How do firms deal with the risks of employing ex-prisoners?

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We use linked employer-employee panel data from Hungary to investigate a large sample of firms that, in nine years, hired ex-prisoners versus people incarcerated later. We first compare their jobs, focusing on attributes, which can reduce the penalty the firm must pay for a wrong hiring decision. Second, we study if employers insure themselves by paying lower wages to ex-prisoners. Third, we analyze whether the probability of the match dissolving within a few months is lower if the firm could potentially base its hiring decision on referrals or its own prior experience in employing released inmates. The composition of former prisoners' employment is biased toward easy-to-cancel jobs. In the low-skill, high-turnover positions held by most ex-convicts, they do not earn less than their counterparts, but a small minority making it to white-collar jobs is paid significantly less. Ex-prisoners' jobs are highly volatile. We find that their jobs are less likely to dissolve within a short time if the hiring firm could potentially base its decision on employee, employer, or labor office referrals.

Keywords: incarceration, reintegration, mobility, discrimination, Hungary

JEL codes: J71, J23, J63

1 Introduction

In this paper, we deal with three types of demand-side obstacles that people with prison experience must face when they try to return to the society of "ordinary" people. First, firms may want to hire ex-prisoners for jobs, where the damage from wrong decisions is low, easy to avert, or easy to shift onto consumers and taxpayers. This type of precaution limits the pool of positions available to ex-convicts. Second, firms may want to insure themselves by paying risky employees lower wages: as far as it happens, it limits the availability of rewarding jobs. Third, many firms have no better choice than to base their hiring decisions on simple signals like a clean sheet – a practice that excludes many potentially productive and eager-to-work applicants. We study the relevance of these concerns empirically by looking at the composition of jobs that firms open to ex-offenders, their wages, and the impact of potential referrals on job matches' survival in the first few months of an employment relationship.[1]

Following Grogger (1995), Raphael (2007), LaLonde and Cho (2008), Pettit and Lyons (2009), Czafit and Köllő (2015), and Kőműves (2015), we compare former and prospective prisoners,

[1] This research was financially supported by Hungary's National Research, Development and Innovation Office (project K124975).

under the conviction that the latter represents a better control group for the ex-inmates than any sample chosen from the general population based on observables. Many obstacles in the way of reintegration (low skills, exposure to racial prejudice, substance use, inexperience in job search techniques, and unfamiliarity with interview situations) are common to the two groups. Still, some hurdles to jump appear or get higher after prison. Losing friends (and quite often the family) reduces one's network access, thereby decreasing their ability to locate job offers. A known criminal record makes the mobilization of referrers more difficult. It exposes the former inmates to both animus (Becker 1957, Goldin and Rouse 2000, Bertrand and Mullainathan 2004, Leonard, Levine and Giuliano 2010) and statistical discrimination (Phelps 1972, Arrow 1973, Coate and Loury 1993, Norman 2003, Rodgers 2009, and others).

The picture on these impediments is far from being complete. A wealth of enterprise surveys, interview-based reports, and planned experiments (Pager 2003, Fahey et al. 2006, Agan and Starr 2018 are often cited examples) yield valuable information on employer behavior but they cannot directly check the implications on the workers subject to firms' precautionary and discriminative practices. Studies based on big administrative samples (Grogger 1995, Kling 2006, Raphael 2007, Holzer 2007, Lalonde and Cho 2008, and Dobbie et al. 2018 in the USA, Nagin and Waldfogel 1995 in the UK, Skardhamar and Telle 2009 and Bhuller et al. 2020 in Norway, Drago et al. 2009 in Italy, Czafit and Köllő 2015 in Hungary) can measure the implications of prison experience on subsequent employment and wages. Still, they typically lack information on the employers, the characteristics and duration of the acquired jobs, coworkers, and ex-convicts' pre-prison labor market careers.[2] Hickes et al. (2016), a noteworthy exception, analyze the careers and relative performance of ex-prisoners employed in the US Army, one of the country's biggest employers.

We contribute to the latter strand of the literature by analyzing a unique linked employer-employee (LEED) panel, which covers 1.1 million admissions by 630 thousand employers, of which more than 29 thousand hired at least one past or future prisoner in nine years. The panel provides

[2] The rich US data, for instance, typically contain information on the subjects' criminal records, court trials, and type of detention, but they cannot identify the employer, and do not even cover public sector workers and those moving to other states (Holzer 2007).

information on the labor market careers of the workers and the characteristics of their employers. The data come from Hungary, 2003-2011.

In the first step, we estimate how a series of firm-level and job-level attributes affect the probability of hiring former *versus* prospective prisoners to employer-occupation 'cells' (jobs, for short). We estimate zero-inflated negative binomial regressions because of many excess zeros and overdispersion (Greene 1994). As a robustness check, we also present results from a penalized maximum likelihood model proposed by Firth (1993) and Coveney (2008). We find that ex-prisoners, compared to future convicts, have a higher probability of working in simple, high-turnover, casual jobs, open toward unemployment, and project-based activities. Moreover, ex-offenders jobs are highly volatile: half of their employment spells terminate within three months, and more than two-thirds do so within six months.

Second, we compare past and prospective prisoners regarding their entry wages using OLS and fixed effects regressions. The data suggest no difference between the two groups in those blue-collar jobs, where the vast majority are employed. However, in managerial and white-collar occupations, the ex-prisoners earn significantly (20 to 40 percent) less, but their aggregate disadvantage primarily stems from an unfavorable change in their occupational affiliation.

Third, we investigate how employers' potential to base their decisions on referrals affects the probability that the job-worker match dissolves within a very short (one, three, or six months) time that we regard as a signal of an erroneous hiring/entry decision. We identify cases when (i) a worker had one-step or two-step acquaintances (a former colleague or a former colleague of a former colleague) in the hiring firm (ii) the entrant arrived from another firm without intermittent unemployment (iii) the worker was registered as unemployed before the entry. These cases raise the likelihood of employee, employer, and labor office referrals. We then compare those entries of the same person that differed along these dimensions. The fixed effects panel regressions show that the mentioned setups decrease the match's early dissolution probability by 2-14 percent, with registration in a labor office having the most substantial impact. We find no or significantly weaker effects for workers incarcerated later.

After introducing the data, the local context, and summary statistics (Section 2-4), Sections 5-7 present the above estimates. Section 8 concludes. Appendix tables and figures are referred to as A1.1.-A*n.n*.

2 Data

The data come from a big LEED panel anonymized by the National Info-Communication Service (NISZ) and prepared for analysis by the Databank of the Center for Economic and Regional Studies (CERS) at the Hungarian Academy of Sciences. The original data came from the registers of five institutions (Pension Directorate, Tax Authority, Health Insurance Fund, Public Employment Service, and the Office of Education). The data covers a 50 percent random sample of Hungary's resident population aged 5-74 in January 2003. Nearly 4.6 million individuals are followed monthly until December 2011. The key variables used to build our datasets include gender, age, employment relationships, days in work during the month, amounts earned, occupational code, hash-coded employer ID, firm-level variables like sales revenues, exports, ownership shares, and the place of residence in 2003.[3] Unfortunately, educational attainment is painfully missing.

Employers have made it to the sample if they paid taxable income to at least one sampled person, at least once in 2003-2011. The sample includes firms, budget institutions, small businesses, and even sole proprietors if they remunerated themselves in a taxable way. We have annual data for the employers, covering the entire period when they existed within the window of observation. The firm-level variables are added to the respective person-month records.

We identify prisoners based on social security contributions transferred by the central budget to the Health Insurance Fund during a person's detention. We know the start and end dates of incarceration but have no information on the type of detention. About 20 percent of the incarcerated are in pre-trial detention (typically spent in prison), while others are guarded in three kinds of facilities of different stringency. This paper speaks of prisoners (incarcerated, convicts, inmates, offenders) if they spent some time behind bars in 2003-2011. We do not distinguish first-time prisoners from recidivists, assuming that one spell of incarceration is sufficient to stigmatize a person.[4] Individuals are called *former convicts* in the months after their first observed prison spell, and *future convicts* before their first observed incarceration.

We derived two special samples from the source file. In the first one, the unit of observation is an employer-occupation cell. We have 1,414,722 cells in 740,337 employers and 8 one-digit ISCO

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[3] On the full data set, see https://adatbank.krtk.mta.hu/en/admin-2-2003-2011/ and a description in Sebők (2019).
[4] The distinction is technically possible within the time window. The start dates of incarceration spells in effect on January 1, 2013 are also known, but we do not observe prison spells completed before that day.

occupations. (The estimation samples are smaller for reasons explained in the relevant sections). We observe the number of prospective and past prisoners hired by the firm for a given type of job sometime between 2003 and 2011, and the number of all entrants. This sample is used to estimate the number of hires to a firm-occupation cell.

In the second data set, used for the wage and 'early exit' estimations, the unit of observation is a past or future prisoner's entry to an employer. The samples used in the multivariate 'estimations are smaller as we had to narrow the time window and select observations suitable for fixed-effects models. Details on the preparation, content and limitations of these data will be given in the respective subsections.

