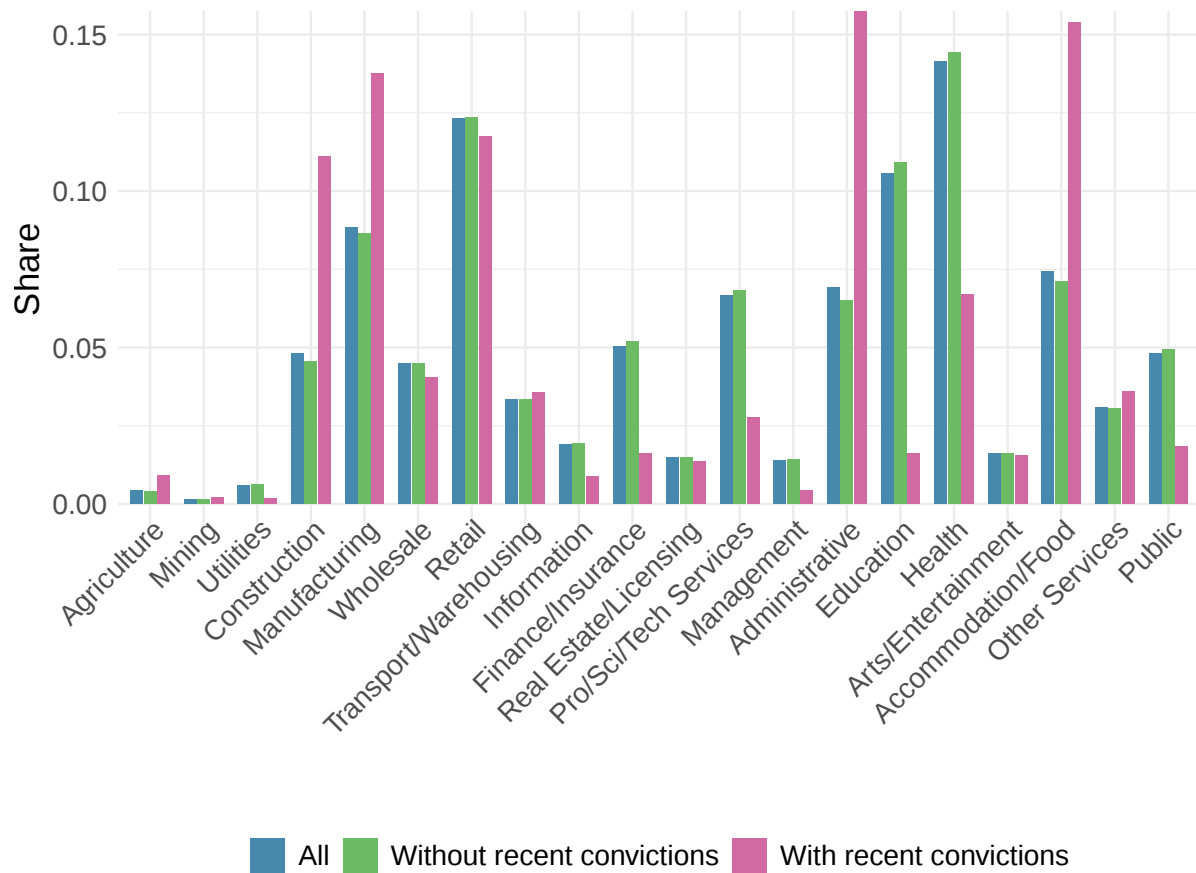
Note: This table demonstrates how local employment by young firms may affect outcomes for individuals with criminal records by regressing county-year level outcomes on the log employment by firms age 5 and under. See text for details. All columns include fixed effects for county, year, misdemeanor rate ventile, felony rate ventile, and ventile of a Bartik variable predicting the local employment at established firms.

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).

