Startups as Engines of Inclusion

Kylie Hwang
Northwestern University

Melanie Wallskog*
Duke University

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Abstract

Startups are widely recognized for their contributions to economic growth and innovation, but their role as engines of economic inclusion has received far less attention. Using administrative data on U.S. workers, firms, and the criminal justice system, we show that startups disproportionately employ those who are often excluded from traditional labor markets, namely workers with criminal records. These patterns hold both cross-sectionally and within-workers' employment trajectories before and after convictions. We argue that startups' relative inclusivity reflects restricted access to established employers, rather than a deliberate preference among workers with records to work for startups. Consistent with this mechanism, disproportionate startup employment is stronger when workers with records face greater exclusion from established employers. We evaluate the outcomes of startup employment, finding that while startups offer better economic prospects than unemployment, the greater instability of young firms may contribute to higher recidivism risk relative to employment at established firms. Our findings underscore startups' potential to mitigate labor market inequality while calling for policies that enhance the quality and stability of these opportunities.

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1 Introduction

The United States has the world's largest population with criminal records, where nearly one-third of working-age American adults have a record, on par with the fraction who have four-year college degrees (Friedman, 2015). Despite the extensive reach of the criminal justice system, people with criminal histories continue to face significant barriers to employment (e.g., Pager, 2003; Holzer, Raphael, and Stoll, 2003, 2006; Mueller-Smith and T. Schnepel, 2021). Unemployment and under-employment remains common (e.g., Western, 2002; Decker et al., 2015; Mueller-Smith, 2015), and failure to reintegrate individuals with criminal records into the labor market often leads to recidivism and exacerbates socioeconomic and racial inequalities (Uggen, 1999; Western, 2002; Yang, 2017).

Much of the policy and research effort aimed at improving employment outcomes for individuals with criminal records has focused on broad interventions designed to change employer behavior across the board. These include policies restricting the use of criminal records in hiring, such as Ban-the-Box or expungement (e.g., Agan and Starr, 2018; Doleac and Hansen, 2020; Rose, 2021), as well as measures to reduce employer liability or perceived risks, such as negligent hiring protections and wage subsidies (e.g., Pyle, 2023; Cullen, Dobbie, and Hoffman, 2023). While these approaches aim to make employers more willing to hire individuals with criminal histories, evidence on their effectiveness is mixed, with studies documenting unintended consequences alongside modest gains (Agan and Starr, 2017; Doleac and Hansen, 2020). Critically, this body of work largely treats employers as a homogeneous group, assuming a uniform reluctance to hire rather than examining whether some employers may already be more receptive to hiring individuals with criminal records.[1]

In this paper, we examine whether startups, or young firms, provide disproportionate employment opportunities for workers with recent criminal convictions, such that startups may serve as "engines of inclusion." On the one hand, startups may be more inclusive to individuals with criminal convictions due to their limited resources, minimal bureaucratic constraints, and emphasis on candidates' potential contributions rather than

[1] Work such as Holzer, Raphael, and Stoll (2002, 2007) provide survey evidence of variation in employers' willingness to hire and history of hiring individuals with criminal records by employer characteristics such as industry, employer size, type of job, and job qualifications.

traditional credentials (Moser, Tumasjan, and Welpe, 2017; Hurst, Lee, and Frake, 2024). On the other hand, they may also be more exclusive given their heightened sensitivity to hiring risks and reliance on informal hiring networks that tend to favor individuals who resemble founders in terms of background and status (Rocha and Brymer, 2025). Furthermore, existing policies that encourage hiring of workers with criminal records, such as the Work Opportunity Tax Credit, are disproportionately less often utilized by smaller — and likely younger — firms (Hamersma, 2011).

We explore this tension using integrated administrative data within Federal Statistical Research Data Centers (FSRDCs), covering labor market and criminal justice system information of the population of individuals in 4 states (Arizona, Maryland, New Jersey, and Wisconsin) between 2010 and 2017. We combine rich data from the Criminal Justice Administrative Records System (CJARS) program, Longitudinal Employer-Household Dynamics (LEHD), Integrated Longitudinal Business Database (ILBD), and Longitudinal Business Database (LBD) and characterize the startup employment patterns of individuals with criminal convictions and evaluate the implications of their engagement in startup employment. Specifically, we investigate whether startups disproportionately hire individuals with convictions, the mechanisms driving this phenomenon, and the subsequent outcomes of startup employment for individuals with convictions.

First, we find that individuals recently convicted of crimes are disproportionately likely to become startup employees. This pattern persists even after accounting for individual characteristics, including demographics, home county, employment sector, and past earnings. These patterns do not simply reflect ex-ante differences between individuals who ever commit crimes and those who never do. When we add worker fixed effects, we see that the rate of working for a startup is 13% higher when a worker has a recent conviction, relative to the mean. In an event study framework, we find 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 suggest treatment effects of gaining a criminal record, as opposed to preexisting differences in ability or education that might otherwise lead individuals to sort into startups (Ouimet and Zarutskie, 2014). We find that these patterns appear across demographic groups, sectors, firm sizes, and types of convictions.

Second, we find that startup employment following criminal convictions is particularly common when barriers to mainstream employment are higher. Namely, startup employment occurs more often when individuals are convicted of more heavily stigmatized offenses, when they reside in slack labor markets with fewer job opportunities, and when they live in regions that are less inclusive toward individuals with criminal records. This suggests that disproportionate startup employment is driven by restricted access to established employers (i.e., a labor demand story), rather than by a deliberate preference among workers with recent convictions to work for startups (i.e., a labor supply story) — reflecting the role of startups as more accessible employers in exclusionary labor markets. Specifically, we find the strongest sorting to startups among workers with felony and property crime convictions — crimes that employers may particularly avoid (Cullen, Dobbie, and Hoffman, 2023) — and very limited sorting for individuals with less-stigmatized crimes like DUIs and disorderly conducts. Furthermore, we find that individuals with recent convictions show disproportionate startup employment in regions where workers with criminal convictions earn relatively less and in regions with fewer new job postings. We also find that Black men with recent convictions show disproportionate startup employment in regions without Ban-the-Box regulations, suggesting that startup employment serves as a particularly important avenue of work for this group when employers are permitted to inquire about criminal histories during hiring.

These patterns are consistent with the idea that startups offer relatively less restricted access to employment because they face greater hiring challenges stemming from limited resources, weaker legitimacy, and lower visibility (Moser, Tumasjan, and Welpe, 2017; Chung and Parker, 2023). In response, startups may adopt hiring practices that emphasize candidates' potential contributions to survival and growth, rather than relying on conventional screening mechanisms focused on traditional credentials or status characteristics (Hurst, Lee, and Frake, 2024; Shane and Venkataraman, 2000; Pongeluppe, 2024). Even if young firms may not choose this differential emphasis, they may lack the financial resources and know-how needed to screen through conventional mechanisms like criminal background checks. To support this idea, we leverage full text data of job postings between 2010 and 2024, sourced from a large job posting aggregation platform (Lightcast). We find that young firms are significantly less likely than established, older employers to explicitly mention conducting background checks in their job ads. Whether this gap displays a lack of sophistication by young firms or an intentional hiring strategy, it bolsters our argument that at least part of the sorting of workers with convictions to startups is born out of firm-side employment decisions.