On top of working with administrative data, we conducted sixty interviews with released prisoners on job search, job finding, and workplace experience. We do not explicitly use the interviews in this paper, but we rely on the lessons learned from them.[5] A not-so-surprising lesson is that about 40 and 30 percent of the jobs attended before and after incarceration were informal, respectively. Thus, this paper can analyze integration to the world of taxpayers working in tax-paying establishments – admittedly a fragment of the whole picture.

3 The local context

Hungary's incarceration rate ranged between 0.16 and 0.19 percent in the last decade – a level deep below those reported for the US (0.6-0.7) and the post-Soviet states (0.3-0.4), but higher than the EU average (Walmsley 2018). The fraction of those incarcerated at least once and possibly wear a stigma is much higher than that.

A generation life table calculation (following Bonzar 2003 and Skardhamar 2014) suggests that about 6.7 percent of the male population would be incarcerated at least once by age 64 if the age-specific first-incarceration rates would remain at their 2009-2011 levels (See Appendix 1). This slightly upward-biased estimate is close to the one reported by Skardhamar for Norway (6.2 percent) and markedly lower than the one reported by Bonzar for the US (11.3 percent).

[5] These lessons are briefly summarised in a Hungarian language book chapter (Köllő at al. 2020) and presented in detail in Csáki and Mészáros (2020). We are grateful to the Program of Excellence of the Hungarian Academy of Sciences, 2018-2021 for funding the interview stage.

We find an estimated 61 thousand 15–49-year-old males with no secondary school attainment (thus belonging to the highest-risk segment of the population) who had prison experience in nine years before December 2011 (equal to 3.7 percent of the group total). Among the registered unemployed, 7.2 percent were incarcerated at least once, with the estimate for the unskilled unemployed amounting to 10.1 percent.[6] Győri (2013) reports that 6.7 percent of homeless people in the countryside and 3.7 percent in Budapest got to the street from a prison.

Some specifics of the institutional and regulatory framework are to be mentioned.

Clean sheet regulations. Civil servant and public servant positions can be filled after presenting clean records. In practice, all public sector employers require a clean sheet for all jobs, and about 25-40 percent of the private companies do so, according to a survey by Csáki and Mészáros (2011). Time until the records clean depend on the duration of the sentence. It takes 3, 5, 8, and 10 years after penalties shorter than one year, 1-5 years, 5-10 years, and more than 10 years, respectively. In these periods, ex-inmates are also excluded from managerial positions in micro-firms and self-employment. Released prisoners can apply to a court for exemption, but their requests are rarely approved. As a result, most ex-inmates interviewed in our research do not even contact employers, who require a clean sheet.

Ban-the-Box, business crime insurance, legal responsibility for negligent hiring. These legal institutions and procedural rules did not exist in Hungary in our observation period. However, several years later, in 2018, a regulation restricted firms' right to require clean criminal records without thorough justification.

Public works (PW). PW is a large-scale program for the long-term unemployed, typically providing simple jobs in street cleaning, road and park maintenance, forestry, and (less frequently) social services. Registered unemployed can be called to do public works on short notice, any time and for any duration. Declining a call may imply exclusion from unemployment assistance for three years. In our time window, the remuneration was equal to the minimum wage. PW employed about 1 percent of the labor force in 2003 and 2 percent in 2011, but the respective shares were 2.1 and 6.6 percent among high-school dropouts.

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[6] We estimate the educational level of released prisoners using data on the prison population in the 2011 Census. (http://www.ksh.hu/nepszamlalas/?lang=en) The number of registered unemployed by education was taken from the 2011.q3 wave of the Labor Force Survey (https://ec.europa.eu/eurostat/cache/metadata/EN/employ_esqrs_hu.htm.)

Subsidies. Subsidies explicitly targeting released prisoners did not exist in 2003-2011, but those available for the employers of former long-term unemployed could reach some ex-convicts.

5 Descriptive statistics

For the paths of employment and wages before and after incarceration, see Appendix 2. Tables 1 and 2 present selected indicators of persons with prison experience and firms and public institutions employing them.

Table 1: Employment of persons with prison experience – Selected indicators

Period relative to the first observed incarceration:	Before	After
Employment 2013–2011		
Was employed at least once (%) [a]	50.7	58.7
Months in work/total time spent outside prison (%)	19.5	23.1
Time until finding the first job after release (months)	..	18.0
Occupation at entry, 2013–2011 (%)		
Manager	4.0	2.7
Professional	1.5	1.0
Technician, assistant	6.4	4.8
Trade and service occupations	9.0	7.1
Skilled blue-collar	15.6	7.1
Assembler, machine operator	13.9	12.4
Elementary occupations	37.5	44.3
Unknown (mostly sole proprietors) [b]	12.1	13.4
Total	100.0	100.0

The data relate to 39,304 former and prospective prisoners.
a) Prisoners in detention throughout 2003-2011 are excluded.
b) Sole proprietors are not obliged to report their occupational code

The first two rows of Table 1 show that while the proportion of those who worked at least once is relatively high, they spent less than 20 and 25 percent of their time outside the prison in employment before and after incarceration, respectively. Finding the first post-prison job took one and a half years on average. The composition of jobs changed in favor of elementary occupations.

The left block of Table 2 shows that 1.5 and 2 percent of the employer-occupation cells hired at least one prospective or past prisoner, respectively, in nine years, but the number of such firms is relatively high (16 and 22 thousand). Public employers running PW programs, labor market services, and temporary work agencies are much more likely to hire prisoners. Still, most future and ex-convicts are employed by business firms, as shown in the middle block.

Table 2: Employers of past and future prisoners – Selected indicators

	Hired at least one prisoner (%)		Distribution of prisoners (%)		Share of prisoners (per-mill)	
	future	past	future	past	future	past
Unit of observation:	Employers		Entries		Entries	
Public sector						
PW providers	9.2	11.0	26.0	31.1	4.2	6.4
Other public institutions	2.1	1.7	4.2	3.3	2.3	1.5
Labor market services	30.2	37.3	3.5	3.1	5.3	12.3
Private sector						
Temporary work agencies	9.8	12.8	3.7	3.9	8.4	9.4
Sole proprietorships	0.2	0.2	1.4	0.7	2.1	1.3
Firms (simple bookkeeping)	1.0	1.4	8.1	8.5	4.4	5.3
Firms (double bookkeeping)	1.7	2.3	53.1	49.4	4.5	5.4
Total/unweighted mean	1.5	2.0	100.0	100.0	4.2	4.8
Number of observations	16,357	21,824	40,144	42,993	7,583,199[a]	

The data relate to 1,051,715 firm-occupation cells. In 505 cases the type of employer is unknown
PW=public works
a) The total number of workers hired in 2003-2011

Hiring a prisoner is a sporadic event. Even in temporary work agencies, the fraction of prisoners within all hires falls short of ten per mill. The third specifics to be mentioned is that prisoners' jobs are highly precarious (Table 3): a quarter, half, and more than two-thirds of their jobs terminate within one, three, and six months, respectively. These rates are higher by a factor of 1.6 than those measured among non-prisoners.

Table 3: Fraction of jobs terminating within a short time

Job terminates within	Non-prisoner	Future prisoner (at entry)	Former prisoner (at entry)
1 month	17.0	26.8	26.7
3 months	33.1	54.0	51.5
6 months	43.8	71.5	67.1

Spells started before February 2003 and those not terminating by December 2011 are excluded.
The samples cover 6,712,494, 6,566,022 and 6,255,838 cases in the three rows, respectively.

The reader might find these dissolution rates, especially those relating to the general population, suspiciously high. In fact, they are not inflated. According to the interview-based LFS, the fraction of jobs terminating within six months amounted to 39.9 percent on average in 2003-2011, only marginally lower than the 43.8 percent based on administrative data in Table 3.[7] The

[7] The difference is presumably explained by cases, when a survey respondent regards her/his long-term attachment to a firm continuous despite short breaks in contribution payment due to stoppages, unpaid leave, or time between two projects. These breaks appear as exit in our data.

respective rates were 19 percent for workers with at least secondary school attainment, 31 percent for a vocational qualification, 43 percent for workers with a primary school background, and 67 percent for incomplete primary education.[8]

6. Hiring ex-prisoners

This section's critical event is the hiring of workers with past versus future prison experience. The firms under scrutiny hired 42,993 workers with one or more prison spells served between January 2003 and the entry date and admitted 40,144 workers, who were incarcerated later. For a description of the estimation sample see Appendix 3.

Our choice of model is shaped by observations on the composition of employers and the size distribution of hiring.

As we saw in Table 2, close to 30 percent of the prisoners were hired by public institutions, especially those running public works programs or providing labor market services. We have several reasons to analyze these employers separately. First, their motives for offering jobs to unskilled workers and discriminated minorities are different from those of profit-oriented businesses. Second, several key variables are missing for their in-depth analysis, such as their industrial affiliation and capital intensity. (The balance sheet data of public institutions are collected by the Treasury and do not appear in our firm-level data, which come from the Tax Authority.) The data on temporary work agencies and micro-firms are also incomplete. In the former case, the firm-level data relate to the agency rather than the employer where the person works. At the same time, small businesses are exempt from double book-keeping and do not report their detailed financial data. Therefore, we will start by estimating our models for all employers, using a limited set of explanatory variables, and following models for business firms using a larger battery of controls.

