

LOCKED UP AND LOCKED OUT? THE IMPACT OF IMPRISONMENT ON LABOUR MARKET ATTACHMENT

OLOF BÄCKMAN*, FELIPE ESTRADA and ANDERS NILSSON

This article investigates what effects a first prison sentence has on labour market inclusion, both by comparing those sentenced to prison to the population as a whole, and by comparing groups of convicted offenders. We utilize longitudinal data on criminal sanctions and earnings available for two complete birth cohorts of Swedish men (N = 107,337). These data enable us to compare the labour market attachment of prison inmates both before and after imprisonment. Results from propensity score matching show small negative effects of imprisonment on post-release labour market attachment. Moreover, we find no effect for those without pre-sentence labour market attachment. Thus, the negative effects are restricted to those with some labour market attachment before imprisonment.

Key Words: incarceration, employment, propensity score matching, life-chances

Introduction

Prison inmates constitute a group characterized by economic precariousness and weak attachment to the labour market. Comparisons between prison inmates and others reveal major differences across a number of dimensions of living conditions such as education, employment, housing and health. Furthermore, those who experience resource deficiencies on one dimension tend also to do so on other dimensions, which is why we tend to find a substantial accumulation of resource deficiencies in this group (Nilsson 2002). Thus, we would expect the range of job opportunities open to newly released prisoners to be limited. In addition, the prison sentence itself may further limit the range of available opportunities. This latter factor constitutes the focus of the present article, in which we look more closely at how imprisonment affects on labour market attachment by analysing a large dataset comprised of Swedish administrative register data.

Criminal convictions, in general, and custodial sanctions, in particular, may affect negatively on individual opportunities on the labour market in several ways. From the point of view of cumulative disadvantage theory, for instance, incarceration may cut off opportunities and prospects for stable employment later in life, thus adding further disadvantage to a life trajectory already characterized by weak bonds to conventional society (Sampson and Laub 1997). Several mechanisms have been suggested in the literature to explain such a process. With criminal sanctions there follows a stigma, which may negatively affect the willingness of potential employers to hire a former prison inmate (e.g. Boshier and Johnson 1974; Pager 2003). This stigma, together with the incarceration itself, may also strengthen a criminal self-identity and criminal capital

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(McCarthy and Hagan 2001). During the time spent in prison, the inmate also forfeits the opportunity to accumulate work experience which, in turn, reduces subsequent opportunities for legitimate work (Apel and Sweeten 2010).

Another possibility is that imprisonment has only a small or even no effect on employment and earnings. As already mentioned, the accumulation of resource deficiencies found among inmates is substantial, usually even prior to the prison term. As a consequence, the latitude available for further deterioration may be limited. A recent article focusing on the employment histories of prison inmates, which identified this particular pattern, described the group as being ‘[d]own before they go in’ (Ramakers *et al.* 2015).

Although only rarely attended to by the literature, a third possible scenario is that imprisonment has positive consequences for labour market attachment upon release. Scandinavian crime policies and penal law have sometimes been described as exceptional, with low levels of imprisonment and also humane prison conditions (Pratt 2008). The individual preventive mission of the Swedish Prison and Probation Service has been expressed in terms of the slogan ‘come better out’, and aims at producing a situation in which the convict leaves prison better equipped and in better shape than was the case prior to the prison sentence (Kriminalvården 2007). In line with this idea, the results of a recent study based on Norwegian administrative register data indicate that a prison sentence may indeed have a positive effect on employability for inmates whose labour market attachments were weak prior to imprisonment (Bhuller *et al.* 2016).

Attempts to examine whether and how prison sanctions affect employability and earnings have proved challenging for several reasons, primarily as a result of the unavailability of data and selection problems, i.e. that prison inmates are heavily selected on the basis of poor living conditions prior to the prison sentence, which renders the ‘true’ effect of imprisonment difficult to assess. This study investigates what effects a first prison sentence has on labour market attachment, both generally, by comparing those sentenced to prison to the population as a whole, and specifically, by comparing groups of convicted offenders. We utilize longitudinal data for two complete birth cohorts of Swedish men born in 1975 and 1980 ($N = 107,337$). These data enable us to compare the labour market attachment of prison inmates both before and after their time in prison. In a first step, we compare those sentenced to prison with those sentenced to probation. In a second step, to optimize the level of comparability, we limit this comparison to those sentenced to an intermediate term of imprisonment. Finally, to determine whether the effects of imprisonment on employment are conditional on pre-sanction labour market attachment, we also perform separate analyses for those with and without labour market income during the year prior to their sentence.

Prison Sentences and Individual Life Chances

The research literature on the (usually negative) effects of prison sentences at the individual level is vast. Besides looking at labour market effects, it has addressed outcomes such as education, social relations and ill health (for a recent review, see Wakefield and Apel 2016). Nonetheless, Loeffler has argued that ‘despite the high level of research interest, no clear consensus has emerged on the magnitude of imprisonment’s effect on the life course’ (Loeffler 2013: 138).

Why then, and how, might we expect imprisonment to affect inmates' future chances on the labour market? Theoretically, reasonable explanations may be posited for imprisonment having no effect or both negative and even positive effects on post-release labour market attachment.

One important explanation as to why imprisonment might be expected to have negative consequences for future employment is the *stigmatization* that results from criminality and official sanctions (Pager 2003). According to classical labelling theory, the reactions of society may obstruct the termination of a criminal career, in part because the offender internalizes the negative expectations of others, and also because alternative and conventional life choices may become unavailable as a result of the associated stigma. Employers, e.g., may regard a previous prison sentence as an indication of unreliable characteristics or future problems (Backman 2012).

The fact that incarceration cuts individuals off from contacts with the labour market may also be part of the problem. Apel and Sweeten (2010) thus suggest that the negative impact of incarceration on earnings is indirect, and that it is due to a lack of work experience rather than to exclusion based on the stigma associated with a criminal record. In line with cumulative disadvantage theory, this means that it is resource deficiencies, rather than the criminal record itself, that constitute the core mechanism for understanding why criminal involvement may lead to weak labour market attachment. Cumulative disadvantage can be described as a process in which each instance of resource deficiency or disadvantage leads to additional consequences, thus producing growing inequalities between cohort sub-groups over time (see also DiPrete and Eirich 2006). According to Sampson and Laub (1997), crime and society's reactions to crime, and in particular custodial sanctions, serve to intensify an unfavourable career—characterized by unemployment and weak bonds to conventional society—which in turn increases the risk of continued criminality and further social marginalization.

