Barred from employment? A study of labor market prospects before and after imprisonment

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3 The effect of labor market absence on finding employment: A comparison between ex-prisoners and unemployed future prisoners■

Abstract

A period of labor market absence reduces one's chances of getting a job. The labor market position of both imprisoned and unemployed individuals tends to worsen after their time out from the labor market. This study considers whether imprisonment has "scarring" effects on job acquisition over and above unemployment. Using a unique quasi-experimental design with a high-risk sample, we conduct event history analyses in order to estimate the time to employment for a group of ex-prisoners (n = 1,159) and a group of unemployed future prisoners (n = 271). The results show that ex-prisoners find employment more quickly and more often than unemployed future prisoners. Although future research is warranted, these findings align theoretical notions in which a prison spell can lead to skill accumulation and deter offenders from criminal involvement.

Keywords: imprisonment, quasi-experimental design, time to employment, unemployment.

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3.1 Introduction

Labor market reintegration for ex-prisoners is a social and public policy challenge (Bushway et al., 2007a). Previous studies have shown that ex-prisoners' employment chances are considerably diminished because of their imprisonment (Apel & Sweeten, 2010; Buikhuisen & Dijksterhuis, 1971; Kling, 2006; Pager, 2003; Waldfogel, 1994; Western & Pettit, 2000), as is their earnings potential (Holzer, 2007; Waldfogel, 1994; Western, 2002). The importance of offender reintegration stems from the fact that finding and holding down a job is an important feature of the reentry process (Farrington et al., 1986; Sampson & Laub, 1993; Staff & Uggen, 2003; Visher & Travis, 2003).

Unfortunately, it is problematic to ascertain the causal effect of imprisonment on employment. First, it is unclear to what extent the relatively worsened labor market prospects of ex-prisoners are an artefact of their proneness to experience labor market difficulty even in the absence of prison. Prisoners tend to be drawn from marginalized segments of the population with diminished prospects in the labor market. Yet existing research is plagued by the use of comparison samples that are not truly at risk of imprisonment, giving rise to a pernicious selection problem that empirical analysts must confront as rigorously as possible. Second, to the extent that there is indeed a causal effect of imprisonment, the mechanisms that underlie the effect are poorly understood. For instance, previous studies have not clarified whether it is imprisonment per se or labor market absence that accounts for ex-prisoners' worsened labor market prospects.

Descriptive studies that compare the labor market outcomes of prisoners before and after imprisonment are unable to resolve these questions. Although these studies can measure change in employment chances, it remains unclear whether this change can be attributed to the prison spell. Other studies compare the employment chances of ex-prisoners with those of other disadvantaged groups (Graffam et al., 2008; Holzer, 1996; Holzer et al., 2004) or with samples of non-imprisoned subjects (Bushway, 1998; Freeman, 1992; Waldfogel, 1994; Western, 2002). Comparability between ex-prisoners and these other groups is often in doubt, however. Ex-prisoners as a group possess, arguably more than other disadvantaged groups, characteristics that severely limit their employment chances. When the comparability of groups is not warranted, we can expect to find a negative effect of imprisonment whereas in fact this effect is owing to other differences between the groups (such as criminal propensity). Some recent studies in which the comparability of groups was better warranted (comparisons of groups with different confinement lengths) did not find a negative effect but instead found that imprisonment can increase employment chances in the short term (Kling, 2006; LaLonde & Cho, 2008; Pettit & Lyons, 2007; Sabol, 2007). However, these studies are unable to clarify whether this effect is the result of imprisonment (the prison experience) or of labor market absence (a time out).

In this study we have advanced previous research in several ways. First, we employed a quasi-experimental design with comparable groups. Both groups will experience a prison spell either at the beginning or the end of our two-year observation window. Both groups are also shown to have relatively poor work prospects. The research design is unique for estimating the effect of imprisonment and is inspired by Grogger's (1995) study of the impact of arrest on wages. We estimated the time to employment for a group of 1,159 persons who entered prison in the first half of 2005 and a group of 271 future prisoners who have a comparable criminal history, were unemployed for some time during the first half of 2005 and entered prison in the second half of 2006. Second, we have gained insight into the effect of two kinds of labor market absence by contrasting the employment chances of a group of ex-prisoners with a group of unemployed future prisoners. Third, we used a large-scale dataset in which information from several administrative sources has been linked. We learned the timing of their prison spell in 2005/6 from data maintained by the Judicial Institutions Department in the Netherlands. For the years 2004–2006, we obtained monthly information on the offenders' socioeconomic circumstances. In addition, information on background characteristics was available. These data come from the Social Statistics Files of Statistics Netherlands.[1] Fourth, as this field of research is dominated by U.S. studies, we contribute to the literature by bringing data on male prisoners from the Netherlands.

All in all, the research design and data offer a unique opportunity for contrasting two types of labor market absence on employment: To what extent do two kinds of labor market absence – imprisonment and unemployment – affect the time to employment?