Data on the distribution of hiring warn that we need a model that can treat both excess zeros and overdispersion. As was shown in Table 2, only 1.5 and 2 percent of the firm-occupation cells hired future and former convicts in nine years, respectively. Those who did, typically hired only one such worker, but a small minority employed many, with the record holders hiring more than one thousand. This pattern implies a high variance compared to the mean. The model should also

[8] Authors' calculation using the LFS. The figures relate to respondents observed as employed in quarter *t* and non-employed in quarter *t*+1. Job tenure in quarter *t* is observed in the survey.

consider that many firms fail to hire ex-convicts because they do not meet them, while others do so because they dislike applicants with criminal records.

Econometric models, which meet the requirements mentioned above, are the zero-inflated Poisson (ZIP) and zero-inflated negative binomial (ZINB) regressions, proposed in Lambert (1992) and Greene (1994), respectively. Both models assume that a part of the zeros are generated by a model other than the process generating the counts. We will use the ZINB, which better suits overdispersed data.

The "inflation equation" of the ZINB estimates the probability of no encounter of a firm and a job seeker (excess zeros). We use two explanatory variables: the total number of entrants and a time-invariant measure of the regional unemployment rate.[9] More vacancies to be filled increases the probability of an encounter between the firm and job seekers with prison experience, while having more unemployed job seekers in the market reduces the likelihood of no applicant for a posted vacancy.[10] The latter might be called the scale effect of unemployment. We expect that both regressors lessen the probability of a zero outcome in the model for future prisoners (an indistinguishable minority). By contrast, in the equation for released prisoners, the effect of unemployment is potentially positive since employers are often aware of the applicant's criminal record, and there are more non-stigmatized competitors per vacancy. Therefore, a selection effect may dominate the scale effect.

The count equation of the ZINB estimates the number of future and past convicts hired for the given job, conditional on a positive outcome. We estimate the equations by adding the total number of entries as an exposure variable, which indicates how many times the event could have happened.

The tables report the incidence rate ratios for the count equations (IRR). An IRR=1.28 belonging to a coefficient of β=0.25 (IRR=e^{β}=1.28) , for instance, indicates that a unit change in the explanatory variable increases the number of hired persons by about 28 percent holding total hires constant. The model also estimates a so-called overdispersion parameter [ln(α)]. If its value significantly differs from zero, we prefer the negative binomial to the Poisson.

[9] The relative unemployment rate is calculated as the firm-occupation level intertemporal mean of u_{irt}/U_t, where u_{irt} is the unemployment rate in month t of region r, where entrant i came from, while U_t is the country-wide unemployment rate in month t.
[10] Ideally, we would use the number of *applications* for the job, but we regrettably do not have data on that.

We estimate separate equations for the hiring of future and past convicts and compare the coefficients using a Wald-test. Significant chi-squared values suggest that the parameters of the two equations differ from each other.

Results

Table 4 presents the estimates for all employers. In the count equation, only employer type dummies and occupation fixed effects are included while excess zeros are explained by the total number of hires and relative unemployment.

Table 4: Zero-inflated negative binomial estimation for the number of entries by prisoners All employers 2003-2011

	Entries by prisoners		
	Before prison	After prison	Wald-test
Count equation			
Public sector, no PW	0.722***	0.420***	21.9***
	(6.30)	(14.03)	(0.000)
Public sector, some PW	0.895***	1.095**	11.0***
	(2.64)	(2.17)	(0.001)
Temporary work agencies	1.200***	1.629***	19.5***
	(3.20)	(8.87)	(0.000)
Labor market services	1.106	1.673***	10.6***
	(0.83)	(4.22)	(0.000)
Sole proprietorships	0.630***	0.342***	31.3***
	(7.26)	(16.22)	(0.000)
Businesses, no tax report	0.996	1.010	0.2
	(0.18)	(0.46)	(0.681)
Manager	0.791***	0.636***	14.2***
	(6.51)	(12.21)	(0.000)
Professional	0.207***	0.191***	0.8
	(28.60)	(28.20)	(0.361)
Other white collar	0.336***	0.326***	0.4
	(35.06)	(35.83)	(0.521)
Trade and service worker	0.394***	0.384***	0.5
	(33.51)	(34.74)	(0.486)
Assembler, operator	0.941**	1.057**	10.2***
	(2.13)	(1.99)	(0.001)
Elementary occupation	1.216***	1.550***	65.5***
	(8.38)	(19.62)	(0.000)
Occupation unknown	0.449***	0.597***	29.2***
	(23.31)	(16.37)	(0.000)
Small firm (<10 workers)	1.398***	2.250***	178.0***
	(15.15)	(36.46)	(0.000)
Constant	0.009***	0.007***	
	(189.1)	(205.6)	
Inflation equation (*probit*)			
All entries	0.980***	0.978***	0.5

	(12.44)	(16.63)	(0.489)
Relative unemployment	0.845***	1.238***	34.3***
	(3.50)	(5.15)	(0.000)
ln(α)	1.779***	1.888***	
	(19.85)	(22.29)	
Number of observations	1,087,078		

* *p*<0.1; ** *p*<0.05; *** *p*<0.01

Count equation: incidence rate ratios. Inflation equation: probit coefficients. Z-values in parenthesis. The standard errors are clustered on employers. Wald-test: significance levels in parenthesis. Exposure variable: all entries. Estimation: Stata *zinb*.

Sample: employer-occupation cells hiring at least one worker in 2003-2011. Reference categories: firms reporting their balance sheet to the Tax Authority; skilled blue collars. PW=public works.

The estimates and the Wald chi-square tests (for the cross-equation equality of the coefficients) suggest that former convicts have a high probability of being hired by temporary work agencies and as assemblers and machine operators. More former than future prisoners are hired by public works suppliers and public institutions providing labor market services. Ex-convicts are less likely to make it to public employers who do not run public works programs and have a lower probability of working in white-collar and trade and service jobs. The inflation equations show that, compared to future convicts, the hiring of ex-convicts depends more heavily on the total number of hires. As expected, the probability of not meeting future prisoners is negatively correlated with average unemployment in the firm's labor market. By contrast, in the case of released prisoners, the probability of a zero outcome rises with unemployment.

The results for firms (Table 5) add further pieces to the picture. Ex-prisoners are more likely to be hired for jobs, where tenures are typically short, rely on unemployed job seekers, and employ casual workers.[11]

State-owned firms with softer-than-average budget constraints, small firms (where monitoring is less costly), and businesses with lower levels of fixed assets per worker are more likely to hire past than future prisoners. We would expect the opposite in the case of exporters, but the coefficient for ex-convicts is significantly higher. This is most probably explained by the dominance of mass producers employing unskilled labor and temporary workers (unobserved in our data).

[11] The dummy for „no completed spell in the time window" is also high for ex-convicts. These firms are typically small (86 percent had 1-4 workers) and most of them existed in either the beginning or the end of the observed period.

Table 5: Zero-inflated negative binomial estimation of the number of entries by prisoners Firm-occupation cells 2003-2011

	Entries of prisoners		
	Before	After	Wald-test
Count equation			
Fraction hired from LTU[a]	0.920*	1.336***	31.8***
	(1.7)	(6.6)	(0.00)
Jobs terminating within 3 months	1.611***	2.471***	48.7***
	(10.9)	(23.6)	(0.00)
No completed spell in the time window	0.300***	1.182***	193.2***
	(16.1)	(3.8)	(0.00)
At least one casual worker[b]	0.558***	1.564***	240.0***
	(11.5)	(10.9)	(0.00)
Sate-owned firm (at least once)	1.015	1.145***	3.2*
	(0.3)	(2.8)	(0.07)
Exporter (at least once)	0.801***	0.849***	3.2*
	(10.0)	(7.5)	(0.07)
Log capital/labor ratio[c]	0.995	0.977***	11.7***
	(1.5)	(7.1)	(0.00)
Small firm (<10 workers on average)	1.205***	1.737***	80.2***
	(7.4)	(21.5)	(0.00)
Percent hired from ‘dense’ Roma zip	13.108***	4.726***	4.6**
	(8.0)	(4.7)	(0.03)
Manager	0.821***	0.533***	37.8***
	(4.6)	(13.3)	(0.00)
Professional	0.276***	0.234***	2.5
	(28.0)	(21.1)	(0.11)
Other white collar	0.359***	0.346***	0.5
	(28.0)	(28.7)	(0.50)
Trade and service worker	0.456***	0.413***	13.5***
	(22.4)	(25.1)	(0.00)
Assembler, operator	1.027	1.193***	13.4***
	(0.8)	(5.6)	(0.00)
Elementary occupation	1.213***	1.329***	6.9***
	(7.3)	(11.0)	(0.01)
Occupation unknown	0.571***	0.408***	22.4***
	(12.3)	(19.0)	(0.00)
Agriculture	1.231***	1.308***	0.9
	(4.49)	(5.85)	(0.34)
Communal services	1.077	1.417***	12.0***
	(1.2)	(6.1)	(0.00)
Construction	1.290***	1.465***	9.2***
	(8.5)	(13.0)	(0.00)
Trade	0.990	1.060**	2.7
	(0.37)	(2.01)	(0.10)
Transport	1.064	1.171***	2.5
	(1.5)	(3.7)	(0.11)
Services	0.810***	0.968	15.5***
	(6.8)	(1.1)	(0.00)
Private health & education	0.693***	0.586***	1.6