Third, we examine the broader implications of startup employment for individuals with criminal records. To do this, we consider cross-sectional differences in future outcomes for workers with and without recent convictions, based on whether they work at young versus older firms. This exercise is non-causal and captures a combination of selection and potential treatment effects, as the workers with convictions who end up at young firms may be selected but also impacted by their experiences at a startup (e.g., Sørensen and Phillips, 2011; Kacperczyk and Marx, 2016). We find that working at a startup, even an unsuccessful one, predicts better outcomes in future employment and recidivism, compared to unemployment, suggesting that startup employment acts as a positive inclusive force for individuals with convictions. Yet, importantly, we find that, among workers with convictions, those who work at young firms are more likely to recidivate and become non-employed in the near future, with these patterns often stronger if the young firm is an unsuccessful firm that exits quickly. These patterns are consistent with assortative matching: the individuals with convictions with the worst prospects struggle to find any job; conditional on finding a job, those with the worst prospects struggle to find a job at an older, more stable firm, and so end up at a startup. We posit that the patterns also reflect a lack of a large, positive treatment effect; i.e., while we may expect negative selection of the workers who end up at young firms, relative to older firms, there could exist a countervailing positive treatment effect, if these startup jobs provided a good opportunity for workers with convictions to gain skills and experience. Any countervailing positive treatment effect does not appear large enough to offset the selection; this may not be surprising, as previous literature has argued that startups are poor employers (Sorenson et al., 2021).

Finally, we consider the aggregate implications of our patterns: if startups provide employment opportunities for individuals with criminal records, then increases in entrepreneurship may lead to increases in employment rates for this population. We consider this with correlations at the county-level, where we show that counties have higher employment rates for individuals with convictions when they have higher entrepreneurship rates; these patterns persist when controlling for county and year fixed effects, as well as economic conditions, including the county conviction rate and county employment rate for individuals without convictions. This positive correlation is particularly true in counties that have not implemented Ban-the-Box policies — i.e., in counties where older firms can more easily screen out workers with convictions — suggesting that startups contribute to boosting inclusive employment for individuals with convictions in regions where they face greater discrimination or biases.

This paper contributes to research on criminal justice involvement and labor market inequality, demonstrating startup employment as a potential route for inclusion and integration for individuals with criminal records.[2] Existing studies on the barriers to employment faced by individuals with criminal records have typically treated employers as homogeneous, rarely exploring *which* employers are more receptive to hiring workers with criminal records. Our findings address this gap by highlighting startups or young firms as potential avenues of inclusion for this population, challenging the prevailing narrative of uniform exclusion from employers.

We also contribute to the entrepreneurship and startup literature by highlighting startups' overlooked role as engines of inclusion. Building on prior work emphasizing startups' capacity to foster general job creation and innovation (e.g., Haltiwanger, Jarmin, and Miranda, 2013; Glaeser, Kerr, and Kerr, 2015), we address the largely under-examined question of *whom* they hire and the inclusivity of these opportunities. Our findings further speak to the intersection of entrepreneurship and inequality scholarship, by emphasizing how entrepreneurship provides pathways for inclusion, not only among founders from marginalized backgrounds (e.g., Aldrich and Waldinger, 1990; Yang, Kacperczyk, and Naldi, 2024; Hwang and Phillips, 2024), but among a broader set of marginalized individuals through employment. Furthermore, by examining longer-term employment outcomes for startup hires, our study engages with research documenting career penalties associated with startup employment (Sorenson et al., 2021), revealing that startup employment leads to negative career and recidivism outcomes for individuals with criminal records. Policymakers aiming to leverage startups for both economic and social benefits should carefully consider these dual dy-

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

namics, designing policies that enhance both the inclusivity and sustainability of startup-driven employment.

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 present 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 startup 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. Below, we link the data from January 2010 through December 2017 to our main sample at the county level; additionally, in a supplementary analysis, we also examine job postings more broadly from January 2010 to December 2024, to compare hiring practices between startups and established firms.

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.

[3]These four states comprise the intersection of states for which we have both criminal justice conviction, work, and entrepreneurship data for 2010-2017.

2.1.1 Measuring criminal justice histories

We measure individuals' criminal justice outcomes, namely conviction histories, through the Criminal Justice Administrative Records System (CJARS) program. The CJARS program collects comprehensive data on the U.S. criminal justice system, with data linkages that allow us to identify and track individuals with criminal records before and after their criminal justice experiences in other Census data. Throughout our paper, we consider two measures of criminal justice experience: whether an individual has been convicted of any crime in the past seven years, and the relative timing of their conviction (i.e., in an event study framework). For the former measure, we focus on the past seven years to mirror the reality of criminal records, as felony convictions remain on an individual's criminal record (collected by employers, etc., through consumer reporting agencies) for seven years.[4] We refer to convictions in the past seven years as "recent" convictions. We also use the CJARS data to study recidivism, by measuring future convictions.

2.1.2 Measuring employment

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

Second, to build out our set of "potential" workers, we measure self-employment using the Integrated Longitudinal Business Database (ILBD). The ILBD contains information on all businesses without employees (e.g., sole proprietorships) in the United States, mostly sourced from Schedule C filings. Crucially for us, starting in 2007 in the ILBD identifies the owner of each firm.[5] 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.

[4]Misdemeanors remain on criminal records for five years.

[5]Prior to 2007, the ILBD only identifies the person filing the tax forms; for married individuals filing jointly, this filing person could either be the business owner or their spouse.

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

2.1.3 Measuring demographics and home county

We source individuals demographics and residence locations from two datasets that are part of the LEHD database. We observe individuals' dates of birth, from which we calculate age, sex, race, and ethnicity. Note that the Census imputes demographics for some individuals; we restrict our entire analysis to individuals with non-imputed birthdates, sex, race, *and* ethnicity.[7]

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.[8]

2.1.4 Measuring barriers to employment

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

First, we consider whether a county enacted a Ban-the-Box policy or has any city with a Ban-the-Box policy in place. A Ban-the-Box policy prohibits employers from conducting criminal background checks or inquiring about criminal histories until later in the hiring process — typically at the time of the conditional

[6] For 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 preformed by Census.

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

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

job offer. In the 2010 to 2017 window for our sample of states, 15 cities and counties have Ban-the-Box policies. We construct a county-year indicator that takes a value of 1 if Ban-the-Box policy was enacted that year or in any of the prior years, and takes a value of 0 otherwise.[9] Specifically, we focus on Ban-the-Box policies that ban public employers from screening criminal records. While Ban-the-Box policies for both private and public employers have a wider reach in the labor market and a larger impact on individuals with criminal records (Rose, 2021), narrowing the focus to private ban-the-box policies within our time frame significantly reduces the size of the treatment group (4 counties or cities within our sample states - Baltimore, MD, Montgomery County, MD, Prince George's County, MD, Newark, NJ). In addition, prior work shows that private employers are likely to follow public employers' Ban-the-Box policies, validating the significance of examining the limited public Ban-the-Box policy (Jacobs, 2005). Given that the public Ban-the-Box policies are weaker policy interventions than private Ban-the-Box policies, we expect to find stronger results when examining the effects of the adoption of Ban-the-Box policies for private employers.

Second, we consider the general "friendliness" of the local labor market to workers with convictions by measuring the pay gap between workers with and without recent convictions, where the pay gap for county c in year t is calculated as

$$\begin{aligned} \text{Conviction pay gap}_{ct} &= \text{Mean Log(Earnings} \mid \text{no recent conviction)}_{ct} \\ &\quad - \text{Mean Log(Earnings} \mid \text{recent conviction)}_{ct}. \end{aligned} \tag{1}$$

We calculate the pay gaps using all workers in a county-year; i.e., we consider the "working" sample described below, but take the universe of workers instead of randomly sampling.

Third, we consider a county's labor market tightness using job postings from the Lightcast data. For specifications with the "New Job Ratio," we focus on job postings between years 2010 to 2017 in our analyses. Specifically, we measure the tightness of a county's labor market by dividing the number of job postings in a county-year by the total employment in that county year (calculated from the sample used for

[9] 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.

the pay gap variable calculation).