3.2 Theoretical perspectives

The expectation that a period of labor market absence worsens one's economic prospects is common to labor economists and sociologists alike (Ackum, 1991; Gregg & Tominey, 2005). However, labor market chances can differ between groups of non-participants. In this study we concentrated on a group of ex-prisoners and a group of unemployed future prisoners to investigate whether two types of labor market absence have a different effect on employment chances. We used insights from deterrence theory, human capital theory and signaling theory.

1 We thank Statistics Netherlands, especially Heike Goudriaan, and the Judicial Information Service, especially Paul Linckens, for providing data from the Social Statistics Files and from the penitentiaries (Tenuitvoerlegging vrijheidsbenemende straffen en maatregelen in penitentiaire inrichtingen, TULP). We refer to Goudriaan and Beijersbergen (2010) for information about the combining of these files.

3.2.1 *Deterrence theory*

According to deterrence theory, punishment reduces criminal behavior. Both the threat of punishment, known as general deterrence, and the personal experience of punishment, known as specific deterrence, are expected to discourage potential and actual criminals (Beccaria, [1764] 1995). The latter kind of deterrence is of interest to this study. Punishment is expected to deter criminals from future criminal behavior through an enhanced perception of the risk of getting caught and the severity of punishment. Imprisonment is the most severe punishment in the Netherlands and is therefore expected to decrease criminal behavior and is, simultaneously, expected to increase lawful behavior. Employment is a main component of lawful behavior because it enables individuals to support themselves financially in a legal manner. It should be noted that, when the punishment experience is not as severe as expected, it might have the opposite effect and increase criminal activity.

3.2.2 *Human capital theory*

Employers will recruit the best person for the job. According to theories of human capital, they base this decision on applicants' general and specific forms of human capital (Becker, 1964). General human capital is useful to all employers (e.g., educational attainment), whereas specific human capital refers to work experience that is useful only to a single employer or industry (such as on-the-job training). Unemployment restricts the accumulation of human capital and can even lead to the erosion of skills as they go unutilized. A prison spell can have a variety of effects on the educational and occupational skills of offenders. The forced time out of the labor market can translate into an erosion of skills as well. In addition, a prison sentence disrupts work and educational training. Ties to legitimate employers are likely to be severed by a prison spell. Also, prisoners can learn new criminal skills through their interaction with other prisoners (McCarthy, Hagan, & Martin, 2002). The accumulation of such 'criminal capital' can have a negative effect on a prisoner's legal labor market position and aspirations after release from prison.

On the other hand, more so than "regular" unemployed individuals, prisoners may learn new skills that benefit them in the labor market. First, some acquire training in job skills, complete educational courses while in prison, or take part in reentry programs after release. Second, prisoners might increase their human capital through mandatory prison labor. This participation can lead to new job skills and social skills that can come in handy after their release.

3.2.3 *Signaling theory*

According to signaling theory, employers differentiate between signals (such as educational training and work experience) and indices (e.g., sex, race)

(Spence, 1973). Employers use applicants' information by translating it into positive and negative signals. Work experience can be seen as a signal of general work competence. It indicates that an individual has certain characteristics, such as discipline, work motivation and social skills, that are relevant for job performance. Even a short period of unemployment can be interpreted as a negative signal. Hence, employers might associate a criminal conviction or prison record with inferior personal characteristics and a generally low work competency. Indeed, previous research has shown that a history of imprisonment can lead to rejection in the hiring process (Pager, 2003).

Both unemployment and imprisonment are assumed to evoke negative signals in the labor market, and consequently to decrease employment chances. However, it is plausible that imprisonment evokes a more influential negative signal. In line with this, Holzer (1996) and Holzer et al. (2004) showed that employers prefer hiring welfare recipients or applicants with little work experience over ex-prisoners.

3.2.4 Expectations

The foregoing theories are ambiguous with respect to the employment chances of ex-prisoners compared with unemployed future prisoners. On the one hand, deterrence and guidance during and after imprisonment are expected to increase the employment chances of ex-prisoners. On the other hand, the accumulation of criminal capital and negative signaling will lead to better employment chances within the unemployed group. It is outside the scope of this study to test the mechanisms that underlie the effect of labor market absence on employment chances. However, in investigating whether imprisonment affects employment chances to a different extent than "regular" unemployment, these theoretical explanations provide a broader context for the interpretation of findings.

