Czafit and Köllő *IZA Journal of European Labor Studies* (2015) 4:21
DOI 10.1186/s40174-015-0044-z

IZA Journal of European Labor Studies
a SpringerOpen Journal

ORIGINAL ARTICLE Open Access

Employment and wages before and after incarceration – evidence from Hungary

Bence Czafit and János Köllő*

* Correspondence:
kollo.janos@krtk.mta.hu
Centre for Economic and Regional Studies, Hungarian Academy of Sciences, Budaörsi út 45, 1112 Budapest, Hungary

Abstract

We study the entry into legitimate employment and earnings of a large sample of convicts released from Hungarian prisons in 2002–08. The employment rate of the prisoners falls short of 20% one year before incarceration, and they earn 25% below the national average. We identify the effect of prison by exploiting differences in the timing of incarceration and also by estimating fixed-effect models. For convicts with a single prison term, we find an initially negative effect on employment, which turns positive after a year, though the impact on earnings is permanently negative. A comparison with recidivists suggests that these results are driven by a drop in the reservation wages of 'reformed' criminals. This reading is supported by further data showing that the average ex-inmate tends to make increased efforts to find a legitimate source of livelihood and support in job search.

Keywords: Incarceration; Prison effects; Unemployment; Wage loss; Discrimination

JEL Classification: K42; J64; J39

1 Introduction

The effects of prison on employment and wages are theoretically unpredictable. A spell in prison brings with it a stigma and often leads to the loss of personal networks, with corresponding adverse consequences for finding a job and (predictably less so) for earnings. Detention impels convicts into a social environment that tempts them into further crime and keeps their labor supply low. At the same time, the experience of prison may deter offenders from committing further crimes, lower their reservation wages, and so improve their chances of getting a job. Lengthy incarceration implies a loss of work experience, and therefore lower wages. The reduced wages on offer may–at least temporarily–decrease the exit-to-job rate of ex-inmates, depending on how rapidly they adjust their reservation wages. Work in prison, training, and support services may simultaneously improve convicts' employability and earning potential. Since the relative strength of these effects varies with the institutional environment, social norms, labor market regulations, and prison practices, the net outcomes may differ substantially across countries and periods, and require empirical investigation. (See Western et al. 2001; Fahey et al. 2006; Geller et al. 2006; Holzer et al. 2003; and Holzer 2007 for comprehensive overviews of the above mentioned effects).

However, the effects of prison on subsequent labor market careers are difficult to identify, especially in research based on administrative data. The barriers to identification arise from the influence of unobserved personal and environmental characteristics that simultaneously predict criminal behavior and meager labor market outcomes. Models comparing released prisoners with observationally similar non-convicts can only estimate what Bound and Freeman (1992) label a 'crime–employment trade-off', rather than a causal effect. Such models indicate severe disadvantages on the part of ex-prisoners, but this might, at least partly, exist even without incarceration (Bound and Freeman 1992; Freeman 1991; Freeman 1996; and Grogger, 1995).

The scope for randomizing the event of incarceration by means of matching methods is limited, even in surveys that provide detailed portraits of the offenders. In research based on official population registers, any attempt to find people with a 'similar probability of being incarcerated' would be purely formal.[1] Finding instruments that affect the incidence or length of a prison term without affecting labor market prospects is also easier said than done.[2] Therefore, the register-based studies typically stay within the boundaries of the offender population and try to find alternative means of identification.

One way to deal with the problem is to collect data from the period preceding incarceration and to control for individual fixed effects in panel regressions that compare labor market outcomes before and after incarceration. While the fixed effects help to account for time-invariant unobserved characteristics, the model has the shortcoming that it treats pre-prison outcomes as the counterfactual for post-prison outcomes–an assumption that may or may not be valid in a changing economic environment and after a number of years spent in custody. Several studies try to overcome this problem by comparing the post-release experience of ex-convicts with the pre-prison experience of observationally similar future convicts, while controlling for calendar time. In this case, the pre-prison experience of the future inmates serves as the counterfactual situation on the assumption that the early and late cohorts are similar in terms of unobserved individual and contextual attributes. Widely cited examples are Grogger (1995), Western (2002), Raphael (2007) and LaLonde and Cho (2008).

Our benchmark model follows these studies in comparing the post-prison employment of ex-convicts with the pre-prison careers of offenders incarcerated later. For offenders with a single spell of prison in 2002–08, we find initially negative effects on both wages and employment. While the wage effect is strong and remains negative throughout the observed post-prison period, the employment effect fades rapidly and turns positive after a year out of prison. Fixed-effects models comparing the pre-prison and post-prison experience of inmates yield similar results, as do the model variants that reduce the potential bias from unobserved recidivism. It is important to note, however, that the legitimate employment rate of the ex-convicts remains very low: less than 25% even after 7 years spent continuously out of prison.

In interpreting this pair of findings, one should take into account the fact that the estimates inseparably capture negative and positive effects. Statistical discrimination tends to decrease both employment and wages, while giving up criminal activity is conducive to lower wages and higher employment. Therefore, the finding of a weak employment effect combined with a strong negative wage effect comes as no surprise in an environment where other effects (such as the loss of experience and improving marketability thanks to support services) are relatively weak. Indeed, most studies that apply such methods find a negative effect on wages, while the effects on employment are ambiguous and tend to be weak, or even positive in some of the more recent papers (see Needles 1996; Western and

Beckett 1999; Tyler and Kling, 2006; Sabol 2007; LaLonde and Cho 2008; Pettit and Lyons 2009, and overviews in Western et al. 2001; Geller et al. 2006; and Holzer 2007).

We try to separate the effects of discrimination and withdrawal from crime by looking at recidivists who continue committing crimes between two prison spells. For these periods, when the stigma of a criminal record is present, but the reservation wage effect is absent, we identify a significant negative impact on employment and only a minor decline in earnings. By contrast, the results for the period after the last observed prison spells of repeat offenders are similar to those of non-recidivists.

Our reading of the findings is valid on the assumption that amoral economic considerations play an important role in shaping the labor supply of ex-inmates, once they have decided to abandon crime on moral grounds, because of the deterrent effect of prison or for long-run utility considerations. We can assume that, among people imprisoned for economic crime (60% in Hungary, according to CSO 2008: 52), a permanent shift away from delinquency strongly affects labor market behavior. In cases where crime generates income without consuming a significant amount of time, the effect of 'reform' is analogous to the impact of a reduction in non-wage income. If crime is time consuming and has diminishing returns, it can be thought of as a kind of 'household production', extended to the point where its marginal product equals the market wage. (See Grogger, 1998 for this line of reasoning, with reference to the seminal model of Gronau, 1977.) The implications of 'reform' are similar in a standard job-search framework: a fall in non-wage income is conducive to lower reservation wages and a higher probability of accepting a given wage offer. Discrimination furthermore reduces the job offer arrival rate, makes extended job search less attractive, and drags the mean of accepted wage offers downward.

Falling reservation wages imply further observable outcomes. If the labor supply of released prisoners increases while firms are reluctant to hire them, we should observe a rise in search intensity among the non-employed ex-inmates. The data on registration at labor offices and on the receipt of public transfers support that ex-inmates tend to invest increased effort in finding a legitimate source of livelihood.

The research behind this paper was motivated by Hungary's growing incarceration rate, which is among the highest in Europe (bar the ex-Soviet states), with 180 prisoners per 100,000 inhabitants in 2013. The growing rigor of sentencing and the introduction of a 'Three Strikes Law' in 2010 leave no doubt that the risk of incarceration will increase further in the foreseeable future.[3] Despite the growing importance of the problem, we know virtually nothing, in quantitative terms, about the labor market experience of inmates. Only a few interview-based studies (Tóth 2005; Csáki et al. 2007; Csáki and Mészáros 2011) report quantitative results on small samples comprising a few dozen ex-convicts. The findings also add to the thin European literature on the aftermaths of incarceration. The effects of prison service have been extensively studied in the US, which has the largest per capita prison population in the world (over 700 per 100,000 inhabitants), but very infrequently in Europe and elsewhere. Nagin and Waldfogel (1995) on Britain, Skardhamar and Telle (2009) on Norway, and Drago et al. (2009) on Italy are three of the few European exceptions.