2.2 Samples

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

2.2.1 Full sample

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

First, we take earnings histories for individuals in our states (Arizona, Maryland, New Jersey, and Wisconsin) in the LEHD between 2010 and 2017.[10] For each individual, we take their highest-paying job within the year (and state) and flag this as their main employment.[11] Second, we merge in records from the ILBD in order to flag individuals as self-employed individuals.[12]

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 choose to interpret these missing years as "non-employment" and interpolate these years into our sample (i.e., "fill-in" the missings).[13] To this filled-in sample we merge in demographic and residence information described above.

Finally, we apply three sample restrictions. We restrict to individuals between the ages of 18 and 65; this means that, even though we filled in missing observations, we do not consider the employment outcomes of, e.g., a 12-year-old who works in the later part of our sample as an adult. We additionally restrict to in-

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

[11] Note that we rank individuals at firms before restricting to their top-paying jobs.

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

[13] While adding these observations is not innocuous, we believe it is important to acknowledge the fact that many people with convictions are not consistently working or self-employed; thus it is important to include these observations when making statements about the relative propensity to work and the average outcomes for individuals living in an area. While missing observations could instead reflect individuals having moved (temporarily or permanently) outside of our sample of states, working for a firm not covered by the LEHD (e.g., the federal government, some agricultural firms), or being retired or deceased., we believe some of these 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.

dividuals 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.[14] 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.[15] To adjust for the imbalance (10% vs. 100% samples), we use weights in all analyses below; i.e., individuals with conviction histories receive weights of 1, while our randomly drawn individuals without conviction histories receive weights of 10.

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

2.2.2 Working 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 working sample consists of 11,260,000 individual-year observations, of which 2,575,000 have recent convictions within the past 7 years.

2.3 Summary statistics

Table 1 presents descriptive statistics for our two main samples, with our full sample in Panel A and our working 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 working 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

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

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

individuals without convictions. This differences persists in the working 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.

Figure 1 plots the sectoral distribution for our working 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 wage 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.[16] This pattern is most salient when we consider the very young firms under the age of 5: 12% of workers with convictions work at these new firms, while only 7% of workers without convictions do. By inverting these distributions, these patterns also imply that younger firms employ relatively more individuals with convictions: 5% of workers at firms under the age of 5 have a recent conviction, while only 2% of those at mature firms over the age of 30 do.

These patterns could arise for a variety of reasons. For instance, Figure 1 shows that workers with convictions more often work in sectors like construction and food services, which are more entrepreneurial and younger (Wallskog, 2025); this age pattern could be a reflection of sectoral sorting. Similarly, Table 1

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

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:

$$\text{Work at a young firm}_{icnt} = \alpha + \beta \text{Recent conviction}_{it} + \mathbf{X}_{it}\delta + \gamma_{ct} + \gamma_n + \gamma_i + \varepsilon_{icnt}, \tag{2}$$

where we regress an indicator for whether a worker works at a young firm (under the age of 5) on an indicator for whether they have a recent conviction in the last 7 years, with various controls.

Specifically, we say that a worker i lives in county c and workers in sector n in year t. $\mathbf{X}_{it}$ 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 (γ_{ct}), sector (γ_n), and individual (γ_i) fixed effects. Below, we walk through the implications of these controls as we build up to the full model. ε_{icnt} reflects idiosyncratic noise.

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

The specifications through column (5) are predominantly cross-sectional, comparing individuals with and without convictions. Despite our controls, there may be a myriad of unobserved characteristics that still cause omitted variable bias. For example, perhaps some individuals are more risk-tolerant and consequently are both more likely to commit crime and work for a new firm. In this case, it is not the conviction that leads to working at a young firm. We tackle this issue in two ways. First, in column (6), we add individual fixed effects in order to remove average differences in proclivities to work at young firms.[17] Because there are individual-level characteristics we do not observe, the inclusion of individual fixed effects shrinks the coefficient on having a recent conviction, but the coefficient remains strongly statistically different from zero. Namely, within an individual, we estimate that having a recent conviction predicts a 0.9 percentage point higher likelihood of working at a young firm, a 13% increase relative to the mean.[18,19]

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:

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

[18] In Table A.1, we consider alternative measures of working at a young firm; workers with convictions more often work at brand new firms (column (1)) and generally should be thought of as *employees* at young firms rather than owners or managers (i.e., are not the top earner of their firm).

[19] Table A.2 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.

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

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

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

Figure 3 presents the results: following a conviction, individuals see a 1 percentage point increase in the rate of working at young firms, relative to two years before their conviction, a 21% increase relative to the mean. This increase persists across the following three years, with a slight decay over time (which could be due to their young firm aging out of being "young"). We see limited evidence of any differential pre-trend in working at young firms leading up to an individual's conviction, with no coefficients statistically distinguishable from the omitted year (two years prior to the conviction). As mentioned above, we expect to see some potential increase in the year prior to the conviction, as an individual's crime and arrest may occur in that year; to the degree that these may already appear on an individual's criminal record the year before their conviction, individual's 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 0.097) that is not statistically different from zero but suggestive of a partial change in that year.

3.3 Heterogeneity

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

First, we consider variation by sex and race/ethnicity. In Table 3, we augment the full model (2) to estimate sorting by demographic group. In column (1), we add interaction terms between having a recent convictions and indicators for sex, race, and ethnicity. In columns (2)-(7), we estimate model (2) 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 Blacks 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 startups 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 startups may play a unique role in absorbing the individuals who are most stigmatized and screened out by established employers. We directly examine this mechanism in Section 4 below.

Second, we consider variation by firm size. This is important, as our argument is that individuals with convictions disproportionately work at *young* firms. It could be that the pattern is actually about working at *small* firms (which young firms often are), which would have different implications. We address this in two ways. We begin by estimating model (2) 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 (2) for different firm size groups; we report results in Table A.3. For each firm size bin, we provide estimates for regressions with and without individual fixed effects. While the version with individual fixed effects remain 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). Across all four firm size bins we consider (under 20, 20-49, 50-99, and over 100 employees), we find that workers with convictions more often work at young firms than workers without convictions (odd columns). For instance, conditional on working at a firm with fewer than 20 employees, workers with convictions are 3.6 percentage points more often working at young firms; among workers at firms with at least 100 employees, this gap is 2.1 percentage points. With the exception of the smallest bin, these patterns persist with the inclusion of individual fixed effects (even columns). We conclude that the fact that workers with convictions disproportionately sort to young firms is not wholly reflecting them sorting to small firms.

Third, we consider variation by firm sector. Recall that our main specification (column (6) of Table 2) controls for sector fixed effects, such that our findings are not driven by individuals with convictions sorting to younger sectors. As with firm size, we additionally explore variation by estimating model (2) 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). In our cross-sectional regressions (even 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 (odd 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.

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

4 Mechanisms

Why do workers with convictions sort to startups? On the one hand, sorting could be explained through supply-side mechanisms; individuals with convictions may be better matched to startup environments because of greater risk-tolerance and entrepreneurial orientation, and prefer jobs that offer more autonomy and less rigidity (Fairlie, 2002; Sauermann, 2018). On the other hand, sorting could be driven by demand-side mechanisms, with startups maintaining lower barriers to entry and displaying greater willingness to hire individuals with convictions. This could reflect differences in startups' beliefs and values, or variation in hiring practices across firm age. For example, younger firms may be less likely to conduct background checks than more established firms, thereby inadvertently screening out fewer applicants with records. Similarly, startups may prioritize demonstrated skills over formal credentials or resume signals, which can expand access to employment for individuals with criminal histories.

In this section, we present evidence that workers with convictions end up at young firms because they face relatively higher barriers to employment at older firms. We do this in three steps. First, we show that the sorting is stronger for individuals who likely face particularly strong barriers at older firms, namely those with convictions for felony and property crimes (Cullen, Dobbie, and Hoffman (2023)). Second, we show that our results dampen in local labor markets where barriers toward individuals with convictions are lower (i.e., employers are less able or willing to screen out workers with convictions). Third, using a separate job posting dataset, we show that younger firms are less likely than established firms to conduct criminal background checks, suggesting that differential hiring practices likely contribute to the sorting pattens.