3.3 Previous research

A number of summary observations can be made from the body of research on the effect of imprisonment on labor market position. First, almost all studies are conducted in the United States, which constitutes a unique social context for prisoner reentry that might not generalize to other nationalities. For instance, U.S. studies include samples of offenders who have served comparatively long sentences in state or federal prisons (a notable exception is Apel & Sweeten, 2010). In the Netherlands, the prison rate increased fourfold between 1977 and 2004 (Tonry & Bijleveld, 2007). Nevertheless, the criminal justice system in the Netherlands is far less punitive than that in the United States. In 2010, the prison rate was 94 per 100,000 in the Netherlands against 760 in the United States (International Centre for Prison Studies, 2010). Moreover, prison sentences are shorter and life circumstances in prison are better in the Netherlands. For instance, most prisoners have a private

cell. A case study on a different prison population and labor market will give more insight into the generalizability of U.S. findings. Second, studies do not uniformly find support for a corrosive effect of imprisonment on employment and earnings that withstands control for a variety of sources of confounding (Apel & Sweeten, 2010; Kling, 2006; Monk-Turner, 1989; Raphael, 2007). Third, an unexpected finding in administrative studies is that employment or earnings often improve in the short term, for the first several quarters following release from confinement (Kling, 2006; LaLonde & Cho, 2008; Pettit & Lyons, 2007; Sabol, 2007). Despite these qualifications, however, most studies do indeed find that imprisonment has a corrosive impact on an offender's employment prospects by reducing the probability of employment, increasing the duration of unemployment, eroding wages and earnings, and exacerbating turnover.

Research findings can be linked to the three mentioned theories in various ways. As regards deterrence theory, most empirical studies do not find that imprisonment reduces criminal behavior (Nagin et al., 2009). Likewise this indicates that imprisonment does not promote conventional behavior such as legal employment. On the other hand, the increased employment chances after release from prison, as found by some U.S. studies, may be the result of short-run deterrence.

With respect to the human capital hypothesis, imprisonment undeniably imposes a period of "time out" from the labor market, representing a permanent loss of work experience for the duration of the prison sentence. One possible indication of the corrosive effects of imprisonment on human capital is that offenders with longer prison sentences tend to have worse employment prospects than offenders with comparatively shorter sentences, other things equal (Sampson & Laub, 1993).

Previous studies also offer support for the contrasting hypothesis that imprisonment leads to an accumulation of skills. Reentry programs for ex-prisoners can have a positive effect on employment after release. However, the evaluation literature reports that most programs produce minimal effects (Bushway & Reuter, 2004). In order to increase the chances of successful entry into the labor market after release, employment-based programs should not only help with finding a job, but should also contain a component of training that improves the "employability" of ex-prisoners (see Apel, 2011).

The possibility that imprisonment imposes reputational losses on ex-prisoners, consistent with the signaling hypothesis, can be discerned from a study by Pager (2003). She conducted a study of matched audit pairs in Milwaukee (U.S.) and found that employers advertising entry-level job openings, were less than half as likely to call back applicants who reported a prison record. Interviews with employers have also consistently documented a reluctance to hire employees with a criminal record (Holzer, 1996; Holzer et al., 2004).

Ex-prisoners thus appear to experience discrimination during the hiring process. They also face a variety of statutory restrictions that categorically prohibit certain types of employment. In the Netherlands, every employer

may ask applicants for a certificate of conduct. In certain sectors this certificate is mandatory (education, the health service, cab driving, security and transportation). It is granted by the Secretary of Security and Justice if a criminal history is not related to the future work activities. In recent years the certificate has become mandatory in more sectors than before. The rules for granting a certificate have become stricter as well (Boone, 2011). However, in contrast to the United States, Dutch employers have few other possibilities to retrieve information about the criminal history of applicants. Furthermore, the Netherlands represents a welfare state in which every person is entitled to health care and benefits. As such, unemployed Dutch citizens might be less inclined than unemployed Americans to seek employment.

3.4 Data

3.4.1 Selection of research group

We compared the time to employment for ex-prisoners and unemployed future prisoners. The prisoners enter prison at the beginning of the reference window and the unemployed enter prison at the end of this period. The groups were selected from the population of male prisoners who entered Dutch penitentiaries in the years 2005 or 2006 as registered in the TULP.[2] These data are linked to the socioeconomic database of Statistics Netherlands. Only those who were registered in the municipal administration during the years 2004–2006 were selected (n = 39,739).[3] For these subjects, we know the socioeconomic category for almost every month. After this selection we divided the individuals into a focal group of ex-prisoners and a comparison group of unemployed future prisoners. Data from calendar year 2004 function to establish any differences between the groups in prior work experience and background characteristics. The first half of 2005 determined into which group a subject is classified. Subjects who entered prison during this period, and were released before September 2005, belong to the focal group. Subjects who were already unemployed or became unemployed in this period, which is indicated by the receipt of benefits for unemployment or welfare, and who entered prison in the second half of 2006 (and were released before March 2007) belong to the comparison group.

As a result of these selections we followed only those with a maximum confinement length of 8 months. For the focal group, the follow-up period starts the month after release (earliest month: February 2005, latest month: December 2006; maximum of 23 months). For the comparison group, the follow-up period starts the month after the first month of unemployment

2 TULP is the prison registration system. For more information on the TULP data, see Linckens and De Looff (2011).

3 As a result, illegal inhabitants are excluded from this study.

until (at latest) three months before the beginning of their prison spell (earliest start: March 2004, latest month possible: September 2006; maximum of 31 months). This last condition is based on the finding that the employment position is relatively worse in periods shortly before imprisonment.

