The Costs of Imprisonment: The Effect of Incarceration on Labor Market Outcomes in Hungary

By

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Abstract

This thesis evaluates the earnings and employment chance losses stemming from incarceration in Hungary over the period of 2003-2011. Using a matched employer-employee panel data set, I find that labor market outcomes of the convicts suffer large losses in the short-run, but recover quickly to their own pre-prison levels. However, losses remain large and sustained if we take into account how the labor market outcomes would have developed in the absence of the incarceration, by using a matched non-incarcerated control group. Occupational and firm-level characteristics do not affect the dynamics of the earnings and employment chance losses substantially. High-skilled workers suffer large earnings losses but not a decrease in employment rates, while the low-skilled experience reduced employment chances but no wage losses. The costs of incarceration radically increase with both sentence length and age.

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Introduction

The transition from socialism to a capitalist economy increased the inequalities in the Hungarian society and led to a steady growth of registered crimes and incarceration rates as well (Köllő and Czafit, 2014). The new Criminal Code of 2012 introduced a more rigorous sentencing policy than ever before[1]. The sentences have become longer (especially for recidivists and for drug related crimes), are applied to both a wider range of crimes and a broader population[2] (Weidinger, 2013a, 2013b). In light of these tendencies, it is increasingly important to evaluate the labor market consequences of incarceration.

Imprisonment can adversely affect employment chances and wages due to the loss of labor market experience, the potential depreciation of human capital and the stigma associated with incarceration (Cho & Lalonde, 2005). However, in-prison education, training programs and recovery from addiction may even lead to a more motivated job search and better labor market prospects after release (Cho & Lalonde, 2005). The effect of imprisonment on labor market outcomes depend on how strongly these factors influence the incarcerated. This is determined by the institutional environment, labor market regulations, business cycle conditions and the society's perception of imprisonment (Köllő & Czafit, 2014). Studies estimating the costs of incarceration for different countries, time periods and subpopulation of the convicts often arrive at different conclusions in terms of the sign, strength and time horizon of the effect (see Grogger, 1996, Cho & Lalonde, 2005 and Köllő & Czafit, 2014).

1 The 2010 amendment of the previous criminal code reflected the same rigour and its changes are incorporated into the new code (Weidinger, 2013a).
2 The minimum age of criminal responsibility was decreased to age 12, which is one of the most rigorous regulations in Europe (Weidinger, 2013b).

As incarceration's effect on labor market outcomes is *a priori* not predictable, thorough empirical investigation is needed to evaluate it.

This thesis aims to uncover the wage and employment chance losses stemming from incarceration for the convicts who are the most connected to the labor market. Using a matched employer-employee panel dataset, I analyze the evolution of labor market outcomes before and after imprisonment for convicts, who ever worked during 2003-2011. I restrict the analysis to those who spent at most a couple of years in prison, and are observed before and after their imprisonment.

Comparing the convicts' labor market outcomes after release to their own pre-prison outcomes suggests large short term losses and fast recovery, which is in line with the findings of the literature (Köllő & Czafit, 2014). On the other hand, when the convicts are compared to a matched group of non-incarcerated they seem to suffer higher losses which last longer. These results suggest that labor market outcomes do not recover to their counterfactual levels, which they would have reached in the absence of the incarceration. The analysis by subgroups suggests that longer sentences lead to larger losses, low skilled workers mostly bear the costs in terms of decreased employment rates and high skilled suffer decreased earnings.

Mapping the labor market consequences of incarceration is a question of great policy relevance. If the convicts face too strong obstacles in finding jobs after release, their risk of recidivism increases critically (Holzer, 2007). This also imposes costs on the majority society, as it leads to higher crime rates and new incarceration spells which are financed by the tax payers. This analysis helps to answer the main questions which underlie policy-making for the incarcerated population. Do the released need stronger short run (or long run)

assistance in finding a job? Is there a wage loss after release for those who manage to find a job? Where does this wage loss come from? Is the effect of incarceration differ between subgroups of the convicts?

As most of the previous studies in the topic work with data from the United States, where in-prison training programs and post-release assistance stand at a very high level, my research adds an important piece of evidence to the literature. It examines how the effects of incarceration operate in an institutional environment where such programs have limited scope. Thanks to the exceptional data set, this study evaluates the costs of incarceration using a matched control group for the first time in the literature to the best of my knowledge.

The paper is organized as follows. Chapter 1 discusses the theoretical and methodological considerations and the previous findings of the literature. Chapter 2 details the main patterns of incarceration in Hungary and the institutional aspects driving the reintegration after release. Chapters 3 and 4 present the data set and describe its main tendencies, while Chapter 5 introduces the estimation strategy, the baseline specification, the matching model and the subgroup analysis. The main results are discussed in Chapter 6, Chapter 7 adds robustness checks to the analysis and the last section concludes the paper.

Chapter 1. Literature Review

1.1. Theory

Theoretically, incarceration could have both positive and negative effects on employment levels and wages after release, thus empirical evidence is needed to determine its sign. It is affected by the incentives and constraints the released face, created by the institutional environment of the judicial system, prison system, the local labor market and also the situation per se.

Incarceration is expected to have a negative effect on employment chances and wages after release because of several reasons. First, human capital accumulation usually stops during incarceration or happens at a slower rate than for those not in prison. Moreover, human capital often even deteriorates due to the non-stimulating environment and the absence of meaningful activities during incarceration (Cho & Lalonde, 2005). This could result in a severe disadvantage on the labor market later on. Second, the incarcerated might build a social network of criminals in prison, acquiring the norms and a better knowledge of criminal practices than before. This often leads to recidivism and results in a failure of reintegration (Cho & Lalonde, 2005). Third, the criminal record may prohibit the released to obtain certain jobs. In Hungary, the criminal record bars the released from working at any public institution in any position, which is a serious burden for the released (Csáki & Mészáros, 2011). Fourth, discrimination on the labor market in hiring practices and the stigma associated with incarceration, might be an obstacle which is not easy to overcome. The access of criminal

record for the employers frequently results in unsuccessful job applications for the released (Pager, 2009 and Holzer, 2002). In Hungary criminal record can be asked about during the application process for any positions (Csáki & Mészáros, 2011). These factors may have a negative effect on the chances of obtaining a job and some of them has an adverse impact on the offered wages after release as well.

Incarceration may also affect labor market outcomes positively after release. First, it could increase the motivation of the incarcerated to look for and try to live from legitimate work. Second, if the incarcerated can participate in educational programs and work in prison, that accumulated human capital might make them more employable and provide them higher wages later on. This is especially true for convicts who had low education before the incarceration, which is a general pattern according to the literature (Holzer, 2007). Third, efficient work release programs could boost overall employment levels after release. Such programs allow a subgroup of the convicts to work outside the prison during the incarceration under a strong supervision. It proves to be a useful work experience rewarded by the labor market, and the workers often obtain jobs at these workplaces after release as well (Turner & Petersilia, 1996). Fourth, effective post-release assistance and supervision can play a key role in facilitating the transition from prison to a living supported by legitimate jobs. Comparing research from different countries, this factor seems to have a crucial impact on the short-run labor market outcomes for the released (see Cho & Lalonde, 2005 and Köllő & Czafit, 2014). In Hungary, in-prison training and help in the reintegration stand at a very low level, which means that their above mentioned beneficial effects are mostly missing. Overall, these factors could positively affect both the employment chances and the bargained wages of the released.

There are factors which influence the employment and wages towards the opposite directions and create a trade-off between them. Köllő and Czafit (2014) call it a “reservation wage

effect" when the incarcerated lower their wage expectations upon release and accept lower paid jobs than before just to escape unemployment. In the data it is very difficult if not impossible to determine whether the lower post-release wages of the convicts are due to discrimination on the labor market or stem from their lowered reservation wages.

1.2. Estimation Methods

While theory provides hypotheses about the factors which drive the effect of incarceration to one direction or to another, there are several empirical difficulties which need to be overcome for identifying such effects.

The first difficulty is data availability. As the incarcerated population is a very special one, in nationwide representative, administrative data sets they often cannot be identified. Even when they can be distinguished, the scarcity of individual characteristics can limit the scope of questions which can be analyzed, as the incarcerated may radically differ from the majority population in unobserved factors. Therefore, the literature often draws from records of the correctional system, which are usually rich in individual characteristics and follow the convicts long after release. The limitation of such data sets is that they mostly lack information on labor market careers before the incarceration. Additionally, self-reported income and employment measures of such data sets often prove to be unreliable (Holzer, 2007). Audit studies provide more insights about the hiring process of the ex-convicts, but are small scale and descriptive.

The second large issue in identifying the costs of incarceration is solving the potential biases caused by unobserved heterogeneities with the appropriate choice of the control groups.

First, comparing the evolution of the labor market outcomes of the incarcerated to the outcomes of those who never been incarcerated, that might lead to biases in the estimation. The evolution of labor market outcomes may differ between convicts and non-convicts even in the absence of incarceration, due to the different characteristics of convicts from the majority. Observing only post release outcomes, it is not possible to differentiate between the effect of incarceration and this unobserved heterogeneity. However, collecting data from the period before the incarceration and carefully matching the convicts with non-convicts can mitigate this problem. With such a matching, it is possible to measure the change in the relative labor market position of the incarcerated due to the imprisonment.

Second, as a control group for the released it is possible to use the pre-incarceration outcomes of those who are observed to enter prison later during the time period of analysis (Köllő & Czafit, 2014 and Cho & Lalonde, 2005). This approach compares groups of convicts to each other and filters the unobserved heterogeneity between the incarcerated and the majority society which affect their labor market outcomes. However, this technique has caveats as well. It is not known how much time those spend incarcerated who enter prison towards the end of the observed period with an unobserved date of release. Therefore, in an extreme case it is possible that the comparison is made between those who committed some misdemeanors and those who will be incarcerated for life. In such cases the estimation might be biased.

Third, the post-release outcomes of the convicts can also be compared to their own pre-incarceration levels. This model is able to overcome the biases caused by the time-invariant unobserved characteristics which influence the labor market outcomes of this population with the use of fixed effects. On the other hand, a second type of bias can arise here especially for longer sentences. If the unobserved characteristics, which drive the evolution of labor market outcomes for the incarcerated are time-variant, estimates can be biased. However, controlling

for changing labor market environment and restricting the analysis to those having short incarceration spells, mitigates this bias.

1.3. Findings of the Literature

Holzer (2007) gives a review the main findings of the empirical literature analyzing the effect of incarceration. First, Holzer summarizes the employer side of the problem, i.e. firms' willingness to hire ex-convicts. The cited employer surveys (Holzer, 2006) and audit studies (Pager, 2003 and later 2009), all report a large aversion of employers to hire someone with a criminal record. The type of crime and the responsibilities related to the job play a key role in the level of this aversion. If the crime is revealed to be non-violent, the job requires no contact with customers and involves no cash handling, the willingness to hire increases greatly. Some characteristics of the offenders such as them being African American or Hispanic further worsened their employment prospects. These factors are likely to be reflected in the estimates conducted in this thesis as well; however, the data does not include information on the type of crime and the race of the offenders.

Second, the review continues with analyzing the employee side of the problem, the earnings and employment chances of the incarcerated. From papers using administrative data sets similar to the one of this thesis, the research of Grogger (1996) is the first, and by far the most cited study. He estimates the effects of arrests on employment and earnings of young workers using a large longitudinal data set from the United States. Grogger uses a fixed effect model, in which the own pre-prison outcomes of the incarcerated serve as a basis of comparison for the post-prison ones. He finds that imprisonment has moderate but negative short-run effects on both the employment and earnings of young arrestees. Kling (2006) focuses on the question how the length of incarceration affects the employment and earnings

prospects after release. He controls for observable factors and instruments the incarceration length, using that randomly assigned judges have different sentencing propensity. He finds that an increase in sentence length has no effect on labor market outcomes in the long run but a small positive effects in the short run. In this analysis, the comparison is made between the post-release outcomes of convicts with different length of incarceration.

To the best of my knowledge Cho and Lalonde (2005) are the first ones to adapt the framework proposed by Jacobson et al. (1993) and other displacement studies to the topic of incarceration. As a large fraction of displacement studies estimate the costs of displacement using panel datasets, their methods can be easily applied to this topic. In their main specification, Cho and Lalonde use a fixed effects model to compare the post-prison employment of the incarcerated to their own pre-prison outcomes, not only at a single point in time but during several periods shortly before entering prison and after release. They also compare the outcomes of the released to the pre-prison outcomes of those who are observed to be incarcerated later during the observed period. With both approaches they find quantitatively very similar results for costs of incarceration. Using a sample of female state prisoners from Illinois over the period 1995-2000, they obtain substantial positive effects on employment rate upon release, which dissipates to no effect in the long run.

Köllő and Czafit (2014) serves as a crucial basis of comparison in every respect for this study. They use an earlier form of the data set which is utilized in this thesis[3]. Their study is the only large scale quantitative research on the wage and employment costs of incarceration conducted on Hungarian data. Köllő and Czafit follow the estimation technique proposed by Cho and Lalonde (2005), and use fixed and random effects models to measure the costs of incarceration. They compare the post-release outcomes of the prisoners to their own pre-

3 It is structurally identical to this dataset, but is for a different time period (2002-2008) and lacks information on firm-level variables.

prison outcomes and also use the soon-to be convicts as a control group. They find strong initial negative effects on wages and employment upon release with both methods. While the employment effects dissipate in the long-run and even turn into positive, the wage costs stay significant and strong.

To the best of my knowledge, there is no analysis which measures the costs of incarceration using a non-incarcerated control group and information on the labor market careers before and after imprisonment. However, in the displacement literature it is common to compare the evolution of the earnings of the displaced to the wages of their former non-displaced colleagues. Jacobson et al. (1993) was the first to assess the losses of the displaced by comparing them at several points in time to their non-displaced former colleagues. Couch and Placzek (2010) go beyond this and compares the earnings of the displaced to the wages of similar colleagues to estimate the earning losses. The similar colleagues are identified with propensity score matching, based on characteristics before the displacement. Using this approach to measure the costs of incarceration allows us to take into account how the individuals' outcomes would have developed in the absence of the incarceration.

In this thesis, I mainly build on the methodology of Cho and Lalonde (2006) and Köllő and Czafit (2014) from the incarceration literature and use Jacobson et al. (1993) and Couch and Placzek (2010) from the displacement literature. Additionally, I use insights from both Grogger (1996) and Kling (2006). My estimation is conducted on a large-scale administrative data set, which includes information on the labor market outcomes for the period both before and after incarceration. First, I compare the post-prison outcomes of the released to their own pre-prison outcomes. Second, to obtain a picture on the change of the convicts' relative labor market position caused by the incarceration, I conduct a matched estimation as well. As no

method is perfect, I will detail not only the advantages but also the caveats and potential biases of both techniques in later chapters.

Chapter 2. Incarceration Patterns in Hungary

The transition from socialism to a capitalist economy eliminated full employment and increased inequalities in the Hungarian society. These processes played a key role in the rising number of registered crimes and increasing incarceration rates after the transition and ever since (Köllő and Czafit, 2014). The number of incarcerated in 1990 was 12 241, which grew to 16 507 by 2003, and further increased to 17 210 by 2011 (Hungarian Statistical Office, 2015a), the latter 8 years being the period analyzed in this study. The capacity of the correctional facilities could not keep up with the increasing number of convicts, thus prisons have become and still are severely overcrowded. Between 2008 and 2011, the correctional facilities were operating at 125-137% of their capacities and a large fraction of them were in a pressing need of major renovations (Hungarian Helsinki Committee, 2013).

The reasons behind the overcrowdedness are the widespread practice of pre-trial detention served in prison,[4] the opportunity to change fines and community service to confinement[5] and the increasing rigor of the Criminal Code.[6] Surveys reveal critically poor living conditions in the prisons and show how much the correctional system lacks the sufficient funding from the state (Hungarian Helsinki Committee, 2013). In light of this, it comes as no surprise that

4 There is no legal upper limit on the length of pre-trial detention served in prison in Hungary, thus it could last for years due to the slow delivery of justice. This group consisted of 29% of the prison population of Hungary in 2012 (Hungarian Helsinki Committee, 2013).

5 The 2010 and 2012 Misdemeanor Law enlarged the category of activities classified as misdemeanors and introduced the practice of automatically changing fines and community service to confinement upon delayed or no payment (Hungarian Helsinki Committee, 2013). Prior to this amendment, changing fines to confinement was optional. These laws bring more short sentences for a larger part of society than before.

6 The 2010 amendment of the Criminal Code introduced the "Three Strikes Rule", which requires that a judge has to sentence to extremely long (or life-long) imprisonment an offender, when he commits a violent crime for the third time. Actual life-long imprisonment is also legal in Hungary, however, it affects a small number of convicts only. The Three Strikes Law increases the number of long sentences in Hungary imposing an extreme burden on the correctional system.

funding for in-prison and post-release programs providing education and help for the convicts' reintegration to society is extremely poor.

Based on the report of Váltósáv Foundation (2006), the majority of the incarcerated is from the socially most disadvantaged strata of the society, with little or no education at all. Based on Koszegi and Varga (2013), in 2011 about 46% of the prison population completed primary education only, and some of those even had difficulties with basic reading competencies. A little more than 14% of the convicts had vocational education (11 school years), 4.3% had a diploma from a vocational school (12 school years) and 3.3% from high school. Only about 2.6 % of the population had an even higher education.[7] Despite this very low educational attainment (much lower than the one of the majority society's average) only 74% of the convicts under the age of 18 participated in any kind of educational program during the incarceration and only 13.7% of those above that age.

There are some opportunities to participate in job trainings listed in the National Qualifications Register during incarceration. However, the number of participants is very limited, and trainings are not necessarily for professions which are demanded on the labor market (Váltósáv Foundation, 2006). Working during the incarceration also do not seem to serve efficiently the accumulation of those skills and experience which could be useful later on the labor market. In prison, jobs mostly require no qualifications and include monotonic manual tasks. One-third of the national legal minimum hourly wage is paid for the work and it does not pay superannuation tax to the pension fund (Váltósáv Foundation, 2006). Overall, most reports suggests that in the Hungarian correctional system there is very little scope for acquiring such skills which can improve employment prospects or earnings capacity after release.

7 The numbers do not add up to 100%, as there is no educational data on the remaining fraction of the prisoner population.

After spending the sentence in such poor circumstances and without many chances to increase the human capital during the imprisonment, the incarcerated face a very difficult situation upon release. There is practically no social work in the prisons and thus no help is provided in finding a job during the incarceration (Csáki & Mészáros, 2005). The convicts are assigned to probational officers only upon release to assist their reintegration. However, this help usually comes too late and due to their heavy case load, the assistance provided by probational officers may be very limited (Csáki & Mészáros, 2005). There are no half-way houses which would provide temporary accommodation after release, and the ex-convicts get no financial help to live from till they find their first job. The lack of these forms of assistance in Hungary usually make it almost impossible for the released without family or other help to start a new life living from a legitimate job (Csáki & Mészáros, 2011). Due to the lack of capacities in the incarceration system it is common that the convicts are incarcerated at a great distance from their homes, which reduces the frequency of visits greatly. This weakens those social ties which would help the reintegration after release (Váltósáv Foundation, 2006). NGOs, religious organizations and volunteers are trying to provide some help, but their capacities cannot replace state-level institutional assistance. This weak system of institutions supporting the reintegration and the high rate of unemployment among the low-skilled are key factors contributing to the exceptionally high rate of recidivism in Hungary.[8]

Besides the prison sentence, part of the retribution is the criminal record. In Hungary, one's criminal record is either clean or not and if not, the type of crime is not included in it. Depending on the judgment the criminal record is cleared 3, 5 or 10 years after release. The criminal record is usually asked during the application process for a job and there are no

8 According to the Valtosav Foundation (2006), on average, 24% of all released becomes recidivists, and from the population of recidivists, 33% is a multiple time recidivist.

regulations which restrict requiring it. Based on Csáki & Mészáros (2012) having a non-clean criminal record often automatically results in rejection during the application for legitimate jobs. Besides this, a clean criminal record is necessary for obtaining any public sector job, which bars the released from several positions they would be qualified for. Officially, the National Employment Offices are responsible for providing support for the released, but their success depends on the level of cooperation between the probational officers and the local Employment Offices. The 1996/6 Regulation of Ministry of Social Affairs and Labor (VII. 16.) provides support (tax refunds) for employers when employing ex-convicts. However, the administrative burdens of this are too high, therefore employers often choose not to hire ex-convicts (Csáki & Mészáros, 2011).