4.1 Sorting by crime

If younger firms are willing to hire workers with convictions that older firms screen out, then the sorting patterns we see should be stronger for types of crimes that are particularly "undesirable" for firms. For example, Cullen, Dobbie, and Hoffman (2023) find that firms are generally unwilling to hire workers with felony convictions, particularly violent and property ones, likely because they fear violence or theft in their firms. Meanwhile, firms may be more tolerant of DUI convictions, which society may view with lower stigma.

We test this by estimating a modified version of model (2) 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 4 presents our findings. On their own in columns (1)-(7), all crimes we consider predict higher rates of working at young firms.[21] When horse-raced against each other in column (8), we find the strongest patterns for the crimes older firms may more often screen against. Namely, 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.

4.2 Local labor market barriers to employment

We next turn more broadly to older firms' incentives and ability to screen out workers with convictions, compared to younger firms. We consider three expansions of model (2), tabulated in Table 5.

First, we consider the role of Ban-the-Box policies. When a location has a Ban-the-Box policy in place, employers are marginally constrained in their ability to screen out potential hires with convictions.[22] We hypothesize that this means that we should see *less* sorting of workers with convictions to young firms under

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

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

Ban-the-Box, as in these settings older firms may hire more of them. We test this in columns (1)-(3) of Table 5. In column (1), we estimate whether the role of recent convictions in predicting working at a young firm varies by whether an worker's home county currently has a Ban-the-Box policy in place; recall that we have county-year fixed effects in our specifications, so we are estimating the relative impact of Ban-the-Box for workers with convictions. We find no difference; i.e., in general, workers with convictions are on average equally more likely to sort to young firms, regardless of Ban-the-Box. However, this does not hold true when we narrow in the population found to be most affected by these policies: Black 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 barriers to employment, such as employers' increased ability to screen out potential hires with criminal backgrounds, contribute to the sorting patterns we observe.

Second, we consider the general "friendliness" of the local labor market to workers with convictions. If firms generally discriminate more against workers with convictions, then our sorting patterns may be stronger, if younger firms are marginally more tolerant. To study this, we consider the local "wage penalty" to having a conviction, namely the log earnings gap between workers with and without recent convictions; we construct this variable as the mean log earnings of workers without convictions in a county-year minus the mean log earnings of workers with convictions in the county-year, such that a larger positive pay gap between the two groups means that workers without convictions are paid relatively more. This "penalty" may reflect several factors, including overt discrimination (i.e., firms pay workers with convictions less for the same positions), sorting (i.e., workers with convictions sort to worse paying firms), or composition (i.e., workers with convictions have lower human capital). There is substantial variation in this pay gap across counties and time: on average, workers without convictions earn 29% more than workers with recent convictions, while the standard deviation of this gap is 34 percentage points.

Column (4) of Table 5 presents our results: when the penalty to having a recent conviction is higher, workers with convictions are even more likely to work at young firms. In numbers, a one standard deviation (34 percentage point) higher pay gap increases the rate of workers with convictions sorting to young firms by 0.4 percentage points, a nearly 50% increase from the baseline sorting coefficient of 0.009. While this exercise is imperfect, since the pay gaps may reflect factors unrelated to older firms' unfriendliness to workers with convictions, it does provide additional supportive evidence that our findings may reflect young firms being more willing to hire these workers.

Third, we consider the role of labor market tightness. We posit that older firms are more willing and able to screen out workers with recent convictions when there is relative slackness in the labor market, i.e., when they can easily find alternative hires. If this is true, then the sorting of workers with convictions to younger firms should be weaker in tighter labor markets. Column (5) of Table 5 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); on average, the number of job postings are 11% of the number of current jobs. We find results consistent with our hypothesis: when the county new job ratio is one standard deviation (6 percentage points) higher, workers with convictions are 0.1 percentage points marginally less likely to sort to young firms a reduction of 14% reduction from the baseline gap of 0.9 percentage points. In other words, when firms are trying to hire more in a county, more workers with convictions find jobs at older firms.

4.3 Evidence from job postings data

To further examine whether startups set lower barriers to employment for individuals with convictions, compared to older firms, we leverage job posting data to show direct evidence that younger firms are less likely to screen out these individuals.

We analyze job posting data from Lightcast between January 2010 and December 2024, containing the full text of over 350 million online job postings. Within these postings, we identify explicit mentions of criminal background checks and use them as a proxy for whether firms screen out individuals with records.

Specifically, our goal is to test whether younger firms report conducting such background checks less frequently, either because they are less formalized in their hiring practices and/or because they intentionally screen less on criminal records. To do this, we estimate job posting-level models of the following form:

$$\text{Job posting mentions a background check}_{pjcnt} = \alpha + \beta \text{Young firm}_{jt} + \mathbf{X}_p \delta + \mathbf{Z}_{jt} \nu + \gamma_c + \gamma_t + \gamma_n + \gamma_j + \varepsilon_{pjcnt}, \tag{4}$$

where p is a job posting for a job at firm j in county c, first posted in year-month t; n is the 2-digit NAICS industry in which the firm operates; the γ's represent county, year-month, industry, occupation, and firm size fixed effects. Our focus is to estimate β, the coefficient capturing any gaps in background checks between positions at young (i.e., age under 5) versus old firms. Unlike in our administrative data, we do not cleanly observe when a firm enters (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. 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." In our analyses below, 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 and 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).

For computational tractability while maintaining statistical power, we analyze all job postings from January 2015 to December 2024 for a random 30% of firms in the dataset. The resulting data covers more than 91 million online job postings, spanning more than 867,000 employers in 3,140 counties. 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 6 presents our results. Columns (1)-(5) estimate the gap in background checks between young and old firms, while column (6) estimates the gaps between age 1 through 5 firms separately, compared to old (age 6 and over) firms. Throughout the specifications, we find that job postings at younger firms mention background checks less frequently than those at older firms. For example, in column (5) we estimate that job postings at startups mention background checks 0.4 percentage points less often, compared to firms over age 5, controlling for county, time, industry, occupation, and firm size fixed effects; this 0.4 percentage points translates into a 5.5% lower rate of mentions of criminal background checks for jobs at startups, 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.[23]

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

We take these results as indicating that young firms screen out workers with criminal backgrounds less often than older firms, which may allow for our main findings that workers with criminal records disproportionately find jobs at young firms. Young firms could be doing this lower screening for various reasons, which we cannot cleanly disentangle here. They could be not screening simply because they are not aware of or are unable to pay for such hiring practices. Alternatively, they may be willing to more informally screen potential hires through interviews, for instance. They may also simply feel that they cannot afford to screen out these workers, in that they may be sufficiently desperate to find any workers.

[23] Mentions of criminal background checks may be driven by firm entry year cohort effects, especially given our data time frame includes firms that entered during and after the COVID 19 pandemic. In order to rule out that these patterns do not simply reflect firm cohort patterns, we show in Table A.5 that our results are consistent when we split our sample into two cohorts of 2015-2019 and 2020-2024.

5 Discussion: Implications

So far, we have argued that workers with recent convictions disproportionately find jobs at younger firms, as these younger firms are more willing to hire them. What are the implications of this sorting? In this section, we consider two sets of implications. First, we consider the outcomes for the workers individually *after* they are hired by young firms. Second, we consider how this sorting informs county-level patterns.

5.1 Outcomes for workers with convictions

When workers with convictions sort to young firms, is this a "good" outcome? On the one hand, the literature has argued that young firms can be poor 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. Having *any* job may be particularly important for individuals with criminal records: 11% of non-employed individuals with recent convictions recidivate within a year (Table 7).