	(5.6)	(7.1)	(0.21)
Industry unknown	1.525	3.077***	2.5
	(1.37)	(4.62)	(0.11)
Inflation equation			
All entries	0.985***	0.981***	2.7
	(9.37)	(13.19)	(0.10)
Relative unemployment	1.221***	1.754***	16.1***
	(3.11)	(10.02)	(0.00)
ln(α)	1.430***	1.396***	
	(9.55)	(9.10)	
Number of observations	629,741	629,741	

* p<0.1; ** p<0.05; *** p<0.01

Dependent variable: the number of prisoners hired in 2003-2011.
Count equation: incidence rate ratios. Inflation equation: probit coefficients. Z-values in parenthesis. The standard errors are clustered on employers. Wald-test: khi-squared, significance levels in parenthesis. Exposure variable: all entries.
Estimation: Stata *zinb*.
Sample: employer-occupation cells of business firms that hired at least one worker in 2003-2011.
Reference categories: skilled blue collars, manufacturing.
a) Fraction hired from LTU: the fraction of workers hired after at least three months of non-employment during which the person was registered as unemployed at least once
b) Employment with a' casual work booklet' grants simplified administrative procedure and lower social security contribution.

Firms customarily hiring workers from zip code areas with a high Roma population share employ much more workers before or after prison, but this is more likely to occur in the former case. However, the implied effect is weak as the average Roma share is low, with a mean of 2.2 percent and a standard deviation of 3.2 percent.

We continue to see that the number of entrants to white-collar and trade and service jobs fall, while semi-skilled and unskilled jobs occur more frequently after than before incarceration. The ban on leading sole proprietorships explains a significant fall in the "occupation unknown" category until the criminal record is not clean (sole proprietors do not report their occupation). The coefficients of the industry dummies indicate a shift toward communal services and construction, two sectors offering simple jobs and project work. The magnitudes of the effects are easy to assess in the case of dummy variables. The IRR values estimated for elementary occupations (1.21 and 1.33), for instance, indicate that 21 and 33 percent more future and past prisoners were hired into these jobs than into the reference category (skilled blue collars), holding the total number of hires constant.

In the case of continuous variables, their variance should also be considered. The fraction of workers hired from long-term unemployment, for instance, has a mean of 0.14 and a standard deviation of 0.28. The IRRs (0.92 and 1.34) measure the effect of a unit change in the explanatory variable. In the relevant range, the effect is about one-third of that, between -3 and 8 percent as we move from the bottom to the top of the standard deviation range. A similar calculation for the effect of short spells (mean=0.28, s.d.=0.36) suggests 16 percent more future prisoner entries at the top than at the bottom of the standard deviation range. In comparison, in the case of former prisoners, the estimated effect is 41 percent. Finally, the prediction is -2 percent for the hiring of future prisoners, and -7 percent for ex-prisoners in response to a one standard deviation difference in the fixed effects per worker ratio (mean=-2.44, s.d.=2.94).

In the inflation equations, the effects of the total number of hires are negative, as expected, and statistically equal. On the other hand, unemployment positively affects the occurrence of certain zeros in both equations but suggests that ex-prisoners are less likely to find prospective employers in a high-unemployment environment.

Robustness checks

While the assumptions of the ZINB precisely fit the problem discussed here, the highly unequal distribution of hiring (Table 6) might raise concerns. On the one hand, the results can be driven by a few outliers.[12] On the other hand, the loss of information from dichotomizing the dependent variable seems to remain within tolerable limits.

Table 6: The distribution of firm-occupation cells by the number of prisoners hired in 2003-2011
(Cells hiring at least one prisoner)

	Mean	St. dev.	Median	P75	P90	Max
Future prisoners	2.1	14.2	1	1	3	1192
Ex-prisoners	2.2	18.0	1	1	3	1438

To check how the results change, we estimate the probability that a cell hired at least one prisoner using a penalized maximum likelihood model (Firth 1993) adapted to Stata by Coveney (2008). The *firthlogit* model is proposed to analyze rare events. It deals better with quasi-separation, and can securely reach convergence compared to the *logit*. In this case, the regressors of the ZINB inflation equation are included on the right-hand side. The coefficients are to be interpreted as in an

[12] Excluding a few heavy outliers actually did not change the results.

ordinary logit. The results (Appendix 4) are like those of the ZINB's count equation. The impacts of all hires are similar on the hiring of former and future prisoners. Unemployment does not affect prospective prisoners but less released convicts are hired in high-unemployment regions.

Experiments with alternative measures of job stability (like the turnover rate, typical duration of unemployment of the entrants before being hired) and firm characteristics (continuous firm and cell size variables, wage level, share in industry-level sales revenues) did not change the qualitative conclusions.

Estimating our models for the whole period of observation may raise concerns because prospective prisoners are more likely to be hired at the beginning of the time window, while ex-prisoners' entries are rather concentrated at the end of it. The before-after difference is potentially explained by a growing share in admissions of firms leaning on the secondary segment of the labor market. In Appendix 5 we show that the employers of former and future convicts differed in terms of our most important explanatory variables in both the first and the second half of the time window.[13]

5 Wages

Do firms "insure" themselves against the risks of employing prisoners by paying lower wages? We study this question by estimating three variants of a wage equation estimated for future and ex-convicts entering employment:

(i) How within-occupation entry wages differ between otherwise similar ex-convicts and future convicts hired for similar jobs in the same month (OLS)?

(ii) How the results change if we consider wages within occupations *and* firms in a model with firm fixed effects?

(iii) How workers' entry wages differ depending on whether they started their jobs before or after incarceration (a model with worker fixed effects)?

The unit of observation in Equation (2) is a job start of a worker, and the dependent variable is her/his average daily wage in the given job spell until the end of the year of entry. Wages are normalized for the economy-wide average wage in the given month.

[13] The only exception is the share of workers hired from long-term unemployment that differs in the first but not the second part of the observed period.

$$(2)\quad w_{ijkt} = \beta_1 P_{it} + \sum_{k=2}^{8} \gamma_k O_{jk} + \sum_{k=2}^{8} \beta_k P_{it} O_{jk} + \boldsymbol{\alpha X} + \boldsymbol{\delta T} + [\mu_i, \mu_j] + \varepsilon_{ijt}$$

In the equation, w_{ijkt} stands for the wage of person *i* starting a job spell in firm *j*, occupation *k*, and month *t*. The P_{it} dummy indicates if the person is before or after prison at time *t*. The O_{jk} dummies denote occupations. **X** and **T** stand for controls and month-of-entry dummies, respectively, while μ_i and μ_j are person and firm fixed effects included alternatively. The $P_{it}O_{ijt}$ interactions allow the wage difference between former and prospective convicts to vary by occupations. The full difference between them in occupation *k* is measured by the sum $\beta_1 + \beta_k$, presented in Table 7. For descriptive statistics of the estimation sample see Appendix 6.

Table 7: Wages after prison relative to wages before prison, by occupations - Regression estimates

		Fixed effects	
	OLS	Firm	Worker
Managers	-0.288***	-0.459**	-0.028
	(5.3)	(2.1)	(1.2)
Professionals	-0.414***	-0.306**	-0.214***
	(22.9)	(4.3)	(33.0)
Other white collars	-0.215***	-0.072*	-0.136***
	(45.0)	(3.7)	(54.7)
Trade and service workers	-0.022**	-0.008	-0.033**
	(4.5)	(0.3)	(4.5)
Skilled blue collars	0.022***	0.028***	0.017
	(10.8)	(12.4)	(2.1)
Assemblers, operators	0.008	0.008	-0.004
	(1.0)	(1.0)	(0.1)
Elementary occupations	-0.007	0.002	-0.016*
	(1.8)	(0.1)	(3.1)
Occupation unknown	0.080***	0.003	0.107***
	(8.3)	(0.1)	(57.2)
Adjusted R^2, within R^2	0.1010	0.0313	0.0356
Number of observations	81,114	81,114	81,114
Number of groups	..	29,311	23,264

* p<0.1; ** p<0.05; *** p<0.01

Regression estimates of Eq 1. The F-ratios in parentheses test the hypothesis that $\beta_1+\beta_k=0$ for the k^{th} occupation. The standard errors are clustered for firms in the OLS and firm fixed effects equations. Controls: gender, age, age square, log NUTS2 regional unemployment rate relative to the national mean, log firm size, industry dummies, and month fixed effects. Constant firm and individual variables drop out from the respective fixed effects equations.

A small minority of prisoners hired for white-collar jobs (recall Table 1) appear to earn significantly less than prospective prisoners. The estimates vary across specifications, but with only a single exception (managers in the person fixed effects model) hint at two-digit percentage points disadvantages. In blue-collar positions, most of the estimates are statistically insignificant, and they are insignificant economically. The "occupation unknown" category mainly includes sole-proprietors and their employees. For them, the OLS and the worker fixed effects estimates indicate a gain. Given that these businesses are typically small, the firm fixed effects estimate should be uncertain (zero in the last row, column two).

