

Not Just Any Job Will Do: A Study on Employment Characteristics and Recidivism Risks After Release

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Abstract

Ex-prisoners' recidivism risks are high. Several theories state that employment can reduce these risks but emphasize that the protective role of employment is conditional on job qualities (work intensity, job duration, etc.). Longitudinal research on the role of employment in ex-prisoners' recidivism patterns is scarce, and most existing work used a simplistic employment measure (i.e., employed vs. unemployed), leaving the topic of job quality underexplored. This study examines the association between employment characteristics and recidivism among Dutch ex-prisoners. Using longitudinal data of the Prison Project ($n = 714$), we found that not just any job, but particularly stable employment and jobs with a higher occupational level could help reduce crime rates among these high-risk offenders. Many ex-prisoners face a human capital deficit that complicates the guidance to high-quality jobs. It might, however, be possible to help place ex-prisoners in stable employment.

Keywords

reintegration, imprisonment, employment, reoffending, recidivism, longitudinal research.

Introduction

More than ten million individuals worldwide are held in penal institutions (Walmsley, 2016). Although time spent in prison is intended to prevent crime, recidivism risks are

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high among ex-prisoners, especially in the first months following release (Langan & Levin, 2002; Wartna et al., 2011). The dramatic change in circumstances and uncertainty that accompany release are plausible explanations for this high recidivism rate. Many ex-prisoners report problems on multiple life domains, such as housing, health, and income (Dirkzwager, Nieuwbeerta, & Fiselier, 2009; Visher & Travis, 2011). Employment, education, and related areas (financial assistance, driver's license, job training) were among the most requested reentry needs of serious offenders upon release (Visher & Lattimore, 2007).

Transitional service programs provide an opportunity to smooth the transition from prison into the community and improve post-release (employment) outcomes. Transitional employment is subsidized employment made available immediately after a person leaves prison, often accompanied by soft skills training and job search assistance. Research that evaluates these programs aims to help identify which ones are likely to improve successful reentry. For example, in the Transitional Jobs Reentry Demonstration offered by the Center for Employment Opportunities (CEO) in New York, offenders were randomly assigned to a treatment or control group to assess the causal treatment effect. The treatment group was given access to CEO's temporary paid jobs and other services, and the control group was offered basic job search assistance at CEO along with other services in the community. Three years after release, reductions in future criminal activity were found for offenders randomly assigned to the treatment group in receipt of transitional jobs (Redcross, Millenky, Rudd, & Levshin, 2012). Although Redcross and colleagues found reductions in recidivism, other research evaluating similar programs have found little or no effect on either post-release employment or recidivism rates (Bushway & Apel, 2012; Farabee, Zhang, & Wright, 2014; Jacobs, 2012; Visher, Winterfield, & Coggeshall, 2005). According to Raphael (2014), "... the juxtaposed evaluation of relatively similar interventions suggests that with regard to transitional job programs, the jury is still out" (p. 71).

Implementation differences in the organization of the program or in participants' opportunities to interact with other recently released inmates are mentioned as possible explanations for the lack of consistency in treatment-effects (e.g., Raphael, 2014). One plausible explanation that received little attention but could be addressed and potentially adjusted is that the type of job that is offered in reentry programs seems unfit to reduce recidivism (Uggen, 1999; Uggen & Wakefield, 2008). Released offenders are often assigned to minimum-wage transitional (temporary) jobs (e.g., Jacobs, 2012; Uggen, 1999).

Yet theories imply that the protective effect of employment is conditional on working in certain types of employment; not just any job but higher quality employment can deter offenders from committing criminal acts. Theories ascribe this crime-reducing effect to different job characteristics (e.g., stability, work intensity, earnings). For instance, Sampson and Laub (1990) emphasized in the informal social control theory that

... employment per se does not increase social control. It is employment coupled with job stability, job commitment, and ties to work that should increase social control and, all else equal, lead to a reduction in criminal behavior. (p. 611)

The current study uses observational data on ex-prisoners with various labor market experiences to examine whether the type of jobs that ex-prisoners find immediately after release *can* prevent them from committing new crimes.

Research Question: To what extent is employment, and characteristics of this employment, related to ex-prisoners' recidivism risk in the first 6 months following release?

Longitudinal Dutch data from the Prison Project are used to answer this question. Men were interviewed shortly after entering pre-trial detention as well as 6 months after release ($n = 714$).¹ Detailed measures of the employment situation, and a long list of measures on prisoners' characteristics and life domains (e.g., partner, substance abuse, housing), in the period prior, during, and after imprisonment, are taken into account in an attempt to control for other predictors of both employment and recidivism. In addition, controlling for those covariates in traditional and more advanced multivariate analyses (i.e., logistic regression and propensity score weighting) helps address the selection bias that is caused by the non-random selection of ex-prisoners into employment and type of job.

We aim to advance on the existing literature in three ways. First, to our knowledge, few systematic studies examined the type of jobs ex-prisoners find. A detailed insight into the type of jobs ex-prisoners find immediately after release, and which and how these different type of jobs correlate with reoffending is therefore warranted. There is evidence to suggest that ex-prisoners often work in temporary and low-wage jobs (Western, 2006) and in construction or manufacturing industries (Schnepel, 2014). The bulk of labor market reentry research has focused, however, on (the effect of imprisonment on) post-release employment probabilities (e.g., Apel & Sweeten, 2010; Huebner, 2005) and earnings (Apel & Sweeten, 2010; Waldfoegel, 1994; Western, 2002). Studies rarely examine multiple post-release employment aspects simultaneously and mostly use small unrepresentative prisoner samples. As the majority of research findings are based on U.S. data, this study uses data from the Netherlands and focuses on various employment measures: the duration of post-release jobs, whether ex-prisoners worked as employees or became self-employed, whether they regained an old job, hours of employment, and occupational level. This approach allows an exploration of the underlying assumptions of different theoretical notions in which the expected crime-reducing effect of employment is ascribed to different job characteristics (e.g., stability, work intensity, earnings). Hence, this study provides some understanding on the relative importance of employment characteristics by examining how different type of jobs correlate with reoffending.