While it is challenging to assess the causal impact of being hired at a young firm, we can characterize what happens to workers following their employment at these firms. This characterization will pool both selection (i.e., who works at young firms) and treatment (i.e., how a young firm impacts a worker's trajectory) effects. To make this characterization, we estimate regressions of the following form:

$$\begin{aligned}\text{Future outcome}_{icnt} = \alpha &+ \beta_1 \text{Recent conviction}_{it} + \beta_2 \text{Work at a young firm}_{it} \\ &+ \beta_3 \text{Recent conviction}_{it} \times \text{Work at a young firm}_{it} + \mathbf{X}_{it}\delta + \gamma_{ct} + \gamma_n + \gamma_i + \varepsilon_{icnt},\end{aligned} \tag{5}$$

where we consider future outcomes for individual i as in model (2), as a function of whether they have a recent conviction and whether they currently work at a young firm. This specification allows us to characterize how workers in general at young firms differ from those at older firms, as well as those who additionally have recent convictions. We consider several future outcomes, including recidivism and future employment and earnings.

We additionally consider an augmented version of model (5) in which we distinguish between working for a "successful" young firm, as opposed to an "unsuccessful" one, where we define success as surviving to age 5. This distinction allows us to consider the role that young firm quality plays, assuming that higher quality young firms survive longer. That majority of workers working at young firms are at the "successful" ones, partially reflecting that these firms operate for more years. Workers with convictions are somewhat more likely to work at unsuccessful young firms; conditional on working at a young firm, workers with recent convictions work at unsuccessful ones 18% of the time, while workers without recent convictions only 15% of the time. This is consistent with workers with recent convictions sorting particularly to lower-quality young firms.

Table 7 presents our results. We begin in columns (1) and (2) by considering short-term recidivism, i.e., whether a worker has a conviction the following year. For workers with recent convictions, this is relatively common, with 9.4% of workers with recent convictions having a conviction the next year; for workers without recent convictions, this rate is only 0.5%. In column (1), we see three patterns: 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 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 older firm, one with a recent conviction working at a younger firm is on average 12% more often recidivating within a year.[24] This is economically meaningful, as it means that workers with convictions who work at young firms may have nearly as high recidivism rates as those who are non-employed. Taking the average recidivism rate of workers with convictions (9.4%) as the baseline, working at a young firm as a worker with a conviction predicts a 10.6% (9.4 + 0.1 + 1.1) recidivism rate. Meanwhile, individuals with recent convictions who are *not* currently employed have an 11% recidivism rate. Consequently, the workers with convictions who find jobs at young firms are likely the most marginally "employable" ones; or, perhaps having a job at a young firms is not that much "better" than having no job at all, at least for recidivism.

[24]Note that, because the recent conviction may have happened anytime in the past 7 years, the next-year recidivism we measure could reflect recidivating after as little as 2 years or as many as 9. In untabulated results, we estimate columns (1) and (2) 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.

Put more formally, there are two potential (non-mutually exclusive) interpretations of the recidivism patterns. First, perhaps young firms generally hire "worse" workers, 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. Column (2) explores the role of these firm exits by splitting on whether the young firm is "successful" (i.e., survives to age 5). We find larger "effects" for the unsuccessful firms: for workers without recent convictions working at young firms, future conviction rates are doubly higher if they are at unsuccessful young firm rather than successful firms; for workers with recent convictions at young firms, rates are 50% higher at the unsuccessful firms. Again, 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).

In columns (2)-(6) we consider economic outcomes two years into the future, namely whether or not workers are still working at any job and whether they have changed jobs.[25] Across the board, workers with recent convictions tend to have worse future economic outcomes, being more likely to be non-employed, change jobs, and have lower earnings; for example, workers with recent convictions are 5.5 percentage points more often not working two years in the future, a large gap given that only 13% of workers without recent convictions are non-employed at that point in time. Similarly, working at a young firm on its own also tends to predict worse future outcomes. As in the case of future convictions, these patterns may reflect a combination of negative selection and treatment effects.[26]

Interestingly, we have slightly less consistent results in terms of the interactions: for future non-employment and earnings, working at a young firm exacerbates the gap between workers with and without recent con-

[25] Table A.6 presents analyses of future earnings (unconditional on working).

[26] Our findings speak to prior work that has found that engaging in startups as a "founder" can reduce recidivism and increase future economic outcomes relative to working in established firms (e.g., Hwang, 2021; Hwang and Phillips, 2024). We reconcile these perspectives by distinguishing between founding and joining startups. Startup founders and employees — or "joiners" — represent distinct types of actors with different motivation and trajectories (Roach and Sauermann, 2015; Sauermann, 2018). Founders are typically individuals with orientations towards autonomy and initiative, traits that can convert exclusion into opportunity. Startup joiners, in contrast, are more likely to end up in startups due to limited access to established firms. Thus, while startup founding can serve as a proactive pathway out of labor market exclusion, startup employment functions more as a fallback option.

victions. I.e., workers with convictions are particularly likely to be non-employed and have lower earnings in the future if they work at a young firm (columns (3) and (7)). Counter to the recidivism patterns, these patterns are actually driven by working at successful young firms (columns (4) and (8)). Put differently, the negative economic consequences (or selection) of working at a young firm that exits quickly is the same regardless of whether a worker has a recent conviction; this may be accounted for by the fact that workers who are displaced (e.g., laid off) generally have worse outcomes (Jacobson, LaLonde, and Sullivan (1993)). Meanwhile, workers with convictions are actually less likely to change jobs if they work at young firms (column (5)), with this appearing both for successful and unsuccessful young firms (column (6)). This may reflect workers with convictions being less likely to quit their jobs at young firms, or having fewer outside opportunities arise.

5.2 Aggregate implications

While outcomes from working at young firms for workers with convictions are not overwhelmingly good, there likely is still a societal benefit to having workers with recent convictions employed. In our full sample, individuals with recent convictions who are not employed have short-term recidivism rates nearly 20% higher than those who work.

In light of this, we wrap up our analysis by considering patterns at the county-level. If young firms are important employers for workers with convictions, then more entrepreneurial counties may have higher employment rates for the recently-convicted population. To examine this, we use county-level data from 2007 to 2018 and support this idea with regressions:

$$\begin{aligned}\text{County employment rate of individuals with convictions}_{ct} = \alpha + \beta \text{County entrepreneurship rate}_{ct} \\ + \mathbf{X}_{ct}\delta + \gamma_c + \gamma_t + \varepsilon_{ct},\end{aligned} \tag{6}$$

where we correlate a county's employment rate of residents with recent convictions (referred to as "convicted employment rates" from here on) with the county's entrepreneurship rate (measured as the share of firms that are new), where c is a county and t is a year. We control for county (γ_c) and year (γ_t) fixed effects, in order to consider variation in rates within a county over time and net out average time trends, respectively. We additionally control for county-year characteristics ($\mathbf{X}_{ct}$) described below. ε_{ct} reflects idiosyncratic noise.

Table 8 presents our results. In column (1), we consider the correlation between entrepreneurship and convicted employment rates, adjusting only for county and year fixed effects. We observe a strong positive correlation, with a statistically significant coefficient on the entrepreneurship rate of 0.74, indicating that a one standard deviation (1.2 percentage point) higher entrepreneurship rate is associated with a 0.9 higher percentage point convicted employment rate, a 1.4% increase relative to the mean. While this value may seem modest, it actually reflects a non-insubstantial reduction of the gap between the convicted employment rate and the non-convicted employment rate. Namely, on average across counties, 78% of individuals without recent convictions (who are employed or self-employed *sometime* in the 2007-2018 window) are working, while only 63% of those with recent convictions are employed. This means that a one standard deviation higher entrepreneurship rate "closes" the convicted versus non-convicted employment gap by 6%.