However, according to the theory of disadvantage saturation, the level of disadvantage may reach a point of saturation at which the individual has little left to lose in terms of resources and future life chances. Thus it is reasonable to assume that there is a level of disadvantage at which sanctions become less consequential, simply because the conditions that influence the life chances of these individuals cannot become much worse (Hannon 2003). To the extent that society sentences people to prison who are already in a disadvantaged position in terms of their labour market prospects, we would therefore not expect imprisonment *per se* to lead to a significant further deterioration in these labour market prospects. This would imply that the effect of imprisonment on labour market attachment is conditional on the individual's labour market attachment prior to imprisonment.

A third possible outcome is that imprisonment has a positive effect on labour market attachment. Since those sentenced to prison constitute a disadvantaged group, society could grasp the opportunity offered by incarceration to compensate for these resource deficiencies. In line with the life course theory of Laub and Sampson (2003), and their emphasis on the potential of resource attainment to turn negative life trajectories around, measures which in addition to keeping inmates locked up also offer them education (e.g. finishing high school), vocational training or treatment for addiction, may provide opportunities for prison to function as a positive turning point (see also Bhuller *et al.* 2016). In line with this view, the head of the Swedish Prison and Probation Service (Nils Öberg) argued in an op-ed in the Swedish daily newspaper 'Dagens Nyheter' that

‘For us to be able to provide constructive measures – treatment, support, influence and control – during detention requires not only resources and competence, but also time. A couple of weeks of incarceration make no difference with respect to inmates’ willingness or ability to leave a criminal career’ (DNdebatt 2016; authors’ translation; see also [The Guardian 2014](#)). This statement seems to suggest that the extent to which imprisonment may serve as a positive turning point is to some extent conditional on the length of the prison term.¹

Previous research on the labour market attachment of prison inmates

Employment rates among former prisoners vary a great deal across different studies (see [Loeffler 2013](#), for a review). To a large extent this is due to the fact that the social composition of the prison population varies across countries ([Aaltonen et al. 2016](#)), and that the operationalization of labour market attachment differs across studies. However, in general the literature shows that former prisoners have weak labour market attachment several years after release (see, e.g. [Western 2002](#); [Apel and Sweeten 2010](#)). As was recently pointed out by both [Ramakers et al. \(2014\)](#) and [Aaltonen et al. \(2016\)](#), most of these studies deal with the situation in the United States. Given the fact that different countries’ punitive, social and labour market policies differ, and that this in turn may be expected to impact on the labour market prospects of prison inmates, this is of course a weakness in this field of research.

Over recent years, Scandinavian researchers have increasingly utilized the rich administrative register databases available in these countries to scrutinize the effect of imprisonment on employment. For example, [Tranæs and Geerdsen \(2008\)](#) analysed income maintenance among non-recidivists five years after their first term of imprisonment. They found that those sentenced to prison had weak attachment to the labour market even before the prison sentence. They also showed that this process of marginalization escalates immediately prior to the sentence and deteriorates further during follow-up. This pattern was particularly salient for those sentenced to longer prison terms (>6 months), a pattern which has also been found in Dutch data ([Ramakers et al. 2014](#)). A negative effect was also identified with respect to incomes: as compared to non-convicts of same age, prison inmates lost 40 per cent of their wages between the four-year period prior to and the four-year period subsequent to the prison term. Studies from both Norway ([Skardhamar and Telle 2012](#)) and Finland ([Aaltonen 2015](#)) have confirmed that ex-inmates have very weak labour market attachment four years after release (see also [Aaltonen et al. 2016](#)).

Despite the observed deterioration of labour market attachment identified in these studies, the question remains as to what extent this is an effect of imprisonment *per se* or whether it is rather an effect of the selection of people suffering from serious resource deficiencies into prison. Thus [Loeffler \(2013\)](#) concluded that one problem associated with this research literature is that many previous studies have compared prison inmates to a population that was never at risk of being sentenced to prison. The

¹It should however be noted that Öberg does not argue for replacing short prison terms with longer periods of incarceration, but rather with longer periods of probation in combination with well-specified rehabilitation interventions.

optimal research design would of course be to randomly assign people to prison. Since this is not possible, researchers need to find natural experiments or use other techniques to account for selection as far as is possible.

Loeffler (2013), e.g., used the variation in the severity of sanctions imposed by judges in Chicago and argued that the most valid comparison is that between those sentenced to prison and those who ‘were sentenced to the nearest alternative disposition (i.e. probation)’ (Loeffler 2013: 143). The first step of Loeffler’s analyses showed, as expected, that imprisonment was linked to weak labour market attachment five years after release. However, the controlled comparison showed that those sentenced to prison did not have significantly weaker labour market attachment than those sentenced to probation.

Andersen (2015) utilized a crime policy reform in Denmark which implemented an alternative sanction to imprisonment (community service) as a way of isolating the effect of imprisonment. By comparing persons convicted before and after the reform using difference-in-difference techniques, she showed that non-custodial sanctions improved the income maintenance situation as compared to imprisonment (see also Larsen 2017). Incarceration reduced chances in the labour market up to five years after release. In another Scandinavian study, Bhuller *et al.* (2016) used Norwegian administrative register data on criminal convictions, sanctions and labour market outcomes. These authors used what they termed ‘judged stringency’ (the average incarceration rate in the cases handled by a given judge) as an instrumental variable to control for selection bias. The results from the study indicated that incarceration significantly increased the post-release employment rate. This effect was driven by inmates who were unemployed prior to incarceration. Among those who were employed prior to the conviction, incarceration instead had a negative effect: there was a drop in employment due to incarceration and this effect lasted over a follow-up period of five years. However, taken together, the effect of incarceration was positive and the authors interpreted this as being a result of the rehabilitative qualities of Norwegian prisons.² Heterogeneity in the effects of imprisonment was also indicated by Verbruggen (2016), who followed a Dutch sample of disadvantaged youths and used fixed effects models to estimate the effects of conviction and incarceration on the likelihood of employment. When employment history before conviction was taken into account, there were no effects of conviction or incarceration on the employment probability for men. For women, however, there was a negative effect of being convicted, but still no additional effect of incarceration.

Apel and Sweeten (2010) used propensity score matching and fixed effects modelling to account for selection bias. Their aim was to control for differences between those sentenced to prison and those sentenced to other sanctions with respect to factors that jointly determine incarceration and labour market attachment. Their longitudinal data (the National Longitudinal Survey of Youth) made it possible to look at labour market attachment over a fairly long follow-up period (6 years). The authors summarized their main result as showing ‘that incarceration upon first-time conviction significantly reduces the probability of employment relative to individuals who were also convicted for the first time but were not incarcerated’ (Apel and Sweeten 2010: 468).

²As already mentioned, prison terms in Scandinavia are often short whereas rehabilitation measures often take time. In the Norwegian study, the length of the prison term is not taken into account. Thus there is no way to evaluate whether the observed effect is conditioned by time in prison.

