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PREVENTING CRIME THROUGH SELECTIVE INCAPACITATION

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PREVENTING CRIME THROUGH SELECTIVE INCAPACITATION

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ABSTRACT: Making the length of a prison sentence conditional on an individual's offense history is shown to be a powerful way of preventing crime. Under a law adopted in the Netherlands in 2001, prolific offenders could be sentenced to a prison term that was some ten times longer than usual. We exploit quasi-experimental variation in the moment of introduction and the frequency of application across 12 urban areas to identify the effect. We find the sentence enhancements to have dramatically reduced theft rates. The size of the crime-reducing effect is found to be subject to sharply diminishing returns.

JEL Classification: K42, K14

Keywords: prolific offenders, incarceration, selective incapacitation, three strikes laws

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1 Introduction

In Steven Spielberg's movie *Minority Report*, the so-called "Precrime" police unit apprehends individuals before they are able to commit a planned or accidental criminal act. The officers from the unit have access to special powers that allow them to look into the future. By incapacitating potential offenders they alter the course of events and effectively ban out crime in Washington DC, the area in which the movie is set.

In everyday reality, law enforcement has devised similar ways of preventing crime. It is common practice to make prison sentences dependent on the predicted future criminal activity of an offender. In the courtroom, the predictor of choice of future criminal behavior is an offender's prior record. The assertion is that earlier criminal activity makes it more likely that a given individual will commit crime in the future. Clearly, law enforcement is too late when an individual has already committed a crime, but with the predictive power of the prior record in mind, enhanced prison sentences for repeat offenders could at least prevent further criminal activity for the duration of the sentence (Greenwood and Abrahamse 1982).

Enhanced prison sentences for repeat offenders can be seen as a separating contract that helps law enforcement to incapacitate those that are most criminally inclined. Individuals reveal themselves to be of the high offense propensity-type by not being deterred by the punishment for first-time offenders.¹ Enhanced prison sentences for only small numbers of offenders could lead to major crime reductions as the distribution of criminal acts over

¹ A policy of selective incapacitation should be distinguished from a policy that makes punishment dependent on the seriousness of the offense committed. If having committed a serious offense is a predictor of committing another offense and possibly also of how serious the future offense will be, then making the use and the duration of prison sentences dependent on the seriousness of the offense is another example of a separating contract (see Shavell 1987).

individuals is highly skewed (Piquero, Farrington, and Blumstein 2007). As worked out by Cook (1986), the crime-reducing effect will depend on the behavioral response of other offenders – who may not only respond to opportunities for crime that now go unexploited but also to the shift of criminal justice resources towards prolific offenders – and the behavioral response of potential victims – who could take fewer precautions to protect themselves against crime. Being highly selective in incapacitating offenders is essential for the welfare impact of the policy because of the high costs related to prison sentences and for humane reasons. Higher punishment for repeat offenders can also correct under-deterrence of the most criminally inclined without the need of raising punishment across the board, which may be prohibitively costly (see Polinsky and Rubinfeld 1991 for an information-based argument, and Polinsky and Shavell 1998 for a similar result based on a model with identical offenders).

Enhancing prison sentences for repeat offenders with the explicit goal of preventing crime through incapacitation has quickly gained popularity among lawmakers across the world. In the US, so-called habitual offender laws have been in place in most states since the 1970s. Some states have been particularly active in applying these laws, including Texas, Florida since 1988, and most famously, California since 1994. In these statutes – in their recent incarnations commonly referred to as ‘three strikes’-laws – a second or third conviction for a serious crime could lead to extremely long prison sentences. Policies of selective incapacitation have been adopted in many other countries as well, including the UK in 1997, Australia in 1997, the Netherlands in 2001, Hungary in 2009, and New Zealand in 2010.

The fierce debate about the costs and benefits of California’s three strikes law (Zimring, Hawkins and Kamin 2001, Shepherd 2002, among others) suggests that it is anything but

trivial whether a policy of selective incapacitation improves social welfare. First of all, the welfare impact of selective incapacitation depends on existing sentencing policies. If there are decreasing returns to the rate of incarceration, for which Liedka, Morrison Piehl and Useem (2006) and Johnson and Raphael (2007) provide evidence, then further sentence enhancements may provide a small crime-reducing effect at high cost.² As incidence of criminal activity has been found to be decreasing with age (Farrington 1986), enhancing already long prison sentences may incapacitate offenders who would have refrained from crime anyway for most of the extra time that they spend in prison. Second, the welfare impact of the sentence enhancements depends on the scale at which the policy of selective incapacitation is applied. Casting the net wide by incapacitating great numbers of repeat offenders for a long time is likely to also affect offenders with relatively low criminal inclinations, which will lower the crime-reducing effect at the margin and negatively affect the net social benefit of the policy (Greenwood et al. 1994). California's law is particularly extreme on this last dimension. It is not very selective. One or two prior convictions for a wide range of crimes can trigger extreme prison terms of up to a 25-years or life for a third strike. Consequently, the law affected great numbers of offenders, and resulted in a further increase in the incarceration rate. Between 1994 and 2005, some 100,000 prolific offenders were sentenced under the law. By 2010 some 41,000 prisoners were serving time under the two or three strikes provisions of the law, equal to some 25 percent of the California prison population (California Department of Corrections and Rehabilitation, 2010).

² In other words, in some cases reducing punishment for first-time offenders rather than increasing punishment for repeat offenders can be regarded as a policy of selective incapacitation (Polinsky and Shavell 1998, 313).

Empirical evidence on the incapacitation effect of habitual offender laws is virtually non-existent. Most of the empirical work into selective incapacitation relates to California's 1994 three strikes law. So far no reliable estimates of the incapacitation effects of the law are available.³ Empirical studies tend to focus on the deterrent effect instead. Using data on arrest profiles of criminals who were released from prison in 1994, Helland and Tabarrok (2007) identify the deterrent effect by comparing re-arrest rates of individuals convicted for two versus one strikeable offense. The two groups are made comparable by selecting those individuals who were tried twice for a strikeable offense, with some of them convicted for a strikeable offense only once, perhaps due to a plea-bargain. They find that the law reduces felony arrest rates among individuals with two strikes by 20 percent. Iyengar (2008) compares re-arrest rates of offenders with a similar criminal record pre and post adoption of the law, but a different order of crime commission. Strikes only start counting after an initial strikeable offense.⁴ Iyengar finds that the law reduced re-offending by 20 percent for second-strike eligible offenders and by 28 percent for third-strike eligible offenders.⁵

³ Marvell and Moody (2001), Kovandzic, Sloan, and Vieraitis (2004), Worrall (2004) and Chen (2008) try to capture some of the incapacitation effects of the three strikes law by including a post-passage linear time trend. As the number of offenders sentenced under the law may well follow a non-linear time trend, and no supporting evidence for the assumption of linear growth is provided, the estimated coefficient of this term is hard to interpret.

⁴ For instance, since burglary is a strikeable offense and theft is not, a theft counts as a second strike after a burglary, whereas for someone who is convicted for a theft first, being convicted for a burglary after the theft will be a first strike.

⁵ In contrast to the evidence for a deterrent effect of the law based on individual criminal records, the evidence based on a comparison of trends in aggregate crime rates is mixed. Marvell and Moody (2001), Kovandzic, Sloan, and Vieraitis (2004) and Chen (2008) identify the crime-reduction effect of three strikes laws using variation in aggregate crime trends across states or cities and a binary variable denoting the passage of the law in a state. Given the great variation in application of the

In a rare study outside the California context, Kovandzic (2001) evaluates the Florida habitual offender law, using county-level variation in aggregate crime rates during 1980-1998. A 1988 revision of the law made repeat offenders no longer eligible for early release. The Florida law has some similarities with the California three strikes law.⁶ Kovandzic finds statistically significant but small negative effects of this incapacitation variable on a number of crimes: four additional months in prison prevent one crime. He finds no evidence for a deterrent effect of the law. Given the presence of simultaneity in the relation between application of the law and county crime rates, the results are likely to be biased towards zero.

Given the paucity of evidence on the incapacitation effects of sentence enhancements for prolific offenders, and the limited empirical evidence outside the California context, experiences with a policy of selective incapacitation elsewhere should be particularly

policy across states, the results are found to be highly heterogeneous – and hard to interpret. Using county-level data for California, Shepherd (2002) improves upon the aforementioned studies by including an indicator of how aggressively the three strikes law has been applied locally. The probability of a three strikes sentence is defined as the number of individuals receiving three strikes sentences divided by the number of offenders imprisoned. Based on data for 1983-1996, she finds evidence for a strong deterrent effect of the law on strikeable offenses. In a follow-up study with similar county-level data for 1989-2000, Worrall (2004) reaches the opposite conclusion. As deterrence variables he uses the number of three strikes sentences divided by the number of arrest for index offenses, and the percentage of people held in custody. Worrall finds only a negative effect of the law on larceny (*ibid.*, 290, Table 2), the only crime for which Shepherd (2002) finds a positive effect of the law (which she explains by displacement towards non-strikeable offenses, including larceny).

