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Serving Time or Serving the Community? Exploiting a Policy Reform to Assess the Causal Effects of Community Service on Income, Social Benefit Dependency and Recidivism

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Abstract

Objectives   There is a widespread belief among criminologists, judges and the like that criminals are better off serving non-custodial sentences instead of going to prison. However, empirical evidence of the effects of such other types of sentences is scarce. To help fill the gap, this paper assesses the causal effect of community service on post-sentence income, dependency on social benefits, and crime.

Methods   For the empirical analyses I exploit a policy reform that implemented the use of community service as punishment among specific groups of criminals, Danish administrative data, and difference-in-difference matching

Results   The results show that community service participants have higher long-run income levels and lower long-run levels of social benefit dependency compared to offenders who serve custodial sentences. However, while community service lowers recidivism among offenders convicted of violent crime, other traffic offences and misdemeanor, there are no overall effects of community service on crime committed after the serving of a sentence.

Conclusions   Serving a sentence through community services rather than in prison, causally improves offenders' post-sentencing outcomes, particularly with regards to their labor market situation. Through this, the offender contributes not only to himself but also to society, and an increased use of non-custodial sentences is then beneficial on several levels. Importantly, my results apply to the Danish legal system, and may not be immediately applicable to other legal contexts.

Keywords   Community service · Difference-in-difference matching · Labor market outcomes · Recidivism

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Introduction

The immediate purpose of placing offenders in prison is to punish their criminal activity. But it is widely recognized that the punishment does not end with release from jail, as several types of subsequent, informal sanctions trouble the lives of ex-prisoners. Studies show that ex-prisoners experience more unstable intimate relationships (e.g. Lopoo and Western 2005), earn lower wages, and have a weaker labor market affiliation (e.g. Waldfogel 1994; Western et al. 2001). However, these findings may reflect either the selection of certain individuals into a criminal career leading to imprisonment, the actual treatment experienced when spending time in prison or both. If the worse outcomes of ex-prisoners is not merely a selection effect, but also reflect a treatment effect, consisting of e.g. the stigma of having spent time in jail or the human capital loss suffered from being away from the labor market, offenders would fare better if they could avoid prison altogether.

Today, justice systems in most countries have different types of sentences at their disposal, including traditional imprisonment, suspended sentences, electronic monitoring and community service. The use of the non-conventional—or more specifically, the non-custodial—types of sentence is often due primarily to considerations of the increasing cost of keeping people in prison and of prison overcrowding, but as a more or less intentional side effect, offenders then also avoid the potential negative effect of spending time in jail. However, empirical evidence on the actual causal effect of non-custodial sentences is scarce. In two excellent review studies Killias and colleagues show that as of 2006 respectively 2008, only five studies had assessed the causal effect of non-custodial sentences on post-sentencing outcomes (Killias and Villetaz 2008; Villettaz et al. 2006). While some of these studies find positive effects of especially community services compared to traditional imprisonment, the review concludes that overall, there is no evidence of positive outcomes from non-custodial sentences. Recently Killias and colleagues published evidence from two controlled, randomized experiments that assess the causal effect of community service and electronic monitoring on different post-sentencing outcomes (Killias et al. 2010a, b). While the first study (Killias et al. 2010a) finds no difference between community service and traditional custodial sentences, the finding of the second study (Killias et al. 2010b) suggests marginally significant differences between two types of non-custodial sanctions, electronic monitoring and community service, in favor of the former. However, the evidence is still relatively limited, particularly as most of the existing studies rely on rather small samples which may explain the frequent result that the effects found are non-significant. In effect, we need more knowledge on the harmful consequences of serving a custodial versus a non-custodial sentence.

This present study contributes to the existing literature on the causal effect of non-custodial sentences by exploiting a policy reform on the use of community service in Denmark. For this purpose, I analyze full-sample individual-level data using difference-in-difference matching, and analyze the effects of doing community service rather than serving a prison sentence on several short- and long-run post-sentence outcomes, including labor market outcomes and recidivism. The results indicate that overall, community service participants earn more and are less dependent on social benefits in the long run, yet, there is no evidence of lower recidivism rates. Importantly, results vary by offender type, and there is evidence of short run effects for those convicted of drunk driving and violence, particularly with regards to social benefit dependency, where drunk drivers doing community service actually experience increased social benefit dependency and lower income in the short run, compared to drunk drivers sentenced to prison. Also it seems as if drunk drivers drive the non-significant effect of community service on recidivism, as all other offender

types experience either short or medium run negative effects of community service on the probability of reoffending.

Custodial and Non-Custodial Sentences

While larger European countries such as Germany, Switzerland and the UK implemented community service in the 1970s, Denmark was more reluctant to initiate the use of this type of non-custodial sanction. But after a trial period, community service was introduced in January 1992.

Initially, community service was used to replace prison sentences for specific types of crime, such as misdemeanor and less serious types of violent crime; however, in 2000 a reform was passed that allowed judges to also impose community service on drunk drivers and other types of traffic offenders that would otherwise have been sent to prison for up till 12 months. In Denmark, whether or not drunk driving triggers a fine or a prison sentence depends on the alcohol count and the number of previous convictions of drunk driving. In general, alcohol counts of more than 2 always release a prison sentence (of between 10 days and 6 months), but also lower alcohol counts may result in imprisonment, e.g. if the driver is a serial offender. The reform did, however, not target drunk drivers requiring treatment of their alcohol use, as this group would be sentenced to participate in a rehabilitation program both before and after the reform.

The same reform also encouraged the increased use of community service as a replacement for prison sentences for non-traffic and non-drunk driving related crime. Hereby the reform represent a shift in the likelihood of being sentenced to community service for both drunk drivers, traffic offenders and other types of offenders who would otherwise be sentenced to 1 year in prison.[1] Today, community service can replace prison sentences for all types of crimes which would otherwise incur prison sentences of less than 12 months. However, judges are obliged to exercise a certain degree of caution and not to use community service in cases where a non-custodial sentence might offend the public's sense of justice (for instance, in cases of robbery or sexual crime).

In Denmark, community service comprises between 30 and 240 h of work that contributes to society (e.g. in public libraries, kindergartens, community centers, etc.), and while many countries determine the number of hours of community service by the length of the non-custodial sentence it replaces, Danish law does not impose a direct scale of conversion. Instead, the judge decides on the appropriate length of the community service, given the offenders' different characteristics (Lagoni and Kyvsgaard 2008). During the period of community service, the offender lives in his or her normal home, and keeps his or her job. In the following section, I discuss whether and how community service may leave the offender better off than traditional prison sentences.

Why and How Does Imprisonment Matter?

One explicit purpose of imprisonment is to prevent recidivism by rehabilitating and restraining the offender. However, both scholars and practitioners seem to agree that the incarceration also has less attractive consequences that the use of non-custodial sentences

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[1] This is evident from the notes to the new law, given by the then Minister of Justice, Frank Jensen. For the Danish version of the law, see http://webarkiv.ft.dk/?/Samling/19991/lovforslag_oversigtsformat/L41.htm, Sect. 3.3.

such as community service may help to avoid (Schwartz and Skolnick 1962). The theoretical and empirical literature on the negative effects of incarceration identifies several explanations for the harmful effects of incarceration.

Stigmatization

One explanation emphasizes the stigmatization related to imprisonment. Here, a number of studies use experiments to demonstrate significant discrimination against job applicants with criminal records. By varying only the criminal history of otherwise identical applicants, the studies demonstrate employers' reluctance to hire ex-offenders, even for positions that do not require the holder to have a clean record (Schwartz and Skolnick 1962; Cohen and Nisbett 1997; Pager 2003). These findings very well reflect the effects of stigmas, which are defined by Goffman as "blemishes of individual character perceived as weak will, domineering or unnatural passions, treacherous and rigid beliefs, and dishonesty, these being inferred from a known record of, for example, mental disorder, imprisonment, addiction, alcoholism, homosexuality, unemployment, suicidal attempts, and radical political behavior" (Goffman 1963: 3). Thus, in the absence of full knowledge of an individual—in this case, the applicant—the people with whom the individual comes into contact (e.g. potential employers) extrapolate from the obtained knowledge of the criminal record to unobserved individual characteristics. This image may or may not correctly describe the individual, but demonstrates the common perception of ex-offenders that may prolong their informal punishment indefinitely. This stigma of incarceration may also work through the prisoners' acceptance of this deviant image given to them by their social relations (Lemert 1972).

