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Abstract

We study the entry to formal employment and earnings of a large sample of convicts released from Hungarian prisons in 2002-2008. We identify the effect of the prison service on post-release careers by exploiting differences in the timing of incarceration, on the one hand, and estimating fixed effects models, on the other. For convicts with a single prison spell, we find initially negative effect on employment that turns positive after about one year while the impact on wages is permanently negative. A comparison with recidivists, for whom the employment effect is negative and the wage effect is weaker, suggests that these results are driven by a drop in the reservation wages of 'converted' criminals rather than the lack of discrimination. This reading is supported by further data showing that the ex-inmates, on average, make increased effort to find legitimate sources of living and support to finding jobs.

Keywords: incarceration, unemployment, wage loss, discrimination

JEL classification: K42, J64, J39

Munkaerőpiaci pálya a börtön előtt és után

Czafit Bence - Köllő János

Összefoglaló

A tanulmányban több mint 30000 elítélt foglalkoztatásának és keresetének alakulását vizsgáljuk, 2002-2008-ra vonatkozó kapcsolt adminisztratív adatok alapján. A szabadultak foglalkoztatása egy év késéssel visszaáll a bebörtönzés előtti szintre, a kereseteik azonban tartósan és nagymértékben csökkennek. A foglalkoztatás kedvezőnek tűnő alakulása nagyrészt a bűnözéssel felhagyó többség megnövekvő munkakínálatának, a bércsökkenés a rezervációs béreik csökkenésének tudható be. A visszaesőknél két büntetés között - amikor a stigmahatás jelen van, de a rezervációs bérlés hiányzik, a foglalkoztatási esély nagymértékben romlik, a keresetek pedig csak kissé esnek vissza. Továbbá, a regisztrációra és transzferekre vonatkozó adatok is a szabadultak átlagosan megnövekvő munkakínálatára utalnak.

Tárgyszavak: bebörtönzés, munkanélküliség, bércsökkenés, diszkrimináció

JEL kódok: K42, J64, J39

1 INTRODUCTION

The effects of prison service on employment and wages are theoretically unpredictable. Prison service entails a stigma and often leads to the loss of personal networks with adverse consequences for job finding and (predictably less so) wages. Detention drives the convicts to a social environment that tempts them into further crime and keeps their labor supply low. At the same time, prison experience may deter the offenders from crime, decrease their reservation wages and in so doing improve their chances to get a job. Lengthy detention implies a loss of work experience thereby lowering the wages offered to the ex-inmates. Falling offer wages at least transitorily decrease the job finding probabilities of ex-inmates, depending on how fast they adjust their reservation wages. Work in prison, training and assisting services may simultaneously improve the convicts' employability and earnings 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 substantially differ across countries and periods and call for empirical investigation.

We study the effects of incarceration on subsequent labor market histories in Hungary using administrative panel data on a large sample of inmates observed in a protracted period. Our benchmark model follows Grogger (1995), LaLonde and Cho (2008) and Pettit and Lyons (2009) in comparing the post-prison careers of ex-convicts with the pre-prison careers of offenders incarcerated later. The comparison is made using panel regressions controlled for both calendar time and relative time i.e. periods before incarceration and after release. For offenders with a single prison spell in 2002-2008, we find negative effects on both wages and employment initially. While the wage effect is strong and remains negative throughout the observed post-prison period, the employment effect fades away rapidly and turns positive after one year out of prison. Fixed effects models comparing the pre-prison and post-prison experience of inmates yield similar results, and so do the model variants that reduce the potential bias from unobserved recidivism.

The findings are consistent with the existing literature based on administrative data in the US: the empirical papers almost uniformly suggest that wages fall after service in prison but the estimated employment effects are often weak and occasionally positive. We think that these results are strongly affected by the withdrawal from crime (and consequently lower reservation wages and increased labor supply) of a sizeable part of the ex-inmates. We test this hypothesis by looking at recidivists who continue committing crime between two prison spells. For these periods, when the stigma of criminal records 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 the recidivists are similar to those of the non-recidivists.

The data on registration at labor offices and transfer receipt also suggest that the ex-inmates, on average, put increased effort into finding legitimate sources of living.

The research behind this paper was motivated by Hungary's growing incarceration rate which ranks among the highest in Europe (barring the ex-Soviet states) with 180 per 100,000 inhabitants.¹ The growing rigor of sentencing and the introduction of a Three Strikes Law in 2010 leave no doubt that the risk of incarceration and the prison population will increase further in the foreseeable future. Despite the growing importance of the problem, we know virtually nothing, in quantitative terms, about the labor market careers of inmates. The existing literature is purely qualitative, based on informal interviews and anecdotal evidence. A few interview-based studies (Tóth et al. 2005, Csáki et al. 2007, Csáki and Mészáros 2011) report quantitative results on small samples comprising a few dozen ex-convicts. Without questioning the merits of small-sample qualitative research we believe that this paper can fill a gap that need to be filled in order to evaluate the system of law enforcement properly and design reasonable policies. The findings also add to the thin European literature on the aftermaths of incarceration. The effects of incarceration have been extensively studied in the US, having 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.

In Section 2 we briefly review the literature with special emphasis on the difficulties of identification and interpretation in models based on administrative registers. Section 3 introduces the data and presents descriptive statistics on the estimation sample. Section 4 explains the estimation strategy and Section 5 presents the econometric results. In Section 6 we add supplementary information on labor force participation, registered unemployment and transfer receipt. Section 7 discusses the results and concludes. Appendix 1 provides a more detailed description of the data and the estimates while Appendix 2 gives a concise overview of incarceration practices and the prison population in Hungary.

¹In Europe 2010, the prison population per 100,000 inhabitants varied between 59 in Finland and 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) according to Walmsley (2010).

2 THE DIFFICULTIES OF IDENTIFICATION - LESSONS FROM THE LITERATURE

The effects of the prison service on subsequent labor market careers are difficult to identify and often hard to interpret. This section briefly discusses these complications with special emphasis on research based on administrative data. For comprehensive overviews of the literature see Western et al. (2001), Fahey et al. (2006), Geller et al. (2006) and Holzer (2007).

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 as a ‘crime-employment trade-off’ rather than a causal effect. Such models indicate severe disadvantage on the part of ex-prisoners that, however, might at least partly exist even in the absence of incarceration (Bound and Freeman 1992, Freeman 1991, Freeman 1996, Grogger 1995).

The scope for randomizing the event of incarceration by means of matching methods is limited even in surveys providing thorough portraits of the offenders. In a research based on official registers, comparable to the one used in this paper, any attempt to find people with ‘similar probability of being incarcerated’ would be purely formal.² Finding instruments, which affect the incidence or length of prison service without affecting labor market prospects is also easier said than done.³ Therefore, the register-based studies typically stay within the boundaries of the offender population and try to find alternative ways of identification.

One way to deal with the problem is collecting data from the period preceding incarceration and 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 of treating pre-prison outcomes as the counterfactual for post-prison ones – 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

²In one of their model variants Pettit and Lyons (2009) match inmates and non-inmates using a particularly rich set of administrative data providing information on criminal records, pre-prison careers and even 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.

³An often-cited exception is Kling (2006) using information on the judge’s usual rigor of sentencing as an instrument for the convict’s length of service. His paper finds that the length of service has no impact on employment and wages.

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 under the assumption that the early and late cohorts are similar in terms of unobserved individual and contextual attributes. Widely cited examples are Grogger (1995), LaLonde and Chow (2008) and Pettit and Lyons (2011).

Most studies applying such methods, including ours, find negative effect on wages while the findings on employment are ambiguous and tend to be weak or even positive in some of the more recent papers (see Needels 1996, Western and Beckett 1999, Tyler and Kling 2007, Sabol 2007, LaLonde and Cho 2008, Pettit and Lyons 2009 and overviews in Western et al. 2001, Geller et al. 2006 and Holzer 2007).

In interpreting this pair of findings one should take into account that the estimates for post-prison employment inseparably capture negative and positive effects. In an environment similar to Hungary's (where in-prison work is infrequent, assisting services are undeveloped and post-release supervision is weak) the outcomes are mainly shaped by the effects of the stigma, on the one hand, and withdrawal from crime, on the other. Statistical discrimination tends to decrease both employment and wages while giving up criminal activity is conducive to lower wages and higher employment. In models similar to ours the estimates for employment capture the negative impact of discrimination and the positive impact of 'conversion' while the coefficients of the wage model capture the negative (or at least non-positive) stigma effect and the negative impact of conversion. Therefore the finding of a weak employment effect combined with a strong negative wage effect does not come as a surprise and can definitely not be interpreted as evidence of no discrimination in hiring but strong discrimination in wage setting.

