

Does Incarceration Length Affect the Labor Market Outcomes of Violent Offenders?

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This paper uses a reform of the Danish Penal Code concerning violent crimes to study the effect of an exogenous increase in incarceration length on labor market outcomes, the first three years after release, measured by unemployment rates, dependency of other public transfers, and earnings. Using a panel of monthly observations constructed from detailed Danish administrative-level data I follow the sample of violent offenders from four years prior to incarceration to conclude that the reform provides an exogenous increase to incarceration length and further, that the two groups' outcomes are subject to equal trends. I find a lower unemployment rate and higher level of earnings for those employed as an effect of the longer incarceration spells induced by reform. Further, the effects of the reform increase with time since release. **Preliminary version - do not cite**

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1 Introduction

The ambition of this paper is to assess the causal effect of length of an incarceration spell on subsequent labor market outcomes for a sample of violent offenders. I define the labor market outcomes as unemployment rates, dependency of other public transfers, and earnings.

The earlier investigations in this field have been inconclusive, as they struggle with the complex relationship between crime, labor market outcomes, and unobserved characteristics along with samples which consist of different and heterogeneous offender types (e.g. Lott (1992a,b); Needles (1996); Kling (2006)). To estimate the causal effect of incarceration length on different labor market outcomes one must address the predicament that individuals serving different incarceration lengths are likely to differ on several observable and unobservable characteristics. For this purpose I apply a reform of the Danish Penal Code, increasing incarceration lengths for violent offenders unrelated to individual behavioural characteristics, thus providing exogenous variation. I construct a highly detailed panel of monthly observation using Danish administrative-level data and analyse the sample four years before incarceration until three years after release.

The previous literature on the field of sanctions towards crime and subsequent labor market outcomes have mainly focussed on the effect of conviction or incarceration per se. Yet, once a person is found guilty of a crime, policy-makers and judges do not only face a discrete choice of whether to sanction the offender for the crime or not, or of whether to sentence her to imprisonment or not. They also face the important decision of determining the lengths of incarceration.

The chosen length of an incarceration spell may affect the offender's labor market productivity positively or negatively, depending on whether the offender loses or accumulates

human capital while incarcerated. The incarceration length may also affect the offender's future life-course in other ways. Informal sanctions from society produced by stigma might influence job opportunities for former inmates and imply that complete redemption is never possible. In addition, informal sanctions associated with incarceration length may amplify the intended sentence.

A longer incarceration spell may not necessarily result in worse labor market outcomes as it might facilitate participation in various rehabilitating programs and training. This may increase human and social capital while strengthening personal traits and consequently result in a positive effect on the subsequent labor market outcomes, and in addition lower recidivism rates. Issues of deterrence which policy-makers and judges long have speculated upon, but nonetheless remain unknown.

I find that the longer incarceration spells induced by the reform result in lower unemployment rates along with a higher level of earnings for those who do obtain employment. These results suggest that an increase in incarceration length can improve the chances of rehabilitation rather than obstructing them.

The remainder of the paper is organized as follows:

Section 2 describes the background of the paper by introducing the link between incarceration length and labor market outcomes. In addition, section 2 briefly reviews the previous literature and findings on this field and highlights the predicaments this paper faces and the proposed solution to these. Section 3 introduces the data and provide descriptives of the sample. Section 4 introduces the econometric framework and section 5 presents the results of the estimations along with specification tests. Section 6 concludes.

2 Background

Theory suggests that productivity determines wage earnings y , and further that experience on the labor market and level of education determines productivity, as e.g. proposed in the seminal works by Becker (1964); Mincer (1974). Here I relax the original description of earnings, and let y denote any labor market outcome - including earnings, unemployment benefits, and reception of other public transfers. Also, many empirical studies show that the components of human capital do not account for all differences between various in individual's labor market outcomes (see e.g. Jencks (1972)). The residual differences may arise due to ones network-connections within the labor market (or alternatively social capital,¹ see e.g. Granovetter (1995)) along with search behavior (see e.g. Holzer (1988)). I.e. the effort in job-search, the productivity of the search method along with general contacts and social bonds within the labor market is a contributing factor to employment status and subsequent earnings. The central theme of the paper is how incarceration length I affects the labor market outcome y , either directly or as a consequence of changes in e.g. human capital, search behaviour etc. While level of education and age often serve as proxies for human capital, search behavior and social capital are - though affected by observable characteristics - largely unobserved.

The effects of an incarceration spell consists in two elements. The first effect arise from the incarceration per se, and the second arise from the length of the incarceration spell.

I define the first effect describing how incarceration per se affects the offenders future labor market outcomes as $dy/dI|_{I=0}$. A vast literature investigates this (for notable examples

¹The economic literature on social capital is much sparser than that on human capital. For an introduction e.g. see Glaeser, Laibson, and Sacerdote (2002).

see Freeman (1992); Waldfogel (1994); Nagin and Waldfogel (1995); Grogger (1995); Nagin and Waldfogel (1998)). Most of the studies find a negative association; that is $dy/dI|_{I=0} < 0$ (when considering outcomes as earnings or employment and the opposite when considering outcomes as unemployment). The literature generally ascribes this finding to stigma from the incarceration, as employers who face imperfect information may use criminal records as signals, revealing otherwise unobserved characteristics.

The second effect - the effect of the incarceration length once convicted to imprisonment - i.e. the effect in focus in this paper is captured by $dy/dI|_{I>0}$. The sign of this is ambiguous, as incarceration length may affect the subsequent labor market outcomes in different ways. On one hand, a longer incarceration spell may result in depreciation of human capital as offenders lose skills and productivity in general or miss potential work-experience and consequently, do not face the same productivity growth as non-incarcerated workers. Longer incarceration spells may also depreciate social capital by eroding personal connections which match workers to employers or provide information about possible job opportunities. On the other hand, longer incarceration spells may increase offenders productivity, e.g. through an increased likelihood of entering treatment for substance abuse or by receiving training while incarcerated. Further, longer incarceration spells may facilitate participation in rehabilitating programs or assistance in job search, thus lowering the costs of finding a job or increasing social capital, *ceteris paribus*. Which effects and theoretical concepts that dominate depends on the nature of the incarceration. If the incarceration spell is long it may affect level of human capital and thus earnings or employment status significantly, whether in positive or negative direction. However, I propose that only incarceration spells of certain length can cause such a change in level of human capital. Hence, for short incarceration

spells, changes in contacts within the labor market or stigma may be the dominant forces.

While a substantial literature investigates the first effect, the literature on the second effect of incarceration length labor market outcomes is much sparser. In addition, results from this literature are inconclusive.

Lott (1992a) estimates a first-difference model on a sample of convicted drug-offenders and finds no significant association between sentencing length and the difference in earnings before and after prison. In contrast, Lott (1992b) (again estimating a first-difference model) finds a significant monetary penalty to larceny- or theft-offenders. The estimates show that serving one additional month in prison reduces post-release earnings by as much as 32 percent compared to pre-incarceration earnings. Contrary to this, his estimated effect on embezzlement- and fraud-offenders were insignificant. Needles (1996) uses a quasi-experiment of randomly assigned “Transitional Aids”² to newly released prisoners convicted of various types of crimes. When examining the marginal changes in incarceration length by using a two-step Heckman procedure, to control for selection into employment, she finds no significant effects on earnings. Kling (2006) uses an instrumental variable of randomly assigned judges as exogenous variation in time incarcerated, on a sample of various types of offenders convicted by the federal judicial system in California.³ He finds no significant effects from incarceration length to neither future employment or earnings nine years after incarceration began. The study further finds relatively small positive but significant short term effects from incarceration length on future employment and earnings (one and two and a half year after release respectively), using data from Florida state prison system along

²A program designed to help newly released prisoners rehabilitate, see Needles (1996).