To conclude, there are strong reasons, from both a theoretical and a policy perspective, to further scrutinize the relationship between imprisonment and post-release labour market attachment. As has been pointed out by among others [Ramakers *et al.* \(2015: 66\)](#), most effect studies outside the United States have lacked access to reliable data on living conditions among prison inmates prior to conviction (see [Tranæs and Geerdsen 2008](#); [Andersen 2015](#); [Bhuller *et al.* 2016](#) for exceptions), which renders any firm conclusions about effects on post-release labour market attachment difficult. [Loeffler \(2013\)](#) notes that the literature on this topic is more disparate than is usually assumed to be the case and emphasizes the importance of making comparisons between individuals who were sentenced to prison and those who were sentenced to probation. Moreover, there appears to be a shortage of studies outside the United States, which take into account the effect of the length of prison terms. Finally, the recent findings based on Norwegian data indicating that the effect of imprisonment on labour market attachment is conditional on previous labour market attachment ([Bhuller *et al.* 2016](#)) suggest a need to further examine this issue.

In this study we approach this research question and some of the knowledge gaps found in the literature by (1) employing large-scale data covering complete birth cohorts and comparing prison inmates to those sentenced to probation; (2) employing an observation window covering several years both before and after the first prison sentence; (3) having access to rich information on childhood, adolescent and adult living conditions and criminal convictions before the first period of imprisonment; (4) investigating the labour market situation among ex-inmates in a welfare state with a low incarceration rate and mainly short prison sentences; (5) stratifying the analyses by labour market attachment prior to the sentence.

Data and Methods

Our data have a cohort design and include all individuals born in 1975 and 1980 who were residents in Sweden at age 16 ($N = 2,10,000$). The data we have on crime and punishment have been collected from the convictions register. Swedish convictions data have a relatively high degree of coverage in relation to crimes committed. This is because Swedish police and prosecutors are bound by the legality principle, which means that, as a rule, the authorities must arrest or prosecute whenever they believe an offence has occurred. Moreover, the Swedish police do not have a sanctioning mandate other than to issue fines for minor traffic offences such as speeding. By definition, these minor offences are thus not included in convictions data ([von Hofer and Lappi-Seppälä 2014](#)). By the age of 30 years, 28.9 per cent of the men and 9.6 per cent of the women in the two cohorts had at least one criminal conviction.³ These convictions include a number of different sanctions, ranging from waivers of prosecution and summary sanction orders to prison. In both cohorts, around 3.7 per cent of the men and 0.2 per cent of the women had been sentenced to prison by the age of 30. Prison sentences below the age

³As a comparison (although definitions and populations differ in various ways), an English study using data from the Offenders Index followed the 1953 birth cohort and found that 33% of men and 9% of women had been convicted of at least one 'standard list' offence by the age of 46 (Prime *et al.* 2001).

of 20 are very rare. This is because youths aged between 15 (the age of criminal responsibility in Sweden) and 17 can be sentenced to prison only when there is 'exceptional cause' for doing so. Those aged 18–20 can only be sentenced to prison when there is a 'special cause' (Shannon *et al.* 2014).

By linking various administrative registers (using the personal identification numbers assigned to all persons with a residence permit), we can account for various circumstances during childhood and adolescence, including convictions prior to the first prison sentence. In addition to these data, we also have access to demographic background variables such as sex and immigrant background and information on resources during childhood relating to the family's financial resources measured as the family's total post-tax and transfer annual income and the parents' social welfare receipts. We have also information on the cohort members' final grades from compulsory education (age 15–16) and data on diagnoses from the hospital discharge register. Data from the register relate to the period up to and including 2010 (see Bäckman *et al.* 2014). The fact that we utilize administrative register data means that we lack information on both non-registered crime and incomes from the informal economy. On the other hand, non-response rates or memory failures, which usually constitute a major challenge in relation to the study's target population, are of no concern in the current context.

The convictions register provides information on the date of the conviction and sanctions imposed. We do not have the exact dates for the start of a prison sentence or for release from prison; instead, we use the date of the conviction and the number of prison days in the sentence, taking into account the rule of release after two-thirds of the sentence as an approximation.⁴ We have tackled this limitation in three ways. (1) An outcome measure, i.e. less sensitive to this inexactness; (2) a long follow-up period, which starts the year after the prison sentence; (3) sensitivity analyses that take into account the time of the sentence and recidivism during follow-up respectively. We elaborate these points further below. A further limitation is that we lack detailed information on the prison sentence itself, e.g. on possible treatment measures received in prison. In Sweden, a prison sentence of up to 6 months may, in certain circumstances, be served in the form of electronic monitoring (EM; Lindström and Leijonram 2008). The proportion of new inmates who have served their sentence in this way has varied somewhat over time, but lay at around 20 per cent at the beginning of the 2000s (KOS 2004). In our analysis, we have, therefore, separated those with prison terms of less than 6 months from those serving a longer term (given the rule specifying release after two-thirds of the sentence, this means 4 months in prison), and have focused on the group of inmates serving an intermediate term in prison (i.e., 4–10 months).

The population included in the matching analyses below is restricted to those sentenced to either prison or probation for the first time at age 20–25 (1995–2000/2000–2005). This 'treatment window' was chosen on the premise that we need both an observation period before the first sentence and a follow-up period (5 years) that allows for an analysis of the medium-term effects of a first prison sentence. Due to the small number of women sentenced to prison in the two cohorts, we have restricted our analyses to men.

⁴In Sweden, prison inmates sentenced to more than one month are routinely released after having served two-thirds of their sentence.

Figure 1 illustrates the way in which we selected the analytical sample. As is shown in the graph, we identified 1,133 men who received their first prison sentence and 3,395 who were sentenced to their first term of probation within the specified age span. The average age at sentence was 21.6 years and the average time spent in prison (among those sentenced to prison) was 4.4 months (range: 1–80 months), taking into account the routine release of Swedish inmates on probation once two-thirds of the sentence has been served. The time served by Swedish prisoners is compatible with that served in other Scandinavian countries and also in the Netherlands, e.g., but is of course much shorter than that in the United States, where state prisoners serve on average two years (Ramakers *et al.* 2014; Aaltonen *et al.* 2016; for a description of the Swedish prison system see Lindström and Leijonram 2008).

Outcome and control variables

The outcome employed in the analyses below is annual labour market income above zero, 5 years subsequent to conviction. The labour market income concept covers wages and other incomes related to employment, such as sick leave benefits and parental leave benefits, but not income sources such as unemployment benefits, disability pensions and student allowances. Thus the measure indicates any labour market attachment and does not distinguish between degrees of labour market attachment. The rationale for this operationalization is two-fold: (1) for a population stratum with this level of deprivation, having or not having a formal labour market income constitutes a fundamental demarcation line, (2) since we do not have the exact start and end dates for the prison sentence, a less sensitive measure than one based on, e.g. annual income is preferable, since it is less likely to be biased by this.