Second, knowledge on the type of jobs ex-prisoners find in the community could help develop policies in the field of employment assistance. While employment programs in which offenders are randomly assigned to transitional jobs are perfectly suited for estimating the causal effect of employment on recidivism (isolating treatment-effects) and have a high internal validity, the small sample sizes, specific job-types, and laboratorial settings make findings less suitable for generalization to a

real-world setting (low external validity; see Sampson, 2010). Moreover, the well-respected idea that deterrence is conditional on cognitive transformation (readiness for change; Maruna, 2001) does not correspond with the fact that participants in employment programs are randomly *assigned* to mostly low-quality jobs. As noted by Skardhamar and Savolainen (2014), these jobs may instead “. . . be perceived as borderline punitive by the client populations,” whereas jobs of higher quality (e.g., that offer a more long-term perspective) might be more in line with clients’ preferences (p. 268). The current study sheds light on whether “real-life jobs” can be (more) effective in transforming reoffending behavior.

Third, this study provides a unique insight into prisoner labor market reentry in the Netherlands. Dutch society, specifically its welfare state, relies on a high labor market participation. This is also reflected in recent policy initiatives and legislation that aim to stimulate the participation of disadvantaged workers (Parliamentary Documents II 2011/12, 33 161, no. 8) and could imply that Dutch ex-prisoners have relatively better labor market prospects. During the last decade, the Dutch unemployment rate centered around 5%, which is low compared with other European countries (Statistics Netherlands, 2012). The economic recession led to an increase in the unemployment rate in many European Union (EU) member states, and to a relatively high increase in the Netherlands. Still, also during the period of data collection, the Dutch unemployment rate remained relatively low for European standards (7% vs. an average of 12.1%; Eurostat, 2013). In addition, even though Dutch penal policies became harsher in recent decades (Downes & Van Swaaningen, 2007), we study prisoner reentry in a relatively mild penal climate (e.g., short sentences, humanitarian prisons, limited access to criminal records) compared with other Western countries. Together with this penal climate, the low unemployment rate indicates that a study of ex-prisoners’ labor market prospects in the Netherlands offers an interesting alternative to many Western countries and the bulk of reentry research that is based on American data.

Theory and Previous Research

The Effect of Employment on Recidivism

Various theories link both employment and characteristics of employment to criminal behavior. Merton’s (1938) strain theory and Agnew’s (1992) general strain theory interpret criminal behavior as an adaptive solution to frustrations individuals feel when the legal means are insufficient to reach the desired material and immaterial goals. Employment assures individuals an income and certain status and therefore makes crimes (for financial gain) less necessary. Economic theories portray a similar rational way of thinking. Criminal behavior is expected to decline when the potential costs for this behavior, for instance, job loss, are higher than its potential returns (Becker, 1968). Hirschi’s (1969) social control theory assumes that individuals will engage in delinquent behavior in the absence of close relationships with conventional others. Conventional relationships with co-workers or employers socialize individuals to obey the dominant law-abiding norms and values. Routine activity theory emphasizes that if,

and to what extent, individuals commit crimes relies on the opportunities to commit crimes (Cohen & Felson, 1979; Miller, 2013). More specifically, the presence of motivated offenders is not sufficient, criminal behavior is dependent of the availability of suitable targets as well as the absence of guardians (or lack of social control). Employment is then expected to reduce criminal behavior because it limits the opportunity structure for such behavior.

The discussed theories offer different underlying mechanisms and link the effect of employment to specific characteristics of that employment. All these theories assume, however, that *employed ex-prisoners have a lower risk to reoffend than unemployed ex-prisoners*.²

Reviews of longitudinal research on the work–crime relationship suggest that employment is indeed related to a significant reduction in criminal behavior (Lageson & Uggen, 2013; Uggen & Wakefield, 2008). Longitudinal studies are, however, scarce among ex-prisoners and almost solely based on U.S. data. Berg and Huebner (2011) and Piquero, Brame, Mazerolle, and Haapanen (2002) used American administrative data to examine the work–crime relationship. Berg and Huebner (2011) estimated the effect of employment in the first month post-release on recidivism behavior in the 45 subsequent months ($n = 401$). Piquero and colleagues examined the effect of “stake in conformity” (combination measure of employment and marital status) on recidivism risk for a period of 7 years ($n = 524$). Both studies found significantly lower recidivism risks among those who were employed. Notably, Piquero and colleagues concluded that this crime reduction was mostly attributable to the marital status of ex-prisoners. Skardhamer and Telle (2012) based their analyses on a large administrative Norwegian data set ($n = 7,476$) with a follow-up of 3 years and concluded that employment can also generate a crime-reducing effect among Norwegian ex-prisoners.

Two studies used American survey data about ex-prisoners and found less convincing evidence for the protective influence of employment. Visher et al. (2008) concluded that ex-prisoners who reported to be employed in the second month after release were as likely to commit a crime in the first 8 months following release as their unemployed counterparts ($n = 740$). Based on a 3-year period of monthly data, Horney, Osgood, and Marshall (1995) found that working was only weakly related to different types of offending ($n = 658$). The only significant change was found for property crimes; employment can increase the likelihood that ex-prisoners report property crimes. Horney and colleagues stated that their “crude” measure of employment (employed or unemployed in given month) might be responsible for these unexpected findings: “We measured none of the aspects of attachment to a job that Sampson and Laub (1993) considered; our measure did not even distinguish part-time from full-time employment, or temporary from permanent work” (p. 668).