This reading of the findings is valid under the assumption that amoral economic considerations play an important role in shaping the labor supply of ex-inmates, once they have decided on quitting crime for reasons of moral concerns, the deterrent effect of imprisonment or long-run utility considerations. The standard labor supply framework is obviously not applicable to convicts incarcerated for traffic violations or breaching of peace, but it is relevant in the case of criminal activities, which generate significant income for the offenders. We can assume that among people incarcerated for economic crime (60 per cent in Hungary according to CSO 2008, p. 52) a permanent shift away from crime strongly affects labor market behavior. In cases when crime generates income without consuming a noteworthy amount of time the effect of 'conversion' 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 a similar line of reasoning with

reference to the seminal model of Gronau 1977).⁴ The implications of ‘conversion’ are similar in a standard job search framework: a fall in non-wage income is conducive to lower reservation wages and higher probability of accepting a given wage offer. Discrimination furthermore reduces the ‘job offer arrival rate’ and makes extended job search less attractive thereby diminishing search unemployment and pulling 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 of search intensity among the non-employed ex-inmates. We shall check if it is indeed the case using data on registration at labor offices and take-up of transfers.

3. 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 by a 50 per cent random draw from Hungary’s resident population aged 16-74 in 2002. Each 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 four million individuals on the 15th day of each month, their days in work during the month and income from employment and/or self-employment. We also have data on registration at a labor office and the receipt of transfers such as unemployment insurance benefit (UI), unemployment assistance (UA), old-age pension, disability payment, child care allowance and social benefits. The data on transfers are available from 2004 onwards.

The *incarcerated* can be identified because the state pays their social security contributions to the Health Insurance Fund. In our period of observation 73 percent of the incarcerated were in prison, 25 per cent in pretrial custody (also served in prison in the typical case) and 2 percent in other types of detention, on average (Calculated using data in CSO 2013). In the data at our disposal, we can not distinguish the three groups. However, we can check how the implications of short periods behind bars differ from the consequences of

⁴ The effect of withdrawal from crime is easy to visualize in charts depicting the choice between consumption and leisure. The optimal supply of hours by offenders working before incarceration increases since the straight segment of the budget constraint shifts downward without a change in its slope. The effect is qualitatively similar on the extensive margin i.e. for offenders who did not work prior to imprisonment. In this case reservation wages are given by the slope of the indifference curves at zero working hours. The slopes tend to decrease as the income from crime disappears, that is, we move downwards vertically on the indifference map.

longer sentences, which practically exclude cases of pretrial detention without subsequent prison service.

Our subjects are regarded as *employed* if their employer or own business reported nonzero personal income to the Pension Fund in a given month. We alternatively use an employment dummy (set to one 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 observed person. 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 accounted 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 1/9 of the average wage. We treat prisoners as non-employed earning zero wages.

We know the age, gender and occupation of the individuals, but not their level of education, except for people registered as unemployed at least once. Instead, we use the information on the offenders' occupational history as a proxy for skill levels. We distinguish occupations that do not require qualification from those requiring college/university diplomas and a residual category (see Table A1 of Appendix 1). This categorization permits to characterize a part of our subjects on the basis of their job histories (never worked in a job requiring qualification, worked at least once in a job requiring tertiary education and a residual category).⁵ We 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 NUTS-2 region codes of the convicts' permanent address are missing for 53 percent of the observations. These codes come from the Health Insurance Fund and the Public Employment Service and only available for inmates, who have had contact with these organizations. Regional affiliation has limited informational value in the case of offenders anyway since they often reside in regions different from their registered place of living.

The starting sample covers 30,835 people aged 16-74 in 2002 and incarcerated at least once in 2002-2008. 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.

⁵ Since the educational proxy defined in this way is not available for inmates with no employment experience in 2002-2008 it can not be used in employment equations relating to the total population of prisoners.

Table 1

Number of observations by spell histories in the cleaned sample (2002-2008)

	Persons	Person-months
Single left censored spell in prison	6,272	521,164
Single right censored spell in prison	2,061	169,965
Single uncensored spell in prison	13,681	947,642
More than one prison spells in the observed period (recidivists)	4,863	408,009
Total incarcerated at least once in 2002-2008	26,877	2,046,780

Four distinct groups can be identified in the seven-years wide window in which we observe the incarceration, time in detention and release of prisoners (Table 1). About one-third of the spells 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 this case was 13.4 months on average. About 1/5 of those incarcerated at least once had further spells in prison after being released from the first spell. We have significantly more left-censored than right-censored spells for two reasons. First, the left-censored spells include long services while the right-censored spells can not be longer than 83 months given our 84 months wide window of observation. Second, many of the left-censored spells may belong to recidivists while those serving more than one prison spells in 2002-2008 are treated separately from those serving only one spell.

Since our data follows 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 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 Table A1 of Appendix1.

Our data has several advantages and disadvantages compared to the administrative panels used in the US, which are typically created by merging data of the prison administration and the state-level UI registers. Compared to the US databases, our data tells very little about the prior criminal records and personal characteristics of the offenders. In the same time, the Hungarian data provides a more detailed view of labor market careers by containing information on occupations, employment relationship, 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.

The administrative registers also have advantages (besides a host of apparent disadvantages) over survey data. Former inmates are difficult to find, hard to convince to participate in a survey, and their answers can be biased. Even more important, 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 for this paper in Section 7.

4. ESTIMATION METHODS

4.1. EXPLOITING VARIATIONS IN THE TIMING OF INCARCERATION

In our benchmark model, we follow LaLonde and Chow (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,

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

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-varying and time-invariant covariates, T_t is a dummy variable equal 1 in month t and 0 otherwise while u_{it} is a residual term. The Q_q dummies denote relative time, that is, set to one if person i is q quarters away from the quarters spent in prison partly or fully. For any quarter containing prison service, q is set to zero. Therefore, $Q_0=1$ covers the quarters of incarceration, prison service 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 holding 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 ten percentage points lower for those in their sixth full quarter after release than it is for offenders more than one year before their incarceration.⁶

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

First, a strong correlation between calendar time and relative time menaces with invalid results about individual predictors including relative time effects. This is particularly the case at the margins of the observation window: the 25th post-prison quarter, for instance, can only take place in 2008 and the same problem arises when a pre-prison quarter substantially

⁶ 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 the employment relationship and a dummy for employment spells lasting for only one day. These spells account for only 0.8 per cent of the total but 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.

precedes the date of incarceration. In order to reduce the risks arising from multicollinearity one should either widen the brackets for 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 suggested 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 $|q| \geq 11$.⁷ To check if multicollinearity is a problem we repeat the estimation after narrowing the time window to 10 quarters before and after prison service.

Second, this specification cannot deal with offenders serving several prison spells in the observed period. Recidivists q quarters after a prison spell can also be $q+x$ quarters after another prison spell and some quarters before their next spell so we cannot define a relative time variable for them in the way we do it 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 on the level of y_{it} one quarter before or after prison but the data for the -27^{th} or 27^{th} 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. The time path of the overall mean is meaningless if it does not reflect the underlying histories of the cohorts.

Las 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 at hand, the risk of such correlation is high, and there is a strong case for estimating fixed effects models.

4.2. 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 define relatively wide brackets for relative time: 1-8 quarters and more than 8 quarters before/after the prison quarters. These periods are labeled as ‘shortly’ and ‘long’ before/after incarceration in

⁷ 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 months long periods in their quarterly panel.

Equation 2. 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.

$$(2) \quad y_{it} = \alpha X_{it} + \delta Z_i + \beta_1 \cdot \text{shortly_before}_{it} + \beta_2 \cdot \text{prison}_{it} + \beta_3 \cdot \text{shortly_after}_{it} + \beta_4 \cdot \text{long_after}_{it} + \sum_{t=2}^8 \gamma_t T_t + c_i + \varepsilon_{it}$$

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 months after release than it was more than 8 quarters prior to incarceration.