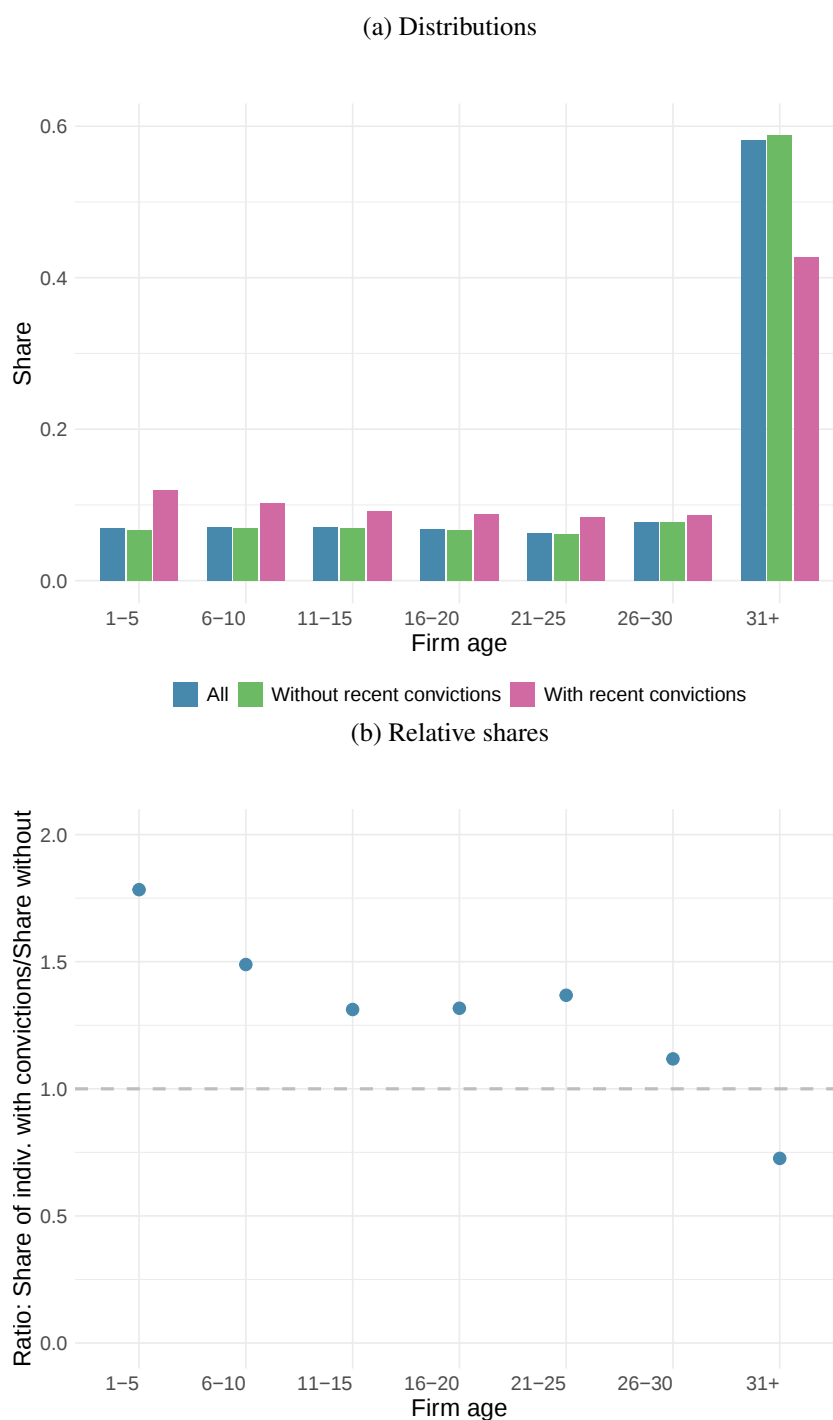
First stage F-stats: Unweighted: 44.5. Weighted, Bartik control: 26.5.