In order to be selected into the focal or the comparison group, some additional conditions were applied. First, the length of confinement had to be at least 15 days. A shorter prison spell does not necessarily affect a labor market position, because such an absence does not need to be justified in the hiring process and because employees can dismiss it as having taken a holiday or being ill. Second, only those in the focal group who were indeed listed as unemployed for a minimum of one month during their prison sentence were selected. The reason for this selection is that we focus our analysis on *finding* employment. Prisoners who were able to keep their job during their prison spell did not have to look for employment after release. Third, we selected only those individuals who are in the *risk pool* for employment by selecting individuals who were employed for a minimum of one month in 2004. After having made these selections, the focal group consisted of 1,159 persons and the comparison group consisted of 271 persons.

3.4.2 Employment

The dependent variable in this research is time to employment and covers the period January 2005 – December 2006. This measure is based on the socioeconomic category in the Social Statistics Database. It is a monthly variable consisting of the following categories: entrepreneur, employee, welfare benefits, disability benefits, unemployment benefits, other benefits, student and pension. A person is assigned to a particular category based on a comparison of the sources that contribute to monthly income. The highest source of income determines the category. Those categorized as an entrepreneur or employee are considered to be employed; all other categories are considered to be unemployed. For about 11 percent of the subjects, data on socioeconomic category are missing for one or more months. We considered these months to be unemployed months. Because the Social Statistics Database data are very wide-ranging in using multiple sources, any income from (legal) employment would have been registered.

3.4.3 Prior work experience

Socioeconomic data from calendar year 2004 serve as a measure of prior work experience, which comprises two variables: the number of employed months and the number of unemployed months. These variables consist of the number of months a person is registered as an employee or entrepreneur, and the number of months a person is registered as unemployed. Here, the number of unemployed months is based on the number of months

a person received benefits (unemployment or welfare).[4] The values on this variable range from 1 to 12 because we selected only those subjects who worked for at least one month in 2004. The number of unemployed months varies from 0 to 11.

3.4.4 Instant offense characteristics

We also had information on the type of crime for which a person was imprisoned in 2005 or 2006, as well as the length of this confinement. Length of confinement represents the actual number of days a person has spent in prison. We distinguished between eight types of crime: violent crimes, property crimes, public order crimes, penal crimes, traffic crimes, drug crimes, other crimes, and unknown crimes.

3.4.5 Criminal history

We controlled for a number of factors known to be related to both imprisonment and employment. We controlled for the number of prior imprisonments in the period 1996–2004. This means that we do not know the total number of prior imprisonments for all subjects. However, for many men this period covers their whole adult life (the average age is 32). Only for 1.5 percent of the sample we did not have any information on the number of previous imprisonments. We added the category "missing" in order to be able to include these individuals in the analyses. In addition, we controlled for the fact that a person was suspected of a crime in 2004 ([Herkenningsdienstsysteem] HKS). If a person is suspected of a crime in a particular year, the HKS documents how many times before he has been a suspect. This means that we have a valid score only for those who were suspected of a crime in 2004 (49.8 percent). Again, a missing data category was created for those without a valid score.

3.4.6 Background characteristics

The Social Statistics Database consists of information on several background characteristics, for instance, date of birth, country of birth, religious denomination, marital status and the number of children under 17 years of age living in the household. Because there might be more employment opportunities in bigger cities than in smaller towns, we included an ordinal six-category measure of urbanization in the analyses. These characteristics are included as static control variables (measured in 2004).

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4 The number of unemployed months does not equal 12 minus the number of employed months in 2004 because there are many more socioeconomic categories (see the construction of 'employment').

3.4.7 *Comparability of research groups*

Our quasi-experimental research design is more suitable than many previous studies for identifying the effect of imprisonment on finding employment. Nevertheless, the comparability of the research groups is not flawless. Below, we will pay more attention to this by discussing the descriptive information in Table 3.1. Some of the characteristics have a skewed distribution or are not interval variables. In these cases, instead of a Chi-square test, we used a Mann-Whitney test to estimate if differences between groups were significant (the missing categories were excluded in these tests).