The Effect of Employment Characteristics on Recidivism

The abovementioned theories presume that the protective effect of employment might be conditional on certain characteristics of that employment. Together with the ambiguity in the findings of the discussed research, this warrants a further examination of the

association between employment and reoffending in which attention is paid to the role of employment characteristics. To date, few scholars have studied the relationship between work (quality) and crime among adult ex-prisoners, with the exception of Uggen (1999). As will be discussed further in a later section, Uggen used a sector-dependent job quality measure and concluded that high job quality decreases recidivism risk among former prisoners. Due to the lack of research on type of employment and recidivism, we supplement the discussed body of knowledge on ex-prisoners with longitudinal research on the effect of job characteristics on crime among other high-risk groups and community samples. Specific attention is paid to the job characteristics under investigation in the current study: job duration, returning to pre-prison employer, working as an employee versus self-employed, hours, and occupational level.

Job duration. Based on Hirschi's (1969) social control theory, Sampson and Laub (1993) stated that employment can lead to a reduction in criminal behavior through the accumulation of conventional ties that accompany steady employment. In other words, stable employment is expected to deter offenders from crime. When ex-prisoners find a job immediately after release and retain it during the 6-month follow-up, they will be able to accumulate bonds with their employer and co-workers (conventional others). Based on notions of social control theories, we therefore expect that *ex-prisoners who are able to retain a job during the 6-month follow-up are less likely to reoffend than ex-prisoners who lose this job*. Empirical studies are, however, ambiguous concerning the effect of job stability. Sampson and Laub (1990, 1993) found that job stability (combination of employment situation, stability of most recent job, and work performances) reduced recidivism risks. Most recent studies based their measure of job stability on the duration of employment. Uggen (1999) did not find conclusive evidence for the crime-reducing effect of job duration (see also Wadsworth, 2006). Dutch longitudinal research among a young high-risk male offender population also did not find support for the protective effect of job stability (Van der Geest, 2011). In another study on partly the same data set as Van der Geest (2011; but including women), Verbruggen, Blokland, and Van der Geest (2012) instead found that longer job durations can indeed decrease the likelihood of recidivism.

Returning to pre-prison employer. It can be argued that returning to the pre-prison employer after release—the second measure for job stability in this study—assures that the pre-prison ties to the workplace (and social control) remain, at least partly, intact. We therefore expect that *ex-prisoners who return to their pre-prison employer after release are less likely to reoffend than ex-prisoners who work in a new job*. Several studies implied that previous employers are important sources of employment for ex-prisoners and relevant for a successful reintegration (Martin & Webster, 1971; Ramakers et al., 2015; Soothill, 1974, Visher, Debus-Sherrill, & Yahner, 2011). Yet, none of these studies examined the influence of job return on recidivism risks.

Employee versus self-employed. Routine activity theory emphasizes that daily activities and the amount of free time determine the risk of reoffending (Cohen & Felson, 1979;

Miller, 2013). As self-employed individuals create their own daily schedule, these workers are less restricted in their opportunities to commit crimes than employees. This line of thinking connects to the power-control theory in which the presence of autonomy and absence of control in supervising functions are expected to increase criminal behavior (Hagan, Gillis, & Simpson, 1985). We therefore expect that *ex-prisoners who work as employees are less likely to reoffend than self-employed ex-prisoners*. Longitudinal studies among community samples of youngsters and adolescents have shown that in jobs in which employees experience more autonomy, individuals are more likely to reoffend, even after taking into account various other characteristics of that employment, such as income, learning opportunities, working in the primary versus the secondary sector and receiving benefits (Huiras, Uggen, & McMorris, 2000; Staff & Uggen, 2003).

Full-time employment versus part-time employment. As mentioned, routine activity theory emphasizes that whether, or how many, crimes individuals commit depends on the opportunity structure of their daily activities. Following this theory, we can also derive a hypothesis concerning the effect of work intensity on crime: *ex-prisoners with a full-time job are less likely to reoffend than ex-prisoners who have a part-time job*. Previous research has investigated the effect of hours of work on recidivism among community samples of young and adolescent individuals. Most of these studies suggest that youngsters who work more hours (>20 hr per week) report more recidivism (e.g., Bachman & Schulenberg, 1993). This finding contrasts our expectation, but connects to the idea that the effect of life events, such as employment, can depend on an individual's stage in the life course (Sampson & Laub, 1993). More recent studies argue that these former studies presented a spurious relationship and could not adequately control for the non-random selection of more crime-prone individuals into more intensive jobs. For instance, Apel et al. (2007) found no overall effect of work hours on the criminal behavior of a large sample of youth when controlling for pre-existing differences between workers and non-workers.

Occupational level. Finally, we use economic theories to derive a hypothesis about the effect of occupational level on recidivism. A higher occupational level implies a higher income and presumably makes a job less interchangeable. Following economic theories, the risk of losing such a job could tip the balance in favor of being a law-abiding citizen. According to strain theory, a good job will make it relatively easy to satisfy needs and desires through legitimate means. We therefore expect that *ex-prisoners with a job of a high occupational level are less likely to reoffend than ex-prisoners with a job of a low occupational level*. Uggen (1999) examined the effect of job pay, job tenure, and a job quality measure that was partly based on sector of employment and occupational level on recidivism. Job pay did not affect recidivism outcomes, and job tenure led to a small, though significant, reduction in recidivism risk. His key finding was, however, that a job-shift from, for instance, the food industry to skilled manual labor led to an 11% crime reduction. This led him to conclude that ex-prisoners who worked in a higher quality job were less likely to reoffend.

