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The Incapacitation Effect of First-Time Imprisonment: A Matched Samples Comparison

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Abstract

Objectives   The logic of incapacitation is the prevention of crime via the forced removal of known offenders from the community. The challenge is to provide a plausible estimate of how many crimes an incarcerated individual would have committed, were s/he free in the community rather than confined in prison. The objective of this study is to provide estimates of the incapacitation effect of first-time imprisonment from a sample of convicted offenders.

Methods   The data are official criminal records of all individuals convicted in The Netherlands in 1997. Two different analytical strategies are used to estimate an incapacitation effect. First, the offending rate of the imprisoned individuals prior to their confinement in 1997 provides a "within-person counterfactual". Second, imprisoned offenders are paired with comparable non-imprisoned offenders using the method of propensity score matching in order to estimate a "between-person counterfactual". Incapacitation estimates are provided separately for juvenile imprisonment (ages 12–17) as well as adult imprisonment (ages 18–50), and for male and female offenders.

Results   The best estimate is that 1 year of incarceration prevents between 0.17 and 0.21 convictions per year. The use of additional data sources indicates that this corresponds to between roughly 2.0 and 2.5 criminal offenses recorded by the police.

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Conclusions The current results suggest that, insofar as imprisonment is used with the primary goal of reducing crime through incapacitation, a general increase in the use of incarceration as the sanction of choice is not likely to yield major crime control benefits.

Introduction

Among the manifold goals of penal confinement, incapacitation aims to impose a period of "time out" from the criminal career, by removing the opportunity for an individual to commit crime in the community for the duration of his or her sentence. After a period of empirical stagnation, questions concerning the incapacitation effect of imprisonment have recently become salient, as indicated by a 2007 issue of *Journal of Quantitative Criminology* especially devoted to research on this topic (see Reuter and Bushway 2007). This recent empirical attention has been spurred, in part, by the unparalleled growth in the use of incarceration in Western society over the last three decades. This growth is most stark in the United States, which at yearend 2006 had an incarceration rate (including jails and state and federal prisons) that was an astonishing 751 per 100,000 residents (Sabol et al. 2007). Yet in Europe as well, two-thirds of the 35 countries surveyed experienced growth in their incarceration rate during the first half of the 2000s (Aebi et al. 2006).

The Netherlands in particular, long known for its liberal and humane penal policies, has witnessed remarkable prison growth from the 1970s to the present (see Bijleveld and Smit 2005; Downes 2007; Tonry and Bijleveld 2007). A prolonged period of decarceration lasted from 1947 to 1975, after which, from 1975 to 2005, the Dutch incarceration rate climbed from just over 20 to nearly 110 per 100,000 (Downes 2007; Tonry and Bijleveld 2007). The Netherlands currently has one of the highest incarceration rates in Western Europe, third only to England/Wales and Spain (Aebi et al. 2006).[1]

A growing literature has become attentive to the societal consequences of prevailing imprisonment policies, and cautions that the social costs of prison growth might outweigh the crime-control benefits. Two pieces of evidence support this contention. One prominent line of research observes that confinement worsens an individual's capacity to adopt a law-abiding lifestyle upon returning to the community (Hagan and Dinovitzer 1999; Uggen et al. 2006; Western 2006). Such "collateral consequences" of incarceration include labor market stigma (Pager 2003), slowed wage growth (Western 2002), and marital disruption (Apel et al. 2010), which have the potential to exacerbate long-term criminal offending (Nagin et al. 2009; Wermink et al. 2010). A second line of research observes that prison growth leads to the confinement of individuals who pose steadily lower risk to society, on the margin and all else equal (Donohue 2009; Donohue and Siegelman 1998). Consistent with the "law of diminishing returns," recent studies of prison expansion show substantial erosion in crime control over the last three decades as the scale of incarceration has grown (Johnson and Raphael 2009; Liedka et al. 2006).

The question of interest in this study concerns the second of these two lines of research. Specifically, we consider the degree to which prison incapacitates criminal behavior. A key

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[1] For an analysis of the historical developments in Western society that have given rise to current penal practice, see Garland (2001). Although chiefly concerned with the US and UK, his cogent insights apply as well to The Netherlands (Downes 2007).

goal for a study of incapacitation is to estimate the number of crimes that an incarcerated offender *would have committed*, were he free in the community rather than confined in prison. This is taken as an estimate of the "incapacitation effect," defined as the number of crimes averted by physically isolating an offender from society at large (Blumstein et al. 1978). This task would seem straightforward at first glance, but the problem on closer inspection is that this quantity is a "counterfactual" which can never, even in principle, be directly observed. With this challenge firmly in mind, we apply two different analytical techniques to estimate the incapacitation effect. The first corresponds with incapacitation research in the eighties and nineties in which offending behavior was estimated in the month prior to their arrest or prison sentence (see Chaiken and Chaiken 1982; Horney and Marshall 1991; Miranne and Geerken 1991). The second technique is based on recent work of Sweeten and Apel (2007) who employed the method of propensity score matching, which identifies for each incarcerated individual one non-incarcerated individual who has an equally high risk of incarceration. The incapacitation effect is then estimated from the offending behavior of this matched, non-incarcerated comparison sample. We rely on data from a 1997 conviction cohort in The Netherlands, allowing us to estimate the incapacitation effect in a general offender sample.