Overall, the incarcerated are from the most socially disadvantaged and least educated segments of the society. Neither the regulations, nor the circumstances which shape their lives in prison and after release mitigate these disadvantages, but rather worsen them.

Chapter 3. Data

The data used in this study is a matched employer-employee panel for the period 2003-2011. It collects information from the administrative data of the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the Hungarian State Treasury and the National Labor Office. It is merged with the corporate administrative data set of the Institute of Economics, Hungarian Academy of Sciences (IE-HAS) which uses information from the National Tax and Customs Administration.[9]

The sample is a 25% random draw from the Hungarian population of age 16 to 74 in January, 2003. From that time the individuals were followed till December 2011, or till exiting the sample due to death or out migration from Hungary. The data set includes information on those who ever worked in 2003-2011 and the firms they worked at for the period they were employed there. Firm-level data is reported annually and includes basic performance measures for each of those firms. The individual level data on labor market outcomes, prison status and welfare allowances are recorded monthly. The variables which are measured in Hungarian Forints (HUFs) in nominal terms are transformed to real values using a chained CPI with the average price level of the previous year as a base (Hungarian National Bank, 2012).

9The dataset used in this thesis is a selected sample of the "AdminII" 25% dataset of IE-HAS available to the researchers affiliated to the Institute and with special permissions.

The incarcerated can be identified from the data, as the state pays their social security contribution while they are in prison.[10] Based on the available information pre-trial custody and detention spent in prison cannot be differentiated from actual prison sentence. No information is known on the type of crime or the date when the criminal record is cleared. On the other hand, it is observed if the incarcerated work in prison and earn wages.

Data on labor market outcomes contains the number of insured days per months and nominal wages in HUFs. The insured days include the days at work and the weekends which are after and followed by days when the individual was employed. Without a major loss of generality, as it is never known how many weekdays the number of insured days reflect, I treat this measure as an approximation for days at work in the subsequent analysis. The days and wages belong to the highest paid job of the individual and so is the firm level data. The wages are used as average daily real wages earned in a given month. No information is available on the hours worked, whether the job is a part-time or a full-time position. The occupation is measured with the two digit International Standard Classification of Occupations (ISCO) code and an additional skill which was included in my original data set.[11] Data on education level is not available, but based on the occupational information can be approximated. Besides labor market outcomes, information is available on gender, age and the reception of disability pension or care allowance. The region level home address of the individuals is also recorded.

10 It has a different administrative code from other groups for which state pays the social security contribution (e.g. armed forces).
11 It is an occupational category measure by skill which contains seven types of jobs. It is calculated based on ISCO codes.

The firm-level data used in this analysis includes the firms' number of employees, industry codes[12], revenues per worker and ownership status (foreign/domestic private/domestic public).

[12] NACE-2 codes, Statistical Classification of Economic Activities in the European Community

Chapter 4. Descriptive Analysis

The unrestricted sample includes individuals who were incarcerated for some time but worked at least one day in a legitimate job during 2003-2011and those who were their colleagues before and after the incarceration. From this population, I analyze the effect of incarceration for those who had a single incarceration spell and are observed before and after the imprisonment. This subpopulation of the incarcerated is likely to be the most connected to the labor market from all convicts, and thus is expected to be the most similar to the non-convicts in the sample. The research does not aim to map the labor market outcomes of incarcerated in general. It rather focuses on how incarceration affects the evolution of labor market outcomes of those who were working before their imprisonment, spent some time (at most several years but not decades) in prison and made effort to work afterwards.

First, data errors were corrected whenever it was possible, and observations were excluded if the source of the error was non-obvious. The wage data was censored by excluding observations which were below the real minimum hourly wage of a given year (as hours worked per day is not observed) or they fell into the top 0.1% of the distribution. Firm level data was also cleaned by excluding the top 0.1% extreme observations of the revenue and size distributions.

I excluded those individuals who were not Hungarian citizens and those who died during the observed period. These groups contained few individuals and their heterogeneity would have introduced noise into the estimation. I focused on those who were between the ages of 16 and 65. This is because only a few individuals outside this age range had legitimate jobs and for

those older than 65 retirement patterns could influence the outcomes. The sample is restricted to males who formed 94% of the incarcerated population of the sample. Based on Cho and Lalonde (2005) female convicts might be radically different from males in terms of types of crimes (non-violent) and motivations to work (custodial parents of children). In the data at my disposal, female prisoners were even less connected to the labor market than males which made my estimations for labor market outcomes very noisy. However, analyzing women's outcomes on a larger sample would be interesting.

Table 6 in the Appendix presents the average characteristics of the incarcerated and non-incarcerated groups in the sample.[13] The statistics show that the incarcerated are on average younger and employed less often. The incarcerated work in 25% of the months they spend outside prison, while the non-incarcerated work in 56% of the time. Those who work, have on average the same number of insured days (28-29 days) per month, but the incarcerated who work earn lower daily wages, 2427 HUFs versus 3922 HUFs. The incarcerated are overrepresented in blue-collar and unskilled positions as well as at smaller firms with less revenue per worker. This data suggests that the incarcerated have inferior labor market outcomes, especially in terms of employment. Thus a careful matching would be needed if one would like to compare the outcomes of the incarcerated and non-incarcerated.

The estimation procedures used in this thesis require the exclusion of several subgroups of the incarcerated from the estimation. The continuously incarcerated are excluded as they never work and earn wages on the regular labor market. Those who have left-censored incarceration spells (i.e. they are in prison in the beginning of the observed period without a

13 It is important to note that the non-incarcerated population of this sample is not necessarily representative of the majority society, as they were selected to be the ones working at firms in which an ex- or a soon-to-be convict worked. This means that comparison of the incarcerated and non-incarcerated groups of my sample gives only the approximation of the true differences between the incarcerated and the majority society in general.

known starting date of the imprisonment) are excluded, as they have no observed pre-prison labor market outcomes. Those who have right-censored prison spells (i.e. with no observed date of release during the analyzed period) are not used in the analysis as they have no recorded outcomes after release. The recidivists having multiple incarceration spells can only be handled with a modified version of my estimation procedure, which is implemented in Chapter 7.2.

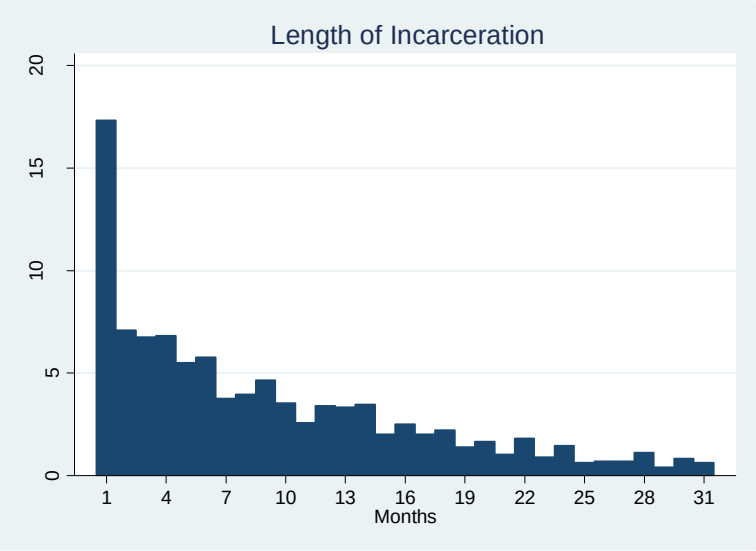
Finally, my sample of convicts includes 1712 individuals who had a single uncensored incarceration spell during the period of 2003-2011. Table 1 illustrates the proportion of the subgroups of incarcerated by spell types, and Table 7 in the Appendix provides a more detailed account of their characteristics. Table 7 shows that while the outcomes of the incarcerated are radically different from the outcomes of those who were never incarcerated, their subgroups are relatively similar to each other. This is line with the suggestions of Csáki & Mészaros (2005, 2011): the overwhelming majority of the incarcerated population comes from the most disadvantaged stratum of the society with poor labor market prospects, which might push them towards crime. This could mean that some of the findings calculated for those who have single uncensored incarceration spells, can at least partly be generalized to other subgroups of the incarcerated.

Table 1. Incarcerated by Prison Spell Type

	Frequency	Fraction	Average Sentence Length (Months)
Single uncensored	1 712	0.52	14.0
Single left censored	392	0.12	10.5
Single right-censored	346	0.11	13.6
Recidivist	815	0.25	11.4
Continuous	14	0.00	108.0
Total	3 279	1.00	

Notes: The table contains information only on male convicts, age 15-65, Hungarian citizens, who worked at least a day in a legitimate job during 2003-2011.

Figure 1. The Length of Incarceration



Notes: The figure contains information on male convicts, age 15-65, Hungarian citizens, who worked at least a day in an official job during 2003-2011. The top 1% by sentence length is excluded for better visibility.

It is important to note here that by focusing on the population having a single uncensored incarceration spell, a positive selection is applied. Data availability and the limits of the estimation technique exclude recidivists, those having long sentences and those who never had a legitimate job. As suggested by Váltósáv Foundation (2006) since the association between not finding a job and recidivism is particularly high we may exclude those from the sample who bear the highest costs of incarceration in terms of employment chances. However, from the data it would be impossible to uncover whether the recidivists in the sample are "career criminals" or those who turned to crime as a response to unsuccessful attempts to obtain legitimate jobs. I return to this issue in Chapter 7.2.

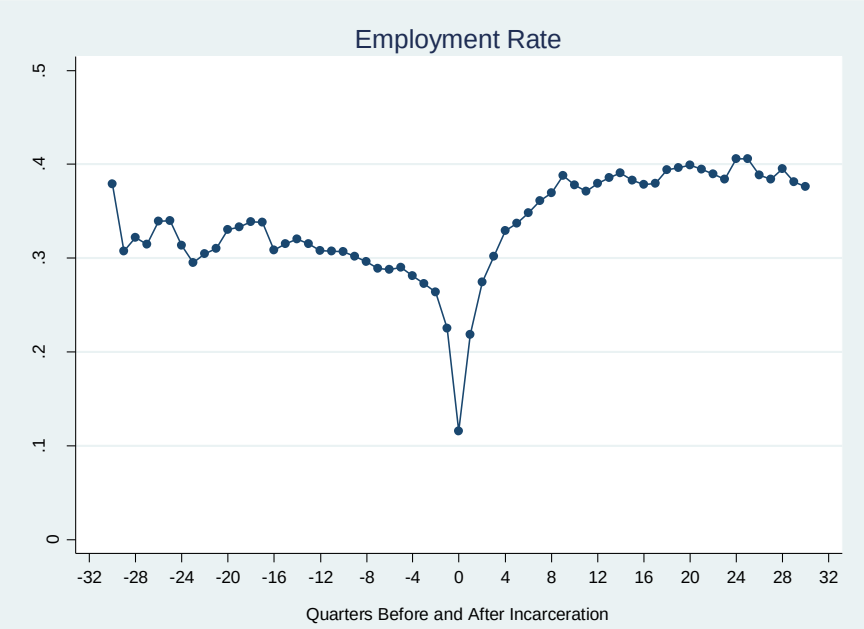
Before turning to the actual estimations, it is crucial to see how long the sentences are and how the raw wage and employment data looks like before and after the incarceration. Figure 1 shows the distribution of the sentence lengths in the sample for those who have a single uncensored incarceration spell. The graph shows a heavily skewed distribution: while the

average sentence length is a little longer than a year, the median sentence length is only 8 months. The quarter of the sentences were shorter than 3 months, while a little more than 17% of the convicts spent at most one month in prison. These short sentences are served for misdemeanors or possibly in pre-trial custody (although the data does not specify this).

Figure 2 and 3 show the evolution of the employment rate and the average real daily wage (for those who work) for quarters before and after incarceration. It is for the incarcerated who have single uncensored incarceration spells. The horizontal axis shows the "relative time", i.e. time compared to the period of incarceration. The length of the prison sentence is collapsed to a single quarter in time, which is shown by the zero point of the axis. The negative values of relative time refer to quarters before entering prison, positive values measure the time after release in quarters. The employment measure shows the average of the employment dummy (which takes value one if a person has at least one insured day in a given quarter) for those individuals who were observed in a given relative quarter.[14] Similarly, the real wage observations are averaged for relative quarters too.

14 It is important to note that the number of observed individuals for each relative quarter is different. While most incarcerated are observed right after and before the incarceration, few of them are observed over four years before entering prison or after release. For instance if one enters prison in the second half of the observed period he will not be observed long after release. Similarly, the individuals with very long sentences are not observed long before and after incarceration.

Figure 2. Employment Rates Before and After Incarceration



Notes: The figure uses information on male convicts, age 15-65, Hungarian citizens, who worked at least a day in an official job during 2003-2011. Each point shows the employment rate of the population who are observed in a given relative time period.

Figure 3. Wages Before and After Incarceration

Notes: The figure uses information on male convicts, age 15-65, Hungarian citizens, who worked at least a day in an official job during 2003-2011. Each point shows the average daily real wage of those who worked and are observed in a given relative time period.

From Figure 2 it is clear that during the observed period, convicts have very low employment rates. The average employment rate is 30-35% before the incarceration's effects start to work and between 35-40 % long after release. As a comparison, the average employment rate for working age males in 2003-2011 was 64.7% (Hungarian Statistical Office, 2015a).

The decrease of employment starts slowly, about 3 or 4 years prior to incarceration, and it starts falling steeply in the year right before entering prison. There may be several explanations for this pre-prison dip in the employment rates. It is possible that individuals who lose their stable employment turn to committing crimes to earn a living, or the engagement in criminal activity provides the individuals with incomes which make regular work unnecessary. It is not possible to measure the extent of informal work in the sample, which is likely to be especially widespread for the incarcerated. It is also unobserved when the trial period starts before the conviction. Trials require the presence at court and are often held in other regions of the country than the individual lives (if the crime was committed in another region). It can cause job loss if the employee has to take too many days off from work or if the employer learns that an employee has to stand trial (Csáki & Mészáros, 2011).

During the incarceration period, about 10.2% of the inmates worked. Two points are important to note here. First, the length of the incarceration period is normalized to a single point in time, so this 10.2% includes those who ever worked during the incarceration at least for one day. Second, this employment is very different in nature from finding a job on the labor market outside prison. It is not trivial from the data whether the in-prison labor market rewards the same skills as the labor market in general. The employment status during incarceration is not highly correlated with strong employment history or being high skilled in this sample.

After incarceration, the employment rate recovers quickly to its pre-prison level, even compared to the levels before the pre-prison dip started to become visible. More than 2 years after release, the employment rate stabilizes at a higher level than before the incarceration. However, this is a recovery to a low level of employment rate and does not take into account that employment rates could have grown in the absence of the incarceration.

Again, several drivers can affect this tendency. First, as discussed earlier, the figure shows a positively selected part of the prison population, who are the most successful on the labor market after release. Second, it is possible that after release, the individuals try hard to earn a living from a legitimate job and may be willing to accept even worse positions than before. Third, based on Csáki and Mészáros (2012), getting and keeping the first job is the biggest challenge for the released and the stigma in hiring often fades once they had their first employment spell. If such a tendency exists, that would mitigate the long-run employment effects of the incarceration. In Hungary, criminal records are cleared after 3, 5 or 10 years following the release depending on the sentence. Although the exact date of clearance is not known from the dataset, we do not see visible spikes in the employment rates after 3,5 or 10 years which suggests that clearance only had a minor effect for this population.[15]

Figure 3 illustrates the evolution of the average daily real wages of those who worked before entering prison and after release. Before entering prison, the daily wages are in the range of 2000-2500 HUF, which is about 50-60% the Hungarian average daily real wages for that period (based on Figure 9 in the Appendix).[16] Wages show a similar pattern to employment rates: they fall steeply during the year preceding the incarceration. In prison, the earnings are

15 However, it is important to note that most of those who are not able to find a job shortly after release end up in prison thus they are not included in this figure. It may be the case that they could not find jobs especially due to the criminal record, but we do not observe them in this data set.

16 It is important to note that the incarcerated have below average qualifications as well. Thus Figure 9 in the Appendix is only an illustration of the relative position of the inmates, without taking into account such skill differences.

capped at one third of the minimum wage. During the first year after release, the wages seem to recover quickly and rise persistently to levels over 3000 HUF, about 60-70% of the average real wages for the period (Figure 9 in the Appendix). Apart from the disruption of the incarceration, the earnings seem to follow an increasing trend, which is becoming especially pronounced after the incarceration. Figure 10 in the Appendix illustrates not only the averages but the distribution of earnings before and after incarceration. The wage distribution after the release is less skewed and is shifted towards higher wages compared to the before incarceration ones. Again, this recovery does not take into account that wages could have grown in the absence of the incarceration.

There can be several explanations behind the described wage dynamics. First, it can be argued that the growth of the real minimum wage during the observed period increased earned wages as well. As large fraction of the incarcerated works in jobs requiring little or no qualification, they earn wages close to the national minimum wage which are likely to be influenced by its regulated amount. Reported earnings at the minimum wage may also serve tax evasion purposes. Figure 14 in the Appendix shows the evolution of earned wages and the minimum wage in the sample for illustration. Second, generally increasing age-earnings profiles could also be responsible for the growth of earnings. It might be the case that the accumulation of human capital during work increases wages, which offsets the effect of incarceration over time. Third, it is possible that those who are employed work more days in a month, having not only occasional jobs but permanent positions which usually pay better. However, Figure 8 in the Appendix shows that those who work, don't work more days on average after the incarceration.

Chapter 5. Empirical Strategy

5.1 Baseline Model

5.1.1 Specification

The methodology of the baseline model originates from a displacement study by Jacobson et al. (1993) and was later adapted to the topic of incarceration by Cho and Lalonde (2005). The estimation strategy compares the evolution of earnings and employment of the incarcerated to their own labor market outcomes long before entering prison. The comparison is not only made at one point in time after the displacement, but during several periods both before and after incarceration. Equation 1 shows my baseline specification in its most flexible form:

Equation 1. Baseline Model

$$y_{it} = \boldsymbol{X_{it}'\beta} + \sum_{-K}^{K} \delta_k \, Relative\ Quarter_{it}^{k} + Calendar\ Time\ Dummies_t + FixedEffects_i + \epsilon_{it}$$

The dependent variable y_{it} is either the employment status or the wage of the individual for each month depending on the specification. These are explained by observed time varying controls, X_{it}, relative time dummies, individual specific effects and calendar time dummies.