Figure 1: Sectoral distributions for workers, by conviction history



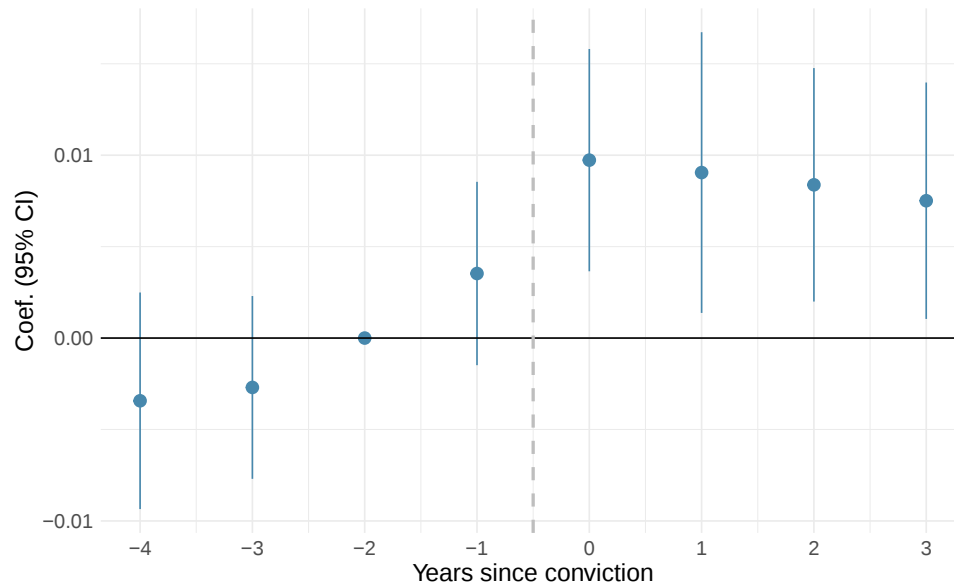
Note: This figure plots the sectoral distribution for workers, both in total ("All") as well as split on whether an individual has recent convictions in the past 7 years. (Note that the "All" values are weighted averages of the split series.)

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



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. Standard errors are clustered at the individual level. Sample restricts to individuals who are working every year 2010-2017.

N = 4,238,000. Mean of dependent variable: 0.046.

Table A.1: 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*** (0.000)	-0.147*** (0.001)	-0.161*** (0.001)	-0.135*** (0.001)	-0.118*** (0.000)	-0.021*** (0.001)
Male		-0.005*** (0.000)	-0.004*** (0.000)	0.007*** (0.000)	-0.003** (0.000)	
Non-Hispanic Black		-0.075*** (0.000)	-0.035*** (0.000)	-0.032*** (0.000)	-0.020*** (0.000)	
Non-Hispanic Other		-0.075*** (0.000)	-0.035*** (0.000)	-0.032*** (0.000)	-0.037*** (0.000)	
Hispanic		-0.034*** (0.000)	-0.010*** (0.000)	-0.001*** (0.000)	0.006*** (0.000)	
Constant	0.768*** (0.000)	0.792*** (0.000)	0.782*** (0.000)	0.774*** (0.000)	0.774*** (0.000)	0.763*** (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.2: 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)	0.000 (0.000)	0.015*** (0.000)
Constant	0.010*** (0.000)	0.011*** (0.000)	0.021*** (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.3: 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*** (0.001)	0.002*** (0.000)	0.007*** (0.001)	0.011*** (0.001)
Constant	0.069*** (0.000)	0.011*** (0.000)	0.058*** (0.000)	0.138*** (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.4: Workers with recent convictions disproportionately work at younger firms, across sectors

Sample:	Dependent Variable: Work at a young firm									
	Natural Resources		Construction		Manufacturing		Trade and Transportation		Information	
	[N = 71,000]		[N = 674,000]		[N = 1,043,000]		[N = 2,142,000]		[N = 155,000]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Recent conviction	0.016*** (0.005)	0.008 (0.008)	0.032*** (0.001)	0.001 (0.002)	0.011*** (0.001)	0.000 (0.001)	0.031*** (0.001)	0.007*** (0.001)	0.015*** (0.004)	0.003 (0.005)
Constant	0.127*** (0.003)	0.120*** (0.001)	0.101*** (0.001)	0.098*** (0.001)	0.030*** (0.000)	0.029*** (0.000)	0.044*** (0.000)	0.048*** (0.000)	0.041*** (0.001)	0.038*** (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		Professional Services		Education and Health Services		Leisure and Hospitality		Other Services	
	[N = 563,000]		[N = 1,625,000]		[N = 2,109,000]		[N = 1,190,000]		[N = 307,000]	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Recent conviction	0.035*** (0.002)	-0.001 (0.003)	0.044*** (0.001)	0.012*** (0.002)	0.055*** (0.001)	0.008*** (0.002)	0.031*** (0.001)	0.008*** (0.002)	0.041*** (0.003)	0.003 (0.005)
Constant	0.046*** (0.000)	0.045*** (0.000)	0.083*** (0.000)	0.087*** (0.000)	0.046*** (0.000)	0.048*** (0.000)	0.158*** (0.001)	0.155*** (0.000)	0.154*** (0.001)	0.147*** (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.5: Younger firms mention background checks less frequently in job postings, over time