There are many reasons for which this correlation should not be interpreted causally. Most importantly, entrepreneurial counties may be experiencing economic booms, which may affect both the incentive to commit crime and the ability to find employment. In columns (2) and (3) of Table 8, we take a step towards accounting for this by controlling for county conviction rates and non-convicted employment rates; we do this by discretizing both variables into ventiles and then controlling for fixed effects for these bins. Controlling for conviction rates reduces the concern that more entrepreneurial locations may have different crime activity, which could impact the employability of the convicted population. Controlling for the non-convicted employment rate helps control for the economic environment; with these controls, we ask how local entrepreneurship rates predict convicted entrepreneurship rates, netting out the effect of general economic booms. The patterns persist in both columns. In column (3) with both sets of fixed effects, we estimate a coefficient on the county entrepreneurship rate of 0.88, which translates into a 1.7 percentage point higher convicted employment rate when the entrepreneurship rate is one standard deviation higher. While adding these controls are far from generating a conclusively causal estimate, they support the idea that entrepreneurship may specifically predict higher employment rates for the recently convicted population.

In column (4), we consider one additional test. Recall from Section 4 that our sorting patterns are weaker when Ban-the-Box policies are in place and reduce the ability of older firms to screen out potential hires with recent convictions. In column (4), we test whether the positive correlation between a county's entrepreneurship rate and convicted employment rate is different under Ban-the-Box. We find striking results: the correlation effectively disappears under Ban-the-Box, with an implied coefficient on the entrepreneurship rate for Ban-the-Box counties of only 0.01 (which would translate into a 0.01 percentage point higher convicted employment rate when the entrepreneurship rate is one standard deviation higher). In other words, higher entrepreneurship rates generally predict higher employment rates for individuals with convictions — but not in settings where young firms are less important employers for that population.

Taken together, these patterns suggest a potential side effect of promoting entrepreneurship in settings where individuals with convictions face barriers finding jobs: higher entrepreneurship may disproportionately generate job opportunities for this marginalized population.

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.

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

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

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Table 1: Workers with recent convictions disproportionately work at younger firms

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***	0.047***	0.051***	0.034***	0.031***	0.009***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Male		-0.001***	-0.001***	-0.002***	0.000**	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Black		-0.004***	-0.006***	-0.005***	-0.007***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Non-Hispanic Other Race		0.021***	0.016***	0.012***	0.007***	
		(0.000)	(0.000)	(0.000)	(0.000)	
Hispanic		0.016***	0.006***	0.002***	0.001*	
		(0.000)	(0.000)	(0.000)	(0.000)	
Constant	0.067***	0.065***	0.067***	0.068***	0.068***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Age FE		x	x	x	x	x
County-year FE			x	x	x	x
Sector FE				x	x	x
Past earnings bin FE					x	
Individual FE						x

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

Note: This table demonstrates how convictions predict working at young firms (under the age of 5), conditional on working. The seven columns gradually build up model (2), slowly adding controls as demonstrated in the table and table footer.

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

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

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

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

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

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

Table 4: Workers with recent convictions disproportionately work at younger firms, by crime type

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

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

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

N = 11,260,000. Mean of dependent variable: 0.069. Conviction rates, conditional on any recent conviction: felony = 0.368; violent = 0.168; property = 0.265; DUI = 0.210; drug = 0.279; public order = 0.348; financial = 0.067. Note that these categories are not mutually exclusive, as individuals can be convicted of multiple crimes.

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

Table 5: Workers with recent convictions disproportionately work at younger firms when older firms can "afford" to not hire them

	Dependent Variable: Work at a firm young firm				
Sample:	All	Black men	White men	All	All
	(1)	(2)	(3)	(4)	(5)
Recent conviction	0.009***	0.011***	0.008***	0.007***	0.011***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Recent conviction × Ban-the-Box	0.000	-0.007***	-0.003		
	(0.001)	(0.002)	(0.002)		
Recent conviction × Pay gap				0.011***	
				(0.002)	
Recent conviction × New job ratio					-0.021**
					(0.007)
Constant	0.069***	0.068***	0.065***	0.069***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean(work at a young firm)	0.069	0.069	0.065	0.069	0.069
Mean(recent conviction)	0.038	0.109	0.050	0.038	0.038
N	11,260,000	1,022,000	4,195,000	11,260,000	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 BTB, pay gap, and new job ratio are collinear with cohort-year fixed effects, and so do not appear on their own in the regressions.

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Table 6: Job Postings by Firm Age: Younger Firms Mention Background Checks Less Frequently

	Dependent Variable: Job Posting Mentions a Background Check					
	(1)	(2)	(3)	(4)	(5)	(6)
Young firm (age $\leq$ 5)	-0.004***	-0.006***	-0.006***	-0.007***	-0.004***	
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	
Firm age = 1						-0.009***
						(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***	0.084***	0.084***	0.085***	0.083***	0.083***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
County FE		x	x	x	x	x
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. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Note: This table demonstrates how job postings vary by firm age. Firm age is the number of years since a firm's initial job posting (plus 1). Data are at the job-posting level; we weight all regressions by the inverse of the number of job postings a firm generates in a given year, such that the estimates may be interpreted at the firm-level. In model (6), the omitted age category is firms over age 5. Standard errors are clustered at the firm level.
$N = 91{,}236{,}025$. Mean of dependent variable: 0.073.

Table 7: Individuals with recent convictions who work at young firms tend to have poor outcomes

Dependent Variable:	Have a conviction next year		Not working in 2 years		Change jobs within 2 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Recent conviction	0.080***	0.080***	0.055***	0.055***	0.144***	0.144***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Work at young firm	0.001***		0.037***		0.104***	
	(0.000)		(0.000)		(0.001)	
Recent conviction	0.011***		0.010***		-0.018***	
× Work at young firm	(0.000)		(0.002)		(0.002)	
Work at unsuccessful young firm		0.002***		0.101***		0.327***
		(0.000)		(0.001)		(0.001)
Recent conviction		0.015***		-0.003		-0.090***
× Work at unsuccessful young firm		(0.001)		(0.004)		(0.005)
Work at successful young firm		0.001***		0.026***		0.064***
		(0.000)		(0.000)		(0.001)
Recent conviction		0.010***		0.010***		-0.011***
× Work at successful young firm		(0.000)		(0.002)		(0.002)
Constant	0.001***	0.001***	0.129***	0.129***	0.398***	0.397***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age FE	x	x	x	x	x	x
County-year FE	x	x	x	x	x	x
Sector FE	x	x	x	x	x	x
Past earnings bin FE	x	x	x	x	x	x
Mean(dep var)	0.008	0.008	0.134	0.134	0.419	0.419
Mean(dep var | no recent conviction)	0.005	0.005	0.131	0.131	0.410	0.410
Mean(dep var | recent conviction)	0.094	0.094	0.200	0.200	0.650	0.650
For non-workers (i.e., not in table):						
Mean(dep var | no recent conviction)	0.006	0.006	0.767	0.767		
Mean(dep var | recent conviction)	0.110	0.110	0.740	0.740		

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 recidivism, employment, and a change in main job.

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. Footer includes mean dependent variables for non-workers (who are not in the estimation sample for this table) for comparison. (Note that the indicator for changing jobs is not defined for non-workers.)

All columns in indicators for sex and race/ethnicity. Note that individual fixed effects are not included; instead, we include controls for past earnings. A "successful" young firm is one in which the firm survives to at least age 5. Throughout, the left-out category is firms age 6 and older. Table A.6 presents analyeses of future earnings..