³Inmates of federal prisons must have committed a crime defined as “being within federal jurisdiction”. Kling mentions “interstate postal fraud and some drug cases” as examples of such crimes.

with the Californian sample (Kling do, however, stress that lack of exogenous variation in the latter model may bias the estimates). In addition, Kling suggest that the results arise because longer incarceration spells might enhance the possibility of receiving assistance which increases employability once released, as e.g. treatment against substance-abuse. Finally, Tranæs (2008); Landersø and Tranæs (2009) using a sample including incarcerated property and violent offenders in Denmark, identify a drop in wages and labor market attachment along with an increase in dependency of public transfers from before to after incarceration. Their conclusions do, however, ignore possible biases caused individual unobserved characteristics and the possibly endogenous relationship between incarceration length and labor market outcomes.

These opposing results from the previous studies underline the earlier introduced ambiguity, and hence $dy/dI|_{I>0}$ is unknown. A point that emphasizes the complexity of the research question here posed. Different incarceration lengths may correlate with unobserved individual characteristics which also affect the labor market outcomes. Incarceration length is then endogenous to labor market outcomes, which obscure the results and the causal reading of the estimates. Some of the existing studies use first difference or fixed effects estimations to obviate time-invariant unobserved individual characteristics, which affect both employability and crime proneness. However, these frameworks do not eliminate any probable relationships between unobserved time-varying components and labor market conditions. This corresponds to the case where layoffs, paycuts etc. prior to the incarceration can spark crime. Here previous levels of labor market outcomes or shocks will correlate with the length of the subsequent incarceration spell, which makes simply eliminating the individual fixed effects insufficient. One solution to this problem of endogeneity is to implement an instru-

mental variable as done in Kling (2006). However, his study suffers from an analogous problem of heterogeneous sample composition, as he uses a sample of pooled offender types like the rest of the previous literature.

Assuming homogeneous treatment effects in the samples of pooled, but fundamentally different offender types, may bias conclusions in arbitrary directions. This paper proposes a setup to take these predicaments into account, by applying a reform of the Penal Code in 2002 that allows me to identify the effect of incarceration length on subsequent labor market outcomes. The reform only affected a specific group of offenders, namely those convicted of a specific type of violent crime. In addition, the reform was unrelated to any fixed or varying behavioural characteristics of the offenders, as it applied to all violent offenders.

The Reform of the Penal Code

On May 8th 2002 the Danish government changed the Penal Code concerning violent crime. The change raised the maximum sentence length of “simple violence”⁴ from 1 year and 6 months to 3 years. The bill was effectuated within the month, and have affected all violent crimes committed after the 31st of May 2002. The aim of the bill was published in a justification:

The government finds that the previous level of sanctions in cases of crime harming others does not adequately reflect the victim’s suffering. Hence, the government wishes to increase the sanctions for such crimes. [Justification by Secretary of Justice Lene Espersen (2002), own translation]

⁴For a legal abbreviation on the term see <https://www.retsinformation.dk/Forms/R0710.aspx?id=126465>. For Statistics Denmark’s documentation see <http://www.dst.dk/Vejviser/dokumentation/times/emnegruppe/emne/variabel.aspx?sysrid=254130×path=5|963>.

Consequently, the reform targeted every violent offender and increased the sanctions for a given violent crime.

Figure 1 depicts the monthly averages of incarceration lengths for the sample⁵ the paper investigates.

(Figure 1 about here)

The figure shows that the average incarceration length is far from the maximum sentence length both before and after the reform. Nevertheless, the average incarceration length increased from 54.13 days before the reform to 60.96 days after; an increase of 12.6 percent.⁶ Further, the median incarceration lengths across the reform increased from 38 to 44 days.

The justification shows that the reform was meant to have a homogeneous effect, by targeting both first time felons and jailbirds. Yet, it is unclear whether the reform affected e.g. the sentence lengths of the few persons who committed the most serious (simple) violence (the ones who were bounded by the maximum sentence length) or all sentences. Figure 2 depicts the distributions of incarceration lengths in the sample for individuals committing crime prior to and after the reform, respectively:⁷

(Figure 2 about here)

Both distributions are most dense in the interval 20 to 70 days, with humps at 30 and 60 days which correspond to sentences of one and two months respectively. However, as seen from the figure it seems that the entire distribution shifts to the right across the reform, as the post-reform distribution in general has a smaller density at the interval in the lower part

⁵I will introduce the sample formally in section 3.

⁶The change is significant with a p-value < 0.01.

⁷The figure has been censored at 150 days. The censored group corresponds to 5.26 percent of the sample prior to the reform and 6.30 after.

of the sentence range and a greater density at the high end of the range. Finally, there does not seem to be remarkable differences between the tails of the two distributions which might question whether the reform increased the incarceration length for all offenders. However, as a consequence of the few observations receiving sentences above 70 days any non-extreme changes are practically undetectable. I therefore conclude, in lieu of figure 2 and the rationale of the bill, that the incarceration length to any type of violent crime within the category of simple violence was increased by the reform.

3 Data

The paper uses administrative data from Statistics Denmark⁸ on criminal records, education, earnings, age, gender, ethnicity, marital status and children, along with information on recipients of public transfers from the DREAM-database.⁹ The different information is linked by the individual specific social security number. Further, the criminal registers includes a unique case specific code, verdict (guilty, acquitted), verdict type (imprisonment, suspended sentence, fine, warning, containment), date of crime, type of crime, incarceration date, release date, type of incarceration (e.g. custody, serving term of imprisonment etc.). I only consider an individual criminal if she has been convicted of a crime and a subsequent trial is not ongoing because of appeals. I further discard all types of convictions not resulting in an sentence to imprisonment¹⁰ for simple violence from the data.

The frequency of violent crime decreases with age and the majority of crimes are com-

⁸Only Danish residents enter the registers.

⁹Containing information on every Danish citizen who have received public benefits/transfers of any kind. For further information see: http://www.dst.dk/upload/microsoft_word_-_beskrivelse_af_dream_koder_-_version_22.pdf.

¹⁰This includes all who have been convicted to detainment, since these are categorised as mentally ill.

mitted by men. As a consequence I censor the data to only include men who are age 45 or younger at the date of the crime,¹¹ in order to assure a minimum of homogeneity between individuals in the sample. Further, as special conditions apply for individuals below age 18 I only include legal adults.

The data is available for several years prior to and after incarceration. Hence, I create a panel with one time series per individual per case (which have lead to conviction to imprisonment for simple violence). However, individuals in the sample experience their specific incarceration spell at different points in time and I therefore create a new time-line. I denote the time of incarceration time 0, the month prior to this time -1 and so forth. Further, I denote the month after release time 1 and the subsequent month time 2 and so forth, and delete all points in time from the incarceration date to the release date, for all individuals.

