Incarceration experience at older ages.

Does employment protect against recidivism?∗

Abstract:
We investigate whether employment protects against and/or postpones recidivism among older males who committed their first crime late in life. We employ a multi-state model and difference-in-differences approach, and contribute to the literature by reporting a causal relationship, as we are able to differentiate between those men who were (treated) and were not (control) employed after incarceration. We use administrative data on the entire set of older males in Poland who had their first conviction late in life, had all of their incarceration spells between 2004 and 2017, and were registered with a public employment office thereafter. The sample covers 34,401 individuals, 44% of whom were employed at least once during the analysed period. Of the men in the sample, 97% were working in routine manual occupations. Our study indicates that among these men, employment affected the likelihood of recidivism, and postponed recidivism. The effect of employment on the risk of reincarceration depended on the number of imprisonment spells the individual had. However, the effect we found also depended on the method used. The results of the multi-state model indicated that employment reduced the risk of recidivism by 7% for those who were at risk of a second incarceration. On the other hand, employment increased the probability of subsequent recidivism by 8-10% for those who were at risk of a third or fourth incarceration. These results suggest that institutional interventions should seek to increase the labour market attachment of former prisoners, and, in particular, should focus on ex-offenders with only one conviction, as being attached to the labour force seems to matter more the fewer imprisonment spells an individual has experienced.

JEL classification: C41, C55, I39, J64, J88
Keywords: recidivism, incarceration, social exclusion, social reintegration, employment, unemployment, recurrent event data models, multi-state models

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∗ This article was prepared within a project: “Registered unemployment as a non-traditional route to non-participation of older workers. Recurrent event longitudinal data analysis” financed by the National Science Centre Poland, project no. UMO-2018/30/E/HS4/00335.
1. Introduction

Many ex-offenders are unrealistically optimistic shortly after they are released from prison, and are convinced that they will easily cope outside of prison without committing another crime (Zamble and Quinsley, 1997). Nevertheless, ex-prisoners are particularly likely to recidivate in the months immediately after release (Langan and Levin, 2002; Wartna et al., 2011). The main explanations for recidivism are that ex-offenders often have a poor economic situation, are unemployed after release, are illiterate, or fail to adhere to social norms (Nally et al., 2014, Visher and Travis, 2003). The crime-reducing effect of employment stems from a combination of factors, such as job stability, work intensity, and earnings (Sampson and Laub, 1990; Kleck and Jackson, 2016; Lageson and Uggen, 2013). Having a job provides ex-offenders with financial independence, social contacts, and the opportunity for informal social control of the workplace (Sampson and Laub, 1993). Employed individuals have less time to spend on criminal behaviour (Warr, 1998). Employment can delay the point in time when an ex-offender engages in another criminal act (Tripodi et al., 2010), and having a longer period of employment can decrease the likelihood of recidivism (Verbruggen et al., 2012). There is, however, evidence, that not all forms of employment prevent ex-offenders from committing criminal acts (Ramakers et al., 2017). Ex-prisoners often work in temporary and poorly paid jobs (Western, 2006). They may face discrimination in the labour market, and their employability level is often low (Holzer et al., 2004; Pager, 2007). Still, receiving benefits might postpone recidivism, as there is evidence that the crime rates of ex-offenders are lower during the active benefit period (Bennett 2020).

In this study, we investigate whether employment protects older males who committed a first crime late in life from recidivism. We define recidivism broadly, as the reincarceration at any time after the first incarceration spell. We contribute in three major ways. First, we explore causality in the relationship between employment and recidivism by differentiating
between individuals who were (treated) and were not (control) employed after incarceration (and prior to subsequent imprisonment). Second, we apply methods tailored to longitudinal analysis: sequence analysis and multi-state models. Third, we use administrative data and have a large sample of older men.