2 Data

The data set collects information from registers of the Pension Directorate, the Health Insurance Fund, the Treasury, and the Public Employment Service. The original sample was created using a 50% random draw from Hungary's resident population aged 16–74 in 2002. Each person in the sample is followed from January 2002 until December 2008 (or exit from the social security registers for reasons of death or out-migration). The data show the labor market and prison status of about 4 million individuals on the 15th day of each month, their days in work during the month, and their income from employment and/or self-employment. We also have data on registration at a labor office and the receipt of such transfers as unemployment insurance benefit (UI), unemployment assistance (UA), old-age pension, disability payment, childcare allowance, and social benefits. The data on transfers are available from 2004 onwards.

Those *incarcerated* can be identified because the state pays their social security contributions to the Health Insurance Fund. In our period of observation, on average 73% of those incarcerated were in prison, 25% were in pre-trial custody (also typically prison), and 2% were in some other type of detention (calculated using data in CSO, 2013). In the data that we have at our disposal, we cannot distinguish between the three groups. However, we can check how the implications of short spells behind bars differ from the consequences of longer sentences, i.e., after excluding cases of pre-trial detention without a subsequent prison term.

Our subjects are regarded as *employed* if their employer or own business reported non-zero personal income to the Pension Fund in a given month. Alternatively, we use an employment dummy (set to 1 if the person was employed on the 15th day of the month) and the number of days in work during the month. The *wage* figure is pre-tax and relates to monthly earnings from all jobs and businesses attended/operated by the person observed. Daily earnings are calculated using information on monthly income and days in work. The file at our disposal records the incidence but not the amount of transfers.

Even if they work, prisoners are treated as a special category, distinct from other wage earners. Working prisoners are paid one-third of the previous year's minimum wage, an amount less than one-ninth of the average wage. We treat prisoners as non-employed, earning zero wages.

We know the age, gender and occupation of the individuals, and also have information on the type of the employment relationship: employees versus public employees and civil servants, self-employed and two other categories of marginal importance.

The starting sample covers 30,835 people aged 16–74 in 2002 and incarcerated at least once in 2002–08. We exclude people who died before December 2008, inmates in prison throughout the observed period, and persons with incomplete payment records, indicating the start date but not the end date of incarceration. These restrictions reduce the sample size from the original 30,835 to 26,877.

Four distinct groups can be identified in the seven-year window through which we observe the incarceration, time in detention, and release of prisoners (Table 1). About one-third of the spells in prison are censored from left or right, and slightly less than half of them are uncensored (i.e., started after January 2002 and ended before December 2008). The completed duration of incarceration in the case of uncensored spells was 13.4 months on average. About a fifth of those incarcerated at least once had further spells in prison after being released on completion of their first term. [4]

Since our data follow the stock of people aged 16–74 in 2002, it excludes more and more young prisoners as we move towards 2008. In our benchmark specification, we

Table 1 Number of observations by spells in prison in the cleaned sample (2002–08)

	Persons	Person-months
Single left-censored spell in prison	6272	521,164
Single right-censored spell in prison	2061	169,965
Single uncensored spell in prison	13,681	947,642
More than one prison spell in the observed period (recidivists)	4863	408,009
Total incarcerated at least once in 2002–08	26,877	2,046,780

restrict the estimations to people older than 22 and include younger offenders in the stage of robustness checks. Statistics on the estimation sample are presented in Appendix.

Our data have several advantages and disadvantages compared to the administrative panels used in the US, which are typically created by merging data from the prison administration and the state-level UI registers. Compared to the US databases, our data tell us very little about the personal characteristics of the offenders. At the same time, the Hungarian data provide a more detailed view of labor market careers, as they contain information on occupations, employment relationships, transfer receipt and registration at a labor office. While the American state-level UI registers exclude public-sector employees, the self-employed and people who work in other states (Holzer 2007), the Hungarian Pension Directorate's register records all legitimate employment spells, irrespective of sector, ownership, and region.

Alongside a host of apparent disadvantages, the administrative registers have certain advantages over survey data. Former inmates are difficult to find, hard to persuade to participate in a survey, and their answers can be biased. Even more importantly, in an interview-based survey, it is practically impossible to reconstruct the offenders' employment and wage paths with acceptable precision. Lack of information on informal employment and earnings is the major drawback of administrative data. We shall discuss the implications this has for this paper in Section 8.

3 Estimation methods

3.1 Exploiting variations in the timing of incarceration

In our benchmark model, we follow LaLonde and Cho (2008) in comparing the post-prison records of released prisoners to the pre-prison records of similar prisoners incarcerated later. The estimated model is a random-effects panel regression,

$$y_{it} = \alpha X_{it} + \delta Z_i + \sum_{q=-4}^{28} \beta_q Q_q + \sum_{t=2}^{84} \gamma_t T_t + u_{it}, \tag{1}$$

where y_{it} stands for the outcome variable (days in work, daily earnings) for person i in month t, X_{it} and Z_{i} are time-variant and time-invariant covariates, T_t is a dummy variable equal to 1 in month t and 0 otherwise, while u_{it} is a residual term. The Q_q dummies denote relative time–set to 1 if person i is q quarters away from any quarters spent in prison, either partly or entirely. Thus for any quarter containing time in prison, q is set to 0. $Q_0 = 1$ covers the quarters of incarceration, prison and release; $Q_1 = 1$ stands for the first full quarter after release; and so on.

The β_q coefficients measure the effect of relative time on the outcome variable, conditional on the X and Z characteristics and keeping calendar time constant. The reference period is all quarters preceding the quarter of imprisonment by more than a year. A coefficient of $\beta_6 = -0.1$, for instance, would suggest that the outcome variable is estimated to be 10 percentage points lower for those in their sixth full quarter after release than for offenders more than one year before their incarceration.[5]

Several caveats apply to the overlapping cohorts model of equation (1).

First, the strong correlation between calendar time and relative time means there is a risk of invalid results about individual predictors, including relative time effects. This is particularly the case on the margins of the observation window: the 25th post-prison quarter, for instance, can only occur in 2008, and the same problem arises when a pre-prison quarter substantially precedes the date of incarceration. In order to reduce the risks arising from multicollinearity, one should either widen the brackets of relative time or narrow the time window to a range where there is sufficient variation of calendar time at a given level of relative time. Our experiments suggest that this range is located at $|q| < 11$, where the correlation between calendar months and relative quarters is 0.32, as opposed to 0.63 in the whole sample, and 0.91 if $|q| \geq 11$.[6] To check whether multicollinearity is a problem, we repeat the estimation after narrowing the time window to 10 quarters before and after time in prison.

Second, this specification cannot deal with offenders who served several prison sentences in the period observed. Recidivists q quarters after a prison spell can also be $q + x$ quarters after another prison spell, and several quarters before their next spell, and so we cannot define a relative time variable for them in the way we do for non-recidivists. Therefore, we estimate equation (1) for offenders with a single prison spell and include recidivists in another model introduced in Section 4.4.

Third, it should be taken into consideration that the relative quarter dummies relate to populations of different size. We have plenty of observations at the level of y_{it} one quarter before or after prison, but the data for the -27th or 27th quarters come exclusively from cohorts incarcerated in 2008 Q3 or released in 2002 Q2, respectively. Therefore, it is important to assess how the time path of the overall average (constructed from the means of y_{it} across all cohorts observed in t) relates to the time paths followed by the individual cohorts.[7]

Last but not least, estimating a random-effects equation is tantamount to assuming that the residual is uncorrelated with unobserved individual attributes affecting the outcome. With only a few control variables to hand, the risk of such correlation is high, and there is a strong case for estimating fixed-effects models.