⁶ Ten years after revision of the Florida habitual offender law, habitual offenders accounted for some 21 percent of the total prison population (Kovandzic 2001, 183), which is not unlike the 25 percent share of three strikes offenders in the California prison population in 2010. The sentence enhancements were also large (for instance, some 6.5 additional years for burglary and 11 additional years for robbery).

informative. In this paper, we estimate the crime-reducing effect of a habitual offender law adopted in the Netherlands in 2001. Offenders with ten or more offenses on their criminal record faced sentence enhancements of some 1,000 percent. Between 2001 and 2007 1,400 mostly non-violent offenders were sentenced under the law. By the end of 2007, these offenders made up 5 percent of the inmate population. To identify the incapacitation effect we relate the number of offenders serving time under the law to variation in the aggregate crime rate in the area where they were found to be criminally active. By relating the policy to aggregate crime rates rather than individual offender behavior, our analysis allows for the earlier mentioned effects of incapacitation of offenders on the behavior of other offenders and potential victims (see Miles and Ludwig (2007) for a further discussion of why the incapacitation effect is best studied by relating sentencing policies to aggregate crime rates). We exploit the fact that adoption of the law happened under quasi-experimental circumstances. The moment of introduction was not left to localities to decide. The national government allowed ten cities to experiment with the policy first, with the selection based on the number of prolific offenders known to be active in an area rather than the local crime trend. Only after a number of years other cities were allowed to apply the law. The rate of application was not related to local crime trends either. Rather, it was greatly affected by implementation problems, including initial reluctance of judges to apply the law. In the longer run, variation in the rate of application is strongly related to the number of prolific offenders active in an area. We argue that variation in application of the policy across urban areas can be seen as exogenous to local crime trends, allowing us to identify the causal effect of the law in a panel data model with area-fixed effects. Using monthly data for 12 urban areas between 2001 and 2007 we find the sentence enhancements to have resulted in a dramatic drop in the rate of theft from car and rate of burglary. We find evidence for

rapidly decreasing returns to scale in application of the policy. The greater the number of offenders serving long-term prison sentences under the law, the smaller the crime-reducing effect at the margin. At current levels of application, the benefits of the policy are estimated to exceed the costs.

Even though it is often difficult to empirically distinguish between deterrence and incapacitation in an analysis that relies on aggregate crime data (Miles and Ludwig 2007), we argue that our results are related to incapacitation and not deterrence. First and most importantly, it is hard to imagine a deterrent effect that varies on a monthly basis with the number of prolific offenders serving enhanced prison sentences, the policy variable in our analysis. Second, in this specific case a deterrent effect is likely to be largely absent. Judges were highly selective in applying the law. Only drug-using, older individuals who had many more than the minimal number of ten offenses on their record and were practically immune to treatment had a chance of being sentenced under the law. This particular group of individuals can be characterized as the most hopeless of all prolific offenders and is difficult to deter as they had little to no alternative options to a life of crime. They relied on theft for a living and to maintain their drug habit, many of them for an extended period of time, sometimes up to twenty years. After two decades of largely failed attempts to address the crime problem created by this group of offenders, long-term incapacitation rather than deterrence was mentioned by policymakers as the primary rationale behind the introduction of the habitual offender law (Vollaard 2010).

The contribution of this paper is twofold. First, we add to the empirical evidence on the impact of selective incapacitation on crime, in particular the size of the incapacitation effect. The existing literature tends to either focus on the incapacitation effect of sentence

enhancements for all offenders (Owens 2009) or, as discussed above, on the deterrent effect of sentence enhancements for repeat offenders. We collect detailed data on the application of the policy at the geographical level at which the prolific offenders are active, the urban area. In addition, we are explicit about the quasi-experimental nature of the variation that we use to identify the effect of the policy. As such, we provide the first evaluation of a habitual offender law adopted in a situation of incarceration rates that were much lower than in the US in the 1990s. In addition, the Dutch law was much more selective in nature than the California three strikes law, which we argued to be an important determinant of the welfare impact of a policy of selective incapacitation. Second, our paper provides unique evidence on decreasing returns to scale in sentence enhancements for repeat offenders. We were able to obtain the criminal records of all of the offenders convicted under the law. We show higher rates of application of the policy to go together with the incapacitation of offenders with relatively less extensive criminal records. Thus far the literature on diminishing returns to incarceration is based on estimating the crime-prison relationship at different rates of incarceration in the US (Liedka et al. 2006, Johnson and Raphael 2007). This type of indirect evidence allows for an alternative interpretation of the findings. The diminishing crime-reducing effect of incarceration could be due to the steep drop in criminal activity in the 1990s and 2000s in the US rather than the higher rate of incarceration. Our analysis focuses on a relatively homogeneous group of offenders over the course of a few years, which leaves the increased scale of the sentencing policy as the only explanation for diminishing returns.

The remainder of the paper is organized as follows. Section 2 introduces the habitual offender law adopted in the Netherlands. Section 3 describes the data. Section 4 presents the identification strategy and section 5 presents our parameter estimates, including a

variety of sensitivity tests. We conduct a cost-benefit analysis of the policy in section 6. Section 7 concludes.

2 The habitual offender law

The Dutch habitual offender law came into force in April 2001. Under the law, judges were given discretion to sentence offenders with ten or more offenses on their criminal record to a prison term of two years. As the months of pre-trial detention were not subtracted from the sentence, the prison term usually totaled some three years. Any offense excluding misdemeanors counted towards the prior record that was used to convict an offender under the law. Given prison sentences of a few weeks or months for the offenses that typically triggered application of the law such as burglary and theft from car, a prison term of 24 to 36 months was a major sentence enhancement.

Application of the law was anything but mechanic. A judge could only sentence an offender to an enhanced prison term if the prosecutor together with the probation council could provide evidence that other means of preventing future offending, such as drug treatment, had failed. The law was meant as an *ultimum remedium*. In 2001, nationwide some 3,000 to 4,000 offenders could be classified as prolific under the law (on a total population of the Netherlands of 16 million). In practice, only a select number of these offenders had a chance of being convicted under the habitual offender law. Because of the *ultimum remedium* provision, judges almost exclusively sentenced drug-using, older individuals under the law for whom there was thought to be no hope of preventing high-rate offending by any other means than incapacitation.

Most of the convicted offenders were not able to maintain a normal life style. They were out of work and did not have stable housing. They committed theft for a living, collecting a daily

income of some 50 to 100 euro (\$70-130) to be able to maintain their habit, which implies stealing property valuing some 300 to 600 euro (\$400-800) on a daily basis. By 2001, many of these highly prolific offenders were aged 40 or over: they had fallen victim of the heroin-epidemic that swept Europe back in the 1980s. The offenders spent some three to four months in prison each year in absence of the new law, and some had as many as 300 offenses on their criminal record. On average, offenders had been convicted 31 times prior to being sentenced under the habitual offender law (Koeter and Bakker 2007, Table 3.7, 71)

The enhanced prison sentence was not only meant to reduce crime through incapacitation but to provide a window for coercive treatment as well. Incarceration was often combined with drug treatment and other rehabilitative services, such as social skills training. Evaluations of the law suggest the treatment programs had little effect on recidivism (Koeter and Bakker 2007), an issue we will return to in section 4.

3 Data

Data on the monthly inflow and outflow of offenders into and out of prison facilities for prolific offenders between January 2001 and December 2007 were obtained from the Department of Corrections (DJI). The prisoner-level data denotes the start of pre-trial detention rather than the start of the extended two-year sentence, which is helpful since that date denotes the start of the actual prison term. If the term starts after the 15th of the month, we assign the prisoner to the inflow of the next month. To obtain our policy variable that denotes the extra time that the convicted offenders are serving compared to the sentences they would have received in absence of the habitual offender law, we start counting as of the fifth month of detention. As discussed in section 2, the offenders spend on average some three to four months per year in prison in absence of the law.

As shown in Figure 1, growth in the number of offenders convicted under the habitual offender law happened in two waves. The first wave started when the law was introduced in ten cities in 2001. The second wave started at the end of 2004 when prison capacity was greatly extended to allow for country-wide application of the law and lasted until mid-2007.

[FIGURE 1 ABOUT HERE]

To determine in which urban area the offenders were criminally active, we obtained the criminal records for all offenders. The criminal record includes data on the locality in which each offense was committed. For the analysis, we used the universe of 31 major cities in the Netherlands (the so-called G31 cities) and 4 neighboring cities. Because of data limitations, we had to exclude four cities in the very north of the country (Alkmaar, Groningen, Leeuwarden, Emmen). Based on the offenses committed during 1998-2008 we matched every convicted offender to one of the remaining 31 cities. Of the 1,407 offenders that had been sentenced to an extended prison term during 2001-2007, 85 percent were criminally active in one of 31 cities. The other offenders were either active in other cities or their whereabouts were uncertain. The match shows that offenders tend to operate very locally. Their working area is mostly limited to one city, but may extend to neighboring cities at a distance of at most 15 kilometers (9 miles). Directly neighboring cities tend to have a particularly dense network of public transport connections, allowing the offenders to extend their working area to another city while keeping the downtown area of the city where they live as a base. Because of the overflow to nearby cities, we grouped the 31 cities into 12 urban areas consisting of 2 to 4 co-located cities.⁷ The offense data show that 85 percent of

⁷ The 12 urban areas include: Amsterdam-Zaanstad-Haarlem, Rotterdam-Schiedam-Vlaardingen-Dordrecht, Den Haag-Zoetermeer-Leiden, Utrecht-Amersfoort, Arnhem-Nijmegen, Breda-Tilburg-'s-

all offenses known to the police were committed within the urban area an offender has been assigned to. The other 15 percent of offenses were mostly committed in smaller communities directly bordering the urban area.

Monthly data on reported and recorded theft at the city level were provided by the Netherlands police. We selected police recorded theft from car and domestic burglary: these are offenses typically committed by the offenders defined as prolific under the law. Because of low and varying reporting rates, we excluded two other offenses popular with these prolific offenders: bicycle theft and shoplifting.⁸ We were able to construct consistent time series for all urban areas for the period January 1999 to December 2007. Table 1 provides summary characteristics for the variables used in the analysis.