So far, few studies have found evidence of a causal relationship between employment and recidivism. Visher et al. (2005) performed a meta-analysis of eight random-assignment employment programs for individuals with a criminal record who were not in custody, and the results did not indicate a reduction in recidivism. More recent studies have shown that employment reduces recidivism (Skardhamer and Telle, 2012; Verbruggen et al., 2012). Fallesen et al. (2018) investigated the effects of active labour market policy programmes (ALMP) on crime, focusing on the effects on unemployed, uninsured welfare recipients who were active. Borland and Hunter (2000) examined a potential simultaneity bias between an individual’s employment status and arrest record. Ramakers et al. (2017) also compared the fact of being employed after release versus unemployment. They used among others propensity score matching technique. In our study, we apply quasi difference-in-differences approach what is an extension to previous research.

Previous research using longitudinal approach is limited (Ramakers et al., 2017). Usually, survival analysis was applied (Skardhamer and Telle, 2012; Tripodi et al., 2010). Survival analysis requires assumption that failure times are independent and recurrent event data violate this assumption. Our multiple-failure data and indicated methods allow us to account for interdependencies between states and multiple incarceration spells. We identify up to four incarcerations per individual and we check the robustness of the results by accounting for the ordinal number of the imprisonment. Such approach provides more holistic view.

D’Alessio (2013) also studied the curvilinear relationship between unemployment and crime.
Quantitative studies have often referred to paroles, young males, participants of employment programmes. Various countries have been studied, still most often these were USA, Scandinavian countries, Italy, Australia. Moreover, analyses were mostly based on relatively small samples, with a few exceptions like D’Alessio (2013), Kang (2019), Schnepel (2017), and Skardhamer and Telle (2012). We examine the specific group of male ex-offenders who had their first imprisonment in older age. We use administrative data for Poland on entire population of ex-offenders born between 1940 and 1965 who served a sentence between 2004 and 2017 and registered with public employment offices thereafter. These data provide information on previous employment and incarceration. The sample consists of 34,401 individuals. This is a peculiar, but very precise, group of ex-offenders, what results in clear quantitatively based qualitative findings, though not necessarily easily generalizable.

Our findings confirm that being employed postpones recidivism. The effect of employment on the risk of reincarceration is mixed depending on the ordinal number of the imprisonment spell and the method used. However, being attached to the labour force seems to matter more the fewer imprisonment spells an individual has experienced. These results may be useful for developing social reintegration policing directed at older ex-offenders.

2. Older previously incarcerated men

Registering with the labour office in Poland is compulsory only for workers who want to and are eligible to collect unemployment benefits. All others have the option of registering, as unemployed or job seekers, in order to take advantage of public employment intermediation, ALMP measures, and to obtain free health insurance. When workers register with a public employment office, they are asked to provide information on previous contributory spells, which include incarceration. This information is complemented along each subsequent registration. In our view, administrative data are a unique source of a large sample of ex-
offenders and provide valuable, longitudinal information on these workers’ labour market attachment.

We study the period between 1 June 2004 and the end of 2017. In that period\(^2\), labour market legislation was partially amended, but there was no special labour market policy directed at ex-offenders. We focused on ex-offenders who experienced incarceration period(s) with a single duration of no longer than two years. This time-censoring is based on the criminal code. Most of the ex-offenders who had been in prison for up to two years had committed a non-violent crime, such as theft or burglary, an unintentional offence, or a minor, low-harm crime. The ex-offenders who committed major crimes were excluded from the analysis, as they were often antisocial and prone to violate social norms even before incarceration\(^3\). Such exercise can potentially generate selection bias, but we assumed ex-offenders who experienced shorter imprisonment spells should, in general, have a stronger labour market attachment.

We focused on men who were convicted for the first time late in life: that is, we examined those individuals who had all of their incarceration spells in the 2004-2017 observation period, and who registered with a public employment office thereafter. Table 1 displays descriptive statistics. Most men were poorly educated, and their total employment tenure before their last registration with a public employment office was quite short. The data also included information on the occupations of the men who found employment, although there were many gaps in this information. Of these occupations, 30% were simple jobs. In addition, 25-27% of the men were employed in services or trade, or were industrial workers or craftsmen (one-digit indications of ISCO major groups). Almost 97% of the men were

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\(^2\) The Act of 20 April 2004 on Employment Promotion and Labor Market Institutions was introduced on 1 June 2004, and we had access the data up to the end of 2017.