In sum, our review of the literature indicates that studies among ex-prisoners are scarce and findings on the protective effect of employment have been mixed. The latter could potentially be caused by the fact that studies used a simplistic participation measure and paid little attention to certain aspects of employment that are essential for a reduced recidivism risk according to theoretical notions. Studies that did measure these aspects are rare but seem to support the importance of job quality and stability. Yet, most studies were based on (American) data on young offender populations or community samples and did not examine multiple employment characteristics simultaneously.

Data

The Prison Project

This study uses data of the Prison Project: a longitudinal research project among Dutch prisoners. The general aim of this project is to study the intended and unintended effects of imprisonment on several life domains of prisoners and their families. Data were collected in the beginning of pre-trial detention, during confinement, as well as 6 months after release from prison. The project targeted 3,983 male prisoners who entered a Dutch detention facility between October 2010 and March 2011, were born in the Netherlands, were between 18 and 65 years old, and did not suffer from severe psychological problems.

The first in-prison interview (P1) was held approximately 3 weeks after the beginning of pre-trial detention and consisted of many retrospective questions on the time since leaving full-time education and the situation during the arrest that led to the current confinement. More than 70% of the targeted respondents were approached ($n = 2,841$), and 48% of the total sample agreed to participate in the first wave, resulting in a sample of 1,904 participants. Difference tests showed that this sample was representative for the larger sample of prisoners on a wide range of background characteristics.³ Nonetheless, a comparison of criminal history measures revealed that participants have a somewhat less extensive criminal history than non-participants (on average: 3.5 vs. 4.5 previous spells and 8.8 vs. 10.5 previous convictions; see also Dirkzwager & Nieuwbeerta, 2014).

The first post-release interview (R1) resulted in a sample of 825 ex-prisoners who participated in P1 and who had been released for a minimum of 6 months when they were reinterviewed (up to January 2013). As expected, the particular lifestyle of the sample made it difficult to contact the ex-prisoners who were eligible for participation in the R1. Still, 52% eventually participated in the R1. The detailed background measures collected in the P1 revealed that P1 and R1 participants were similar in many ways.⁴

As we aim to examine the relationship between employment and recidivism, respondents with missing data on employment status in the first month after release ($n = 31$) or recidivism ($n = 86$) were excluded. This resulted in 714 cases for analyses.

Recidivism

Recidivism is based on the General Documentation Files of the Dutch Ministry of Security and Justice, which contain information on all registered crimes and convictions until July 11, 2012. In the current study, the registered recidivism risk is available for R1 participants who were released for a minimum of 6 months at that time.⁵ Note that, instead of counting reconvications, we count whether or not charges were registered after release. Reconvications are likely to present an underestimation of criminal activity because not all charges that have been registered at the Prosecutor's Office will lead to a conviction within the follow-up period.⁶ Based on registered crimes, 33.9% of the prisoners reoffended within the first half year following release (see also Table 1).

Employment and Employment Characteristics

Employed are those individuals who reported to work at least 1 hr in the first month after release (29.3%, see Table 1).⁷ We know whether those employed ex-prisoners worked as an *employee* (68.9%) or were self-employed. In addition, we know whether these employed ex-prisoners were able to retain the same job during the follow-up. The measure *job retention* thus refers to the 6 months following release, whereas the other employment variables pertain to the situation in the immediate month after release. R1 data showed that 46.6% of the ex-prisoners who were employed in that first month were able to retain that job, at least until the 6th month after release.

As is shown in Table 1, we also study the role of job retention for employees exclusively. Moreover, additional job information is examined for this selection of ex-prisoners. The first additional job characteristic refers to work intensity. We distinguish between individuals who worked full-time (>32 hr per week; 70.1%) and part-time. Second, following the Standard for Classification of Occupations (Standaard Beroepenclassificatie [SBC]) of Statistics Netherlands (Westerman, 2010), information about the job title, type of business, and (executive) tasks was used to classify salaried workers into one of five occupational levels: elementary, low, middle, high, or scientific. Individuals who were classified in the two highest occupational levels are seen as workers with a higher occupational level (17.9%). They have higher positions in businesses or work, for instance, as a manager or real estate agent.

Finally, we are able to measure for those who were salary workers at the time of arrest as well as in the first month after release whether they *returned to their pre-prison employer* after release (70.6%).⁸

Control Variables

We control for a range of covariates that pertain to the period prior, during, and after release and are assumed to correlate with both employment and criminal outcomes. Table 2 offers an overview of all 34 covariates.

The data include information about sociodemographic characteristics, social ties, employment situation at the time of arrest (non-participant, unemployed, employed,

Table 1. Recidivism and Employment (Characteristics).

	N ^a	%
Dependent variable		
Registered recidivism	714	33.9
Independent variables		
<i>All</i>	714	
Employed in first month after release	209	29.3
<i>Of those employed in first month after release</i>	209	
Employee (vs. self-employed)	209	68.9
Retained job during 6-month follow-up	208	46.6
<i>Of those who worked as employee in first month after release</i>	144	
Retained job during 6-month follow-up	144	40.3
Full-time job	144	70.1
Higher occupational level	140	17.9
<i>Of those who worked as employee at time of arrest and in first month after release</i>	85	
Returned to pre-prison employer	85	70.6

^aThere are few missing values on the post-prison job characteristics (retained job, $n = 1$; higher occupational level, $n = 4$).

self-employed), general measures of employment history, and lifestyle. In addition, we control for prisoners' motivation to work, based on nine items pertaining to motivation (e.g., "everyone who can work, should work," Cronbach's $\alpha = .67$). Moreover, we include detailed measures on the index offence and the criminal history as registered in the General Documentation Files of the Ministry of Security and Justice.

Two covariates pertain to the period during imprisonment. Imprisonment length refers to the actual time prisoners spent in detention. We also include whether or not the prisoners participated in an educational or vocational training during their imprisonment.