   While offenders who serve both custodial and non-custodial sentences are likely to suffer from the stigma of having a criminal record, society may make a harsher judgment of offenders who have been in jail: Society may perceive a prison stay as a stronger marker of a bad personality than a non-custodial sentence. Interestingly, this perception is partially supported by the advice judges get not to use community service where it may injure the public sense of justice (as mentioned earlier), which is a strong signal that more serious offenders should serve their sentences in prison. As a result, ex-offenders who served non-custodial sentences may experience less stigmatization than ex-offenders who served custodial sentences (Western et al. 2001).

Investments

According to a second explanation incarceration may affect human capital by allowing prison inmates fewer years at the ordinary labor market. This could erode their job skills and restrict their possibilities of acquiring experience (Western et al. 2001; Waldfogel 1994). Such a mechanism may not only apply to labor market experience, but could also affect the offender's possibilities of "investing" in other social relations, such as friendships and marriage, that could have promoted positive outcomes both at the labor market and in other domains (Hagan 1993; Lopoo and Western 2005; Sampson and Laub 1993). Thus, to the extent that ex-offenders have worse outcomes than others, this could resort from their unstable affiliation with normal society which prevents them from making continuous investments in relationships. However, serving a non-custodial sentence will reduce offenders' absence from society, which suggests that ex-offenders who serve a non-custodial sentence may have better outcomes than ex-offenders who serve custodial sentences.

Harmful Effects of Community Service?

But while the literature presents several explanations for the effects of incarceration, we may also speculate as to whether a community service sentence is simply a traditional sentence without the prison element, or whether this type of non-custodial sentence involves other potentially useful or harmful elements that affect post-sentence outcomes. For instance, whereas conventional custodial sentences facilitate a separation between offenders' criminal sphere and their other spheres (e.g. legal work and family life), community service will confuse these spheres, as the offender now serves his or her sentence while at the same time acting as an active member of a family and an active employee. Consequently, the non-criminal sphere is no longer unaffiliated with the offender's criminal life, and this may impair the use of the non-criminal sphere as a lever to encourage desistance after the sentence has been served (see, for example, Sorensen *and* Kyvsgaard 2009). Serving a sentence as community service may then impede desistance from crime among this group of offenders. In addition, one aim of imprisonment is to deter present and future offenders from committing (further) crime (Gorecki 1979; Gibbs 1988), and this deterrence is lacking from, or at least reduced in, community service. As a result, community service participants may not fully realize the implications of their wrongdoings, and consequently be less likely to desist from crime after the sentence (1983; see also Windzio 2006).

Thus, while we may be able to think of several negative consequences of serving a prison sentence that result primarily from impairment of the offender's reputation and social and human capital, but which may also result from other mechanisms such as bad peer relations, community service may also be far from ideal and cause negative outcomes by affecting individual processes of desistance. Hereby, we may expect negative outcomes for both offenders sentenced to imprisonment and offenders sentenced to community service; however, the common perception is that imprisonment leaves the offender worse off than community service. To identify the causal effect of participating in community service on various labor market outcomes and on recidivism, I exploit the implementation of the community service scheme in Denmark, as described below. Importantly though, my study is unable to empirically distinguish between the different mechanisms presented above, however, knowledge on them still provide a useful framework for understanding the empirical evidence that I present in later sections.

Identification Strategy

Despite the introduction of community service in 1992, Danish judges displayed an initial and protracted reluctance to use this non-custodial sentence. However, with the reform in 2000 that mainly made drunk drivers and other traffic offenders eligible for community service, but which also clearly specified the Minister of Justice's wish for a general increase in the use of this non-custodial sentence, the use of community service accelerated. The reform effect is particularly evident when we look at the punishment of offenders of misdemeanor, simple violence, drunk driving and other traffic offenses. Figure 1 shows the use of community service between 1990 and 2009. The figure demonstrates a doubling between 1999 and 2001 in the number of misdemeanor and violent crime offenders who were sentenced to community service. It also shows how the number of drunk drivers and other traffic offenders who received a community service sentence rose from zero to 1,500 and 500 per year respectively over the same period, which was a direct effect of the reform.
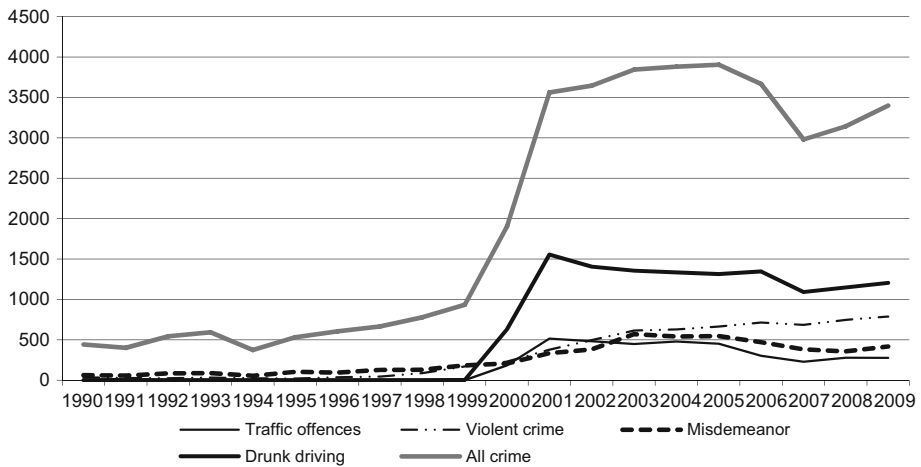
Fig. 1 The use of community service, different types of offenses, 1990–2009. *Source* Own calculations based on data from Statistics Denmark

Today, approximately 2/3 of all offenders who commit crimes that would trigger prison sentences of up to 12 months are actually imprisoned, and 1/3 are sentenced to doing community service. Obviously allocation to one or the other type of sentence does not happen at random: Allocation into community service relies on the judge's assessment of the offender's suitability for this type of sentence. This implies that differences after completion of sentences between the offenders who are sent to jail and offenders who are sentenced to community service are likely to reflect the initial differences that determine their type of sentence rather than a causal effect of how they served their sentence. If, for instance, offenders sentenced to community service are more likely to have a job at the time of the conviction—since having a job may make the judge assume that the offender is more likely to complete the community service—this characteristic is also likely to increase the probability that offenders in this group will still hold a job after finishing the sentence. This would then cause differences in, for example, the post-sentence income and unemployment of imprisoned offenders and offenders doing community service that would only be spuriously related to the type of sentence. Rather, such differences would reflect the initial selection criterion used by the judge. As a result, one cannot arrive at a causal estimate of the effect of community service compared to traditional imprisonment just by comparing the outcomes for the two groups, as the allocation of offenders into sentence types is endogenously related to the different outcomes. However, with the right empirical strategy, the observed changes in the use of community service shown in Fig. 1 can be used to facilitate causal inference.

Instrumental Variables Approach or Difference-In-Difference Estimation?

As is evident from Fig. 1, an offender's probability of being assigned to community service rather than traditional imprisonment is strongly correlated with year of conviction. Thus even if type of sentencing is correlated with individual characteristics across these years, the dramatic increase in the use of community service from 1999 to 2001, will push a relatively large group of offenders—that is, those at the margin—from traditional

imprisonment into community service. This shift represents an exogenous change in the probability of being sentenced to community service that reflects the reform, and the shift is uncorrelated with the individual characteristics of the offenders at the margin. If we assume that average offender characteristics do not change over the period covered by the figure, the implementation of community service would represent a natural experiment that we may exploit for making causal inference in an instrumental variables (IV) model (Wooldridge 2002; Greene 2003). However, this assumption is not fulfilled in my sample, as offenders who are convicted in different years differ with regard to several observed— and probably also unobserved—background characteristics (results not shown). Thus, even though the presence of a reform makes IV estimation an obvious choice, this strategy is not possible with my sample.