The current study adds to the literature in three ways. This study expands the scope of contemporary research on the effects of criminal punishment to the understudied international context of the Netherlands, providing an important opportunity to assess the generalizability of findings in previous studies that to date have been largely limited to the US context. Second, we estimate the incapacitation effect using unique longitudinal data. While studies that used the same dataset focused on the effects of community service orders and imprisonment on subsequent criminal behavior (Wermink et al. 2010) and the effect of prison length on subsequent criminal behavior (Snodgrass et al. 2011) none have focused on the incapacitation effect. Third, compared to the study of Sweeten and Apel (2007), who also applied propensity score matching to estimate incapacitation effect, we include offenders in a broader age range (12–50) and a broader set of offense types, so that our results are generalizable to a larger population.

In what follows, we first provide an overview of research on the subject of incapacitation by tracking the development of "bottom-up" studies of prison effectiveness. We then propose an alternative methodology to estimate the incapacitation effect. Following a description of the data and analytical approach, we present the empirical findings. We conclude our study with a discussion of the implications and conclusions.

Approaches to Estimation of Incapacitation Effects of Prison

In this section, we review past empirical efforts to estimate the incapacitation effect of imprisonment. Our review focuses on "bottom-up" studies of prison effectiveness (Spelman 2000), or empirical attempts to estimate individual criminal offending rates which can then inform estimates of incapacitation effects.[2] The intent of this review is not to be exhaustive, but rather to provide context for the current study. Interested readers may

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[2] This contrasts with "top-down" studies of prison effectiveness (Spelman 2000), which provide aggregate estimates of the total effect of imprisonment yet are agnostic about whether the effects operate through deterrence or incapacitation (Gibbs 1968; Kuziemko and Levitt 2004; Levitt 1996; Marvell and Moody 1994). Some researchers advocate a purely top-down approach to the study of incarceration (see Miles and Ludwig 2007).

consult several excellent reviews that are available elsewhere (Cohen 1983; Spelman 1994; Visher 1987).

Estimation of the incapacitation effect of imprisonment is fundamentally an actuarial exercise (see Harcourt 2007).[3] The empirical challenge is one of overcoming the *counterfactual problem,* that is, providing a credible estimate of behavior that is not observed because an individual is locked up in jail or prison rather than free in the community. The key quantity for incapacitation is known as lambda, λ, representing the annual offending frequency conditional on active offending, which can be taken as an estimate of the number of crimes avoided through incarceration. Zimring and Hawkins (1995, pp. 81) observe that there are two approaches to estimating lambda:

> [T]o determine the level of crime that would have occurred if a particular group had not been confined, one must either study the criminal activity of the same group at a different time in their lives to estimate what that group would have done if not confined, or one must study the behavior of persons other than those confined to approximate the crimes avoided by imprisonment in the past.

We refer to the first as a *within-person counterfactual approach* and to the second as a *between-person counterfactual approach*. Point of departure for both approaches mentioned is that the crimes the offender would have committed while free are actually prevented by incarcerating the offender. As such these methods do not account for the mitigating effects of replacement—other offenders filling in the position left vacant by incarcerating the original offender—or the fact that co-offenders might have committed the particular crime regardless of the absence of the incarcerated offender. Replacement however seems especially relevant for drug offenses, and is deemed largely irrelevant for the large majority of crimes (Piquero and Blumstein 2007). Group offending in turn might be more relevant for juveniles than for adults since juvenile offenders more often co-offend (Farrington 2003).

The most influential incapacitation studies are universally of the first kind in Zimring and Hawkins' (1995) typology. These studies provide within-person counterfactual offending rates from self-report surveys of arrestees or prison inmates. With this approach, targeted individuals are questioned about their criminal activity during the months leading up to their arrest or confinement. These estimates are taken as the number of crimes they committed on an annual basis when they were free (prior to incarceration), and by implication, the number of crimes they would have committed per year during the time that they were incarcerated. Therefore, the counterfactual offending rate for incarcerated individuals is their own offending rate in the months preceding their confinement.

The best known source of a within-person counterfactual is the Rand Corporation's second inmate survey, a study of over 2,000 male inmates in California, Michigan, and Texas (Chaiken and Chaiken 1982; Greenwood and Abrahamse 1982; Peterson et al. 1982; Rolph et al. 1981).[4] The report by Chaiken and Chaiken (1982), for example, revealed that annual offense frequencies for ten different crimes were highly skewed, with active offenders at the median committing 15 offenses and offenders at the 90th percentile

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[3] The earliest studies of incapacitation grew out of the field of operations research (Avi-Itzhak and Shinnar 1973; Blumstein and Cohen 1979; Blumstein and Nagin 1978; Shinnar and Shinnar 1975), although the topic also garnered more general criminological attention (Clarke 1974; Greenberg 1975).