Several post-release circumstances were measured using a life event calendar in which respondents reported on their behavior in each month. We know whether the prisoners had a romantic partner or housing during the first half year following release (for at least 1 month). In addition, this calendar enables us to measure whether or not ex-prisoners reported substance abuse in at least one of the 6 months (i.e., daily drugs use/drink at least five glasses of alcohol daily) and control for the time spent in prison during the follow-up period. We also know whether the prisoners had contact with a probation officer after release, had valid identification or debts, and whether they received benefits.

Method

Bivariate and Multivariate Analyses

This study offers an insight into the relationships between employment and job characteristics on one hand and recidivism on the other hand. Both traditional and more

Table 2. Descriptive Information on Covariates Pertaining to the Period Prior, During, and After Imprisonment.

	N	M	Median	SD	Minimum	Maximum
Covariates prior to imprisonment						
Age	714	30.91	28.50	10.89	18	65
Non-ethnic Dutch	714	0.33			0	1
Higher level of education ^a	714	0.38			0	1
Partner	714	0.46			0	1
Child(ren)	714	0.37			0	1
Employment at time of arrest	713				0	3
Non-participant		0.22				
Unemployed		0.39				
Employed		0.27				
Self-employed		0.12				
Wage at time of arrest (€)	714	1,288.07	0.00	6,185.69	0	100,000.00
Wage at time of arrest (€; workers only)	279	3,296.35	1,600.00	9,565.07	100.00	100,000.00
Duration longest job since leaving full-time education (years)	648	4.31	2.50	5.41	0	45
Duration unemployment since leaving full-time education (years)	709	4.07	1.00	7.03	0	47
Excessive drinking (almost every day > 5 glasses)	711	0.12			0	1
Excessive consumption of drugs (almost every day)	711	0.30			0	1
Homeless	714	0.10			0	1
Motivation to work ^b	658	3.50	3.44	0.52	1.67	4.89
Number of previous convictions	714	7.78	5.00	9.08	0	92
Previous prison sentence	714	0.55			0	1
Age of onset	713	19.35	17.04	7.53	11.74	65.30
Type of crime	692				0	2
Violent		0.43				
Property		0.35				
Other		0.22				

(continued)

Table 2. (continued)

	N	M	Median	SD	Minimum	Maximum
Covariates pertaining to imprisonment						
Length of imprisonment (days)	714	124.94	101.50	87.76	1	511
Followed training/course	714	0.22			0	1
Covariates after imprisonment						
Partner	665	0.26			0	1
Excessive drinking (almost every day > 5 glasses)	672	0.11			0	1
Excessive consumption of drugs (almost every day)	672	0.22			0	1
Homeless	667	0.11			0	1
Months spent in prison	675	0.35	0.00	1.07	0	6
Missing calendar information	714 ^c	0.08			0	1
Contact with probation officer	714	0.61			0	1
Valid identification	710	0.86			0	1
Debts	712	0.60			0	1
Received benefits	714	0.43			0	1

^aHigher educated are those with a higher level of secondary schooling.

^bAverage score on nine items (1 = *completely disagree*, 5 = *completely agree*).

^cIndividuals with a missing value on one or multiple RI-calendar variables (partner, excessive drinking/drugs, homeless, exposure time) score "1" on this variable ($n = 55$). In the multivariate analyses, these individuals score "0" on the calendar variables.

advanced analytical strategies are applied to examine the robustness of the findings. We first present odds ratios (ORs) to describe the bivariate associations. Thereafter, we examine whether these associations remain after controlling for the aforementioned covariates by performing a logistic regression analysis and a propensity score weighting technique.

Multivariate analytical strategies are used because a simple comparison of recidivism risks between employed and unemployed ex-prisoners is confounded with pre-existing factors (e.g., work history) that affect not only the employment situation but also recidivism risks. Hence, to the extent that employment is influenced by self-selection, the work-crime relationship is potentially spurious. The same is true for the relationships between specific employment characteristics and recidivism.

Only an experimental design, in which individuals are randomly assigned to employment or unemployment (or specific job characteristics), would ensure that all

possible confounders are controlled for. These designs are, however, rare in the field of social sciences and also not without limitations. For instance, Sampson (2010) pointed out that the laboratorial settings in experimental studies can make findings less suitable for generalization to a real-world setting.

In recent years, propensity score methods are frequently used in quasi-experimental studies to deal with bias caused by observable covariates. We also employ a propensity score technique because it confronts the selection problem and is more robust with respect to model misspecification compared with regular regression adjustment (Drake, 1993). Another advantage of propensity score methods over regression analyses is the internal validity that results from this approach, as it assures the exclusion of “treated” individuals for whom no comparable “controls” are available.

The propensity score method involves performing a regression analysis to calculate the conditional probability (propensity) of being in the treated group given a set of observed baseline covariates (see Rosenbaum & Rubin, 1983). Once the propensity scores are calculated, a number of methods can be used to assure that the distribution of observed baseline covariates will be similar between treated and untreated subjects (see, for instance, Austin, 2011). In the current study, similarity in the propensity score distribution between the treated and untreated groups was assured by weighting on propensity scores, as will be discussed in more detail below.

The Use of Propensity Scores in This Study

The success of propensity score models relies on the set of confounding variables and sample size (see Shadish, 2013). The Prison Project data enable us to study a substantial group and take into account 34 potential confounding factors when estimating the effects of employment and employment characteristics on reoffending (see Table 2). As noted, there may still be hidden biases confounding our results after the differences in observables are taken into account. Moreover, the sample sizes differ depending on the employment characteristic under study (see Table 1). Nevertheless, a substantial share of the potential confounders can be eliminated and balance diagnostics give insight into the compatibility of the propensity score weighting method.