The analysis only includes observations related to a crime committed between December 2000 and November 2003. The data thus consists of individuals incarcerated as a consequence of simple violence committed in a band of 18 months on each side of the reform. I have chosen a band width to reach a sample size that on one hand obviates random variation, and on the other does not allow changing demographics and macro trends to compromise the comparability between the earlier and latter parts of the sample. The group committing the crime before the reform is the control group and the group committing it after the reform is the treatment group. No individual in the sample experience more than one incarceration for simple violence in the time-span considered in this paper.

For estimation purposes I discard the individuals incarcerated prior to the specific incarceration at time 0 and not released at least 12 months before. Additionally, some individuals

¹¹Individuals aged 46 or higher at the time of the crime and women in general consists 3.7% of the sample at this point.

experience incarceration, immigrate or die during the three years after release I investigate. To avoid multiple treatments and attrition I discard these from the sample. The panel is therefore perfectly balanced. This censoring regards 43 and 40 persons from the treatment and control groups respectively (2.5 and 2.2 percent of the final sample). Of these, a large proportion is due to death or immigration. Only 48 persons in total (22 of the control and 26 of the treatment group) leave the sample as a result of a sentenced to imprisonment, a suspended sentence or unexplained events during the first three years upon release. The censoring brings the final sample size to 1,748 individuals; 875 individuals belonging to the control group and 873 belonging to the treatment group.

Descriptives of the outcomes

This study focuses on three outcomes: Unemployment, dependency of other public transfers and earnings.

Table 1 shows the means of the three outcome variables. I measure the means of the outcome variables at time -12 (12 months prior to incarceration).

(Table 1 about here)

The table shows that the sample has few resources as measured by the three outcomes. Average monthly earnings (that is gross excluding public transfers) are approximately 8,800. Further, a sizable proportion - 33 and 15 percent respectively - are unemployed or dependent on other public transfers.

Table 2 presents the corresponding means of the outcome variables by the treatment and control group, along with the p-values for a t-test for differences in the means, in order to

investigate whether the reform was unrelated to the two groups' labor market outcomes prior to incarceration.

(Table 2 about here)

The table shows no significant differences in any of the outcome variables.

Figure 3 depicts the sample's average monthly rates of unemployment along with the average monthly dependency of other public transfers for the two groups from time -60 to time 36.

(Figure 3 about here)

The figure shows that approximately 20 percent of the control and treatment group are unemployed 60 months prior to incarceration and the two fractions follow each other closely hereafter. The rates increase the last three years prior to incarceration to almost 40 percent, and both groups' unemployment rates show large increases upon release. These are followed by even drops in the remaining period. A drop that is largest for the treatment group. The rates of unemployment for the two groups do not differ significantly from each other in any of the 60 months prior to incarceration.

Furthermore, the figure shows that the average dependency of other public transfers of both groups initially are approximately 12 percent; though slightly higher for the control group. For both groups dependency of other public transfers increase slightly until the time of incarceration. After release, the dependency of other public transfers of both groups increase evenly toward the end of the depicted period, to a level of almost 20 percent. While some of the differences between the treatment and control groups are significant at a 5 percent level, all differences observed less than 30 months prior to the incarceration are insignificant.

Figure 4 shows the average monthly earnings of both groups, from time -60 to time 36.

(Figure 4 about here)

As figure 4 shows, monthly earnings are approximately 8,000 DKK¹² (1,072 Euros) for both groups, 60 months prior to incarceration. Average earnings of both groups increase the subsequent three years. However, I observe a drop the last two and a half year leading up to the incarceration. Upon release, the average earnings increase monotonically, though the increase is largest for the treatment group. The two group's average earnings are not significantly different from each other on a five percent significance level in any months during the last four years leading up to the incarceration spell, except for a single month (at time -8).

Descriptives of the covariates

Table 3 shows summary statistics for characteristics of the offenders and indicators of previous criminal history. I measure the socio-economic variables the table presents at time -12, while I measure the indicators of previous criminal history at time 0.

(Table 3 about here)

The table shows that the sample has a low level of resources as measured by socio-economic variables. Relatively few are employed, the majority holds no qualifying education beyond secondary school and few are married or cohabiting. Table 3 also shows that the offenders have experienced their first recorded contact with the police at time -109. Further, they have committed the first convicted crime at time -98, and 18 percent of these were violent crimes.

¹²In 2005 prices 1 DKK corresponds to 0.134 Euros

Moreover, they have experienced their first incarceration at time -46. More than 80 percent have received a conviction prior to the one this paper studies. 43 percent have received a conviction of a violent crime, whereas 64 percent have received a conviction for a property crime. Finally, the average individual in the sample has more than four convictions prior to the one in question.

Table 4 shows descriptives of the sample divided by treatment status along with the p-value for a t-test of differences between the means of the two groups. Like table 2, this comparison shows whether the reform is the only difference between the two groups.

(Table 4 about here)

As shown by the table, significantly more controls are married and fewer treated have children. I find no other significant differences between the two groups.

In addition to individual level characteristics, I also need to consider potential macro level differences. Denmark experienced a small recession that began in late 2001 and ended as the year 2004 began. The recession was replaced by a boom that lasted for remaining part of the papers data period. Yet, figure 3 and 4 suggested that the two group's labor market outcomes did not suffer from these potential differences. Further, the official policy for the transition from life in jail to life in freedom did not change in the period of time the paper considers. Hence, I conclude that the two groups face the same general conditions on the labor market before and after their incarceration spell and that there is little sign of a change in average characteristics of the "violent offender" across the reform nor any sign of a difference between the two groups as a result of macroeconomic trends. Consequently, the two groups are comparable and further subject to equal trends. The reform therefore provides an exogenous increase in incarceration length.

It should be noted that the time paths of both the unemployment rate and earnings display a spike/dip in the months leading up to the incarceration. The spikes/dips could indicate the initiation of a criminal trajectory. Additionally, they display a great resemblance to Ashenfelter’s dip (Ashenfelter (1978)). As this suggested with the effect evaluation within labor economics, so does the spikes/dips of this paper imply that there might be a heavy self-selection into incarceration. However, the magnitude spikes/dips do not differ between the treatment and control group and I can therefore disregard these in the further analysis.

4 Econometric Framework

This paper evaluates the effect of a treatment (i.e. a reform of the Penal Code) on subsequent labor market outcomes. I assess this treatment effect by following the terminology first introduced by Rubin (1974) and adopted by the general treatment literature and define the treatment effect on the treated as:

$$\delta_{ATT} = E(\delta \mid D_i = 1) = E(y_i(1) \mid D_i = 1) - E(y_i(0) \mid D_i = 1) \quad (1)$$

where D_i is a binary treatment indicator equal to 1 if i receives treatment and 0 otherwise. $y(1)$ denotes the outcome for individual i sentenced by the post-reform guidelines and $y(0)$ for the same person sentenced by the pre-reform guidelines. I.e. δ_{ATT} expresses the difference between the expected outcome of individual i in the treatment and control states respectively, under the condition that he is eligible for treatment. Obviously I cannot observe individual i in both states. However, if the treatment is completely random I may substitute $E(y_i(0) \mid D_i = 1)$ with $E(y_i(0) \mid D_i = 0)$ as these are equal. In order to reduce

the variance of the estimated effect, it is convenient to condition on independent covariates. Given the exogeneity of the reform, $E(y_i(0) | x_i, D_i = 1) = E(y_i(0) | x_i, D_i = 0)$ is also satisfied. Following Rosenbaum and Rubin (1983) I may therefore alternatively obtain δ_{ATT} as:

$$\delta_{ATT} = E(\delta | D_i = 1) = E(y_i(1) | x_i, D_i = 1) - E(y_i(0) | x_i, D_i = 0) \quad (2)$$

Imposing a parametric form to the relationship between the outcome y , the observable characteristics x , the reform D and the unobserved components over time (defined as months), I may express this as:

$$y_{is} = \beta x_{is} + \delta D_i + a_i + e_{is} \quad (3)$$

where y_{is} is a given labor market outcome of individual i in month $s > 0$ (that is month s after the incarceration at time 0), x_{is} a set of observable characteristics summarizing human capital etc. that account for observed differences between individuals, a an unobserved fixed effect and e an unobserved idiosyncratic error term. δ is the parameter of interest - i.e. the effect on y of the increase in incarceration length induced by the reform.