[4] This formative study was followed by a series of re-analyses (Blumstein et al. 1993; Canela-Cacho et al. 1997) as well as a number of replication efforts (English 1993; DiIulio 1990; Horney and Marshall 1991, 1992; Miranne and Geerken 1991).

committing 605 offenses (these figures exclude drug offending). They estimated the mean offending rate to be between 187 and 278 crimes per year, depending on how ambiguous survey responses were treated.[5] Based on these figures, incarceration was shown to have the capacity to substantially incapacitate criminal behavior. In fact, on the basis of this research, policy-oriented criminologists began to advocate "selective incapacitation" of high-rate criminal offenders as an explicit penal policy (Chaiken and Rolph 1980; Greenwood and Abrahamse 1982).[6]

These incapacitation studies had limitations, however. First and foremost is the question of the reliability of offender self-reports of their criminal behavior (Spelman 1994; Zimring and Hawkins 1995). Specifically, it is unclear whether incarcerated offenders, particularly high-rate offenders, can accurately recall their prior criminal activity. Even if they can, they may not be motivated to report it honestly to interviewers. Spelman (1994), in fact, found evidence of both overreporting and underreporting in the second Rand inmate survey, but by individuals at different locations in the distribution of offending rates. Second, the presence and nature of "crime spurts" can introduce serious distortions in estimates of lambda (Blumstein et al. 1986), and therefore in estimates of incapacitation effects. Research has shown that offenders experience relatively short periods of high-rate offending immediately prior to incarceration, meaning that incapacitation effects will be severely overstated, especially if reporting windows are comparatively narrow (Rolph et al. 1981). Moreover, a portion of the pre-incarceration crime spurt appears to be artifactual rather than behavioral; that is, a function of the way that individuals are filtered through the criminal justice system and selected for custodial sentences rather than reflective of genuine growth in offending frequency (Blumstein et al. 1993; Maltz and Pollock 1980; Tierney 1983).

Finally, these early incapacitation studies were guilty of overly optimistic assessments of the incapacitative benefits of incarceration. The studies showed self-report offending rates to vary widely from one jurisdiction to another. For example, Chaiken and Chaiken (1982) reported the median total non-drug crime frequency among prisoners (not including jail inmates) in California, Michigan, and Texas to be 42, 17, and 9, respectively. The median offender reported committing 4.2 non-drug offenses per year in Nebraska (Horney and Marshall 1991), 4.4 non-drug offenses in New Orleans (Miranne and Geerken 1991), and 12 non-drug offenses in Wisconsin (DiIulio 1990).[7] Relatedly, the incapacitation estimates from this research tend to assume constant offending during the reference period, whereas Horney and Marshall (1991) demonstrated that most offenders were actively involved in crime only intermittently, and that failure to account for this intermittency

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[5] While the obvious measure of central tendency in incapacitation studies is the mean (Piquero and Blumstein 2007), this estimate is sensitive for outliers in the sample. In a sample, such as the Rand sample, in which half of the population committed very few offenses and a much smaller percentage commit many crimes annually the mean of the group will increase sharply. In such highly skewed distributions it could be problematic to interpret the mean offending rate as the number of crimes averted by incarceration. Given that the current focus is on the effects of first imprisonment, the criminal histories of offenders in our sample are more homogeneous, rendering estimating an overall mean incapacitation effect less problematic.

[6] Research efforts using risk assessment tools to identify high-rate offenders and target them for longer prison sentences have been notoriously unsuccessful (Auerhahn 1999, 2003; Gottfredson and Gottfredson 1994; Greenwood and Turner 1987), calling into question the wisdom of a selective incapacitation policy.

[7] When drug offenses were included in the latter two studies, median offense rates were doubled.

would inflate estimates of the incapacitative benefits of imprisonment when offense rates are annualized.[8]

Until recently, within-person counterfactual studies exhausted all empirical research in the incapacitation tradition. Two studies in the last 3 years have adopted the second approach in Zimring and Hawkins' (1995, pp. 81) typology, that of estimating the offending rate of "persons other than those confined to approximate the crimes avoided by imprisonment." Sweeten and Apel (2007) relied on propensity score matching to select incarcerated and non-incarcerated individuals who closely resembled each other on a wide variety of background variables, including criminal history. Using data from a contemporary, nationally representative sample of American youth, they limited their attention only to individuals who were not incarcerated prior to the reference conviction. They estimated lambda among the non-incarcerated sample to be about 9 offenses per year among 16–17 year olds, and about 6 offenses per year among 18–19 year olds.