[TABLE 1 ABOUT HERE]

4 Identification

The aim of this paper is to identify the incapacitation effect of the habitual offender law.⁹ Ideally, to identify the effect, prolific offenders should only receive the enhanced prison

Hertogenbosch, Eindhoven-Helmond, Enschede-Hengelo-Almelo, Maastricht-Heerlen-Sittard-Geleen, Lelystad-Almere, Venlo-Roermond, Deventer-Zwolle.

⁸ Comparing police recorded crime and data from victimization surveys suggest that some 85 percent of bicycle theft and more than 90 percent of shoplifting goes unreported.

⁹ We do not attempt to estimate the deterrent effect of the policy. As discussed in the introduction, deterrence effects are best studied with individual offender data, not with aggregate crime data. Kessler and Levitt (1999) propose to study the crime rate immediately following the passage of the law as a way of identifying the deterrence effect. In their reasoning, this short-term effect can only be attributed to deterrence as it will take longer before incapacitation starts affecting the crime rate. In our case, it is uncertain whether the deterrent effect is concentrated at the moment of introduction in an urban area or materializes when a greater number of prolific offenders is convicted under the law. Moreover, given the short prison terms in the Netherlands, the

sentences in a group of randomly selected urban areas, keeping the other urban areas as controls (assuming that the offenders remain in the same area, an issue we return to later in this section). In addition, the urban areas in the treatment group should work towards incapacitating a randomly selected target level of prolific offenders. Under these conditions, both the moment of introduction of the law and the rate of application of the law in an urban area would be independent from the local crime trend. The causal effect of the law could then be identified by comparing the change in the crime rate of the urban areas with the policy to those without the policy – while taking into account variation between urban areas in the number of offenders that were convicted under the law.

Although no such experiment was conducted, implementation of the law happened under circumstances that closely resemble this experiment. First of all, the moment of introduction of the policy in an urban area was not left to the local authorities to decide. Rather, to experiment with the new policy, the national government restricted application of the law to ten cities initially. As a result of a conflict about funding, one of these ten selected cities, The Hague, only introduced the measure in 2004. After the initial period of 3.5 years, all other cities were allowed to try offenders under the habitual offender law, something they all started to work on almost immediately. Figure 2 shows that the date at which the first prolific offender was sentenced under the law in an urban area is strongly related to the decisions that were made at the national level.

incapacitation effect may be felt in the crime rate shortly after introduction of the policy. Shepherd (2002) identifies a deterrent effect of the California three strikes law using data on the probability of apprehension. We do not have monthly data on the population of offenders that can be qualified as prolific under the criteria of the law, however. Without a proper way of identifying the deterrent effect, we abstract from it in the analysis.

[FIGURE 2 ABOUT HERE]

Selection of the initial group of cities was based on the number of prolific offenders known to the police, rather than the local crime trend. In other words, we should not see an upward crime trend prior to the moment of introduction. Figure 3 shows the crime trends up to the moment of introduction for the three groups of urban areas distinguished in Figure 2. As discussed in Section 3, we look at crimes typically committed by offenders sentenced under the habitual offender law: theft from car and domestic burglary. As expected, the crime trends shown in Figure 3 show no relation with the moment of introduction of the law.

[FIGURE 3 ABOUT HERE]

The rate of application of the habitual offender law in an urban area was also based on the number of prolific offenders in the area, rather than the local crime trend. During the first wave, the national government used the number of prolific offenders in an area in 2000 to allocate prison capacity across the urban areas. The cities steadily worked towards filling the allotted capacity. The number of prolific offenders convicted under the law in an urban area at the end of the first wave was strongly correlated with the allotted prison capacity (pairwise correlation coefficient of 0.85). Shortly into the second wave, when it appeared that capacity was abundant, all capacity constraints were lifted. Still, the number of prolific offenders convicted under the law at the end of the second wave remained strongly correlated with the number of prolific offenders known to the police at the beginning of the second wave (pairwise correlation coefficient of 0.85). Figures 4 and 5 show how the urban areas steadily worked towards incapacitating a target number of prolific offenders, after which growth in application of the law leveled off.

Short-term adjustments to the target level of offenders to be long-term incapacitated were practically infeasible – which also explains the gradual growth in application shown in Figures 4 and 5. Introduction of the law was plagued with many implementation problems that took several years to resolve. Getting prolific offenders through the system took considerable effort, including intensive coordination between the multiple agencies involved in identifying, arresting, and making a case against a prolific offender. These efforts were often frustrated by unwilling judges. Initially, many judges were highly reluctant to apply the long-term prison sentences, and did not follow the sentence demanded by the public prosecutor in many cases.¹⁰ Only after some positive experiences with what the long-term prison sentences meant for the mental and physical health of the targeted offenders (through observing a regular day and night rhythm, eating proper food for a number of months, conversations with case workers, treatment) judges became more willing to convict offenders under the habitual offender law (Vollaard 2010). Witnessing the gradual growth in Figures 4 and 5, progress was slow. Clearly, it was not the local crime trend that was driving decisions of judges, but feedback from incarcerated offenders.

[FIGURES 4 AND 5 ABOUT HERE]

Since both the moment of introduction and the rate of application in an urban area can be argued to be independent from local crime trends during 2001-2007, variation in application of the law across the twelve urban areas can be seen as a natural experiment. The causal effect of the policy can then be identified by comparing changes in crime rates across urban areas. The key prediction is that more intensive use of the policy goes together with a more

¹⁰ Evidence from several cities suggests that initially judges sentenced repeat offenders to the enhanced prison terms in 1 in 3 or 1 in 4 cases that the sentence was demanded (Vollaard 2010).

favorable trend in the local crime rate. At the end of this section, we discuss three additional assumptions underlying a causal interpretation of the results, including independence from other policies, the local nature of the criminal activities of the offenders, and a negligible effect of treatment on recidivism rates.

As a first step towards identifying the effect, we graphically examine how crime rates changed after introduction of the policy. We specify an event time model that allows us to non-parametrically estimate the average trend in crime rates before and after the moment of introduction. As all urban areas gradually increased the number of long-term incapacitated prolific offenders, introduction of the policy can be seen as a discrete event.

We estimate the following equation:

$$C_{j,t} = \sum_{\tau=-T}^T \alpha_{\tau} W_{\tau} + X_{j,t} \beta + \gamma_j + \Phi_y + \delta_q + \varepsilon_{j,t} \quad (1)$$

The dependent variable is the rate of recorded theft per 10,000 population in urban area j in month t . The event time indicator variables W_{τ} track the month when the policy is introduced in an area and the months preceding and following the introduction. The indicator variable W_0 equals 1 for the month that the policy is introduced in one of the areas and is zero otherwise; W_1 equals 1 for the first month after the introduction and is zero otherwise, and so on. Because our data cover an 8-year period and most urban areas introduced the policy within 3 years from January 1999, τ is capped at 3 years before and after the moment of introduction. We bin the event time indicators at both tails of the 3-year period. To compare changes in the crime rate over time rather than differences in crime levels between urban areas, we include urban area fixed effects γ_j . The area fixed effects prevent estimation bias from unobserved factors that remained approximately stable over the study period and that caused crime rates to differ across urban areas. Think of the locally

available capacity for voluntary treatment of addiction to drugs, for instance. X is a vector of time-varying characteristics of urban areas, including the quarterly rate of unemployment and, to control for broader changes in crime policy, the monthly number of arrests per 10,000 population. Other area characteristics such as age structure and ethnic composition of the population were not included as they are only available at an annual frequency and show little to no variation within the 8-year period covered in the analysis. In the sensitivity analysis, we show our results to be robust to including area-specific linear time trends. The year fixed effects Φ_y control for events that could raise or lower crime rates in a given year across the twelve urban areas. To prevent estimation bias from seasonal effects, we also include quarter fixed effects δ_q .

Figure 6 plots the event time indicator coefficients α_τ from equation (1). Event time is plotted on the horizontal axis. Event time zero corresponds to the month the policy is introduced in an urban area. The plotted event time coefficients denote the average crime rate in month τ relative to the crime rate at the moment of introduction – while controlling for urban area, year and quarter fixed effects, the rate of unemployment and the arrest rate. Figure 6 suggests that introduction of the policy resulted in a drop in the crime rate. We do not see a discrete drop in the crime rate around the moment of introduction, which is to be expected since the policy had a very slow and gradual start. The incapacitation effect of the law was slow in coming, which explains the rather gradual change in the crime trend shown in Figure 6. Three years after the introduction, the average crime rate was reduced by some 5 thefts per 10,000 population. Given an average rate of theft of some 25 per 10,000 population during 1999-2000, the event time analysis suggests that the policy had a major impact on the crime trend.

[FIGURE 6 ABOUT HERE]

To identify the effect on the crime rate of convicting one additional prolific offender under the habitual offender law, we need to be more specific about the rate at which the law was applied once it was introduced. In addition, we have to specify the nature of the relationship between a change in the number of offenders incapacitated per capita and the crime rate. We estimate the following equation:

$$C_{j,t} = \alpha \text{PRISON}_{j,t} + X_{j,t} \beta + \gamma_j + \Phi_y + \delta_q + \varepsilon_{j,t} \quad (2)$$

Policy variable *PRISON* denotes the number of prolific offenders per 10,000 population from area *j* incapacitated under the law in month *t*. As noted in Section 3, we start counting as of the fifth month of detention to approximate the extra time that offenders are serving in prison.¹¹ Our parameter of interest is α , which denotes the change in crime resulting from one additional offender serving a long-term sentence (both per 10,000 population). Equation (2) assumes a linear relationship between the policy variable and the crime rate. As we will see in the next section, we find strong evidence for decreasing returns to scale once we allow for a non-linear relationship.