3.2 Fixed-effects estimates

Estimating equation (1) by simply allowing for individual fixed effects is infeasible, because calendar time and relative time are very strongly correlated within personal histories. (If we had relative month dummies instead of quarter dummies, the correlation would be perfect by construction for persons serving only one month in prison.) Therefore, we set fairly wide brackets for relative time: 1–8 quarters and more than eight quarters before/after the prison quarters. In equation (2), these periods are labelled 'shortly before', 'long before', 'shortly after' and 'long after'. The residual u_{it} is decomposed to an individual fixed effect (c_i) and a residual term (ε_{it}) assumed to be uncorrelated with the regressors.

$$y_{it} = \alpha X_{it} + \delta Z_i + \beta_1 \cdot shortly\_before_{it} + \beta_2 \cdot prison_{it} + \beta_3 \cdot shortly\_after_{it} + \beta_4 \cdot long\_after_{it} + \sum_{t=2}^{7} \gamma_t T_t + c_i + \varepsilon_{it} \tag{2}$$

The model compares post-prison outcomes to the same person's pre-prison outcomes: a coefficient of $\beta_3 = -0.1$, for instance, would mean that the outcome variable is 10 percentage points lower in the first eight quarters after release than it was more than eight quarters prior to incarceration.

3.3 Taking care of unobserved recidivism

In the case of offenders incarcerated *before* the period of observation, 'pre-prison' employment and wages bear the effect of previous prison experience–and this leads to an underestimation of the negative consequences of incarceration (Holzer 2007). Due to a lack of information on the previous criminal records of offenders, one has to fall back on second-best solutions, such as starting the analysis later than the start date of the observed period, as LaLonde and Cho (2008) do. Therefore we repeat the fixed-effects estimations for offenders incarcerated in 2005–08. In choosing this sub-sample, we utilize the information available on recidivism: 80% of those who returned to prison within our seven-year window of observation did so within three years, and more than 95% returned within five years. Therefore, we can be confident that the great majority of those incarcerated in 2005–08 had not been in prison before 2002.

3.4 Estimates for recidivists

As discussed previously, we expect differences in the post-release behavior of one-time offenders and of recidivists. To assess how this difference affects labor market outcomes, we estimate equation (3).

$$y_{it} = \alpha X_{it} + \delta Z_i + \beta_0 \cdot prison_{it} + \beta_1 \cdot between_{it} + \beta_2 \cdot after_{it} + \sum_{t=2}^{84} \gamma_t T_t + c_i + u_{it} \tag{3}$$

The model is estimated separately for recidivists and for offenders with a single prison term in the period observed. In the equation, *prison* stands for the months in custody, *between* denotes months between two prison spells, and *after* indicates months after the last observed prison sentence. The reference category is the period before incarceration. The *between* dummy is obviously omitted for non-recidivists. The equations are estimated with fixed effects.[8]

We anticipate that the β_2 coefficients will be similar for the two groups under examination. For recidivists, the β_1 coefficients are expected to indicate a significant negative impact on employment and a weak effect on wages, as they capture the outcomes in a period when the stigma effect is present but the 'reform effect' is absent.

4 Results

Raw data on the *paths of employment and wages* and estimates of the β coefficients of equation (1) are shown in the six panels of Fig. 1. We observe a gradual erosion of employment as offenders approach the quarter of incarceration. This process, which has been observed in all studies using similar data, is explained by several reasons. First,

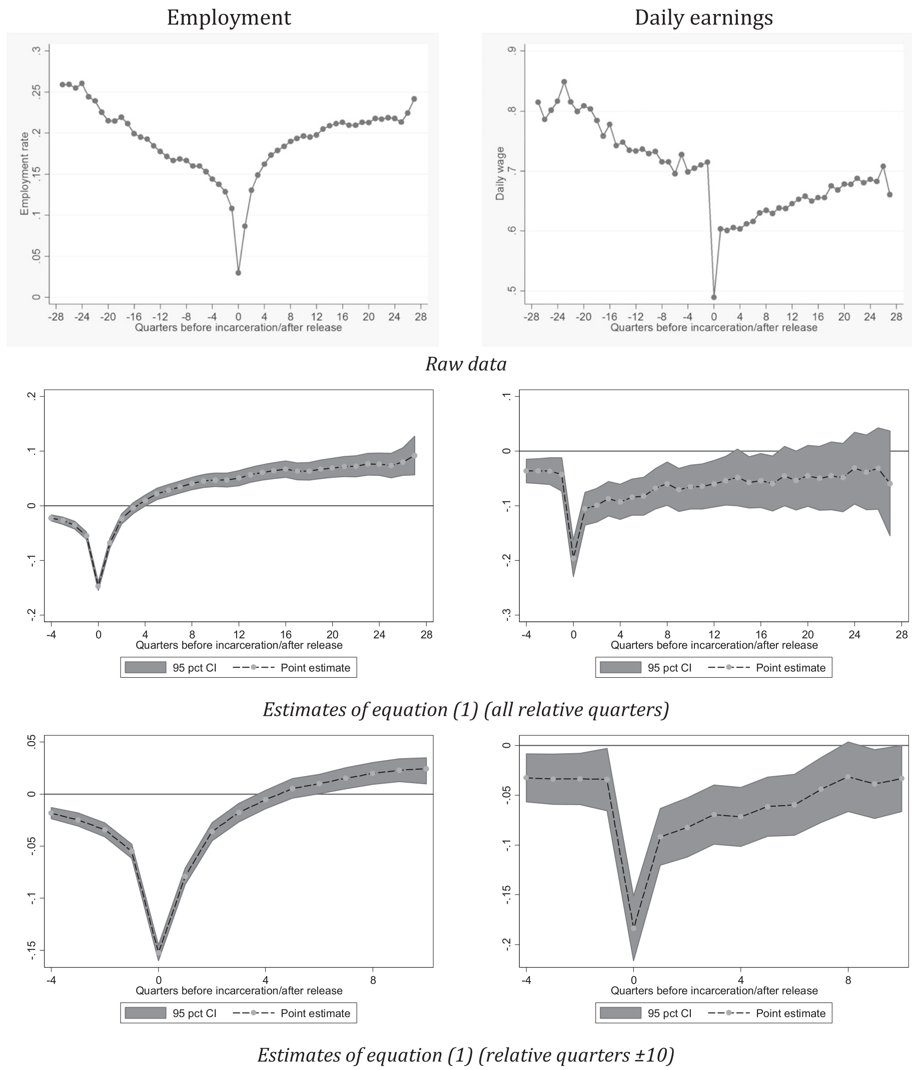

Fig. 1 Employment and wages in quarters before/after incarceration. Sample: offenders with a single prison spell in 2002–08, age >22. Quarter 0 stands for all quarters that included prison. Reference: five or more quarters before the first prison quarter. The shaded areas indicate the 95% confidence intervals. Standard errors are adjusted for clustering by persons

and most importantly, the share of persons actively engaged in criminal activity (rather than paid work) rises as we move toward the date of incarceration. Second, many offenders lose their jobs in the period of investigation and trial–especially as a trial takes place in the region where the crime was committed, often far from the offender's permanent place of abode and work. Third, many employers will sack a worker if they are informed of the worker's involvement in the judicial process. Workers who try to hide this might be dismissed for unexplained absenteeism, while others quit voluntarily in order to keep their involvement secret.

We also observe attrition in daily earnings, from about 80 to 70% of the national average. Deductions for 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 trials than are street-corner dealers and thieves, and they face a greater risk of being fired if their employer hears of their involvement in crime. Therefore, the sample of future prisoners is gradually biased toward unskilled workers as the time of incarceration approaches.