Owens (2009) took advantage of a change in the sentencing guidelines in Maryland that lowered the age at which an offender's juvenile record could be used to add "criminal history points" for the purpose of criminal sentencing. This policy change effectively reduced the recommended sentence faced by certain groups of young adults with a juvenile criminal history. In her study, individuals who were sentenced after the policy change accumulated 2.8 arrests per year with an implied Index offending rate of 1.5 offenses per year during the time that they would still have been confined under the old sentencing regime.[9]

A Matched Samples Approach to Estimating Incapacitation Effects

The approach adopted in this study is a quasi-experimental method of estimating the number of crimes averted through incarceration. The study by Sweeten and Apel (2007) is the only application of this approach thus far. Specifically, the offending rate of a "non-captive" but high-risk comparison sample is taken as the counterfactual offending rate for an incarcerated sample during the period of its confinement. In other words, the incapacitation effect is the offending rate of a contemporaneous sample of individuals who are not incarcerated (but might be at a later date).

This particular approach has appeal because it addresses the counterfactual problem in a straightforward and transparent way. The intuition underlying propensity score matching can be elaborated by drawing comparisons with an experimental approach. In a hypothetical experiment, individuals would be targeted for incarceration at random. This ensures that the experimental and control groups are *balanced*, meaning that they are indistinguishable (in expectation) on all possible sources of non-equivalence which might confound incapacitation estimates. By virtue of randomization, the control group serves as

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[8] The assumption of constant offending is also problematic because with age offenders tend to decrease their offending rate (Farrington 1986). Especially when estimating the effect of long-term prison sentences, disregarding intermittency and the effect of age on offending would result in grossly overestimating the incapacitation effect. In contrast to within-person incapacitation estimates, the between-person counterfactual approach takes into account that offending rates are not stable over the life course—especially if offenders in the control group are matched to offenders in the experimental group directly on their age.

[9] The studies by Sweeten and Apel (2007) and Owens (2009) represent only two of a new generation of incapacitation research. One strand of this research employs simulation techniques (Bhati 2007; Blokland and Nieuwbeerta 2007), while another strand takes advantage of a variety of aggregate "natural experiments" (Barbarino and Mastrobuoni 2008; Johnson and Raphael 2009; Kessler and Levitt 1999; Ramirez and Crano 2003).

a *counterfactual* for the experimental group, or an estimate of what the experimental group's behavior would have been had it not been imprisoned. Randomization ensures that no additional adjustments are required, because all sampled individuals were "at risk" of being incarcerated with known probability; that is, at the start of the experiment, all were equally eligible to be assigned to the experimental group. In this scenario, the *assignment mechanism* is controlled by a statistically random process that renders it ignorable. The incapacitation effect of imprisonment then is simply the mean (or median) offending rate of the entire control group during the experimental group's confinement.

Propensity score matching is a quasi-experimental approach that attempts to approximate the conditions of a randomized experiment by creating "synthetic" experimental and control groups that are balanced on a wide variety of confounding variables. Pioneered by Rosenbaum and Rubin (1983, 1984, 1985), a propensity score is a type of *balancing score* defined as "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin 1983, pp. 41). The technique is particularly advantageous in situations where randomization is not possible but the assignment mechanism is nevertheless well understood and can be explicitly modeled from variables that are readily available to the analyst. A well specified model increases confidence that the assignment mechanism is ignorable conditional on the propensity score, a situation that satisfies what is known as the *conditional independence assumption*.

The initial step of this approach is estimation of the propensity score, which in the present context can be defined as the predicted probability of incarceration conditional on criminal conviction. To meet the conditional independence assumption requires intimate knowledge of the incarceration process; a process that is indeed fairly well understood from decades of criminological research. For instance, research on criminal sentencing suggests that the key determinants of incarceration are the characteristics of the instant offense, the offender's criminal history, and, to a much lesser degree, the offender's personal characteristics and life circumstances, characteristics of the sentencing judge, and jurisdictional factors (Spohn 2009).

Once the incarceration process has been adequately modeled and the propensity score estimated, attention turns to the matching process. The goal at this stage is to identify, for each incarcerated individual, a non-incarcerated counterpart who is observationally equivalent. A "suitable" match is therefore defined as a non-incarcerated individual who has a propensity score which is identical (or if not identical, at least within a minimum distance known as a caliper) to that of his or her referent, incarcerated peer. A property of propensity score matching is that not all incarcerated subjects will necessarily have a suitable match, and conversely, not all non-incarcerated subjects will necessarily be matched. A feature of the *common support condition* is that only those individuals who most closely resemble one another on the propensity score—and by implication, the variables indexed by the propensity score—are actually matched and therefore contribute information to the estimate of interest. With matched samples in hand, the final step is to estimate the counterfactual of interest.