Keeping in mind D_i is a dummy indicator of treatment that I seek to evaluate over several periods from time of release s - while correcting for pre-incarceration levels and personal characteristics - I get that:

$$\Delta y_{is} = \beta \Delta x_{is} + \sum_{s=2}^{s=36} \gamma_s ds + \sum_{s=1}^{s=36} \delta_s D_i + \Delta e_{is} \quad (4)$$

by differencing of equation (3) with period time -12 (that is 12 months prior to the

beginning of the incarceration spell). Hence $\Delta y_{is} = y_{is} - y_{i,-12}$, $\Delta x_{is} = x_{is} - x_{i,-12}$, $\Delta e_{is} = e_{is} - e_{i,-12}$, β a vector measuring the effects of the covariates¹³ and ds is a indicator of time since release equal to 1 if $time = s$ and 0 otherwise. δ_s is the effect of the reform on the labor market outcomes in period s . Thus δ_s captures the effect of the reform each month since release, in a total of three years.

In order to obtain consistent results, non of the terms I include in equation (4) can correlate with the unobserved components. Section 3 showed that the magnitude of the dip along with the trends and levels of the outcome variables prior to incarceration did not differ significantly across the two groups and the reform provided an exogenous shift in incarceration length (so D_i is orthogonal to the unobserved factors embedded in the time invariant a_i and the idiosyncratic error e_{is}). In addition, the conditions apply to the covariates. The econometric framework therefore applies a method that eliminates all time-invariant variables including a_i because the condition that personal characteristics $E(a_i x_{is}) = 0$ is likely not satisfied.

If prior and recent levels of characteristics x are independent of the idiosyncratic error term e , if the reform provides an exogenous shift in incarceration length, and if the parameters β and δ_s from equation (4) are homogeneous across individuals, I can estimate the parameters consistently by *OLS*. As the observable differences between the two groups are negligible, it seems reasonable to assume that there are no fundamental differences between the unobserved characteristics of the two groups. Thus I consider the conditions satisfied.

¹³ Δx includes a constant term, changes in four agesplines, changes in marital status, changes in children, three indicators of changes in education status and changes in area of residence.

5 Results

Table 5 shows the results of (δ_s) from month 1 to 36 after release - i.e. the effect of incarceration length identified by the reform on the three outcome variables - along with the standard errors of the estimates in parentheses.¹⁴

I measure unemployment rate and dependency of other public transfers in percentages. Hence, a parameter estimate of e.g. 0.01 correspond to an increase of the respective rate of 1 percentage-point. I measure earnings on monthly basis; hence the parameter estimate corresponds to an change in earnings of 2005 DKKs (0.13 Euros) per month.¹⁵ Earnings are by definition zero for persons who are unemployed. Note that the estimates (δ 's) are not accumulative; i.e. a parameter estimate of 0.01 one year after release implies that individuals from the treatment group experiences an outcome one percentage-point higher than the control group, all else equal, at that given time - regardless of the sign and size of the earlier estimates.

(Table 5 about here)

Unemployment The table shows that the all of the estimates are negative and, except for six of the estimates within the first year of release, significant on a five percent significance

¹⁴As proposed by Bertrand, Duflo, and Mullainathan (2004) a DID estimation of treatment effects tend to overrate the significance of the given treatment, because ordinary standard errors fail to take account of serial correlation in the outcomes, which is often observed in outcomes related to the labor market. They emphasise that even the “normal” heteroskedastic robust standard errors does not correct the bias. As one solution they propose to obtain the standard errors of the estimates by a wild-bootstrapping procedure.

The paper applies a wild-bootstrap procedure as proposed by Flachaire (2005); Davidson and Flachaire (2008), with $\rho_{is} = \begin{cases} -1 & \text{with probability } 0.5 \\ 1 & \text{with probability } 0.5 \end{cases}$, so $E(\rho_{is}) = 0$ and $\sigma_\rho^2 = 1$.

¹⁵Though it is customary, I do not use log earnings because 77 percent of the sample experience months with zero earnings.

level. The size of the estimates indicate that the reform induced a drop in the unemployment rate of approximately 4-5 percentage-points. As time since release increases so does the numerical size of the estimates; to a level of roughly 7 to 10 percentage-points two years after release. This level is persistent for the remaining estimates. Thus the reform results in significantly lower unemployment rates, and the effect increases with time since release.

Dependency of other public transfers The table shows that all of the estimates (but two) are positive. Nonetheless, they are all numerically small and insignificant even at a ten percent level. In addition, the estimates are not significant when tested jointly. One may have suspected a substitution between unemployment and other public transfers. However, the insignificant estimates of dependency of other public transfers reject this suspicion.¹⁶ This thus suggest that the change in incarceration length which followed the reform increased employment for the sample.

Earnings The numerical size of the estimates exhibit greater volatility than the estimates with unemployment and dependency of public transfers. In addition, the estimates show no effect of the reform on subsequent earnings, during the first 18 months after release.

¹⁶In order to investigate the possibility of opposing effects within the composite measure of dependency of other public transfers, I have estimated the model with a subdivision of this outcome into two general categories. First, voluntary efforts revealing an interest in (re-)entering the labor market at some point. E.g. education support, financial aid to upgrading work-oriented skills, specific voluntary labor market programs. Second, mandatory programs in order to be eligible for reception of benefits or passive support without any requirements, such as sick leave, early retirement relating to lack of employability etc. Common for all the types of benefits included the latter group is that they do not target a future place in the labor market. The former was unaffected by the reform, while there was weak sign of a increase to the latter category as a consequence of the reform. Yet, this result was not robust nor significant on a sufficient level to draw any conclusions from.

Additionally I have estimated the model with total dependency of public transfers (the sum of unemployment and dependency of other public transfers). The results with this outcome did not change any conclusions, as they were not significantly different from those with rate of unemployment as an outcome. Hence, I conclude that the results are not caused by a substitution effect.

After the first 18 months the estimates show a positive and significant effect of the reform of approximately 1,000 DKK (134 Euros). This effect is persistent and even slightly increasing over time. As a consequence of the reform, the treatment group earns around 1,500 DKK (201 Euros) more than the control group three years after release. The estimates thus suggest a positive effect of the increase in incarceration length, as induced by the reform. An effect that increases over time. The data does not allow me to distinguish between whether the earnings increases appear due to higher levels of productivity or lower unemployment. However, since table 5 also showed that employment increased when treated, resulting in a greater number of individuals with none-zero earnings, I suspect this to be the dominant force.