	Dependent Variable: Job posting mentions a background check				
	(1)	(2)	(3)	(4)	(5)
Panel A: 2015-2019					
Young firm	-0.001 (0.001)	-0.002** (0.001)	-0.002* (0.001)	-0.005*** (0.001)	-0.002** (0.001)
Constant	0.071*** (0.001)	0.072*** (0.001)	0.072*** (0.001)	0.074*** (0.001)	0.072*** (0.001)
Panel B: 2020-2024					
Young firm	-0.003*** (0.001)	-0.008*** (0.001)	-0.008*** (0.001)	-0.009*** (0.001)	-0.006*** (0.001)
Constant	0.088*** (0.000)	0.091*** (0.000)	0.091*** (0.000)	0.091*** (0.000)	0.090*** (0.000)
County-Year-Month FE		x	x	x	x
Industry FE			x	x	x
Occupation FE				x	x
Firm size FE					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 across two time windows (2015-2019 versus 2020-2024). 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.

In Panel A: $N = 36,859,243$. Mean of dependent variable: 0.071.

In Panel B; $N = 54,250,035$. Mean of dependent variable: 0.088.

Table A.6: 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	-0.003 ⁺ (0.001)	-0.004** (0.001)	-0.005** (0.002)	-0.007*** (0.001)	-0.004* (0.001)	
Firm age = 0						-0.010*** (0.002)
Firm age = 1						-0.004 ⁺ (0.002)
Firm age = 2						0.001 (0.002)
Firm age = 3						-0.000 (0.002)
Firm age = 4						0.002 (0.002)
Constant	0.074*** (0.001)	0.075*** (0.001)	0.076*** (0.001)	0.077*** (0.001)	0.075*** (0.001)	0.076*** (0.001)
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
Mean of DV (ref. group)	0.074	0.074	0.074	0.074	0.074	0.074
Observations	8,274,371	8,274,371	8,274,371	8,274,371	8,274,371	8,274,371
R-squared	0.000	0.040	0.047	0.073	0.073	0.073

Standard errors in parentheses. ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the relationship between firm age and the likelihood of mentioning background checks in job postings among the 4 states used in our main analysis (Arizona, Maryland, New Jersey, and Wisconsin). Firm age is measured as years since the firm's first observed job posting; a young firm has an age weakly less than 5 (ages 0–4). In all columns, firm age 5+ 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 = 8,274,371. Mean of dependent variable: 0.074.

Table A.7: Individuals with recent convictions who work at young firms tend to have poor outcomes: Within-worker