The employment status variable is a dummy which takes the value one if an individual had at least one day at work (one insured day) in a given month. The wage variable is used as average daily real wage per month in its natural logarithm. The wages only take values when a person is employed and are missing otherwise. Although some of the convicts are employed

and earn wages in prison, as those are determined by different dynamics than the ones emerging on the labor market, they are not used in the estimations.

The time-varying controls, depending on the specification, include age, the reception of disability or care allowance, the employment spell characteristics of the individual (occupation category and tenure) and the characteristics of the firm the person works at in a given month (size, revenue per worker, ownership status). The disability allowance is a dummy taking value one in a month if an individual receives allowance from the state. It is not prohibitive of working, but is a signal that the individual has decreased ability to work. Care allowance is also a dummy which takes the value one if an individual received allowance for taking care of a sick/disabled child, infant or a relative. Similarly to the disability allowance, work during its reception is allowed, but is less likely. Occupational categories are measured with a series of dummies taking showing whether a person works in a white-collar, skilled blue-collar, blue-collar or unskilled position in a given month. Firm size is measured with four categorical dummies based on the firm's number of employees: 0-4, 5-50, 50-300, more than 300 employees. The revenue per worker variable is used in real terms and in natural logarithms. The ownership status is a binary variable showing whether the firm is foreign owned (has a majority foreign share).

The relative time dummies show how many periods before entering prison or after release the individual is in a given calendar time period[17]. These are the main coefficients of interest, as they capture how the earnings and employment status of the incarcerated develop over time before and after the incarceration. The duration of the prison sentence is normalized to a single point in time regardless of its length (following Cho and Lalonde, 2005). In most

17 For example if the relative quarter (-1) dummy takes value one in the first quarter of 2005 for an individual, that is his last quarter before entering prison. The incarceration will start in the second quarter of 2005. If the relative quarter (+1) dummy takes value one in the first quarter of 2006 that is the person's first quarter as a released. In this case the person is incarcerated from the second to the last quarter of 2005.

specifications the baseline period, to which the evolution of the outcomes are compared, is more than three years before the entering prison. This timeframe is chosen based on the dynamics revealed by Figure 2 and 3.

The individual specific fixed effects account for time-constant unobserved heterogeneity across individuals. The time fixed effects (the series of calendar time dummies) proxy the general business cycle conditions which affect each individual uniformly at a given period. These dummies also filter the potential seasonality in the data. Clustered errors are used to handle possibly correlated shocks in the labor market career of each individual.

5.1.2 Identifying Assumptions

As discussed earlier, a great advantage of my estimation technique is that it filters time-constant unobserved heterogeneity across individuals from the estimated effects of incarceration. However, this specification gives an internally valid estimation for the costs of incarceration only under certain assumptions.

First, in this specification it is assumed that there is no such unobserved heterogeneity which would drive the labor market outcomes to be different before and after incarceration other than the fact of imprisonment. This specification does not take into account how the labor market outcomes would have developed in the absence of incarceration. If they would have improved, for instance due to the accumulating work experience, the costs of incarceration are underestimated as they do not include the foregone earnings and the lost growth of the employment rates. If the outcomes would have deteriorated, for instance due to business cycle conditions, the costs of imprisonment may even be overestimated. With this estimation technique such biases cannot be eliminated, however, the use of time fixed effects and restricting the analysis to those with short sentences mitigate the biases.

Second, selection biases into employment may also plague the estimated coefficients in the wage regressions. Positive wages can only be observed for those in the sample who work. As those who work are non-randomly selected from the population, the coefficient estimates of the wage equation may be inconsistent because of sample selection biases. Such biases are mostly corrected with Heckman's procedure in cross section analysis. However, in panel estimation individual fixed effects take care of time constant heterogeneity influencing the selection to work (Dustman & Rochina-Barrachina, 2007). The problem stems from the fact that this selection based on unobserved factors might change in time. In this thesis, similarly to Jacobson et al. (1993) and Köllő and Czafit (2014) such selection bias will not be controlled for, but taken into account when interpreting the result.

5.2. Matching Model

The seminal displacement study of Jacobson et al. (1993) also measures the earnings losses of displaced workers by comparing their wages to the earnings of their former colleagues who were not displaced. This method not only compares the individuals' post-displacement earnings to their own pre-displacement levels, but to the counterfactual level of their earnings in the absence of the displacement. This method cannot be implemented on this sample easily, as the incarcerated have very spotty employment history and often they are registered as individual entrepreneurs when working. Focusing on those incarcerated who worked and had colleagues in a given period radically reduces the sample size. Moreover, when similar colleagues were chosen as a control group, the sample was further reduced and the analysis was not possible.

Couch and Placzek (2010) revisit Jacobson et al. (1993), and besides re-estimating the above mentioned model they also use propensity score matching to find a control group for the

displaced. Their estimation technique includes three steps. First, by a logit model they estimate the *ex ante* probability of getting displaced later, given the prior characteristics of the individuals. Then, they identify the closest matches for the displaced from the group of non-displaced, based on this estimated probability (the propensity score). Third, using the matched pairs, they estimate the average effect of displacement for the displaced. Their propensity score model gives almost identical results to Jacobson et al. and has an important advantage which makes it suitable for this study. In cases when exact matching cannot be done, i.e. when for a given combination of characteristics no pairs of treatment and control individuals are found, propensity score matching can still be implemented.

In this section, I adapt the approach of Couch and Placzek (2010) to the topic of incarceration. I match each convict with a non-convict based on the propensity score and evaluate the losses stemming from incarceration by comparing the labor market outcomes of the two groups. This estimation allows me to take into account how earnings would have developed in the absence of the incarceration, thus potentially gives a richer estimate than the one introduced before. On the other hand, the matching assumes that based on the characteristics used, the treated (incarcerated) and the control (non-incarcerated) individuals in each pair have equal chances for getting incarcerated, which is a rather strong assumption.

Incarceration depends on many factors which are unobserved in this study and it affects a small and selected group of the society. Even with the most careful matching when comparing incarcerated and non-incarcerated using administrative data, it is possible that the estimation compares two very different groups in terms of unobserved characteristics. Thus, when conducting this estimation I do not believe that the match is perfect and my results give an unbiased estimation of the effect of incarceration. However, I think that the estimation

gives a good illustration of the main tendencies, how incarceration may change the individuals' relative position on the labor market.

I selected the calendar quarter more than three years before incarceration (relative quarter -13) to implement the matching. This is before the time when the labor market outcomes start to deteriorate prior to entering prison. In this calendar, time I estimated the propensity score, the probability of being incarcerated later, with a logit model.[18]As control variables, I used age, age-squared, daily real wages and occupational category dummies.[19] The same regression is estimated in every calendar quarter when the matching is implemented. Trying several alternative specifications, this estimation had the highest explanatory power and gave the most balanced matching for all years on average. This matching assumes that those with a similar age and employment characteristics would have similar evolution of labor market outcomes in the absence of the incarceration of one group.[20]

The estimated pscore is used in this analysis to find the most similar non-incarcerated control group for the incarcerated. Trying several matching methods and options, I chose to estimate a logit model for the pscore as a nearest neighbor matching with caliper (0.001), with no replacement and using observations only on the common support.[21] This method matches the

18 After a random sorting of the individuals, the psmatch2 module of STATA was used to implement the matching and the balancing tests (Becker and Ichino, 2002).

19 The daily real wages in this example are set to zero when the individual is not working, thus they include information on the employment status and the earnings of those who work as well. The occupational categories are the blue-collar and unskilled dummies. The omitted category is the white-collar position. All variables are measured quarterly in the matching regressions.

20 Naturally, this estimation gives a better match to those who are employed and have occupational measures in the sample. However, not many variables are available to make match for those who are non-employed. Regional codes, approximated skill levels and allowance reception status proved to be entirely insignificant and did poorly on the balancing tests as well. Tenure and industry controls were also tried but performed poorly.

21 The radius and k-nearest neighbor estimates gave very similar results to this one. Methods which use weighting were difficult to implement as required defining a weight cut-off, based on which some of the matched control observations were discarded and some were used. I could not find a universal cut-off which worked well for all quarters when the matching was implemented. Some chose to drop too few, others dropped too many matched control individuals. Therefore, the kernel matching and other weighted matching methods are not discussed here.

most similar non-incarcerated to each incarcerated based on the given characteristics, provided that there is one which is similar enough.

Table 9 shows the results of the balancing tests which were conducted to evaluate the match quality. The tests were implemented on the unmatched sample before the matching and on the matched sample as well in each quarter. It reveals large differences in the average characteristics of the groups before the matching, but a5lso their striking similarity after the matching. The matched incarcerated and non-incarcerated pairs from each calendar quarter are pooled together for an overall estimation sample of 815 incarcerated and 815 non-incarcerated individuals.[22]

On this sample the same fixed effects specification is estimated which is introduced in Chapter 5.1. The main difference that in the matching model, the outcomes of the incarcerated are compared to the matched control group's outcomes. In this specification the relative time dummies always take the value zero for the non-incarcerated and work the same way for the incarcerated as before.

5.3. Subgroup Analysis

Using different control groups both the baseline and the matching model estimated the costs of incarceration with a fixed effects specification. This estimation technique cannot estimate coefficients for time-invariant controls. By interacting the relative time measures with different subgroup characteristics, I analyzed whether the labor market outcomes evolve differently for certain groups of the sample.

22 The first 22 calendar quarters from the 36 were used in this analysis to implement the matching. None of the incarcerated from the matches in the 23rd quarter or later had observations after release, due to the constrained time horizon.

The estimated equation for *k* number of subgroups looks the following way in each estimation.

Equation 2. Subgroup Analysis

$$y_{it} = \beta_0 + \beta_1\, Age_{it} + \beta_2 Age_{it}^2 + \sum_1^K \beta_{k3}\, Subgroup_{ki} * Shortly\ Before_{it} + \sum_1^K \beta_{k4}\, Subgroup_{ki} *\ Shortly\ After_{it}\ + \sum_1^K \beta_{k5} Subgroup_{ki} *\ Medium\ After_{it} + \sum_1^K \beta_{k6}\, Subgroup_{ki}\ * Long\ After_{it} + Calendar\ Time_t +\ FixedEffects_i + \epsilon_{it}$$

The quarterly relative time dummies are grouped into five categories for the purpose of this analysis to simplify the estimation and interpretation of the interactions. The baseline period, which is omitted to serve as basis of comparison, refers to times earlier than 2 years before entering prison. The Shortly Before dummy takes value one if an individual *i* at a given calendar time *t* is at most two years before entering prison.[23] The Shortly After dummy refers to the period within the first year after incarceration, the Medium After dummy to times between one to three years after incarceration, while the Long After dummy to the period after that. These dummies are interacted with the subgroups of individuals in the sample. This allows each subgroup to have its own dynamics of the incarceration's effect. The equations are estimated with quarterly calendar time dummies, individual fixed effects and clustered errors on individual level.

First, subgroups are defined based on the regional address of the individuals, as location can largely determine labor market outcomes. Employment opportunities are in general more meager in Eastern and North-Eastern part of the Hungary as opposed to the Western parts and

23 In the previous specification the baseline period is more than 3 years before entering prison. The time frame is more restricted in this equation as the subgroups include fewer observations for each relative quarters.

Budapest. On the other hand, crime rates are also higher in those parts which could reduce the stigma following a period of imprisonment (Hungarian Statistical Office, 2008).

Second, the sample is also separated by birth cohorts (of a decade) to analyze whether the outcomes of young or old workers suffer more from the effects of incarceration.

Third, as unfavorable business cycle conditions may affect post-release employment chances, I control for whether the release happened during the 2008 crisis.

Fourth, three subgroups are defined based on the incarceration lengths of the convicts in the sample: (1) those who spent not more than a month in prison, (2) those who spent more than a year prison (3) those who are in between those two. It could be the case that short sentences hurt the labor market career less, but it is also possible that motivations for finding legitimate work are stronger for those having longer spells.

Further interactions try to control for skills and the overall "quality" of the workers. As the dataset includes no information on educational levels, these specifications attempt to proxy skill level from job types and employment history. First, I divide the sample based on whether the individual ever worked in a high-skilled position during the observed time period.[24] Second, groups are formed based on completed tenure length before the incarceration period.[25] High tenure might be a sign of an individual having characteristics rewarded by the labor market but unobserved in the data. Thus, for high tenure workers faster recovery after release can be expected. Third, individuals are grouped based on their most

24 The first three occupational categories based on the 1 digit ISCO codes require some professional qualification, thus are considered as skilled positions in this analysis.
25 Those are classified as high tenure workers in this analysis who had an employment spell which lasted at least two years. This measure is calculated based on jobs more than 2 years before entering prison. Later than that employment rates start to decrease and employment spells might end because of the trial period.

frequent job long before incarceration.[26] Their typical jobs are likely to reflect their unobserved education and skill levels. It is possible that for the unskilled the employment rates, while for high-skilled the wages suffer more. Fourth, I also treat separately those who were working during the incarceration. However, it is not obvious how it is decided who works in prison, the selection to into in-prison employment may correlate with success on the labor market.

Finally, I analyze whether the industry in which the convict most frequently worked at before incarceration is a determinant of the losses. Some of the industry categories, such as the construction or the manufacturing sector might provide homogenous and interesting subsamples for analysis.[27]

26 This measure is calculated based on jobs more than 2 years before entering prison. The categories are formed based on the mode of the occupational codes of the individual. In case of multiple modes or no mode, the job requiring the least skills was chosen, to be on the safe side and avoid overestimation. If a person had no jobs during the period the variable has a missing value.

27 Industry categories were created by grouping the NACE codes into fewer categories for easier interpretation, (Hungarian Statistical Office, 2014b)

Chapter 6. Results

6.1. Baseline Model Results

Figure 4 shows the coefficients of the relative time dummies from the baseline specifications with their 95% confidence bounds. The relative quarter dummies cover three years before and four years after the incarceration. This means that the model compares the evolution of labor market outcomes to their levels more than three years before incarceration. The full table of estimation results is included in Appendix Table 8.

6.1.1. Employment

The baseline model with employment rate as a dependent variable includes only age, age squared and the reception of disability or care allowance as controls. Apart from these variables, it is mostly the workers' time invariant characteristics which explain their employment status. The effect of those is discussed in Chapter 6.2.

Figure 4 suggests that compared to the baseline period, employment rates already start to decrease slowly but steadily more than three years before the incarceration and fall steeply in the last year before entering prison. In the last non-incarcerated quarter the employment rate is 8.5 percentage points lower than in the baseline period. Recalling that the employment rate during the baseline period was only a little higher than 30% (even with a very loose definition of employment), such decrease leads to a drastically low employment rate. Based on the data we cannot infer the direction of causality, whether the loss of employment pushes the

convicts towards crime or the trial period and criminal activity decreases the employment rate.

For this population, the recovery of employment rates to their pre-prison levels is strikingly fast. In the first quarter upon release, the employment rate reaches its lowest level, a loss of 12.8 percentage points, and in the second quarter it is 6.5 percentage points lower than before. However, by the end of the first year and in the following period the employment rate is not significantly different from its level in the baseline period. This estimation technique for this selected population of the incarcerated finds only short-run employment losses. These tendencies are in line with the results of Köllő and Czafit (2014) who found the initial negative effects which turn into no effects, then positive effect in the long run.[28]

Interestingly, the estimated employment effect turns into positive, starting from the third year after release. Although there is no information in the dataset on the clearing date of the criminal record, legally, the earliest date when it can be cleared is three years after the release. Thus, this positive effect may at least partially stem from the cleared criminal records.

The age variable has the expected positive and diminishing effect on the employment rate. Disability and care allowance reception decreases employment rates in a given quarter by 15 and 10 percentage points respectively. Although these effects are very strong, only a small fraction of the population is affected by them. The quarterly calendar time dummies which proxy general economic environment are mostly significant and positive, with a few

28 Kollo and Czafit compare the evolution of employment to a baseline period more than two years before entering prison. They estimate a less flexible specification and report a shortly before incarceration effect for two years prior entering prison (-4.7% points) and an effect for the first 2 years after release (-4.6% points) compared to the baseline. Their dependent variable is the proportion of days worked in a month from all days of a given month.

exceptions when they are insignificant. This is even true for the end of the observed period which includes the 2008 crisis and its aftermath.

6.1.2. Wages

Three main specifications are estimated with wage as a dependent variable: (1) the baseline specification includes only the age, age-squared as control variables additionally to the individual and time fixed effects; (2) the baseline regression is extended with job characteristics (tenure and occupation, which encompasses the white-collar, skilled blue-collar, blue-collar, unskilled dummies; (3) the regression is extended further with firm characteristics (size categories: 0-4, 5-50, 50-300, more than 300 employees), revenue per worker (in real terms natural, natural logarithm) and the firm's ownership status (foreign, non-foreign).[29]

The first specification captures the basic tendencies in the evolution of earnings. The care and disability allowance variables were excluded as they were highly insignificant, suggesting that their reception mostly affects the decision to work, but not the wages once the individual is employed. The second equation aims to filter the bias stemming from the possibility that individuals may earn less because they work in positions requiring lower level of skills after release. The third specification goes even beyond this accounting for the possibility that individuals may earn less after release as they work for different firms than before the incarceration. Similarly to the employment regression a full set of interactions are used to conduct a subgroup analysis using the baseline specifications as well. Those results are presented in Chapter 6.3.

29 The omitted categories in the regression are the white collar occupations and the non-foreign owned firms in the smallest size category.

The baseline wage regression which is estimated only for those who work shows that wages start falling later than the employment rates do; the estimated coefficients are only significant in the last half year prior to entering prison. From Figure 4 and Table 8 in the Appendix it can be inferred that six months before entering prison the daily real wages for those who work are already more than 12% lower than in the baseline period. In the last quarter before prison wage losses reach 30.2%. These losses are drastic if we take into account the low average wages of the soon-to-be convicts which were about 2500 HUF, 60% of the average daily wages in Hungary during the baseline period.

The recovery in terms of wages is stunningly fast as well; however, the losses last longer than the losses in terms of employment rates. Starting from 22.5% lower wages than the baseline upon release, by the end of the first year the wages are only 9.4% lower than before. More than one and a half years after release, the wages are not significantly different from the baseline. Unlike in the employment rate regression, the effects on the wages do not turn into positive over time. Köllő and Czafit (2014) finds similar tendencies, but longer-run effects.[30]

The age has the same positive but diminishing effect as in the employment regression, however, the calendar time dummies may have an additional interesting interpretation in this specification. There is a yearly jump in the magnitude of the coefficients, which is likely to mark the annual increase in the legal minimum wages in Hungary. Figure 14 in the Appendix suggests that annual hikes in the wages may be related to the minimum wage regulations which the calendar time dummies filter (along with some seasonality).

The second wage equation includes tenure and occupational categories as well. It aims to evaluate whether the wage losses stem from the fact that individuals work in positions which

30 Unlike this study, Köllő and Czafit use a wage measure which is the wage compared to the national average wages in Hungary. That measure reflect well the relative position of convicts in the wage distribution on which the incarceration seem to have a more lasting and scarring effect.

pay lower wages and that they might fall into a lower part of the wage distribution within an occupational category than before.[31] Tenure proves to be a significant control with small positive coefficient. Compared to white collar jobs, all occupational categories requiring less skills pay lower wages. Their coefficients are high and significant with the exception of the other blue collar occupation category.[32]

For the period shortly before incarceration and shortly after release, the factor that individuals earn less because they work at positions requiring less skill might play a role, as the coefficients are smaller than before. Figure 4 and Table 8 in the Appendix show that the wages in the last two quarters prior to entering prison are 12% and 27% lower than in the baseline period. The incarcerated earn 20% and 12.5% less in their first two quarters after release than before. After this point, the estimated coefficients are higher and show a more prolonged decrease in wages than in the previous specification. This suggests that wage costs stemming from worse positions and shorter employment spells hurt the outcomes right before and after the incarceration. However, there may be longer term disadvantages within occupational categories. Overall, occupational categories in general do not change the baseline results substantially.