Our approach is unique in several respects. First, we employ longitudinal data in order to study incarceration prospectively, which also allows us to control as rigorously as possible for an individual's accumulated criminal history. Second, we have detailed information on the offense of conviction, allowing us to control for a variety of characteristics of the instant offense. Third, we use official data on the number of convictions to estimate the incapacitation effect of imprisonment. While this will yield a lower bound estimate of the incapacitation effect, it does allow us to bypass the reliability problems that inhere in the use of self-report data (see Spelman 1994). Finally, we can distinguish

incapacitation effects by subjects' prior incarceration experiences. This allows us to esti-mate incapacitation effects of first-time imprisonment. Because we focus on first-time imprisonment, we can be sure that our estimate of the prison effect (if any) is measuring incapacitation rather than deterrence or rehabilitation.

Data and Methods

Our task in this study is to estimate the number of crimes prevented in the community by first-time imprisonment. The data used in this study are official criminal records of all offenders convicted in The Netherlands in 1997 (N = 153,252), made available by the Research and Documentation Centre (WODC) of The Netherlands Ministry of Justice. "Rap sheets" or recorded arrest histories of the offenders were also reconstructed using abstracts from the General Documentation Files (GDF) of the Criminal Record Office, which contains information on every criminal case registered by the police at the Public Prosecutor's Office.

For each offender sentenced in 1997, his or her entire officially recorded criminal history was compiled beginning with age 12 (the minimum age of criminal responsibility in The Netherlands) and ending in calendar year 2005. From the original sample, we impose several restrictions that invariably reduce the sample size eligible for the analysis. First, because first-time incarceration of the elderly is quite rare, we excluded convicted individuals who were older than 50 in 1997 (N = 17,470). Second, to examine incarcer-ation prospectively, we exclude offenders who were sentenced to prison prior to 1997 (N = 23,840). Third, while the GDF contain information on all registered offenses that led to any type of judicial action, we excluded offenders for whom the case resulted in acquittal or a prosecutorial disposition due to insufficient evidence (N = 321).[10] Fourth, offenders who were sentenced to both imprisonment and community service were excluded (N = 61), as were offenders who were sentenced for any period of time to a penitentiary mental hospital (N = 267). Finally, we omitted the small contingent of offenders with sentences longer than 2 years because of concerns that they might differ in unmeasured ways to such a degree that no credible counterfactual can be obtained (N = 257). The foregoing restrictions thus result in an estimation sample comprised of 111,036 offenders, representing 72.5 percent of the original sample. Of the 111,036 offenders 6,260 were incarcerated in 1997—and are the so called treatment group in our study.

Measures

The outcome variable in our study is the conviction rate prior to the imposed prison sentence (within-person counterfactual approach) and after the conviction to prison (between-person counterfactual approach). In the section 'Analytical approach' more information is provided for these measures.

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[10] In the Dutch criminal justice system, the public prosecutor has the discretionary power not to prosecute every case forwarded by the police. For instance, he or she may decide to drop the case if prosecution would probably not lead to conviction due to lack of evidence or for technical considerations (e.g., procedural or technical waiver). The public prosecutor is also authorized to waive prosecution "for reasons of public interest" (i.e., waiver for policy considerations). The Board of Prosecutors-General has issued national prosecution guidelines under which a public prosecutor may decide to waive a case for policy reasons, for example, if measures other than penal sanctions are preferable or more effective, prosecution would be disproportionately unjust or ineffective in relation to the nature of the offense or the offender, or prosecution would be contrary to the interest of the state or the victim (Tak 2003).

The independent variable of interest is first-time incarceration. All criminal sentences involving deprivation of liberty, including being placed in a reformatory school, are defined as incarceration. Recall that the analysis is limited to first-time incarceration beginning in calendar year 1997, of no more than 2 years' duration, of offenders between the ages of 12 and 50. Just over one in twenty (5.6 %) individuals in the sample meet these conditions (N = 6,260). The data also include information on the length of the sentence, which ranges from 2 days to 2 years, with 12 percent of prisoners sentenced to more than 1 year. As shown in Table 1, the mean sentence length is 146 days, or about 4.8 months.

The analysis also takes into account variables that prior research has demonstrated to be important as determinants of incarceration. First, we include three measures of the instant offense, or the 1997 conviction that brought the offender into the sample. These include the number of crimes involved in the instant offense, the type of offense, and the severity of the offense. Offense type is coded from a series of 20 dummy variables describing the nature of the most serious conviction offense (following the Statistics Netherlands classification).[11] The category 'other crimes' is set as reference category and includes less common offenses such as, deliberate deprivation of freedom and untruth in the use of a trade name or brand. For all offenses we also included an indicator of offense severity based on the maximum penalty (ranges from 0 to 20).

Second, we control for the official criminal history of the offender. We include a variety of measures of criminal history. We make a distinction between the number of registered offenses in the past year (total number of charges that led to a conviction in the 1 year period prior to the index conviction), and the total number of charges in the past 10 years prior to the index conviction. We included these measures for property, violent, and other crimes separately. Descriptive results show that the criminal histories of the offenders in our data are not extensive, probably because only those offenders who experienced no prior imprisonment are included in the analyses (see Table 1).