Another useful strategy is to employ the difference-in-difference estimator. This estimator eliminates all observed and unobserved time-invariant differences by comparing differences between treatment and control groups in changes in social benefit dependency, wages and crime rates before (t − 1) and after (t + 1) the offenders served their sentences. Equation 1 explains the procedure. Here, $\bar{Y}_{t-1}$ is the pre-treatment outcomes and $\bar{Y}_{t+1}$ the post-treatment outcomes. T and C signify the treatment status (T are the treated and C are the controls).

$$\hat{\delta}_{DD} = (\bar{Y}_{t+1}^T - \bar{Y}_{t-1}^T) - (\bar{Y}_{t+1}^C - \bar{Y}_{t-1}^C) \tag{1}$$

The difference-in-difference model does solve many of the problems related to making causal inference, but is, however, sensitive to differences in time trends between the treated and the controls. This could be a problem if one wants to assess, for example, the treatment effect on recidivism when the treated and the controls are not the same age (due to selection bias in treatment status). In such a case, we would find the two groups at different points on the age-crime curve, which means that the "natural" slope of their criminal activities differs, whereby also their post conviction outcomes will differ, not due to their sentence type, but due to these initial differences. I solve this problem by reframing the difference-in-difference setup as a matching estimator, where I match my samples of treated and controls on a range of background characteristics, including thorough controls for previous crime, income and dependency (i.e. $\bar{Y}_{t-r}$, where r' signifies any period prior to the treatment date). My outcome is $\bar{Y}_{t+r}$ (r signifies any period after the treatment date), and provided that my samples are adequately matched on $\bar{Y}_{t-r}$, my results—the difference between the treated and controls– is a treatment effect that has the same properties as $\hat{\delta}_{DD}$, but which also balances the samples with respect to time trends prior to the treatment. This estimator produces unbiased results if the distribution of the unobserved individual specific components that affect selection into treatment and subsequent outcomes is similar across the matched treated and controls (Chabé-Ferret 2010). In some cases, this may be a strong and unrealistic assumption; however, the procedure I use for selecting the treated and the controls is likely to meet the conditions necessary for that assumption to be valid. I use local linear matching (llr) based on tricube kernel (as is standard): As described in Heckman et al. (1997) local linear matching is the preferred matching strategy for difference-in-difference matching because it adapts well to different data densities and converges faster than other types of kernel matching (p. 630). However, I test whether my results are robust to other matching algorithms.

Data

In Denmark all residents have a unique personal number which identifies them in a great many transactions, such as submission of tax forms, interaction with the welfare system, schooling, registration of work status, and registration of residence. Statistics Denmark collects the information annually, and makes these data available—in anonymous form— for statistical and research purposes. The available data constitute a panel that goes back as far as 1980 and currently ends in 2009. The data provide information on each resident's criminal behavior—e.g. information on offense dates, reasons for charges, conviction dates, and type of sentence—and also on income and a range of background characteristics. We obtain information on dependency rate from DREAM, an administrative database run by the Ministry of Employment, which provides weekly information on benefit receipts in Denmark. While the data from Statistics Denmark also contain this information, the DREAM database contains the most recent information on individual-level receipt of benefits.

From this data, I choose a sample of individuals who have been convicted of one of the offenses that judges most frequently punish with community service. These are drunk driving, misdemeanor (e.g. shoplifting and vandalism), violent crime and other traffic offenses. To obtain a suitable sample, I first delimit the sample to those who have been sentenced to either community service or prison. Community service is intended to be a substitute for imprisonment, and not for other types of sentence, which makes individuals sentenced to prison the appropriate control group to compare with the group of community service participants. Second, I delete individuals sent to prison who received sentences of longer than a year—community service only replaces prison sentences shorter than a year.

Third, I delimit the sample to include only those who were sent to prison in 1999 and those who were sentenced to community service in 2001. This is to arrive at as accurate a control group as possible that will fulfill the assumptions of my model (as described earlier): Individuals who were sentenced to prison in 2001, when the community service option was available, are likely to be poor matches to community service participants convicted in 2001, and will thus not have the same distribution of individual-specific unobserved characteristics as those who were sentenced to community service. However, in contrast, we are likely to find perfect matches among the individuals sentenced to prison in 1999, when community service was not an option.

Fourth, I drop individuals who both served a prison sentence and did community service during the 3-year period 1999–2001. These individuals would belong to both the treatment and the control groups, and this makes it difficult to include them in either group. Fifth, as Danish women do not commit much crime, I focus only on male offenders. Thus my final sample consists of 6,042 observations, 4,279 who were sentenced to prison in 1999 and 1,763 who were sentenced to community service in 2001. Given the selection criteria described above, the higher number of offenders sentenced to prison terms makes sense, as this group contains the smaller subgroup who would have gotten community service sentences had it been an option in 1999.

Potential Sample Problems

Importantly, with these definitions of treatment and control groups, changes in contextual differences—such as the business cycle—might affect differences in outcomes for the two groups, and thus bias the estimates. I test this by comparing the results to similar calculations based on the current control group of offenders who were sent to prison in 1999 and

a treatment group of offenders sent to prison in 2001, and by adjusting for time trends observed in a group of non-criminal but otherwise comparable males.

Another potential problem with my choice of treatment and control group arises if judges change their sentencing behaviour in response to the reform. If for instance judges are unwilling to sentence offenders to community service, they could increase their use of prison sentences of more than a year and then avoid considering community service in a number of cases. Hereby, a group of offenders who would be eligible for the control group in 1999—i.e. those that get sentences of just below 1 year—would get slightly longer sentences in 2001 due to the changed judge behaviour, and this would make then ineligible for my treatment group. Such changed judge behaviour is likely to result in systematic observed and unobserved differences between treatment and control group. Another possibility represented by the net widening argument is that the availability of community service inclines judges to punish offenders who would otherwise have been let off hook (Austin and Krisberg 1982). As a consequence, offenders sentenced to community service in 2001 would represent an entirely different—and probably less criminal—group of offenders than offenders sentenced to prison in 1999. Differences in outcomes between the two groups would then represent the causal effect of community service along with a range of other differences between the treated and the controls. Killias et al. (2000) show evidence that the introduction of community service in Switzerland in 1991 changed judge behaviour.

The first concern is difficult to test empirically, since we have no information on the length of prison sentence that the community service replaces. This means that we cannot compare changes in the distribution of actual and potential prison sentences across the reform. However importantly, prison sentences of more than a year are not very common in Denmark: more than 95 % of all sentences are shorter than a year and approximately 94 % are shorter than 6 months. This implies that only few criminal cases are candidates for sentence extensions that push them across the 1 year threshold. Also we do not observe any remarkable increase (or decrease for that matter) in the number of sentences that are longer than 1 year across the reform years—in 1999 374 offenders received prison sentences of 1–2 years duration, in 2000 it was 369 offenders and in 2001 364 offenders received such sentences. Thus on average, judges do not seem to have changed their sentencing behavior in response to the reform.

As a test of the second concern, the net widening hypothesis, we may check whether judges are more or less likely to give prison sentences across the reform years. If there is no change in the number of prison sentences from the years 1999 to 2001, where we observe a substantial increase in the use of community service, this could be an indication of judges' net widening behavior. In that case, the community service offenders are taken from a non-imprisoned group of offenders, whereby offenders imprisoned for less than a year in 1999 would be an inappropriate control group. Figure 2 shows the use of imprisonment and suspended prison sentences from 1996 to 2004. The use of suspended prison sentences is relevant in this context since official registers categorize community service as a suspended prison sentence. The figure shows an almost direct reverse tendency in the use of the two types of sentences: from 1999 to 2001, the number of prison sentences drops from 13,659 to 9,521 while the number of suspended prison sentences increases from 8,422 to 12,967. This is a strong indication that the reform has the intended effect and moves offenders from serving their sentence in prison to community service. Hence problems pertaining to net widening behavior do not seem to affect my design.