A causal interpretation of the parameter of interest in equation (2) relies on three additional assumptions. First of all, implementation of the habitual offender law in the 12 urban areas should not be related to any other policy that may have affected crime. The effect estimated with equation (2) is relative to the existing policies in dealing with prolific offenders, such as government provision of heroin and methadone to registered drug users, a common practice

¹¹ Allowing for a greater than 5 months-lag does not affect the results. Shortening the time window reduces the size of the effect, what is to be expected since the incapacitation effect of the law is driven by *enhanced* prison terms.

in the Netherlands. If stable over time, differences in alternative policies across urban areas could lead to some heterogeneity in the results but should not bias estimation of the incapacitation effect.¹² During 2001-2007 no other major policy initiatives targeted at the specific group of prolific offenders affected by the habitual offender law were launched. In other words, the extended prison terms substitute any alternative short-term punishment and treatment that offenders may have received in absence of the law. In a broader campaign against crime, other criminal justice policies were intensified during 2002-2005. The policies were mainly aimed at tougher policing of delinquent youth and forwarding a greater number of criminal cases from the police to the public prosecution council (Vollaard 2010). These initiatives are partly reflected in the arrest rate, included as covariate in equation (2), which shows a rise and fall during 2002-2005. In the sensitivity analysis, we show our results not to be affected by other policies during the first wave of application of the habitual offender law: the estimated incapacitation effects pre and post 2005 are similar.

Second, we assume the targeted offenders to remain in the same urban area. Evading arrest by moving to another area that is also included in our analysis may lead us to overestimate the incapacitation effect. The offenders affected by the law were hampered in their ability to move elsewhere because of their reliance on local networks of drug dealers and middlemen that buy stolen goods. Anecdotal evidence from police officers suggests that the prolific offenders tend to be well-known to local law enforcement. Police officers know perfectly well how the offenders make a living. Regular arrests do not make the offenders to move away: the gate of the local prison is a 'revolving door'. The introduction of the habitual offender law was very gradual and did not change these local dynamics (Vollaard 2010). In

¹² We cannot explicitly model other policies of dealing with prolific offenders because data on these policies are not systematically collected at the city-level.

line with the anecdotal evidence, in the next section, we show that the number of prolific offenders active in a locality goes down proportionally with an increase in the number of prolific offenders from that same locality that is serving an enhanced prison sentence. Moreover, in the sensitivity analysis, we show the size of the effect to be similar pre and post 2005, whereas opportunities to evade a long-term prison sentence by moving to another area only existed up to the end of 2004. This finding also suggests that offenders remained active in the same urban area.

A final assumption underlying our method of identification is that treatment during the enhanced prison sentence had no or a small effect on the rate of offending after release. This assumption is not unrealistic given the available evidence (Mitchell et al. 2006). In the presence of a strong treatment effect an inflow of prolific offenders has a larger effect in absolute terms on the local crime rate than an outflow. In terms of equation (2): the effect on the crime rate from a net monthly increase in the number of offenders sentenced under the law will then be higher than the effect of a net monthly decrease. Parameter α in equation (2) can be interpreted as the average effect of net increases and net decreases in the number of prolific offenders incapacitated. If treatment has an effect on recidivism rates, then our estimate of the incapacitation effect will be biased towards zero. In the sensitivity analysis we conduct a test of whether the effect on the local crime rate from net increases is different from net decreases in the policy variable. We find similar effects of net increases and net decreases, suggesting that our assumption of a negligible treatment effect is valid.

5 Estimation results

Table 2 presents the estimation results based on equation (2). The parameter estimate for the number of prolific offenders incapacitated under the habitual offender law has the

expected negative sign. Under the assumption of a linear relationship between the policy and outcome variable we are not able to precisely estimate an average marginal effect, however. As discussed previously, incapacitating greater numbers of prolific offenders for an extended time is likely to reduce the crime-reducing effect at the margin. To allow for a non-linear relationship between the policy variable and the crime rate, we estimate the average marginal effect at four regular intervals in the second column. In the sensitivity analysis we show that the results are similar when we estimate the average effect at seven rather than four intervals. We find strong evidence for diminishing returns. The incapacitation effect is lower at higher rates of application of the policy, which is what we expect. The effect can now be more precisely estimated. The difference between the high-end and low-end estimates is statistically significant.

Given an average number of prolific offenders that are serving long-term prison sentences of 0.55 per 10,000 population, the estimation results suggest that incapacitating one additional prolific offender results on average in some 9 fewer recorded thefts per month, which is equal to more than 100 recorded thefts per year. With a total number of recorded thefts of 176,000 per year in 2001, the 1,200 prolific offenders that were at some point serving time under the law in one of the 12 urban areas during 2001-2007 account for some 70 percent of these two types of theft, which is similar to what Machin and Marie (2009) find based on self-reported crime data. Clearly, this finding reinforces the basic tenet underlying a policy of selective incapacitation that only a few offenders are responsible for a large proportion of all crime.

[TABLE 2 ABOUT HERE]

We provide separate estimates for theft from car and domestic burglary in the last two columns of table 1. The effect on theft from car is statistically significant at all rates of application of the policy; the effect on burglary is statistically significant for the high-end and low-end estimates. It is striking how different the estimates are: the effect on theft from car is much larger than the effect on burglary. The difference is statistically significant. A possible explanation for this difference is the high return to burglary relative to theft from car. The typical prolific offender needs to collect a daily income from theft. The value of stolen goods is a factor of three to four times higher for burglary than for theft from car (Home Office 2005). In other words, to achieve the income target, the offender needs to commit fewer burglaries than thefts from car.

To illustrate the size of the estimated incapacitation effect, Figure 7 plots the simulated trend of the rate of theft in absence of the law. The dotted line shows what the theft rate would have been if the policy had not been adopted. We take into account the diminishing returns reported in Table 2. The simulation suggests that crime went down during 2002-2005 also for other reasons than the habitual offender law, probably as a result of other policy initiatives that were discussed in the previous section. The crime drop during 2002-2005 can be observed in most urban areas and is captured in the year-fixed effects that are included in the estimation equation. Figure 7 shows that the incapacitation effect of the law grows as long as greater numbers of offenders are sentenced to the enhanced prison terms, which is until shortly before the end of 2007. By that time, the rate of theft is some 30 to 40 percent lower as a result of selective incapacitation of prolific offenders. The size of the drop in crime corresponds with the results of some back-of-the-envelope calculations. If 1,200 offenders are responsible for 70 percent of crime, as we argued above, then the close to 700 offenders serving time under the law by mid-2007 are responsible for 40 percent of crime.

That percentage may actually be somewhat higher, as crime has been coming down for other reasons during 2001-2007. The crime-reducing effect of the law is smaller than this percentage share, however, since some of the offenders would have been doing time also in absence of the law. Assuming 8 additional months of incarceration per year as a result of the law, the drop in crime can be put at some 30 percent, which is close to what we find in the simulation.

[FIGURE 7 ABOUT HERE]

Supporting evidence for the estimated incapacitation effect

Our findings are roughly in line with what is known about the criminal activities of this particular group of offenders. An average estimated effect of some 100 recorded thefts per year implies some 120 incidents of theft since not all theft is reported and recorded. In a self-report study, offenders sentenced under the habitual offender law admitted to 256 cases of theft per year on average (Koeter and Bakker 2007, 71, Table 3.7). The authors note that the self-reported number of thefts is likely to be an underestimate of the actual number of thefts. Our results suggest that theft from car and burglary account for about half of the self-reported total. Shoplifting and bicycle theft, the two other property offenses popular with these prolific offenders, can easily account for the other half. If the average haul of theft is what drives offending patterns, then the number of thefts from shops and bicycle thefts prevented through incapacitation is likely to be at least as high as the number of thefts from car and burglaries.

Supporting evidence for a reduction in the number of active prolific offenders

The police provided us with annual data on the number of prolific offenders aged 25 or over that were active in each of the urban areas during 1996-2007. The definition of a prolific offender used in the police data is similar to the definition used in the habitual offender law (ten or more offenses committed over the last five years, of which one in the current year). The data on the offender population allow us to verify the estimation results reported in Table 2. If the policy was effective in lowering crime by way of the incapacitation effect, then we should see lower numbers of active prolific offenders when higher numbers of offenders are serving long-term prison sentences. We estimate the following equation:

$$PROLIFIC_{j,t} = \alpha PRISON_{j,t} + \gamma_j + \Phi_y + \lambda_t + \epsilon_{j,t} \quad (3)$$

Dependent variable *PROLIFIC* is the number of prolific offenders known to the police in urban area *j* and year *t*. Again, *PRISON* is the number of offenders serving a long-term prison sentence after being convicted under the habitual offender law. We include area fixed effects γ_j and year fixed effects Φ_y . The data covers a period of five years before the law was implemented in the first group of cities, which allows us to also include a linear time trend λ_t for each of the urban areas. In line with our prior identification strategy, we assume application of the law to be exogenous to the trend in the number of prolific offenders in an area. We expect α to be around 1: one additional incapacitated offender results in one less prolific offender out on the streets. We do not expect the coefficient to be much greater than 1. That would be the case if prolific offenders move out of the area in response to introduction of the law or if offenders manage to avoid arrest, for instance by reducing their rate of offending. As discussed previously, offenders tend to remain in the same area and to continue offending because of a lack of alternatives. We also do not expect the effect to be

much smaller than 1. For instance, such would be the case if long-term incapacitated offenders were replaced by other offenders that are attracted by opportunities for theft that now go unexploited (Cook 1986). Alternatively, attention of the police may be focused on the well-known local drug addicts, leaving room for other offenders to become more active. The offending of this particular group of addicted offenders seems to be largely driven by a need to make an income rather than chance opportunities, creating a demand for stolen goods that may vanish as soon these offenders are incapacitated.