6. Potential sources of referrals and job duration

Ex-prisoners' jobs tend to be short-lived, as was previously discussed (Table 3). One reason behind "early exit" can be a wrong hiring decision based on insufficient or misleading information about the applicant. In this section, we study whether a job has a better chance to survive a short initial period if the hiring firm was in the position to acquire person-specific information. We look at three potential sources of information.

Acquaintance in the hiring firm. We identify the presence of prisoners' former colleagues and people who formerly worked with a former colleague of the prisoner. We will call them one-step and two-step acquaintances and assume that their presence increases the likelihood of a better hiring decision, thereby decreasing the probability of an early dissolution of the job-worker match. At least one acquaintance was present in 37 percent of the entries.

Job-to-job flows. Hiring a worker directly from another firm raises the probability of acquiring employer referrals. We identified cases when the time between two jobs did not exceed one month (the worker was employed in firm *A* on the 15^{th} day of month *t*, and firm B on the 15h day of month t+1). As in the case of acquaintances, we only observe *potential* referrals. The share of offenders hired via job-to-job movement amounted to 14 percent.

Registration at a labor office. The public employment service can provide detailed information about a worker and screen the applicants based on information about both parties. Twelve percent of the future convicts and 19 percent of the ex-convicts were registered at an office at least once in three months preceding the examined entry.

Before we start, recall Section 4 suggesting that employment shifted toward firms offering unstable jobs. It does not imply that ex-prisoners have shorter employment spells than their counterparts foreseeing incarceration. Several factors like criminal activity, pre-trial detention, and court trials interrupt employment in the former group, while many released from prison wish to work permanently. Table 8 compares the probability that an employment spell terminates within three months (τ<*3*) within the quintiles of firm-occupation cells sorted by E(τ<*3*). Ex-convicts are less likely to lose or leave their jobs within three months within each category of the firm-occupation cells. Even so, ex-convicts' probabilities of early exit are high by any standards.

Table 8: The probability that an employment spell terminates within three months (τ<*3*)

Quintiles of firm-occupation cells sorted by E(τ<3)	Future convicts	Ex-convicts
1 (lowest)	0.228	0.179
2	0.437	0.350
3	0.544	0.432
4	0.637	0.542
5 (highest)	0.807	0.784

The data relate to the estimation sample of Table 9, column 3 (entries of past and future prisoners 2006-2011)

Also, note that the importance of referrals in ensuring longer tenure and less discrimination is a debated issue. Several papers have identified a positive role (Decker and Cornelius 1979, Kirnan et al. 1989, Simon and Warner 1992, Petersen et al. 2000), but others (including recent research, stricter about identification) cast doubts. Taylor and Schmidt (1983) did not find longer tenure or lower absenteeism among referred applicants. Breaugh and Mann (1984) found minimal recruitment-source differences in turnover, and referrals had higher turnover than direct applicants. Williams, Labig, and Stone (1993) and Werbel and Landau (1996) also failed to find differences in turnover across recruitment sources. Fernandez et al.'s (2000) report that workers hired through referrals did not have lower turnover than those recruited in other ways. Padulla and Pager (2019) do not find evidence that connecting the job seeker with someone at the company plays a statistically significant mediating role. We nevertheless expect that referrals should be a trigger in finding a suitable job for a minority struck with poor network access and network mobilization capacity.

Acquaintances at the hiring firm

Following Boza and Ilyés (2018, 2020), we first check the presence of acquaintances. A one-step acquaintance is a person who formerly worked with the prisoner for at least one month (i) in a firm employing less than 50 workers (ii) a bigger firm but the color of their collar was similar (iii) a bigger firm, but the acquaintance was a manager. Cases of re-entry are included. A two-step acquaintance did not work with the entrant but worked with a third person, who worked together with the entrant in the past. For an example, see Appendix 7. We identified 12,618 one-step and 5858 two-step acquaintances, and 4733 cases when both types were present, We estimate the probability of the job's termination within k=[1,3, 6] months as shown in Equation 2:

$$(2) \quad \Pr(\tau_{ijt} < k) = \beta_1 P_{it} + \beta_2 A_{ijt} + \beta_3 P_{it} A_{ijt} + \alpha X_{it} + \gamma F_{jt} + \mu_i + \epsilon_{ijt},$$

where τ_{ijt} measures the completed duration of a job of person *i* in firm *j*, starting in month *t*. X_{it} and F_{jt} denote time-varying individual and firm attributes. The μ_i fixed effects capture unobserved personal characteristics, while ε_{ijt} is an error term. The P_{it} dummy indicates if the person is before or after prison at time *t*. A_{ijt} is set to one if there was at least one acquaintance in firm *j* at the time of the prisoner's entry.

The β_2 coefficient of A_{ijt} measures the effect of potential reference. The parameter of interest is β_3 of the interaction term $P_{it}A_{ijt}$: $\beta_3<0$ would suggest that the influence of an acquaintance is more substantial in the case of post-prison entries when the applicant is exposed to an additional component of discrimination.

The effects in question are identified from cases when the same person entered different jobs. The scope for such within-career changes is broad: 93 percent of the ever-employed prisoners had more than one job, with the average number of entries amounting to 5.7.[14]

The analysis must cope with limitations and biases. We observe those, who had been hired, not the applicants. We do not know if the potential referrer proposed the applicant or not. On top of that, we face endogeneity, selection bias, and measurement errors.

Sample selection. You could have a former colleague at your new workplace if you had jobs in the past. If you had jobs, you belong to the upper tiers of the prisoner population. The bias for prisoners attached to the labor market is further bolstered by the requirement of observing at least three jobs: one to collect potential former colleagues and two subsequent ones of a different character. The experience of such a selected sample does not necessarily predict the impact of possible referral on the average prisoner but, we believe, is informative of employer behavior.[15]

Endogeneity. All we can strive at is estimating a *correlation* between the presence of acquaintances and job duration. Referrals can help find stable jobs and increase job tenure thanks to better

[14] Within-firm shifts between occupations are excluded since the employer knows the worker.
[15] As Silva (2018) argues, referred applicants belonging to a discriminated minority may be a more selective group than referred applicants coming from the majority. We admit this as a weakness of our analysis.

match quality and commitment to the referrer (A_{ijt} causes τ_{ijt}). At the same time, the firm is likely to rely on referrals in the case of stable jobs ($E(\tau_{ijt})$ causes A_{ijt}). We found no instrument correlated with and affecting the outcome only through A_{ijt}. Trying to assess the sign and strength of the correlation still makes sense. Whether it *causes* stable jobs or a *precondition* of admission, person-specific information can help achieve better matches.

Measurement errors. First, *g*iven that we work with a 50 percent sample, we fail to observe half of the current and former coworkers. This is a random error implying inward bias. Second, we do not observe colleagues before 2003. The probability of this error rises with age. Therefore we re-estimate the model for young people (aged 27 or younger in 2003). Third, the likelihood that a potential referrer fails to refer increases with firm size. As a robustness check, we re-estimate the model for small firms. (See the results of both estimations in Appendix 8). Finally, we ignore other potential referrers (friends, relatives, neighbors) and have no basis for judging their role relative to employees. Several pieces of the literature (Miller and Rosenbaum 1997, Holzer 2007) suggest that employees are the most successful referrers.

In brief, our models capture a correlation, but we do not regard it as a grave problem. They relate to people attached to the labor market - an issue we cannot cure. We have measurement errors, but the most serious one implies that we underestimate the actual effects. Third, the models only distinguish very short (1-6 months) spells from longer ones, partly because early exit is our focus of attention, partly for technical reasons.[16]
We narrow the time window to 2006-2011, leaving time for the accumulation of former colleagues, and successively exclude jobs starting after months 102, 105, and 107 (since we cannot check if they terminated three, two, or one month later).

As shown by the parameter of A_{ijt} in Table 9, the presence of an acquaintance reduces the risk of an early exit in the period before prison. The jobs started after release are less likely to terminate within a short time (as shown by the parameter of P_{it}). Having an acquaintance in the firm

[16] We unsuccessfuly experimented with fixed effects survival models (Cox regressions). As Allison (2009, 71-79) shows, this model is rather demanding: on top of the restrictions we make, we lose cases in which the second of two employment spells is shorter than the first one (op.cit.79). A further problem arises because the length of the last (typically right-censored) spell is not independent of the lenghts of the preceding spells.

decreases this probability further by 2-4 percentage points, as indicated by the coefficients of $P_{it}*A_{ijt}$.[17]

Table 9: Acquaintance at entry and job duration, 2006-2011

(P=the entrant is ex-prisoner, A=at least one acquaintance at entry)

	Linear panel regression The job terminates within			Conditional panel logit The job terminates within		
	1 month	3 months	6 months	1 month	3 months	6 months
P_{it}	-0.045***	-0.109***	-0.115***	-0.335***	-0.541***	-0.704***
	(2.8)	(58)	(6.2)	(3.4)	(5.8)	(6.3)
A_{ijt}	-0.029**	-0.038***	-0.027**	-0.175**	-0.194***	-0.178**
	(2.5)	(3.0)	(2.5)	(2.5)	(2.9)	(2.3)
$P_{it}*A_{ijt}$	-0.022*	-0.036**	-0.039***	-0.171**	-0.207***	-0.252***
	(1.7)	(2.4)	(2.8)	(2.0)	(2.6)	(2.7)
Entries	34,263	33,206	31,165	15364	17,571	13,474
Persons	14,941	14,688	14,112	3960	4688	3748
Months	37-107	37-105	37-102	37-107	37-105	37-102
Within R^2	0.051	0.039	0.032	..	..	..