Fig. 2 changes in the use of imprisonment and suspended prison sentences, 1996–2004
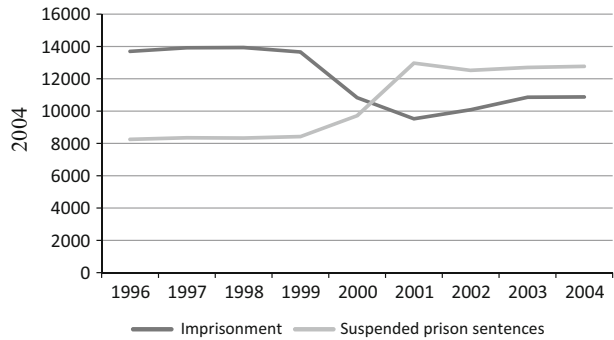
Variables

As stated in the introduction, this paper assesses the short- and long-term effects of serving a community service sentence on three outcomes: social benefit dependency, wage income and crime. From the data I obtain information on weekly benefit dependency and wage income in the 2nd to the 5th years after the year of the conviction (t + 2, t + 3, t + 4 and t + 5), and on crime in the 2nd to the 4th years after the year of the conviction (t + 2, t + 3 and t + 4). Note that social benefit dependency includes all types of unemployment benefit for insured and uninsured workers (including time spent in active labor market programs), early retirement pension, and sick leave benefits. Crime includes all convictions except traffic offenses. Note that I exclude traffic offenses as they are often not considered real crime in the literature.

The first two rows of Table 1 show the distribution of these outcomes in the treatment and control group. We see that community service participants have lower dependency rates during all the years considered. As we measure dependency by weeks, the differences are not trivial, as they amount to almost 5 weeks during the first year (26.46–21.81 = 4.65), and to more than 9 weeks (29.61–20.07 = 9.54) during the last year (t + 5). Second, and in line with the higher dependency rates, the community service participants earn more in all the years of measurement. The difference in, for example, t + 5 is 5.09, corresponding to DKK 50,900 (EUR 6,787). Again, these differences are non-trivial. Third, the community service participants commit less crime after their convictions than the imprisoned offenders (I measure crime as the number of recorded convictions).

As discussed earlier, my matching setup is meant to resemble a difference-in-difference approach, where results reflect the change in outcomes caused by the treatment (or, for the control group, the absence of the treatment). Consequently, a thorough control for pre-treatment outcomes, $\bar{Y}_{t-r}$, is essential. I therefore include values on pre-treatment outcomes measured at t − 1, but also at t − 2 and t − 3, to account for time trends prior to the year of the conviction. Table 1 shows the descriptive statistics for these variables, and as can be seen—and as was expected—community service participants fare far better on these variables. They have lower dependency rates, higher incomes and lower crime rates.

The lower section of Table 1 shows descriptive statistics for the control variables. First, I include information on the type of crime committed (whether drunk driving, violence, misdemeanor or another type of traffic offense). I also control for the extent to which the offenders had been convicted of such crimes in the 3 years prior to the current conviction. I

Table 1 Descriptive statistics

	Means					
	Pre-matching		Post-matching			
	Treated	Controls	Treated	Controls	% bias	t test for differences in means
Outcome variables						
Dependency rate						
t + 2	21.81 (22.19)	26.46 (21.93)				
t + 3	22.56 (22.73)	27.11 (22.23)				
t + 4	21.36 (22.43)	28.46 (22.38)				
t + 5	20.07 (22.55)	29.61 (23.13)				
Annual income (× DKK 10,000)						
t + 2	12.55 (13.56)	9.71 (12.08)				
t + 3	12.88 (13.19)	9.50 (12.15)				
t + 4	13.26 (13.50)	8.88 (12.13)				
t + 5	14.13 (13.91)	9.04 (12.42)				
Crime						
t + 2	0.21 (0.56)	0.42 (0.84)				
t + 3	0.20 (0.53)	0.40 (0.87)				
t + 4	0.23 (0.64)	0.39 (0.86)				
Control variables						
Pre-treatment outcomes						
Dependency rate						
t − 1	15.48 (19.76)	23.48 (21.54)	15.47	16.16	−3.3	−1.04
t − 2	14.32 (19.56)	22.62 (21.30)	14.43	15.30	−4.2	−1.23
t − 3	13.96 (19.66)	20.96 (21.16)	14.05	14.53	−2.4	−0.74

Table 1 continued

	Means		Post-matching		% bias	t test for differences in means
	Pre-matching					
	Treated	Controls	Treated	Controls		
Annual income (× DKK 10,000)						
t − 1	14.26 (13.02)	10.18 (11.83)	14.30	13.77	4.3	1.22
t − 2	13.86 (13.33)	9.78 (11.57)	13.93	13.36	4.5	1.31
t − 3	15.52 (12.66)	9.43 (11.49)	13.57	13.16	3.4	0.97
Crime						
t − 1	0.18 (0.49)	0.48 (0.91)	0.18	0.16	3.5	1.62
t − 2	0.19 (0.53)	0.52 (1.02)	0.19	0.20	0.2	−0.10
t − 3	0.18 (0.50)	0.47 (0.97)	0.18	0.17	2.3	1.10
Other controls						
Drunk driving						
t − 1	0.05 (0.22)	0.04 (0.20)	0.05	0.05	1.6	0.47
t − 2	0.06 (0.24)	0.06 (0.24)	0.06	0.07	−3.3	−0.94
t − 3	0.06 (0.24)	0.05 (0.22)	0.06	0.06	−2.2	−0.63
Violence						
t	0.16 (0.37)	0.38 (0.48)	0.17	0.17	0.16	0.39
t − 1	0.02 (0.16)	0.03 (0.19)	0.02	0.03	−1.3	−0.39
t − 2	0.02 (0.16)	0.06 (0.27)	0.02	0.02	−1.5	−0.64
t − 3	0.01 (0.12)	0.05 (0.25)	0.01	0.01	−0.3	−0.13
Misdemeanor						
t	0.09 (0.28)	0.14 (0.35)	0.09	0.08	2.7	0.92
t − 1	0.03 (0.19)	0.07 (0.28)	0.03	0.03	2.6	1.00
t − 2	0.02 (0.13)	0.08 (0.33)	0.02	0.01	2.5	1.52
t − 3	0.03 (0.18)	0.06 (0.28)	0.03	0.02	2.6	1.09

Table 1 continued

| | Means | | | Post-matching | | | |
| | Pre-matching | | | | | | |
	Treated	Controls		Treated	Controls	% bias	t test for differences in means
Other traffic offenses							
t	0.07 (0.26)	0.03 (0.16)		0.07	0.07	1.6	0.39
t − 1	0.10 (0.34)	0.11 (0.38)		0.10	0.09	2.7	0.85
t − 2	0.08 (0.30)	0.11 (0.37)		0.08	0.07	0.7	0.23
t − 3	0.08 (0.31)	0.10 (0.37)		0.08	0.08	−0.5	−0.17
Age	34.52 (12.26)	32.65 (11.05)		34.53	35.10	−4.9	−1.46
Single	0.58 (0.49)	0.66 (0.47)		0.59	0.58	1.4	−0.41
No. of children	0.50 (0.93)	0.44 (0.90)		0.50	0.50	0.2	0.05
More than elementary school	0.44 (0.50)	0.35 (0.48)		0.43	0.45	−3.9	−1.12
N	1,763	4,248		1,763	4,248		

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

moreover control for age, marital status, number of children, and education (whether the offender has more education than elementary school). These are valid controls, as they are likely to signal information that the judge may take into account when deciding on sentence types. Again we see how the community service participants score better on these measures than the offenders sentenced to prison, as they are less likely to have been convicted of any of the four types of crime prior to the current offense, more of them have families, and they are better educated.

Results

Table 2 shows the results from the propensity score model—note that this model is similar across outcomes, as the treatment remains the same, regardless of whether we focus on social benefit dependency, income or crime as our outcome.