4.3. TAKING CARE OF UNOBSERVED RECIDIVISM

As noted in Holzer (2007) studies similar to ours should take care of recidivism even if they study offenders with a single prison spell in their windows of observation. In the case of offenders incarcerated before the period of observation 'pre-prison' employment and wages carry the effect of previous prison experience - this leads to an underestimation of the negative consequences of incarceration. In lack of information on the prior criminal records of the offenders one has to recourse to second best solutions such as starting the analysis later than the start date of the observed period as LaLonde and Chow (2008) do. Therefore we repeat the fixed effects estimations for offenders incarcerated in 2005-2008.

In choosing this sub-sample, we utilize the available information on recidivism. As shown in Appendix Figure A1, 80 per cent of those who returned to prison within our seven years wide window of observation did so within 3 years and more than 95 percent returned within 5 years. Therefore, we can be confident that the large majority of those incarcerated in 2005-2008 had not been imprisoned before 2002.

4.4. ESTIMATES FOR RECIDIVISTS

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

$$(3) \quad y_{it} = \alpha X_{it} + \delta Z_i + \beta_0 \cdot \text{prison}_{it} + \beta_1 \cdot \text{between}_{it} + \beta_2 \cdot \text{after}_{it} + \sum_{t=2}^{84} \gamma_t T_t + u_{it}$$

The model is estimated separately for recidivists and offenders with a single prison spell in the observed period. 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 spell. 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. The β_1 coefficients are expected to indicate a significant negative impact on employment and weak effect on wages as they capture the outcomes in a period when the stigma effect is present but the ‘conversion effect’ is absent.

5. RESULTS

5.1. MAIN 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 Figure 1. We observe a gradual erosion of employment as the offenders are approaching the quarter of incarceration. The process of attrition, which has been observed in all studies using similar data, is explained by several reasons. First, 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 all the more as the trials take place in the region where the offenders committed crime, often far from their permanent place of living and working. Third, many employers lay off their workers when they get informed of their involvement in judicial process. Workers who try to hide this information 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 percent of the national average. Deductions because of absenteeism and lower earnings from self-employment and payment-by-result schemes might play a role in this. Furthermore, white collar offenders are most probably exposed to longer investigation and trial compared to street-corner dealers and thieves and face a higher risk of being fired in case their employer got news about their involvement in crime. Therefore, the sample of future prisoners is gradually biased toward unskilled workers as the time of incarceration is approaching.

The path of employment is nearly symmetric: employment starts to grow from virtually zero to 18 percent by the end of the first post-prison year and exceeds 20 percent from the

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.

second year onwards – levels corresponding to the pre-prison levels at similar distance from the period of prison service. This is not the case with daily wages, which fall substantially and stay below their pre-prison level throughout the observed period.

The estimates of Equation 1 reinforce the impressions based on raw data. In the first year following 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 4 years as suggested by the 95 percent confidence intervals shown in the middle panels of Figure 1.⁹ The conclusions remain if we narrow the window of observation to $|q| < 11$ (bottom panels).

In Figure A2 of Appendix 1, we study how the paths of particular cohorts relate to the path of the cross-section means shown in Figure 1. The charts show the pathways of employment and wages for cohorts distinguished by the year of release. (Charts for inmates incarcerated in the same year yield similar results). Apart from a small number of outliers the cohort-specific levels fall very close to the full-sample means. The cohort histories are similar, and also hint at the recovery of employment in the longer run and a long lasting decline in wages.¹⁰

The *fixed-effects estimates* (Equation 2) are summarized in Table 2 while the detailed results including test statistics are presented in Table A3 of Appendix 1. 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 the Sargan-Hansen overidentification test proposed in Schafer and Stillman (2010) instead.

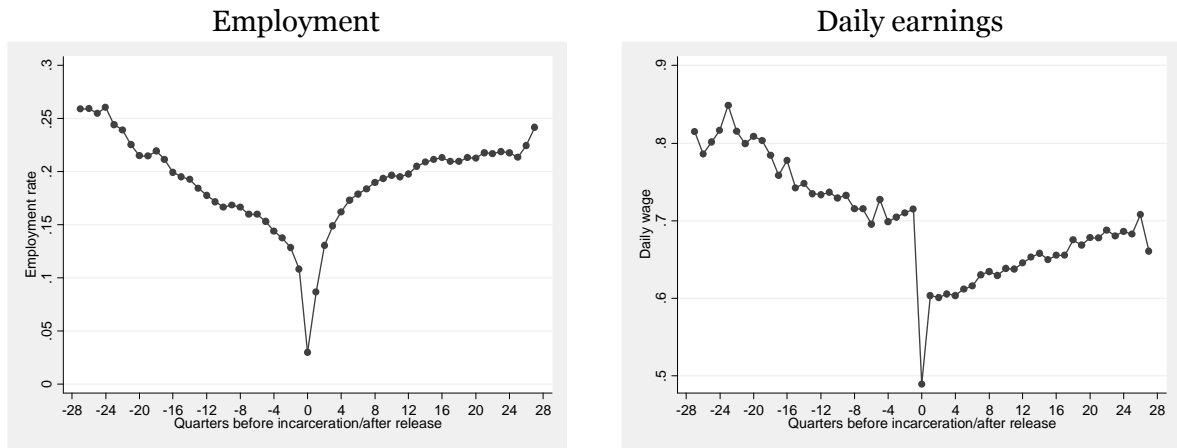
The estimates reinforce that the path of employment is symmetric: 1-8 quarters after release the employment rate stands where it stood 1-8 quarters prior to incarceration while in the longer run it returns to the level observed more than two years before imprisonment. By contrast, daily earnings are estimated to fall by 8-10 percentage points relative to the immediate pre-prison period and stay at that level later. In the longer run (9 to 28 quarters after release) the ex-inmates still earn less by 11-12 percentage points than they did two years (or more) prior to incarceration.

⁹ Estimates of the control variables of Equation 1 are shown in Table A2 of Appendix 1.

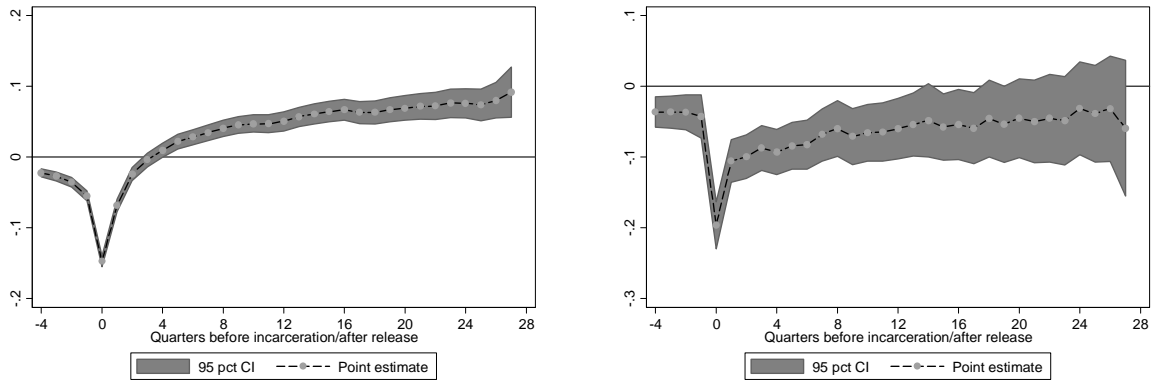
¹⁰ Note that in this case the data excludes right-censored prison spells. Therefore the paths of employment and wages slightly differ from those shown in Figure 1.

Figure 1

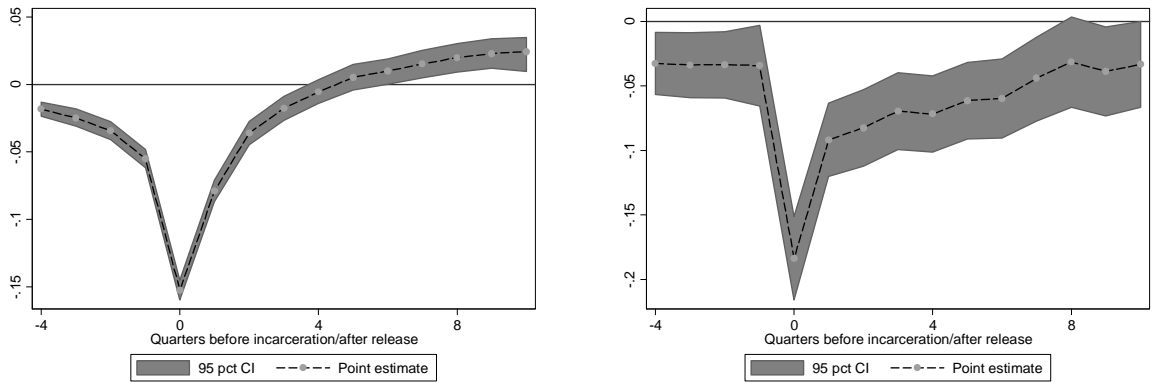
Employment and wages in quarters before/after incarceration



Raw data



Estimates of Equation 1 (all relative quarters)



Estimates of Equation 1 (relative quarters ± 10)

Sample: offenders with a single prison spell in 2002-2008, age >22. Quarter 0 stands for all quarters that included prison service. Reference: 5 or more quarters before the first prison quarter. The shaded areas indicate the 95 per cent confidence intervals. Standard errors are adjusted for clustering by persons.