The third specification includes additional firm level controls to test whether wage losses after release stem from that individuals work for firms which pay lower wages and that they

31 Including these variables in the regression introduces the econometric problem of outcome variables into the specification. These occupational characteristics result from the bargain between employer and employee thus can be outcome variables as well. A separate estimation could be conducted to analyze how the individuals of the sample are sorted to certain positions. The exact magnitude of the occupational coefficients is not the main interest of this analysis. Their inclusion in the regression only serves the purpose of estimating the effect of incarceration more clearly, therefore this issue will not be discussed in more details.

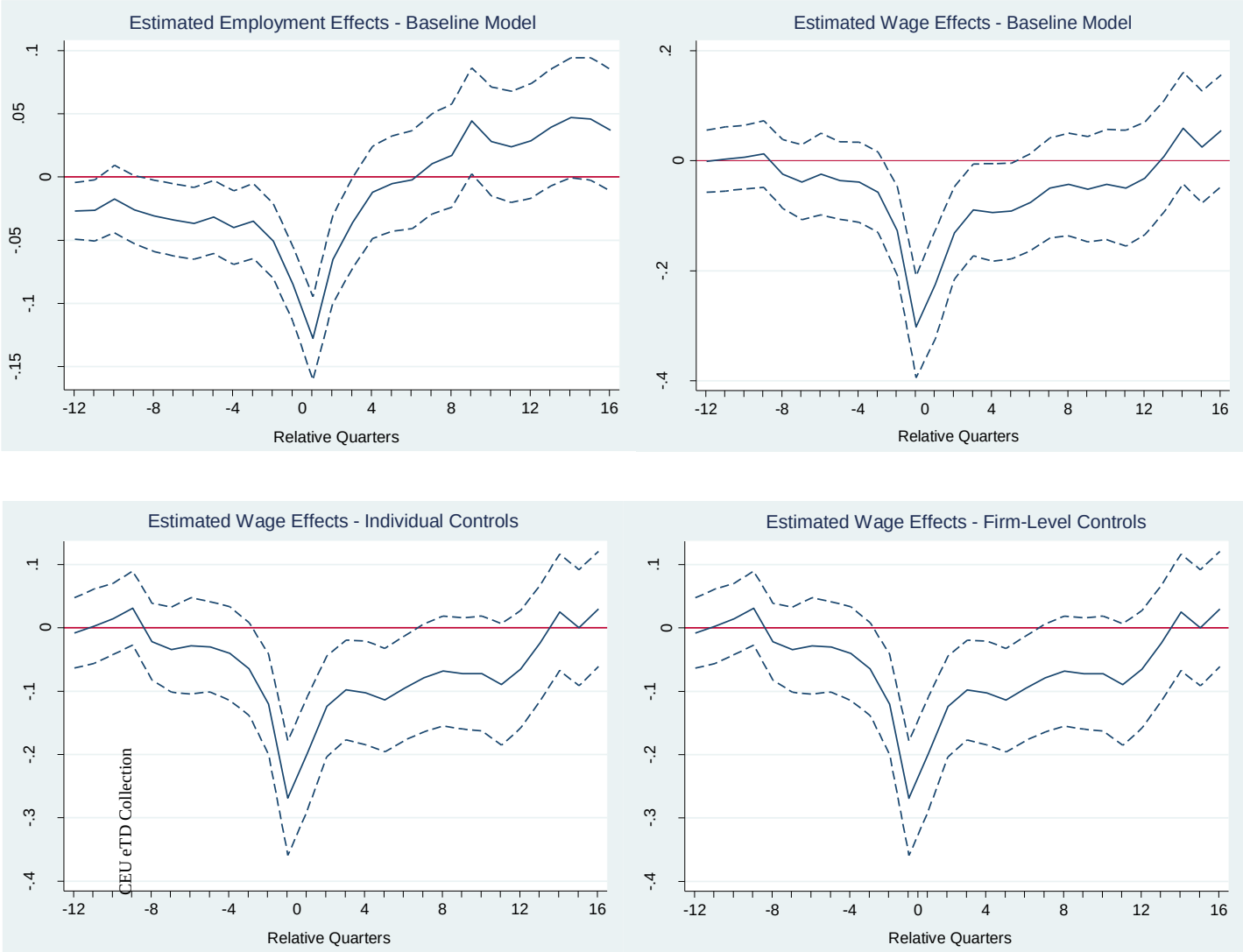
32 The "other blue collar" occupational group includes machine operators, assemblers and other positions not requiring special qualifications, yet not entirely unskilled ones. The coefficient of this position is non-significant and it is probably due to the great within group heterogeneity.

fall to a lower part of the within a firm wage distribution than before. [33] In this analysis, size, foreign ownership status and revenue per worker have a strong positive association with the wages. Therefore, this analysis reveals whether the wage costs of imprisonment partly come from the fact that the incarcerated work at smaller, non-foreign and less productive firms after release.

This specification further decreases the effect of incarceration in the shortly before and after periods. From Figure 4 and Table 8 in the Appendix, it is clear that the decrease in wage starts in the second quarter before entering prison when wages are 12.2% lower than in the baseline period. In the last quarter prior to incarceration wages are 26.5% lower than before. The wages in the first two quarters after release are 17% and 11% percent below the baseline wages, respectively. This suggests that the costs of incarceration stemming from the fact that the incarcerated work at firms which pay lower wages may only matter shortly upon release. It is also apparent from the coefficients that after controlling for the occupation and tenure, firm characteristics do not change the estimated effects substantially.

33 Similarly to the occupational categories firm level controls also raise the problem of outcome variables. Firm level variables reflect dynamics of the employer-employee match and the relation between firm performance and hiring ex-convicts. The analysis of the firm-employee matching in detail is beyond the scope of this thesis. As before. The inclusion of firm level controls in this analysis help to identify the coefficients on the relative time controls more clearly and the exact value of their coefficients is not my main interest. Therefore the problem of outcome variables will not be discussed in more details.

Figure 4. Estimated Employment and Earnings Losses – Baseline Model



6.2. Matching Model Results

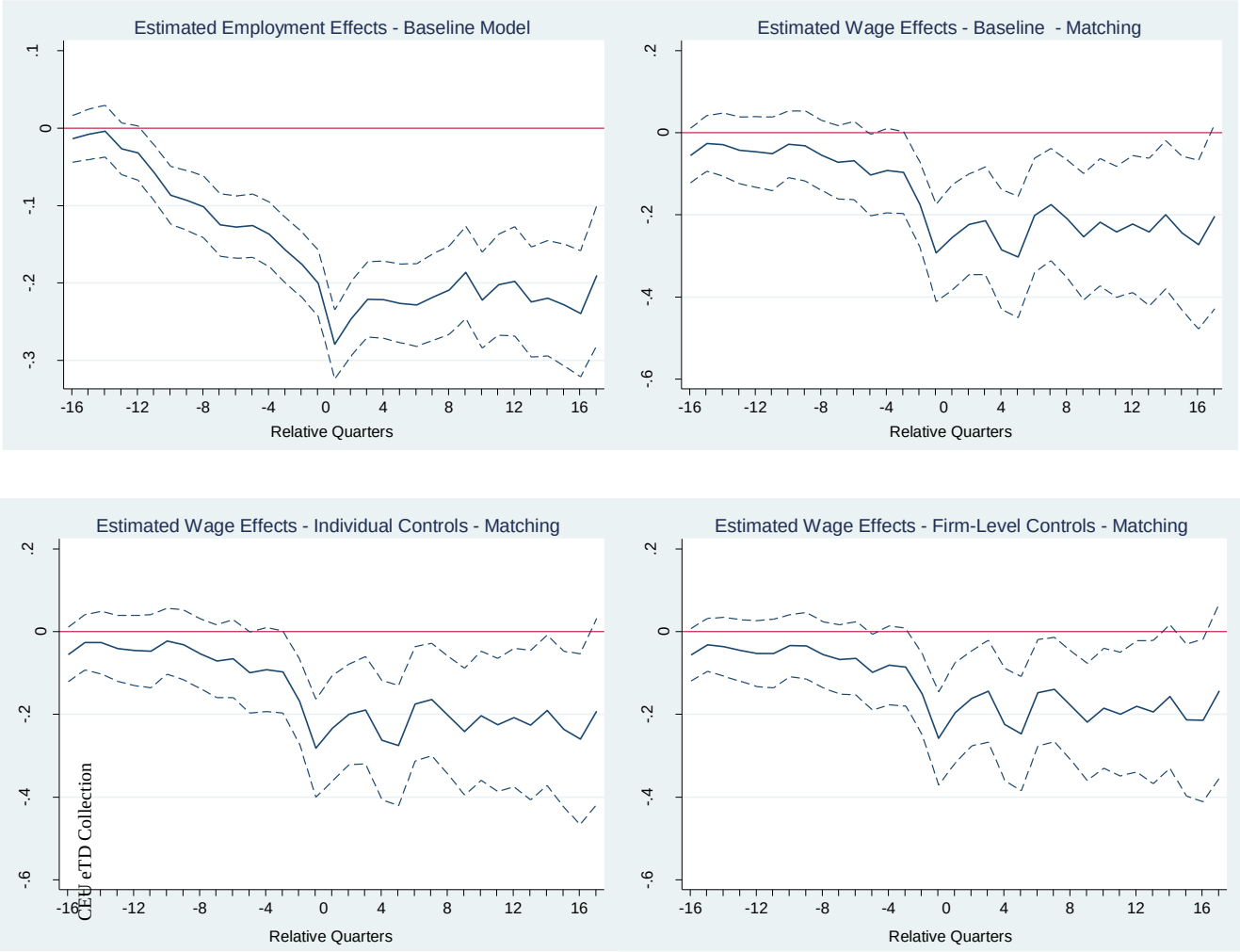
On the sample of the incarcerated and matched non-incarcerated control group the same four specifications were estimated as the ones described in Section 6.1. Figure 5 shows the coefficients of the relative time dummies with their 95% confidence bounds. In the matching specifications the event window covers four years before and four years after incarceration. The full table of estimation results can be found in Table 10 in the Appendix.

Figure 4 shows high and prolonged losses for the incarcerated compared to the control population. The employment rate three years before entering prison and earlier than that is not significantly different for the incarcerated and the non-incarcerated. This suggests that the employment rates of the matched groups evolve similarly before the pre-prison dip in the employment rates of the incarcerated begins. The employment rate of the convicts becomes lower than the control group's three years before entering prison and stays lower than theirs whole the rest of the observed period. By the last quarter before entering prison the wedge between the employment rates of the two groups widens up to a 20 percentage points difference which further increases to 27 percentage points upon release. The employment rate of the convicts do not reach the rate of the non-convicts for the whole length of the observed period after release. Again, all three wage equations show essentially the same pattern. Wages for those who work from the two groups are not significantly different in the beginning of the period, when the matching was implemented. During the last year before entering prison, the wages of the incarcerated group start falling steeply and are almost 30% lower than the wages of the control group. The difference between the wages of those who work from the two groups, fail to entirely disappear during the observed period, however,

more than three years after release the differences start to become insignificant. Occupational and firm level controls do not affect this tendency substantially.

The propensity score matching model finds higher and more prolonged losses both in terms of the wages and employment chances of the incarcerated than the model which compares their post-prison outcomes to the pre-prison ones. Some part of this difference might arise from the imperfection of the matching procedure: it is possible that we compare two groups which differ substantially in their unobserved characteristics. However, the good balancing properties of the matching (Appendix Table 9) and the insignificant differences in the early relative quarters between the groups suggest that at least initially, their outcomes evolve similarly. Overall, these results suggest that wages and employment chances may recover to their pre-prison levels, but the costs of imprisonment are potentially still high in terms of foregone earnings and employment chance growth over the career.

Figure 5. Estimated Employment and Earnings Losses – Matching Model



6.3. Subgroup Analysis Results

The costs of incarceration were estimated for several subgroups in the sample, using both the pre-prison outcomes of the incarcerated and the outcomes of the matched non-incarcerated individuals as control groups. Table 2 and 3 present the estimated effects of incarceration on the employment rate by subgroups with the baseline and the matched models. Table 4 and 5 show the estimated earnings losses with both models by subgroups. As a basis of comparison the first lines of the table include the coefficients for the time dummies without any interactions in the regression. In essence, both methods show broadly the same tendencies, however, the matching model obtains larger and more prevalent losses, similarly to the previous sections.

First, the region-level analysis suggests that the losses are non-uniform for the incarcerated living in different parts of the country and the baseline effect is driven by strong losses experienced in some regions. The South Dunantul region suffers the highest losses according to the baseline model, while the matching method finds high losses in Northern and Eastern regions as well. The baseline model finds the largest earnings losses for the South Alfold region, while the matching reveals some in Central Hungary too. Overall, the employment losses are stronger and more likely to be present in those parts of the country where the local unemployment rates are higher (Hungarian Statistical Office, 2015b).

Second, the estimation by birth cohorts reveals that older generations suffer radically higher employment losses due to the incarceration than the younger ones and this difference grows with time after release. On the other hand, the wages of those few who manage to work from the oldest generation do not seem to be hurt by the incarceration in any of the specifications.

The baseline model estimates growing employment rates after release for the youngest and the matching model suggests only moderate losses for them. None of the specifications find earnings losses for the young after release and the baseline model even estimates growing wages in the longer run. As the majority in the sample works in positions requiring physical work, it is likely that the young have an advantage in getting those jobs and paid well for them.

Third, the matched sample model finds systematically higher losses for those who were released after the second half of 2008, suggesting that the business cycle has a strong effect on the outcomes. On the other hand, the baseline model finds only short run negative effects for those affected by the crisis. The lack of longer run effects can be explained by that only a few convicts are observed long after release from those who were affected by the crisis.

Fourth, the analysis by sentence lengths reveals that the longer the sentence is, the larger the costs of incarceration will be on most time horizons with both specifications. There is no earnings loss for those spending less than a month in prison for some misdemeanor, while for those serving more than a year earnings losses after release can reach 14-18%, depending on the specification. The length of the observed period restricts my sample to the analysis of those who spend at most a couple years in prison. As losses from the incarceration seem to show a high positive correlation with the time spent in prison, it is likely that the estimated losses would be higher on a sample using a wider population of the incarcerated.

Fifth, the comparison of those who ever worked in a white collar position during the observed period to those who never did yields interesting conclusions. Lower skilled individuals suffer higher losses in terms of employment while the higher skilled ones lose more in terms of earnings. This pattern is likely to stem from the fact that low skilled workers earn close to the

regulated minimum wage in Hungary, thus if they are employed their wages show some rigidity downwards. On the other hand, skilled workers likely to find some jobs after release, but maybe for lower wages than before. I further decomposed this effect by analyzing the occupational groups of the workers based on their most frequent positions in the baseline period. The large earnings losses for the highest skill group and losses only in terms of employment rates for the unskilled are found with every specification. This implies how strong this pattern is. However, for the groups of blue collar workers the results are rather inconclusive.[34]

Sixth, the industry in which the individual most frequently worked in during the baseline period has a great influence on the dynamics of the incarceration's effect. The shortly before employment effects are only present for the manufacturing and construction sectors and for those who work in healthcare and education. The post-release effects are significant and large for most sectors.[35] The high and prolonged losses for those who work in education and healthcare might stem from the regulations which prohibit working in the public sector after release. Wage losses are the highest and most sustained in the manufacturing industry (metal, durables and machinery) and for those working in transportation, communication.[36]

Seventh, having high tenure in jobs prior to the incarceration may be a signal of the convict being a "good type", having skills which are rewarded by the labor market but are

34 I also checked whether those, whose most common job was white collar before the incarceration, but not anymore after release, have different labor market dynamics after the incarceration. Unfortunately, I did not arrive at a meaningful conclusion. The analysis focusing on the subgroup who mostly worked in unskilled positions after release but not before, also yielded very mixed results.

35 Analysing the effect of industry categories greatly restrict the sample as the industry code variables are missing for a large fraction of jobs.

36 I also conducted an analysis by grouping workers based on the fact whether they switched industry after release compared to their typical industry of work before the incarceration. Accumulation of industry specific human capital which they cannot use after release could lead to significant wage losses. However, such estimation did not give conclusive results. It highly restricted the size of the subgroups and led to very noisy results. It does not necessarily imply that this factor is irrelevant, only that a larger sample and less missing values of the industry codes would be needed for a reliable analysis.

unobserved to us. In both types of estimations, the employment rate of high tenure workers recovers more quickly on a longer run after release, which may confirm this hypothesis. On the other hand, large earnings losses are found which may mean that high tenure workers are more willing to work even for lower wages.

The last subgroup to analyze separately is those who work during their prison spell. This group is more likely to work in the year before entering prison and suffers no employment losses in the short run after release. Based on the matching model, they suffer larger earnings losses than those who never worked, but other patterns cannot be identified. These dynamics may suggest that in the short run it matters whether the convicts lose connection with working life during their incarceration.

Overall, the subgroup analysis uncovered that even when a relatively homogenous group of the incarcerated is analyzed, there are several characteristics in which they radically differ from each other and which affect their labor market outcomes greatly. The main insights from this analysis were that the released living in regions with worse economic conditions bear higher costs of the incarceration in terms of employment. The longer the sentences and the older the incarcerated are, the slower the recovery of the labor market outcomes will be. Higher skilled (and high-wage) workers are likely to have more pronounced wage than employment costs. Low skilled workers on the other hand, have lower employment rates but their wages are rigid downwards. Business cycle conditions and industry categories seem to matter, but a larger sample would be needed for a better identification of their effects.