In order to account for selection into the group sentenced to prison, the analyses below include a number of demographic and socio-economic factors from childhood and adolescence that are known to affect on the risk for social exclusion and criminality (see e.g. Bäckman and Nilsson 2011). We also account for the type of crime in the sentence. In addition, we control for the existence of labour market income during each of the four years prior to sentencing. Descriptives for all factors can be found in Tables A1 and A2.

Analytical strategy

In the first step of the analyses, we describe the development of labour market attachment and income in four groups in the birth cohort at ages 20–30. We compare the prison population to non-convicted individuals, to those who were subject to probation, and to those who were subject to other forms of sanctions. Thus in this descriptive first step, rather than searching for comparable control groups in order to obtain causal estimates, we simply compare the development of labour market attachment and income within the cohort. Many first-time prisoners are relatively young and at an early stage of their employment careers. Thus, labour market attachment and income are expected to improve due to the fact that many may still have been in education or in a process of labour market entry during the period prior to imprisonment. Consequently, a life-course transition from education to employment might cause a

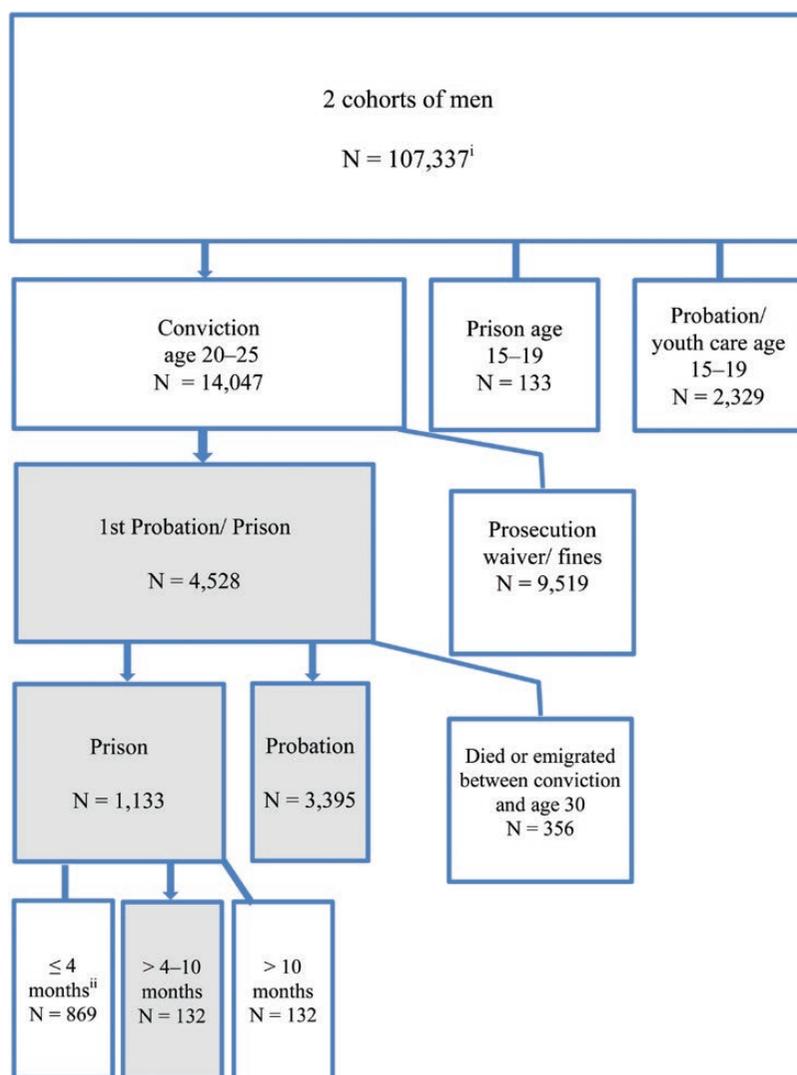


FIG. 1 Selection of sample. Grey shading indicates the analytical sample. ⁱ Alive at age 20 and have not emigrated prior to this age. ⁱⁱ Time served.

within-individual increase in employment and income even though prison may have had a negative effect on employment.

In the next step we aim to isolate the effect of imprisonment on labour market attachment. For this purpose we employ propensity score matching (PSM). Since a random control design is not possible, we instead use PSM to produce comparable groups of imprisoned persons and others. With PSM, we estimate the probability (propensity score) of imprisonment at age 20–25 by means of logistic regression including a range of observed characteristics (see above). PSM is a counter-factual approach in which we ask the question what would have happened to a group subject to some form of treatment if they had not been treated. The propensity score obtained is then used to match

the treatment group (i.e., imprisoned) with an ‘untreated’ control group with a similar estimated ‘propensity’ for imprisonment, but who were not sentenced to prison. The treated and the untreated are thus chosen on the basis of the estimated probability of being members of the treatment group.

After having tested a number of matching procedures, including variations in the number of matches in nearest neighbour matching, 1:3 nearest neighbour matching, in which every individual in the treatment group is matched to three controls, rendered the best covariate balance (see below).⁵

The output from the PSM, i.e. of most interest in the present study, is the average effect of treatment on the treated (ATT), which is the outcome difference between the matched treatment and non-treatment groups (see [Becker and Ichino 2002](#)). In the analyses below we report the ATTs for years 1–5, where year 0 is the year of the sentence, and also the corresponding difference between the treated and untreated groups during the four years prior to the sentence.

The interpretation of the ATT as the ‘true’ effect of treatment on the treated holds only if both the assumption of covariate balance and the conditional independence assumption (CIA) are not violated. The covariate balance assumption can be checked by inspecting balance tables reporting the standardized difference between treated and controls on each observed covariate after matching. A standardized bias of less than ten per cent is considered as balance ([Austin 2011](#)). The CIA implies that there is no selection on unobservables. To check whether this assumption holds, we adopt a technique suggested by [Ichino *et al.* \(2008\)](#); see also [Nannicini 2007](#)). With this technique we simulate confounders to assess the magnitude of selection needed to drive the ATT to zero. The average ATT and odds ratios for treatment and outcome generated by this procedure constitute the basis from which we can assess the likelihood of the existence of such a variable or set of variables.

In order to assess a ‘true’ effect of imprisonment on any outcome, we need to compare the imprisoned not only to a similar group in general, but also to a group which realistically could have ended up in prison. In the PSM we have therefore only included those who were sentenced to prison or sentenced to probation. We have also excluded those convicted and sentenced to either of these sanctions prior to the age of 20, see [Figure 1](#).