Dependent Variable:	Not working in 2 years			
Specification:	Base (Table 9)		Add individual FEs	
Sample:	Workers	All individuals	Workers	All individuals
Omitted group:	<i>Work at old firm</i>	<i>Not working</i>	<i>Work at old firm</i>	<i>Not working</i>
	(1)	(2)	(3)	(4)
Panel A: Outcomes by age of firm				
Recent conviction	0.055*** (0.001)	-0.037*** (0.001)	0.001 (0.001)	-0.033*** (0.001)
Work at young firm	0.037*** (0.000)	-0.545*** (0.000)	-0.005*** (0.000)	-0.077** (0.000)
Recent conviction × Work at young firm	0.010*** (0.002)	0.102*** (0.002)	0.004** (0.000)	0.063*** (0.002)
Work at established firm		-0.601*** (0.000)		-0.081** (0.000)
Recent conviction × Work at est. firm		0.101*** (0.001)		0.058*** (0.001)
Panel B: Outcomes by age and success of firm				
Recent conviction	0.055*** (0.001)	-0.037*** (0.001)	0.001 (0.001)	-0.033*** (0.001)
Work at unsuccessful young firm	0.101*** (0.001)	-0.472*** (0.001)	0.017*** (0.001)	-0.046*** (0.001)
Recent conviction × Work at unsuccessful young firm	-0.003 (0.004)	0.084*** (0.004)	-0.001 (0.003)	0.052*** (0.003)
Work at successful young firm	0.026*** (0.000)	-0.558*** (0.000)	-0.009*** (0.000)	-0.084*** (0.000)
Recent conviction × Work at successful young firm	0.010*** (0.002)	0.103*** (0.002)	0.005*** (0.002)	0.065*** (0.002)
Work at established firm		-0.601*** (0.000)		-0.082*** (0.000)
Recent conviction × Work at est. firm		0.101*** (0.001)		0.058*** (0.001)
Mean(dep var)	0.134	0.321	0.134	0.321
Mean(dep var recent conviction)	0.200	0.422	0.200	0.422

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 age, county-year, and sector as well as individual fixed effects. 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 A.8: Counties with more jobs at young firms have better employment outcomes for individuals with recent convictions: Robustness analysis

Dependent Variable:	Employment rate			Log mean earnings		Share above poverty line		Recidivism rate	
Sample:	All	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: OLS, Unweighted									
Log employment at young firms	0.003** (0.001)	0.008*** (0.002)	0.008*** (0.002)	0.030*** (0.006)	0.046*** (0.009)	0.008*** (0.002)	0.007*** (0.002)	0.003*** (0.001)	0.002 (0.001)
Panel B: 2SLS, Unweighted									
Log employment at young firms	0.024* (0.011)	0.079*** (0.021)	0.076*** (0.022)	0.370*** (0.075)	0.425*** (0.094)	0.089*** (0.020)	0.068*** (0.017)	0.013 (0.009)	0.020 (0.013)
Panel C: OLS, Weighted by Labor Force Size									
Log employment at young firms	0.006** (0.002)	0.022*** (0.004)	0.025*** (0.005)	0.046*** (0.013)	0.097*** (0.020)	0.009* (0.004)	0.009** (0.003)	0.004 (0.003)	0.003 (0.003)
Panel D: 2SLS, Weighted by Labor Force Size									
Log employment at young firms	-0.039 (0.026)	0.065 (0.054)	0.140* (0.069)	0.053 (0.191)	0.656* (0.263)	0.011 (0.051)	0.112** (0.042)	-0.042 (0.049)	-0.017 (0.039)
Unweighted mean(dep var)	0.935	0.626	0.465	9.426	8.731	0.319	0.166	0.155	0.112
Weighted mean(dep var)	0.936	0.603	0.449	9.380	8.693	0.303	0.159	0.158	0.112

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

Note: This table repeats the analyses in Table 10 but restricts the instrumental variable calculations to exclude sectors where more than 10% of individuals with recent convictions work — construction, manufacturing, administrative, and accommodation and food. See text and Table 10 for details.

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: 25.9. Weighted, Bartik control: 19.8.