Table 3 reports the estimation results based on equation (3). As expected, we find the estimated effect to be close to one. In the second column, we account for a one-year lag in the police data on active offenders. The annual counts of prolific offenders active in the area include those that were incapacitated at some point during the year. As a result, it may take another year before incapacitated prolific offenders drop out of the active population. If we include the policy variable and its one-year lagged equivalent in equation (3) we find the total effect to be more or less equally spread over the current and following year. The analysis of changes in the population of prolific offenders known to the police confirms that the effect on crime works through incapacitation. These results imply that it is valid to interpret the parameter estimates in Table 2 as the number of crimes prevented per offender.

[TABLE 3 ABOUT HERE]

Supporting evidence for the presence of decreasing returns to scale

The criminal records of the offenders that were sentenced under the habitual offender law allow for a verification test of the presence of decreasing returns to scale. If diminishing returns are present, then we should see offenders with the most extensive criminal records

to be incapacitated first and offenders with somewhat less extensive criminal records to be incapacitated later. For each offender, we select the offenses on the criminal record that were committed in the four years prior to conviction under the habitual offender law.¹³ Figure 8 shows the relationship between the rate of at which the policy is applied and the mean number of offenses on the criminal record of offenders that were incapacitated at that rate of application. The police data confirm our findings. At higher levels of application of the policy, the mean number of offenses on the criminal record of offenders tends to be lower. Of all 12 areas, the Rotterdam-region is the only exception. We do not know why the selection process of prolific offenders was different in this area. Overall, the mean number of offenses drops by about a third when comparing the highest and the lowest rate of application of the policy. The difference is large, albeit smaller than the difference in the estimated marginal effects reported in Table 2.

[FIGURE 8 ABOUT HERE]

Sensitivity analysis

As a test of the robustness of our findings, we re-estimate our model using a number of alternative specifications. Our identifying assumption is that the application of the policy is unrelated to local crime trends. In our baseline model, we include covariates to prevent estimation bias stemming from a systematic relation between police activity and economic conditions and application of the policy. As a further test, we include linear time trends for each of the urban areas. These trends prevent bias from any relationship between growth in application of the policy and trends in an area. The results are presented in the second column of Table 4. The parameter estimates hardly change when including area-specific

¹³ The results are similar when selecting a shorter or longer time-window.

linear time trends, providing further support for our assumption that application of the policy is exogenous to local crime trends.

To allow for a non-linear impact of the law on the crime rate, we estimated the incapacitation effect at four regular intervals in Table 2. The number of intervals may be too small to reliably estimate the relationship, resulting in biased parameter estimates. In the third column of Table 4, we extend the number of intervals to seven. We find the results to be highly similar to the baseline estimates. Thus our results are robust to a smoother approximation of the non-linear relation between the policy and the crime rate.

[TABLE 4 ABOUT HERE]

As discussed in section 3, implementation of the habitual offender law happened in two phases, with a first wave related to the introduction of the policy in ten cities and a second wave when application of the policy was widened to all cities at the end of 2004. Capacity constraints were lifted in the second wave, although the rate of application remained closely related to the number of prolific offenders known to the local police, the variable that was used to allocate prison capacity in the first wave. To test how robust our results are to the two different phases of implementation of the policy, we allow the effect of the law on the crime rate to differ before and after 2005. We cannot identify an effect of high rates of application of the policy before 2005, as application rates had not achieved high levels before 2005 yet. The results in the fourth column of Table 4 show that the results are similar. The small differences in the parameter estimates are not statistically significantly different from zero.

As discussed in section 4, abstracting from the effect of incarceration-based treatment on crime rates through reduced rates of recidivism could bias our estimate of the incapacitation

effect. We estimate the relation between the net inflow of offenders in a month and the monthly change in the local crime rate. Implicitly, we assume the effect of inflow on the local crime rate to be similar to the effect of outflow. If this assumption does not hold, then the estimated incapacitation effect is biased towards zero. To test whether a bias exists, we separately model increases and decreases in the net inflow of prolific offenders sentenced under the new law (cf. Mocan and Bali 2010). In case of an increase, the inflow dominates our policy variable. Similarly, in case of a decrease, the outflow dominates. If the effect of outflow strongly deviates from the effect of inflow, then this should show in the estimated effects for increases and decreases in the policy variable. We alter the estimation equation as follows:

$$C_{j,t} = \alpha_1 PRISON_{+j,t} + \alpha_2 PRISON_{-j,t} + X_{j,t} \beta + \gamma_j + \Phi_y + \delta_q + \varepsilon_{j,t} \quad (4)$$

$PRISON_{+}$ takes on the values of the previously defined policy variable $PRISON$ in case of an increase relative to the previous month; $PRISON_{-}$ takes on the values in case of a decrease. The results in the last column of table 4 show that the two estimates are similar, suggesting the absence of a bias through a change in recidivism rates. This finding suggests that a rehabilitation effect, if present, is small. Clearly, a precise test of the size of the treatment effect should be based on individual level data on offenders rather than aggregate crime rates. Based on our analysis, we can only conclude that the treatment effect is sufficiently small not to bias estimation of the incapacitation effect.

6 Costs and benefits of selective incapacitation

Identifying the effects of the habitual offender law *relative to alternative policies* in dealing with prolific offenders as in equation (2) is important for an analysis of costs and benefits.

For a meaningful analysis of the welfare impact of selective incapacitation, the policy should be compared to a practical alternative.

The costs of incapacitating and providing treatment to prolific offenders amount to some 200 euro per day or 73,000 euro per year. In absence of the new law, on average, prolific offenders spend some four months per year in prison at a slightly lower cost of 180 euro per day. Taking the difference between the two, we find the additional costs of the policy to be equal to 51,400 euro per year per prolific offender.

In addition to these costs, the costs of use of the criminal justice system may have changed as well. On the one hand, offenders are sentenced once for a period of two to three years, rather than going in and out of prison a couple of times per year, saving resources within the criminal justice system. On the other hand, substantial costs are related to identifying, arresting and convicting prolific offenders under the new law. In the absence of detailed information on the use of resources of the police, the public prosecutor, the probation service and the courts, we have to leave these costs outside the analysis.

Based on the results in Table 2, correcting for non-reporting and non-recording of crime, and under the assumption that the affected offenders spend 8 additional months in prison per year, the law prevents some 80 thefts from car and 9 domestic burglaries annually per long-term incapacitated offender. That implies that the costs per crime prevented are equal to some 600 euros. In other words, if the social costs of a domestic burglary and a theft from car are higher than 600 euros, then the policy is welfare improving.

Estimates of the costs of crime are surrounded by controversy. Ex post approaches estimate the cost of crime that has already occurred to identifiable victims. Based on jury awards, Roman (2009) estimates the average costs of a burglary to be \$4,444 (3,300 euro).

Accounting for all of the costs that are known to be related to crime, including damage and the costs of use of the criminal justice system, the Home Office produces a somewhat higher estimate of the costs of a burglary of 4,600 euro (\$6,000) per incident. A separate estimate for the cost of a theft from car is only available from the Home Office study, which puts it at 1,200 euro (\$1,600) per incident. Ludwig (2010) argues that an ex ante rather than the ex post perspective is more appropriate as it corresponds to the actual resource allocation problem facing policy makers. Compared to the ex post estimates, studies based on willingness-to-pay produce much larger cost estimates. Cohen et al. (2004) put the average costs of a burglary at \$27,901 or 21,000 euro.¹⁴ In any case, the costs of crime incidents are much higher than 600 euros, the average costs per crime prevented. When we take the two estimates from the Home Office (2005), the prevented crimes can be valued at 140,000 euro in total. At the average rate of application, the benefits exceed the costs by more than a factor of two. Cost of crime estimates based on willingness to pay would result in even more favorable cost-benefit ratios.

[FIGURE 9 ABOUT HERE]

We found the marginal effect of incapacitating offenders to decrease with more intensive application of the policy. Taking the Home Office (2005) cost of crime estimates, Figure 9 shows how the annual benefits vary with the rate of application of the policy and how the benefits relate to the costs. The estimated effects at different intervals are smoothed to generate a gentle downward sloping benefit-curve. The figure suggests that at the highest

¹⁴ Given the anomalies in preventative behavior observed in other areas than crime, with some people not taking health precautions or building up pension savings even though they seem to know they would be better off if they did, it is a question for further research whether the high ex ante estimates based on stated preferences provide a realistic picture of the costs of crime.

rate of application, which was achieved in 2007 in the urban areas around Amsterdam, Rotterdam and The Hague, the benefits of the policy become equal to the costs. Clearly, the actual benefits are much higher if we would also take into account other offenses that these prolific offenders would have committed during incarceration, including shoplifting and bicycle theft (as discussed previously, the deterrent effect of the law on the crime rate over and above the incapacitation effect is likely to be small). As we underestimate the benefits in Figure 9, we conclude that the social returns to selective incarceration seem to be positive over the whole range of application of the policy during 2001-2007.

The costs and benefits of the policy for society should be weighed against the consequences for the individual offender. As noted before, most of the offenders involved are addicted to hard drugs, out of work and do not have stable housing. The two to three-year window for incarceration-based treatment is meant to lower their drug dependence and provide training. Process evaluations of the policy show that many offenders see the long-term prison sentence as an opportunity to break with a life dominated by drug use rather than as a severe punishment (Koeter and Bakker 2007). Surveys among offenders show that some 60 percent feel substantially better after the two to three year prison sentence. Only a small group considers the sentence as unfair and denies all treatment. These findings suggest that the crime prevention benefits may actually go together with an improvement of the lives of the majority of the prolific offenders. Thus the welfare impact of the policy may actually be more favorable rather than less favorable when taking into account the consequences for the offenders affected by the habitual offender law.