The path of employment is nearly symmetrical: employment starts to grow from virtually zero to 18% by the end of the first post-prison year and exceeds 20% from the second year onwards–levels that correspond to the pre-prison levels at a similar distance from the spell in prison. This is not the case with daily wages, which fall substantially and stay below their pre-prison level throughout the period under observation.

The estimates of equation (1) reinforce the impressions gained from the raw data. In the first year after release, employment falls short of the reference value. It then starts to grow and significantly exceeds the reference level from the fifth full post-prison quarter onwards. By contrast, wages drop significantly below the reference level for at least 12 quarters, as suggested by the 95% confidence intervals shown in the middle panels of Fig. 1.[9] The conclusions remain if we narrow the window of observation to $|q| < 11$ (bottom panels).

The *fixed-effects estimates* (Equation 2) are summarized in Table 2, while the detailed results, including test statistics, are presented in Appendix. The tests of random versus fixed-effects models suggest that the latter are preferable to the former. The Hausman tests resulted in high positive chi-squared values but were based on covariance matrices that failed to meet the requirement of being positive semidefinite. Therefore, we apply instead the Sargan–Hansen over-identification test proposed in Schaffer and Stillman (2010).

The estimates reinforce the impression that the path of employment is symmetrical: 1–8 quarters after release, the employment rate stands where it stood 1–8 quarters prior to incarceration, and in the longer run it returns to the level observed more than two years before imprisonment. By contrast, daily earnings are estimated to fall by 8–10 percentage points relative to the immediate pre-prison period and to stay at that level later.

Table 2 Fixed-effects estimates of changes in employment and daily earnings[a]

	Employment[b]		Daily earnings uncontrolled[c]		Daily earnings controlled[d]	
	Entire sample	Incarcerated after 2004	Entire sample	Incarcerated after 2004	Entire sample	Incarcerated after 2004
Quarters −8 to −1	−0.047***	−0.037***	−0.044***	−0.037***	−0.042***	−0.035***
	(13.6)	(9.5)	(3.7)	(2.8)	(3.6)	(2.7)
Prison quarters	−0.182***	−0.177***	−0.238***	−0.263***	−0.230***	−0.254***
	(44.1)	(32.8)	(10.9)	(9.5)	(10.8)	(9.4)
Quarters 1 to 8	−0.046***	−0.034***	−0.134***	−0.129***	−0.122***	−0.118***
	(8.3)	(5.3)	(6.4)	(5.0)	(6.0)	(4.7)
Quarters 9 to 28	−0.005	0.004	−0.125***	−0.114***	−0.113***	−0.105***
	(0.8)	(0.5)	(5.0)	(3.5)	(4.6)	(3.3)
Reference values:						
Mean	17.7	17.9	73.6	73.6	73.6	73.6
Standard dev.	(37.8)	(37.9)	(61.1)	(61.0)	(61.1)	(61.0)

Significant at the *0.1, **0.05, ***0.01 level; t-values in parentheses. Standard errors are adjusted for clustering by persons
The data relate to inmates serving a single prison spell in 2002–08 or 2005–08
[a]Reference period for relative time: quarters −28 to −9
[b]Days in work during the month. Controlled for age, age squared and calendar months
[c]Controlled for age, age squared and calendar months
[d]Also controlled for occupation and type of employment relationship
For further test statistics, see Appendix

In the longer run (9–28 quarters after release) ex-inmates still earn 11–12 percentage points less than they did two years (or more) prior to incarceration.

The estimates for the whole sample and for those imprisoned after 2004 are similar. The results for wages controlled for occupation and type of employment relationship differ only slightly, suggesting that the bulk of the wage loss comes from wage disadvantages within broad occupational categories.

Finally, we turn to the comparison of recidivists and non-recidivists. Two remarks are in order before we look at the estimates. First, since the evaluation periods (before, between, after) are wide enough, we can continue to estimate fixed-effects models without worrying that calendar time and relative time are very closely correlated. The specification tests suggest that the fixed-effects models are preferable to the random-effects ones. Second, in this model we want to test how accepted wage offers differ, depending on whether the convicts continue to commit crimes or at least some of them stop. Lower reservation wages may manifest themselves in lower accepted wages within occupations and/or increased propensity to accept job offers in low-wage occupations and sectors. Since we are interested in the outcome rather than in how it comes about, we do not control for job characteristics in the wage equations.

The results, summarized in Table 3, are consistent with expectations. The employment rate of recidivists falls significantly between two prison spells–by 3.4 percentage points, equivalent to a 40% decline relative to the pre-prison level. At the same time, we observe a minor and statistically insignificant fall in earnings between two prison spells. By contrast, when the recidivists are released from their last observed prison spell, and at least some of them withdraw from crime, their estimates do not differ from those of non-recidivists: the relative wages of both groups fall by 9–10 percentage points, and their employment rates do not differ (or do so only slightly) from their pre-prison reference values.

The scope for checking the robustness of the results is limited by the availability of alternatives to the data used in the preceding section. Table 4 collects estimates of the employment and wage effects for different sample designs. In all cases, we estimate fixed-effects equations with dummies for the prison and post-prison periods, treating pre-prison months as a reference and controlling for calendar months. The table

Table 3 Estimates for convicts with one versus several prison spells in 2002–08. Fixed-effects estimates of equation (3)

	Employment	Daily earnings
Recidivists between two prison spells	−0.034***	−0.048
	(8.64)	(1.41)
Recidivists after their last prison spell	−0.000	−0.096**
	(0.10)	(2.02)
Non-recidivists after their single prison spell	−0.009**	−0.089***
	(2.47)	(5.00)
Reference values for recidivists (pre-prison period):		
Mean (%)	8.0	66.1
Standard deviation	(26.6)	(46.3)

Significant at the *0.1, **0.05, ***0.01 level; t-values in parentheses. Standard errors are adjusted for clustering by persons
The coefficients show the level of employment/wages in the indicated periods relative to the pre-prison period
For further test statistics, see Appendix

Table 4 Fixed-effects estimates for selected sub-samples of non-recidivists

Sample variants	Employment	Daily earnings
Both genders[a]	−0.004	−0.080***
	(1.06)	(5.55)
Only men[a]	−0.004	−0.086***
	(1.04)	(4.83)
Persons aged 16–22 inclusive	0.002	−0.067***
	(0.44)	(4.29)
Only persons older than 22 in 2002	−0.004	−0.077***
	(0.91)	(4.35)
Aged 22–35	0.003	−0.067***
	(0.51)	(2.96)
Older than 35	−0.008	−0.134***
	(1.59)	(4.51)
Inmates serving 1–3 months in prison[a]	−0.012*	−0.080**
	(1.76)	(2.21)
Inmates serving 1–6 months in prison[a]	−0.012**	−0.086***
	(2.13)	(2.68)
Inmates serving more than 3 months in prison[a]	−0.008	−0.078***
	(1.56)	(3.17)
Inmates serving more than 6 months in prison[a]	−0.007	−0.064***
	(1.10)	(2.89)

The coefficients relate post-prison employment and daily earnings to their pre-prison values; t-values in brackets. Significant at the *0.1, **0.05 and ***0.01 level. Standard errors are adjusted for clustering by persons
[a]The estimates relate to persons older than 22

presents the coefficients for the post-prison dummy. (The full results are available on request.)

Unsurprisingly, the results for men and for the entire sample are practically identical. Including young offenders or restricting the sample to persons older than 22 in 2002 makes no difference to the estimated employment effects, but the wage effect is slightly weaker if we include young people. This is consistent with the observation that older ex-convicts lose significantly more in terms of wages than do their younger counterparts: −13.4 percentage points compared to −6.7 percentage points.

The length of incarceration has a weak impact: inmates serving a short spell lose marginally more in terms of employment and less in terms of wages. The similarity of the results for short and long spells is all the more important, as these contain different mixtures by type of custody: the share of inmates in pre-trial detention is presumably high in the case of short, completed spells, whereas those people serving more than 3 months must be predominantly convicted prisoners.