\(^3\) Out of the entire dataset for the 1990-2017 period, around 85% of the ex-prisoners had served sentences with a maximum length of two years.
employed in routine manual jobs\(^4\). Around 20\% of all individuals collected unemployment benefits\(^5\) at least once, but 62\% of them did so exactly once. However, the ex-offenders collected unemployment benefits in only 8.0\% of unemployment spells.

Table 1. Descriptive statistics of the individuals’ characteristics

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Mean</th>
<th>Std. dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age at the beginning of the 1(^{st}) spell (in years)</td>
<td>47.6</td>
<td>5.2</td>
</tr>
<tr>
<td>Educational level (percentage distribution)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>primary</td>
<td>0.553</td>
<td>-</td>
</tr>
<tr>
<td>vocational</td>
<td>0.362</td>
<td>-</td>
</tr>
<tr>
<td>secondary vocational, general secondary, post-secondary, or tertiary</td>
<td>0.085</td>
<td>-</td>
</tr>
<tr>
<td>Total tenure (in years)</td>
<td>12.5</td>
<td>9.8</td>
</tr>
<tr>
<td>Unemployment benefit duration (in months)</td>
<td>6.0</td>
<td>3.9</td>
</tr>
</tbody>
</table>

Notes: no. of observations (individuals) 34,401, no. of observations for education 34,312, no. of observations for total tenure 27,898.

Source: own elaboration.

Figure 1. Distribution of the longest incarceration spell

Source: own calculation.

We observed 44,094 incarceration spells. Almost 79\% of the ex-offenders had experienced only one incarceration spell in the analysed time span, while 16\% had two such spells, almost 4\% had three spells, and less than 1.5\% had at least four imprisonment spells. The sentences were usually expressed in full months, in line with the criminal code, as confirmed in the duration histogram. The mean duration of the incarceration spells did not differ

\(^4\) Occupations unified according to the international standard classification of occupations (ISCO-08) assigned to task content groups in Acemoglu and Autor (2011), and adjusted for the Polish labour market by Hardy et al. (2018).

\(^5\) In 2009, the unemployment benefit system was reformed. Since then, unemployment benefits are granted for 180 or for 365 days (previously, they were granted for up to 18 months), depending on the local labour market conditions and the tenure and the age of the unemployed person.
with respect to the ordinal number. However, if we look at the longest incarceration period each individual experienced, we see that the recidivists spent more time in prison (compare Figure 1). Moreover, the mean gap between multiple incarceration spells was 1.9 years, while the median gap was 1.4 years. These values decreased with each subsequent incarceration period: on average, the second spell occurred within 2.1 years of the first one (median equalled 1.5 years), while the third spell occurred within 1.7 years of the second spell (median equalled 1.2 years).

3. **Is employment associated with a lower recidivism rate?**

The data included information on the labour market status, which we grouped into four main categories: employment (including ALMP), unemployment, incarceration and non-participation. Employment referred to formal employment regardless the type of contract as well as participation in ALMP that resulted in deregistration from labour office. Unemployment referred to being registered with public employment office. Non-participation included all other situations that could not be classified as employment, unemployment or incarceration. Data included daily dates of transitions between particular states what allowed us to retrieve the full labour market attachment history of individuals. We investigated complete spells and we explored the trajectories of the ex-offenders in the labour market.

In the first step, we employed longitudinal analysis: social sequence analysis and multi-state models to examine the relationship between employment and recidivism. Table 2 displays descriptive statistics concerning the duration of particular states. The distribution of the length of particular kinds of spells was generally asymmetric: i.e., it was right-skewed, with some outliers at the ends of both tails. As the ex-offenders frequently transitioned between states, many of them experienced multiple spells. Nonetheless, slightly more than 56% of the ex-offenders did not work even once during the analysed time span. The incarceration spells were directly preceded by unemployment spells in 44% of cases and by non-participation in
52% of cases. The mean duration of these non-participation spells was $13.5 \pm 19$ months, although the median duration was just 6.0 months. The incarceration spells were followed by unemployment spells in 15% of cases, and by non-participation spells in almost 85% of cases. These spells were, however, very short: the median was just 11 days, while the mean was $2.2 \pm 7$ months. In 95% of cases, the unemployment spell occurred after these short non-participation spells.