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

Table 2

Fixed effects estimates of changes in employment and daily earnings

	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*** (13.6)	-0.037*** (9.5)	-0.044*** (3.7)	-0.037*** (2.8)	-0.042*** (3.6)	-0.035*** (2.7)
Prison quarters	-0.182*** (44.1)	-0.177*** (32.8)	-0.238*** (10.9)	-0.263*** (9.5)	-0.230*** (10.8)	-0.254*** (9.4)
Quarters 1 to 8	-0.046*** (8.3)	-0.034*** (5.3)	-0.134*** (6.4)	-0.129*** (5.0)	-0.122*** (6.0)	-0.118*** (4.7)
Quarters 9 to 28	-0.005 (0.8)	0.004 (0.5)	-0.125*** (5.0)	-0.114*** (3.5)	-0.113*** (4.6)	-0.105*** (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 *)0.01 **)0.05 *)0.1 level. t-values in parentheses. Standard errors are adjusted for clustering by persons.

The data relate to inmates serving a single prison spell in 2002-2008 or 2005-2008.

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 the employment relationship

For further test statistics see Tables A2 and A3 of Appendix 1

Finally, we turn to the comparison of recidivists and non-recidivists. Two remarks are due 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 risking 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 committing crime or at least a part of them stop doing so. 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 the way it comes into being we do not control the wage equations for job characteristics.

Table 3

Estimates for convicts with one versus several prison spells in 2002-2008

Fixed effects estimates of Equation 3

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

Significant at the *)0.01 **)0.05 *)0.1 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 Table A4 of Appendix 1.

The results, summarized in Table 3, are consistent with the expectations. The employment rate of recidivists falls significantly between two prison spells, by 3.4 percentage points, equivalent to a 40 percent decline relative to its pre-prison level. In the same time, we observe a minor and statistically insignificant fall in earnings between two prison spells. This pair of observations is in harmony with what is expected when the stigma effect is present, but the 'conversion effect' is absent.

Equally important, when the recidivists are released from their last observed prison spell, and at least a part of them withdraw from crime, their estimates do not differ from those of the non-recidivists: the relative wages of both groups fall by 9-10 percentage points and their employment rates do not, or only slightly, differ from their pre-prison reference values.

5.2. SENSITIVITY ANALYSIS

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

Table 4

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

Sample variants	Employment	Daily earnings
Both genders ^a	-0.004 (1.06)	-0.080 ^{***} (5.55)
Only men ^a	-0.004 (1.04)	-0.086 ^{***} (4.83)
Persons aged 16-22 included	0.002 (0.44)	-0.067 ^{***} (4.29)
Only persons older than 22 in 2002	-0.004 (0.91)	-0.077 ^{***} (4.35)
Aged 22-35	0.003 (0.51)	-0.067 ^{***} (2.96)
Older than 35	-0.008 (1.59)	-0.134 ^{***} (4.51)
Inmates serving 1-3 months in prison ^a	-0.012 [*] (1.76)	-0.080 ^{**} (2.21)
Inmates serving 1-6 months in prison ^a	-0.012 ^{**} (2.13)	-0.086 ^{***} (2.68)
Inmates serving more than 3 months in prison ^a	-0.008 (1.56)	-0.078 ^{***} (3.17)
Inmates serving more than 6 months in prison ^a	-0.007 (1.10)	-0.064 ^{***} (2.89)
Persons with non-missing region code, no region dummies ^a	-0.027 ^{***} (4.13)	-0.077 ^{***} (4.35)
Persons with non-missing region code, region dummies added ^a	-0.026 ^{***} (4.39)	-0.079 ^{***} (4.39)

Note: 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.

Unsurprisingly, the results for men and the entire sample are practically identical. Including young offenders or restricting the sample to persons older than 22 in 2002 makes no difference in 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 their younger counterparts: -13.4 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 pretrial detention is presumably high in the case of short completed spells while those serving more than 3 months must be predominantly convicted prisoners.

The only specification, where the employment coefficients markedly differ is the one estimated for inmates with known permanent address. As was previously mentioned, regional affiliation is only known for people who had contact with the health insurance fund

or the employment service. A non-missing region code actually serves as a (noisy) proxy for pre-prison employment: only 18 percent of the offenders with no observed pre-prison employment experience have region codes as opposed to 44 percent among those who had at least one employment spell prior to incarceration. The estimates suggest a significant 2.7 percentage point's fall in the employment rate of this group. This result is biased by regression to the mean i.e. the tendency of the conditional expectation of a random variable to move toward its long-run mean under non-restrictive conditions (see Keuzenkamp 2006, for instance). Having or not having jobs in a relatively short period prior to incarceration is affected by sheer luck. Therefore, we expect some erosion in the employment rate of those having pre-prison work experience and positive employment rate for those, who did not work at all in the years before imprisonment. Importantly, the estimates are robust to the inclusion of region dummies suggesting that regional affiliation either has no measurable impact on the outcomes, or, the permanent addresses do not effectively proxy the labor market conditions of the geographical area where the inmates actually look for jobs.

6. LABOR FORCE PARTICIPATION, JOB SEARCH AND ALTERNATIVE SOURCES OF INCOME

The majority of released prisoners stop committing crime and look for alternative ways of survival. About 80 percent of those released in 2002-2003 did not return to prison until December 2008 i.e. spent 6 or 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 have a job. Despite that, the employment rate of the ex-prisoners does not exceed its pre-prison level. Therefore we expect that the ex-prisoners, on average, make increased efforts to find alternative sources of income and assistance to job finding.

Table 5 shows logistic panel regression estimates of the probabilities of (i) registration at the labor office (ii) receipt of UI or UA (iii) receipt of other transfers, conditional on being non-employed. The data relate to 2004-2008. The models have been estimated with random effects because prior employment experience and gender are strong individual-specific predictors, which would drop out from a fixed effects model. We included calendar year dummies to control for the rising trend in registration and benefits receipt in 2004-2008. The table shows odds ratios and z-values adjusted for the clustering of observations by persons. The odds ratios measure how a unit change in the explanatory variable affects the probability of a positive outcome (p) divided by the probability of a negative outcome [$p/(1-$

p)]. The odds ratios are set to 1.00 for the reference categories (more than 8 quarters prior to incarceration in the case of relative time).

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 jumps to 1.66 in the immediate post-prison period and 2.91 in the longer run. By contrast, the chance of receiving other transfers, less closely related to unemployed status, falls significantly already before incarceration and stays at low levels throughout the post-prison period.