5 Job search and alternative sources of income

The majority of released prisoners stop committing crimes and look for some alternative livelihood. About 80% of those released in 2002–03 did not return to prison before December 2008–i.e., they spent 6–7 years without being incarcerated again. (The ratios are probably similar for people released later, but our time window is too narrow to observe them in the long run.) These people are unlikely to be imprisoned again after surviving such a long time at liberty, and many of them want to hold down a job. Despite that, the employment rate of the ex-prisoners does not exceed its pre-prison level. Therefore, we expect the average ex-prisoner to make increased efforts to seek alternative sources of income and assistance in their job search.

Table 5 shows logistic panel regression estimates of the probabilities of (i) registration at the labor office, (ii) receipt of UI or UA, and (iii) receipt of other transfers, conditional on being non-employed. The data relate to 2004–08. The models have been estimated with random effects because prior employment experience and gender are strong individual-specific predictors, which would drop out of a fixed-effects model.

Table 5 Registration at a labor office and transfer receipt – odds ratios. Random-effects panel logit estimates for months out of employment in 2004–08

Dependent variable:	Registration at a labor office	UI or UA	Other transfers[a]
0–8 quarters before incarceration	0.97	0.90	0.71**
	(0.4)	(1.0)	(2.0)
Prison quarters	0.16***	0.13***	0.12**
	(15.4)	(11.4)	(9.8)
0–8 quarters after release	1.96***	1.66***	0.32***
	(5.4)	(2.6)	(4.8)
9–28 quarters after release	2.14***	2.91***	0.61*
	(5.7)	(5.3)	(1.8)
Male	0.95	1.23**	0.16***
	(0.6)	(2.6)	(3.7)
Age	1.20***	1.62***	0.70***
	(5.2)	(9.6)	(4.9)
Age squared	0.99***	0.99***	101***
	(6.8)	(10.2)	(5.8)
Employed prior to incarceration[b]	3.35***	6.24***	0.34***
	(12.0)	(11.0)	(5.0)
2005	1.14***	1.21***	1.53***
	(4.1)	(4.18)	(6.5)
2006	1.18***	1.75***	2.12***
	(3.6)	(8.9)	(6.5)
2007	1.36***	3.31***	1.88***
	(5.5)	(15.7)	(4.0)
2008	1.70***	4.26***	1.63**
	(8.5)	(17.4)	(2.4)
Number of person-months	981,229	981,229	981,229
Number of persons	20,476	20,476	20,476
Wald chi-squared	25373.8	23157.6	13059.3
	(0.000)	(0.000)	(0.000)
Rho (standard error)	0.83 (0.003)	0.89 (0.003)	0.98 (0.003)
Mean of the dependent variable (%)	14.7	8.4	10.8

Z-values in brackets. Significant at the *0.1, **0.05, ***0.01 level. Standard errors adjusted for clustering by persons. The estimates relate to 2004–08 and cover inmates with a single spell in prison. Reference categories: female, non-employed throughout the period prior to incarceration, 9th to 19th quarters prior to incarceration, 2004

[a]Pension, disability pension, childcare allowance, social benefits, compensation for disabled family members, compensation for widows and orphans

[b]Employed at least once

We included calendar year dummies to control for the rising trend in registration and benefits receipt in 2004–08. The table shows odds ratios and z-values adjusted for the clustering of observations by persons.

The results suggest a nearly twofold increase in the odds of being registered in the first two years after release, and a further rise to 2.14 later. Similarly, the odds of receiving either UI or UA jump to 1.66 in the immediate post-prison period and to 2.91 in the longer run. By contrast, the chances of receiving other transfers, less closely related to unemployed status, fall significantly even before incarceration and stay at a low level throughout the post-prison period.

In order to see how the magnitudes compare to those in the general population, we compare the data of offenders to similar data for unskilled males observed in the Labor Force Survey (LFS). In 2005, 17.2% of non-employed ex-prisoners were registered, which had grown to 20.3% by 2008. These rates are somewhat lower than those measured among unskilled prime-age men in the general population: 21.3% in 2005 and 25.9% in 2008.[10]

Benefits and other transfers raised the proportion of ex-inmates earning some legitimate income (wage or benefit) to 35.2% in 2005 and 42.5% in 2008. A comparison with LFS data is possible after excluding transfers other than UI, UA, pensions, and childcare benefits. The rates of transfer receipt defined in this way (31% in 2005 and 37.6% in 2008) are much lower than those measured in the comparable general population, in which 85% earned a wage or received a transfer in 2005, and 91.3% in 2008. Of the non-employed, 68% had income from transfers in 2005 (73.8% in 2008), as opposed to only 15% of ex-inmates.

6 A note on post-prison jobs

The difficulties of re-employment partly stem from the rigor of Hungarian clean-sheet regulations, which exclude ex-convicts from public-sector jobs and leading positions in micro-firms and self-proprietorships for a minimum of three years.[11] As the upper right panel of Fig. 2 shows, the 7% pre-prison share of public-sector jobs falls to 2%, and the share of self-employment drops from a range of 10–15% to only 2–3%, in the first three post-prison years. (The non-zero rates after release are probably explained by cases when the arrestee was set free from pre-trial detention without being sentenced to prison.) These figures compare to an 11% share of public-sector jobs and a 7.6% share of self-employment among unskilled prime-age males (14.6% and 13.6% among prime-age males with vocational qualifications), according to the 2005 Q1 wave of the Labor Force Survey. The share of registered casual workers within total legitimate employment increases substantially, but is of marginal importance (about 2% share) even at its peak.[12]

7 Discussion and implications for policy

The most important piece of information we have learned from the data is the low level of legitimate employment of Hungarian prisoners both before and after incarceration. In the period under investigation, their legitimate employment rate amounted to 17% on average (when out of custody), a level substantially lower than that observed in the US literature: Pettit and Lyons (2009) report that the ex-inmates studied by them spent 26% of their time in UI-covered jobs; LaLonde and Cho (2008) find a 30% post-prison

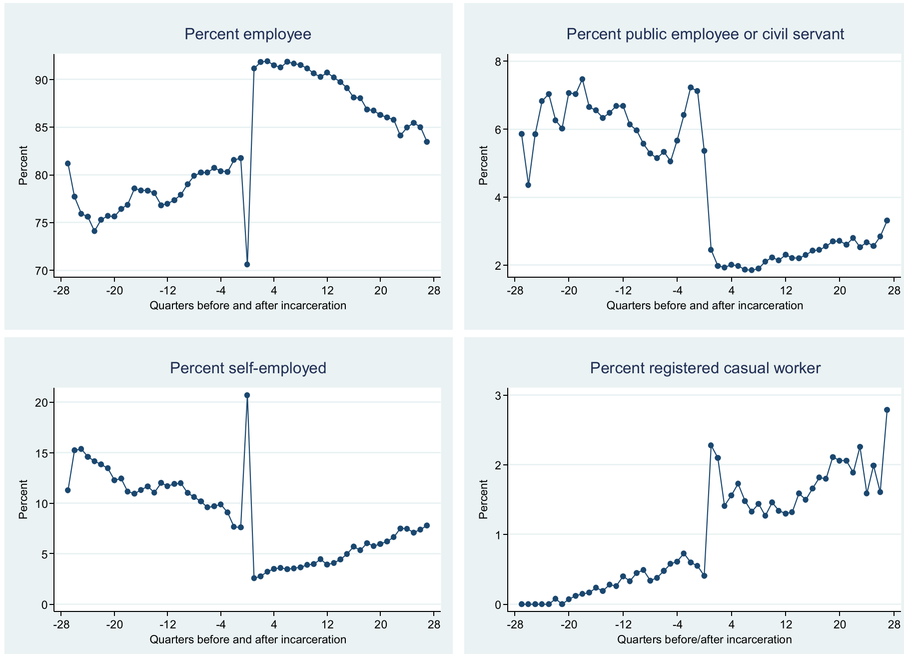

Fig. 2 Jobs before and after incarceration. (percent, all employed persons = 100). Notes. People with unknown (0.7%) or unspecified (1.1%) types of contract and those with several types of contracts (1.4%) are included in total employment but not shown separately. While employees are covered by the Labor Code, public employees (*közalkalmazott*) and public servants (*köztisztviselő*) are subject to special regulations. The self-employed are owners of own business, members of business partnerships and assisting family members. Registered casual workers are employed with 'casual work booklet' allowing simplified administration and reduced contribution payment for a limited period

legitimate employment rate among economically disadvantaged and previously imprisoned *mothers* in the US; Geller et al. (2006) studied predominantly young males and reported a 41% level of legitimate employment in the first year after release. Our estimate, by contrast, is 14.4% for the same period. Skardhamar and Telle (2009) report a 25% legitimate employment rate immediately after release in Norway. This compares to 10% for non-recidivists and 2% for recidivists in Hungary, in the first full quarter after release.[13]