Table 3.1 Descriptive information on focal group of ex-prisoners (n = 1,159) and comparison group of unemployed future prisoners (n = 271)

	Ex-prisoners		Unemployed future prisoners		
	Mean / %	SD	Mean / %	SD	Sign.
Index offense					
Length of imprisonment (days)	70.4	48.7	62.4	38.0	
Type of crime					***
Violent crime	28.2		39.5		
Property crime	24.8		17.3		
Public order	7.2		5.5		
Penal crimes	7.2		5.9		
Traffic	4.8		2.6		
Opium act	8.4		15.9		
Other	2.9		2.6		
Unknown	7.7		9.6		
Missing	8.8		1.1		
Background characteristics					
Employed months (2004)	6.1	4.0	6.0	3.9	
Unemployed months (2004)	1.9	3.2	3.4	3.6	***
Age	30.5	9.4	32.6	9.6	
Suspect					***
1 time	2.8		4.4		
2-3 times	6.2		9.6		
4-10 times	22.0		14.8		
>10 times	20.8		12.2		
Missing	48.1		59.0		
Prior imprisonment					***
None	51.0		64.6		
1 time	21.7		17.7		
2-3 times	16.0		8.9		
4-10 times	9.0		7.0		
>10 times	0.9		-		
Missing	1.4		1.8		

Table 3.1 continued

	Ex-prisoners		Unemployed future prisoners		
	Mean / %	SD	Mean / %	SD	Sign.
Country of birth					
Netherlands	43.7		48.7		
Morocco	15.5		10.3		
Turkey	9.1		8.9		
Surinam	9.0		7.4		
Antilles and Aruba	6.9		8.1		
Other non-western countries	7.7		9.6		
Other western countries	8.1		7.0		
Marital status					*
Single	67.5		61.3		
Partner	11.7		13.7		
Married	7.9		13.3		
Else	10.1		9.2		
Missing	2.8		2.6		
Denomination					
None	34.5		43.2		
Protestant	7.6		10.3		
Catholic	13.2		14.0		
Muslim	22.9		21.8		
Else	2.3		2.2		
Missing	19.5		8.5		
Urbanization					
None	4.6		4.8		
Some	11.4		12.9		
Average	16.9		18.5		
Strong	33.7		28.4		
Very strong	33.2		35.4		
Missing	0.2		-		
Children <17					*
No children<17	50.0		57.6		
Children<17	47.3		39.9		
Missing	2.8		2.6		

***p=<0,001; **p=<0,01; *p=<0,05

Table 3.1 shows the characteristics of both groups. The duration of the "current" prison spell, mean age, number of employed months in 2004, country of birth, denomination and urbanization in the city of residence did not differ between the groups. Owing to the research design, the criminal history for both groups is fairly comparable. Nonetheless, we found some significant differences. The focal group of ex-prisoners has been suspected of a crime more often and has also been to prison more often than the compari-

son group of unemployed future prisoners. In addition, although a violent crime is the most common crime in both groups, this percentage is significantly higher among the unemployed future prisoners. Other differences are that ex-prisoners were more often single and more often lived with children under 17 years of age. Finally, men in the comparison group had been unemployed for more months in 2004 than the focal group. We controled for these group differences in the analyses.

The fact that the comparison group will become imprisoned in the future may lead to a difference in job search and employment chances between the groups. Presumably, future prisoners will be less motivated to find employment if they know that they will go to prison in the near future. For many future prisoners this will not play a role because their prison spell began in custody awaiting trial (53.5 percent). Such prison spells follow directly from an arrest and are therefore unexpected. Another reason the search for employment might be different for future prisoners can be that, instead of searching for a job, they are engaged in the criminal activities that will lead to their imprisonment. We reduced this possible bias by excluding the three months prior to the prison spell from the follow-up period of future prisoners. Moreover, sensitivity analyses in which a different comparison group was used, led to similar findings.[5]

3.5 Methods

We performed the Kaplan–Meier technique to study the time to employment for both groups. An advantage of this technique is that it accounts for unbalanced data, differences in observation length between subjects, and subjects

5 The fact that the comparison group will become imprisoned in the future may lead to a difference in job search and employment chances between the comparison and the focal group. In order to get an insight into whether this incomparability might have biased the results, we constructed an additional comparison group of unemployed short-term prisoners. They have a comparable criminal history. The added value of including this additional comparison group is that a shorter prison spell is expected to have a smaller effect on both the search for a job and the chances of finding a job (before and after imprisonment). First, we can study whether those with a shorter prison sentence will be less affected by their sentence in finding employment than future prisoners, who will probably be more engaged in criminal behavior in the run-up to their prison spell. Second, we can study whether this comparison group is less affected by their prison spell in finding employment than the focal group of prisoners with longer sentences. The maximum confinement length of the comparison group of unemployed short-term prisoners is seven successive days in 2005–2006. They become unemployed in the first six months of 2005 and only those individuals who were employed for at least one month in 2004 were selected. The comparison group of unemployed short-time prisoners consists of 228 individuals. When we included this additional comparison group in the analysis we found that this group takes a position between the unemployed future prisoners and the ex-prisoners. They seem to find a job sooner than the unemployed future prisoners but less quickly than the ex-prisoners. This result indicates that ex-prisoners make the transition to the labor market more rapidly than comparable groups of individuals who experience a 'regular' spell of unemployment.

who do not find employment during the research window (censored cases). The Kaplan–Meier analysis compares survival curves of groups over a period of time. Although we can use this technique to compare the survival curves of ex-prisoners and unemployed future prisoners, it does not allow the inclusion of other covariates.