Table 2. Descriptive statistics of the duration of particular spells (in months)

<table>
<thead>
<tr>
<th>Spell</th>
<th>Mean</th>
<th>Std. dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Employment</td>
<td>7.0</td>
<td>10.6</td>
</tr>
<tr>
<td>Unemployment</td>
<td>9.6</td>
<td>12.0</td>
</tr>
<tr>
<td>Non-participation</td>
<td>7.3</td>
<td>12.4</td>
</tr>
<tr>
<td>Incarceration</td>
<td>7.8</td>
<td>5.7</td>
</tr>
</tbody>
</table>

Notes: no. of observations: employment 38,462; unemployment 142,447; non-participation 158,459; incarceration 44,094.

Source: own calculation.

By means of social sequence, we identified the labour market status of each individual on each day of the observation period. We centred the outcome around the beginning of each individual’s first incarceration spell. This enabled us to compare the individual’s labour market status before and after imprisonment. The results are presented in the stacked tempogram in Figure 2.

Figure 2. Stacked tempogram of the shares of ex-offenders in a particular labour market state in the observation period before and after the beginning of the first incarceration spell (upper part), and the number of individuals observed (lower part)
Notes: The jump in the frequency at zero is attributable to the fact that for around 9000 of the ex-offenders, the first observed spell was imprisonment.

Source: own elaboration.

We formally tested the relationship between employment and recidivism by looking at the correlation and the causal relationship between the two events. First, we tried to determine the impact of employment on the probability of having a subsequent incarceration spell. To do so, we employed a multi-state model (MSM). In such models, individuals move among a finite number of states. If a state is transient, further transitions are possible, otherwise the state is absorbing. A multi-state process (compare Meira-Machado et al., 2009; de Wreede et al., 2010) is a stochastic process \((X(t), t \in T)\) with a finite state space \(S = \{1, \ldots, N\}\), where \(T = [0, \tau], \tau < \infty\) is a time interval and the value of the process at time \(t\) is the state occupied at that time. Over time, as the process evolves, a history \(H_{t-}\) is generated over the interval \([0, t)\). It includes information on the preceding states, the timing of transitions, etc. The multi-state process is fully characterised through transition probabilities between states \(h\) and \(j\): 

\[ p_{hj}(s, t) = \mathbb{P}(X(t) = j | X(s) = h, H_{s-}), \]  

for \(h, j \in S, s, t \in T, s < t\) or through transition intensities, which represent the instantaneous hazard of progression to state \(j\) conditionally on occupying state \(h\):

\[ \alpha_{hj}(t) = \lim_{\Delta t \to 0} \frac{p_{hj}(t, t+\Delta t)}{\Delta t}, \]  

(Meira-Machado et al., 2009). The flexibility of the multi-state models allows us to account for the transition-specific covariates in semi-parametric analysis. Here, we
can diversify the impact of particular variables on certain transition rates; for example, between states \( h \) and \( j \) (de Wreede et al. 2011):

\[
\alpha_{h,j}(t|\mathbf{Z}) = \alpha_{h,j,0}(t) \exp(\beta^T \mathbf{z}_{hj})
\]

where: \( \alpha_{h,j,0}(t) \) is the baseline hazard for this transition, \( \mathbf{Z} \) is the vector of covariates at baseline, and \( \mathbf{z}_{hj} \) is the vector of transition-specific covariates.

We proposed a semi k-progressive model, which is displayed in Figure 3. Here we reclassified the type of spells. We created a new type of spell: a non-incarceration spell which comprised all spells but imprisonment between subsequent incarceration spells. In this model, we observed individuals since the beginning of the first incarceration spell that occurred no sooner than on 1 June 2004 (so we censored non-incarceration spell before the first imprisonment). When an ex-offender ended the incarceration spell, he experienced a non-incarceration spell, which included information on his employment experience. Next, an ex-offender may have had another incarceration spell if he was a recidivist. Each state could be transient or absorbing (apart from the first incarceration spell)\(^6\) if it finished the trajectory we observed for the given individual. We estimated the model non-parametrically and semi-parametrically by applying \textit{mstate} package in R (compare de Wreede et al. 2010; de Wreede et al. 2011).