Unsurprisingly, the results presented in Table 2 describe the same pattern that we saw in Table 1. First, the results indicate that offenders sentenced to community service (i.e. who receive the treatment), are less likely than offenders sent to prison to depend on social benefits prior to their conviction, at least in some of the years. The significant coefficient of −0.008 for social benefit dependency in the second year prior to the conviction corresponds to a marginal effect of a .2 % decrease in the probability of receiving a community service sentence per week of social benefit dependency. Thus compared to offenders who were unemployed the second year prior to their conviction, offenders who were fully employed during that year had a 10 % higher probability of receiving a community service sentence (52 weeks × 0.2 %). The results also indicate that pre-conviction income matters: the probability of serving a community service sentence increases by .2 % for each additional 10,000 DKK (EUR 1,333) the offender earns annually. Thus, an offender with an annual income of 150,000 DKK (EUR 20,000) is 2 % more likely to receive a community service sentence than an offender with an annual income of 50,000 DKK (EUR 6,667). Also the extent of previous crime affects the probability of serving a community service sentence as oppose to going to jail. The coefficient for crime committed in the year prior to the conviction is −0.295, which corresponds to a marginal effect of −0.096. Thus each crime committed in c1 increases the probability of going to jail by almost 10 %. Importantly, of the variables that measure pre-treatment outcomes (benefit dependency, income and crime) not all are significant. This could be an indication of correlations between the variables measured at t − 1, t − 2 and t − 3. However, I retain all the variables in the model in order to ensure proper balancing of the two samples, particularly with regard to time trends prior to conviction.

The results presented in Table 2 furthermore indicate that offenders convicted of misdemeanour are 8 % less likely to receive a community service sentence (the coefficient of −0.266 corresponds to a marginal treatment effect of 0.080) and that offenders convicted of violence are 18 % less likely to receive this type of sentence (the reference category is offenders convicted of drunk driving). This last result is a strong indication that judges follow the advice mentioned earlier, to only use community service with caution (an advice that is often more relevant in cases of violence than in cases of misdemeanour). In contrast, offenders convicted of other traffic offences are 9 % more likely than drunk drivers to receive a community service sentence. Also the nature of crime committed prior to the current conviction matters, but here, being convicted of violence and misdemeanour in t − 1 actually increases the probability of receiving a community service sentence by 13 respectively 9 %. The coefficients are big enough to level out the effect of those types of

	Coefficient
Table 2 Results from the propensity score model	
Pre-treatment outcomes	
Social benefit dependency	
t − 1	−0.002 (0.002)
t − 2	−0.008 (0.002)***
t − 3	−0.000 (0.002)
Annual income from wages (× DKK 10,000)	
t − 1	−0.000 (0.003)
t − 2	−0.007 (0.004)
t − 3	0.007 (0.003)**
Crime	
t − 1	−0.295 (0.046)***
t − 2	−0.066 (0.041)
t − 3	−0.137 (0.044)**
Controls	
Drunk driving	
t − 1	0.265 (0.097)**
t − 2	−0.021 (0.084)
t − 3	0.116 (0.088)
Violence	
t	−0.619 (0.050)***
t − 1	0.408 (0.116)***
t − 2	−0.131 (0.107)
t − 3	−0.193 (0.125)
Misdemeanor	
t	−0.266 (0.065)***
t − 1	0.284 (0.099)***
t − 2	−0.259 (0.114)*
t − 3	0.139 (0.103)
Other traffic offenses	
t	0.270 (0.086)***
t − 1	0.051 (0.052)
t − 2	−0.095 (0.057)
t − 3	−0.069 (0.055)
Age	−0.004 (0.002)*
Single	−0.085 (0.044)
No. of children	0.001 (0.023)
More than elementary school	−0.000 (0.040)
Intercept	0.147 (0.092)
Chi^2	741.85***
Pseudo R^2	0.1022
N	6,011

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

Source: Own calculations based on data from Statistics Denmark

offences in t. This suggests that judges are more likely to use community service for those who frequently commit and are convicted of the same type of crime, which may reflect judges' observation that a prison stay fails to deter the offender from future crime (due to the reform, most of these previous convictions would have been served in prison). Thus these results may reflect judges' use of a new sentence type in acknowledgement of the observed absent deterrence effect of prison for specific types of offenders.

Last, the probability of being sentenced to community service decreases by age—getting 1 year older reduces the probability by .1 %. Note also that the R^2 of this model is relatively high, which suggests that the model manages to reasonably account for the allocation into sentence type.

The last 4 columns of Table 1 show the balancing properties of the matching. Column 3 and 4 show means of all control variables in the matched samples and column 5 shows the difference between the treated and the controls on these variables calculated as the % bias. If the bias is larger than 5 on a variable, it is an indication that the two samples differ unacceptably much on that particular variable. However the importance of that bias should also be considered in light of the *t* test for the difference in means. As shown the two samples are perfectly balanced on all variables (as indicated by the low %bias-values and the low t − values). In combination with the balancing properties of the matching models indicated by a low and insignificant Chi^2-value and a tiny R^2 (last two rows of Table 3) this then suggests that the treated and controls are properly balanced.

Results from the Matching

Table 3 shows the results from the matching. Columns with the headings "treated" and "controls" show average levels of benefit dependency, income and crime for each group at t + 2, t + 3, t + 4 and t + 5. In the column headed "Diff." a positive coefficient indicates a positive treatment effect and a negative coefficient indicates a negative treatment effect.

From the table we learn that community service participants have significantly lower dependency rates in the long run. The differences are quite high, and vary between nearly 2.2 and 4.7 weeks. Moreover, community service participants also have higher earnings than offenders who serve in prison, but the differences are only significant in the long run. However, at this point they are non-trivial, as the coefficient of 2.11 corresponds to DKK 21,100 (EUR 2,813)—recall that the average annual income at t − 1 is approximately DKK 113,800 (EUR 15,173), which amounts to a treatment effect of almost 20 %.

Last, community service participants commit fewer crimes in the first 2 years after the sentence, but same amount of crimes in the last year. Importantly, these differences are not significant.

These results thus demonstrate quite distinct positive long-term effects of community service on income and dependency. These results are not sensitive to choice of matching algorithm, as nearest-neighbor matching and 1:10 nearest-neighbor matching produce similar results (results not shown).

Figures 3, 4 and 5 show the results graphically. Here we see that the two groups—the control group, which consists of offenders sentenced to prison, and the treated group, which consists of offenders sentenced to community service—are very similar with regard to benefit dependency, income and criminal activity prior to their conviction (which is the result of the reform in combination with the matching). But we also see that they differ—in some cases significantly—after their conviction.

Table 3 Results from llr matching

	Controls	Treated	Diff. (SE)
Social benefit dependency			
t + 2	20.84	21.81	0.97 (0.57)
t + 3	21.52	22.49	0.98 (0.54)
t + 4	23.54	21.27	−2.27 (0.56)***
t + 5	24.66	19.96	−4.70 (0.57)***
Annual income from wages (×DKK 10,000)			
t + 2	12.89	12.54	−0.35 (0.36)
t + 3	12.53	12.89	0.35 (0.34)
t + 4	11.85	13.28	1.42 (0.37)***
t + 5	12.03	14.14	2.11 (0.39)***
Crime			
t + 2	0.24	0.21	−0.03 (0.02)
t + 3	0.23	0.20	−0.02 (0.02)
t + 4	0.23	0.23	−0.00 (0.02)
Chi^2 for balancing			18.08
Pseudo R^2 for balancing			0.004
N	4,248	1,763	

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

Source: Own calculations based on data from Statistics Denmark

Fig. 3 Social benefit dependency before and after conviction. *Source* Own calculations based on data from Statistics Denmark
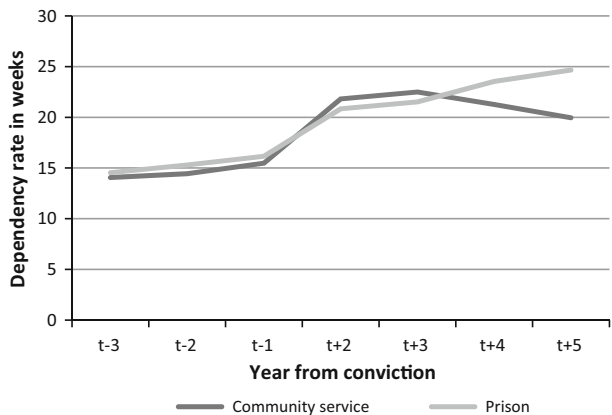
Results by Offender Type

As mentioned earlier, my sample consists of four different types of offenders: drunk drivers, perpetrators of misdemeanor and violent crime, and other traffic offenders. As these types of crime are very diverse, it is likely that the treatment effect—the effect of doing community service—differs among offender groups, meaning that each type of offender contributes differently to the overall effect. The results presented in Table 4 test this assumption by presenting results based on sub-samples defined by offender types.