We therefore performed a logistic regression in order to control for other effects. According to Allison (1982), logistic regression is an appropriate technique for studying the effects of multiple variables on the occurrence of an event when data of discrete time (months) is used. Instead of estimating the time to employment, the logistic model estimates the chance of finding employment within the follow-up period. This model is based on a person-period data file in which each person is represented by multiple lines depending on the number of follow-up months. We controlled for time by including month dummies in the model.[6] Because this technique treats multiple time units for each individual as though they were independent, standard errors might be somewhat deflated, which as a result may lead more easily to significant results. However, we do not expect that another technique would lead to a different conclusion with respect to the group difference in employment chances (see also Allison, 1982). To illustrate, a Cox regression model on time to employment led to similar results. Because the proportional hazard assumption of the Cox regression was violated with respect to group membership (the variable of interest), we chose to present the results of the logistic regression model.

As mentioned above, the discrete time periods in this study were months. For ex-prisoners, the period of observation starts in the month after release from prison and ends not later than December 2006. The follow-up period of the unemployed future prisoners starts one month after the month they became unemployed and ends not later than three months before their prison spell. Naturally, the observation period also ends when employment is found.

3.6 Results

3.6.1 Kaplan-Meier technique

The survival curves in Figure 3.1 show that ex-prisoners find employment sooner than unemployed future prisoners. The survival rate for a specific month represents the probability that the event (finding employment) has not occurred by that time. Consequently, the figure shows for both groups the probability per month that an average group member will stay unemployed

6 The maximum follow-up period is around 24–31 months for some unemployed future prisoners (those who were already unemployed in March 2004 or later that year). Because of low cell frequencies, inclusion of these months in the binary regression led to inflated standard errors. For that reason the logistic regression uses a maximum follow-up period of 23 months.

after that month. A log-rank test showed that the two survival curves of employment are significantly different from each other ($\chi^2 = 21.930$, df = 1, $p < .001$).

Immediately after the period of labor market absence, we observe a pronounced difference between groups. A substantial percentage of the ex-prisoners (20 percent) finds employment right after release from prison. Based on the estimated means of the Kaplan-Meier analysis, the average ex-prisoner finds employment after 12 months, whereas the average unemployed future prisoner finds employment after 18.3 months. For unemployed future prisoners it takes more time to return to the labor market.

In addition to showing that ex-prisoners find employment sooner, Figure 3.1 also shows that they have an overall higher chance of finding employment than unemployed future prisoners. A simple comparison of employment ratios showed that approximately 80 percent of the ex-prisoners and 55 percent of the unemployed future prisoners find employment (for at least one month) within the follow-up period. Below, we discuss whether this difference in overall employment rate remains after controlling for other effects.

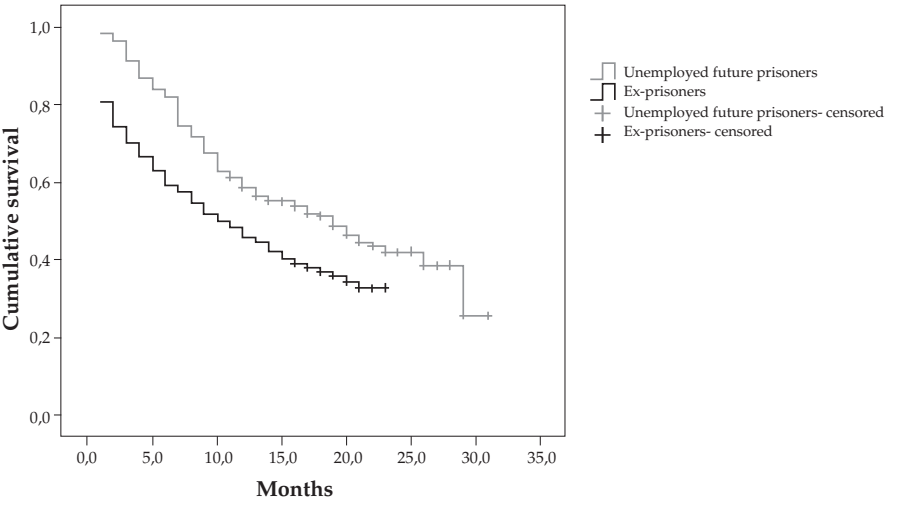

Figure 3.1 Kaplan Meier survival functions of time to employment after labor market absence

3.6.2 Logistic regression

Table 3.2 shows the unstandardized regression coefficients and odds ratios of the logistic regression analyses on finding employment. Model 1 includes only a dummy for group membership; ex-prisoners have a higher chance of finding a job than unemployed prisoners (OR = 1.617). More precisely, the chance of finding employment versus the chance of not finding employment is 1.617 times higher for ex-prisoners than for unemployed future prisoners.

