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DISCUSSION PAPER SERIES

IZA DP No. 16645

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ABSTRACT

How Do Firms Deal with the Risks of Employing Ex-prisoners?*

We use linked employer-employee data to investigate a large sample of past and future prisoners in Hungary, 2003-2011. We first compare their jobs, focusing on attributes that can reduce the penalty the employer must pay for a mistaken 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. The composition of former prisoners' employment is biased toward easy-to-cancel jobs. In the unskilled jobs held by most of them, they do not earn less than future convicts, but a minority in white-collar positions are paid significantly less. Ex-prisoners' jobs are less likely to dissolve quickly if the hiring firm potentially had access to co-worker, employer, or labor office referrals.

JEL Classification: J71, J23, J63

Keywords: incarceration, reintegration, mobility, discrimination, Hungary

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* We used the Admin2 linked employer-employee panel built from administrative registers by the Databank of the HUN-REN Centre for Economic and Regional Studies. See a description at https://adatbank.krtk.mta.hu/en/adatbazisok/elerheto-adatbazisok/. The data is available for the international academic community for scientific purposes, via remote access. For a permission write to adatkeres@krtk.hu.

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 empirically study the relevance of these concerns 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 under the conviction that future inmates represent 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 appear or get higher after incarceration. Losing friends (and often the family) decreases 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 of 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) yield valuable information on employer behavior. Still, they cannot directly check the implications of firms' precautionary and discriminative practices. Studies based on big

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

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

In the first step, we estimate how 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). Compared to future convicts, we find that ex-prisoners have a higher probability of working in elementary, 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' 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 less (20 to 40 percent) – their aggregate disadvantage primarily stems from an unfavorable change in their occupational affiliation.

[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).

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 in the hiring firm, (ii) the entrant arrived from another firm without intermittent unemployment, and (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 (Sections 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 were 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

[3] On the full data set, see https://adatbank.krtk.mta.hu/en/admin-2-2003-2011/ and a description in Sebők (2019).

the employers, covering the entire period they existed within the observation window. 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 serve their sentences in three kinds of facilities of different stringency. This paper speaks of prisoners (incarcerated, convicts, inmates, offenders) who spent some time behind bars between 2003 and 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* after their first observed prison spell and *future convicts* before their first observed incarceration.

We derived two estimation 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 eight one-digit ISCO occupations. We observe the number of future and former prisoners hired by the firm between 2003 and 2011 and the number of all entrants. This sample is used to estimate the effect of firm and job characteristics on the number of hires to a firm-occupation cell.

The observation unit is a *past or future prisoner's entry* to an employer in the second data set, used for the 'entry wage' wage and 'early exit' estimations. 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,

[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.

[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.

respectively. Thus, this paper can analyze integration to the world of taxpayers – 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 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 estimate by Bonzar for the US (11.3 percent).

Furthermore, we find that 3.7 percent of the 15–49-year-old males with no secondary school attainment (thus belonging to the highest-risk segment of the population) had prison experience in 2003-2011. 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 changed 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 private companies do so, according to a survey by Csáki and Mészáros (2011). The time until the records are clean depends on the sentence's duration: 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

[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.)

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.[7]

Public works (PW). PW plays a vital role in the employment of ex-convicts. 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, at 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.

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

4 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.

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 in employment before and after incarceration, respectively. Finding the first post-prison job took one and a half years on average. The composition of employment changed in favor of elementary occupations.

Hiring a prisoner is a sporadic event. 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 likelier to hire prisoners. Still, most future and ex-convicts are employed by business firms, as shown in the middle block of Table 2.

[7] A few years later, in 2018, a regulation restricted firms' right to require clean criminal records without thorough justification.

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

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 book-keeping)	1.0	1.4	8.1	8.5	4.4	5.3
Firms (double book-keeping)	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 entries in 2003-2011

The third specific 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)
One month	17.0	26.8	26.7
Three months	33.1	54.0	51.5
Six months	43.8	71.5	67.1

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

The reader might find these dissolution rates suspiciously high, especially those relating to the general population, but other sources yield similar results. In 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. The difference is presumably explained by cases when survey respondents regard their long-term attachment to a firm as continuous despite short breaks in contribution payment due to stoppages, unpaid leave, or time between two projects. These breaks appear as exits in our data.[8]

5. Hiring ex-prisoners

This section's critical event is hiring 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 incarcerated later. For a description of the estimation sample, see Appendix 3.