Third, we control for key sociodemographic characteristics of the offenders. These include sex, age, and nationality. Age is measured continuously at the time of conviction and is entered as a polynomial in the empirical model. Nationality is a dummy variable indicating whether the offender was born in The Netherlands (0 = No; 1 = Yes). Table 1 shows that the typical individual in the full sample is male (85 %), a Dutch native (70.4 %), and 29.2 years of age at the time of conviction.

Analytic Approach

The goal of this study is to provide an estimate of the incapacitation effect—the mean number of convictions prevented due to incarceration. The challenge of any such study is to estimate a quantity that cannot be directly observed. Because incarcerated offenders, by definition, lack the opportunity to commit crime in the community, this counterfactual offending rate must be estimated. The counterfactual in this setting represents a projection of the number of registered crimes the imprisoned offenders would have committed had he or she been free in the community rather than confined in prison. We employ two different methods to do so.

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[11] Fencing, property, theft, violent sexual, fornication (younger than 16 years), violent theft, threatening/assault, aggravated assault, aggressive severe injury, public violence, trespass, offense against authority, public order, insulting, arson endangering human life, other/cruelty to animals, traffic, opium act, weapons act, other crimes.

Table 1 Means and SD of offenders sentenced to prison (N = 6,260) and the estimation sample offenders (111,036) in 1997

	Imprisoned		Estimation sample	
	Mean	SD	Mean	SD
Length of incapacitation				
Length of incapacitation (in days)	145.96	158.36		
Social demographics				
Female	0.10		0.15	
Age	27.70	8.31	29.20	9.90
Non-native	0.61		0.30	
Type of offense/conviction				
Other crimes	0.04		0.14	
Trespass	0.00		0.00	
Public violence	0.02		0.04	
Offense against authority	0.00		0.00	
Public order	0.02		0.02	
Aggressive severe injury	0.05		0.01	
Insulting	0.00		0.00	
Threatening, assault	0.03		0.07	
Aggravated assault	0.01		0.01	
Arson endangering human life	0.01		0.00	
Other, cruelty to animals	0.01		0.05	
Violent sexual	0.02		0.00	
Fornication younger than 16 years	0.02		0.01	
Fencing	0.12		0.08	
Property	0.07		0.11	
Theft	0.20		0.09	
Violent theft	0.12		0.02	
Traffic	0.04		0.28	
Opium act	0.19		0.04	
Weapons act	0.02		0.03	
Criminal history				
Number of property crimes past year	0.13	0.59	0.04	0.30
Number of violent crimes past year	0.04	0.26	0.02	0.17
Number of other crimes past year	0.08	0.36	0.06	0.29
Number of property crimes past 10 years	0.40	1.09	0.17	0.63
Number of violent crimes past 10 years	0.14	0.50	0.07	0.33
Number of other crimes past 10 years	0.37	0.93	0.32	0.85
Index offense				
Number of crimes in case of conviction	1.79	1.13	1.21	0.61
Severity of offense	5.08	3.81	2.52	2.75

The first method parallels the incapacitation research conducted in the 1980s and 1990s, which uses the criminal behavior of offenders in the months leading up to their arrest or confinement as the counterfactual offending rate (e.g., Chaiken and Chaiken 1982; Horney
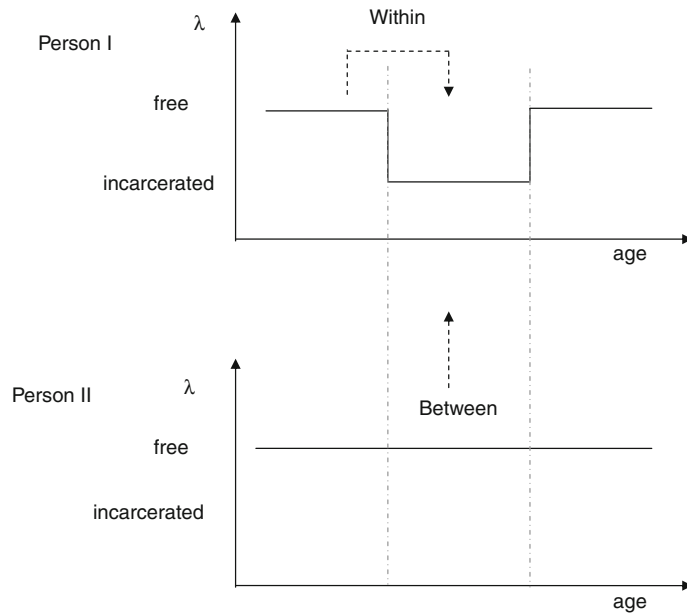
Fig. 1 Schematic presentation of within-individual and between-individual approach to estimate incapacitation effects

and Marshall 1991; Miranne and Geerken 1991). We refer to this estimate as a *within-person counterfactual*, because incarcerated offenders serve as their own counterfactual (see Fig. 1).