Macroeconomic trends

Identifying an effect using a reform may pose a problem if the reform is introduced parallel to other macro level changes. In that case the pre- and post reform groups would be subject to other macroeconomic trends, which I would - wrongfully - ascribe as effects from reform. To investigate whether that is a problem in my analysis, I restrict the sample such that I only include persons sentenced to imprisonment for a crime committed 6 months prior to or after the implementation of the reform (rather than 18), reducing the sample size to 611; 361 in the control group and 250 in treatment. The reduced sample size may affect significance levels. However, it should not affect the size of the estimates from table 5, if these are robust.

Table 6 shows the estimates (corresponding to table 5) along with the standard errors of the estimates in parentheses.¹⁷

(Table 6 about here)

¹⁷Again I have calculated the standard errors using a wild-bootstrap procedure.

The table reveals some noteworthy differences between the original estimates from table 5 and those obtained using the restricted sample; both in size and level of significance. The estimated parameters of the effect on the unemployment rate for the first 18 months, are generally numerically larger than the estimates presented in table 5. Still, the majority is highly significant.

The estimated effects on dependency of other public transfers show some sign a positive effect from the reform. However, the estimates are only significant for the first year since release and additionally, the size and significance of the estimates are not consistent, as the latter estimates are numerically smaller than the former and generally insignificant.¹⁸

The estimated effects on earnings from table 6 display some differences when comparing these to the results obtained from the full time span sample. Hence, there is little evidence of the positive effect on earnings found in the initial estimates. The coefficients are of mixed sign and only one is significant on a five percent level.

In general I expected fewer estimates to be significantly different from zero, due to the heavily reduced sample size. However, I still need to accept the change of sign of some coefficients, as an indicator lack of robustness. Hence, it is possible that the positive effect on earnings obtained in the first model results from different macroeconomic settings rather than from the reform. Contrary to this, the effect on unemployment appears robust, as it shifted in the opposite direction of the probable macro trend bias. To confirm this, I define a series of pseudo reforms for each month from July 1999 to June 2005 and test for joint significance for the effects on unemployment. The pseudo reforms that does not coincide

¹⁸Again I have estimated the model with total dependency of public transfers. The results were not significantly different from those to unemployment. Therefore, they suggest that the rate of unemployment drops as a consequence of the reform, while there is no attrition from the labor market as it does not affect dependency of other public transfers.

with the time of the reform at June 2002 should all be insignificant, while the effects in the time around the real reform should be significant. Figure 5 show the p-value for a Wald-test for joint significance.¹⁹

(Figure 5 about here)

The figure shows that the effects of every pseudo reform applied to data that does not include June 2002 are insignificant. As the pseudo reforms approaches the timing of the real reform the level of significance increase. This confirms that the effects of the reform are robust and not caused by macroeconomic trends or fluctuations.

Non-zero earnings

By definition, monthly earnings are zero if an individual experiences full unemployed in a given month. 459 individuals from the full time-span-sample and 151 from the restricted are unemployed all 36 months after release. In the following, I restrict the sample to the fractions of the full time-span- and restricted time-span-samples, which obtain employment in at least one of the first 36 months after release from incarceration. Table 7 shows the estimates with earnings as outcome.

(Table 7 about here)

When comparing the first set of estimates of table 7 to the last two columns in table 5, no noteworthy differences arise. The size, signs and levels of significance are approximately alike. However, when comparing the second set of estimates in table 7 to those of table 6, I see that eliminating fully unemployed individuals results in an increased number of positive

¹⁹For each pseudo reform the data is constructed as described in Section 3. Hence, each new sample includes violent crimes committed in a band of 18 months on each side of the pseudo reform.

significant estimates. Further, the effects of the reform on the earnings of the individuals who actually obtain employment are jointly significant. Hence, the table show a positive effect on earnings from the increase in incarceration length for those employed.

6 Conclusion

The paper investigates how incarceration length affects unemployment rates, dependency of other public transfers and earnings. I use a reform of the Penal Code in 2002 to facilitate causal inference.

My estimates showed that an increase in incarceration length resulted in persistently lower rates of unemployment in a sample of violent offenders. During the first year upon release, unemployment dropped 4-5 percentage-points, and the drop increased in the subsequent years. This result was robust to limitation of the time span and in addition to a series of pseudo reforms. In contrast, dependency of other public transfers was not affected by the reform. This suggests that the increase in incarceration length increased the residual outcome employment. Finally, the effects on earnings were increasing with time from release. However, the increases in earnings were not robust to limitation in the time span. Yet, the fraction of the treatment group who had employment experienced a positive significant effect; also when limiting the time span. I therefore conclude that the increase in incarceration length, resulted in higher earnings for the employed.

Limitation and discussion

The results of this paper relies on the exogeneity of the reform. I should therefore consider the possibility that the reform caused the average offender to change. It is reasonable to assume that longer sentences would result in a treatment group that has weaker socio-economic potential than the control group - or in other words with less employment-security and poorer affiliation to the labor market etc. However, the results (especially on the rate of unemployment) show the opposite. The likely direction of bias, if the use of the reform in this setting is endogenous, is thus *not* against zero. The direction of the bias will be changed indeterminately if the two groups experienced different trends of the outcomes or alternatively if the dips/spikes prior to incarceration differed. The paper showed that this was not the case. Nonetheless, a point which call for further investigation is the the pre-incarceration dips/spikes. Investigations which should elucidate whether the dips/spikes are a general phenomenon for all offender-types and further, how they are related to the criminal act itself and the severity of crime in order to uncover any self-selection.

The conclusion that longer incarceration length does not result in worse - but possibly better - labor market outcomes is in accordance with the results from Needles (1996) and Kling (2006). It seems unreasonable to assume that the results stem from changes in human capital, as the reform only increased the average incarceration lengths by approximately 7 days. A likely explanation of the results is that the increase in incarceration length as a result of reform actually increases participation in rehabilitating programs that the offender's pre-reform sentence would have been too short to participate in, otherwise leaving the offenders solely with the possible stigma, job-loss and general alienation from the labor market which incarceration might involve. This participation could then dominate any stigma from the

incarceration in general and the increased incarceration length in particular. The finding that the fraction with employment experienced higher earnings as well, is in accordance with this proposition, as lower costs of job-search and higher social capital should increase earnings.

The data does not allow me to identify the causes of the effect on earnings, though I suspect the higher level of employment to be the dominant factor. Additionally, the effects may suffer from a selection bias from employment, e.g. as proposed by Needles (1996). Consequently, I cannot extrapolate the positive effect on earnings to the entire sample, but only to those who actually obtain employment.

While the paper is very selective with respect to the composition of the sample, there are still noteworthy heterogeneity which may obscure the results. Most disturbing is the long tail of the distribution of lengths of incarceration spells. Discarding the observations belonging to the tail could provide a more homogeneous sample. However, selecting observations because of incarceration length would reintroduce the endogeneity that the paper attempts to avoid. In addition, if the increase in incarceration length from the reform was local and not general, the results are a local average treatment effect rather average treatment effect on the treated. Yet, this does not change the general interpretation of the results.