Choice of model

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 many zeros and a high variance compared to the mean. The model should also 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 that meet the requirements mentioned above are the zero-inflated Poisson (ZIP) and zero-inflated negative binomial (ZINB) regressions proposed in Lambert (1992) and

[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.

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 between 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 increase the probability of an encounter between a firm and a job seeker with prison experience. Having more unemployed job seekers in the market reduces the likelihood of no applicant for a posted vacancy. 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 case of 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.

Choice of sub-samples

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 and (quite often) discriminated workers differ from those of profit-oriented businesses. Second, several key variables, such as industrial affiliation and capital intensity, are missing for an in-depth analysis.[10] Third, the data on temporary work agencies and micro-firms are incomplete. In the former case, the firm-level data relate to the agency rather than the employer where the person works. Small businesses are exempt from reporting their detailed financial data. Therefore,

[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 and region r, where entrant i came from, while U_t is the country-wide unemployment rate in month t.
[10] 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.

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.

Results

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) 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 an overdispersion parameter [ln(α)]. We prefer the negative binomial to the Poisson if its value significantly differs from zero.

We estimate separate equations for hiring 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.

Table 4 presents the estimates for all employers. The estimates and the Wald chi-square tests (for the cross-equation equality of the coefficients) suggest that former convicts are highly likely to be hired by temporary work agencies and as assemblers and machine operators. Public works suppliers and public institutions providing labor market services hire more former than future prisoners. 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, trade, and service jobs. The inflation equations show that, compared to future convicts, hiring ex-convicts depends more heavily on the total number of hires. As expected, the probability of not meeting future prisoners negatively correlates with unemployment in the firm's labor market. By contrast, in the case of released prisoners, unemployment increases the likelihood of a zero outcome.

The results for businesses (Table 5) add further pieces to the picture. Ex-prisoners are likelier 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 fixed assets per worker are more likely to hire past than future prisoners. We would expect the opposite for exporters, but the

[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.

coefficient for ex-convicts is significantly higher – a finding most probably explained by the dominance of mass producers employing temporary workers (unobserved in our data).

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.

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 workers[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 more future or past prisoners, 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 falls, while semi-skilled and unskilled employment occurs more frequently after than before incarceration. The ban on leading sole proprietorships explains a significant fall in the "occupation unknown" category. 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) indicate, for instance, 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. For instance, the fraction of workers hired from long-term unemployment 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, the estimated effect is 41 percent in the case of former prisoners. Finally, the prediction is -2 percent for hiring 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 and statistically equal. On the other hand, unemployment positively affects the occurrence of certain zeros in both equations. Still, it suggests that ex-prisoners are less likely to meet 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 hirings (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 similar to the ZINB's count equation. The impacts of all hires are identical on the hiring of former and future prisoners. Unemployment does not affect prospective prisoners; fewer released convicts are hired in high-unemployment regions.

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

Estimating our models for the whole observation period may raise concerns because prospective prisoners are more likely to be hired at the beginning of the time window. At the same time, ex-prisoner entries are biased for the end of it. The before-after difference is potentially explained by a growing share in admissions by 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 the first and the second half of the time window.[13]

6 Wages

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

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

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

(iii) How do 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 (1) is the job start of a worker, and the dependent variable is her 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 by groups in the first but not the second part of the observed period.

$$(1)\quad w_{ijkt} = \beta_1 P_{it} + \sum_{k=2}^{8} \gamma_k O_{jk} + \sum_{k=2}^{8} \beta_k P_{it} O_{jk} + \boldsymbol{\alpha X} + \boldsymbol{\delta T} + [\mu_i, \mu_j] + \varepsilon_{ijt}$$

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 any time. 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 occupation. The difference between them in occupation *k* is measured by $\beta_1 + \beta_k$, 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 appear to earn significantly less than prospective prisoners. The estimates vary across specifications, but except for managers in the person fixed effects model, they hint at two-digit percentage points of disadvantages. In blue-collar positions, most of the estimates are statistically insignificant, and they are also negligible economically. The "occupation unknown" category mainly includes sole proprietors and their employees. The OLS and the worker fixed effects estimates indicate a gain for them. These businesses are typically small, so the firm fixed effects estimate should be uncertain (zero in the last row, column two).