Fig. 4 Income before and after conviction. *Source* Own calculations based on data from Statistics Denmark
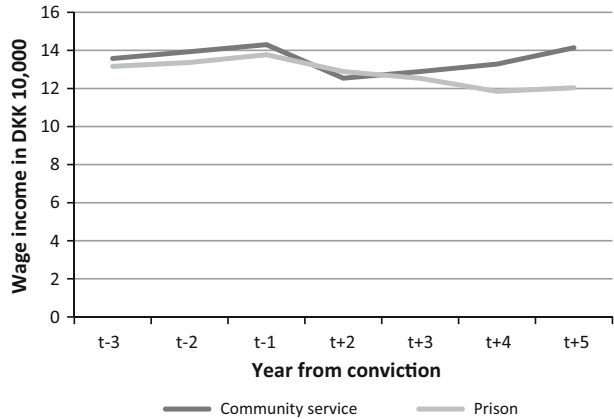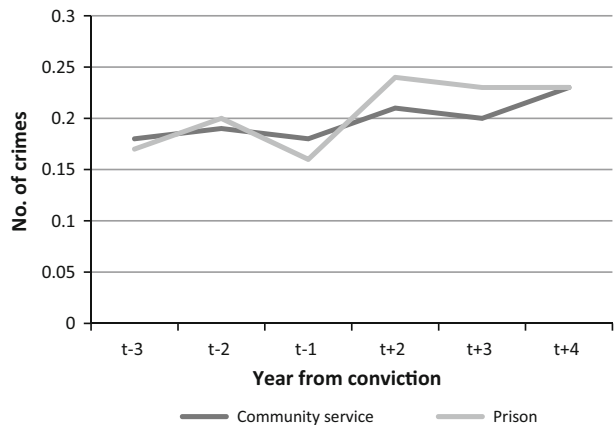

Fig. 5 Crime before and after conviction. *Source* Own calculations based on data from Statistics Denmark

These results show an interesting pattern, and it is evident that the four groups of offenders do indeed contribute differently to the overall conclusions. First, we find a positive effect of community service on short-term benefit dependency and a negative effect on income for drunk drivers. Second, the samples of drunk drivers and violent offenders drive the effect of community service on long-term benefit dependency. Third, we retrieve the long-term effect of community service on income in the sample of drunk drivers, violent offenders, and misdemeanor offenders. However, the size of the coefficients varies remarkably between samples, and it seems that effects are strongest for offenders of misdemeanor, where the income difference between community service participants and offenders sentenced to prison is as much as DKK 28,700 (EUR 3,773) in the year $t + 4$. Fourth, we find significant effects of community service on short-term crime among offenders of violence and misdemeanor and among offenders of other traffic offences. Among these groups, community service reduces recidivism by as much as 0.45 crimes in the short run. Note also that the model for other traffic offences has limited explanatory powers (as indicated by the Chi^2- and R^2-values), which result from the low number of observations (N = 230).

Table 4 Results from llr matching, by offender type

Type of crime outcome	Drunk driving	Misdemeanor	Violence	Other traffic offense
Social benefit dependency				
t + 2	2.17 (0.73)**	1.17 (2.00)	−3.00 (1.19)*	−2.00 (3.01)
t + 3	2.13 (0.78)**	0.69 (2.08)	−1.99 (1.26)	−1.16 (3.60)
t + 4	−1.66 (0.80)*	−2.37 (2.19)	−3.19 (1.58)*	−3.99 (3.75)
t + 5	−5.00 (0.81)***	−3.51 (2.05)	−4.12 (1.35)**	−2.53 (4.30)
Annual income from wages ($\times$ DKK 10,000)				
t + 2	−0.93 (0.39)*	1.22 (1.38)	0.48 (0.80)	−2.31 (3.10)
t + 3	0.09 (0.45)	1.19 (1.16)	1.28 (0.74)	−4.59 (3.06)
t + 4	1.23 (0.48)*	2.87 (1.14)*	2.15 (0.75)**	−4.10 (3.02)
t + 5	1.98 (0.47)**	2.57 (1.37)*	2.60 (0.82)**	−4.27 (3.35)
Crime				
t + 2	−0.00 (0.02)	−0.05 (0.07)	−0.16 (0.05)**	−0.07 (0.15)
t + 3	0.02 (0.01)	−0.19 (0.08)*	−0.09 (0.05)	−0.45 (0.18)*
t + 4	0.01 (0.02)	−0.03 (0.10)	−0.07 (0.05)	0.01 (0.12)
Chi^2 in propensity model	90.67***	161.25***	222.20***	9.36
R^2 in propensity model	0.0217	0.2111	0.1362	0.0296
Chi^2 for balancing	15.02	11.04	14.56	21.08
Pseudo R^2 for balancing	0.005	0.026	0.018	0.06
N	3,144	747	1,880	230

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

Source: Own calculations based on data from Statistics Denmark

Preliminary Discussion of Results

In sum, the results from the matching then demonstrate relatively large overall differences in the effect over time, and across offender types, and of these differences, the lack of an overall short run effect on the two labor market outcomes is the most puzzling (even if we do find short run effects for specific offender types). The question is why community service only matters in the long run? A possible explanation for these findings could be that the disadvantages of being sent to jail are reinforced over time. If the community service servers benefit from a continuous labor market affiliation while serving their sentence, they will have a stronger foothold at the labor market, and the development of their human capital and employee benefits over time is more likely to follow that of other employed people. This will generate pay rises and other benefits that follow from a continuous labor market affiliation and from increased experience. In contrast, offenders sent to prison, will not experience this development, and may be more likely to find unstable jobs with little prospect of higher incomes or the appreciation of increased experience. In addition, they would have to explain the prison related gap in their labor market affiliation to a future employer, and this may compromise their chances of getting a job. As a result, offenders serving a prison sentence may become tied to low paid jobs, and as the community servers progress in their jobs over years, the difference between the two groups becomes more and more pronounced. If this explanation is valid, it clarifies why we only see a long term effect of community service.

But also differences in the effects across offender types require an explanation. Of these, the short run positive effects of community service on the social benefit dependency and the negative effect on income of drunk drivers is the most puzzling. The finding that community service raises the social benefit dependency of drunk drivers by more than 2 weeks in t + 2 and t + 3 and reduces their income in t + 2 is at odds with expectations, as well as with the explanation of the lacking overall short run effect discussed in the previous section. These findings indicate that drunk drivers who serve a community service sentence are exposed to higher levels of post-sentence informal punishments than drunk drivers who are sent to prison. A possible interpretation is that allowing drunk drivers to avoid prison injures the public sense of justice and inclines society to exert additional punishment over and above the legal punishment.

But in addition to these findings we also observe other variations in the effect of community service between offender groups and we see that community service has almost no significant effects among traffic offenders. While this last finding mainly results from the low number of traffic offenders, the differences in findings between offender groups may reflect differences in the social and personal characteristics of the four offender types. For instance, offenders convicted of drunk driving and violence are less likely to be unemployed prior to their conviction than e.g. offenders convicted of misdemeanour (i.e. average unemployment differs by as much as 6 weeks per year). Also these two offender groups have higher pre-conviction income (average annual income differ by as much as 80,000 DKK or EUR 10,666) and fewer previous convictions. Hence, these two offender groups are less marginalised from the normal society, and this may result in a stronger effect of avoiding prison: if it is common perception that prison is something very different from normal society, a prison stay may represent a more significant experience to those that are more integrated into society because their usual surroundings are less prepared for handling the consequences of a prison stay. Hereby expecting a uniform effect of community service across offender groups is not very realistic.

Problems Pertaining to Bias?

Importantly, my results may also suffer from bias that may be the source of unexplainable results. With propensity score matching we estimate our treatment effects relying on selection on observables, which means that our balancing of treatment and control groups rely only on variables observed in our data. In case unobserved variables affect the selection into treatment and their influence is systematically different between the treatment and the control group, results from propensity scores matching will be biased, and cannot be used for causal inference. We cannot know whether and to which extent such unobserved variables influence our results, however, using Rosenbaum bounds we get a sense of how big this influence should be to alter our overall conclusions. The Rosenbaum bounds work by providing an indication of how large the effect of an unobserved component should be (in odds ratio) in order for it to either change levels of significance or the sign of a coefficient. A rule of thumb says that if it takes an unobserved component with odds of 2 or more to alter the conclusions, the estimated treatment effect is quite robust.