Table 2. The Effect of Incarceration on Employment by Subgroups - Baseline Model

	Baseline Model		Employment: Base(Age, Age Squared)			
	Group	Number	Shortly Before (2Years)	Shortly After (1 Year)	Medium After (1-3 Years)	Long After (3+ Years)
	Overall:	1602	-0.039***	-0.056***	0.020	0.055**
Region:	Central Hungary	475	-0.025	-0.022	0.071***	0.098***
	Central Dunantul	177	-0.038	-0.071*	0.033	0.049
	Western Dunantul	124	-0.027	-0.004	0.081	0.127**
	South Dunantul	121	-0.095***	-0.092***	0.013	0.117**
	North Hungary	152	-0.020	-0.086**	-0.022	-0.024
	North Alfold	238	-0.013	-0.038	0.070*	0.161***
	South Alfold	215	-0.029	-0.047	-0.012	0.042
Birth Cohort:	1950's	152	-0.091**	-0.095*	-0.054	-0.061
	1960's	326	-0.062***	-0.109***	-0.032	0.000
	1970's	625	-0.042***	-0.044**	0.032	0.069**
	1980's	432	0.018	0.004	0.093***	0.148***
Business Cycle	Released After 2008	666	-0.035**	-0.076***	0.009	0.066
	Released Before 2008	938	-0.041***	-0.044**	0.023	0.054**
Sentence Length:	Very Short (<1 month)	251	-0.023	-0.034	0.049	0.146***
	Short (1 month - 1 year)	776	-0.020	-0.044**	0.014	0.048*
	Medium (> 1 year)	577	-0.070***	-0.069***	0.035	0.062**
Tenure:	High Tenure (2+years, pre-prison	909	-0.100***	-0.022	0.074***	0.126***
	Not High Tenure	695	0.032**	-0.068***	-0.027	-0.064**
Skill:	Worked in White Collar Jobs	366	-0.039	-0.033	0.056*	0.086**
	Not Worked in White Collar J.	1238	-0.039***	-0.063***	0.008	0.046**
Most Jobs Before Incarceration:	White Collar	141	-0.059	-0.267***	-0.258***	-0.356***
	Skilled Blue Collar	282	-0.122***	-0.290***	-0.300***	-0.371***
	Blue Collar Other	112	-0.097**	-0.209***	-0.166***	-0.246***
	Unskilled	240	-0.066***	-0.209***	-0.189***	-0.252***
In Prison Work	Employed (ever)	224	0.195***	0.042	0.011	-0.040
	Not Employed	1380	-0.079***	-0.070***	0.022	0.066***
Industry Before Prison	Agriculture, Fishing, Forestry	31	0.007	-0.191***	-0.170***	-0.283***
	Manufacturing Nondurables	66	-0.093**	-0.229***	-0.237***	-0.273***
	Primary and Fabricated Metal	32	-0.130*	-0.272***	-0.230**	-0.384***
	Manufacturing Durables	58	-0.127***	-0.173***	-0.103*	-0.236***
	Energy Sector	62	-0.043	-0.215***	-0.187***	-0.259***
	Construction	140	-0.112***	-0.273***	-0.270***	-0.334***
	Wholesale trade	40	-0.067	-0.293***	-0.308***	-0.290***
	Retail trade	107	-0.068	-0.202***	-0.293***	-0.362***
	Transportation, Communication	22	-0.059	-0.253**	-0.230**	-0.283***
	Accommodation	41	0.016	-0.137**	-0.061	-0.273**
	Finance, Insurance, Real Estate	145	-0.001	-0.143***	-0.130***	-0.222***
	Education, Healthcare, Culture	70	-0.127**	-0.316***	-0.205***	-0.256***

Notes: The number of observations used in the estimations are not the same for each types of subgroups categories as some of them (e.g. occupational codes and industry categories) contain missing observations and some others do not (e.g. incarceration length). The categories, high tenure, job type and industry category are formed based on the jobs of individuals in the baseline period. For identifying the most common job and industry the individual worked at, the mode of ISCO and NACE codes was used. When multiple modes were found the larger was selected, which means occupations requiring less skills. In the regressions using birth cohorts, the Age and Age-Squared explanatories are dropped from the model. The stars represent the significance level calculated with clustered standard errors on the individual level: *** p<0.01, ** p<0.05, * p<0.1

Table 3. The Effect of Incarceration on Employment by Subgroups - Matching Model

	Matching Model		Employment: Base (Age, Age Squared)			
	Group	Number Convicts	Shortly Before (2Years)	Shortly After (1 Year)	Medium After (1-3 Years)	Long After (3+ Years)
	Overall:	815	-0.069***	-0.151***	-0.111***	-0.101***
Region:	Central Hungary	255	-0.045**	-0.129***	-0.062**	-0.100**
	Central Dunantul	89	-0.055	-0.108**	-0.094**	-0.045
	Western Dunantul	45	-0.066	-0.131*	-0.087	-0.084
	South Dunantul	67	-0.130***	-0.154***	-0.133***	-0.028
	North Hungary	75	-0.095***	-0.193***	-0.151***	-0.116
	North Alfold	120	-0.046	-0.143***	-0.084*	-0.089
	South Alfold	108	-0.061*	-0.183***	-0.133***	-0.065
Birth Cohort:	1950's	60	-0.096*	-0.209***	-0.269***	-0.283***
	1960's	153	-0.087***	-0.211***	-0.168***	-0.150***
	1970's	393	-0.071***	-0.132***	-0.097***	-0.091**
	1980's	299	-0.041**	-0.120***	-0.064**	-0.077
Business Cycle	Released After 2008	597	-0.078***	-0.182***	-0.131***	-0.081
	Released Before 2008	215	-0.026	-0.073***	-0.075***	-0.076**
Sentence Length:	Very Short (<1 month)	161	-0.088***	-0.143***	-0.082**	-0.087*
	Short (1 month - 1 year)	444	-0.061***	-0.131***	-0.110***	-0.119***
	Medium (> 1 year)	210	-0.074***	-0.207***	-0.137***	0.007
Tenure	High Tenure (2+years pre-prison)	315	-0.127***	-0.153***	-0.076***	-0.041
	Not High Tenure	500	-0.032**	-0.150***	-0.141***	-0.157***
Skill:	Worked in White Collar Jobs	183	-0.058**	-0.123***	-0.081**	-0.078
	Not Worked in White Collar J.	637	-0.072***	-0.160***	-0.121***	-0.107***
Most Jobs Before Prison:	White Collar	108	-0.056	-0.250***	-0.195***	-0.237***
	Skilled Blue Collar	219	-0.111***	-0.261***	-0.285***	-0.289***
	Blue Collar Other	89	-0.074*	-0.165***	-0.150***	-0.170***
	Unskilled	190	-0.063***	-0.200***	-0.188***	-0.206***
In Prison Work:	Employed (ever)	201	0.088***	-0.054	-0.104***	-0.150***
	Not Employed	614	-0.119***	-0.184***	-0.114***	-0.086***
Industry Before Prison:	Agriculture, Fishing, Forestry	23	0.049	-0.121**	-0.114***	-0.269***
	Manufacturing Nondurables	46	-0.094**	-0.214***	-0.196***	-0.194**
	Primary and Fabricated Metal	28	-0.159**	-0.269***	-0.218**	-0.249*
	Manufacturing Durables	38	-0.124***	-0.131**	-0.075	-0.211**
	Energy Sector	36	-0.011	-0.208***	-0.202***	-0.140**
	Construction	97	-0.106***	-0.265***	-0.277***	-0.270***
	Wholesale trade	28	-0.121	-0.258***	-0.301***	-0.219**
	Retail trade	70	-0.052	-0.195***	-0.346***	-0.390***
	Transportation, Communication	21	-0.039	-0.228**	-0.197*	-0.266**
	Accommodation	29	0.025	-0.171***	0.041	-0.217***
	Finance, Insurance, Real Estate	105	-0.001	-0.140***	-0.099**	-0.214***
	Education, Healthcare, Culture	49	-0.147***	-0.317***	-0.203***	-0.178

Notes: The number of observations used in the estimations are not the same for each types of subgroups categories as some of them (e.g. occupational codes and industry categories) contain missing observations and some others do not (e.g. incarceration length). The categories, high tenure, job type and industry category are formed based on the jobs of individuals in the baseline period. For identifying the most common job and industry the individual worked at, the mode of ISCO and NACE codes was used. When multiple modes were found the larger was selected, which means occupations requiring less skills. In the regressions using birth cohorts, the Age and Age-Squared explanatories are dropped from the model. The stars represent the significance level calculated with clustered standard errors on the individual level: *** p<0.01, ** p<0.05, * p<0.1

Table 4. The Effect of Incarceration on Wages by Subgroups - Baseline Model

	Baseline Model		Daily Real Wage in Logs: Base (Age, Age Squared)			
	Group	Number	Shortly Before (2Years)	Shortly After (1 Year)	Medium After (1-3 Years)	Long After (3+ Years)
	Overall:	1602	-0.071***	-0.097**	-0.025	0.109**
Region:	Central Hungary	475	-0.057	-0.091	-0.077	0.032
	Central Dunantul	177	-0.139**	-0.111	-0.047	0.122
	Western Dunantul	124	-0.071	-0.059	0.010	0.149
	South Dunantul	121	-0.047	-0.083	0.034	0.245*
	North Hungary	152	0.010	0.082	0.288**	0.386***
	North Alfold	238	-0.008	-0.041	0.069	0.188**
	South Alfold	215	-0.093	-0.311**	-0.205*	-0.024
Birth Cohort:	1950's	152	-0.067	-0.133	0.001	0.068
	1960's	326	-0.055	-0.169*	-0.116	0.004
	1970's	625	-0.085**	-0.122**	-0.081	0.026
	1980's	432	-0.013	0.104	0.204*	0.432***
Business Cycle:	Released After 2008	666	-0.073**	-0.125*	-0.073	-0.193
	Released Before 2008	938	-0.058	-0.074	-0.003	0.120**
Sentence Length:	Very Short (<1 month)	251	-0.048	0.038	-0.039	0.102
	Short (1 month - 1 year)	776	-0.065*	-0.107**	-0.009	0.109*
	Medium (> 1 year)	577	-0.104**	-0.140*	-0.064	0.101
Tenure	High Tenure (2+years pre-prison)	909	-0.053*	-0.150***	-0.090	0.057
	Not High Tenure	695	-0.062	-0.037	0.057	0.168**
Skill:	Worked in White Collar Jobs	366	-0.075*	-0.182**	-0.136*	0.029
	Not Worked in White Collar J.	1238	-0.064**	-0.039	0.048	0.163***
Most Jobs Before Prison:	White Collar	141	-0.165***	-0.500***	-0.323***	-0.332***
	Skilled Blue Collar	282	-0.069	-0.188**	-0.091	0.018
	Blue Collar Other	112	-0.130***	-0.263***	-0.241***	-0.123
	Unskilled	240	-0.097	-0.059	-0.135	-0.173
In Prison Work:	Employed (ever)	224	-0.108***	-0.166**	0.039	0.139
	Not Employed	1380	-0.037	-0.063	-0.052	0.095*
Industry Before Prison:	Agriculture, Fishing, Forestry	31	-0.062	-0.159	0.070	0.527
	Manufacturing Nondurables	66	-0.095	-0.277**	-0.252*	-0.260*
	Primary and Fabricated Metal	32	-0.199*	-0.438**	-0.590***	-0.391*
	Manufacturing Durables	58	-0.220**	-0.352***	-0.215*	-0.088
	Energy Sector	62	-0.104	-0.396	-0.270	-0.141
	Construction	140	-0.074	-0.045	-0.003	0.024
	Wholesale trade	40	-0.014	-0.050	-0.069	-0.060
	Retail trade	107	-0.042	-0.050	0.032	0.176*
	Transportation , Communication	22	-0.215***	-0.288***	-0.385**	-0.668***
	Accommodation	41	-0.273	-0.121	0.067	0.180
	Finance, Insurance, Real Estate	145	-0.086	-0.215	-0.173	-0.064
	Education, Healthcare, Culture	70	0.060	0.207	0.551***	0.690***

Notes: The number of observations used in the estimations are not the same for each types of subgroups categories as some of them (e.g. occupational codes and industry categories) contain missing observations and some others do not (e.g. incarceration length). The categories, high tenure, job type and industry category are formed based on the jobs of individuals in the baseline period. For identifying the most common job and industry the individual worked at, the mode of ISCO and NACE codes was used. When multiple modes were found the larger was selected, which means occupations requiring less skills. In the regressions using birth cohorts, the Age and Age-Squared explanatories are dropped from the model. The stars represent the significance level calculated with clustered standard errors on the individual level: *** p<0.01, ** p<0.05, * p<0.1

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	Overall:	815	-0.050*	-0.160***	-0.139***	-0.128*
Region:	Central Hungary	255	-0.060	-0.194***	-0.228***	-0.198*
	Central Dunantul	89	-0.045	0.031	-0.050	-0.088
	Western Dunantul	45	-0.061	-0.201	-0.119	-0.069
	South Dunantul	67	-0.019	-0.188	-0.169	0.303
	North Hungary	75	-0.007	0.088	0.189	0.277
	North Alfold	120	0.030	-0.130*	-0.011	-0.117
	South Alfold	108	-0.057	-0.353**	-0.214	-0.235
Birth Cohort:	1950's	60	-0.009	-0.099	-0.106	-0.116
	1960's	153	-0.033	-0.321**	-0.263**	-0.216
	1970's	393	-0.062	-0.189***	-0.192***	-0.211**
	1980's	299	-0.046	-0.003	0.066	0.091
Business Cycle	Released After 2008	597	-0.076**	-0.207***	-0.192***	-0.280**
	Released Before 2008	215	0.037	-0.059	-0.056	-0.066
Sentence Length:	Very Short (<1 month)	161	-0.108	-0.118	-0.033	-0.210
	Short (1 month - 1 year)	444	-0.024	-0.157***	-0.132**	-0.106
	Medium (> 1 year)	210	-0.080	-0.187**	-0.207**	-0.134
Tenure	High Tenure (2+years pre-prison)	315	-0.023	-0.248***	-0.214***	-0.192*
	Not High Tenure	500	-0.067	-0.075	-0.067	-0.071
Skill:	Worked in White Collar Jobs	183	-0.046	-0.19***	-0.141**	-0.1953**
	Not Worked in White Collar J.	637	-0.058	-0.1104*	-0.126*	-0.0574
Most Jobs Before Prison:	White Collar	108	-0.076	-0.420***	-0.329***	-0.017
	Skilled Blue Collar	219	-0.017	-0.122***	-0.030	-0.023
	Blue Collar Other	89	-0.054	-0.258***	-0.258***	-0.213**
	Unskilled	190	-0.077	-0.045	-0.116	-0.119
In Prison Work:	Employed (ever)	201	-0.047	-0.249***	-0.223**	-0.189*
	Not Employed	614	-0.050	-0.100**	-0.084	-0.084
Industry Before Prison:	Agriculture, Fishing, Forestry	23	-0.097	-0.227	-0.024	-
	Manufacturing Nondurables	46	-0.090	-0.286**	-0.378**	-0.197
	Primary and Fabricated Metal	28	-0.211**	-0.262	-0.538**	-0.489
	Manufacturing Durables	38	-0.120	-0.444***	-0.278**	-0.334***
	Energy Sector	36	-0.006	-0.487	-0.294	-0.181
	Construction	97	-0.026	-0.069	0.039	-0.268*
	Wholesale trade	28	0.091	-0.080	-0.026	-0.144
	Retail trade	70	-0.033	0.015	-0.012	0.145
	Transportation, Communication	21	-0.128*	-0.201**	-0.336*	-0.632***
	Accommodation	29	-0.272	-0.038	0.216	0.318**
	Finance, Insurance, Real Estate	105	-0.018	-0.142	-0.241	-0.026
	Education, Healthcare, Culture	49	0.009	0.246	0.533***	0.653***

Notes: The number of observations used in the estimations are not the same for each types of subgroups categories as some of them (e.g. occupational codes and industry categories) contain missing observations and some others do not (e.g. incarceration length). The categories, high tenure, job type and industry category are formed based on the jobs of individuals in the baseline period. For identifying the most common job and industry the individual worked at, the mode of ISCO and NACE codes was used. When multiple modes were found the larger was selected, which means occupations requiring less skills. In the regressions using birth cohorts, the Age and Age-Squared explanatories are dropped from the model. The stars represent the significance level calculated with clustered standard errors on the individual level: *** p<0.01, ** p<0.05, * p<0.1

Chapter 7. Robustness Checks

7.1. Selection from the Incarcerated Population

As discussed earlier, the pattern of the wage and employment effects might be largely influenced by my selection of incarcerated from the total population of convicts. In this section, I analyze the evolution of labor market outcomes for the recidivists and those having left or right censored incarceration spells. This discussion provides information on the extent to which my results can be generalized to the total population of the incarcerated.

Figure 11 in the Appendix compares the post-release outcomes of those having an uncensored incarceration spell to the outcomes of those having a left-censored spell. Those having a left-censored spell are also expected to have relatively good labor market outcomes as they escape recidivism during the observed time period. However, they potentially have longer sentences than the other group. The evolution of the employment rates are very similar for the two groups but the wages of convicts with left censored incarceration spells recover from a lower level and do not reach the wages of those with single uncensored spells. The more prolonged and larger earnings losses could potentially be the costs of longer sentences, which is in line with findings of the subgroup analysis section.

Figure 6. Estimated Employment and Wage Effects for Recidivists.

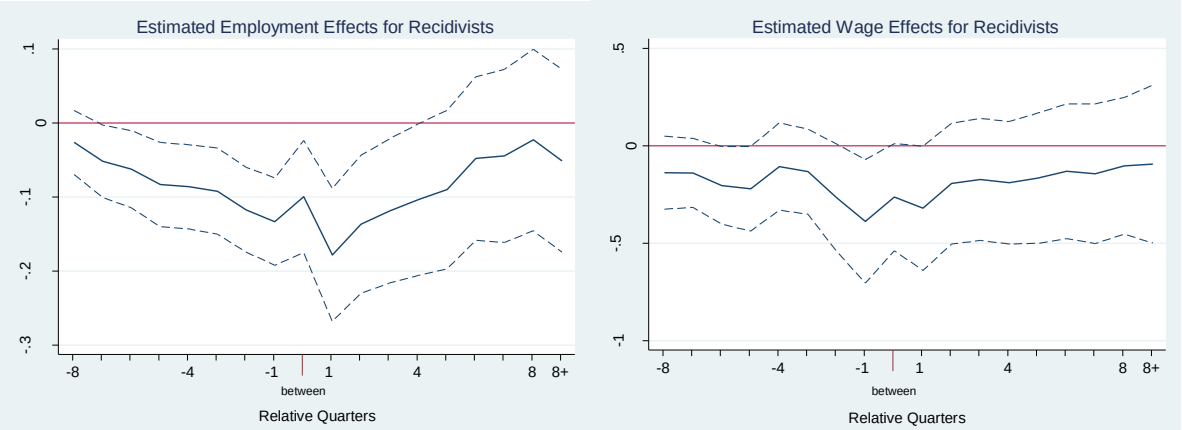


Figure 12 in the Appendix compares the single uncensored incarceration spell group to those having a right-censored spell. It shows that those having a right-censored spell have higher and more volatile employment rates. They also have higher and increasing wages before entering prison, which drop less before entry. These patterns may be explained by the fact that this group is potentially more heterogeneous as its members may have very different sentence lengths.

The most interesting comparison is given by contrasting the outcomes of the recidivists and the outcomes of those having single uncensored prison spells. Appendix Figure 13 compares the employment and wages of individuals having single uncensored prison spells to recidivists' outcomes during their periods between incarceration spells. The recidivists are grouped based on the time they spent out of prison between sentences.[37]

All the graphs show the same striking pattern, that during almost all periods both the employment rate and the wage of the recidivists are below the outcomes of the other group's. Those having a single uncensored spell start with higher employment rates and wages in the first months after release and they manage to maintain a steady pace of growth. Employment

37 Multiple recidivists of the sample might be represented more than once, appearing in different recidivists groups if they had between incarceration periods with different lengths.

rates and wages for the recidivists, however, do not exhibit a monotonic growth and tend to stop increasing after a point. It is impossible to determine whether turning to crime happens due to the bad outcomes on the labor market or the other way around. However, this pattern confirms the belief, that those having a single uncensored incarceration spell are positively selected from the incarcerated population.