Further, the conclusion is only valid for the subgroup of violent offenders that the paper includes in the sample and it is not a general treatment effect that I may extrapolate to other offender types. However, if the origin of positive effects of these changes in incarceration length on employment and earnings result from increase program participation, the conclusion may hold for other types of offenders receiving sentences of approximately a month or two. Hence, an obvious task for future analysis is to test the conclusion on other offender

types. Also, it is worth while to consider whether very short sentences only stigmatize offenders without providing a proper rehabilitation and transition to life in freedom.

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A Figures and tables from the main text

Figure 1: Monthly averages of incarceration lengths

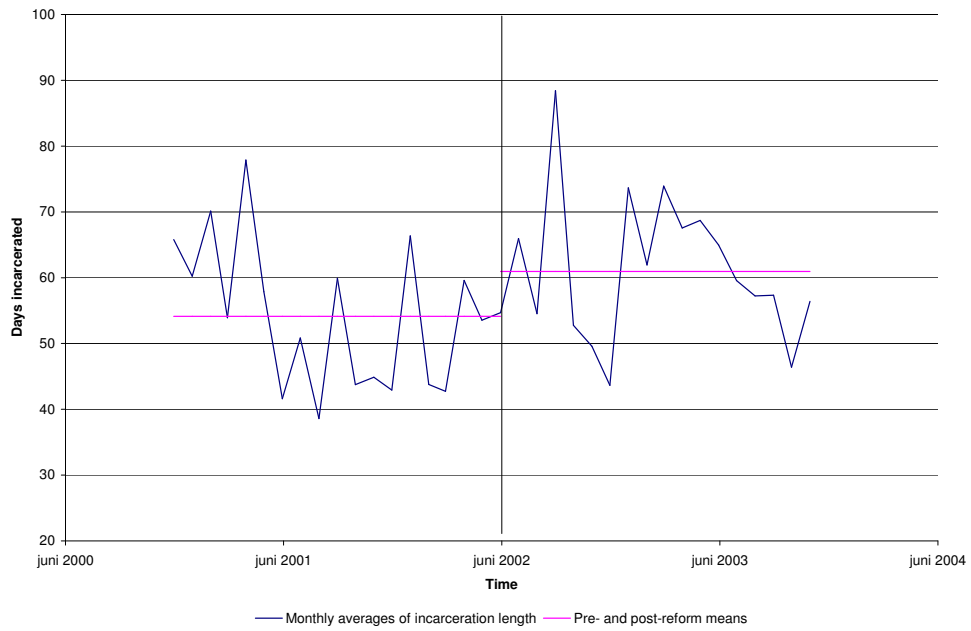


Figure 2: Pre- and Post-Reform Distributions of Incarceration Length

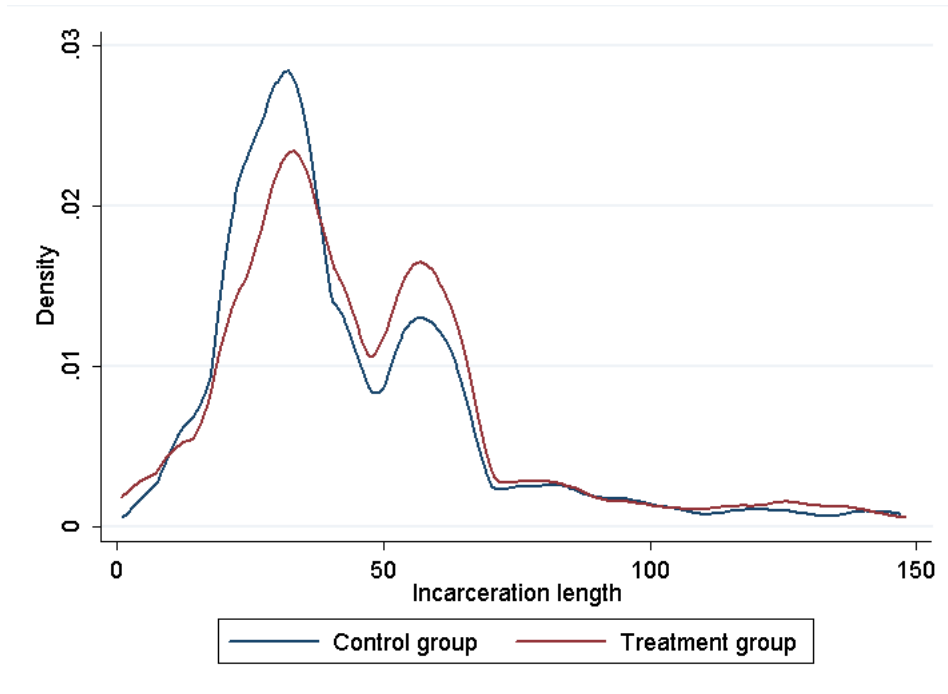


Figure 3: Average rates of unemployment and dependency of other public transfers for the control and treatment group