However, for courts, the range of possible sanctions for a specific crime is restricted. The option of choosing between probation and a long term of imprisonment is rarely available. For those sentenced to prison for at most six months, it is in certain cases possible to serve this sentence in the form of EM (see above). In a second step we have therefore chosen to restrict our comparison to those who were sentenced to probation and those who were sentenced to a prison sentence longer than 6 months and shorter than 15 months (i.e., with a period of imprisonment of between four and ten months, when the rule of release after two-thirds of the sentence is taken into account).

We also run separate models for those with and those without labour market income in year –1, in order to investigate whether or not the results are conditional on previous labour market attachment as was recently shown to be the case in Norway ([Bhuller *et al.* 2016](#)).

⁵The `psmatch2` module in STATA was employed to perform PSM (Leuven and Sianesi 2003).

We have right-censored for death and emigration (Aaltonen *et al.* 2016). Thus, all cohort members do not contribute equally to the denominator in all years.

Finally, the robustness of the results has been checked in different ways. Firstly, since our data do not allow us to identify the exact dates spent in prison, the magnitude of this ‘street-time effect’ has been assessed for the follow-up period. Secondly, the assumptions underlying our interpretation of the PSM results as ‘true effects’ have also been tested.

Results

To illustrate the difference in labour market careers between those sentenced to prison, those sentenced to probation, those convicted and given other types of sanctions and the non-convicted, Figure 2a and b present labour market attachment and income for these four groups among men born in 1975 and 1980, at age 20–30.⁶ Labour market incomes increase over time in all four sub-groups. However, there is a clear stratification with respect to the different sanctioning forms. The most positive development is found for the non-convicted and the least positive among those who were ever sentenced to prison within this age range, and these differences increase as the cohorts grow older. For those sentenced at least once to a less severe sanction, we note a development, i.e. close to that of the non-convicted. Those sentenced to probation fare better than the imprisoned but worse than the other two groups. The pattern shown in Figure 2b reveals less dramatic increases, but the pattern whereby those sentenced to prison lag behind the others is repeated. Note, however, that even among those sentenced to prison, a majority have a labour market income. Still, these crude comparisons allow us to conclude that those sentenced to prison do indeed have poorer

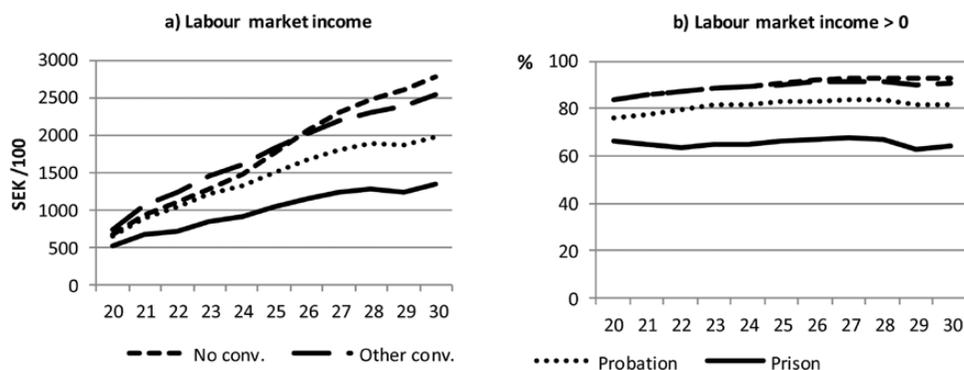


FIG. 2 Labour market income (2010 prices) and proportion with labour market income > 0 among men at age 20–30. Non-convicted, sentenced at least once, sentenced to probation at least once and sentenced to prison at least once at age 20–30. Swedish residents at age 20.

⁶When analysed separately the patterns are virtually identical in the two birth cohorts (not shown).

prospects on the labour market, but to what extent can these differences be attributed to the prison sentence *per se*? To address this question, we turn to our propensity score analyses.

PSM I: probation vs prison

We begin by trying to assess the effect of imprisonment, irrespective of length of the prison term. The outcome in the PSM is annual labour market income above zero during the year of the sentence and the following five years.

As was mentioned in the methodology section, although we match on the propensity score we need to ensure that the sample of treated and untreated individuals is balanced with respect to the matching variables from the logit model. [Table A1](#) shows both the log-odds obtained from the logit model and the % bias in the matching variables. The rule of thumb is that the % bias after matching should not exceed ten ([Austin 2011](#)). With one exception, no such values remain after matching. However, in this case (other crime) matching overcompensates and transforms the prevalence from being significantly higher to significantly lower in the control group. In most instances the % bias is already insignificant in the unmatched sample. As with the logit model, this is most likely due to the sample selection.

[Figure 3](#) shows the difference in the prevalence of labour market income for those sentenced to prison and those sentenced to probation (matched sample). Year 0 indicates the year of the first such sentence. There is a post-sentence gap indicating a negative effect of imprisonment. This gap narrows over time and is no longer significant at the 5% level in years 4 and 5. Thus, it seems that being sentenced to prison rather than probation has a negative effect on labour market income in the short run but less so in the long term.

PSM II: probation vs intermediate prison term

In the next analysis we turn to the comparison between those with probation and those with an intermediate prison term. [Table A2](#) shows the log-odds and balance diagnostics for this analysis.

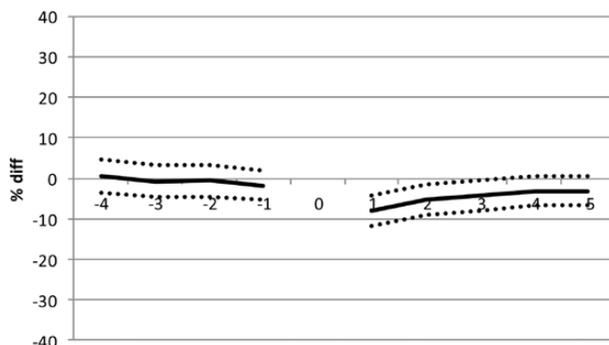


FIG. 3 Difference in prevalence (per cent) of labour market income between individuals sentenced to probation and to prison (solid line), four years before and five years after sentence. Swedish men at age 20–25 at sentencing. Matched sample. 95 % Confidence intervals (dotted lines).

Figure 4 shows the results after matching. As in the former analysis, we find differences between prison group and controls during the years following the sentence, but these are now more conspicuous. During the first three years of follow-up (the observations for which we find significant differences), an average of 60 per cent of the prison group and 74 per cent of the controls have some level of income from work, i.e. a 14 percentage point difference

PSM III: are the effects conditional on previous labour market attachment?

To analyse whether the effect of imprisonment on earnings is conditional on previous labour market attachment, we have stratified our sample on the basis of the prevalence of labour market income during the year prior to the prison/probation sentence (year $t-1$). The results from these analyses are shown in Figure 5a and b.