\textbf{Figure 3. K-progressive model describing a transition pathway between incarceration and non-incarceration spells}

Notes: \( I1 \) – first incarceration spell, \( NI1 \) – first non-incarceration spell, \( I2 \) – second incarceration spell, \( NI2 \) – second non-incarceration spell, \( I3 \) – third incarceration spell, \( NI3 \) – third non-incarceration spell, \( I4 \) – fourth incarceration spell, \( NI4 \) – fourth non-incarceration spell.

\textit{Source: own calculation.}

\(^6\) We excluded ex-offenders for whom we observed only one incarceration spell (\( I1 \)) and nothing thereafter (1273 individuals). For them we did not observe any complete non-incarceration spell. This might mean that the person registered with a public employment office, but the spell was incomplete.
In the non-parametric estimates, we computed transition intensities and transition hazards, while assuming a separate baseline hazard for each of the transitions. Next, we identified a transition probability matrix. Non-parametric methods were applied to samples of ex-offenders who experienced either two, three, or four incarceration spells. Figure 4 displays the results by means of stacked transition probabilities for particular models. The vertical distance between two adjacent curves represents the probability of being in the corresponding state in a given moment (de Wreede et al., 2011).

Figure 4. Non-parametric estimates of stacked transition probabilities, ex-offenders who had either two, three, or four incarceration spells

Notes: I1 – first incarceration spell, NI1 – first non-incarceration spell, I2 – second incarceration spell, NI2 – second non-incarceration spell, I3 – third incarceration spell, NI3 – third non-incarceration spell, I4 – fourth incarceration spell. The vertical distance after the last incarceration spell is not interpretable, since for some individuals the given imprisonment spell was the last one observed and in consequence we did not observe complete non-incarceration spell thereafter.

Source: own calculation.
In the next step, we applied a semi-parametric approach to the k-progressive model. We examined both whether employment postponed recidivism, and whether employment affected the risk of recidivism. In the first part, we grouped individuals into samples of those who had a given number of incarceration spells. In the second part, we grouped individuals into samples of those who had either $n$ or $n + 1$ incarceration spells. We also accounted for some other covariates. As in the case of non-parametric estimates, we assumed a separate baseline hazard for each transition. The estimated coefficients and their standard errors are shown in Table 3.

Table 3. Semi-parametric estimates of the k-progressive model

<table>
<thead>
<tr>
<th>Variable / estimate</th>
<th>Parameter estimate (standard errors)</th>
<th>if employment postpones recidivism</th>
<th>if employment affects recidivism</th>
</tr>
</thead>
<tbody>
<tr>
<td>type of analysis</td>
<td></td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>no. of incarceration spells</td>
<td></td>
<td>-0.2758*** (0.022)</td>
<td>-0.2734*** (0.037)</td>
</tr>
<tr>
<td>empl. N1→I2 (no – yes)</td>
<td></td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>empl. N2→I3 (no – yes)</td>
<td></td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>empl. N3→I4 (no – yes)</td>
<td></td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Concordance</td>
<td></td>
<td>0.636 (se=0.003)</td>
<td>0.589 (se=0.005)</td>
</tr>
<tr>
<td>Likelihood ratio</td>
<td></td>
<td>3059 (df=8)</td>
<td>616.6 (df=9)</td>
</tr>
<tr>
<td>Wald test</td>
<td></td>
<td>3121 (df=8)</td>
<td>644.5 (df=9)</td>
</tr>
<tr>
<td>Number of events</td>
<td></td>
<td>15854</td>
<td>6385</td>
</tr>
</tbody>
</table>

Notes: standard errors reported in parentheses, * - significant at the 10% level, ** - significant at the 5% level, *** - significant at the 1% level; equations also included: age at the beginning of the last non-incarceration spell observed, education estimated at the last non-incarceration spell observed, longest incarceration spell observed estimated at the last non-incarceration spell observed (up to one month, 1-3 months, 3-6 months, 6-12 months, 12-24 months).