7 Conclusions

We find that in a situation of a relatively low rate of incarceration sentence enhancements for a carefully selected group of prolific offenders can dramatically reduce the crime rate. A habitual offender law adopted in the Netherlands in 2001 allowed for a two to three year prison sentence for offenders with ten or more offenses on their criminal record. Although the group of offenders sentenced under the law accounted for only 5 percent of the prison population six years after its introduction, the sentencing policy lowered the rate of burglary and theft from car by an estimated 40 percent through the incapacitation effect alone. The estimated impact of the law is large, but in line with self-reported crime. In addition, police counts of active prolific offenders are found to go down proportionally with the number of prolific offenders serving extra time in prison as a result of the law.

When comparing the cost of enhanced prison sentences with the social benefits of lower crime rates, we find the benefits of the policy to exceed the costs. Even for this highly selective sentencing policy that only affected 1,400 offenders in the period 2001-2007 we find evidence for rapidly decreasing returns to scale. The marginal crime-reducing effect of incapacitating another prolific offender declines by more than half from the lowest to the highest rate of application of the law. The benefit-cost ratio drops sharply when more offenders are serving time under the habitual offender law. The social returns to selective incarceration remain positive over the whole range of application of the policy, however. The finding of a non-linear impact of the policy on the crime rate contributes to the small literature on diminishing returns in the use of incarceration as form of punishment.

The incapacitation effect may be particularly large in the case of the Netherlands as the habitual offender law primarily affected offenders that were addicted to drugs, heroin in

particular. These offenders tend to have an age-crime curve that is flatter than that of other groups of offenders – even other prolific offenders. Possible negative effects of longer prison sentences on the life of offenders such as disruption of employment, relationships and housing were limited as most of the affected offenders were out of work and did not have stable housing.

The Dutch policy of selective incapacitation started from a low base. The rate of incarceration in the Netherlands around 2001 was similar to the rate in the beginning of the 1970s in the US, for instance. Enhancing prison sentences of a few weeks or months to three years is likely to have a greater payoff in terms of preventing crime than enhancing prison sentences that are already many years long. To compare: an enhanced prison sentence for burglary of 2 to 3 years based on the Dutch habitual offender law is comparable to the default sentence for burglary in the United States (Lochner 2010, Table 1). Our finding that the habitual offender law adopted in the Netherlands had a large incapacitation effect should therefore not be interpreted as evidence that all policies of selective incapacitation are likely to have a similarly favorable cost-benefit ratio. Given the rapidly diminishing returns to incarceration, the high costs of the enhanced prison sentences may soon exceed the benefits of crime prevented. How much of an incapacitation effect other policies of selective incapacitation had is a question that begs further empirical study.

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Table 1 Summary statistics, 12 urban areas, January 1999 – December 2007

	Mean	Standard deviation	Min	Max
Domestic burglary	5.90	1.88	1.46	13.73
Theft from car	15.87	9.06	2.37	67.73
Domestic burglary and theft from car	21.76	10.28	5.36	76.81
Number of prolific offenders serving time under habitual offender law	0.34	0.47	0	2.19
Number of arrests	0.55	0.16	0.14	1.07
Rate of unemployment (%)	2.82	0.81	1.33	5.17
Number of observations	1,296			

Notes. All variables are per 10,000 population, unless otherwise noted. Crime statistics are police recorded crime incidents.

Table 2 Estimated incapacitation effect of habitual offender law on rate of recorded theft per month, 12 urban areas, January 1999 – December 2007

Dependent variable:	Theft from car and burglary	Theft from car and burglary	Theft from car	Burglary
Number of incapacitated offenders	-4.58 (2.31)*			
Number of incapacitated offenders ≤ 0.5		-10.86 (3.07)***	-9.54 (2.94)***	-1.31 (0.74)*
0.5 < Number of incapacitated offenders ≤ 1.0		-8.64 (3.25)**	-8.05 (3.24)**	-0.60 (0.48)
1.0 < Number of incapacitated offenders ≤ 1.5		-5.32 (1.70)***	-4.72 (1.65)***	-0.60 (0.50)
Number of incapacitated offenders > 1.5		-4.28 (1.49)***	-3.59 (1.44)**	-0.69 (0.30)**
Number of observations	1,296	1,296	1,296	1,296

Notes. All variables are per 10,000 population. Standard errors between parentheses. Other covariates include rate of unemployment, number of arrests per population, area-fixed effects, year-fixed effects and quarter-fixed effects. Standard errors clustered by urban area. * Statistically significant at the 10 percent level; ** Statistically significant at the 5 percent level; *** Statistically significant at the 1 percent level.

Table 3 Estimated effect of habitual offender law on the number of active prolific offenders known to the police, 12 urban areas, 1996-2007

Dependent variable:	Prolific offenders aged 25 or over known to the police	Prolific offenders aged 25 or over known to the police
Number of incapacitated offenders	-0.92 (0.18) ^{***}	-0.58 (0.24) ^{**}
Number of incapacitated offenders (t-1)		-0.49 (0.23) ^{**}
Number of areas	12	12
Number of years	12	12
Number of observations	144	144

Notes. Standard errors between parentheses. Other covariates include area-fixed effects, year-fixed effects and area-specific linear time trends. * Statistically significant at the 10 percent level; ** Statistically significant at the 5 percent level; *** Statistically significant at the 1 percent level.

Table 4 Estimated incapacitation effect of habitual offender law on rate of recorded theft per month, sensitivity analysis

Dependent variable: theft from car and burglary	Baseline model	Incl. area-specific linear trends	Seven intervals	Before and after 2005	Increase vs. decrease
Incapacitated offenders ≤ 0.5	-10.86 (3.07)***	-9.28 (3.23)***			
0.5 < Incapacitated offenders ≤ 1.0	-8.64 (3.25)**	-7.00 (2.89)**			
1.0 < Incapacitated offenders ≤ 1.5	-5.32 (1.70)***	-5.77 (2.37)**			
Incapacitated offenders > 1.5	-4.28 (1.49)***	-4.98 (2.34)*			
Incapacitated offenders ≤ 0.5			-11.21 (3.14)***		
0.5 < Incapacitated offenders ≤ 0.75			-9.66 (3.10)***		
0.75 < Incapacitated offenders ≤ 1.0			-7.82 (3.51)**		
1.0 < Incapacitated offenders ≤ 1.25			-5.37 (2.18)**		
1.25 < Incapacitated offenders ≤ 1.5			-5.49 (1.46)***		
1.5 < Incapacitated offenders ≤ 1.75			-5.47 (1.77)***		
Incapacitated offenders > 1.75			-3.53 (1.28)**		
Incap. offenders ≤ 0.5 (before)				-11.05 (3.66)***	
Incap. offenders ≤ 0.5 (after)				-11.41 (2.98)***	
0.5 < Incap. offenders ≤ 1.0 (before)				-7.71 (3.74)*	
0.5 < Incap. offenders ≤ 1.0 (after)				-9.39 (3.38)**	
1.0 < Incap. offenders ≤ 1.5 (before)				-	
1.0 < Incap. offenders ≤ 1.5 (after)				-5.66 (2.07)**	
Incap. offenders > 1.5 (before)				-	
Incap. offenders > 1.5 (after)				-4.52 (1.73)**	
Incapacitated offenders ≤ 0.5 (+)					-10.42 (3.02)***
Incapacitated offenders ≤ 0.5 (-)					-13.79 (3.12)***
0.5 < Incapacitated offenders ≤ 1.0 (+)					-8.72 (3.44)**
0.5 < Incapacitated offenders ≤ 1.0 (-)					-8.48 (2.62)***
1.0 < Incapacitated offenders ≤ 1.5 (+)					-5.39 (1.74)***
1.0 < Incapacitated offenders ≤ 1.5 (-)					-5.16 (1.64)**
Incapacitated offenders > 1.5 (+)					-3.80 (1.43)**
Incapacitated offenders > 1.5 (-)					-5.07 (1.52)***

Notes. Number of observations equal to 1,296 for each specification. All variables are per 10,000 population. Other covariates include rate of unemployment, number of arrests per population, area-fixed effects, year-fixed effects and quarter-fixed effects. Standard errors between parentheses. Standard errors clustered by urban area. * Statistically significant at the 10 percent level; ** Statistically significant at the 5 percent level; *** Statistically significant at the 1 percent level.