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

Dependent Variable:	Employment rate		Log mean earnings		Share above poverty line		Recidivism rate	
Sample:	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony	Misdemeanor	Felony
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS, Unweighted								
Log employment at young firms	0.005** (0.002)	0.005** (0.002)	0.021*** (0.005)	0.035*** (0.008)	0.004** (0.002)	0.005** (0.001)	0.003** (0.001)	0.002 (0.001)
Panel B: 2SLS, Unweighted								
Log employment at young firms	0.059** (0.023)	0.057* (0.022)	0.314*** (0.090)	0.430** (0.131)	0.070*** (0.021)	0.066*** (0.020)	0.022 (0.013)	0.008 (0.013)
Panel C: OLS, Weighted by Labor Force Size								
Log employment at young firms	0.014*** (0.004)	0.017*** (0.004)	0.025 (0.013)	0.071*** (0.017)	0.000 (0.003)	0.004 (0.003)	0.003 (0.003)	0.001 (0.003)
Panel D: 2SLS, Weighted by Labor Force Size								
Log employment at young firms	0.106* (0.047)	0.218** (0.078)	0.067 (0.154)	0.888** (0.296)	-0.002 (0.040)	0.106* (0.043)	-0.035 (0.051)	-0.021 (0.030)
Unweighted mean(dep var)	0.626	0.465	9.426	8.731	0.319	0.166	0.155	0.112
Weighted mean(dep var)	0.603	0.449	9.380	8.693	0.303	0.159	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 affect outcomes for individuals with criminal records by regressing county-year level outcomes on the log employment by firms age 5 and under. See text for details. All columns include fixed effects for county, year, misdemeanor rate ventile, felony rate ventile, and ventile for both the actual local employment at established firms and the actual local employment rate.

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: 27.6. Weighted, Bartik control: 24.5.

Table A.10: 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** (0.007)	-0.803 (0.437)	0.004 (0.010)	-0.003 (0.008)	0.019** (0.007)
Felony rate (2001, Z-score)	0.015* (0.007)	-0.508 (0.433)	0.030** (0.010)	0.028*** (0.008)	-0.009 (0.007)
Employment rate (2000, Z-score)	-0.000 (0.006)	-0.344 (0.357)	0.013 (0.009)	0.019** (0.006)	0.015* (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 (2) of Table 10 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: 0.378. Industry-specific coefficient of the effect of log young firm employment on the employment rate of individuals with misdemeanors: 0.053.

NAICS 213: Prime contracting (construction). Rotemberg weight: 0.264. Industry-specific coefficient: 0.102.

NAICS 713: Amusement, gambling, and recreation. Rotemberg weight: 0.084. Industry-specific coefficient: 0.043.

NAICS 561: Administrative and support services. Rotemberg weight: 0.073. Industry-specific coefficient: 0.173.

NAICS 812: Personal and laundry services. Rotemberg weight: 0.050. Industry-specific coefficient: 0.091.

Sum (mean) of positive Rotemberg weights: 1.14 (0.02). Sum (mean) of negative Rotemberg weights: -0.14 (-0.00).

Correlation of Rotemberg weights and industry-specific coefficient: 0.043.

Table A.11: 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* (0.005)	-0.020 (0.011)	-0.087 (0.158)	-0.020** (0.007)	-0.024** (0.009)
Felony rate (2001, Z-score)	0.013*** (0.003)	0.046*** (0.009)	-0.167 (0.106)	0.035*** (0.006)	0.026*** (0.006)
Employment rate (2000, Z-score)	0.001 (0.004)	0.046*** (0.010)	-0.369* (0.182)	0.046*** (0.007)	0.028*** (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 (2) of Table 10 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: 0.397. Industry-specific coefficient of the effect of log young firm employment on the employment rate of individuals with misdemeanors: 0.044.

NAICS 561: Administrative and support services. Rotemberg weight: 0.154. Industry-specific coefficient: 0.286.

NAICS 213: Prime contracting (construction). Rotemberg weight: 0.141. Industry-specific coefficient: 0.134.

NAICS 541: Professional, scientific, and technical services. Rotemberg weight: 0.099. Industry-specific coefficient: -0.012.