The employment gap would predictably be narrower but still rather wide if we took informal employment into consideration. In a small-sample survey, Csáki and Mészáros (2011) find that about 40% of the jobs held by future convicts are unregistered in Hungary.[14] Taking this information at face value, we can estimate the offenders' total employment rate at about 30%. Supposing that half of the jobs held by ex-offenders are informal, we still end up with a rate short of 40%. For the sake of comparison, one might refer to studies by Grogger (1995; 1998), which incorporate off-book employment using data from the National Longitudinal Survey of Youth (US). The 1995 paper observed a 54% total employment rate, while the later paper reported that only 5.5% of young men with criminal income in 1979 were non-employed throughout the year, and the average person spent 1500 h in work during the year. In our sample, 45% had no legitimate job at all in a period of seven years. The average person spent a maximum of 488 h in legitimate work annually (calculated as days in work, multiplied by eight hours a day). Even if we assume equal shares of formal and informal jobs, the non-employment ratio would decrease to only 20–25%, and estimated hours would continue to fall short of 1000 h per annum.

Turning to the results on how incarceration affects labor market outcomes, we found that the employment rate of the Hungarian ex-prisoners rises from near zero levels at the moment of release to a range of 20–25%. This result (i.e., the recovery of legitimate employment rate in the longer run) compares favorably to most of the findings based on US administrative data. Waldfogel (1994) Grogger (1995) and Geller et al. (2006) identify employment losses between 3 and 9%, depending on the samples under scrutiny and model specifications. While Pettit and Lyons (2009) and Lalonde and Cho (2008) find an initial rise in the employment rate, this is followed by a process of attrition, which drives the employment rate back to, or below, its pre-prison level. Similarly, the data of Jung (2011) suggests that an initial rise of the employment rate is followed by a decline: employment is lower by 8 percentage points 9 to 33 quarters after release than it was 9 to 18 quarters before incarceration.

Data on total (formal plus informal) employment would most probably indicate zero or even a positive prison effect for non-recidivists, and would not change our qualitative conclusions. The share of informal employment within total employment is most probably higher after incarceration than before, given the administrative constraints on entering formal employment without a clean record and the existence of statistical discrimination (for which we have ample anecdotal evidence).

The employment rate's tendency to return to its modest pre-incarceration level might give the impression that prisoners face similarly hard constraints to reintegration before and after prison. But this impression is patently false. The post-release labor market experience of the prison population is strongly affected by the behavior of those convicts who give up criminal activity as a source of livelihood and are ready to accept low-paid jobs. Consistent with this expectation, we find that the earnings of released prisoners fall substantially, and fall more for those who do not return to prison. The results for recidivists confirm that, in the absence of a 'reform' effect, employment falls and wages do not decline as much as they do for last-case offenders. Furthermore, we observe signs of increased effort to find a legitimate source of livelihood on the part of the latter group.[15]

At least three observations suggest that the existing practices and regulations do not help the ex-convicts effectively.

First, in contrast to the findings of several papers in the US that show the highest post-prison employment rates immediately after release, we find that less than 5% of Hungarians leaving prison secure a job in the first full month after release, and 10% have a job in the first full quarter after release. This pattern casts doubt on whether post-release supervision and assistance are effective.

Second, the unconditional exclusion of ex-convicts from public sector jobs is a questionable policy, implying that people convicted of stealing a few bottles of perfume or of evading taxes cannot be hired as cleaners, janitors, or drivers in public institutions.

Third, the 'clean sheets' issued by the authorities are rather laconic: they simply inform a would-be employer of the absence or presence of a criminal record, without indicating the type of crime committed or the severity of the punishment. Under these regulations, employers may find themselves basing their hiring decisions on a very poor set of information, and this understandably makes them cautious. Arguably, employer access to the criminal records database could weaken, rather than strengthen, statistical discrimination.

Endnotes

[1]In one of their model variants, Pettit and Lyons (2009) match inmates and non-inmates using a particularly rich set of administrative data that provides information on criminal records, pre-prison careers, and even General Educational Development (GED) test scores. Even so, they report significant differences in the two groups' labor market experience prior to the inmates' incarceration, hinting at permanent productivity differentials.

[2]An often-cited exception is Kling (2006), who uses information on judges' usual stiffness of sentencing as an instrument to gauge the convict's sentence. That paper finds that the length of the prison term has no impact on employment and wages.

[3]In Europe in 2010, according to Walmsley (2010), the prison population per 100,000 inhabitants varied from 59 in Finland to 164 in Serbia–excluding two outliers (206 in Jersey and 227 in Montenegro) and the former Soviet republics (254 in Estonia, 276 in Lithuania and 314 in Latvia). In Hungary, it declined from more than 200 in 1987 to less than 120 in 1990, due to amnesties in 1988, 1989 and 1990. Since then, the incarceration rate has been following a rising trend, and it reached 180 per 100,000 inhabitants in 2013 (see CSO 2013 for 1990–2013 and Eurostat 2014 for 1987–89.) The capacity of prisons has not grown proportionally. According to a Helsinki Watch report (Helsinki, 2013) capacity utilization grew from an already high rate of 118 persons per 100 places in 2008 to 140 in 2012. In 2011, prison overcrowding was the second worst in Europe, after Italy (Csóti, 2011).

[4]We have significantly more left-censored than right-censored spells for two reasons. First, the left-censored spells include long sentences, while the right-censored spells cannot be longer than 83 months (given our 84-month window of observation). Second, many of the left-censored spells may belong to recidivists, while those serving more than one prison spell in 2002–08 are treated separately from those serving only one term.

[5]The employment equations in the paper are controlled for gender, age, and age squared. The wage equations are controlled for gender, age, age squared, occupational status, type of employment relationship, and a dummy for employment spells lasting for only one day. These spells account for only 0.8% of the total, but are associated with exceedingly high earnings. This phenomenon is most probably explained by regulations in some branches of the public sector, which do not allow payments for contract work unless the subcontractor is formally 'hired' as an employee.

[6]We also broaden the brackets for relative time by using quarter rather than month dummies, similar to LaLonde and Cho, who apply relative time dummies for six-month periods in their quarterly panel.

[7]In Czafit and Köllő (2014) we show that, apart from a negligible number of outliers, the cohort-specific levels fall very close to the full-sample means.

[8]In this case we cannot break down the pre- and post-prison periods by distance from the date of incarceration and release, since relative time cannot be clearly defined for recidivists.

[9]Estimates of the control variables of equation (1) are shown in Appendix.

[10]Authors' calculation using the 2005 Q1 and 2008 Q1 waves of the LFS. The data relate to non-employed men aged 22–62 with primary or uncertified vocational education.

[11]Internet forums joined by ex-prisoners often mention cases where clean sheets are also required by private employers, despite the fact that they cannot be sued for 'negligent hiring', as is the case in several US states. See http://www.jogiforum.hu/forum/21/9835, for instance.