Figure 6 presents the estimated relative time effects for the sample of one-time recidivists. The estimation compares the evolution of their employment rates and wages compared to a baseline period, chosen to be the period at least two years before entering prison. The model is specified the same way as it is presented in Equation 1 with an additional dummy variable which controls for the time if an individual is between periods of incarceration. The full results of the estimation are presented in Table 11 in the Appendix. The results suggest that before the first incarceration spell and after the last one the employment rates evolve similarly to the one of those having single uncensored incarceration spells. On the other hand, in the between incarceration spells period they experience a slower recovery of the employment rate than non-recidivists do after release. This result further strengthens the argument that there is a strong correlation between the slow recovery of labor market outcomes and recidivism. For those few who work, significant earnings losses are only found in the first quarter before entering prison

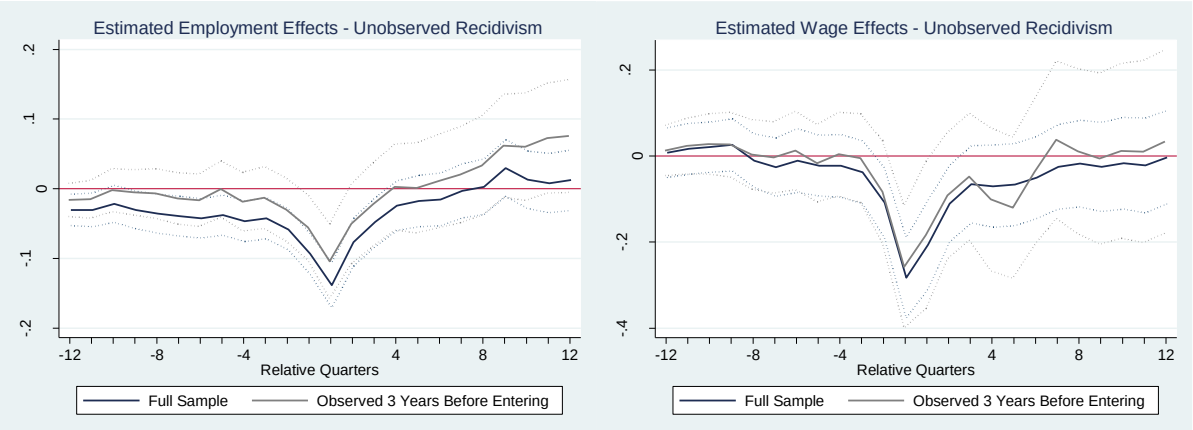
7.2. Unobserved Recidivism

Unobserved recidivism is a potential data problem in this analysis. It is not known how large fraction of the incarcerated were in prison before the observed time period. If some were incarcerated before, their baseline employment rates and wages already reflect the costs of previous imprisonments, which biases the estimations. Based on Holzer (2007) and Cho and

Lalonde (2005), the presence of such biases can be tested by restricting the sample, re-estimating the original equation on it and comparing the estimated coefficients with the ones obtained on the full sample. Holzer (2007) finds that most recidivists relapse into crime within 3-5 years after release which also holds in this sample. If the released manage to stay out of prison for that long, it is likely that they do not go back again. Therefore, those who are observed not to be incarcerated in the first three years of the time frame are likely to be actual non-recidivists. Thus I restricted the sample to this subgroup and estimated the baseline employment and wage specifications again.

Figure 7 compares the relative quarter coefficients from the regressions estimated on the full sample with the ones estimated on the restricted sample. Table 12 in the Appendix shows the full estimation results. Estimates conducted on the full sample, which may include more recidivists, find larger costs for the incarceration both in terms of employment and wages before entering prison. Such results may mean that the once incarcerated populations have a more fragile employment status, which suffers more before the incarceration. However, there is no large difference in the dynamics of the recovery. The wage estimations show larger costs after the recovery as well, suggesting that not only the most recent incarceration but previous sentences may leave their marks on wages.

Figure 7. Unobserved Recidivism



Conclusion

This paper uses a matched employer-employee panel data set for the period 2003-2011, for Hungary, which includes those incarcerated who worked at least one day during this period, and those who were their colleagues before the incarceration or after release. Focusing on the incarcerated population who had a single incarceration spell, the descriptive analysis shows that convicts have very poor labor market outcomes compared to the overall population. While the non-incarcerated in this sample have a 56% average employment rate, only 26.7% of the convicts worked before and 31% after incarceration. The non-incarcerated who worked earned 3922 HUFs daily on average in real terms. The real wages of the working convicts only reached 2490 HUFs before, and 2900 HUFs after release. These numbers show a large gap between the outcomes of the two groups during the whole length of the observed period.

With my baseline model, which compares the evolution of labor market outcomes of the convicts to their own pre-prison levels, I find large but only short run losses in terms of employment and wages. However, the absence of significant long term losses still do not imply favorable labor market outcomes. With the second model which uses a matched non-incarcerated control group, I find large and prolonged losses and a widening gap between the labor market outcomes of the convicts and non-convicts. This suggests that if we take into account how labor market outcomes would have developed in the absence of the incarceration, the estimated costs of imprisonment are higher. However, this finding may also reflect the imperfections of the matching. Including occupational and firm-level controls do

not affect the main tendencies of the wage and employment chance losses in any of the specifications. The subgroup analysis reveals that losses are higher for those serving longer sentences. The costs of incarceration for the lowest skilled appear in terms of lower employment rates, while the high skilled mostly find jobs after release, but for lower wages.

The patterns identified in this analysis provide some basis for policy recommendations.

First, my analysis finds a large and robust drop in earnings and employment rates during the year before entering prison. As mentioned before, part of this effect may come from the possibility that individuals are displaced during the trial period which can affect those as well who are not convicted in the end. Simple regulatory changes, such as ensuring that the trials are held in the region where the individuals live, or job protection for the trial period may mitigate the losses.

Second, my analysis finds that employment rates and wages are the lowest right after release, 21% and 2200 HUFs daily, for those having single uncensored incarceration spells. For those who become recidivists these outcomes are even worse after release. Stable employment and earnings after leaving the prison help escaping recidivism and facilitate the reintegration of the incarcerated into the society (Cho and Lalonde, 2005). Studies using data from the United States, such as Cho and Lalonde (2005) and Kling (2006), find the highest employment rates upon release which is likely to stem from the efficient in-prison and post-release programs implemented there. The introduction of the good practices from those programs, such as preparing the convicts for reintegration already during the incarceration, establishing half-way houses and supporting firms who hire ex-convicts with tax reimbursement, could change the adverse patterns found in Hungary. Csáki and Mészáros (2012) emphasize that the criminal record heavily constrains the employment chances of the already disadvantaged

group of the convicts in Hungary right after release. Even if the elimination of the criminal record would not be supported by the majority of the society, based on Holzer (2006), including the type of crime in it could mitigate the disadvantages for some convicts.

Third, in this data set the incarcerated population has mostly worked in jobs requiring little qualification and earn low wages. This seem to confirm the conclusions of Váltósáv Foundation (2006), i.e. incarceration is largely a poverty problem in Hungary and is related to the low economic status and educational attainment of the incarcerated population. Increasing the education and skill levels of the convicts during the incarceration could provide a long-term solution for the poor labor market outcomes of the released. However, such programs would require a high level of funding and would impose large costs on the state.

The external validity of my analysis is a crucial question. First, as discussed before the incarcerated population I use for the main analysis is not representative of the convicts overall. I focus only on non-recidivists, with short sentences who are at least weakly connected to the labor market. For the incarcerated population in general, it is likely that estimated costs would be larger. Second, as informal employment is especially widespread for the incarcerated (Váltósáv Foundation, 2006), my analysis would reflect the real costs of imprisonment better if data were available on such employment. It would be also an interesting question to analyze how incarceration and the criminal record affect the patterns of such informal employment. Third, my estimation period covers the 2008 crisis and its aftermath which may affect the estimated coefficients substantially. The subgroup analysis reveals that the labor market outcomes for those who were released during the crisis period suffered more in the short run. However, the length of the analyzed time period is not sufficient to draw conclusions for longer-term effects. Overall, it might be the case that the costs of incarceration prove to be significantly smaller when the business cycle conditions are

more favorable. Fourth, when broader generalization and cross country comparison is considered, it should always be kept in mind that the incarceration's effect is largely influenced by regulatory practices, the institutional environment and the local labor market characteristics.

Finally, this thesis gives a detailed analysis of incarceration's effect on labor market outcomes in Hungary, which can be extended in several directions in further research. First, the existing method could be implemented using different control groups as well. Comparing the outcomes of the released with the outcomes of those who were unemployed for the same length of time would help to uncover how large part of the incarceration's costs are due to lost labor market experience. Second, analyzing the discrimination against ex-convicts on the labor market with the decomposition of their earnings, would also be an interesting topic to analyze. However, the scarcity of available individual characteristics may be a serious limitation to such research. Lastly, analyzing the matching between firms and ex-convicts or studying the performance of the firms who hire the released are also rich areas of research to investigate.

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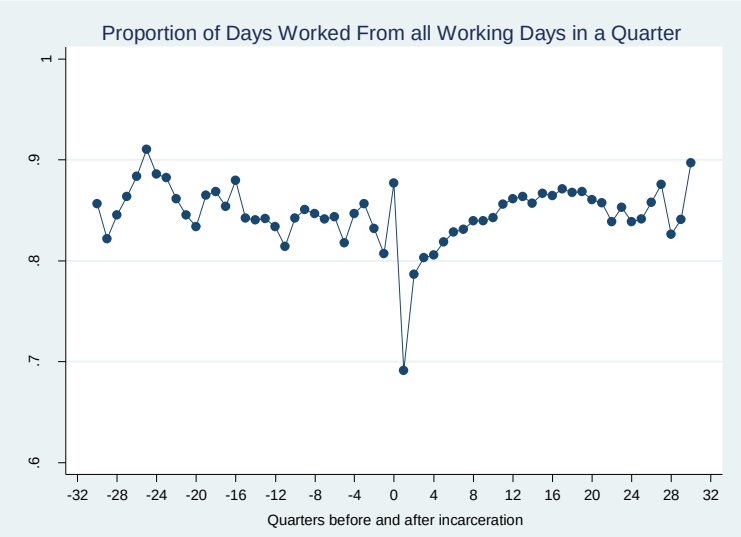
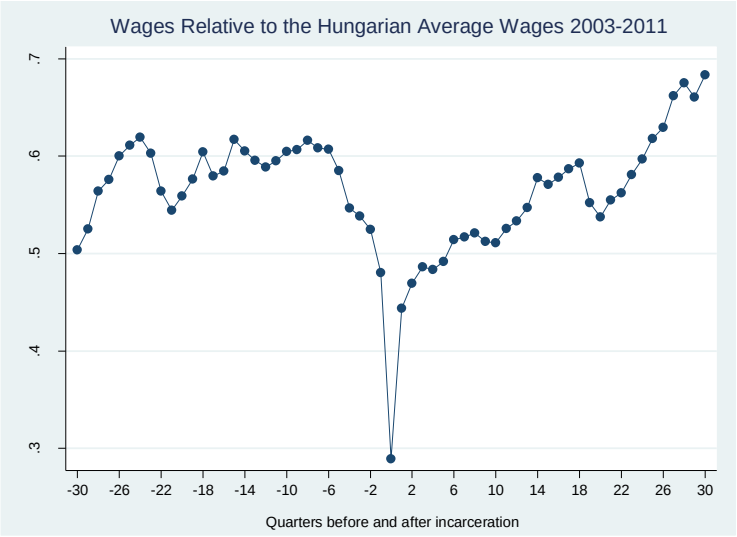
Appendix

Figure 8. Days Worked Before and After Incarceration.



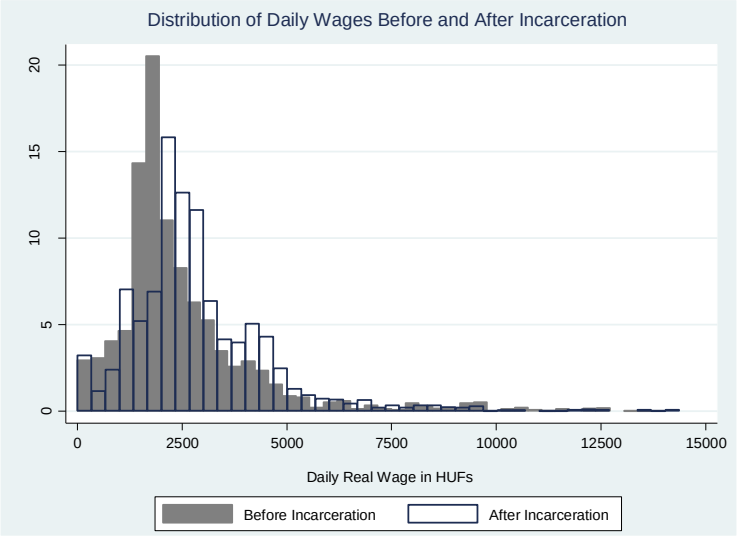
Notes: Each point in the figure represents the average of the measure, which divides the number of the days worked in a quarter with the number of all days in a quarter, for individuals who have observations for a given relative quarter.

Figure 9. Relative Wages Before and After Incarceration



Notes: Each point in the figure represents the average daily relative wage of individuals who had observations for a given relative quarter. Each point represents the average of the measure, which divides the number of the days worked in a quarter with the number of all days in a quarter, for individuals who have observations for a given relative quarters

Figure 10. Wage Distribution Before and After Incarceration



Note: The wages of displayed on this graph are real daily wages earned in a month by a person.

Figure 11. Employment and Wages with a Left-Censored Incarceration Spell

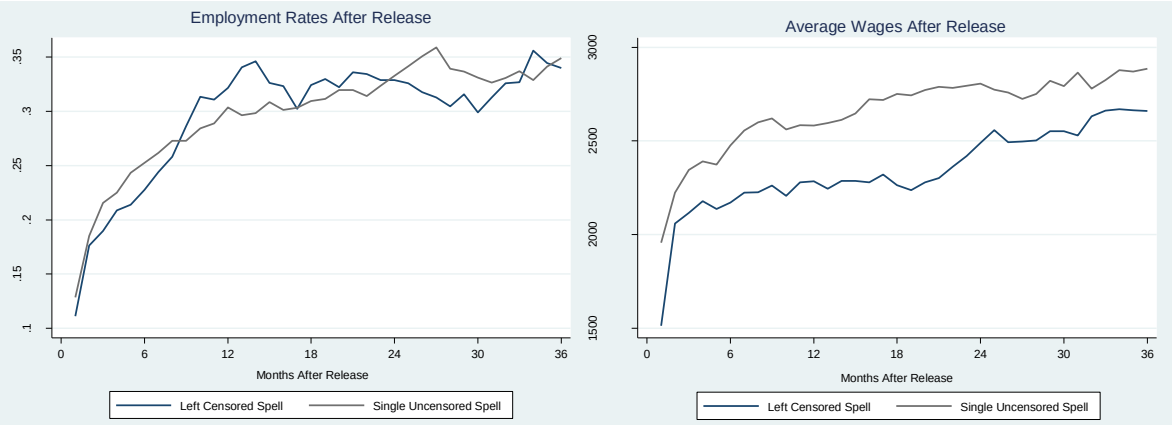


Figure 12. Employment and Wages with Right-Censored Incarceration Spell

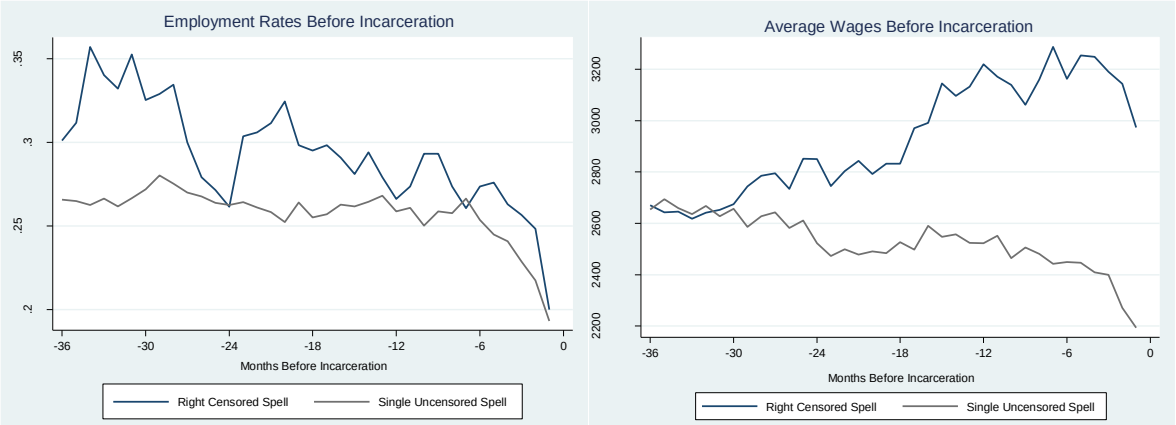
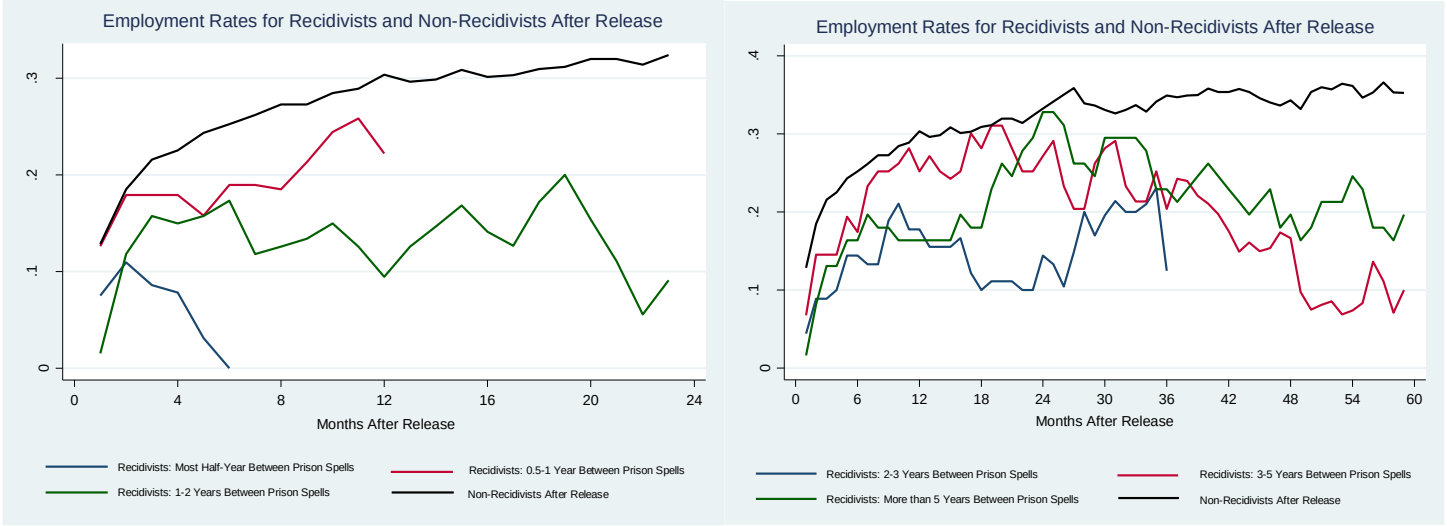


Figure 13. Employment and Wages of Recidivists between Incarceration Spells.