Source: own calculation.

Only employment in the last non-incarceration spell for ex-offenders who either had or did not have a subsequent incarceration can be interpreted as the covariate affecting the likelihood of recidivism (columns 4-6 in Table 3). In all other cases, the parameter estimate should be interpreted according to whether it postponed recidivism, since all of the individuals in the given sample experienced a subsequent incarceration spell (compare the survival analysis presented in Tripodi et al., 2010). For example, for ex-offenders who had two or three imprisonment spells, employment after the first conviction postponed (or did not postpone)
recidivism, but employment after the second conviction affected (or did not affect) whether reincarceration occurred.

Employment reduced the risk of recidivism by 7% for those who were at risk for a second incarceration spell. But those who were at risk of a third or fourth incarceration spell fared worse, as employment increased their probability of subsequent recidivism by 8-10%. The other results consistently indicated that employment postponed the subsequent incarceration spell. Among most of the ex-offenders, the greatest impact was after the first imprisonment, regardless of the total number of convictions the individual had. However, among those who had four imprisonment spells, the greatest effect was after the third conviction.

In the last step, we applied a difference-in-differences method to examine the causal relationship between employment and reincarceration. The ex-offenders who were employed were ‘treated’, while the ‘control’ group consisted of those who did not work. We are aware that this was a quasi-causal framework, since we did not have an exogenous intervention that occurred at a certain point in time. Rather, we centred the time before or after particular incarceration spells, which could have occurred at any time between 2004 and 2017 (compare Ryan et al., 2015). The interaction term referred to those ex-offenders who were employed in the last non-incarceration spell (that preceded potential reincarceration). We have also accounted for the high-dimensional fixed-effects vector that included the year of the first incarceration spell observed, the level of education, and the longest incarceration spell observed. The equation became:

\[ y_{it} = \beta_0 + \beta_1 \cdot treat_i + \beta_2 \cdot interact_i + \mu FE_{it} + u_{it} \]

where \( y_{it} \) is the outcome variable (i.e., whether the individual had a subsequent incarceration spell, or the time to reincarceration), \( treat_i \) refers to whether the person was employed at any
time in the observation period), \( i_{nteract} \) refers to whether the employment occurred in the last non-incarceration spell, and \( FE \) stands for fixed effects. \( \beta_2 \) is the coefficient of interest.

### Table 4. Quasi difference-in-differences analysis results

<table>
<thead>
<tr>
<th>Variable / estimate</th>
<th>Parameter estimate (standard errors)</th>
</tr>
</thead>
<tbody>
<tr>
<td>type of analysis</td>
<td>whether employment postpones recidivism</td>
</tr>
<tr>
<td>dependent variable</td>
<td>time to reincarceration</td>
</tr>
<tr>
<td>no. of incarceration</td>
<td>2 or 3 or 4</td>
</tr>
<tr>
<td>spells</td>
<td>2 or 3 or 4</td>
</tr>
</tbody>
</table>

| treat                | -112.9019*** (21.574)               | 0.0982*** (0.005)                    |
| interact             | 604.6488*** (29.377)                | -0.1469*** (0.006)                  |
| no. of observations  | 5,448                                | 32,475                               |

Notes: standard errors reported in parentheses, * - significant at the 10% level, ** - significant at the 5% level, *** - significant at the 1% level; equations also included: age, education, longest incarceration spell observed (up to one month, 1-3 months, 3-6 months, 6-12 months, 12-24 months), and year of the first incarceration spell observed.

Source: own calculation.

The results reported in Table 4 indicate that employment postponed subsequent convictions (columns 1-3); that employment reduced the risk of reincarceration; and that the negative effect occurred, irrespective of the number of previous incarceration spells.

### 4. Concluding remarks

We investigated the relationship between employment and reincarceration by looking for a correlation and a causal association between them. We examined the criminal behaviour of a particular group of older males who were first convicted of a crime late in life. However, we restricted the sample to those who were sentenced to spend up to two years in prison (in a single spell), and who might have retained an attachment to the labour force.