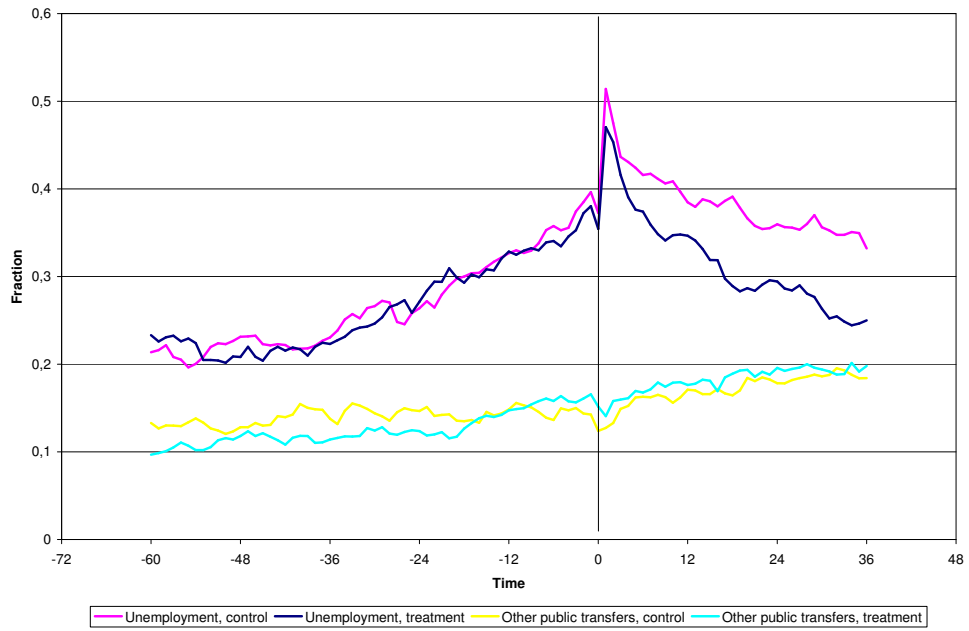


Figure 4: Average earnings for the control and treatment group

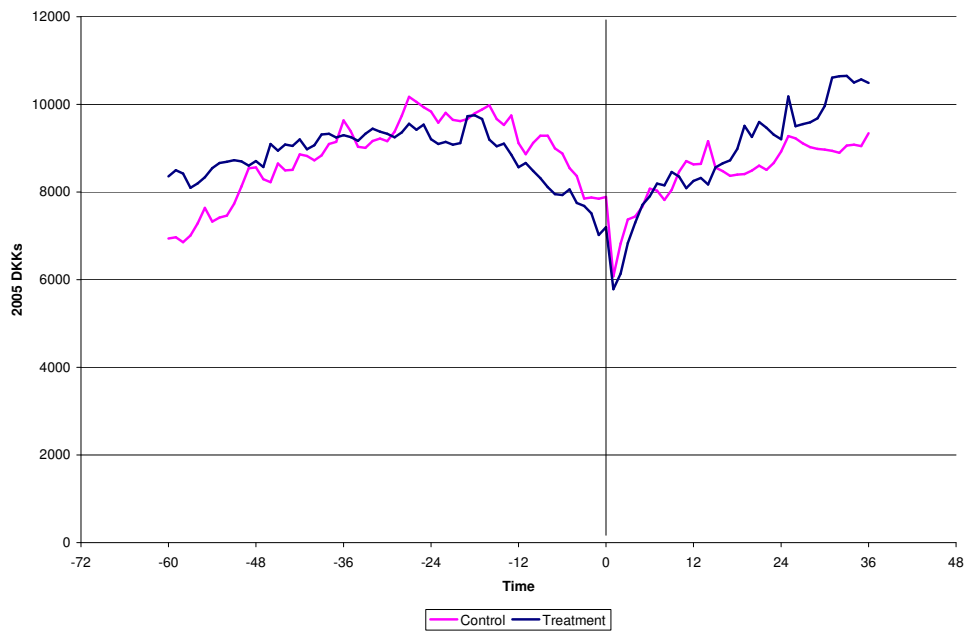
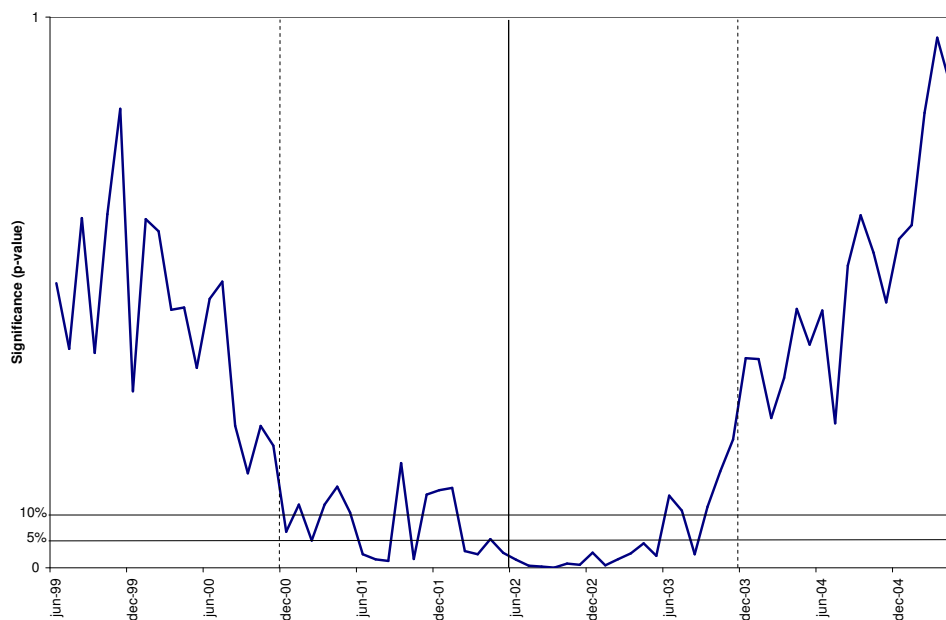


Figure 5: Significance level for joint tests of pseudo reforms on unemployment rate



The vertical solid line mark the time of the reform. The vertical stapled lines mark a band of 18 months prior to and after the reform, to indicate where the associated timespan coincide with the real reform.

Table 1: Means of the outcomes variables

Variable	Mean	Std. Dev.
Unemployed	0.33	0.46
Dependent of other public transfers	0.15	0.48
Earnings	8,843	11,507
N	1,748	

Table 2: Means of the outcome variables by treatment status

Variable	Control	Treatment	P-value
Unemployed	0.33	0.33	0.94
Dependent of other public transfers	0.15	0.15	0.95
Earnings	9,118	8,566	0.32
N	875	873	

Table 3: Summary statistics of the sample

Variable	Mean	Std. Dev.
Age	28.13	7.80
Married	0.23	0.42
Cohabitant	0.28	0.45
Have children	0.36	0.48
Non-western immigrants or descendants	0.12	0.48
Employed	0.52	0.48
No skilled education	0.67	0.47
Vocational or skilled	0.23	0.42
Upper secondary or higher	0.10	0.30
Have been convicted before	0.83	0.37
Have been convicted of a violent crime before	0.48	0.50
Have been convicted of a property crime before	0.67	0.47
Months since 1st date of crime leading to an indictment	109	83
Months since 1st date of crime leading to an conviction	98	84
Months since 1st incarceration	46	73
First conviction (if any) was for a violent crime	0.18	0.44
Number of previous convictions	4.44	5.89
N	1,748	

Table 4: Summary statistics of the sample by treatment status

Variable	Control	Treatment	P-value
Age	28.40	27.86	0.15
Married	0.26	0.20	<0.01
Cohabitant	0.28	0.27	0.65
Have children	0.38	0.34	0.10
Non-western immigrants or descendants	0.11	0.13	0.24
Employed	0.52	0.52	0.97
No skilled education	0.68	0.67	0.73
Vocational or skilled	0.23	0.23	0.71
Upper secondary or higher	0.09	0.10	0.98
Have been convicted before	0.83	0.83	0.98
Have been convicted of a violent crime before	0.50	0.47	0.20
Have been convicted of a property crime before	0.67	0.66	0.49
Months since 1st crime leading to an indictment	110	108	0.64
Months since 1st crime leading to a conviction	99	97	0.53
Months since 1st incar.	47	44	0.51
First conviction (if any) was for violent crime	0.17	0.19	0.44
Number of previous convictions	4.45	4.43	0.94
N	875	873	