Table 8: Employment rates for individuals with convictions are higher in counties with more entrepreneurship

	Dependent Variable: County employment rate of individuals with convictions			
	(1)	(2)	(3)	(4)
County entrepreneurship rate	0.740***	0.659**	0.875***	0.955***
	(0.206)	(0.239)	(0.212)	(0.219)
Ban-the-Box				0.051***
				(0.010)
County entrepreneurship rate × Ban-the-Box				-0.945***
				(0.223)
Constant	0.599***	0.602***	0.594***	0.590***
	(0.007)	(0.009)	(0.008)	(0.008)
County FE	x	x	x	x
Year FE	x	x	x	x
Conviction rate bin FE		x	x	x
Non-convicted emp rate bin FE			x	x

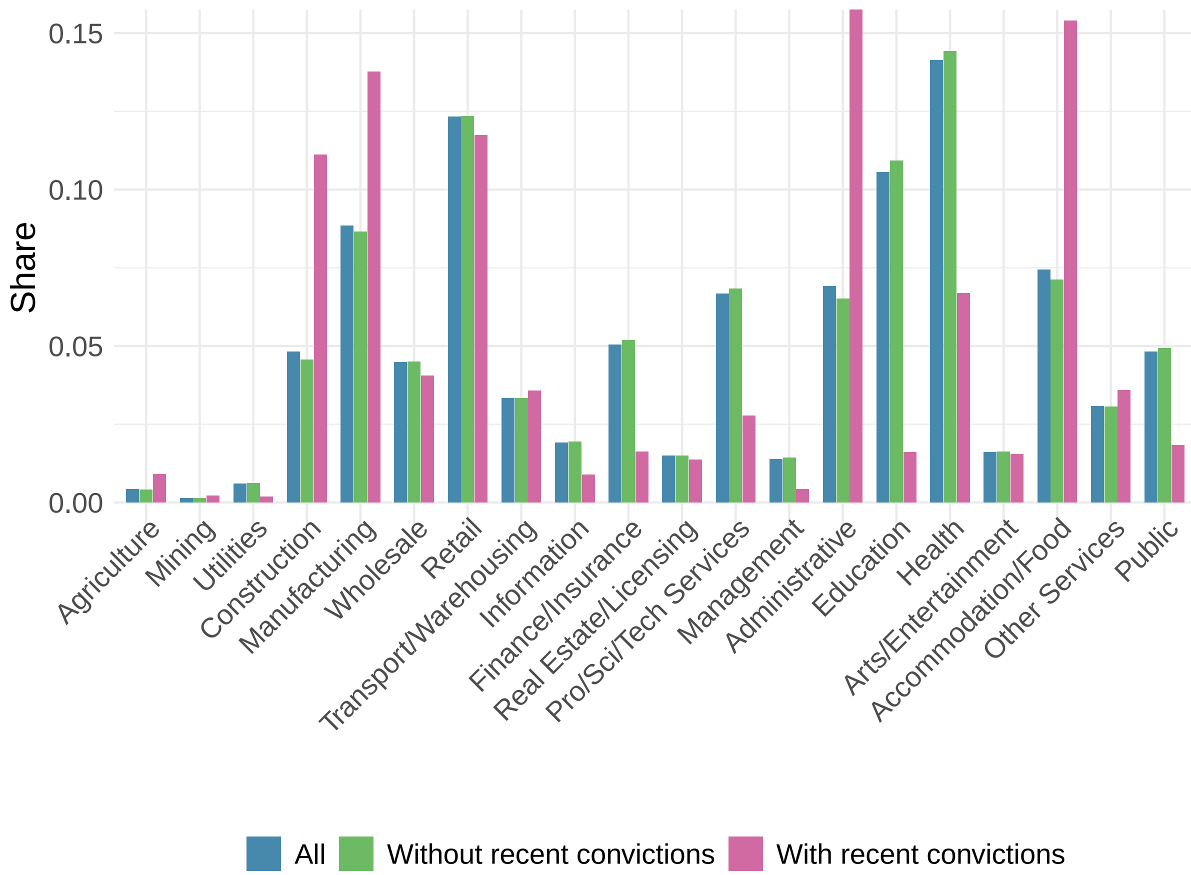
Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table demonstrates counties with higher entrepreneurship (i.e., share of firms that are new entrants) have higher employment rates for individuals with convictions, even controlling for local conviction rates and employment rates of individuals *without* recent convictions. This pattern disappears under BTB.

N = 1,600. Mean of dependent variable: 0.625. Mean (std dev) of county new firm rate: 0.036 (0.012).

Observations are weighted by county-year population. Conviction rate bin FE refers to fixed effects for ventiles of the indicator for having a recent conviction. Non-convicted emp rate bin FE refers to fixed effects for ventiles of the employment rate for individuals without recent convictions.

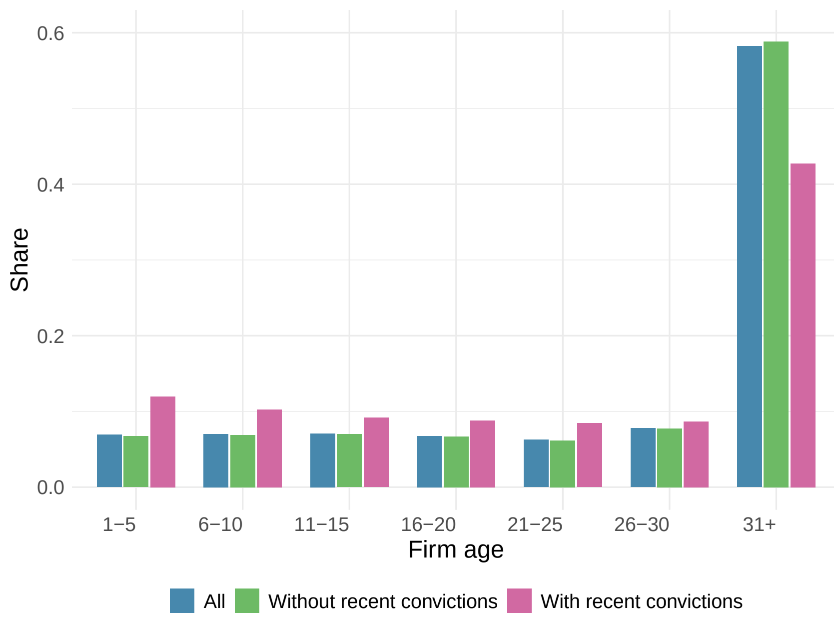
Figure 1: Sectoral distributions for workers, by conviction history



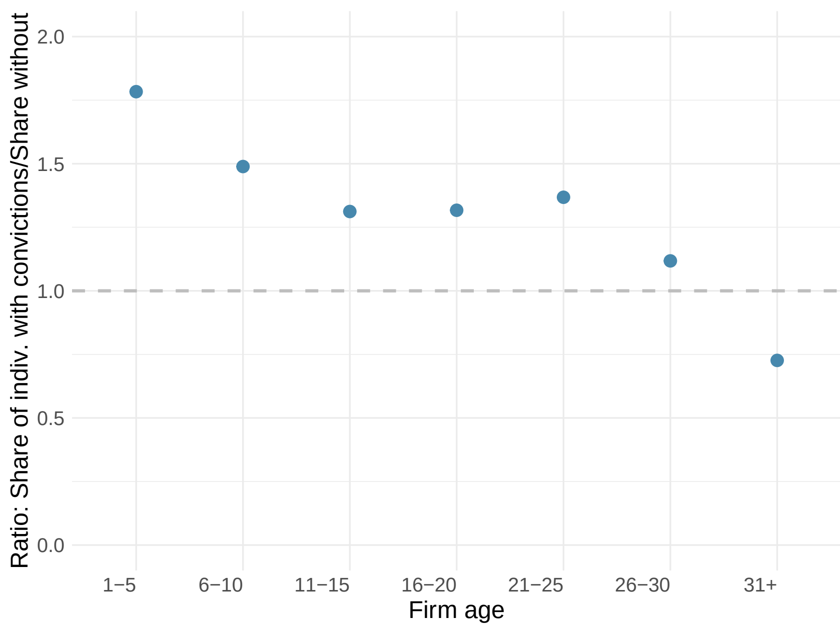
Note: This figure plots the sectoral distribution for workers, both in total ("All") as well as split on whether an individual has had any convictions in the past 7 years. (Note that the "All" values are weighted averages of the split series.)