Our study indicates that employment affected the likelihood of recidivism. Our estimation results produced a consistent picture showing that employment postponed recidivism. The effect of employment on the risk of reincarceration was mixed depending on the ordinal number of the imprisonment spell and the method used. An individual’s labour force attachment seemed to matter more the fewer imprisonment spells he had experienced.

Our results are in line with those of previous research, which indicated that not all kinds of employment are equally efficient in helping ex-offenders re-integrate into society.
Most of the ex-offenders in the sample had simple jobs that were poorly paid and did not require specific qualifications. We performed a robustness check of the results by re-estimating a k-progressive model with an additional covariate indicating whether the individual collected unemployment benefits. We restricted the analysis to examining the impact of receiving unemployment benefits, and not to being unemployed in general. The eligibility criteria included having been employed for at least 12 months out of the preceding 18 months. Thus, collecting benefits was a direct indication of prior labour market attachment. The statistically significant results proved that among these ex-offenders, collecting unemployment benefits postponed a second incarceration spell (for those who were at risk of third imprisonment spell), decreased the risk of a second incarceration spell, but increased the risk of a third and fourth imprisonment spell. Compared to the effect of employment, the impact of collecting unemployment benefits was weaker on postponing recidivism and was stronger on the likelihood of recidivism, although it was similar in direction. These findings may suggest that income alone is not a sufficient proxy for employment diversity. These results are also in line with those of a study (Bennett 2020) showing that transitions between different states (between being employed and receiving passive benefits and between receiving active benefits and receiving social assistance) might be linked to crime spikes, and could, therefore, generate associated social costs. Other job characteristics – like the type of job; or the quality, safety, and stability of the job – may also influence the quantitative impact of employment on reincarceration.

Using a multi-state model and a difference-in-differences approach, we also accounted for the impact of other covariates. Across the samples and the methods we applied, the effects of the ex-offenders’ age, educational level, and longest incarceration spell were found to be mixed in terms of significance, magnitude, and direction. These results might be explained in

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7 Detailed results are available from the authors upon request.
part by the peculiarities of the sample we examined, and the substantial asymmetries in the
distribution of particular covariates.

We are aware that even though our findings are very promising and robust (especially
with respect to the labour market attachment covariates), our analysis has some limitations, and
that there are major concerns about the representativeness of the data we used. The
administrative data are not representative of all unemployed individuals. Between 1995 and
2016 in Poland, an average of 72% of the workers who were unemployed according to the LFS
were registered with a public employment office. On the other hand, around 61% of those
registered with a public employment office were unemployed, according to the ILO definition
adopted in LFS. We also assessed the population coverage by comparing administrative data to
demographic data. Around 30% of those individuals born in the 1940s, but more than 50% of
those born between 1950 and 1965, ever appeared in a public employment office registry. Given
the peculiarities of the data, we have tried to estimate our coverage of incarceration spells by
comparing our data with penology data for the 2004-2017 period. Our data provide information
on around 2% of the stock of ex-prisoners at the end of a given year, and on 3.5% of the inflow
and 3.5% of the outflow of ex-offenders during a given year. While these figures seem small,
the time trends were similar, which is, undoubtedly, an advantage.

Our database did not allow us to unequivocally explain all of the observed
relationships. The attitudes of former prisoners towards employment are not caused by their
lack of knowledge of their rights. It is the duty of the prison services, often in cooperation with
social welfare centres, to instruct prisoners leaving prison about their options for securing a job.
Nevertheless, while keeping in mind the limitations of our research, we argue that employment
played an important role in postponing and reducing the risk of recidivism among the men we
studied. Our policy recommendation is that the state should take action to increase the labour
market attachment of ex-prisoners. It seems reasonable to focus such efforts on ex-offenders
with only one previous conviction, as the impact of these programmes is likely to be the greatest for these individuals. Multi-recidivists might find themselves in a vicious circle, and may be doomed to subsequent reincarceration. Thus, helping these individuals may be more difficult. Employability, as expressed by direct employment and job search skills, is essential to re-integrating ex-offenders into society. Making use of this largely untapped labour supply is crucial not only for the ex-offenders themselves, but for society, as the failure to do so has social costs, as well as direct labour market costs.
Literature