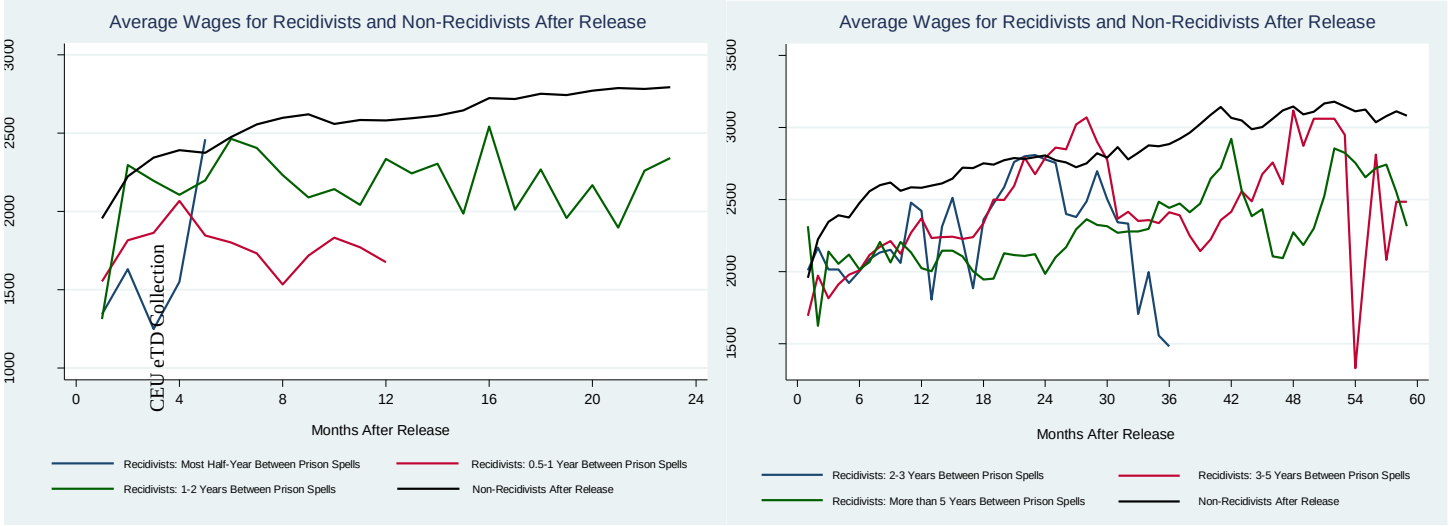
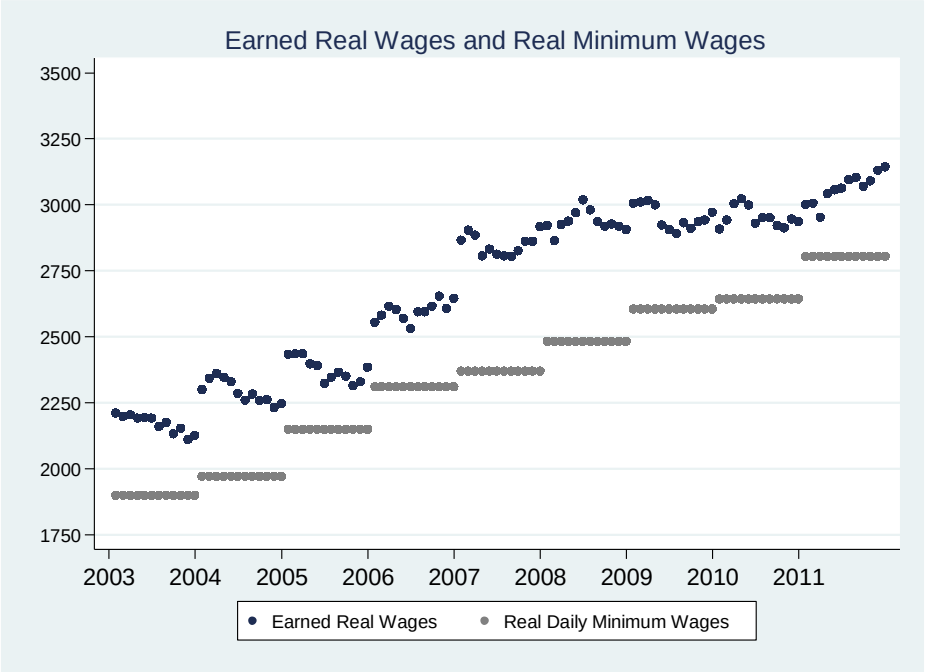


Figure 13. Earned Wages and Real Minimum Wages in Hungary



Notes: All values are transformed to real terms with chained CPI (Hungarian National Bank (2015)). There was no regulated daily minimum wage in 2003 thus that year is omitted. The monthly minimum wage data (Hungarian Statistical Office, 2015c) was used to generate the real minimum wage variable the same way as the wage data was calculated in the sample of analysis. The gap between the two series is partly due to the fact that the daily minimum wages are only for working days, and the monthly wage when divided by the number of days includes weekends as well.

Table 6. Descriptive Statistics: Incarcerated and Non-Incarcerated

	Incarcerated	Non-Incarcerated
Age	33.3	36.7
Fraction of Months When Employed	0.25	0.56
Days Worked in Month	7.6	16.8
Fraction of Month When Worked Full Month	20.7	54.7
Daily Real Wage in HUF	693.5	2455.4
Employed:		
Days Worked in Month	27.9	29.4
Daily Real Wage in HUF	2428.0	3922.6
Type of job, Fraction of Months as a/an		
Top Manager	0.009	0.009
Manager (Other Manager)	0.105	0.095
Professional	0.029	0.118
Other White Collar	0.073	0.098
Skilled Blue Collar	0.319	0.369
Other Blue Collar (Assembler, machine operator)	0.156	0.175
Unskilled Lborer	0.225	0.045
Not known	0.083	0.091
Type of firm, Fraction of Months at a		
Foreign Firm	0.085	0.223
Domestic Firm	0.616	0.460
Public Sector Firm	0.029	0.103
Not known	0.270	0.214
Firm Size	1984.2	3050.8
Revenue Per Worker (Yearly)	15005.5	19289.7
Capital Per Worker (Yearly)	4231.7	6986.9
Completed Tenure of Employment Spells (Months)	10.1	17.9
Proportion of High Skilled workers	0.211	0.312
Number of Months When Received Care Allowance	1.413	0.658
Number of Months Receiving Disability Allowance	1.637	3.822
Length of Incarceration	21.235	-
Total Number	3279.000	438356.000

Notes: The table contains data on the corrected sample for males 16-65. The labor market outcomes for the incarcerated are calculated excluding the time when they were incarcerated. The numbers are always simple averages of the values of individuals in the given group. (The days at work variable is based on the number of insured days)

Table 7. Descriptive Statistics: Incarcerated by Spell Types

	Single	Left Censored	Right Censored	Recidivist	Always in Prison	Never in Prison
Age	33.63	36.70	31.32	31.95	34.63	36.71
Length of Incarceration	12.80	21.77	22.22	28.74	108.00	
Fraction of Months Employed	0.27	0.31	0.26	0.16	0.12	0.56
Days Worked in Month	8.40	9.45	8.50	4.78	5.74	16.82
Fraction of Months Full Time	23.85	25.02	22.38	11.66	19.90	54.72
Fraction Worked During Prison	0.11	0.04	0.19	0.13	1.00	
Daily Real Wage in HUF	789.5	870.2	719.9	396.5	466.9	2455.4
Employed:						
Days Worked in Month	28.4	28.4	28.2	26.7	28.1	29.4
Daily Real Wage in HUF	2500.1	2498.2	2351.3	2268.1	666.9	3922.6
Fraction of Months as a/an						
Manager	0.13	0.06	0.17	0.03	0.00	0.11
Professional	0.03	0.03	0.04	0.01	0.02	0.12
Other White Collar	0.08	0.05	0.05	0.03	0.09	0.10
Skilled Blue Collar	0.32	0.34	0.27	0.17	0.31	0.37
Other Blue Collar	0.15	0.24	0.10	0.07	0.42	0.18
Unskilled Laborer	0.21	0.26	0.19	0.15	0.14	0.05
Not known	0.08	0.03	0.19	0.53	0.02	0.09
Fraction of Months at a						
Foreign Firm	0.08	0.10	0.08	0.08	0.13	0.22
Domestic Firm	0.63	0.66	0.52	0.59	0.64	0.46
Public Sector Firm	0.03	0.02	0.02	0.03	0.00	0.10
Not known	0.26	0.22	0.38	0.29	0.23	0.21
Firm Size	1728.3	2268.5	2394.4	2244.6	5556.8	3050.8
Revenue Per Worker (Yearly)	15817.5	16115.0	14927.2	12575.9	1604.9	19289.7
Tenure (in Months)	11.1	11.7	11.3	7.0	0.4	17.8
High Skilled workers	0.2	0.2	0.3	0.1	0.2	0.3
Region (%):						
Central Hungary	32.3	27.7	30.2	30.3	21.4	
Central Dunantul	11.5	9.4	10.6	11.1	21.4	
West Dunantul	8.5	13.3	6.7	8.8	7.1	
South Dunantul	7.7	8.6	7.6	8.5	14.3	
North Hungary	10.2	11.4	12.4	13.6	21.4	
North Alfold	15.8	19.1	19.0	16.4	0.0	
South Alfold	14.0	10.5	13.6	11.4	14.3	
Number	1712.0	392.0	346.0	815.0	14.0	438356

Contains data on the corrected sample for males 16-65. The labor market outcomes for the incarcerated are calculated excluding the time when they were incarcerated. The numbers are always simple averages of the values of individuals in the given group. (The days at work variable is based on the number of insured days).

Table 8. Estimation Results: Baseline Model

VARIABLES	Employment	Ln (Wage)	Ln (Wage)	Ln (Wage)
Age	0.028***	0.057**	0.037	0.046*
	(0.007)	(0.023)	(0.023)	(0.027)
Age Squared /100	-0.037***	-0.083***	-0.054*	-0.062*
	(0.009)	(0.032)	(0.032)	(0.037)
Disability Allowance	-0.142***			
	(0.046)			
Care Allowance	-0.093***			
	(0.028)			
Tenure			0.002**	0.002**
			(0.001)	(0.001)
Skilled Blue Collar Position			-0.128**	-0.130**
			(0.056)	(0.063)
Blue Collar Position (Other)			-0.041	-0.083
			(0.057)	(0.064)
Unskilled Position			-0.227***	-0.222***
			(0.058)	(0.063)
Firm Size: 5-50				0.136***
				(0.031)
Firm Size: 50-300				0.303***
				(0.041)
Firm Size: Greater than 300				0.430***
				(0.070)
Foreign Firm				0.152**
				(0.061)
Log Revenue per Worker				0.029***
				(0.010)
Relative Quarter (-12)	-0.027**	-0.001	-0.008	0.007
	(0.011)	(0.029)	(0.028)	(0.025)
Relative Quarter (-11)	-0.026**	0.003	0.002	0.013
	(0.012)	(0.030)	(0.030)	(0.030)
Relative Quarter (-10)	-0.017	0.006	0.014	0.018
	(0.014)	(0.029)	(0.029)	(0.033)
Relative Quarter (-9)	-0.026*	0.012	0.031	0.024
	(0.014)	(0.031)	(0.030)	(0.035)
Relative Quarter (-8)	-0.031**	-0.024	-0.022	-0.039
	(0.014)	(0.032)	(0.031)	(0.035)
Relative Quarter (-7)	-0.034**	-0.039	-0.034	-0.044
	(0.015)	(0.035)	(0.034)	(0.040)
Relative Quarter (-6)	-0.037**	-0.024	-0.028	-0.042
	(0.014)	(0.038)	(0.039)	(0.047)
Relative Quarter (-5)	-0.032**	-0.036	-0.030	-0.040
	(0.015)	(0.036)	(0.036)	(0.039)
Relative Quarter (-4)	-0.040***	-0.039	-0.040	-0.022
	(0.015)	(0.037)	(0.038)	(0.036)

Relative Quarter (-3)	-0.035**	-0.057	-0.065*	-0.051
	(0.015)	(0.037)	(0.037)	(0.036)
Relative Quarter (-2)	-0.051***	-0.127***	-0.120***	-0.122***
	(0.015)	(0.041)	(0.040)	(0.041)
Relative Quarter (-1)	-0.085***	-0.302***	-0.269***	-0.265***
	(0.015)	(0.047)	(0.046)	(0.049)
Relative Quarter (+1)	-0.128***	-0.225***	-0.198***	-0.170***
	(0.017)	(0.050)	(0.046)	(0.053)
Relative Quarter (+2)	-0.065***	-0.131***	-0.124***	-0.110**
	(0.018)	(0.043)	(0.040)	(0.043)
Relative Quarter (+3)	-0.036**	-0.089**	-0.098**	-0.056
	(0.018)	(0.043)	(0.040)	(0.041)
Relative Quarter (+4)	-0.012	-0.094**	-0.103**	-0.062
	(0.019)	(0.045)	(0.042)	(0.041)
Relative Quarter (+5)	-0.005	-0.092**	-0.114***	-0.090**
	(0.019)	(0.044)	(0.042)	(0.042)
Relative Quarter (+6)	-0.002	-0.076*	-0.095**	-0.076*
	(0.020)	(0.045)	(0.042)	(0.043)
Relative Quarter (+7)	0.010	-0.050	-0.079*	-0.078*
	(0.020)	(0.046)	(0.043)	(0.044)
Relative Quarter (+8)	0.017	-0.043	-0.068	-0.073
	(0.021)	(0.048)	(0.044)	(0.044)
Relative Quarter (+9)	0.044**	-0.052	-0.072	-0.074*
	(0.021)	(0.049)	(0.045)	(0.044)
Relative Quarter (+10)	0.028	-0.043	-0.072	-0.082*
	(0.022)	(0.051)	(0.046)	(0.046)
Relative Quarter (+11)	0.024	-0.050	-0.090*	-0.088*
	(0.022)	(0.054)	(0.049)	(0.048)
Relative Quarter (+12)	0.029	-0.032	-0.065	-0.056
	(0.023)	(0.052)	(0.047)	(0.046)
Relative Quarter (+13)	0.039*	0.008	-0.024	-0.015
	(0.024)	(0.052)	(0.047)	(0.045)
Relative Quarter (+14)	0.047*	0.059	0.025	0.023
	(0.024)	(0.052)	(0.047)	(0.044)
Relative Quarter (+15)	0.046*	0.025	0.000	-0.006
	(0.025)	(0.052)	(0.047)	(0.046)
Relative Quarter (+16)	0.037	0.055	0.030	0.029
	(0.025)	(0.052)	(0.046)	(0.044)
After +16 Relative Quarters	0.063**	0.082	0.051	0.053
	(0.027)	(0.057)	(0.051)	(0.047)
Calendar Time (Quarters)				
Constant	-0.213	6.546***	7.067***	6.452***
	(0.133)	(0.399)	(0.405)	(0.464)
Observations	144,856	41,233	37,780	28,951
R-squared	0.02	0.13	0.13	0.19
Number of Individuals	1,601	1,573	1,516	1,279
Adj. R-squared	0.02	0.12	0.13	0.19
Clustered standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1				

Table 9. Balancing Tests by Quarters

		Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	Unmatched								
	Matched	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t	Control	p>t	Control	p>t
		419.32	-4.10	810.16	-2.95	791.5	-3.09	673.89	-3.82
Wage	U	2039.3	0.000	2056.5	0.003	2071.6	0.002	2068.1	0.000
		419.32	0.09	810.16	-0.65	791.5	0.63	673.89	0.37
	M	400.36	0.925	1045.8	0.514	607.37	0.528	601.51	0.710
		30.958	-2.01	31.634	-1.56	29.714	-2.66	26.296	-5.12
Age	U	34.494	0.044	34.62	0.118	34.757	0.008	34.884	0.000
		30.958	0.11	31.634	0.20	29.714	-0.45	26.296	-0.50
	M	30.771	0.915	31.244	0.842	30.714	0.657	26.963	0.620
		10.31	-2.37	10.825	-1.88	9.7519	-2.73	7.3733	-5.06
Age Squared	U	13.379	0.018	13.479	0.060	13.588	0.006	13.688	0.000
		10.31	0.11	10.825	0.27	9.7519	-0.52	7.3733	-0.49
	M	10.182	0.914	10.469	0.784	10.58	0.604	7.7648	0.626
		.10417	-3.72	.12195	-3.28	.19048	-2.47	.2037	-2.49
Blue Collar	U	.36205	0.000	.36885	0.001	.37525	0.013	.36742	0.013
		.10417	-0.32	.12195	0.35	.19048	0.28	.2037	-0.00
	M	.125	0.752	.09756	0.728	.16667	0.779	.2037	1.000
		.04167	-0.62	.12195	1.21	.07143	-0.09	.01852	-1.50
Unskilled	U	.06353	0.535	.07297	0.228	.07517	0.927	.07077	0.134
		.04167	0.58	.12195	0.74	.07143	0.46	.01852	0.00
	M	.02083	0.562	.07317	0.463	.04762	0.649	.01852	1.000
Unmatched Sample Size		167848		168982		169890		171064	
Matched Sample Size		96		82		84		108	

		Quarter 5		Quarter 6		Quarter 7		Quarter 8	
	Unmatched								
	Matched	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t	Control	p>t	Control	p>t
		789.73	-2.44	1502.2	-1.29	422.77	-4.03	855.96	-2.94
Wage	U	2085.7	0.015	2102.3	0.196	2120.2	0.000	2108	0.003
		789.73	0.35	1502.2	-0.58	422.77	0.17	855.96	0.12
	M	674.74	0.731	1949	0.565	391.4	0.864	821.19	0.903
		31.357	-1.55	29.389	-2.77	29.163	-3.21	30.429	-2.57
Age	U	34.986	0.121	35.129	0.006	35.268	0.001	35.404	0.010
		31.357	-0.30	29.389	-0.22	29.163	0.17	30.429	-0.10
	M	32.107	0.764	29.917	0.828	28.837	0.867	30.643	0.917
		10.704	-1.76	9.7011	-2.70	9.3195	-3.27	10.086	-2.76
Age Squared	U	13.772	0.079	13.884	0.007	13.995	0.001	14.103	0.006
		10.704	-0.23	9.7011	-0.15	9.3195	0.19	10.086	-0.14
	M	11.11	0.822	9.9453	0.882	9.0823	0.849	10.275	0.889
		.14286	-2.33	.08333	-3.51	.11628	-3.44	.2381	-1.66
Blue Collar	U	.35369	0.020	.36461	0.000	.36925	0.001	.36139	0.096
		.14286	-0.69	.08333	-0.00	.11628	-0.62	.2381	-0.25
	M	.21429	0.494	.08333	1.000	.16279	0.539	.2619	0.804
		.03571	-0.59	.02778	-1.03	.06977	-0.12	.09524	0.67
Unskilled	U	.06283	0.554	.07203	0.304	.07462	0.904	.06896	0.502
		.03571	-0.00	.02778	-0.00	.06977	0.46	.09524	-0.35
	M	.03571	1.000	.02778	1.000	.04651	0.650	.11905	0.728
Unmatched Sample Size		171686		172636		173670		174598	
Matched Sample Size		56		72		86		84	

	Unmatched	Quarter 9		Quarter 10		Quarter 11		Quarter 12	
	Matched	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t	Control	p>t	Control	p>t
		1010.5	-2.37	257.5	-3.75	368.29	-3.62	223.44	-3.92
Wage		2220.7	0.018	2238.6	0.000	2256	0.000	2264	0.000
		1010.5	0.20	257.5	0.21	368.29	0.25	223.44	-0.19
	M	922.24	0.842	222.25	0.834	304.13	0.803	253.87	0.851
		29.486	-2.83	34.281	-0.61	28.313	-3.34	30.5	-2.43
Age	U	35.511	0.005	35.648	0.540	35.789	0.001	35.83	0.015
		29.486	-0.27	34.281	-0.01	28.313	0.15	30.5	-0.16
	M	30.086	0.786	34.313	0.991	27.969	0.881	30.824	0.874
		9.4663	-2.94	12.785	-0.90	8.8494	-3.27	9.9962	-2.67
Age Squared	U	14.191	0.003	14.3	0.371	14.413	0.001	14.476	0.008
		9.4663	-0.32	12.785	-0.05	8.8494	0.18	9.9962	-0.13
	M	9.9203	0.750	12.877	0.959	8.6003	0.859	10.165	0.900
		.11429	-2.87	.09375	-3.09	.0625	-3.52	.11765	-3.18
Blue Collar	U	.34453	0.004	.35493	0.002	.36103	0.000	.38231	0.001
		.11429	0.39	.09375	0.00	.0625	-0.46	.11765	-0.00
	M	.08571	0.695	.09375	1.000	.09375	0.648	.11765	1.000
		.05714	-0.07	.09375	0.47	.03125	-0.96	0	.
Unskilled	U	.06008	0.942	.07224	0.638	.07635	0.337	0	.
		.05714	-0.46	.09375	0.00	.03125	0.00	0	.
	M	.08571	0.648	.09375	1.000	.03125	1.000	0	.
Unmatched Sample Size		174762		175748		176744		164814	
Matched Sample Size		70		64		64		68	