* *p*<0.1; ** *p*<0.05; *** *p*<0.01

T and z values in paranthesis. Standard errors are adjusted for clustering by persons. On the model see Eq 2 and the accompanying text. Note that singleton observations drop out from the logit.
Sample: entries of past and future prisoners 2006-2011
Controls: age at entry, log size of the employer-occupation cell, industry, occupation, and month of entry dummies.

Job-to-job flows

The model for the effect of job-to-job flows is identical with the one in Equation (1), except that the A_{ijt} dummy is replaced with J_{ijt} denoting an entry by person *i* to firm-occupation cell *j* at time *t*. J_{ijt} is set to one if only one month elapsed between the exit from the previous job and entry to the current one. In this case, we retain the employment spells starting after t=2, and before t=107, 105, and 102. We continue to look at within-person effects by comparing different entries of the same person (Table 10).

The coefficient of the interaction term $P_{ijt}*J_{ijt}$ is insignificant in the equation estimating the probability of exit within one month. The estimates for $\tau<3$ and $\tau<6$ are negative and significant. Job spells starting with a job-to-job transition are less likely to dissolve quickly both before and after prison, but the probabilities are lower by about 3 percent in the case of post-prison spells.

[17] The marginal effect of the interaction term cannot be expressed as a scalar (Norton, Wang & Ai, 2009). We make do with the fact that the coefficients are significantly negative.

Table 10: Entry through job-to-job flow and job duration, 2006-2011
(P=the entrant is ex-prisoner, J=max two months between leaving the previous job and entry)

	Linear panel regression The job terminates within			Conditional panel logit The job terminates within		
	1 month	3 months	6 months	1 month	3 months	6 months
P_{it}	-0.051***	-0.101***	-0.107***	-0.416***	-0.557***	-0.649***
	(6.3)	(10.8)	(11.7)	(7.9)	(11.9)	(12.3)
J_{ijt}	-0.007	-0.022**	-0.023**	-0.048	-0.113**	-0.143***
	(1.0)	(2.1)	(2.5)	(0.9)	(2.5)	(2.9)
P_{it}*J_{ijt}	-0.011	-0.028**	-0.036***	-0.067	-0.148**	-0.190***
	(1.1)	(2.3)	(3.0)	(1.0)	(2.5)	(2.9)
Entries	74,988	73,168	69,895	43,923	50,290	40,563
Persons	22,213	21,956	21,483	8411	10,052	8348
Months	3-107	3-105	3-102	3-107	3-105	3-102
Within R^2	0.106	0.082	0.055	..	..	..

* p<0.1; ** p<0.05; *** p<0.01

T and z values in parentheses. Standard errors are adjusted for clustering by persons. On the model see Eq 2 and the accompanying text. Note that singleton observations drop out from the logit.
Sample: entries of past and future prisoners 2006-2011
Controls: age at entry, log size of the employer-occupation cell, industry, occupation, and month of entry dummies.

Registration at a labor office

We continue to look at workers in their pre-prison and post-prison periods, hired by firm *j* in month *t*. We set the dummy L_{ijt}=1 for those entries, which were preceded by registration at a labor office at least once in months *t*-1, *t*-2, and *t*-3. Identification comes from the work histories of people who had entries with and without prior registration. The specifications and the estimation methods are otherwise identical to those in the previous subsections.

As in the previous subsections, we find that post-prison jobs are less likely to terminate within a short time (Table 11, first row). Spells started after registration have a lower probability of ending within a short time both before and after incarceration. The coefficients of the interaction terms suggest that the likelihood of dissolution within 1, 3, and 6 months is lower by about 8, 9, and 14 percentage points, respectively. The employment spells we are looking at in Table 11 tend to last longer because the employers of registered unemployed receive a wage subsidy for a limited period. However, eligibility for the subsidy is unrelated to one's criminal history.

Table 11: Registration at a labor office and job duration
(P=the entrant is ex-prisoner, L=the entrant was registered as unemployed before being hired)

	Linear panel regression The job terminates within			Conditional panel logit The job terminates within		
	1 month	3 months	6 months	1 month	3 months	6 months
P_{it}	-0.051***	-0.102***	-0.104***	-0.405***	-0.567***	-0.665***
	(5.8)	(9.9)	(10.2)	(6.9)	(10.8)	(11.1)
L_{ijt}	-0.050***	-0.061***	-0.081***	-0.298***	-0.321***	-0.505***
	(4.7)	(4.7)	(6.2)	(4.1)	(4.9)	(6.6)
$P_{it}*L_{ijt}$	-0.079***	-0.087***	-0.138***	-0.680***	-0.526***	-0.887***
	(6.6)	(5.6)	(8.5)	(7.5)	(6.6)	(9.4)
Entries	63,444	61,896	59,314	34,839	39,427	30,225
Persons	21,756	21,495	21,006	7622	8981	7124
Months	4-107	4-105	4-102	4-107	4-105	4-102
Within R^2	0.125	0.097	0.058	..	..	..

* p<0.1; ** p<0.05; *** p<0.01

T and z values in parentheses. Standard errors are adjusted for clustering by persons. On the model see Eq 2 and the accompanying text. Note that singleton observations drop out from the fixed effects logit.
Sample: entries of past and future prisoners 2006-2011
Controls: age at entry, log size of the employer-occupation cell, industry, occupation, month of entry dummies, and person fixed effects.

Appendix 8 presents the estimates of the interaction terms $P_{ijt}*A_{ijt}$, $P_{ijt}*J_{ijt}$, and $P_{ijt}*L_{ijt}$ for young workers (27 or younger in 2003) and relatively small firms (employing less than 100 workers on average in 2003-2011). For young entrants, the effects of acquaintances, job-to-job flows and prior registration are negative and significant, with only one exception. The estimates for small and medium-sized firms are negative and significant for $\Pr(\tau<6)$ and $\Pr(\tau<3)$, but insignificant for $\Pr(\tau<1)$.

6. Discussion and implications for policy

According to official statistics on the prison population, the population we dealt with is predominantly unskilled: 65 percent has primary school or lower educational attainment, and a further 7 percent have dropped out of high school (Börtönstatisztikai Szemle 2016). Many of them belong to the poverty-stricken and discriminated Roma minority.[18] Census data on the prison population indicated a 26 percent Roma share in 2011. An earlier prison-based survey by Huszár (1999) reported 40 percent based on self-reported data and 44 percent based on interviewers' judgment. Important as they are, these attributes play a limited role in shaping the net contribution of prison

[18] See Kertesi and Kézdi (2011a) and (2011b) on Roma's disadvantages in school and the labor market, respectively, and Kende (2000) and Bernáth and Messing (2013) on discrimination.

experience to labor market failures. Skills are unlikely to erode strongly during the typically short episodes of incarceration (16 months on average in our sample) and can even improve thanks to in-prison education and work.[19] Roma people are exposed to discrimination before and after incarceration - their ethnic affiliation matters only if their goals and motivations evolve differently from the majority during incarceration (a possibility we cannot check).

We tried to capture the effect of prison experience by comparing released and prospective prisoners. First, we observed several signals of precautious employer behavior: firms and public institutions hiring ex-convicts tend to be smaller, the jobs they offer are typically simple and short-lived, they tend to hire from unemployment and employ casual workers, their equipment per worker ratio is lower. More former than future convicts are offered employment in public works programs, temporary work agencies, and project-based activities like construction. In brief, the composition of their employment shifts toward the "secondary segment" of the labor market (Doeringer and Piore 1971, Reich, Gordon, and Edwards 1973, Blossfeld and Mayer 1988, Hudson 2007).

Second, we found that in simple jobs, ex-prisoners earn the same wage as those incarcerated later, but they have a two-digit disadvantage in white-collar positions. (We suspect that they attend relatively simple tasks within the broad and heterogeneous one-digit occupational categories.) While the raw data hint at a wage loss (similar to findings in Lyons and Pettit 2011, Western, Kling and Weiman 2001, Holzer 2007, and Czafit and Köllő 2015), we do not find evidence of an incarceration penalty. Firms do not seem to "insure" against the risks of employing ex-prisoners via wage discrimination. This is not a striking outcome since discrimination within firm-occupation groups incurs costs due to workplace conflicts, discontent, and quits. Finally, we found that potential referrals reduce the risk of a quick dissolution of job-worker matches, more so than in the case of *former* prisoners.

We think that the results have messages for policymaking.

[19] In 2018, 45 percent of the prison population worked, and 17 percent studied according to Börtönstatisztikai Szemle (2019).

First, the public sector's contribution to ex-convict employment is strikingly modest (apart from institutions running public works programs). The Hungarian regulations exclude ex-offenders from civil servant positions (*közalkalmazott*) until cleaning their criminal record, that is, for a minimum of three years. The exclusion relates to the simplest jobs: in 2011, 73 percent of the public sector workers (excluding PW participants) in elementary occupations were employed as civil servants.[20] This kind of unconditional exclusion from cleaning, portal services, and similar positions is hard to justify, in our opinion.