Figure 1 Number of prolific offenders sentenced under the habitual offender law, 2001-2007

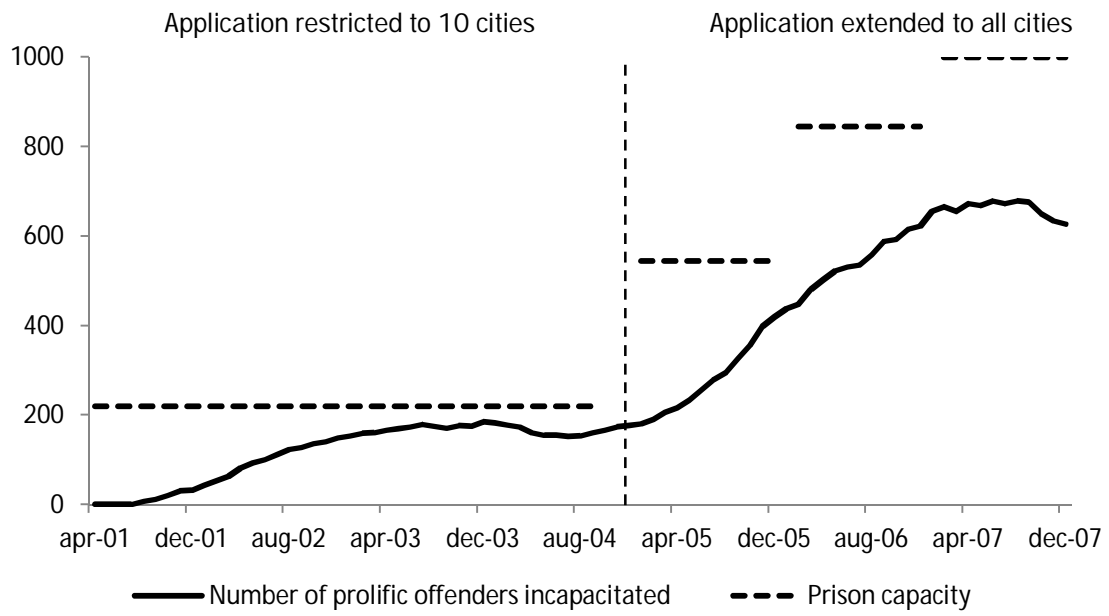


Figure 2 Moment of introduction of the habitual offender law, 12 urban areas

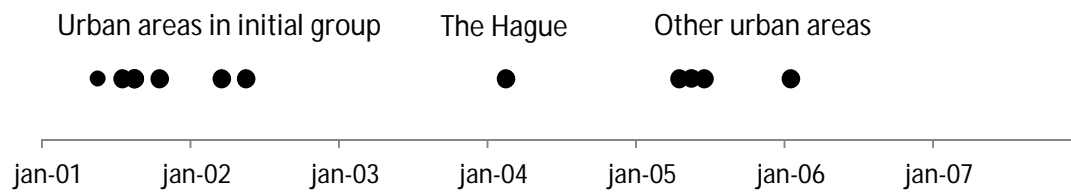
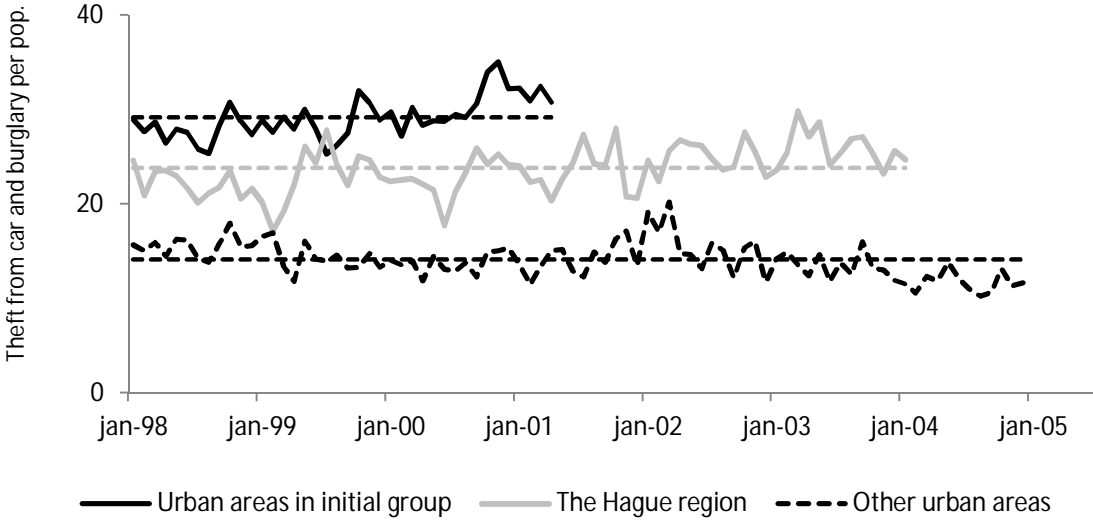


Figure 3 Police recorded theft from car and domestic burglary per 10,000 population, urban areas grouped by moment of introduction, January 1998 until moment of introduction of law



Note. A dotted line denotes the average crime rate over the period January 1998 until the moment of introduction.

Figure 4 Number of offenders sentenced under the habitual offender law since introduction until October 2004 ('first wave'), urban areas in initial group and The Hague-region

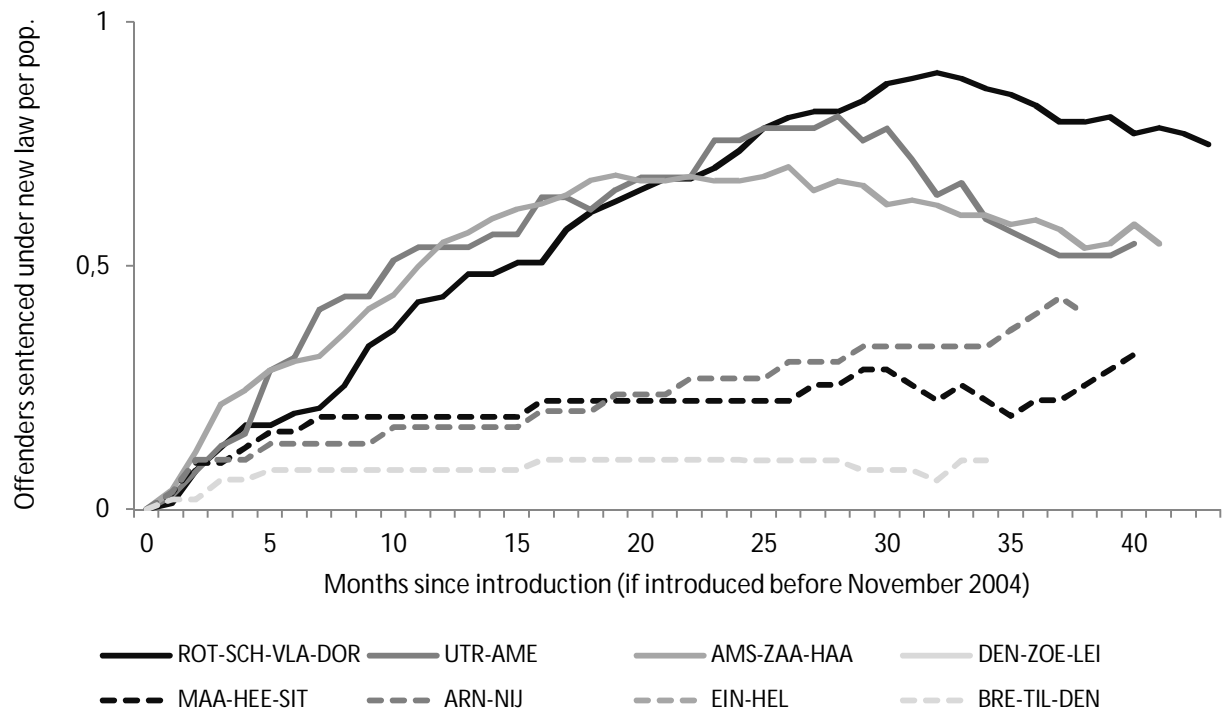
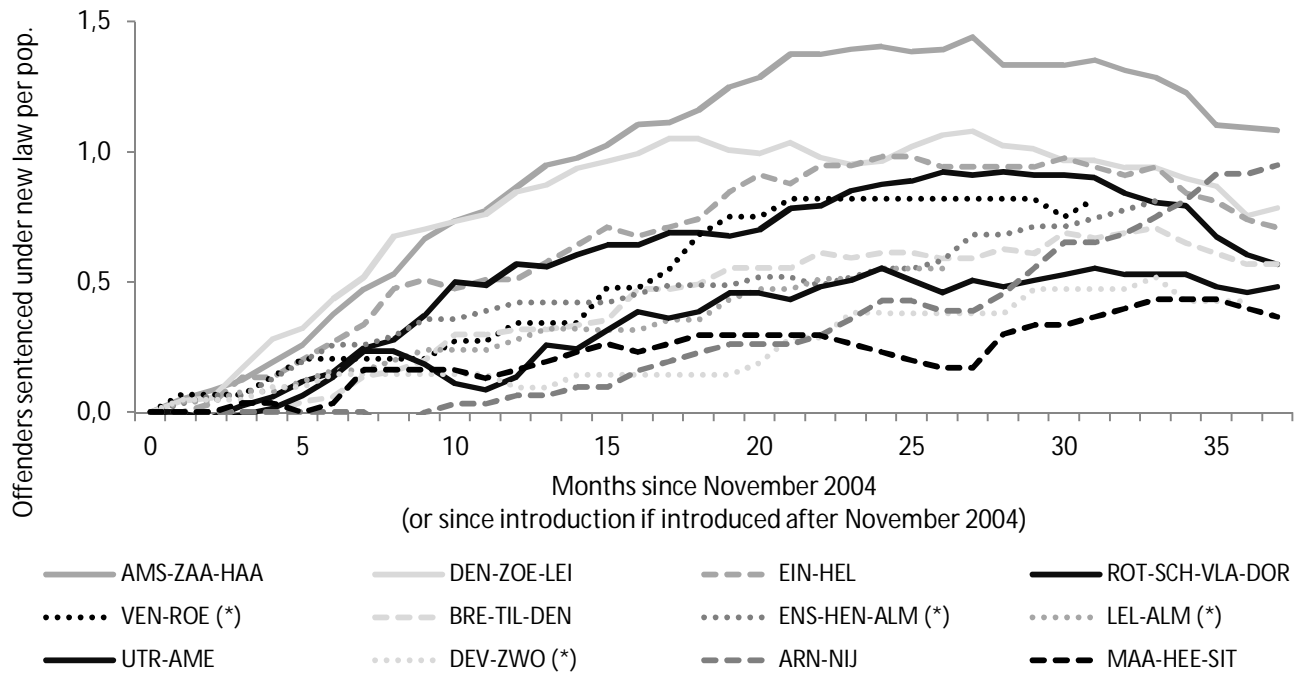


Figure 5 Number of offenders sentenced under the habitual offender law since November 2004 or since introduction if introduced after November 2004 ('second wave'), 12 urban areas



Note. The number of offenders sentenced under the new law is set to zero at November 2004 for the eight urban areas that introduced the policy before this date.