Table 3.2 Logistic regression model on finding employment (Ex-prisoners: n persons = 1,159, n lines = 12,290; Unemployed future prisoners: n persons = 271, n lines = 3,649)

	Model 1				Model 2			
	B	SE	Sign.	OR	B	SE	Sign.	OR
Constant	-3.221	0.09	***	.040	-1.742	0.295	***	0.175
Ex-prisoners	0.481	0.09	***	1.617	0.451	0.100	***	1.570
Employed months (2004)					0.055	0.011	***	1.056
Unemployed months (2004)					0.004	0.014		1.004
Age (18=0)					-0.028	0.005	***	0.972
Suspect								
1 time *(ref.)*								
2-3 times					-0.155	0.237		0.856
4-10 times					-0.188	0.210		0.828
>10 times					-0.638	0.224	**	0.528
Missing					-0.247	0.201		0.781
Prior imprisonment								
None *(ref.)*								
1 time					0.116	0.093		1.123
2-3 times					0.062	0.116		1.064
4-10 times					0.203	0.149		1.225
>10 times					0.507	0.411		1.660
Missing					0.210	0.290		1.234
Country of birth								
Netherlands *(ref.)*								
Morocco					0.010	0.165		1.010
Turkey					-0.098	0.179		0.906
Surinam					-0.162	0.148		0.851
Antilles and Aruba					0.114	0.149		1.121
Other non-western countries					-0.328	0.153	*	0.720
Other western countries					0.099	0.135		1.104
Marital status								
Single *(ref.)*								
Partner					0.037	0.114		1.037
Married					-0.092	0.144		0.912
Else					-0.105	0.125		0.900
Missing					0.512	0.213	*	1.668
Denomination								
None *(ref.)*								
Protestant					0.045	0.136		1.046
Catholic					0.080	0.117		1.083
Muslim					-0.213	0.157		0.808
Else					-0.129	0.260		0.879
Missing					0.007	0.107		1.007

Table 3.2 continued

	Model 1				Model 2			
	B	SE	Sign.	OR	B	SE	Sign.	OR
Urbanization								
None *(ref.)*								
Some					0.192	0.191		1.211
Average					0.059	0.186		1.061
Strong					-0.091	0.178		0.913
Very strong					-0.192	0.184		0.825
Missing					-0.768	1.082		0.464
Children <17								
No children<17 *(ref.)*								
Children<17					0.034	0.079		1.034
Months								
Month 1 *(ref.)*								
Month 2					-0.948	0.138	***	0.388
Month 3					-1.152	0.151	***	0.316
Month 4					-1.189	0.157	***	0.304
Month 5					-1.263	0.166	***	0.283
Month 6					-1.218	0.167	***	0.296
Month 7					-1.319	0.178	***	0.267
Month 8					-1.186	0.174	***	0.306
Month 9					-1.151	0.175	***	0.316
Month 10					-1.211	0.185	***	0.298
Month 11					-1.699	0.229	***	0.183
Month 12					-1.171	0.189	***	0.310
Month 13					-1.543	0.225	***	0.214
Month 14					-1.324	0.211	***	0.266
Month 15					-1.480	0.230	***	0.228
Month 16					-1.634	0.251	***	0.195
Month 17					-1.560	0.258	***	0.210
Month 18					-1.878	0.329	***	0.153
Month 19					-1.559	0.331	***	0.210
Month 20					-1.107	0.320	***	0.331
Month 21					-1.277	0.426	**	0.279
Month 22					-2.434	1.011	*	0.088
Month 23					-1.302	1.024		0.272
Nagelkerke R^2	0.005				0.078			

***p=<0,001; **p=<0,01; *p=<0,05

Logistic regression is not suitable for comparing estimated parameters and explained variances between models (Mood, 2010). We therefore cannot be certain about the extent to which the group difference in employment chanc-

es changes when we control for other variables in Model 2. This model can, however, tell us that the group difference still exists after controlling for covariates and month dummies (OR = 1.570). Model 2 shows as well that most of the individual characteristics do not influence the chance of finding employment. However, those with more work experience (OR = 1.056) and those who have a missing value on marital status (OR = 1.668) have a higher chance of employment. Older individuals (OR = 0.972^(age-18)) and those who were born in another non-Western country (OR = 0.720) as well as those who were suspected of a crime more than ten times (OR = 0.528) find employment relatively less often within the follow-up period. The month dummies show that the chance of finding employment is highest in the first month after labor market absence. In addition, these dummies seem to indicate that the chance of finding employment decreases in subsequent months.