A more general lesson is that private employers are cautious in hiring ex-prisoners. They tend to open short-lived, easy-to-cancel jobs to ex-offenders. It is unlikely that anti-discrimination regulations could radically change employer behavior. We believe that policies should accept this behavioral pattern and strive to bring as many ex-offenders behind the factory gate as possible. Requiring stable employment as a precondition of tax allowances seems to us counter-productive in the given context. Financial support to short-term jobs, simplified procedural rules, and business insurance might bring better results.

Third, as far as person-specific information helps improve the quality of worker-job matches, more rather than less information on ex-prisoners could be helpful. Hungary followed the US by restricting firms' access to the criminal records of job applicants. Findings on the unintended side effects of the Ban-the-Box regulations in the US (see Doleac and Hansen 2016, Rose 2019, Jackson and Zhao 2017, Agan and Starr 2018) warn that this practice can lead to more discrimination against social groups with a high crime rate, similar to the Roma minority in Hungary. Given the limited network efficiency of released prisoners, substantive information might come from civil organizations engaged in assistance to them. Sharing information about vacancies, including them in profiling, and utilizing their competencies in counseling could do a part of the screening necessary to contain statistical discrimination.

[20] Authors' calaculation using the Wage Survey of 2011.

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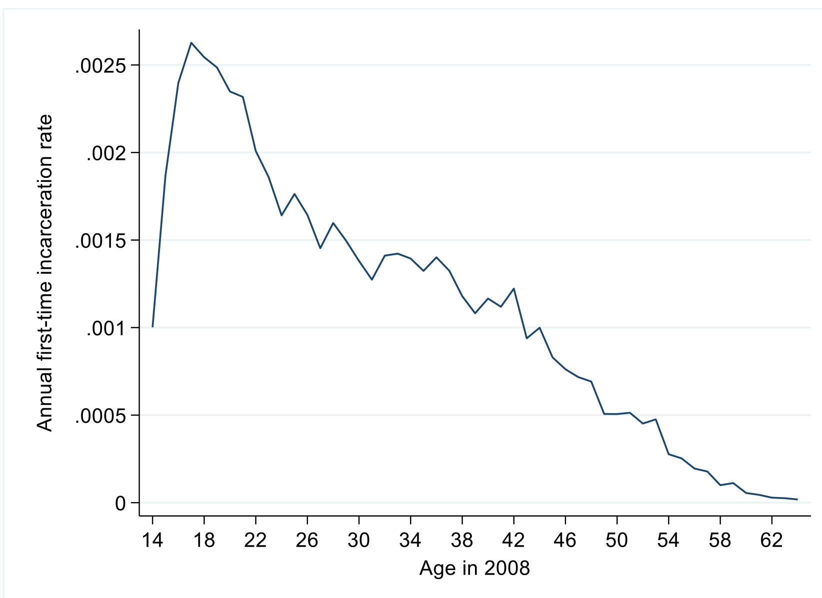
Appendix 1: Lifetime risk of incarceration – Generation life table estimation

The generation life table is used to estimate the fraction of a cohort incarcerated at least once until a specific age limit is reached. It treats the cross-section age-incarceration profile as if it described the evolution of a birth cohort over time. The estimation assumes that the first-time incarceration rates by single year of age remain valid over the life span of the youngest cohort.

In the Hungarian case, we further assume that for people not incarcerated in 2003-2007 the year 2008-2011 incarceration rates yield an acceptable (slightly downward-biased) approximation of the *first-time* incarceration rates. This assumption is justified by the patterns of recidivism and the age profile of incarceration. As shown in Czafit and Köllő (2015) using a similar data set, 80 per cent of those who returned to prison within seven years did so within three years, and more than 95 percent returned within five years. Therefore, we can be confident that the large majority of those incarcerated in 2008-2011, and not imprisoned between 2003 and 2007, went to prison for the first time. Second, as shown in Figure A2.1, incarceration rates are much higher at a young age than later. People, who were 15–25-year-old in 2008, were 10–20-year-old in 2003 and were unlikely to be incarcerated before our period of observation.

Figure A2.1.: Annual incarceration rates by age in 2008-2011



The estimated number of people incarcerated at least once is given by the area under the curve of Figure A2.1. Comparing this figure with the starting population of the 14-year-olds (as of 2008) suggests that under unchanged conditions, 6.7 percent of this cohort would be incarcerated at least once until age 64.

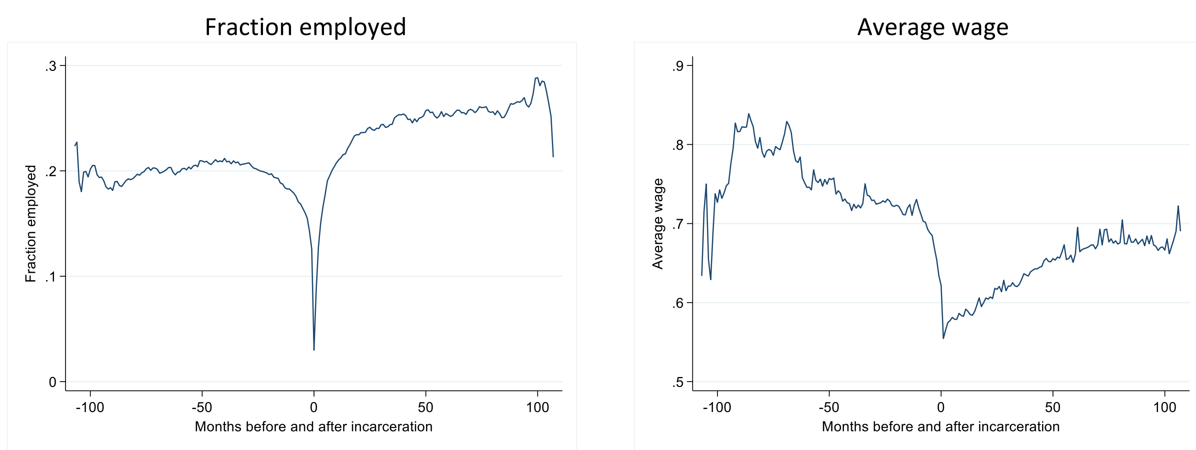
Appendix 2: Employment and wages before and after incarceration

Figures A3.1 relates to people incarcerated at least once in 2003-2011. We observe a gradual erosion of employment as the offenders are approaching the quarter of incarceration. First, the share of persons engaged in criminal activity rises as we move toward the date of incarceration. Second, many offenders lose their jobs in the period of investigation and trial, all the more as the trials take place in the region where the offenders committed crime, often far from their permanent place of living. Third, many employers lay off their workers when they get informed of their involvement in the judicial process. Workers who try to hide this information might be dismissed for unexplained absenteeism, while others quit voluntarily to keep their involvement secret.

We also observe attrition in daily earnings from about 70-80 to 55 percent of the national average. Deductions because of absenteeism and lower earnings from self-employment and payment-by-result schemes might play a role in this. Furthermore, white collar offenders are most probably exposed to longer investigation and trial compared to street-corner dealers and thieves, and face a higher risk of being fired in case their employer got news about their involvement in crime. Therefore, the sample of future prisoners is gradually biased toward unskilled workers as the time of incarceration is approaching.

The path of employment is nearly symmetric: employment starts to grow from virtually zero to 18 percent by the end of the first post-prison year and exceeds 20 percent from the second year. Daily wages fall substantially and stay below the pre-prison level throughout the observed period.

Figure A3.1. Fraction employed and average wage before and after incarceration



Note: Month zero stands for the period of incarceration. The months range from -107 to 107. A person is employed if she/he had income subject to pension contribution payment. Wages are normalized for the national average wage of the given calendar month.