[12]Registered casual workers are employed with a so-called 'casual work booklet', which allows for simplified administration and reduced advance social security contribution payments for a limited period of time. At the same time, this form of employment provides legitimate earnings and accrual points.

[13]The unemployment statistics yield similar worrisome results. The registered unemployment rate of ex-prisoners amounted to 42.1% in 2005, the midpoint of our period of observation, more than twice as high as in the comparable general populations: 19.5% among prime-age men with only a primary education and 6.5% among those with a vocational qualification. Authors' calculation using microdata from the 2005 Q1 wave of the LFS.

[14]Estimates for the general population range between 15 and 20% (Benedek et al. 2013: 164–167).

[15]The US results on wage changes are perplexingly mixed. Waldfogel (1994), Grogger (1995), Needles (1996) and Geller et al. (2006) find a two-digit decline in wages, while Pettit and Lyons (2009) identify a much smaller loss. At the same time, Lalonde and Cho (2008) detect a minor gain, and Jung (2011) finds a substantial increase in real earnings when working. Kling (2006) reports no effect of incarceration length.

Appendix

Table 6 Descriptive statistics for the estimation sample (percent except if otherwise indicated)

	Non-recidivists	Recidivists
Male	91.2	94.0
Age in 2002 (year)	36.9 (9.9)	35.5 (8.9)
Number of prison spells in 2002–08	1	2.1 (0.8)
Average duration of completed prison spells, months (standard deviation)	12.8 (13.2)	12.9 (11.9)
Employed at least once in 2002–08	61.6	40.5
Employed on the 15th day of the month 2002–08	18.2	7.4
Days in work/potential days in work 2002–08 (standard deviation)	17.3 (37.3)	6.7 (24.5)
Days in work/potential days in work when employed	95.4	91.9
Employed for one day during the month	0.8	2.5
Average completed duration of employment spells, months (standard deviation)	8.2 (13.7)	5.0 (8.8)
Completed employment spells shorter than:		
- 1 month	23.1	29.3
- 3 months	48.2	58.7
- 6 months	65.1	75.9
- 1 year	81.0	89.1
Occupational status when employed		
- elementary	45.5	54.0
- middling	39.4	34.6

Table 6 Descriptive statistics for the estimation sample (percent except if otherwise indicated) *(Continued)*

- professional	7.7	4.4
- unknown	7.2	7.0
Employment relationship		
- Employee	86.2	89.4
- Public employee, civil servant	3.6	2.4
- Self-employed	6.8	4.2
- Registered casual worker	1.3	2.4
- Other	0.7	0.8
- Mixed (more than one type at a time)	1.4	0.8
Monthly earnings/national average (standard deviation)	63.1 (43.3)	53.9 (32.6)
Daily earnings/national average (standard deviation)	67.1 (58.2)	60.0 (45.1)
Received transfer when non-employed		
- Unemployment insurance benefit	8.9	5.2
- Pension, disability pension	4.1	1.8
- Other transfer	6.4	1.4
- Several transfers at a time	0.9	0.2
- None of the above transfers	79.7	91.5
Registered at a labor office when non-employed	14.5	10.3

Elementary: cleaners, material handlers, porters and guards, other unskilled manual, machine operators, drivers. Professionals: managers, teachers, doctors, other professionals, professional military personnel. Middling: all other occupations

Table 7 Effects of the control variables in equation (1)

	Employment	Daily earnings
Male	0.044***	0.060***
	(7.73)	(4.61)
Age	0.018***	0.013***
	(11.27)	(3.12)
Age squared/100	−0.025***	−0.017***
	(12.92)	(3.02)
Job characteristics		
Elementary	-	−0.061***
		(7.39)
Professional	-	0.102***
		(4.12)
Unknown or missing	-	−0.077**
		(2.45)
Employment relationship		
Public servant, public employee	-	0.305***
		(10.31)
Self-employed	-	−0.126***
		(4.46)
Employed with 'casual work booklet'	-	−0.366***
		(3.95)
Other	-	−0.114*

Table 7 Effects of the control variables in equation (1) *(Continued)*

		(1.96)
Multiple job holder	-	0.242***
		(4.45)
Employed for one day during the month	-	0.836***
		(5.65)
Constant	−0.125	0.460
Number of observations	1,361,558	197,382
Number of persons	19,815	10,746
R-squared within	0.056	0.033
R-squared between	0.003	0.053
R-squared overall	0.045	0.065
Wald chi2 (118)	6224.63	1304.19
	(0.000)	(0.000)

Random-effects panel regression. Reference categories: non-elementary, non-professional job (for job characteristics) and employee (for the employment relationship). Dependent variables: 'employment' stands for days in work during the month, as a fraction of potential days. 'Daily earnings' stand for all recorded income during the month divided by the number of days in work. Normalized for the national average in the given month

Table 8 Coefficient estimates and test statistics for equation (2)

	Employment		Daily earnings (controlled)	
	Random effects	Fixed effects	Random effects	Fixed effects
Estimates for the entire sample				
Quarters −8 to −1	−0.047	−0.047	−0.035	−0.042
	(13.9)	(13.6)	(3.3)	(3.6)
Prison quarters	−0.179	−0.182	−0.212	−0.230
	(41.8)	(44.1)	(12.8)	(10.8)
Quarters 1–8	−0.048	−0.046	−0.105	−0.122
	(8.68)	(8.3)	(7.6)	(6.0)
Quarters 9–28	−0.001	−0.005	−0.090	−0.113
	(0.8)	(0.8)	(5.7)	(4.6)
No. of observations	1,361,558	1,361,558	197,382	197,382
No. of groups	19,815	19,815	10,746	10,746
Observations per group	68.7	68.7	18.4	18.4
R^2 within	0.052	0.052	0.020	0.019
R^2 between	0.001	0.000	0.067	0.040
R^2 overall	0.042	0.033	0.057	0.041
Wald chi2	6017.1	-	1316.9	-
Prob > chi2	(0.0000)	-	(0.0000)	-
F-test	-	66.6	-	10.91
Prob > F	-	(0.0000)	-	(0.0000)
Corr (u_i, Xb)	-	−0.076	-	−0.057
Sargan–Hansen	1022.1 (0.0000)		288.1 (0.0000)	
Estimates for inmates incarcerated in 2005–08				
Quarters −8 to −1	−0.038	−0.037	−0.032	−0.035
	(9.9)	(9.5)	(2.5)	(2.7)
Prison quarters	−0.175	−0.177	−0.229	−0.254

Table 8 Coefficient estimates and test statistics for equation (2) *(Continued)*

	(34.0)	(32.8)	(10.5)	(9.4)
Quarters 1–8	−0.035	−0.034	−0.103	−0.118
	(5.8)	(5.3)	(6.4)	(4.7)
Quarters 9–28	0.002	0.004	−0.087	−0.105
	(0.3)	(0.5)	(4.6)	(3.3)
No. of observations	832,204	832,204	125,409	125,409
No. of groups	12,182	12,182	6557	6557
Observations per group	68.3	68.3	19.1	19.1
R^2 within	0.049	0.050	0.020	0.020
R^2 between	0.002	0.003	0.065	0.038
R^2 overall	0.042	0.041	0.064	0.045
Wald chi2	3547.5	-	887.7	-
Prob > chi2	(0.0000)	-	(0.0000)	-
F-test	-	39.5	-	7.65
Prob > F	-	(0.0000)	-	(0.0000)
Corr (u_i, Xb)	-	−0.070	-	0.012
Sargan–Hansen	2075.7 (0.0000)		241.8 (0.0000)	

Table 9 Estimates for convicts with one versus several prison spells in 2002–08. Sargan_Hansen tests for equation (3)

	Employment	Daily earnings
Recidivists between two prison spells	462.353	125.945
Recidivists after their last prison spell	(0.0000)	(0.0050)
Non-recidivists after their single prison spell	796.131	357.188
	(0.0000)	(0.0000)

Other test statistics: Employment equation for recidivists and non-recidivists: corr(ui, Xb) = −0.165 and −0.081, rho = 0.304 and 0.511. Wage equation: corr(ui, Xb) = −0.392 and −0.148, rho = 0.423 and 0.408

Competing interest

The IZA Journal of European Labor Studies is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles.