The second method is inspired by the recent work of Sweeten and Apel (2007), who rely on propensity score matching of incarcerated individuals with a sample of non-incarcerated individuals who are at high risk of incarceration based on their index conviction, their criminal history, and their sociodemographic background. We refer to this estimate as a *between-person counterfactual*, because a non-incarcerated comparison sample serves as the counterfactual (see Fig. 1).

In our study the incapacitation effects of both methods will be presented in the following two ways 1) an annual estimate and 2) an estimate controlled for the actual length of imprisonment. For the first strategy we estimate the number of convictions in the year leading up to confinement (within-person counterfactual approach) and we estimate the number of convictions in the year following the conviction to imprisonment (between-person counterfactual approach). We include an annual estimate to be able to compare our incapacitation estimates with findings of prior empirical studies on the incapacitation effect. Typically, these prior studies only show annual incapacitation estimates. In the second strategy to present the incapacitation effects we adjust for the actual length of imprisonment.[12] In The Netherlands prison sentences are considerably shorter than prison sentences in the US. Using a yearly metric for the incapacitation effect is therefore less

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[12] We did not calculate imprisonment effect for the exact duration of the prison term the offender was sentenced to, which was in most cases less than a full year, because we lack information on the exact timing of the actual prison term, as well as on the exact data of commission of the offense. Estimates were based on dates of registration of the offense and dates of sentencing.

appropriate for the Dutch context. We provide details on the between-person counterfactual approach in the next section.

Propensity Score Estimation and Balance Diagnostics

As discussed above, one way to estimate the incapacitation effect of imprisonment is to track the criminal behavior of matched controls during time periods post incarceration. This is the method of between-person counterfactual estimation. This between-person counterfactual approach relies on propensity score matching to identify pairs of incarcerated and non-incarcerated offenders, and uses the criminal behavior of non-incarcerated offenders to estimate the incapacitation effect.

The first step for propensity score matching is to develop a model of the assignment mechanism. In this study, this refers to the probability of first-time incarceration in 1997. We use a logistic regression model to estimate this probability. The results are shown in the "Appendix". We include as predictors a variety of measures of the current offense, the offender's criminal history, and the offender's sociodemographic profile. Consistent with the sentencing literature, the most salient determinants of incarceration are "legal" characteristics. The results show that compared to offenses in the category non-criminal law, the likelihood of being sentenced to prison is higher for most of the offense types, especially opium act offenses and violent theft are significant predictors of being sentenced to imprisonment. Furthermore, demographic characteristics also influence the likelihood of incarceration. For example, the odds for female offenders to be sentenced to prison are almost half than that for male offenders. For non-native offenders, the odds of incarceration are 3.6 times as high as that of Dutch born offenders.

The second step in a propensity score model is to evaluate the support condition. This concerns the degree of overlap between the propensity score distributions of incarcerated and non-incarcerated offenders. Regions of common support are those regions in which there is sufficient representation of both incarcerated and non-incarcerated offenders to yield matches, while regions that fail to meet the support condition (typically in the tails of the propensity score distribution) lack a sufficient number of either incarcerated or non-incarcerated individuals to match. The propensity score distributions, stratified by incarceration status, are illustrated in Fig. 2.

Figure 2 shows that there is sufficient common support: at nearly all propensity scores there are potential controls for the imprisoned. With this level of common support, the results do not relate to only a select (non-representative) part of the offenders in the experimental or control group which indicates that propensity score matching is a suitable method for this data.

With the propensity score in hand and the support condition satisfied, the third step is to examine covariate balance between incarcerated and non-incarcerated offenders. For each variable, two different statistics are estimated prior to matching as well as after matching. The goal is to ensure that the propensity score model successfully created equivalence between incarcerated and non-incarcerated offenders. The first statistic used is the well known t test for independent samples. The second statistic is the standardized difference endorsed by Rosenbaum and Rubin (1985).

Covariate imbalance is determined by a t test exceeding 1.96 in absolute value or a standardized difference exceeding 20 in absolute value. These balance diagnostics are shown in Table 2.