I test the robustness of my results using Rosenbaum bounds. As discussed earlier, one may fear that offenders with favourable characteristics are more likely to receive a community service sentence, and this type of selection would cause an overestimation of the positive effect of community service on my various outcomes. However, the test shows that all results concerning social welfare dependency and income are very robust and that unobserved components should influence selection into treatment with the odds of 2 or

more to alter my conclusions. In contrast, the results concerning recidivism are less robust: if unobserved components influence the selection into treatment with odds of between 1.3 and 1.5 (depending on the crime outcome in question), the sign of the treatment effect would change. My results concerning recidivism should then be interpreted with caution.

Bias Caused by Business Cycles?

While my calculations of the Rosenbaum bounds suggest that the influence from bias does not seriously affect my results, it is still important to consider possible sources of bias in my analyses. As mentioned earlier, differences in the state of the business cycle experienced by the control group and the treatment group represent a significant source of bias—the two groups were sentenced 2 years apart, and changes in the Danish economy in this period are likely to have affected both the dependency rates and the income options of the two groups. We know, for instance, that Denmark suffered from a small recession in 2003 and 2004, and this recession is likely to have affected our treatment effects. These changes will be disguised as part of the treatment effect, which will then be biased. I test this possibility in two ways.

First, I conduct an additional llr matching with the same control group, but a treatment group consisting of offenders convicted of drunk driving, other traffic offenses, misdemeanor and violent crimes who were sentenced to imprisonment for less than a year in 2001. This treatment group is somewhat artificial, as its treatment does not differ from the treatment of the control group—both were sent to prison for the same offenses and for the same amount of time. However, the two groups are treated at times when the economic situation was different, and the effect of this "treatment" will give an indication of potential bias in my original estimates caused by the different economic situations. Thus, comparing the results from these analyses with my original results (presented in Table 3) will show if and how these original results are affected by time trends. Note, however, that such comparisons rely on the assumption that time trends affect the two groups equally.

In a second sensitivity analysis, I conduct a time trend adjustment of my original results using time trends in dependency rates, income and crime rates observed in the general population of Danish men. Under the assumption that these time trends are the same as the time trends of the population of community servers, this procedure should then cleanse the original results of these time trends.

Community Service Participants and Offenders Sent to Prison

Table 5 shows the results of the first analysis, where I compare the original effects of community service presented in Table 3 with time trends for offenders sent to prison. The table shows whether the differences between the two sets of results are significant. We may interpret the size of the difference as the effect of community service, cleansed of potentially disturbing period effects.

As can be seen, there are some significant period effects, as offenders imprisoned in 1999 and 2001 differ, particularly with regard to their post-sentence dependency rates. These differences follow the same patterns as the differences observed for the community service participants. We also note a significant period effect with regard to income in $t + 3$—imprisoned offenders convicted in 2001 have lower income in $t + 3$ than offenders convicted and sent to prison in 1999.

But in addition, the differences between the two sets of results—for community service participants and offenders sentenced to prison respectively—are significant for all

Table 5 Comparison of treatment effects and period effects

	Community service participants	Offenders sentenced to prison	Diff. (SE)
Social benefit dependency			
t + 2	0.97 (0.57)	1.58 (0.50)**	−0.61 (0.01)***
t + 3	0.98 (0.54)	1.62 (0.50)***	−0.64 (0.01)***
t + 4	−2.27 (0.56)**	−0.71 (0.59)	−1.56 (0.01)***
t + 5	−4.70 (0.57)***	−2.22 (0.57)***	−2.48 (0.01)***
Annual income			
t + 2	0.35 (0.36)	−1.00 (0.27)***	0.65 (0.01)***
t + 3	0.35 (0.34)	−0.63 (0.28)*	0.98 (0.01)***
t + 4	1.42 (0.37)***	0.76 (0.29)**	0.66 (0.01)***
t + 5	2.11 (0.39)***	1.31 (0.33)**	0.80 (0.01)***
Crime			
t + 2	−0.03 (0.02)	0.08 (0.03)**	−0.11 (0.00)***
t + 3	−0.02 (0.02)	0.05 (0.03)	−0.07 (0.00)***
t + 4	−0.00 (0.02)	0.07 (0.03)***	−0.07 (0.00)***

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

Source: Own calculations based on data from Statistics Denmark

outcomes. This first of all suggests a treatment effect of community service net of period effects. We also observe that while the effect of community service on income is positive (though insignificant), the effect of imprisonment in 2001 versus imprisonment in 1999 is negative, suggesting that the effect of the community service is underestimated. Thus, our community service participants probably would have had significant positive gains from their non-custodial sentence also in the short run, had it not been for the negative time trend in income. These calculations do however also suggest that the long term effect of community service may be overestimated: the period effect is positive and subtracting it from the treatment effect reduces the treatment effect by more than 50 %. To the extent that we may rely on the validity of the period effect used here, this is then an indication that the effect of community service on income does not increase over time as suggested in Table 3. Rather, community service has an immediate and relatively stable impact from the first year after the conviction and onwards. The same is true for the last outcome, crime. Here, the period effect is positive—suggesting higher levels of recidivism—and this leaves us with an even larger negative and significant effect of community service.

Table 6 shows the same calculations by offender type. All results follow the pattern from Table 5, where the period effect correction reinforces the original results, except that we get significant positive effects of community service on the social benefit dependency of drunk drivers in t + 4 and t + 5, and significant negative effect of community service on annual income in all years for other traffic offences. Particularly the last set of results is odd, given that we find negative treatment effects of community service on social benefit dependency for other traffic offences—this means that traffic offenders sentenced to community service are both less likely to be unemployed and have lower income. It may however reflect the low number of traffic offenders in both groups used for the calculations in Table 6 (N = 230 respectively 222), which result in imprecisely estimated effects. Also, as indicated in Table 4, the propensity score model for other traffic offenders has limited

explanatory power (as implied by low Chi^2- and R^2-values) and this limits a useful interpretation of any results produced by this model.

Community Service Participants and a Non-Criminal Comparison Group

In the second sensitivity analysis I rerun the original analysis using time trend corrected outcome and balancing measures. This is to test whether general time trends drive my conclusions. For this purpose I calculate mean income, dependency and crime rates in a group of Danish males for all relevant years and determine an index year (2000). I then use the ratio between the index year and year t to adjust outcome and balancing measures in year t in my original sample of offenders imprisoned in 1999 and offenders in community service in 2001. To the extent that the time trends in my original sample and in the general sample of Danish males are the same, this strategy should then produce results that are unbiased by time trends.

Table 7 shows the time trends adjusted results. As can be seen, the results do not change dramatically. Results indicate significant long run effects of community service on dependency rates and on income, however now, the insignificant short run effect have the same signs as the significant long run effects. In contrast to what we saw in the original results we now observe a significant and negative short run effect of community service on crime. Thus to the extent that we can rely on the assumptions of this time trend correction, there is no reason to fear that the original results are severely biased by fluctuations in the business cycle.

Table 8 shows the results by offender type. What is notable about these results is that they do not reproduce the few counter intuitive results presented in Table 4, where we saw positive short term effect of community service on the social welfare dependency of drunk drivers and short term negative income effects for the same group. Instead, all significant results presented in this table are in line with what we would expect, given theories on the harmful effects of prison discussed earlier.

Discussion

The sensitivity checks and robustness analyses presented above suggests that my initial results are quite robust. This is particularly clear from the results of my calculation of the Rosenbaum bounds. And while the results from the Rosenbaum bounds do not support the suspicion that bias may drive my results it is still interesting how the models relying on time trend corrected measurements manage to eliminate almost all of the few less intuitive findings of the initial results.

Unfortunately, the differences in the results of the initial models and the subsequent sensitivity checks and robustness analyses then introduce the question of what results to believe the most. The results from models with time trend corrected measurements are intuitively appealing because they align the results quite closely with the theoretical predictions. Also, given that one of the strong assumptions of the difference-in-difference model is that treated and controls are exposed to the same time trends, it would seem haphazard to disregard results from the time trend corrected model, as this model ensures the similarity of time trends across groups. However even with the two types of time trend corrections presented here, we cannot know if we have accounted for the specific time trends—and differences in trends—to which treated and controls are exposed. The question is if the time trend affecting Danish men in general and Danish men imprisoned in 1999 respectively 2001 is the time trend that also affect my treated and controls?