Figure 6 Crime rate three years before and after introduction of habitual offender law, 12 urban areas

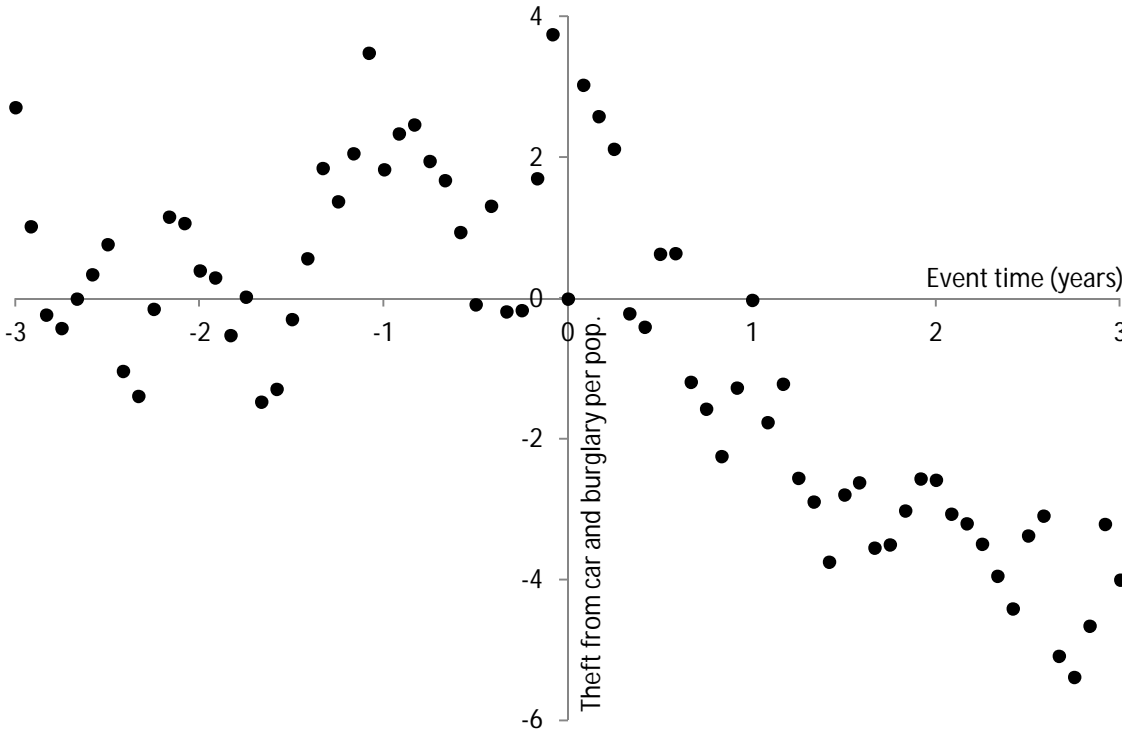
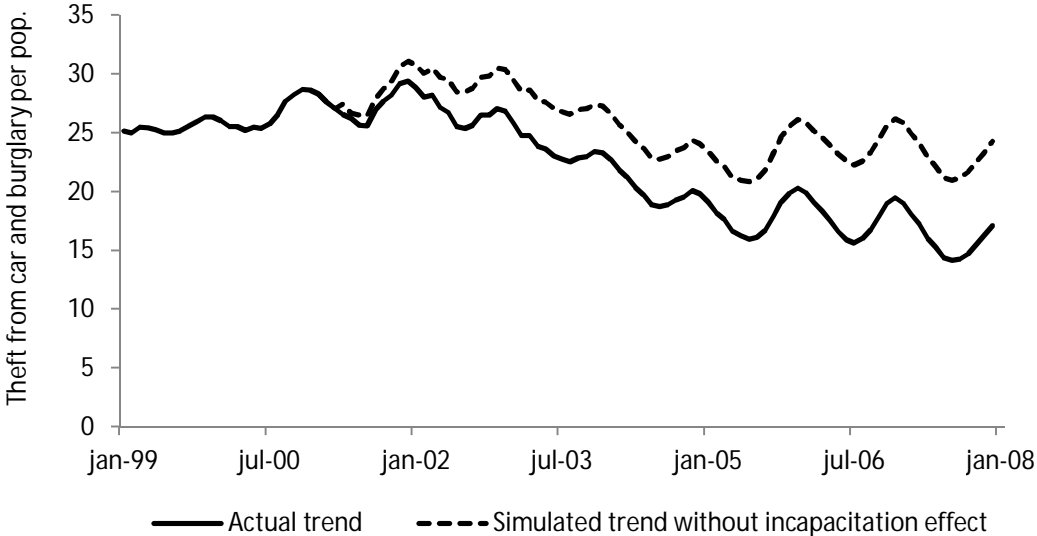


Figure 7 Monthly rate of recorded theft with and without the estimated effect of incapacitation, 12 urban areas, January 1999 – December 2007



Note. The trend without the incapacitation effect is the sum of the actual number of thefts plus the number of thefts prevented for each individual urban area, based on the parameter estimates in the second column of Table 2. Shown is the five-months averaged trend.

Figure 8 Number of offenses on criminal record of offenders sentenced under the 2001 law at different rates of application of the law, annual averages, 12 urban areas, 2001-2007

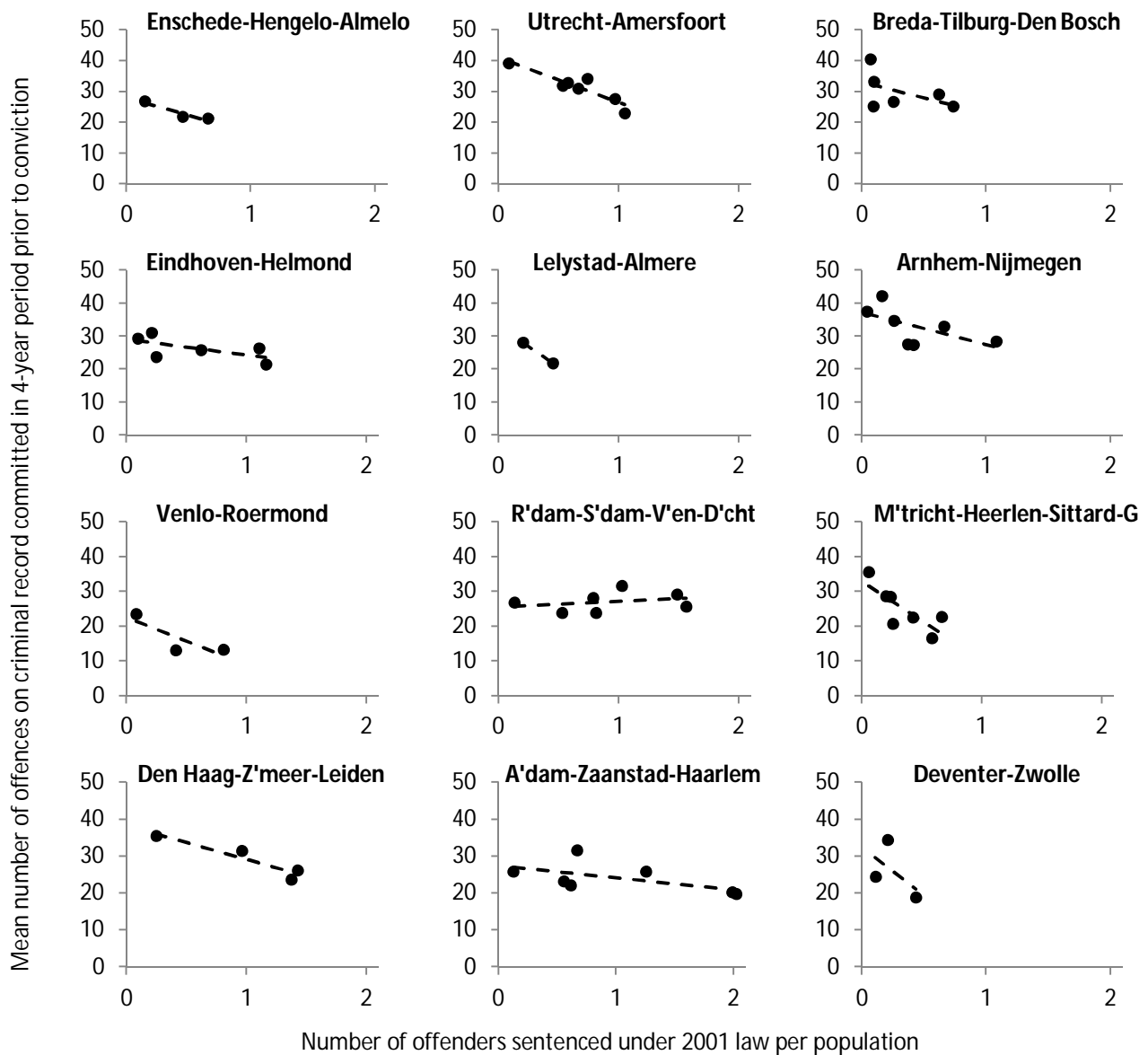


Figure 9 Estimated annual costs and benefits for an urban area of incapacitating *one additional prolific offender* under the habitual offender law