In sum, even after controlling for other effects, ex-prisoners find employment more often than unemployed future prisoners. More so than regular unemployment, imprisonment seems to encourage the transition to the labor market. Several sensitivity analyses showed that this conclusion is robust.[7]

3.7 Conclusion

The goal of this study was to ascertain the effect of imprisonment versus unemployment on finding employment. We used a quasi-experimental design in which ex-prisoners were compared with unemployed future prisoners. This design improved the ability to make causal statements in comparison with previous studies in which ex-prisoners were compared with non-prisoners. The negative effect of imprisonment reported in these studies was at least partly a result of the fact that ex-prisoners have characteristics that led to a relatively worse labor market position even prior to their imprisonment. These studies are therefore likely to overestimate the negative effect of imprisonment. The quasi-experimental design used in this study reduces the influence of such unobserved differences significantly (see also Grogger, 1995). Our results were based on administrative information on more than 1,100 ex-prisoners and almost 300 unemployed future prisoners. We used monthly data on employment and imprisonment during a period of two years. In addition, we had information on various control variables that are often lacking in studies that use administrative data. As

7 Although we do not have complete information on criminal history (see data section) we repeated the analyses on – according to our available data – first-time prisoners. These analyses showed similar results. Sensitivity analyses showed that ex-prisoners who have committed a violent crime have more difficulties finding employment than ex-prisoners who committed a property offense or other type of crime. All three groups have a significantly higher chance of finding employment than unemployed future prisoners. See also note 22 for additional sensitivity analyses.

such, this study adds to a body of research that is dominated by U.S. scholars and has not been carried out in the Netherlands (Dirkzwager et al., 2009).

We found that ex-prisoners find a job more often and more quickly than unemployed future prisoners. After accounting for possible differences between the groups, the positive effect of imprisonment holds. Although this finding might seem unexpected and counterintuitive, it is in line with some recent U.S. studies. Studies based on administrative data find that employment chances are higher in the period shortly after release (Kling, 2006; LaLonde & Cho, 2008; Pettit & Lyons, 2007; Sabol, 2007). Moreover, one should keep in mind that we compared ex-prisoners with unemployed future prisoners. We could not find previous studies that compared these two groups. It should also be noted that, unlike previous effect studies, we included only those individuals who were employed for at least one month in the year prior to their imprisonment or unemployment. This means that, within a group of individuals with relatively poor labor market prospects, we selected those who were actually in the *risk pool* for employment. Nevertheless, the high percentage of employed ex-prisoners seems remarkable. It is possible that finding employment is more necessary for ex-prisoners than for unemployed future prisoners because the latter group is more likely to receive benefits. During imprisonment, all benefits are stopped (law on prisoners' social security rights) and it may take some time for benefits to become available after release.

The findings are in line with an hypothesis following from human capital theory. Prisoners can learn skills during their prison spell and by participating in reentry programs immediately after release. This kind of human capital is not available for the unemployed. Especially in recent years, the Dutch government increased the provision of aftercare. This could explain the relatively advantaged position of ex-prisoners. However, it should be noted that many programs are offered only to the more long-term prisoners, and the aftercare programs were relatively new or impending during our research window. One should therefore be cautious about ascribing the positive effect of imprisonment on employment chances to assistance in prison. Guidance after release from prison, for instance by a probation officer, might offer a more plausible explanation. In the Netherlands, a prison sentence is often combined with a conditional prison sentence and probation supervision. In addition, deterrence theory states that a prison spell can deter prisoners from criminal behavior. Our finding could therefore also reflect a deterrent effect. Another possible explanation is that ex-prisoners might have the opportunity to return to former employers (Visher et al., 2011). Unfortunately, our data do not allow us to study the validity of the proposed mechanisms. We view such explanatory research as an essential avenue for future research.

As a result of the quasi-experimental design, our estimation of the imprisonment effect is more meaningful than in many previous studies. Nevertheless, the design has limitations. First, the comparability of the

research groups is not perfect. The ex-prisoners and unemployed future prisoners are alike with respect to many characteristics, and we control for the variables in which they differ. However, they might also differ in other characteristics that are not easily measured (such as social background or self-control). Only an experimental research design in which individuals are randomly assigned to a prison and a comparison group can ensure that such characteristics do not bias the results. A second limitation is that our maximum follow-up period is only two years. A longer follow-up period would increase our insight into post-release employment patterns and enables us to improve the research design. Using a longer follow-up we would be able to account more rigorously for the fact that the comparison group will become imprisoned in the future (which may lead to a difference in job search and employment opportunities between the comparison and the focal group). A third limitation is that we only have information on registered employment and criminal behavior. Although we found that many ex-prisoners find registered and thus legal employment, previous studies have speculated that (ex-)prisoners often participate in employment off-the-books. Adding such data would give more insight into the labor market position of ex-prisoners. Nonetheless, an advantage of using registered data is that social desirability and memory loss cannot have obscured the findings.

To close, the finding that ex-prisoners find a job more often and sooner than unemployed future prisoners suggests that imprisonment, more so than regular unemployment, seems to encourage the transition to the labor market. The high recidivism rates after imprisonment do not seem to be in line with our finding. Like much research, this study thus raises new questions. Do ex-prisoners commit crimes while they are employed (perhaps at the workplace)? Do they return to former employers easily? Are ex-prisoners unable to hold on to a job? By answering these questions, future research will better understand ex-prisoners' pathways to employment and crime.