Naturally, the effect shown in Figure 4 is only valid for those with a labour market income in year $t-1$ (Figure 5a). For those with no income that year, imprisonment does not seem to lead to any further deterioration in their already unfavourable labour market position (Figure 5b). The difference in the prevalence of labour market attachment is between 15 and 20 percentage point during years 1–3 after the prison sentence. This effect becomes weaker in year 4 and ceases to be significant in year 5.

Thus, these results are in line with those of Bhuller *et al.* (2016) to the extent that the negative effect of imprisonment is driven by those with a pre-existing labour market attachment, i.e. disrupted by the prison sentence. However, we find no positive effect of imprisonment for those with no prior labour market attachment in the Swedish context.

Robustness of results: street-time

So far we have not taken into account the fact that inmates are not available to the labour market during the time spent in prison. Moreover, during the five-year follow-up, a larger proportion of those sentenced to prison than of those sentenced to

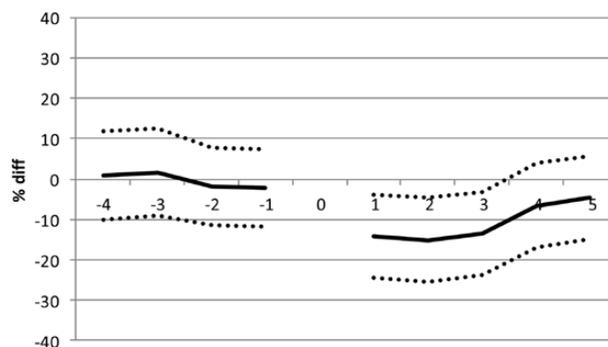


FIG. 4 Difference in prevalence of labour market income between sentenced to probation and to intermediate prison term (time in prison >4 months ≤ 10 months; solid line), four years before and five years after sentence. Swedish men at age 20–25 at sentence. Matched sample. 95 % Confidence intervals (dotted lines).

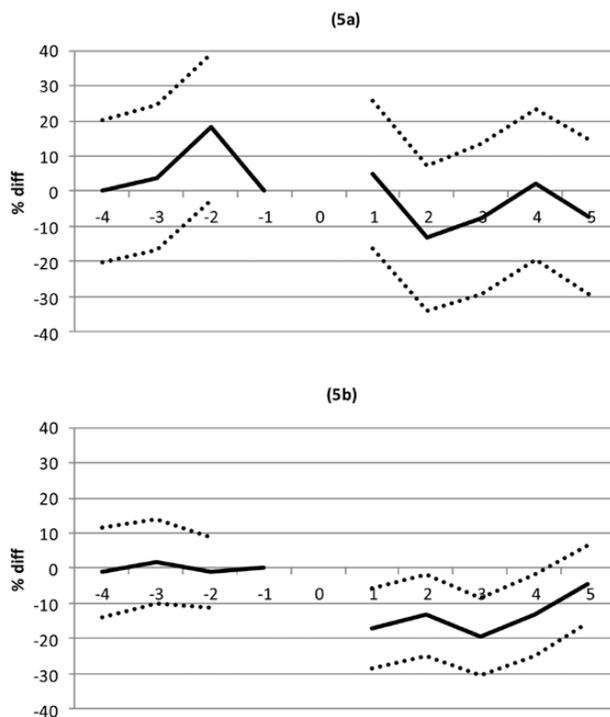


FIG. 5 Difference in prevalence of labour market income (solid line), four years before and five years after sentence to probation or intermediate prison term among those without (5a) and with (5b) labour market income in the year before sentence ($t-1$). Matched sample. 95 % Confidence intervals (dotted lines).

probation (control) had recidivated and received a new prison sentence (22.1 per cent as compared to 15.2 per cent). Thus, time in prison may be an important mediating variable in the imprisonment–labour market attachment link. Although our data do not allow us to identify the exact dates spent in prison, the magnitude of this ‘street-time effect’ should be assessed. We have approached this issue in two ways. Firstly, we re-weighted the data in follow-up years 1–4 according to the amount of time served in prison t (years 0 and 5 were excluded since the effect of the uncertainty regarding the actual start and end dates is likely to be greatest at the start and at the end of the follow-up). On this basis, we found that as much as approximately two-thirds of the difference between the probation and the prison group can be attributed to the fact that the treated spent more time in prison during the follow-up period than the untreated (not shown).

Secondly, we also re-ran the analyses after having excluded all recidivists with a new prison sentence during follow-up. The reductions in the effects noted in these analyses were of similar magnitudes to those attained by re-weighting.

To conclude, a significant proportion of the estimated effect of imprisonment on post-release labour market attachment is mediated by the higher incarceration rates among ex-inmates as compared to those whose first sentence was probation.

Robustness of results: PSM assumptions

As indicated in the methodology section, the interpretation of the results from the PSM analysis as ‘true effects’ holds only if the assumption of covariate balance and the CIA are not violated. The balance tables in the Appendix show clearly that balance was reached by means of the initial logit regressions. No standardized bias exceeds five after matching (except for the over-compensation referred to above). Many covariates were balanced even before matching, since the analytical samples were strongly selected at the outset. However, when assessing balance in continuous variables—both manifest and latent—it is not only mean values and prevalence that need to be similar between the exposed and non-exposed group in the matched sub-samples, but rather the entire distribution. Therefore, the balance of the distribution of the underlying income measures was tested by means of so-called quantile–quantile plots (Austin 2009). These show that balance was also achieved for the distribution of these variables (not shown, available upon request).

To assess the robustness of the results with respect to the CIA, we performed a number of analyses in which we simulated confounders with different degrees of selection effects in order to assess the degree of selection needed to drive the observed ATT to zero (Nannicini 2007; Ichino *et al.* 2008). In summary, these simulations show that in order to turn the PSM results presented above to null-effects, unrealistic confounder characteristics are required (not shown, available upon request).

Conclusions

In this article, we have analysed the effect of a first prison sentence on labour market attachment up to five years after release from prison. Previous research on the labour market consequences of imprisonment has pointed in different directions; there are studies showing negative (e.g. Apel and Sweeten 2010; Andersen 2015), non-existent (e.g. Loeffler 2013; Ramakers *et al.* 2015), and even positive (Bhuller *et al.* 2016) effects. Although it is possible to find research supporting each of these scenarios, there is a tendency in the literature that the better the control for selection bias, the weaker the support for an independent effect of incarceration on post-release labour market outcomes (Loeffler 2013). The present study fits well with this pattern. However, when the analysis is conducted for different groups on the basis of labour market attachment prior to conviction, we find negative effects of imprisonment for those who had some form of labour market attachment before being imprisoned.