Appendix 3: The estimation samples of the ZINB models

Table A3.1. Descriptive statistics of the estimation sample of ZINB – All employers

	Mean	St.dev.	Min	Max
All entries	6.964	227.1	1	93110
Entries of future prisoners	.0359	1.902	0	1192
Entries of ex-prisoners	.0393	2.477	0	1438
Public sector, no PW	.0146		0	1
Public sector, some PW	.0080		0	1
Sole-proprietorship	.1234		0	1
Temporary work agencies	.0042		0	1
Labor market services	.0003		0	1
Firms, no double bookkeeping	.2010		0	1
Firms, double bookkeeping	.6480		0	1
Manager (including of micro-firms)	.1254		0	1
Professional	.0660		0	1
Other white collar	.1661		0	1
Trade and service	.1547		0	1
Skilled blue collar	.0986		0	1
Assembler, machine operator	.0537		0	1
Elementary	.1064		0	1
Occupation unknown[a]	.2288		0	1
Firm size: less than 10 workers	.8893	.3137	0	1
Relative unemployment	.9549	.2999	.4731	1.856
Number of employer-occupation cells		1,087,078		
Number of employers		627,191		

a) The self-employed do not have to report their occupational code

Table A3.2. Descriptive statistics of the estimation sample of ZINB – Firms

	Mean	St.dev.	Min	Max
All entries	7.052	79.776	1	27705
Entries of future prisoners	.0393	.8405	0	348
Entries of ex-prisoners	.0389	.9788	0	560
Fraction hired from unemployment (>3 months)	.1403		0	1
Fraction of completed spells shorter than 3 months	.1		0	1
No completed spell in the time window	.1897		0	1
Employed at least one casual worker	.0346		0	1
State-owned at least once	.0176		0	1
Exporter at least once	.2677		0	1
Log fixed assets/worker ratio (mean)	-2.47	2.966	-14.1764	8.7848
Small firm (less than 10 workers on average)	.8418		0	1
Roma share[a]	.0203	.0287	0	.98347
Manager (including of micro-firms)	.1659		0	1
Professional	.0850		0	1
Other white collar	.203		0	1
Trade and service	.1476		0	1
Skilled blue collar	.1153		0	1
Assembler, machine operator	.0676		0	1
Elementary	.1197		0	1
Occupation unknown[a]	.0953		0	1
Agriculture	.0352		0	1
Manufacturing	.1567		0	1
Communal services	.0148		0	1
Construction	.1298		0	1
Trade	.2941		0	1
Transport	.0468		0	1
Services	.2788		0	1
Temporary work agencies	.0065		0	1
Health, education, administration (private)	.0362		0	1
Industry unknown	.0008		0	1
Relative unemployment	.9337		.4736	1.8562
Number of employer-occupation cells		629,741		
Number of employers		289,473		

a) Average 2011 Roma population share in the ZIP code area where entrants to the firm came from

Appendix 4: Hiring at least one prisoner

Table A4.1: Penalized maximum likelihood (Stata firthlogit) estimate of the probability of hiring at least one future or former convict

Dependent variable: hired at least one future/former prisoner

	Future	Former
All entries	0.015***	0.014***
	(62.03)	(60.20)
Fraction hired from unemployment (>3 months)	-0.121***	0.082**
	(2.73)	(2.19)
Fraction of short (<3 months) employment spells	0.323***	0.573***
	(9.88)	(19.76)
Employed at least one casual worker	-0.362***	1.443***
	(2.85)	(20.53)
State-owned at least once	0.429***	0.322***
	(5.99)	(4.86)
Exporter at least once	0.153***	0.170***
	(6.33)	(7.80)
Log fixed assets/worker ratio	-0.009**	-0.013***
	(2.36)	(3.77)
Roma share[a]	2.431***	1.802***
	(7.84)	(6.22)
Relative unemployment	-0.058	-0.290***
	(1.43)	(7.77)
Manager (including of micro-firms)	-0.937***	-1.368***
	(19.44)	(27.24)
Professional	-1.495***	-1.640***
	(20.05)	(22.84)
Other white collar	-0.837***	-0.798***
	(18.56)	(19.79)
Trade and service	-0.382***	-0.438***
	(9.25)	(11.60)
Assembler, machine operator	0.002	0.050
	(0.07)	(1.54)
Elementary	0.279***	0.371***
	(9.50)	(14.30)
Occupation unknown	-1.156***	-1.600***
	(19.71)	(27.96)
Agriculture	-0.181***	-0.091*
	(3.28)	(1.90)
Communal services	0.185**	0.422***
	(2.53)	(6.85)
Construction	-0.192***	-0.066**
	(5.74)	(2.19)
Trade	-0.320***	-0.254***
	(10.05)	(8.70)
Transport	-0.208***	-0.111**
	(4.30)	(2.56)
Services	-0.338***	-0.207***
	(9.60)	(6.50)
Health, education, administration (private)	-0.706***	-0.748***

	(6.68)	(7.24)
Industry unknown	0.557*	0.887***
	(1.95)	(3.74)
Constant	-4.342***	-4.668***
	(54.81)	(65.11)
	517,911	517,911

* p<0.1; ** p<0.05; *** p<0.01

a) Average 2011 Roma population share in the ZIP code area where entrants to the firm came from

Appendix 5: Key variables in the first and second part of the time window

Table A5.1 Characteristics of the firm-occupation cells hiring prisoners before/after their first sentence

Job characteristics (mean values)	First half 2003.01 - 2007.07	Second half 2007.08 - 2011.12	Entire period 2003.01 - 2011.12
Percent hired for blue collar jobs			
Before	77.8	73.6	76.4
After	81.6	77.6	78.6
Percent hired for elementary jobs			
Before	36.4	41.3	37.9
After	39.2	46.0	44.4
Job terminates within 1 month[a]			
Before	25.8	31.2	27.5
After	27.8	32.8	31.5
Job terminates within 2 months[a]			
Before	35.6	42.6	37.8
After	37.8	45.3	43.5
Job terminates within 3 months[a]			
Before	43.3	51.2	45.8
After	45.6	54.6	52.4
Fraction hired from long-term unemployment[a]			
Before	24.7	24.8	24.7
After	26.5	24.2	24.7
Fraction hired by small firms (*<10 workers*)			
Before	32.6	28.7	31.4
After	34.1	35.3	35.0

a) Unweighted mean of the hiring cells

Appendix 6: Descriptive statistics of the sample used in the wage and "early exit" models

Table A6.1: Descriptive statistics of the sample used in the wage and early exit models

	Mean	St.dev.	Min	Max
Former prisoner	.5175		0	1
Male	.9340		0	1
Age in 2003	28.4	9.527	8	65
Daily wage relative to the national mean	.5668	.5226	0	60.688
Log firm size (number of workers)	4.621	3.195	0	10.663
Relative unemployment	1.000	.309	.5057	1.8869
Manager (including of micro-firms)	.0335		0	1
Professional	.0120		0	1
Other white collar	.0555		0	1
Trade and service	.0798		0	1
Skilled blue collar	.1501		0	1
Assembler, machine operator	.1310		0	1
Elementary	.4101		0	1
Occupation unknown[a]	.1275		0	1
Agriculture	.0347		0	1
Manufacturing	.1470		0	1
Communal services	.0356		0	1
Construction	.1002		0	1
Trade	.1051		0	1
Transport	.0381		0	1
Services	.0948		0	1
Temporary work agencies	.0705		0	1
Health, education, administration (private)	.0146		0	1
Industry unknown	.3588		0	1
Number of entries		83,642		
Number of persons		23,453		
Number of employers		29,331		

a) Average 2011 Roma population share in the ZIP code area where entrants to the firm came from.
Note that the estimation samples are smaller because of the restrictions required by the models

Appendix 7: One-step and two-step acquaintances – An example

Table A7.1 One-step and two-step acquaintances

Time	*Y is a former collegue of X* (*one-step acquaintance*)			
0	Firm A (hiring firm)	X	Y	
-1	Firm A		Y	
-2	Firm A		Y	
-3	Firm B	X		Y
-4	Firm B			Y
	Z works in the hiring firm. N is a former colleague of Z, with whom X worked together in the past. (*Z is a two-step acquaintance of X*)			
0	Firm A (hiring firm)	X	Z	
-1	Firm A		Z	N
-2	Firm A		Z	N
-3	Firm C	X		
-4	Firm C	X		N

A, B, and C denote firms. X stands for the entrant we are interested in. Y, Z, and N are other workers

Appendix 8: Estimates of the "early exit" equations for young workers and small firms

Table A8.1.: The coefficients of the post-prison dummy interacted with the presence of acquaintances (A), job-to-job flows (J), and registration at a labor office (L)

Estimates for young workers and small firms

Young workers (aged 27 or younger in 2003)

	Linear panel regression The job terminates within			Conditional logit The job terminates within		
	1 month	3 months	6 months	1 month	3 months	6 months
$P_{ijt}*A_{ijt}$	-0.040**	-0.061***	-0.042***	-0.253**	-0.317***	-0.361***
	(2.4)	(3.1)	(2.6)	(2.7)	(3.1)	(3.0)
$P_{ijt}*J_{ijt}$	-0.013	-0.046***	-0.041**	-0.085	-0.238***	-0.223**
	(1.0)	(2.8)	(2.6)	(0.9)	(3.0)	(2.5)
$P_{ijt}*L_{ijt}$	-0.075***	-0.085***	-0.151***	-0.577***	-0.503***	-0.941***
	(4.8)	(4.4)	(7.4)	(5.3)	(5.1)	(7.8)

Small and medium-sized firms (less than 100 workers)

	Linear panel regression The job terminates within			Conditional logit The job terminates within		
	1 month	3 months	6 months	1 month	3 months	6 months
$P_{ijt}*A_{ijt}$	-0.014	-0.031*	-0.037**	-0.109	-0.158	-0.230*
	(0.9)	(2.1)	(2.1)	(1.0)	(1.5)	(1.9)
$P_{ijt}*J_{ijt}$	-0.029**	-0.039**	-0.041***	-0.176*	-0.189**	-0.222***
	(2.3)	(2.5)	(2.6)	(1.9)	(2.4)	(2.7)
$P_{ijt}*L_{ijt}$	-0.107***	-0.139***	-0.213***	-0.731***	.0.744***	-1.191***
	(4.9)	(5.5)	(7.8)	(5.0)	(5.5)	(7.8)

P = post-prison spell. A = acquaintance in the hiring firm. J = job-to-job flow. L = registration