Table 5

Registration at a labor office and transfer receipt – Odds ratios

Random effects panel logit estimates for months out of employment in 2004-2008

Dependent variable:	Registration at a labor office	UI or UA	Other transfers ^a
0-8 quarters before incarceration	0.97 (0.4)	0.90 (1.0)	0.71** (2.0)
Prison quarters	0.16*** (15.4)	0.13*** (11.4)	0.12** (9.8)
0-8 quarters after release	1.96*** (5.4)	1.66*** (2.6)	0.32*** (4.8)
9-28 quarters after release	2.14*** (5.7)	2.91*** (5.3)	0.61* (1.8)
Male	0.95 (0.6)	1.23** (2.6)	0.16*** (3.7)
Age	1.20*** (5.2)	1.62*** (9.6)	0.70*** (4.9)
Age squared	0.99*** (6.8)	0.99*** (10.2)	101*** (5.8)
Employed prior to incarceration ^b	3.35*** (12.0)	6.24** (11.0)	0.34*** (5.0)
2005	1.14*** (4.1)	1.21*** (4.18)	1.53*** (6.5)
2006	1.18*** (3.6)	1.75*** (8.9)	2.12*** (6.5)
2007	1.36*** (5.5)	3.31*** (15.7)	1.88*** (4.0)
2008	1.70*** (8.5)	4.26*** (17.4)	1.63** (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 (0.000)	23157.6 (0.000)	13059.3 (0.000)
Rho (s.e.)	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 2004-2008 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, child care allowance, social benefits, compensation for disabled family members, compensation to widows and orphans b) Employed at least once

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. In 2005, 17.2 percent of the non-employed ex-prisoners were registered, which grew to 20.3 percent by 2008. These rates are somewhat lower than those measured among unskilled prime-age men in the general population: 21.3 percent in 2005 and 25.9 percent in 2008 according to the Labor Force Survey (LFS).¹¹

Benefits and other transfers raised the fraction of ex-inmates earning some legitimate income (wage or benefit) to 35.2 percent in 2005 and 42.5 percent 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 percent in 2005 and 37.6 percent in 2008) are much lower than those measured in the comparable general population. As many as 85 percent of the unskilled prime age men earned a wage or received transfer in 2005 and 91.3 percent in 2008. Out of the non-employed, 68 percent had income from transfers in 2005 (73.8 percent in 2008) as opposed to only 15 percent of the ex-inmates.

7. DISCUSSION AND CONCLUSIONS

The most important piece of information we have learnt 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 percent (when out of custody), a level substantially lower than those observed in the US literature. Pettit and Lyons (2009) report that the ex-inmates studied by them spent 26 percent of their time in UI-covered jobs. LaLonde & Cho (2008) find a 30 percent post-prison legitimate employment rate among economically disadvantaged and previously imprisoned mothers in the US. Geller et al. (2006) study predominantly young males and report a 41 percent level of legitimate employment in the first year after release. Our estimate is 14.4 percent for the same period. Skardhamar and Telle (2009) report a 25 percent rate immediately after release in Norway. This compares to 10 percent for non-recidivists and 2 percent for recidivists in Hungary, in the first full quarter after release.

The unemployment statistics yield similar worrisome results. The registered unemployment rate of ex-prisoners amounted to 42.1 percent in 2005 and 43.3 percent in 2008. These rates are several times higher than those measured in the Labor Force Survey for comparable populations: in 2005 the unemployment rate was 'only' 19.5 percent among

¹¹Authors' 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.

prime age men with primary education background and 6.5 percent among those with vocational qualification. In 2008, the rates were 24.4 and 8 percent, respectively.¹²

The Hungarian data draw attention to an extreme degree of isolation from the society of ‘ordinary people’ earning their living in legitimate jobs. The gap would predictably be narrower but still rather wide if we took informal employment into consideration. Based on interviews in a small-sample survey Csáki and Mészáros (2011) find that about 40 percent of the jobs held by future convicts are unregistered. Taking this information at face value, we can estimate the offenders’ total employment rate to be about 30 percent. Supposing that every second job of the offenders is informal we still end up with a rate short of 40 percent.

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 former paper observed a 54 percent total employment rate. The latter paper reports that only 5.5 percent of young men with criminal income in 1979 were non-employed throughout the year, and the average person spent 1500 hours in work during the year. In our sample 45 percent had no legitimate jobs at all in a period of seven years. The average person spent a maximum of 488 hours in legitimate work annually (calculated as days in work times 8 hours a day). Even if we assumed equal shares of formal and informal jobs the non-employment ratio would decrease to only 20-25 percent and estimated hours would continue to fall short of 1000 hours per annum.

Data on informal work could also change the picture of *changes* of employment. The share of informal employment within total employment is most probably higher after than before incarceration given the administrative constraints to enter formal employment without clean records and the existence of statistical discrimination for which we have ample anecdotal evidence.¹³ The estimates presented in the previous sections suggested zero or weak negative effect of incarceration on legitimate employment. Data on total employment would most probably indicate zero or even positive prison effect for non-recidivists and would not change our qualitative conclusions.

In contrast to the findings of several papers in the US, showing the highest post-prison employment rates immediately after release, we find that less than 5 percent of the Hungarians leaving prison have legitimate jobs in the first full month after release and 10 percent have jobs in the first full quarter after release. Starting from these extremely low values employment increases slowly and remains low in absolute value even at its peak, several years after release.

¹²Authors’ calculation using microdata from the 2005 and 2008 Q1 waves of the LFS.

¹³ Csáki and Mészáros claim that the share of informal work remains basically unchanged after release but this information is based on a very small number of observations.

This pattern calls into question, first of all, if post-release supervision and assistance are highly effective. Second, it might reflect the difficulties of finding jobs in new labor markets. The Hungarian clean sheet regulations exclude the ex-convicts from public sector jobs and leading positions in micro-firms and self-proprietorships for a minimum of three years. As shown in the upper right panel of Figure 3, the 7 percent pre-prison share of public sector jobs falls to 2 percent and the share of self-employment drops from a range of 10-15 percent to only 2-3 percent, in the first three post-prison years.¹⁴ These figures compare to an 11 percent share of public sector jobs and 7.6 percent share of self-employment among unskilled prime age males (14.6 and 13.6 percent among prime-age males with vocational qualification) according to the 2005 Q1 wave of the Labor Force Survey. The share of registered casual workers within total legitimate employment increases substantially but has marginal importance (about 2 percent share) even at its peak.¹⁵

It seems that the existing regulations and ‘mainstream’ public institutions do not help the ex-inmates effectively. The clean sheets issued by the authorities are rather laconic: they simply inform the would-be employer about the lack or existence of criminal records without indicating the type of crime committed and severity of the punishment. This practice implies that people convicted for stealing a few glasses of perfume or evading taxes must not be hired as cleaners, janitors or drivers in public institutions and state-owned firms. On top of being categorically excluded from public sector jobs, the ex-prisoners have limited access to insurance-based benefits (such as UI, several child allowance schemes and pensions) and about 60 percent of them are left without any kind of legitimate income even years after release. While their propensity to ask assistance from the public employment service rises substantially after being set free, the vast majority of them remains unregistered and unassisted. Compensating themselves by running their businesses is also made difficult by the short-cut regulations in effect.

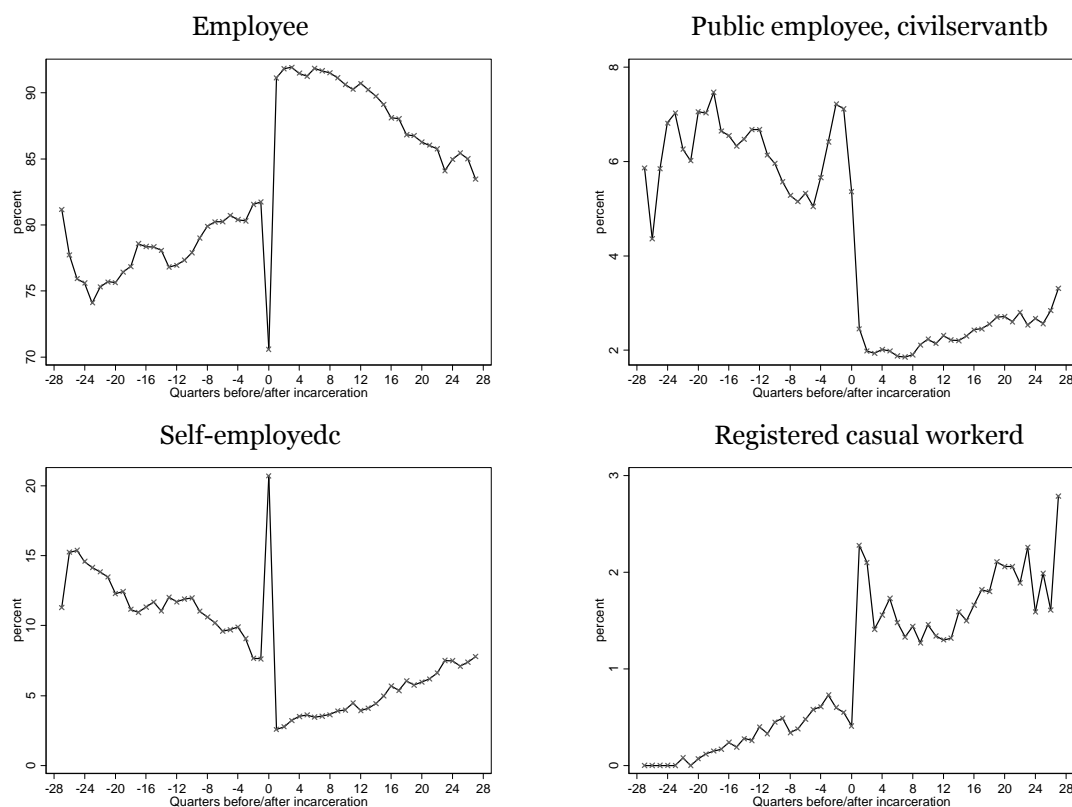
¹⁴ The non-zero rates after release are probably explained by cases when the arrestee was set free from pretrial detention without being condemned to prison service.