When we look at the development of labour market incomes in the age range 20–30 we find, in line with previous observational studies, substantial differences between those sentenced to prison, those sentenced to probation, those sentenced to other types of sanctions and the non-convicted. As we would expect when analysing this particular period in the life cycle, we see that labour market incomes increase with age in all four sub-groups, but it is clear that the most positive development is found for those with no convictions and the least positive for those who were sentenced to prison at least once. Moreover, these differences increase as the cohorts grow older. When we look at any labour market attachment (income >0), we find a similar, but less dramatic, pattern. The main question, then, is of course to what extent these differences can be attributed to the prison sentence per se.

Our comparisons of those sentenced to prison and the matched controls indicate that being sentenced to prison rather than to probation has negative, although small, effects on labour market income in the short run, but that there are no significant differences in the longer term. However, in the second matching analysis, in which we compare those sentenced to probation only to those given an intermediate prison term, the effect of imprisonment is more conspicuous.

In the final set of analyses, where we study whether or not the effect of imprisonment on earnings is conditional on previous labour market attachment, we find that the negative effect of imprisonment is driven by those who had a pre-existing labour market attachment that was then disrupted by the prison sentence. However, and contrary to the findings of [Bhuller *et al.* \(2016\)](#), we found no positive effect of imprisonment among those with no prior labour market attachment during the year before imprisonment.

Although it seems likely that the PSM analyses violate neither the assumption of covariate balance nor the CIA, our further complementary analyses seem to suggest that a large proportion of the effects we have noted are mediated by a higher rate of recidivism into prison among those sentenced to prison at onset.

Thus, if anything, the overall picture suggests a worsening employment situation as a result of imprisonment. However, for the most disadvantaged segments of this population, e.g. those with no income during the year before imprisonment, the additional impact of time served does little to alter their labour market situation, which is already characterized by disadvantage prior to incarceration. This is a result that fits well with the theory of disadvantage saturation, i.e. imprisonment has no effect on employment and earnings in this particular group, since their accumulation of resource deficiencies is already significant before the prison term ([Hannon 2003](#); [Ramakers *et al.* 2015](#)). For this group, the policy response in terms of probation or a prison sentence may thus be of little importance in relation to their chances on the labour market. Other types of policies are required to improve the labour market attachment of these individuals.

We have also identified a group of individuals for whom the choice of a probationary or prison sentence is of importance. In line with the idea of cumulative disadvantage, it seems that a prison term may have additional negative consequences for those who do have some kind of attachment to the labour market prior to the prison sentence. For these individuals, being sentenced to prison rather than probation results in a weakening of their bonds to society, which consigns them to a more unfavourable labour market career after release.

However, when it comes to explaining the labour market effects of imprisonment, the high reincarceration rate among ex-inmates is at least as important as weakening bonds, stigma or forfeited opportunities for the accumulation of human capital. Quasi-experimental studies looking at the impact of the introduction of EM schemes show that EM reduces the risk of recidivism as compared to imprisonment (e.g. [Bales *et al.* 2010](#); [Andersen and Telle 2016](#)). Thus, much of the effect we have identified is probably indirect and mediated by new prison terms during which those imprisoned are literally locked out of the labour market.

This finding also hints at the complex processes involved here. Although we have not tried to unpick the reversed causal order, we have emphasized the low level of labour market attachment that already existed prior to the prison sentence. This suggests that we are dealing with a recursive process, which would be very much in line with cumulative disadvantage theory. In addition, it should be noted that our operationalization

of no labour market attachment is very strict and that any labour market income will tip a person over into the labour market attachment category. This means that we do not address the nuances of labour market attachment, such as precarious employment and low pay, aspects which have been shown to be important ingredients of the labour market careers of former inmates (Ramakers *et al.* 2016). Moreover, the question of the ways in which other potential consequences of imprisonment and criminal careers, such as drug use, ill health and poor social relations, tap into this process lies beyond the scope of our analyses. Unpicking the ways in which the links and reciprocal processes at work among these different aspects of marginalization shape the life course of former inmates represents a major challenge of course. A first step for future research might thus be to describe these post-incarceration pathways. However, this would require other types of methodological approaches than those applied here—approaches, such as sequence analysis, which are able to depict various aspects of the life course simultaneously.

Finally, it should be emphasized that in this study we have focused on terms of imprisonment (of 4–10 months) that would be regarded as short sentences in many other countries, not least in the Anglo-Saxon world. This could be seen as a conservative test, and we may assume that it underestimates the effect of some of the mechanisms identified as important for understanding negative outcomes (e.g. time away from the labour market). In addition to our methodological reasons for employing this approach, we would argue that focusing on those with fairly short sentences also represents the most interesting option from a policy perspective, since it is primarily for this group that alternative (non-custodial) sanctions constitute a realistic option. Still, not even an ‘exceptional’ prison system (Pratt 2008) that promotes itself by reference to an ambition to ensure their clients to ‘come better out’ manages to function as a positive turning point.

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Appendix

TABLE A1 Balance scores and odds ratios (OR) from logit regression. Treatment = prison; control = probation. N (treated) = 1,057. 1:3 nearest neighbour matching

Covariate	Range	OR	Sample	Mean		%bias
				Treated	Controls	
Income >0 year-4	0–1	1.01	Unmatched	0.63	0.65	–3.5
			Matched	0.63	0.65	1.1
Income >0 year-3	0–1	0.94	Unmatched	0.68	0.70	–5.2
			Matched	0.68	0.68	–1.8
Income >0 year-2	0–1	0.92	Unmatched	0.70	0.73	–6.3
			Matched	0.70	0.70	–1.4
Income >0 year-1	0–1	0.93	Unmatched	0.73	0.76	–7.3*
			Matched	0.73	0.75	–4.1
Number of convictions before sentence (ref: 0)						
1	0–1	0.93	Unmatched	0.19	0.22	–5.5
			Matched	0.19	0.20	–1.6
2	0–1	1.08	Unmatched	0.12	0.12	–0.6
			Matched	0.12	0.12	–0.1
≥3	0–1	1.74	Unmatched	0.26	0.18	18.7***
			Matched	0.26	0.24	3.9
Age at 1st conviction	15–25	0.98	Unmatched	18.63	19.02	–12.2***
			Matched	18.63	18.65	–0.7
Parental household type at age 16 (ref: two parents)						
Single parent	0–1	1.03	Unmatched	0.36	0.35	1.3
			Matched	0.36	0.36	–0.5
Other	0–1	1.15	Unmatched	0.03	0.03	2.0
			Matched	0.03	0.03	–1.7
Immigrant status (ref: native, native parents)						
Native immigrant parents	0–1	1.22	Unmatched	0.06	0.05	4.6
			Matched	0.06	0.06	0.7
Immigrant age <13	0–1	1.51***	Unmatched	0.18	0.13	15.3***
			Matched	0.18	0.17	3.8
Immigrant age ≥13	0–1	1.09	Unmatched	0.05	0.03	7.0
			Matched	0.05	0.05	–1.4
Years with SA in parents' household age 8–17 (ref: 0) ⁱ						
1–2 years	0–1	0.94	Unmatched	0.15	0.16	–1.5
			Matched	0.15	0.15	–0.3
≥3 years	0–1	1.00	Unmatched	0.33	0.31	4.0
			Matched	0.33	0.33	0.1
Low education in parents (ref: no) ⁱⁱ						
Yes	0–1	1.13	Unmatched	0.23	0.20	8.2*
			Matched	0.23	0.23	1.0
Final grades compulsory school (ref: high) ⁱⁱⁱ						
Low	0–1	1.09	Unmatched	0.61	0.60	2.3
			Matched	0.61	0.61	–0.1
Medium	0–1	1.11	Unmatched	0.36	0.37	–1.3
			Matched	0.36	0.36	–0.1
Psychological diagnosis age 15–20 (ref: no) ^{iv}						
Yes	0–1	0.60*	Unmatched	0.03	0.04	–6.3
			Matched	0.03	0.03	–0.2
High school (ref: dropout)						
Never started	0–1	0.96	Unmatched	0.02	0.02	0.8
			Matched	0.02	0.01	5.1
Graduated	0–1	0.90	Unmatched	0.58	0.62	–7.6*
			Matched	0.58	0.60	–2.6
Birth year (ref: 1980)						
1975	0–1	1.51***	Unmatched	0.53	0.48	9.8**
			Matched	0.53	0.54	–1.3