Acknowledgements

The authors thank Péter Elek, Gábor Kőrösi and seminar participants in Budapest for helpful comments, and Clive Liddiard for language editing. The data set used in the analysis has been acquired with financial help of Hungary's Social Renewal Operational Programme supported by the European Social Fund (Commission Decision No C(2007)4306).
Responsible editor: Alan Barrett

Received: 3 April 2015 Accepted: 26 August 2015
Published online: 28 October 2015

References

Benedek D, Elek P, Köllő J (2013) Tax avoidance, tax evasion, black and grey employment. In: Fazekas K, Benczúr P, Telegdy Á (eds) The Hungarian Labour Market – Review and Analysis. Centre for Economic and Regional Studies, Hungarian Academy of Sciences & NationalEmployment Non-profit Public Company Ltd, Budapest, pp 161–169

Bound J, Freeman RB (1992) What went wrong? The erosion of relative earnings and employment among young black men in the 1980s. Q J Econ 107(1):201–232

Csáki A, Mészáros M (2011) Fogvatartottak és szabadultak társadalmi és munkaerő-piaci beilleszkedése (The integration of inmates and released prisoners). OFA, Budapest, http://www.valtosav.hu/szakmai_anyagok/be_ki_zarva.pdf

Csáki A, Mészáros M, Sponga I (2007) Fogvatartásból szabadult fiatal felnőttek társadalmi reintegrációjának lehetőségei (Prospects of reintegrating young adults released from prison). Börtönügyi Szemle 26:1

CSO (2008) Az ismertté vált bűncselekmények és elkövetőik Magyarországon (Criminal offences and offenders in Hungary). Központi Statisztikai Hivatal (Central Statistical Office), Miskolc, http://www.ksh.hu/docs/hun/xftp/idoszaki/regiok/orsz/ismertbun.pdf

CSO (2013) Stadat database of the Hungarian Central Statistical Office., http://www.ksh.hu/stadat_eves_2_8
Csóti A (2011) A büntetőjog változásainak hatása a büntetés-végrehajtási szervezetre (Effects of changes in criminal justice on the Prison Service). Börtönügyi Szemle 2:1–12
Czafit B, Köllő J (2014) Labor Market Careers Before and after Incarceration. IZA Discussion Paper No. 8644., Available at SSRN: http://ssrn.com/abstract=2529353
Drago R, Galbiati R, Vertova P (2009) The deterrent effects of prison: Evidence from a natural experiment. J Polit Econ 117(2):257
Eurostat (2014) Prison population: historical data 1987–2000, downloaded 10 October 2014, https://datamarket.com/data/set/1a5k/prison-population-historical-data-1987-2000#!ds=1a5k!6gdo=2.5.h&display=line
Fahey J, Roberts C, Engel L (2006) Employment of ex-offenders: Employer perspectives, Crime and Justice Institute October 31., http://208.109.185.81/files/ex_offenders_employers_12-15-06.pdf accessed January 2, 2015
Freeman RB (1991) Crime and the employment of disadvantaged youth, NBER Working Paper No. 3875
Freeman RB (1996) Why do so many young American men commit crime and what can we do about it? J Econ Perspect 10(1):25–42
Geller A, Garfinkel I, Western B (2006) The effects of incarceration on employment and wages: An analysis of the Fragile Families Survey, Center for Research on Child Wellbeing, Working Paper No. 2006-01-FF, August
Grogger J (1995) The effect of arrests on the employment and earnings of young men. Quarterly Journal of Economics, February, pp 51–71
Grogger J (1998) Market wages and youth crime. J Labor Econ 16(4):756–791
Gronau R (1977) Leisure, home production, and work: The theory of time allocation revisited. J Polit Econ 85:1099–1123
Helsinki (2013) Lehetőség szerint": ember per vaságy ('As far as possible': Men per iron bed)., 3 January 2013, downloaded 20 November 2013, http://helsinkifigyelo.blog.hu/2013/01/03/_lehetoseg_szerint_ember_per_vasagy
Holzer HJ (2007) Collateral costs: The effects of incarceration on the employment and earnings of young workers, IZA Discussion Paper 3118. Institute for the Study of Labor (IZA), Bonn
Holzer HJ, Raphael S, Stoll MA (2003) Employment barriers facing ex-offenders, paper prepared for roundtable on 'The Employment Dimensions of Prisoner Reentry: Understanding the Nexus between Prisoner Reentry and Work'. New York University, New York, 19–20 May 2003, http://www.urban.org/sites/default/files/alfresco/publication-pdfs/410855-Employment-Barriers-Facing-Ex-Offenders.PDF
Jung H (2011) Increase in the length of incarceration and the subsequent labor market outcomes: Evidence from men released from Illinois state prisons. J Policy Anal Manage 30(3):499–533
Kling JR (2006) Incarceration length, employment, and earnings. Am Econ Rev 96(3):863–876
LaLonde RJ, Cho R (2008) The impact of incarceration in state prison on the employment prospects of women. J Quant Criminol 24(3):243–267
Nagin D, Waldfogel J (1995) The effects of criminality and convictions on the labor market status of young British offenders. Int J Law Econ 15:109–126
Needles KE (1996) Go directly to jail and do not collect? A long-term study of recidivism, employment and earnings patterns among prison releases. J Res Crime Delinq 33:471–496
Pettit B, Lyons CJ (2009) Incarceration and the Legitimate Labor Market: Examining Age-Graded Effects on Employment and Wages. Law Soc Rev 43(4):725–756
Raphael S (2007) Early Incarceration Spells and the Transition to Adulthood. In: Danziger S, Furstenberg F, Rouse C (eds) The Price of Independence: The Economics of Early Adulthood. Russell Sage, New York, pp 278–306
Sabol W (2007) Local labor market conditions and post-prison employment experience of offenders released from Ohio prisons. In: Bushway S, Stoll M, Weiman D (eds) Barriers to Reentry? The labor market for released prisoners in post-industrial America. Russell Sage, New York
Schaffer ME, Stillman S (2010) Stata module to calculate tests of overidentifying restrictions after xtreg, xtivreg, xtivreg2 and xthtaylor., http://ideas.repec.org/c/boc/bocode/s456779.html
Skardhamar T, Telle K (2009) Life after prison. The relationship between employment and re-incarceration, Discussion Paper No. 597, October 2009. Statistics Norway, Research Department, Norway
Tóth H (2005) Hungarian Country Report. Center for Policy Studies, Central European University, Budapest, https://cps.ceu.hu/sites/default/files/publications/cps-research-report-mip-final-report-full-version-2005.pdf, accessed January 2, 2015
Tyler JH, Kling JR (2006) Prison-based education and re-entry into the mainstream labor market, NBER Working Paper No. 12114
Waldfogel J (1994) The Effect of Criminal Conviction on Income and the Trust "Reposed in the Workmen", Journal of Human Resources, University of Wisconsin Press, vol. 29(1), pp 62–81.
Walmsley R (2010) World Prison Population List, 3rd edn. Home Office Research Development and Statistics Directorate, London
Western B (2002) The Impact of Incarceration on Wage Mobility and Inequality'. Am Sociol Rev 67(4):526–546
Western B, Beckett K (1999) How unregulated is the U.S. labor market: The penal system as a labor market institution. Am J Sociol 104:1030–1060
Western B, Kling JR, Weiman D (2001) The labor market consequences of incarceration, Crime & Delinquency 47(3):410-427