Table 6 Comparison of treatment effects and period effects, by offence type

	Drunk driving	Misdemeanor	Violence	Other traffic offences
Social benefit dependency				
t + 2	−0.39 (0.02)***	−0.82 (0.08)***	−4.70 (0.03)***	−2.05 (0.25)***
t + 3	−1.48 (0.02)***	−0.61 (0.08)***	−3.74 (0.03)***	−1.27 (0.32)***
t + 4	−1.38 (0.03)***	−3.28 (0.08)***	−2.21 (0.03)***	−2.91 (0.37)***
t + 5	−6.53 (0.02)***	−2.29 (0.08)***	−1.89 (0.03)***	0.18 (0.37)
Annual income				
t + 2	2.63 (0.01)***	2.38 (0.05)***	1.60 (0.02)***	−2.96 (0.31)***
t + 3	1.18 (0.01)***	2.16 (0.04)***	1.90 (0.02)***	−5.19 (0.29)***
t + 4	1.49 (0.01)***	2.67 (0.04)***	1.07 (0.02)***	−4.87 (0.28)***
t + 5	0.67 (0.01)***	1.62 (0.05)***	0.97 (0.02)***	−6.42 (0.31)***
Crime				
t + 2	−0.06 (0.00)***	−0.17 (0.00)***	−0.21 (0.00)***	0.00 (0.61)
t + 3	−0.03 (0.00)***	−0.19 (0.01)***	−0.17 (0.00)***	−0.17 (0.02)***
t + 4	−0.05 (0.00)***	−0.17 (0.00)***	−0.12 (0.00)***	0.27 (0.01)
N (treatment effect)	3,144	747	1,880	230
N(period effect)	2,398	1,019	2,955	222

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

Source: Own calculations based on data from Statistics Denmark

Table 7 Time trend adjusted results

	Controls	Treated	Diff. (SE)
Social benefit dependency			
t + 2	20.11	19.50	−0.61 (0.52)
t + 3	19.70	19.32	−0.38 (0.49)
t + 4	20.82	18.34	−2.48 (0.53)**
t + 5	21.00	16.92	−4.08 (0.51)***
Annual income from wages (× DKK 10,000)			
t + 2	12.36	13.25	0.88 (0.31)
t + 3	12.67	13.37	0.70 (0.41)
t + 4	12.15	13.50	1.35 (0.39)**
t + 5	12.09	14.02	1.93 (0.38)**
Crime			
t + 2	0.24	0.20	−0.04 (0.02)*
t + 3	0.22	0.20	−0.03 (0.02)
t + 4	0.22	0.23	0.00 (0.02)
Chi^2 for balancing			23.71
Pseudo R^2 for balancing			0.005

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

Source: Own calculations based on data from Statistics Denmark

But even if there are small inconsistencies between the results based on different models, all results do seem to point in the same direction: that offenders are better off when serving a community service sentence than when going to prison. This then suggests that

Table 8 Time trend adjusted results by offender type

	Drunk drivers	Misdemeanor	Violence	Other traffic offences
Social benefit dependency				
t + 2	0.30 (0.75)	0.11 (2.33)	−4.21 (0.99)***	1.48 (3.46)
t + 3	0.36 (0.62)	−0.75 (1.71)	−2.84 (1.28)*	1.44 (2.87)
t + 4	−2.21 (0.72)**	−2.36 (1.97)	−3.21 (1.10)**	0.13 (2.94)
t + 5	−4.49 (0.60)***	−3.10 (2.02)	−3.41 (1.05)***	1.04 (3.34)
Annual income from wages (× DKK 10,000)				
t + 2	0.03 (0.41)	1.96 (1.41)	1.59 (0.76)*	4.20 (2.78)
t + 3	0.18 (0.50)	1.18 (1.23)	1.43 (0.79)	1.59 (3.08)
t + 4	1.00 (0.44)*	2.46 (1.31)	1.81 (0.75)**	1.01 (3.59)
t + 5	1.60 (0.45)***	2.23 (1.52)	2.09 (0.94)**	1.47 (3.35)
Crime				
t + 2	−0.01 (0.02)	−0.09 (0.07)	−0.18 (0.05)***	−0.01 (0.16)
t + 3	0.02 (0.01)	−0.18 (0.10)	−0.09 (0.05)	−0.07 (0.16)
t + 4	0.02 (0.02)	−0.02 (0.09)	−0.06 (0.06)	0.07 (0.13)
Chi^2 for balancing	16.77	15.25	13.57	36.87***
Pseudo R^2 for balancing	0.005	0.036	0.077	0.106

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

Source: Own calculations based on data from Statistics Denmark

the findings of previous studies mentioned in the introduction that ex-prisoners experience subsequent, informal sanctions is not merely a selection effect reflecting their initial unfavourable characteristics, rather it is a treatment effect of spending time in prison. These informal sanctions may represent the stigma experienced by the ex-prisoners, where a potential employer extrapolates from the knowledge of the person's criminal record to his or her skills as an employee. Or they may result from the eroding of job skills and the reduced possibilities of acquiring labor market experience that imprisoned individuals are likely to face. Regardless of the mechanisms at stake, it seems obvious that the potential harmful effects also described and discussed in the background section, do not dominate the overall impact of community service on the various outcomes studied here.

Conclusion

The overall conclusion that we may derive from the analyses presented above is that the type of sentence that offenders serve—either custodial or non-custodial—does matter. Spending time in prison does seem to have a negative effect on offenders, at least compared to alternative, non-custodial sanctions, such as community service. In particular, community service seems to affect the labor market prospects of ex-offenders, by reducing the risk of unemployment and increasing income. There is less evidence that serving a community service sentence instead of going to prison affect desistance, even though conclusions regarding this outcome are less clear.

Importantly, my study does have a number of limitations. First, my identification strategy relies on a number of assumptions that may not be fulfilled. Using the propensity score matching we must accept the assumption of selection on observables, and using the

difference-in-difference model, we rely on the assumption of parallel time trends. Both assumptions may be violated, however my robustness checks demonstrate that possible violations are unlikely to change my results in any significant way.

Second, one may speculate on the external validity of my results, with regards to both whether they generalize to other Danish offenders and offenders subjected to other criminal justice systems, like e.g. the American. With regards to generalization to other Danish offenders, the estimated differences between offender types in the effect of community service presented in this paper, is an indication that we cannot expect this type of non-custodial sentence to affect all offenders similarly. Effect will vary by offender type, because offenders of different types of crime have different personal and social characteristics that will interact with the effect of the community service. However, my results do seem to indicate that community service is more beneficial to offenders with a strong labor market affiliation, and this result may be valid also for offender groups not studied in this paper.

Also we need to speculate whether evidence from a 14-year old reform provide any insights into the present day situation of Danish offenders. Unfortunately we cannot infer the answer to this question from the results presented in this paper. Future studies should test whether punishment regimes, offender characteristics and the extent of informal punishment in the Danish society has changed to such an extent that community service is likely to have significantly different implications today.

It is also difficult to say whether my results generalize to offenders subjected to other criminal justice systems, like the one found in the US. The Danish and the US criminal justice system differ on quite a range of parameters, of which the average duration of prison sentences is one of the most significant. In a US context, offenders comparable to the Danish offenders studied in this paper would be subjected to significantly longer prison sentences, and community service would then replace a far more encompassing prison experience. Thus, if the informal punishment that follows from a prison stay is as strong in the US as it is in Denmark, and the severity of the informal punishment is correlated with the length of the prison stay, the use of community service may prove to be even more beneficial in a US context

However, even with these limitations, the current study does show nonnegligible evidence that incarceration reduces offenders' possibilities of re-entering and contributing to society. Hereby the use of imprisonment as a punishment for both more and less serious crimes is not only costly during the incapacitation, but is also costly in the long run as it weakens the labor market affiliation and thus the contribution to society of the former prisoners. Thus, more initiatives like the extended use of community service and electronic monitoring would be highly useful.

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