We estimated the propensity of treatment in a logistic regression. The next step was to assure the similarity in the propensity score distribution between the treated and untreated groups by dividing the data into propensity score percentiles and assigning a weight to each observation within a percentile. The weight assigned to the percentiles in the control group was based on the distribution of percentiles (number of observations per percentile) in the treatment group, resulting in similar distributions across groups.

Separate propensity score models were estimated for all the independent variables of interest: employed, employee, job retention, full-time job, higher occupational level, return job.⁹ To assess the specification of the propensity score models, we examined the overlap in propensity scores between the treated and control group (i.e., common support) and calculated two-sample *t* tests for group differences in baseline covariates before and after weighting to assess if balance improved using the proportional weighting technique.

Table 3. Balance Diagnostics Propensity Score Models.

	N	N without extreme PS	Out-of-balance covariates before weighting	Out-of-balance covariates after weighting	Balance improved after PSW
All (n = 714)					
Employed in first month after release	714	691	25	0	yes
Employed in first month after release (n = 209)					
Employee (vs. self-employed)	209	209	15	20	no
Retained job during 6-month follow-up	208	188	14	0	yes
Employee in first month after release (n = 144)					
Retained job during 6-month follow-up	144	144	13	7	yes
Full-time job	144	143	2	4	no
Higher occupational level	140	131	7	2	yes
Employee at time of arrest and in first month after release (n = 85)					
Returned to pre-prison employer	85	22	7	NA: small N	NA

Note. PS = propensity scores; PSW = propensity score weighting; NA = not applicable.
[†]p < .10. *p < .05. **p < .01.

Table 3 summarizes these balance diagnostics and shows that propensity score weighting proved to be a compatible analytical strategy for four out of seven models (employment, job retention [twice], higher occupational level). In these models, there was sufficient overlap in propensity scores between the treatment- and control group; for only a few observations, no “controls” were available and as such few observations had to be excluded. Moreover, in these models, balance improved substantially after weighting the data on the propensity scores; no or few group differences remained. The remaining out-of-balance covariates were controlled for in a logistic regression analysis. As is shown in Model 4 of Table 4, this led to similar conclusions in all instances.

Propensity score weighting did not seem an appropriate analytical strategy for three out of the seven models. With respect to the effect of working as an employee (vs. self-employed) and the effect of full-time work, the number of out-of-balance covariates increased rather than decreased. Moreover, the effect of job return could not be estimated using the weighting technique because the low common support in propensity scores percentiles led to the exclusion of too many observations. As a result, we base our conclusions on the role of these three employment characteristics in reoffending likelihood on the logistic regression analyses (Table 4; Model 2). Considering the previously discussed disadvantages of this analytical strategy

compared with the propensity score method, more caution is advised when interpreting these findings.

Results

Bivariate Analyses

Model 1 of Table 4 presents the bivariate (or naïve) associations between employment (characteristics) and recidivism. Starting with the importance of having a job (vs. being unemployed), we find that more than a quarter of the employed ex-prisoners (26.3%) are registered for a new crime in the 6 months following release. For unemployed ex-prisoners, this risk is significantly higher (37.0%; OR = 0.61). So while both groups are at high risk to reoffend within the first 6 months following release, unemployed ex-prisoners are more likely to reoffend than ex-prisoners who experience a quick transition to employment.

Reoffending risk also seems to be related to the kind of job that ex-prisoners find. More specifically, the various job stability measures seem to be associated with reduced risks. First, ex-prisoners who are able to retain the same job during the follow-up have a significantly lower risk of getting registered for a new crime than those who lose their job (15.5% vs. 35.1%; OR = 0.34). This association holds true when we only include those who worked as employees after release (13.8% vs. 32.6%; OR = 0.33). Second, ex-prisoners who worked as employees prior to their confinement and returned to their pre-prison employer upon release are significantly less likely to get registered for a new crime than those previous employees who found a new post-prison job (13.3% vs. 36.0%; OR = 0.27). Notably, in all three of these comparisons, the latter groups experience a recidivism risk that is comparable with the reoffending risk of ex-prisoners who did not find employment in the first month after release (37.0%).

Another notable finding is that a higher occupational level seems to be related to a lower risk of reoffending (12.0% vs. 27.8%). Working as an employee (vs. self-employed) and work intensity does not seem to correlate with this risk.

Multivariate Analyses

The next step is to control for factors that potentially confound the associations between employment (characteristics) and reoffending in logistic regression analyses and propensity score analyses (Table 4; Models 2, 3, and 4). As mentioned in the method section, the latter more rigorous analyses could be performed for four out of seven models: employment (vs. unemployment), job retention (twice), and occupational level.

A key finding is that, after controlling for pre-existing differences between employed and unemployed ex-prisoners, employment no longer seems to be related to reoffending risk. Both the logistic regression and the propensity weighting technique (Table 4; Models 2, 3, and 4) indicate that ex-prisoners who find a job in the first

Table 4. Odds Ratios (ORs) Employment (Characteristics) and Recidivism Risk Using Four Different Analytical Strategies.

	N	Employment category	% reoffended	Model 1		Model 2		Model 3		Model 4	
				Unweighted OR	OR logistic regression	PS weighted OR	PS weighted OR + direct adjustment out-of-balance covariates				
All (n = 714)											
Employed in first month after release	714	No	37.0	0.61**	0.87	0.912	NA: 0 out-of-balance covariates	—	—	—	—
	209	Yes	26.3								
Employed in first month after release (n = 209)											
Employee (vs. self-employed)	209	Self-employed	29.2	0.81	1.04	—	—	—	—	—	—
	65	Employee	25.0								
Retained job during 6-month follow-up	208	No	35.1	0.34**	0.62	0.48*	NA: 0 out-of-balance covariates	—	—	—	—
	111	Yes	15.5								
Employee in first month after release (n = 144)											
Retained job during 6-month follow-up	144	No	32.6	0.33*	0.93	0.76	—	—	—	—	0.59
	86	Yes	13.8								
Full-time job	144	No	27.9	0.81	0.87	—	—	—	—	—	—
	43	Yes	23.8								
Higher occupational level	140	No	27.8	0.35†	0.23	0.29†	—	—	—	—	0.23†
	115	Yes	12.0								
Employee at time of arrest and in first month after release (n = 85)											
Returned to pre-prison employer	85	No	36.0	0.27*	0.10	—	—	—	—	—	—
	25	Yes	13.3								