¹⁵ Registered casual workers are employed with a so called ‘casual work booklet’ which allows simplified administration and reduced advance social security contribution payment for a limited period of time. In the same time, this form of employment provides legitimate earnings and accrual points.

Figure 3

Jobs before and after incarceration

(percent, all employed persons = 100)^a



a) People with unknown (0.7 percent) or unspecified (1.1 percent) types of contract and those having several types of contracts (1.4 percent) are included in total employment but not shown separately.

b) Special regulations of employment and wages apply to public employees ('közalkalmazott') and public servants ('köztisztviselő') accounting for about 70 percent of the workforce in the public sector.

c) Owners of own business, members of business partnerships and assisting family members

d) Workers employed with 'casual work booklet' allowing simplified administration and reduced advance social security contribution payment for a limited period.

Private enterprises employ the overwhelming majority of ex-inmates and this is a segment where more reasonable regulations and assisting services could generate particularly high returns. Internet forums attended by convicted people and ex-prisoners often mention cases when clean sheets are also required by private employers despite the fact that they can not be sued for 'negligent hiring' as is the case in several US states.¹⁶ Under the recent regulations, employers can base their hiring decisions on a very poor set of information which understandably makes them precautious. Arguably, employers' access to the criminal records database could weaken rather than strengthen statistical discrimination. Narrowing the range of jobs for which a clean sheet is required could also alleviate the converted offenders' path to employment. Last but not least, more direct assistance by civil

¹⁶See <http://www.jogiforum.hu/forum/21/9835>, for instance.

organizations could help in providing quality information to employers and build bridges leading back to the society of 'ordinary people'.

The employment rate's tendency to return to, or grow beyond, its pre-incarceration level might give the impression that the prisoners face no serious obstacles in reintegration. This impression is evidently 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 living and ready to accept low paid jobs. Consistently 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 'conversion' 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 legitimate sources of living on the part of the latter group.

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APPENDIX 1

Table A1: 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 2002-2008	1	2.1 (0.8)
Average duration of completed prison spells, months (s.d.)	12.8 (13.2)	12.9 (11.9)
Employed at least once in 2002-2008	61.6	40.5
Employed on the 15 th day of the month 2002-2008	18.2	7.4
Days in work/potential days in work 2002-2008 (s.d.)	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 (s.d.)	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
- 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 onetypes at a time)	1.4	0.8
Monthly earnings/national average (s.d.)	63.1 (43.3)	53.9 (32.6)
Daily earnings/national average (s.d.)	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 A2: Employment and wage effects of the control variables in Equation (1)

	Employment	Daily earnings
Male	0.044 ^{***} (7.73)	0.060 ^{***} (4.61)
Age	0.018 ^{***} (11.27)	0.013 ^{***} (3.12)
Age squared/100	-0.025 ^{***} (12.92)	-0.017 ^{***} (3.02)
<i>Job characteristics</i>		
Elementary	-	-0.061 ^{***} (7.39)
Professional	-	0.102 ^{***} (4.12)
Unknown or missing	-	-0.077 ^{**} (2.45)
<i>Employment relationship</i>		
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 [*] (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 (0.000)	1304.19 (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. Normalised for the national average in the given month.

Table A3: 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/-1	-0.047 (13.9)	-0.047 (13.6)	-0.035 (3.3)	-0.042 (3.6)
Prison quarters	-0.179 (41.8)	-0.182 (44.1)	-0.212 (12.8)	-0.230 (10.8)
Quarters 1/8	-0.048 (8.68)	-0.046 (8.3)	-0.105 (7.6)	-0.122 (6.0)
Quarters 9/28	-0.001 (0.8)	-0.005 (0.8)	-0.090 (5.7)	-0.113 (4.6)
No of obs	1,361,558	1,361,558	197,382	197,382
No of groups	19,815	19,815	10,746	10,746
Obs. per group				
min	1	1	1	1
average	68.7	68.7	18.4	18.4
max	84	84	84	84
R-sq within	0.052	0.052	0.020	0.019
R-sq between	0.001	0.000	0.067	0.040
R-sq 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 assumed	-0.076	0 assumed	
Sargan-Hansen	1022.1 (0.0000)		288.1 (0.0000)	
Estimates for inmates incarcerated in 2005-2008				
Quarters -8/-1	-0.038 (9.9)	-0.037 (9.5)	-0.032 (2.5)	-0.035 (2.7)
Prison quarters	-0.175 (34.0)	-0.177 (32.8)	-0.229 (10.5)	-0.254 (9.4)
Quarters 1/8	-0.035 (5.8)	-0.034 (5.3)	-0.103 (6.4)	-0.118 (4.7)
Quarters 9/28	0.002 (0.3)	0.004 (0.5)	-0.087 (4.6)	-0.105 (3.3)
No of obs	832,204	832,204	125,409	125,409
No of groups	12,182	12,182	6557	6557
Obs. per group				
min	1	1	1	1
average	68.3	68.3	19.1	19.1
max	84	84	84	84
R-sq within	0.049	0.050	0.020	0.020
R-sq between	0.002	0.003	0.065	0.038
R-sq 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 assumed	-0.070	0 assumed	0.012
Sargan-Hansen	2075.7 (0.0000)		241.8 (0.0000)	

Table A4: Estimates for convicts with one versus several prison spells in 2002-2008

Fixed effects estimates of Equation 3

	Employment ^a		Coefficient (t-value)	Daily earnings ^b	
	Coefficient (t-value)	Sargan- Hansen ^c		Sargan-Hansen ^c	
Recidivists between two prison spells	-0.034*** (8.64)	462.353 (0.0000)	-0.048 (1.41)	125.945 (0.0050)	
Recidivists after their last prison spell	-0.000 (0.10)		-0.096** (2.02)		
Non-recidivists after a single prison spell	-0.009** (2.47)	796.131 (0.0000)	-0.089*** (5.00)	357.188 (0.0000)	

Significant at the *)0.01 **)0.05 *)0.1 level. T-values in parentheses. Standard errors are adjusted for clustering by persons.

Other test statistics. Employment equation for recidivists and non recidivists: $\text{corr}(u_i, X_b) = -0.165$ and -0.081 , $\rho = 0.304$ and 0.511 . Wage equation: $\text{corr}(u_i, X_b) = -0.392$ and -0.148 , $\rho = 0.423$ and 0.408 .