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

(a) Distributions

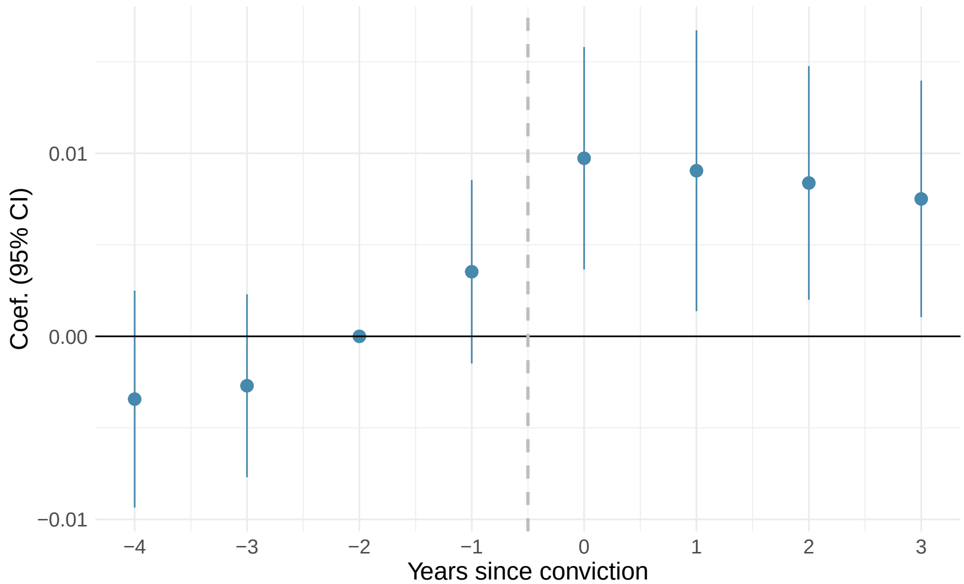


(b) Relative shares



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

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



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

A.1 Appendix Tables and Figures

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

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

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

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

N = 11,260,000.

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

Table A.2: Individuals with recent convictions are less likely to work

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

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

Note: This table demonstrates how convictions predict working. The columns gradually build up model (2), slowly adding controls as demonstrated in the table footer.

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

Table A.3: Workers with recent convictions disproportionately work at younger firms, regardless of firm size

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

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

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

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

Table A.4: Workers with recent convictions disproportionately work at younger firms, across sectors

	Dependent Variable: Work at a young firm									
Sample:	Natural Resources [N = 71,000]		Construction [N = 674,000]		Manufacturing [N = 1,043,000]		Trade and Transportation [N = 2,142,000]		Information [N = 155,000]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Recent conviction	0.016***	0.008	0.032***	0.001	0.011***	0.000	0.031***	0.007***	0.015***	0.003
	(0.005)	(0.008)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.005)
Constant	0.127***	0.120***	0.101***	0.098***	0.030***	0.029***	0.044***	0.048***	0.041***	0.038***
	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Individual FE		x		x		x		x		x
Mean(work at a young firm)	0.120	0.120	0.098	0.098	0.029	0.029	0.048	0.048	0.038	0.038
Mean(recent conviction)	0.067	0.067	0.084	0.084	0.056	0.056	0.034	0.034	0.016	0.016

Sample:	Financial Services [N = 563,000]		Professional Services [N = 1,625,000]		Education and Health Services [N = 2,109,000]		Leisure and Hospitality [N = 1,190,000]		Other Services [N = 307,000]	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Recent conviction	0.035***	-0.001	0.044***	0.012***	0.055***	0.008***	0.031***	0.008***	0.041***	0.003
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.005)
Constant	0.046***	0.045***	0.083***	0.087***	0.046***	0.048***	0.158***	0.155***	0.154***	0.147***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Individual FE		x		x		x		x		x
Mean(work at a young firm)	0.045	0.045	0.088	0.088	0.048	0.048	0.156	0.156	0.147	0.147
Mean(recent conviction)	0.015	0.015	0.047	0.047	0.012	0.012	0.071	0.071	0.040	0.040

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

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

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

Table A.5: Job Postings by Firm Entry Year Cohort Robustness: Background Check Mentions by Firm Age, 2015–2019 vs. 2020–2024

	Dependent Variable: Job Posting Mentions a Background Check									
	2015–2019					2020–2024				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Young firm	−0.001	−0.002**	−0.002**	−0.005***	−0.002**	−0.003***	−0.008***	−0.008***	−0.009***	−0.006***
(age ≤ 5)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.071***	0.072***	0.072***	0.074***	0.072***	0.088***	0.091***	0.091***	0.091***	0.089***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
County FE		x	x	x	x		x	x	x	x
Year-month FE		x	x	x	x		x	x	x	x
Industry FE			x	x	x			x	x	x
Occupation FE				x	x				x	x
Firm size FE					x					x

Standard errors in parentheses. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Note: This table demonstrates how job postings vary by whether firm age is less than or equal to 5, split by two cohorts of job posted year. Data are at the job-posting level. Standard errors are clustered at the firm level.
$N = 36{,}912{,}778$ in models (1)-(5). Mean of dependent variable: 0.070. $N = 54{,}323{,}247$ in models (6)-(10). Mean of dependent variable: 0.076.

Table A.6: Individuals with recent convictions who work at young firms tend to have low future earnings

Dependent Variable:	Total earnings in 2 years		Log total earnings in 2 years	
	(1)	(2)	(3)	(4)
Recent conviction	-12,800***	-12,850***	-0.553***	-0.949***
	(143)	(143)	(0.004)	(0.006)
Work at young firm	-8,030***		-0.949***	
	(103)		(0.006)	
Recent conviction × Work at young firm	3,790***		-0.068***	
	(406)		(0.017)	
Work at unsuccessful young firm		-12,010***		-1.272***
		(254)		(0.011)
Recent conviction × Work at unsuccessful young firm		4,436***		0.043
		(915)		(0.039)
Work at successful young firm		-7,321***		-0.425***
		(111)		(0.005)
Recent conviction × Work at successful young firm		3,814***		-0.063***
		(444)		(0.019)
Constant	46,900***	46,870***	9.045***	9.045***
	(41.77)	(41.77)	(0.002)	(0.002)
Age FE	x	x	x	x
County-year FE	x	x	x	x
Sector FE	x	x	x	x
Past earnings bin FE	x	x	x	x
Mean(dep var)	48,040	48,040	9.001	9.001
Mean(dep var | no recent conviction)	49,020	49,020	9.05	9.05
Mean(dep var | recent conviction)	23,200	23,200	7.762	7.762
For non-workers (i.e., not in table):				
Mean(dep var | no recent conviction)	6,139	6,139	2,978	2,978
Mean(dep var | recent conviction)	2.132	2.132	2.194	2.194

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 nominal earnings. Non-workers are assigned zero earnings. Note that columns (3) and (4) considers log(1 + earnings) in order to include the non-workers' earnings.

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. Footer includes mean dependent variables for non-workers (who are not in the estimation sample for this table) for comparison.

All columns in indicators for sex and race/ethnicity. Note that individual fixed effects are not included; instead, we include controls for past earnings. A "successful" young firm is one in which the firm survives to at least age 5. Throughout, the left-out category is firms age 6 and older.