Note. PS = propensity scores; O-O-B = out-of-balance; NA = not applicable.
†p < .10. *p < .05. **p < .01.

month after release do not differ in recidivism risk from their unemployed counterparts. This implies that not the job per se, but pre-existing differences between employed and unemployed ex-prisoners drove the significant OR in the previously discussed naïve comparison of these two groups in recidivism risk.

In contrast to the null-effect of being employed, the findings pertaining to all working ex-prisoners indicate that type of job remains significantly related to reoffending after controlling for pre-existing differences. For instance, ex-prisoners who succeed in working in the same job during the entire 6-month follow-up, are less likely to get registered for a new crime than those who lose this job (Table 4; Models 3 and 4: OR = 0.48). This indicates that it seems valuable to distinguish between short-term employment and longer term employment, even within our relatively short period of follow-up. Short-term employment (<6 months) is less likely to result in lower recidivism risks than longer employment (6 months).

Shifting the focus to the models for salary workers exclusively, we find, different from the aforementioned result, that job retention is not related to reoffending among employees only. Among this group, occupational level seems to be of more importance; those who work in a higher occupational level are relatively less likely to reoffend (Table 4; Model 3: OR = 0.29, Model 4: OR = 0.23).

The final analysis pertains to ex-prisoners who worked as employees prior to their confinement and immediately upon release. After controlling for all potentially confounding covariates in a logistic regression analysis, no longer was a difference in reoffending likelihood found between individuals who returned to their pre-prison employer or found a job with a new employer. This non-significant effect might, however, result from a lack of statistical power (many covariates, small sample size, $N = 85$). Sensitivity analyses, in which different combinations of covariates were included in logistic regression analyses separately, indicated furthermore that the effect of job return disappeared only after we controlled for job retention. We found a strong association between these two employment characteristics; 63% of those who return to their prior employer immediately after release are able to hold down this job during the 6-month follow-up, compared with 16% of those who are not able to return ($OR = 9.06$, $p < .001$). Notwithstanding the small sample size, returning to a previous work environment might thus play an indirect role in an individual's reoffending likelihood through job retention.

Discussion

Using longitudinal data from the Netherlands, the current study examined the role of employment and employment characteristics in ex-prisoners' recidivism risks. A key finding was that the mere presence or absence of a job did not reduce ex-prisoners' recidivism risks after confounding factors were controlled for.

Although this finding might seem unexpected, it is in line with theoretical notions that imply that not just *any* job but jobs with *certain qualities* can affect reoffending. In addition, surprisingly, there is limited research on whether employment can deter high-risk adult offenders—who generally face a multitude of problems after release

(Visher & Travis, 2011)—and the handful of studies that are based on prisoner data show ambiguous findings. Research based on administrative data seems to confirm the crime-reducing effect of employment (Berg & Huebner, 2011; Piquero et al., 2002; Skardhamer & Telle, 2012), while survey-based research is less conclusive (Horney et al., 1995; Visher et al., 2011). In the current survey-based study, employment data were based on ex-prisoners' reports and as such include all economic activity (e.g., self-employment, off-the-books employment). In contrast, administrative data only capture formal employment as reported by employers. This difference in measurement might offer an explanation for the lack of strong evidence for the protective effect of employment in survey-based studies. In these studies, employment represents a wider range of (perhaps lower quality) jobs than the formal employment arrangements portrayed in administrative studies. Future research could test the validity of this explanation by using both survey and administrative data to measure the employment patterns of ex-prisoners.

A second important finding was that certain type of jobs seemed to be related to reoffending. Holding down a job during the 6-month follow-up and working in a job with a higher occupation level seems to decrease reoffending likelihoods.

The importance of job retention for reoffending was first emphasized by Sampson and Laub (1990, 1993, 2005). They showed that a self-reported measure of job stability decreased offenders' chances of getting registered for a new crime or reporting a crime. In their informal social control theory, Sampson and Laub ascribe this protective effect to offenders' involvement with co-workers and employers. Our finding on job retention seems to support these assumptions. We do note, however, that, in line with other previous work (Uggen, 1999; Van der Geest, 2011; Wadsworth, 2006), the findings with respect to job stability were not robust across all models. For instance, no significant association was found between job retention and reoffending when we focused on salary workers exclusively. We found some indication for the importance of a newly introduced indicator of job stability; returning to the pre-prison job seems negatively correlated with reoffending through job retention. This finding was, however, based on a small sample of prisoners ($N = 85$), which limited the use of a rigorous analytical strategy. Further research on larger sample sizes is therefore warranted to examine the importance of job retention and job return in reoffending behavior. Moreover, future qualitative research could advance on existing work by not only examining these objective job stability indicators but also examining more precisely the perceived quality of the ties to the workplace. This would allow researchers to measure more directly the validity of the theoretical mechanisms as discussed by Sampson and Laub (1993).

The importance of occupational level for reoffending behavior is discussed in economic theories; a good job will make it easier to satisfy needs and desires through legitimate means and the risk of losing such a job could stimulate offenders to desist. We found indeed that ex-prisoners who worked in higher positions (e.g., manager, real estate agent) were less likely to recidivate within the first half year. This is somewhat in line with the finding of Uggen (1999) that a job-shift from, for instance, the food industry to skilled manual labor led to an 11% crime reduction. Following economic

theories, it would be interesting to study the role of wage—another measure of job quality—in future research.