Figure A1

The cumulative distribution of the time between two prison services

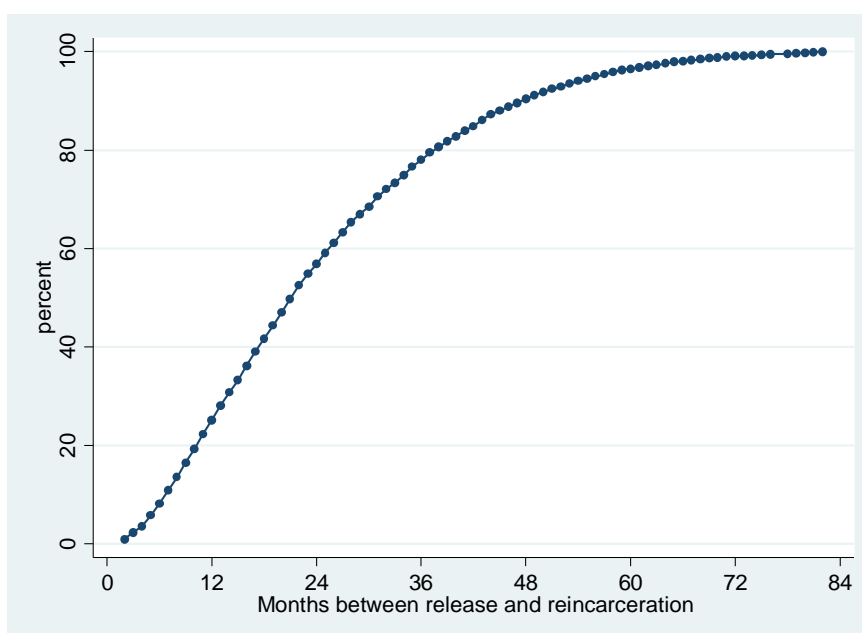
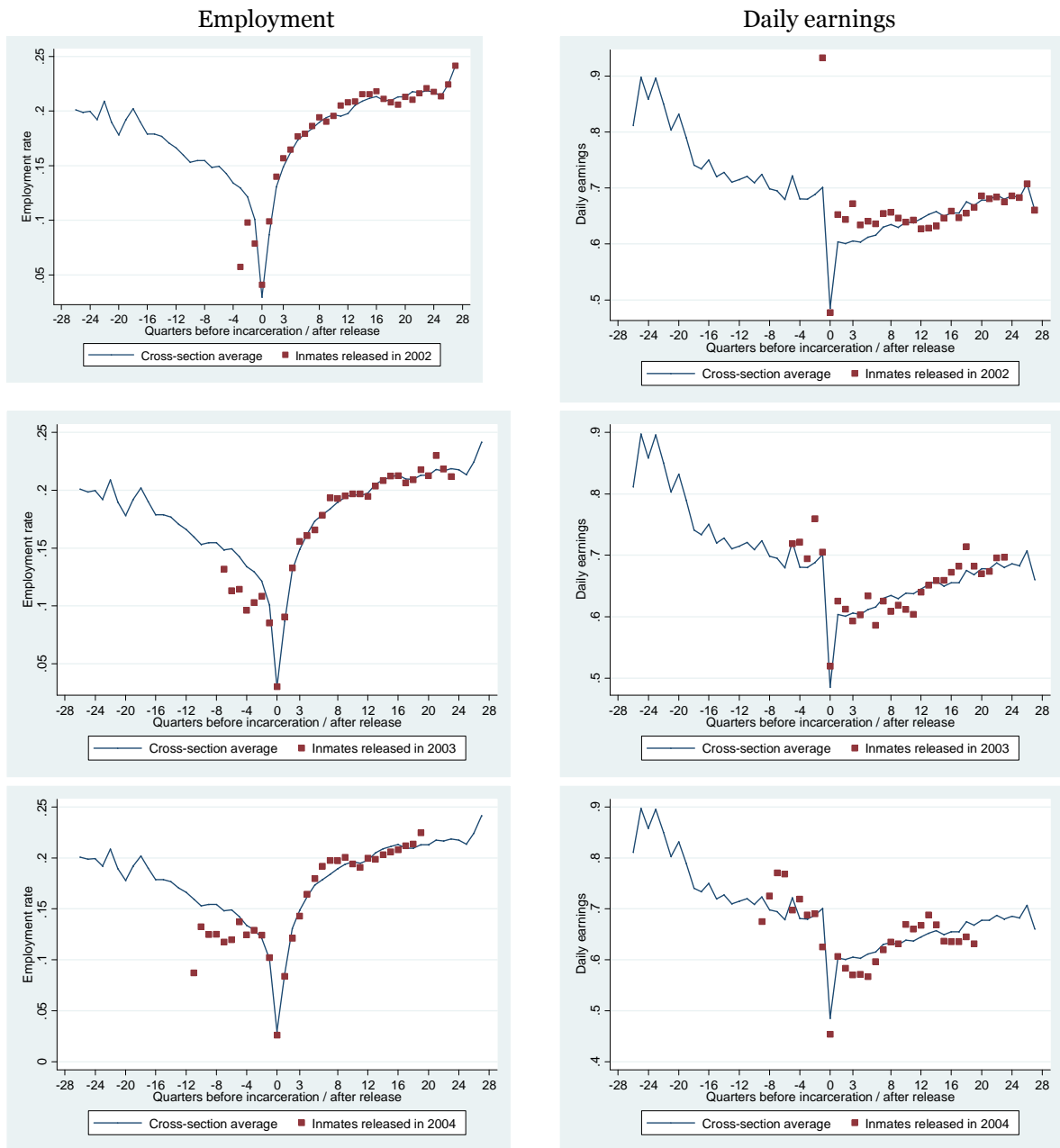
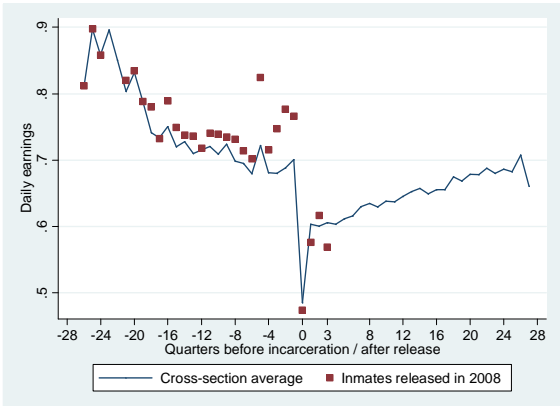
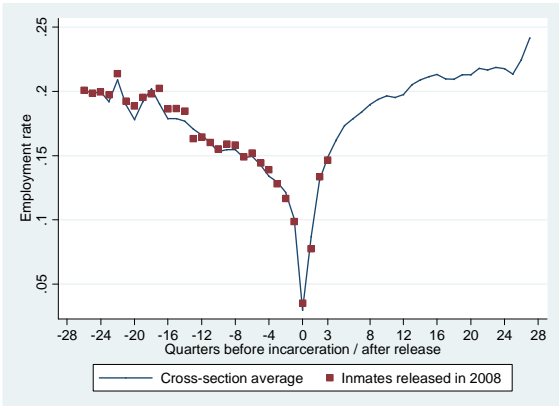
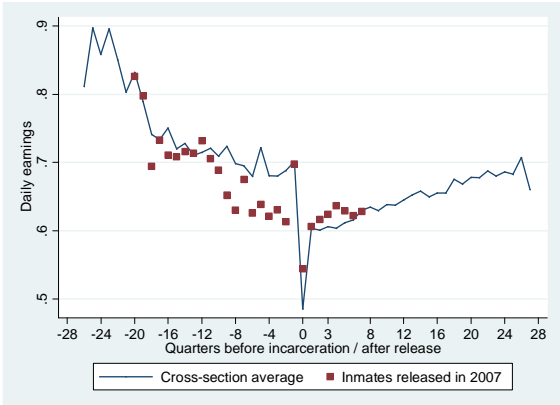
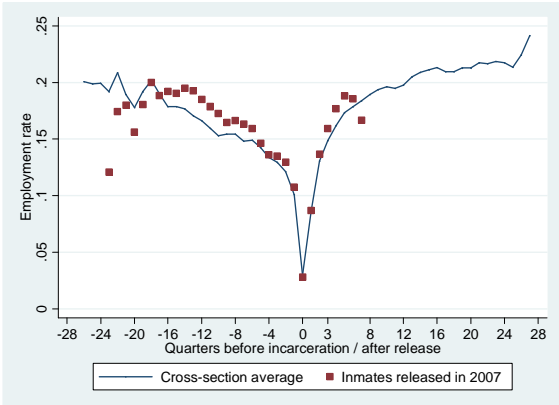
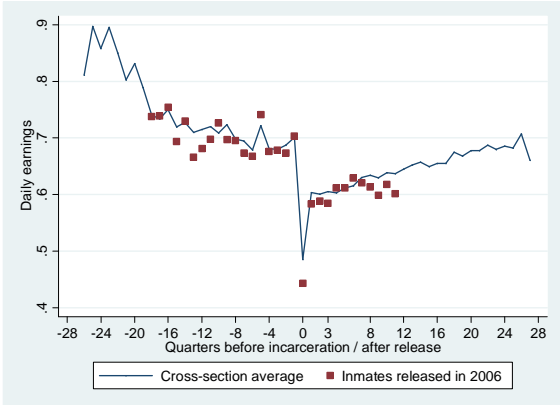
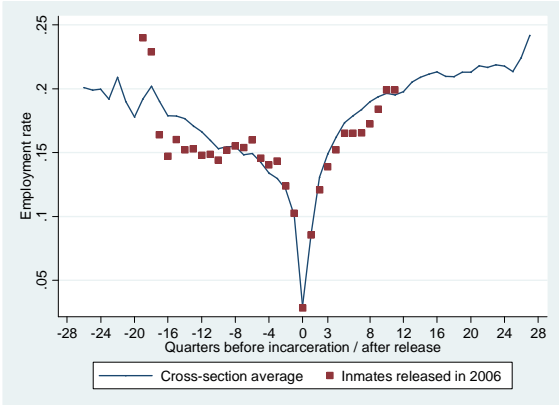
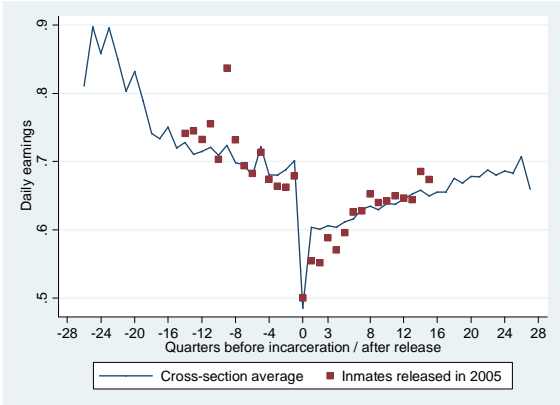
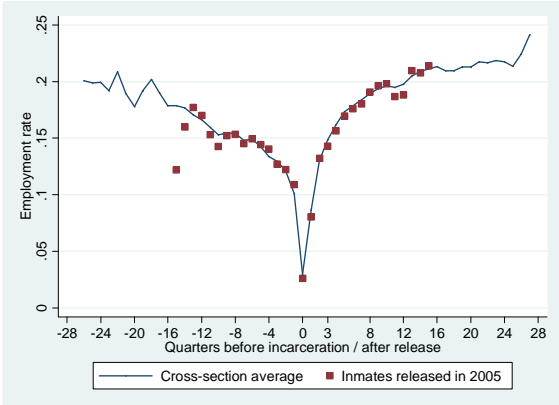