7. 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 mistaken 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 is in a 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 (one-step acquaintances) and people who formerly worked with a prisoner's former colleague (two-step acquaintances). We 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.[14] The share of prisoners 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.

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

[14] The worker was employed in firm *A* on the 15th day of month *t*, and firm B on the 15h day of month t+1.

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. (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 with inferior network access and network mobilization capacity.

Before we start, recall Section 4, which suggests that employment shifted toward firms offering unstable jobs. It does not imply that ex-prisoners have shorter employment spells than their counterparts foreseeing incarceration. Factors like ongoing criminal activity, pre-trial detention, and court trials interrupt employment in the former group, while many ex-prisoners wish to work permanently. Table 8 compares the probability that an employment spell terminates within three months ($\tau<3$) within the quintiles of firm-occupation cells sorted by E($\tau<3$). 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)

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. A two-step acquaintance did not work with the entrant but with a third person who had previously worked with the entrant. 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.

Model

We estimate the probability of the job's termination within k=[1,3, 6] months by Equation 2:

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

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 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.

A fixed effects model identifies the effects 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.[15]

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).

Limitations and biases

We observe those who have 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 error.

Sample selection. You could have a former colleague at your new workplace if you had jobs in the past. If you had jobs, you belonged 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

[15] Within-firm shifts between occupations are excluded since the employer knows the worker.

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.[16]

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 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, given that we work with a 50 percent sample, we fail to observe half of the current and former coworkers. This random error implies 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 an acquaintance 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.[17]

[16] 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.

[17] 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.

Results: acquaintances

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 decreases this probability further by 2-4 percentage points, as indicated by the coefficients of $P_{it}*A_{ijt}$.[18]

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.

Results: job-to-job flows

The model is identical to the one in Equation (1), except that we replace the A_{ijt} dummy with J_{ijt} set to one if only one month elapsed between exit from the previous job and entry to the current one. 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

[18] 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.

before and after prison, but the probabilities are lower by about 3 percent in the case of post-prison spells.

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			Conditional panel logit		
	The job terminates within			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.

Results: registration at a labor office

Instead of A_{ijt}, we set the dummy L_{ijt}=1 for entries 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 registration.

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 quickly both before and after incarceration. The coefficients of the interaction terms suggest that in the case of prior registration, 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, among others, 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.

Robustness checks

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 $Pr(\tau<1)$.

8. 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.[19] Census data on the prison population indicated a 26 percent Roma share in 2011. An earlier prison-based survey by Huszár (1999)

[19] 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.

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 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.[20] Roma people are exposed to discrimination before and after incarceration - their ethnic affiliation only matters if their goals and motivations evolve differently from the majority during incarceration (a possibility we cannot check).

We tried to capture the effect of the 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 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 the post-prison job-worker matches.

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

We think that the results have messages for policymaking.

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 they clean their criminal record, that is, for a minimum of three years. The exclusion relates to the simplest jobs, since in 2011, 73 percent of the public sector workers (excluding PW participants) in elementary occupations were employed as civil servants.[21] In our opinion, this kind of unconditional exclusion from cleaning, portal services, and similar positions is hard to justify.

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 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 be counter-productive in the given context. Financial support for 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. 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.