Several limitations of this study deserve attention in future research. To start, observational data can only approximate an experimental design, which would effectively rule out all potential confounders of the relationship between employment and recidivism outcomes. We used rich longitudinal data on post-release employment and are confident that our models severely reduce selection bias by accounting for many more potential confounders than most previous studies. In addition, we studied the temporal order of processes using fine-grained monthly units of time. Nonetheless, our analyses only account for observable covariates and the small (sub)samples limit the statistical power of our models and the compatibility of the more advanced propensity score methods in three out of seven models. Yet, for four of the seven models, we were able to use a propensity score technique. Such techniques are more robust with respect to model misspecification than regular regression analyses and are better in preserving the internal validity by excluding “treated” individuals for whom no comparable “controls” are available. A recommendation for future research is to confront the pernicious issue of selection bias by using large samples and multiple analytical strategies to confirm the robustness of the obtained effects.

A second limitation is that we focused on the employment situation in the immediate month after release and used a relatively short follow-up period. Certain types of employment, such as return jobs or assigned jobs (as part of a reentry program), are arguably overrepresented due to our measurement of employment. In addition, although the data include detailed information on the main job, we were unable to examine whether ex-prisoners were working multiple jobs. Although a similar design was used in previous work (Berg & Huebner, 2011; Visher et al., 2008), we encourage future research to examine the robustness of our findings using a more dynamic measurement of employment outcomes and a longer follow-up period.

Third, it is a matter of speculation whether we would find similar results using data of other Western countries. We used data from the Netherlands, an interesting case study with a relatively low unemployment rate, mild penal climate, and restricted access to criminal history records. Potentially, these circumstances result in relatively good labor market prospects for Dutch ex-prisoners. Yet, especially countries in Northern Europe resemble the Netherlands in their policies and practices, and this could mean that our findings might apply to these countries.

Notwithstanding the limitations of this study, we conclude that not the guidance to a job, but guidance to *certain type* of jobs could help to reduce crime rates among ex-prisoners. The findings seem to favor policies that help connect ex-prisoners to stable jobs and jobs of a higher occupational level. Most evaluation studies find few differences in employment and rearrest between program participants and non-participants (Farabee et al., 2014; Visher et al., 2005). A drawback of such programs is that they often connect offenders to temporary and low-skilled employment, while theories and scholars underscore the importance of job quality (Uggen, 1999). Problems arise, however, when ex-prisoners are assisted to find high-quality jobs. Many ex-prisoners face a human capital deficit that complicates the guidance to such jobs. Moreover, as

noted by Uggen (1999), “How can policy makers justify allocating the best jobs (or the training required to access them) to the least serving members of a large and needy underclass population?” (p. 145). It might, however, be achievable to help place ex-prisoners in more sustainable employment. Guidance to a familiar work environment may potentially help increase ex-prisoners’ participation rate and decrease their reoffending rate without necessitating much additional investment of public funds in prisoner reentry or harming the interests of employers. Further research on the effectiveness of this policy suggestion is, however, warranted.

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Notes

1. The Dutch institutional environment is known for its relatively large proportion of pre-trial detainees. Whereas this group accounts for roughly 20% of the prison population in most Western-European countries and the United States, pre-trial detainees account for almost half (47%) of the prison population in the Netherlands (Walmsley, 2014). While most of the inmates were not yet convicted at the time of the first interview (in pre-trial detention), reports of Statistics Netherlands indicate that the chance of conviction is very high among this group. In the Netherlands, 90% of *all* suspects charged with a criminal offence are found guilty (van Rosmalen, Kalidien, & Heer-de Lange, 2012). This percentage is likely to be higher among the selection of serious suspects who are sent to pre-trial detention.
2. In this study, “unemployment” refers to all jobless ex-prisoners and is not limited to those ex-prisoners who are actively searching but cannot find a job. Note, however, that we do make a distinction between unemployed individuals (searching for a job) and non-participants (not searching for a job) in Table 2.
3. The sample was generally representative of all prisoners who met the selection criteria in terms of age, marital status, type of crime, and receiving an unconditional prison sentence for the index offence. Chi-square tests were performed to test for significant differences between groups, and Mann–Whitney tests were used for ordinal or interval variables with skewed distributions.
4. Importantly, difference tests showed comparability between the post-release interview (R1) and in-prison interview (P1) samples across a wide range of baseline covariates (e.g., marital status, parenthood, educational level, homelessness, index offence, number of previous convictions, time served). Registered criminal behavior during the follow-up period

was available for the larger P1 sample. The groups showed a similar likelihood of reoffending within the first 6 months after release.

5. Most of the 86 missing values on recidivism are caused by the fact that it concerned ex-prisoners who had not been released for 6 months on July 11, 2012.
6. Note that not all charges will necessarily result in a conviction. Yet, given that, in 2011, approximately 90% of all charged suspects in the Netherlands are found guilty (van Rosmalen et al., 2012), and our research group consists of relatively serious offenders, we view the risk of overestimation as a lesser concern (than the risk of underestimation that arises when we use reconviction data to measure recidivism).
7. Note that our survey data can include all kinds of employment (formal and informal work) reported by ex-prisoners because we did not give precise definitions on what we define as "work." This approach aligns with previous research showing that ex-prisoners might often fail to make a distinction between formal and informal work because they spend their whole working lives in the informal labor market (Fletcher, 2008).
8. Note that this percentage is somewhat lower when we include the eight previously employed ex-prisoners who reported to be self-employed in the first month after release (65%). For reasons of validity, we excluded these persons in the model on the effect of returning to previous employer versus working for a new employer.
9. In the different models on the effect of employment characteristics, information on additional job characteristics was added to the propensity model.

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