TABLE A1 *Continued*

Covariate	Range	OR	Sample	Mean		%bias
				Treated	Controls	
Crime type in sentence (ref: theft)						
Other crime	0-1	9.22***	Unmatched	0.45	0.28	37.1***
			Matched	0.45	0.50	-10.5*
Threat/Violence	0-1	5.28***	Unmatched	0.37	0.36	2.0
			Matched	0.37	0.34	6.5
Drug crime	0-1	12.57***	Unmatched	0.10	0.04	23.5***
			Matched	0.10	0.08	6.7

*p < 0.05; ** p < 0.01; ***p < 0.001

ⁱsocial assistance benefits.

ⁱⁱboth parents compulsory school (9 years) only.

ⁱⁱⁱLow: 1st quintile; Medium: 2nd-4th quintile; High: 5th quintile.

^{iv}In-patient hospital care; including self-destructive behaviour.

TABLE A2 *Balance scores and odds ratios (OR) from logit regression. Treatment = prison <4 months ≤10; control = probation. N (treated = 122). 1:3 nearest neighbour matching*

Covariate	Range	OR	Sample	Mean		%bias
				Treated	Controls	
Income >0 year-4	0-1	0.59**	Unmatched	0.46	0.65	-38.3***
			Matched	0.46	0.45	1.7
Income >0 year-3	0-1	0.73	Unmatched	0.59	0.70	-22.7*
			Matched	0.59	0.57	3.4
Income >0 year-2	0-1	1.54	Unmatched	0.72	0.72	-0.6
			Matched	0.72	0.74	-4.3
Income >0 year-1	0-1	0.99	Unmatched	0.71	0.76	-10.9
			Matched	0.71	0.73	-5.0
Number of convictions before sentence (ref: 0)						
1	0-1	2.19	Unmatched	0.22	0.21	1.7
			Matched	0.22	0.25	-6.6
2	0-1	2.34	Unmatched	0.14	0.12	5.9
			Matched	0.14	0.17	-9.7
≥3	0-1	4.65**	Unmatched	0.39	0.19	46.8***
			Matched	0.39	0.37	4.3
Age at 1st conviction	15-25	1.03	Unmatched	17.56	19.00	-48.1***
			Matched	17.56	17.33	7.8
Parental household type at age 16 (ref: two parents)						
Single parent	0-1	0.96	Unmatched	0.39	0.35	6.5
			Matched	0.39	0.40	-4.0
Other	0-1	1.22	Unmatched	0.03	0.03	4.1
			Matched	0.03	0.07	-21.0
Immigrant status (ref: native, native parents)						
Native immigrant	0-1	0.91	Unmatched	0.05	0.05	-1.1
parents			Matched	0.05	0.05	-1.2
Immigrant age <13	0-1	2.31***	Unmatched	0.33	0.13	47.4***
			Matched	0.33	0.34	-4.0
Immigrant age ≥13	0-1	1.37	Unmatched	0.10	0.04	25.1***
			Matched	0.10	0.13	-12.1

TABLE A2 *Continued*

Covariate	Range	OR	Sample	Mean		%bias
				Treated	Controls	
Years with SA in parents' household age 8–17 (ref: 0) ⁱ						
1–2 years	0–1	0.87	Unmatched	0.15	0.16	–3.5
			Matched	0.15	0.16	–4.5
≥3 years	0–1	0.93	Unmatched	0.39	0.32	14.7
			Matched	0.39	0.37	3.4
Low education in parents (ref: no) ⁱⁱ						
Yes	0–1	0.76	Unmatched	0.21	0.20	2.1
			Matched	0.21	0.22	–0.7
Final grades compulsory school (ref: high) ⁱⁱⁱ						
Low	0–1	1.28	Unmatched	0.66	0.60	12.5
			Matched	0.66	0.68	–5.6
Medium	0–1	1.25	Unmatched	0.33	0.37	–9.1
			Matched	0.33	0.31	3.4
Psychological diagnosis age 15–20 (ref: no) ^{iv}						
Yes	0–1	1.54	Unmatched	0.08	0.04	17.4*
			Matched	0.08	0.07	3.4
High school (ref: dropout)						
Never started	0–1	1.52	Unmatched	0.02	0.01	7.0
			Matched	0.02	0.01	7.8
Graduated	0–1	1.04	Unmatched	0.50	0.62	–23.3
			Matched	0.50	0.51	–1.1
Birth year (ref: 1980)						
1975	0–1	0.73	Unmatched	0.32	0.48	–32.4**
			Matched	0.32	0.36	–7.9
Crime type in sentence (ref: theft)						
Other crime	0–1	1.38	Unmatched	0.11	0.27	–40.0***
			Matched	0.11	0.13	–3.5
Threat/Violence	0–1	4.24***	Unmatched	0.55	0.37	36.5***
			Matched	0.55	0.53	3.9
Drug crime	0–1	10.31***	Unmatched	0.22	0.05	51.6***
			Matched	0.22	0.21	2.5

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

ⁱsocial assistance benefits.

ⁱⁱboth parents compulsory school (9 years) only.

ⁱⁱⁱLow: 1st quintile; Medium: 2nd–4th quintile; High: 5th quintile.

^{iv}In-patient hospital care; including self-destructive behaviour.