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

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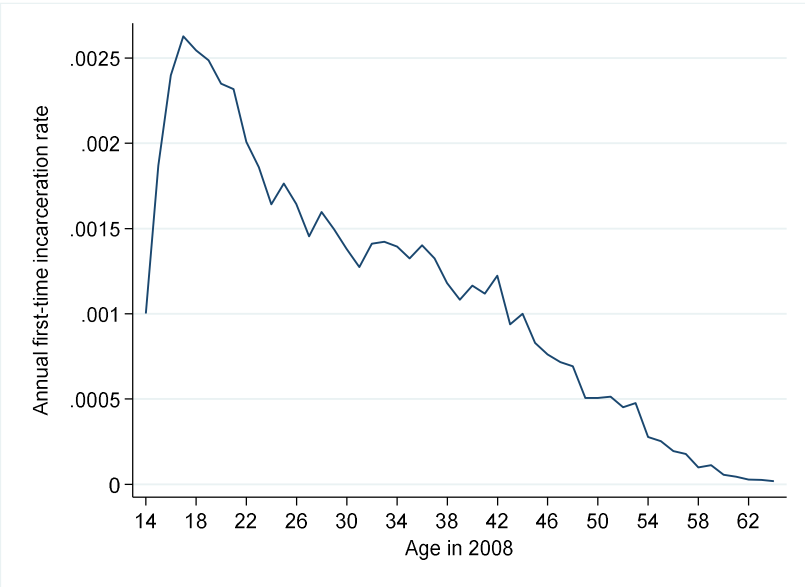
Appendix 1: Lifetime risk of incarceration – Generation life table estimation

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 age-first-incarceration profile as if it described the evolution of a birth cohort over time. The estimation assumes that the first-time incarceration rates by 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 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 percent 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 most 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 to 25 years old in 2008 were 10 to 20 years old in 2003 and were unlikely to be incarcerated before our observation period.

Figure A1.1.: 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 A1.1. Comparing this figure with the starting population of 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

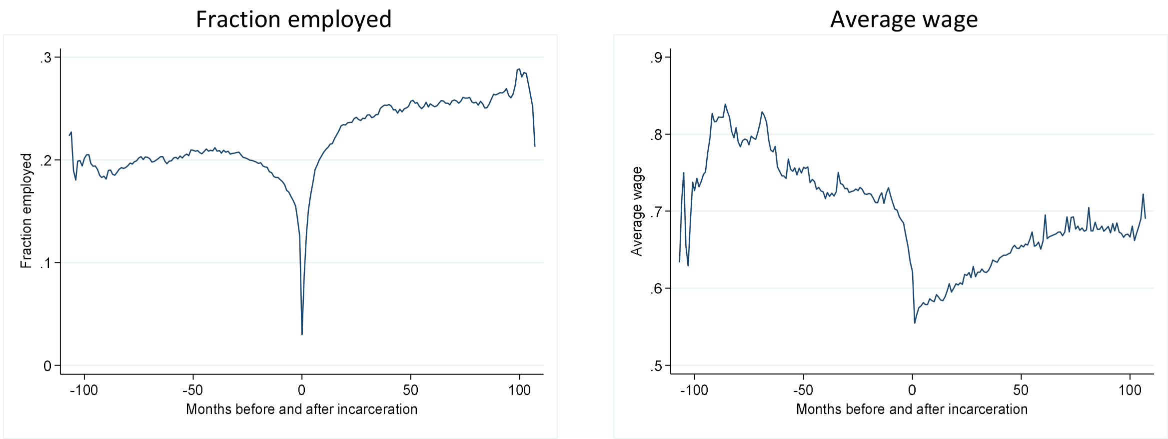
Figure A2.1 relates to people incarcerated at least once in 2003-2011. We observe a gradual erosion of employment as the offenders approach the incarceration date. First, the share of persons engaged in criminal activity rises as we move toward the date of incarceration. Second, many offenders lose their jobs during the period of investigation and trial, all the more so as the trials take place in the region where the offenders committed the crime, often far from their permanent place of living. Third, many employers lay off their workers when 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.

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 if their employer gets 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 employment path 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 A2.1. 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 book-keeping	.2010		0	1
Firms, double book-keeping	.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 – Firthlogit estimates

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 colleague 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. (*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

	Linear panel regression: The job terminates within 1 month	3 months	6 months	Conditional logit: The job terminates within 1 month	3 months	6 months
Young workers (aged 27 or younger in 2003)						
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)						
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