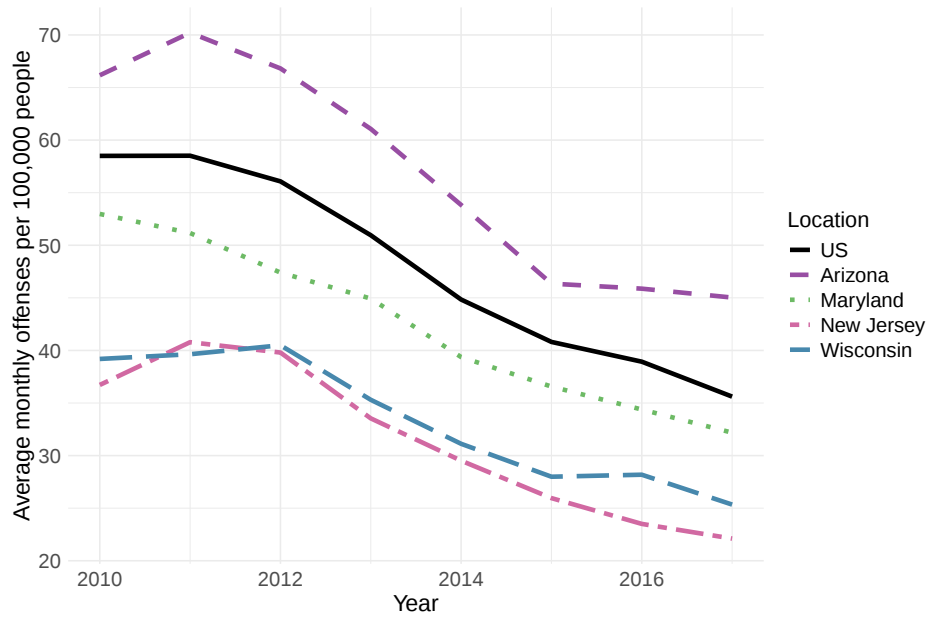
NAICS 713: Amusement, gambling, and recreation. Rotemberg weight: 0.082. Industry-specific coefficient: 0.062.

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

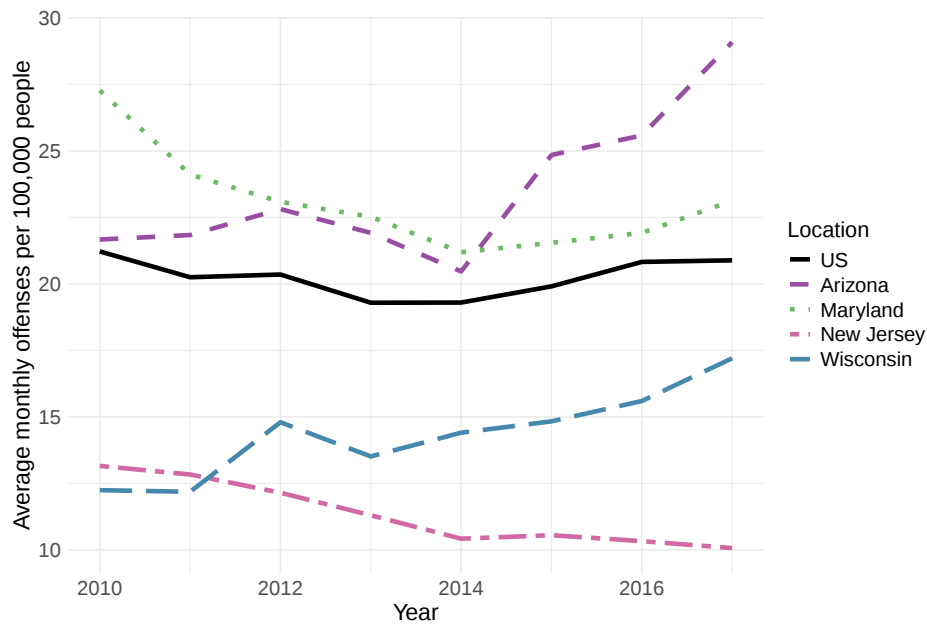
Correlation of Rotemberg weights and industry-specific coefficient: 0.013.

Figure A.1: Crime rates by location

(a) Burglary



(b) Aggravated assault



Note: Panel a of this figure plots the average monthly burglary rate (offenses per 100,000 people) by year for the US and our four main sample states; Panel b presents the same for aggravated assault. Data comes from the Uniform Crime Reporting (UCR) Program and is sourced from the FBI Crime Data Explorer (<https://cde.ucr.cjis.gov/LATEST/webapp/#/pages/explorer/crime/crime-trend>). We focus on burglary and aggravated assault rates because they have consistent coverage in the UCR over time. Note there is some seasonality within a year in terms of when crimes are recorded in the data.