	Unmatched	Quarter 13		Quarter 14		Quarter 15		Quarter 16	
	Matched	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t	Control	p>t	Control	p>t
		828.29	-3.20	1101.9	-2.70	942.09	-2.39	668.12	-3.93
Wage	U	2292	0.001	2353.6	0.007	2385.7	0.017	2405	0.000
		828.29	0.45	1101.9	-0.09	942.09	-0.11	668.12	0.11
	M	680.87	0.657	1140.9	0.929	990.43	0.915	639.05	0.910
		32.489	-1.88	32.465	-1.92	30.84	-2.16	28	-4.55
Age	U	36.062	0.060	36.215	0.055	36.378	0.031	36.533	0.000
		32.489	0.04	32.465	-0.17	30.84	-0.22	28	-0.29
	M	32.4	0.965	32.86	0.868	31.52	0.823	28.553	0.775
		11.51	-2.14	11.777	-1.98	10.575	-2.18	8.7136	-4.33
Age Squared	U	14.631	0.032	14.751	0.047	14.879	0.030	15.001	0.000
		11.51	0.14	11.777	-0.08	10.575	-0.24	8.7136	-0.22
	M	11.323	0.891	11.908	0.937	11.074	0.810	8.9851	0.823
		.06667	-3.91	.23256	-1.71	.2	-1.72	.21277	-2.09
Blue Collar	U	.34386	0.000	.35754	0.087	.36549	0.086	.35937	0.036
		.06667	-0.73	.23256	0.26	.2	0.76	.21277	0.80
	M	.11111	0.464	.2093	0.798	.12	0.451	.14894	0.427
		.04444	-0.57	.13953	1.57	.08	0.08	.04255	-0.72
Unskilled	U	.06542	0.569	.07607	0.117	.0756	0.934	.06907	0.474
		.04444	0.00	.13953	0.32	.08	-0.86	.04255	0.00
	M	.04444	1.000	.11628	0.750	.16	0.394	.04255	1.000
Unmatched Sample Size		177936		178638		179300		180008	
Matched Sample Size		90		86		50		94	

	Unmatched	Quarter 17		Quarter 18		Quarter 19		Quarter 20	
	Matched	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t	Control	p>t	Control	p>t
Wage	U	236.49	-3.96	794.81	-3.59	906.14	-3.36	1062.4	-2.53
		2512.1	0.000	2580.4	0.000	2604.3	0.001	2621.6	0.011
	M	236.49	-0.10	794.81	-0.44	906.14	-0.47	1062.4	-0.33
		256.08	0.921	942.32	0.664	1098.6	0.639	1225.9	0.746
Age	U	30.806	-2.53	31.146	-2.81	27.615	-4.51	32.5	-1.82
		36.666	0.011	36.812	0.005	36.978	0.000	37.143	0.068
	M	30.806	-0.08	31.146	-0.44	27.615	0.58	32.5	-0.18
		31	0.940	32.049	0.661	26.308	0.565	33	0.855
Age Squared	U	10.505	-2.56	10.587	-2.95	8.6603	-4.12	11.553	-1.97
		15.106	0.011	15.221	0.003	15.352	0.000	15.482	0.049
	M	10.505	-0.04	10.587	-0.37	8.6603	0.58	11.553	-0.11
		10.585	0.965	11.071	0.714	7.8328	0.563	11.764	0.911
Blue Collar	U	.09677	-2.95	.14634	-2.87	.17949	-2.44	.19231	-1.83
		.34972	0.003	.36158	0.004	.36776	0.015	.36468	0.068
	M	.09677	-0.00	.14634	0.67	.17949	0.30	.19231	-0.00
		.09677	1.000	.09756	0.506	.15385	0.765	.19231	1.000
Unskilled	U	.03226	-0.74	.12195	1.08	.10256	0.52	.07692	0.03
		.06517	0.458	.07706	0.281	.07999	0.603	.07553	0.979
	M	.03226	0.00	.12195	0.35	.10256	0.84	.07692	-0.00
		.03226	1.000	.09756	0.728	.05128	0.402	.07692	1.000
Unmatched Sample Size		180126		180732		181426		181868	
Matched Sample Size		62		82		78		52	

	Unmatched	Quarter 21		Quarter 22	
	Matched	Mean Treated	t-test t	Mean Treated	t-test t
Variable	Sample	Control	p>t	Control	p>t
Wage	U	654.66	-3.56	1248.8	-1.78
		2653.5	0.000	2719.4	0.075
	M	654.66	0.50	1248.8	-0.42
		466.1	0.616	1535.9	0.680
Age	U	30.743	-2.98	25.938	-3.54
		37.291	0.003	37.466	0.000
	M	30.743	-0.20	25.938	0.15
		31.257	0.841	25.5	0.885
Age Squared	U	10.495	-2.95	7.4844	-3.20
		15.599	0.003	15.735	0.001
	M	10.495	-0.26	7.4844	0.21
		10.953	0.794	7.1037	0.835
Blue Collar	U	.08571	-3.32	.375	0.12
		.35366	0.001	.36087	0.906
	M	.08571	-0.39	.375	-0.35
		.11429	0.695	.4375	0.729
Unskilled	U	.14286	1.72	.0625	-0.22
		.0691	0.085	.07707	0.827
	M	.14286	-0.00	.0625	1.00
		.14286	1.000	0	0.325
Unmatched Sample Size		181730		182144	
Matched Sample Size		70		32	

Table 10. Estimation Results: Matched Regression

VARIABLES	Employed	Log(Wage)	Log(Wage)	Log(Wage)
Age	0.059***	0.066***	0.066***	0.075***
	(0.007)	(0.016)	(0.016)	(0.015)
Age Squared /100	-0.084***	-0.106***	-0.105***	-0.116***
	(0.009)	(0.023)	(0.022)	(0.021)
Disability Allowance	-0.295***			
	(0.058)			
Care Allowance	-0.132***			
	(0.029)			
Skilled Blue Collar			0.169***	0.144***
			(0.053)	(0.051)
Blue Collar			0.280***	0.203***
			(0.057)	(0.053)
Unskilled			0.062	0.026
			(0.053)	(0.051)
Tenure			0.002***	0.002***
			(0.001)	(0.001)
Size: 5-50 Employee				0.143***
				(0.029)
Size: 50-300 Employee				0.372***
				(0.034)
Size: 300+ Employee				0.493***
				(0.043)
Relative Quarter (-16)	-0.014	-0.056	-0.055*	-0.056*
	(0.015)	(0.034)	(0.034)	(0.032)
Relative Quarter (-15)	-0.008	-0.026	-0.026	-0.032
	(0.017)	(0.035)	(0.034)	(0.033)
Relative Quarter (-14)	-0.004	-0.029	-0.026	-0.037
	(0.017)	(0.039)	(0.039)	(0.036)
Relative Quarter (-13)	-0.027	-0.043	-0.041	-0.046
	(0.017)	(0.041)	(0.041)	(0.038)
Relative Quarter (-12)	-0.032	-0.047	-0.046	-0.053
	(0.021)	(0.044)	(0.043)	(0.041)
Relative Quarter (-11)	-0.058**	-0.051	-0.047	-0.053
	(0.018)	(0.046)	(0.045)	(0.042)
Relative Quarter (-10)	-0.087***	-0.028	-0.023	-0.034
	(0.019)	(0.041)	(0.041)	(0.038)
Relative Quarter (-9)	-0.094***	-0.032	-0.032	-0.034
	(0.020)	(0.044)	(0.043)	(0.041)
Relative Quarter (-8)	-0.102***	-0.055	-0.053	-0.056
	(0.020)	(0.044)	(0.043)	(0.041)
Relative Quarter (-7)	-0.125***	-0.072	-0.071	-0.067
	(0.021)	(0.046)	(0.045)	(0.043)
Relative Quarter (-6)	-0.128***	-0.068	-0.065	-0.064
	(0.021)	(0.049)	(0.048)	(0.045)

Relative Quarter (-5)	-0.126***	-0.103**	-0.099**	-0.098**
	(0.021)	(0.051)	(0.050)	(0.047)
Relative Quarter (-4)	-0.137***	-0.092*	-0.092*	-0.081*
	(0.021)	(0.052)	(0.052)	(0.049)
Relative Quarter (-3)	-0.158***	-0.097*	-0.098*	-0.086*
	(0.022)	(0.051)	(0.051)	(0.048)
Relative Quarter (-2)	-0.176***	-0.175***	-0.169***	-0.152***
	(0.022)	(0.053)	(0.052)	(0.050)
Relative Quarter (-1)	-0.200***	-0.293***	-0.282***	-0.258***
	(0.022)	(0.060)	(0.060)	(0.057)
Relative Quarter (+1)	-0.279***	-0.254***	-0.233***	-0.196***
	(0.023)	(0.065)	(0.065)	(0.062)
Relative Quarter (+2)	-0.246***	-0.223***	-0.200***	-0.161***
	(0.024)	(0.063)	(0.062)	(0.058)
Relative Quarter (+3)	-0.221***	-0.214***	-0.190***	-0.144**
	(0.025)	(0.067)	(0.066)	(0.063)
Relative Quarter (+4)	-0.222***	-0.285***	-0.262***	-0.224***
	(0.025)	(0.074)	(0.073)	(0.069)
Relative Quarter (+5)	-0.226***	-0.303***	-0.276***	-0.247***
	(0.026)	(0.075)	(0.074)	(0.071)
Relative Quarter (+6)	-0.228***	-0.201***	-0.175**	-0.148**
	(0.027)	(0.071)	(0.070)	(0.066)
Relative Quarter (+7)	-0.218***	-0.175**	-0.164**	-0.140**
	(0.029)	(0.070)	(0.069)	(0.064)
Relative Quarter (+8)	-0.209***	-0.210***	-0.203***	-0.179***
	(0.029)	(0.073)	(0.073)	(0.067)
Relative Quarter (+9)	-0.186***	-0.253***	-0.242***	-0.218***
	(0.030)	(0.079)	(0.078)	(0.072)
Relative Quarter (+10)	-0.222***	-0.218***	-0.204**	-0.185**
	(0.032)	(0.079)	(0.080)	(0.074)
Relative Quarter (+11)	-0.202***	-0.241***	-0.225***	-0.199***
	(0.033)	(0.081)	(0.082)	(0.076)
Relative Quarter (+12)	-0.198***	-0.223***	-0.208**	-0.181**
	(0.036)	(0.085)	(0.085)	(0.081)
Relative Quarter (+13)	-0.224***	-0.242***	-0.226**	-0.194**
	(0.036)	(0.092)	(0.092)	(0.088)
Relative Quarter (+14)	-0.220***	-0.200**	-0.190**	-0.157*
	(0.038)	(0.092)	(0.093)	(0.089)
Relative Quarter (+15)	-0.228***	-0.245**	-0.236**	-0.214**
	(0.040)	(0.096)	(0.096)	(0.094)
Relative Quarter (+16)	-0.240***	-0.273***	-0.260**	-0.215**
	(0.041)	(0.105)	(0.105)	(0.100)
After 4 years	-0.190***	-0.204*	-0.192*	-0.143
	(0.046)	(0.115)	(0.115)	(0.108)
Calendar Time (Quarters):				
Constant	-0.500***	6.750***	6.646***	6.206***
	(0.132)	(0.289)	(0.291)	(0.274)

Observations	160,275	59,684	59,684	59,684
R-squared	0.04	0.17	0.18	0.25
Number of anon	1,626	1,604	1,604	1,604
Adj. R-squared	0.04	0.17	0.18	0.24

Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients of the calendar time dummies are not presented here for better visibility but are available on request from 2003Q2 to 2011Q4.

Table 11. Estimation Results: Recidivists

VARIABLES	Employed	Log(Wage)
Age	0.024**	0.097**
	(0.010)	(0.044)
Age Squared /100	-0.036***	-0.130**
	(0.013)	(0.054)
Disability Allowance	-0.166**	
	(0.066)	
Care Allowance	-0.045**	
	(0.022)	
Relative Quarter (-8)	-0.026	-0.138
	(0.022)	(0.095)
Relative Quarter (-7)	-0.052**	-0.139
	(0.025)	(0.090)
Relative Quarter (-6)	-0.063**	-0.204**
	(0.026)	(0.101)
Relative Quarter (-5)	-0.083***	-0.221**
	(0.029)	(0.110)
Relative Quarter (-4)	-0.086***	-0.106
	(0.029)	(0.114)
Relative Quarter (-3)	-0.092***	-0.133
	(0.030)	(0.111)
Relative Quarter (-2)	-0.117***	-0.268*
	(0.029)	(0.140)
Relative Quarter (-1	-0.133***	-0.388**
	(0.030)	(0.161)
Between	-0.099**	-0.264*
	(0.039)	(0.140)
Relative Quarter (+1)	-0.178***	-0.321**
	(0.046)	(0.162)
Relative Quarter (+2)	-0.137***	-0.194
	(0.047)	(0.158)
Relative Quarter (+3)	-0.119**	-0.173
	(0.050)	(0.159)
Relative Quarter (+4)	-0.104**	-0.190
	(0.052)	(0.160)
Relative Quarter (+5)	-0.090*	-0.166
	(0.054)	(0.170)
Relative Quarter (+6)	-0.048	-0.131
	(0.056)	(0.176)
Relative Quarter (+7)	-0.045	-0.143
	(0.059)	(0.182)
Relative Quarter (+8)	-0.023	-0.103
	(0.062)	(0.178)
After 2 Years	-0.051	-0.093
	(0.063)	(0.206)

Constant	-0.153	5.835***
	(0.207)	(0.921)
Calendar Time FE		
Observations	34,903	6,304
R-squared	0.02	0.09
Number of anon	422	416
Adj. R-squared	0.02	0.08
Clustered standard errors *** p<0:01, ** p<0.05, * p<0.1		

Notes: The coefficients of the calendar time dummies are not presented here for better visibility but are available on request from 2003Q2 to 2011Q4.

Table 12. Estimation Results: Unobserved Recidivism

VARIABLES	Full Sample Employment	Restricted Sample Employment	Full Sample Log(Wage)	Restricted Sample Log(Wage)
Age	0.026***	0.033***	0.055**	0.045
	(0.007)	(0.009)	(0.022)	(0.027)
Age Squared /100	-0.035***	-0.049***	-0.080**	-0.073*
	(0.009)	(0.012)	(0.031)	(0.040)
Disability Allowance	-0.147***	-0.192***		
	(0.045)	(0.068)		
Care Allowance	-0.097***	-0.102***		
	(0.027)	(0.034)		
Relative Quarter (-12)	-0.030***	-0.016	0.008	0.013
	(0.011)	(0.012)	(0.029)	(0.030)
Relative Quarter (-11)	-0.030**	-0.015	0.017	0.023
	(0.012)	(0.014)	(0.030)	(0.033)
Relative Quarter (-10)	-0.022	-0.002	0.021	0.028
	(0.014)	(0.016)	(0.030)	(0.036)
Relative Quarter (-9)	-0.031**	-0.005	0.026	0.027
	(0.014)	(0.017)	(0.031)	(0.038)
Relative Quarter (-8)	-0.036**	-0.007	-0.011	0.004
	(0.014)	(0.018)	(0.032)	(0.041)
Relative Quarter (-7)	-0.039***	-0.014	-0.026	-0.003
	(0.014)	(0.019)	(0.035)	(0.042)
Relative Quarter (-6)	-0.043***	-0.016	-0.011	0.013
	(0.014)	(0.019)	(0.038)	(0.046)
Relative Quarter (-5)	-0.038***	-0.000	-0.022	-0.017
	(0.015)	(0.021)	(0.036)	(0.046)
Relative Quarter (-4)	-0.047***	-0.019	-0.023	0.004
	(0.015)	(0.021)	(0.037)	(0.050)
Relative Quarter (-3)	-0.042***	-0.013	-0.038	-0.005
	(0.015)	(0.023)	(0.037)	(0.053)
Relative Quarter (-2)	-0.058***	-0.030	-0.109***	-0.083
	(0.015)	(0.023)	(0.041)	(0.060)
Relative Quarter (-1	-0.093***	-0.056**	-0.283***	-0.257***
	(0.015)	(0.024)	(0.048)	(0.072)
Relative Quarter (+1)	-0.139***	-0.104***	-0.207***	-0.184**
	(0.016)	(0.027)	(0.053)	(0.087)
Relative Quarter (+2)	-0.077***	-0.050*	-0.110**	-0.092
	(0.017)	(0.029)	(0.045)	(0.075)
Relative Quarter (+3)	-0.048***	-0.022	-0.066	-0.048
	(0.018)	(0.030)	(0.046)	(0.075)
Relative Quarter (+4)	-0.024	0.002	-0.070	-0.101
	(0.018)	(0.032)	(0.049)	(0.085)
Relative Quarter (+5)	-0.018	0.001	-0.066	-0.120
	(0.019)	(0.033)	(0.049)	(0.083)
Relative Quarter (+6)	-0.015	0.011	-0.050	-0.037
	(0.019)	(0.034)	(0.049)	(0.086)

Relative Quarter (+7)	-0.003	0.020	-0.025	0.038
	(0.020)	(0.035)	(0.050)	(0.093)
Relative Quarter (+8)	0.003	0.033	-0.018	0.011
	(0.020)	(0.036)	(0.051)	(0.098)
Relative Quarter (+9)	0.030	0.062	-0.025	-0.006
	(0.021)	(0.038)	(0.053)	(0.101)
Relative Quarter (+10)	0.013	0.060	-0.017	0.012
	(0.021)	(0.039)	(0.054)	(0.104)
Relative Quarter (+11)	0.008	0.073*	-0.022	0.010
	(0.022)	(0.040)	(0.056)	(0.108)
Relative Quarter (+12)	0.012	0.076*	-0.003	0.034
	(0.022)	(0.041)	(0.055)	(0.109)
More than 3 Years After	0.035	0.089**	0.098	0.059
	(0.024)	(0.045)	(0.059)	(0.128)
Calendar Time FE				
Constant	-0.185	-0.234	6.582***	6.849***
	(0.130)	(0.171)	(0.391)	(0.483)
Observations	148,495	78,852	42,454	21,602
R-squared	0.02	0.01	0.13	0.09
Number of Individuals	1,604	821	1,583	810
Adj. R-squared	0.02	0.01	0.13	0.08

Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: The restricted sample uses observations for only those who are observed not being incarcerated in the first 3 years of the observed time period. Unlike the baseline model, here only 12 relative quarters are used for estimation not to restrict the sample size of the restricted model further. The coefficients of the calendar time dummies are not presented here for better visibility but are available on request from 2003Q2 to 2011Q4.