Table 5: Estimation results

Months	Unemployment	Public transfers	Earnings
1	-0.052** (0.025)	0.014 (0.018)	334 (493)
2	-0.031 (0.026)	0.029 (0.020)	-196 (541)
3	-0.029 (0.026)	0.015 (0.020)	-40 (524)
4	-0.048* (0.025)	0.012 (0.019)	361 (525)
5	-0.054** (0.026)	0.013 (0.021)	434 (556)
6	-0.045* (0.026)	0.008 (0.021)	207 (575)
7	-0.062** (0.026)	0.012 (0.021)	695 (672)
8	-0.066*** (0.026)	0.018 (0.020)	765 (566)
9	-0.067*** (0.026)	0.011 (0.020)	893 (669)
10	-0.066*** (0.026)	0.020 (0.020)	489 (572)
11	-0.056*** (0.026)	0.015 (0.020)	-55 (590)
12	-0.045*** (0.027)	0.000 (0.020)	538 (569)
13	-0.048*** (0.026)	0.003 (0.020)	317 (576)
14	-0.067*** (0.026)	0.012 (0.020)	-256 (834)
15	-0.076*** (0.026)	0.012 (0.021)	633 (583)
16	-0.070*** (0.026)	-0.001 (0.020)	728 (571)
17	-0.096*** (0.026)	0.024 (0.021)	421 (576)
18	-0.105*** (0.026)	0.032 (0.022)	573 (591)
19	-0.098*** (0.026)	0.028 (0.022)	1,260** (635)
20	-0.082*** (0.026)	0.014 (0.022)	926 (610)
21	-0.076*** (0.026)	0.006 (0.021)	1,321** (603)
22	-0.067*** (0.026)	0.008 (0.021)	1,093* (597)
23	-0.066*** (0.026)	0.009 (0.021)	1,020* (600)
24	-0.071*** (0.026)	0.019 (0.021)	722 (594)
25	-0.080*** (0.026)	0.018 (0.021)	1,372 (735)
26	-0.082*** (0.026)	0.017 (0.021)	653 (610)
27	-0.072*** (0.026)	0.017 (0.022)	718 (633)
28	-0.088*** (0.026)	0.021 (0.021)	933 (643)
29	-0.100*** (0.026)	0.017 (0.021)	973 (644)
30	-0.096*** (0.026)	0.017 (0.021)	1,184* (622)
31	-0.105*** (0.025)	0.013 (0.021)	1,933*** (664)

Significance levels: * : 10% ** : 5% *** : 1%

Table 5: Estimation results continued

Months	Unemployment	Public transfers	Earnings
32	-0.097*** (0.025)	-0.001 (0.021)	1,985*** (643)
33	-0.102*** (0.025)	-0.002 (0.022)	1,891*** (637)
34	-0.112*** (0.025)	0.014 (0.021)	1,819*** (610)
35	-0.112*** (0.026)	0.010 (0.021)	1,980*** (602)
36	-0.091*** (0.025)	0.013 (0.021)	1,499*** (608)
R ²	0.030	0.016	0.030
N	1,748		

Significance levels: * : 10% ** : 5% *** : 1%

Table 6: Estimation results reduced time span

Months	Unemployment	Public transfers	Earnings
1	-0.090** (0.042)	0.067** (0.028)	-150 (812)
2	-0.058 (0.043)	0.089*** (0.032)	-901 (836)
3	-0.064 (0.042)	0.090*** (0.034)	-710 (879)
4	-0.064 (0.042)	0.069** (0.035)	-224 (912)
5	-0.082** (0.041)	0.074*** (0.035)	-194 (977)
6	-0.074* (0.041)	0.065** (0.033)	-264 (1,049)
7	-0.113*** (0.041)	0.083*** (0.034)	241 (954)
8	-0.103*** (0.041)	0.064* (0.034)	732 (955)
9	-0.121*** (0.042)	0.062* (0.034)	1,188 (914)
10	-0.141*** (0.042)	0.070** (0.033)	782 (932)
11	-0.154*** (0.042)	0.079** (0.033)	427 (912)
12	-0.130*** (0.043)	0.069** (0.035)	23 (942)
13	-0.143*** (0.042)	0.070** (0.034)	564 (941)
14	-0.168*** (0.042)	0.084** (0.034)	737 (956)
15	-0.192*** (0.043)	0.076** (0.035)	1,536 (986)
16	-0.163*** (0.044)	0.032 (0.035)	1,936 (945)
17	-0.164*** (0.044)	0.046 (0.035)	1,667 (978)
18	-0.144*** (0.044)	0.052 (0.035)	1,344 (966)
19	-0.147*** (0.043)	0.060* (0.035)	2,222 (1,187)
20	-0.107** (0.045)	0.044 (0.037)	878 (952)
21	-0.116*** (0.043)	0.042 (0.037)	1,050 (963)
22	-0.101** (0.044)	0.030 (0.037)	1,172 (977)
23	-0.105** (0.043)	0.046 (0.037)	611 (976)
24	-0.104** (0.044)	0.066* (0.038)	-300 (1,002)
25	-0.142*** (0.044)	0.076** (0.039)	1,201 (1,506)
26	-0.113*** (0.045)	0.072* (0.038)	-786 (1,111)
27	-0.106** (0.044)	0.078** (0.037)	531 (1,053)
28	-0.086** (0.044)	0.071* (0.038)	-1,108 (1,092)
29	-0.085** (0.044)	0.057 (0.037)	-1,534 (1,260)
30	-0.106** (0.044)	0.052 (0.036)	-253 (1,037)
31	-0.124*** (0.043)	0.057 (0.037)	829 (1,188)

Significance levels: * : 10% ** : 5% *** : 1%

Table 6: Estimation results reduced time span continued

Months	Unemployment	Public transfers	Earnings
32	-0.080* (0.043)	0.038 (0.037)	690 (1,067)
33	-0.104** (0.045)	0.040 (0.036)	983 (1,063)
34	-0.107** (0.045)	0.063* (0.037)	381 (1,034)
35	-0.105** (0.044)	0.051 (0.037)	240 (1,036)
36	-0.107*** (0.043)	0.054 (0.036)	-196 (990)
R ²	0.052	0.052	0.039
N	611		

Significance levels: * : 10% ** : 5% *** : 1%

Table 7: Estimation results with non-zero earnings

Full time-span			Restricted time-span				
Months	Earnings	Months	Earnings	Months	Earnings		
1	594 (628)	19	1,648** (846)	1	-5 (1,138)	19	3,170 (1,548)
2	-137 (686)	20	1,196 (818)	2	-1,006 (1,219)	20	1,588 (1,255)
3	55 (672)	21	1,764** (797)	3	-1,020 (1,145)	21	1,881 (1,284)
4	559 (657)	22	1,432* (779)	4	-363 (1,144)	22	2,052 (1,235)
5	607 (720)	23	1,312* (770)	5	-198 (1,210)	23	1,256 (1,226)
6	309 (750)	24	936 (783)	6	-124 (1,383)	24	69 (1,249)
7	945 (743)	25	1,781* (952)	7	1,165 (1,297)	25	1,932 (1,854)
8	1050 (748)	26	853 (814)	8	1,766 (1,323)	26	-638 (1,413)
9	1235 (720)	27	95 (826)	9	2,167 (1,247)	27	-418 (1,354)
10	654 (736)	28	1,218 (824)	10	1,634 (1,249)	28	-1,183 (1,409)
11	-95 (787)	29	1,247 (856)	11	1,133 (1,216)	29	1,701 (1,588)
12	326 (766)	30	1,536* (813)	12	427 (1,215)	30	-142 (1,348)
13	448 (757)	31	2,505*** (851)	13	958 (1,247)	31	1,391 (1,564)
14	-311 (1,085)	32	2,581*** (843)	14	1,654 (1,240)	32	1,375 (1,365)
15	912 (760)	33	2,484*** (815)	15	2,554** (1,228)	33	1,841 (1,340)
16	1,005 (758)	34	2,357*** (797)	16	3,026*** (1,180)	34	986 (1,315)
17	557 (791)	35	2,554*** (791)	17	2,048 (1,298)	35	742 (1,313)
18	767 (776)	36	1,931*** (826)	18	1,352 (1,387)	36	272 (1,242)
R ²	0.035			0.044			
N	1,289			460			

Significance levels: * : 10% ** : 5% *** : 1%