Figure A2

**The paths of cohorts versus the cross-section
averages of employment and wages**
Cohort = inmates released in a given year





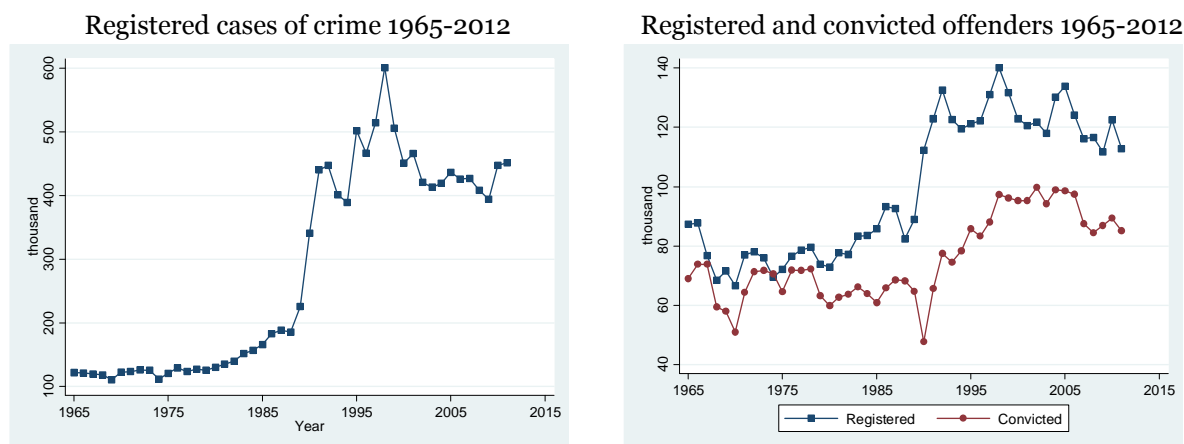
Note: The data relate to inmates with a single uncensored or left-censored spell in prison. For that reason, the paths relating to the entire sample slightly deviate from those in Figure 1. Cohort-quarter cells with less than 100 observations have been suppressed.

APPENDIX 2. INCARCERATION IN HUNGARY

The collapse of state socialism in 1989 brought about a large increase of criminal activity: the average annual number of registered cases jumped from an annual average of 148 thousand in 1965-90 to 449 thousand in 1991-2012. Smaller but still major changes took place in the number of registered offenders (80 to 124 thousand) and convicted culprits (65 to 89 thousand). See Figure A3.

Figure A3

Registered cases of crime, registered and convicted offenders in Hungary



The post-communist transition incited the rise of registered crime in several ways. First, the rise of unemployment from virtually zero to two-digit levels and fast-growing income inequality boosted the motivation to commit crime. The emergence of drug traffic and the introduction of personal income and value added taxation had similar effect.¹⁷ The conditions for criminal activity as a lifestyle improved substantially with the abolition of regulations treating joblessness as unlawful. Privatization enhanced the safeguard of property after many years of undisturbed stealing from state-owned enterprises. Last but not least, as organized crime appeared on the scene, more and more culprits were charged with dozens or even hundreds of crimes within a case that tends to inflate the statistics on registered felony.

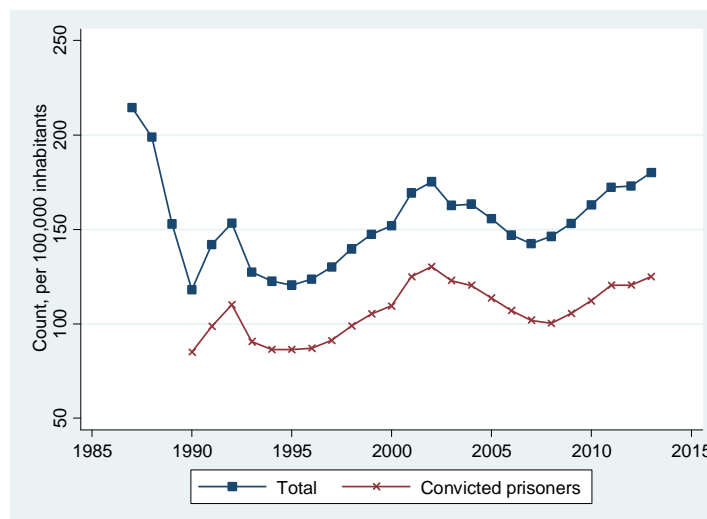
¹⁷Until 1987, payroll taxes were collected directly from state-owned enterprises while the turnover tax was levied on a few wholesale companies, leaving no space for individual tax evasion.

The prison population declined from more than 200 per 100,000 inhabitants 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 reached 180 in 2013.

The capacity of prisons did not grow 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. The overcrowding rate was the second highest in Europe after Italy already in 2011 (Csóti 2011).

Figure A4

The prison population per 100,000 inhabitants in Hungary 1987-2013



Data sources: CSO (2013) for 1990-2013. Eurostat (2014) for 1987-89.
 No data are available for convicted prisoners in 1987-89
 file D:\KOLLOJ\Czafit\KSH\KSH_1987_2013.dta

According to a survey by Aleku et al. (2006) 47.9 percent of the prime age male prisoners were involved in work in April 2005, and 9 percent participated in training. In other categories, the participation rates were significantly lower with the exception of juvenile convicts of whom nearly one-third participated in elementary education. Kőszegi (2010) reports that about 13 percent of all prisoners receive formal education. This is a relatively low rate in view of the fact that three out of four prisoners have only primary school attainment or less, as opposed to only one in four people in the general population.

The relevance of prison experience for the entire labor market is difficult to judge. Using the data introduced in this paper, we can estimate the number of people incarcerated at least once in 2002-2008 to amount to 62,000, almost four times the average stock in prison. A comparison with LFS figures suggests that close to 3 percent of the low educated 16-59-year-

old males were in custody at least once in a period of seven years.¹⁸ We have no sufficient information to estimate the incidence of prison service during the lifetime. The only piece of information we know about this comes from a survey of the Roma population in 1971 (Kemény1976). Using the micro data of the survey we estimate that 16 percent of the 40-60-year-old Roma males had prior prison experience.¹⁹ We think that this ratio has risen since then.

¹⁸Authors' calculation. Low educated stands for people with primary or uncertified vocational qualification.

¹⁹Note that only 1.5 percent of the 40-60 year old Roma males observed in 1971 had higher than elementary education and close to 40 percent of them did not go to school at all. The Roma accounted for about 3 percent of the Hungarian population in 1971 and 6 percent today.