Panel A of Table 2 reports comparisons of the full sample of individuals who were imprisoned for the first time over the 12–50 age range with the individuals who were
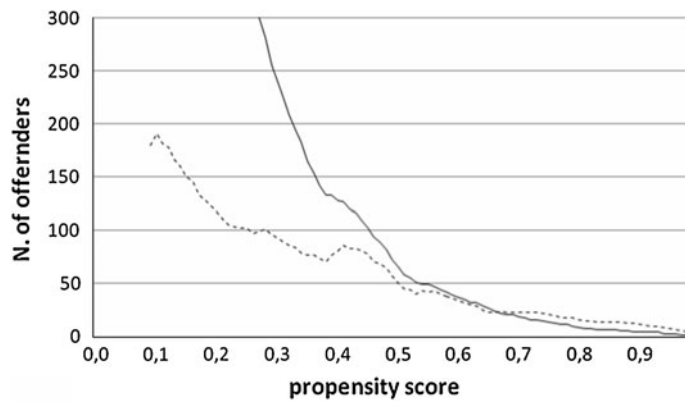
Fig. 2 Distribution of propensity scores of first-time imprisoned (*dashed line*) and sentenced but non-imprisoned (*solid line*) in full sample (N = 111,036). The lines are smoothed using a 10-point moving average. Note that the *grey line* enters Fig. 1 at a propensity score of 0.31. This means that a large group of offenders who were not convicted to imprisonment have propensity scores lower than 0.31. Thus, a large share of offenders in the control group has a very small chance to be sentenced to imprisonment based on observed confounders

convicted but not imprisoned. Clearly, prior to matching the imprisoned differ in important ways from the convicted but not imprisoned: for all variables the *t* test shows that the imprisoned differ statistically from the convicted but not imprisoned. Moreover, for many variables the standardized difference *D* is greater than 20. The imprisoned were more likely to be convicted of more severe crimes, they had a higher number of crimes in case of conviction and a more extended criminal history. Furthermore they were less likely to be female and of Dutch decent.

Panel B of Table 2 shows the balance diagnostics for the two matched samples of incarcerated and non-incarcerated offenders. Each incarcerated individual is matched to his or her nearest neighbor within a caliper of 0.05. Using this criterion, we were able to identify a suitable match for 91.17 % of all of the 6,260 incarcerated offenders in our sample, so that 5,707 incarcerated offenders are matched to 5,707 non-incarcerated offenders.[13] The results show that our matching procedure was very successful in achieving balance on the characteristics generally assumed relevant for both sentencing and criminal behavior.[14] Before matching there were many imbalances, but after matching, the number of significant differences is down to none according to the Standardized

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[13] Offenders from the experimental group for which we were unable to find a match differ from offenders in the experimental group that we could match. The propensity scores of the unmatched offenders in the experimental group were high compared to those of offenders we were able to find a match for; these offenders were too different in terms of observed variables from offenders in the control group. Additional analyses showed that the persons we were not able to match were relatively more criminally active in the year prior to their incarceration. Furthermore, female offenders are slightly underrepresented in the matched sample and regarding age the matched sample is on average younger (mean 25.61) compared to the unmatched sample (mean 27.95).

[14] Although age is included in the propensity score model, two matched offenders can have the same propensity score but might not actually be of the same age. Therefore, we additionally match directly on age, sex and offense type. The strategy of estimation within subpopulations defined by the covariates has precedence in the literature on the propensity score methodology (see Rosenbaum and Rubin 1984). We distinguish eight age categories: (1) 12-17; (2) 18-21; (3) 22-25; (4) 26-30; (5) 31-35; (6) 36-40; (7) 41-45; (8) 46-50.

Difference (D) and the t test.[15] This implies that we can be confident the incapacitation effect is not biased due to already existing differences between groups.

Results

Within-Person Counterfactual Approach

For comparative purposes, we show the incapacitation estimates using the within-person counterfactual approach. Here we restrict our attention to individuals incarcerated for the first time in 1997 (N = 6,260) and reconstruct their criminal history to estimate the incapacitation effect. When doing this we stratify the sample by age in 1997 so that we can distinguish juvenile from adult incarceration and we stratify by sex so that incapacitation effects for male and female offenders can be distinguished.[16]

First, we present an annual estimate of the incapacitation effect. This refers to the number of registered crimes committed in the year leading up to the conviction to imprisonment. Our baseline within-person estimate of incapacitation, in which the criminal histories of all offenders sentenced to their first imprisonment in 1997 are included, yield a total incapacitation effect of 0.17 (see Table 3). This is to say that each year of first-time incarceration avoids 0.17 convictions. Incapacitation effects are higher for male offenders (0.18) compared to female offenders (0.15) and the effect is higher for juveniles (0.29) compared to adults (0.16).

Second, we also present estimates of the incapacitation effects in which we controlled for the length of incarceration. We do so, because the sentence length of imprisonment in The Netherlands is short compared to the US—the average sentence length in this sample is only 0.4 year (or 4.8 months). When controlling for the length of imprisonment, the total within-person effect of incapacitation is 0.05. The average first-time prison sentence thus avoids 0.05 convictions. Consequently, in total 313 convictions are avoided by incarcerating 6,260 offenders for the first time in 1997.

Between-Person Counterfactual Approach

Since the propensity score model has been successfully used to create synthetic "treatment" and "control" groups based on incarceration status in 1997. We can now use the

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[15] Note that for the incarcerated persons 14.5 percent was correctly assigned in the full sample by the propensity score model compared to 44.2 percent in the matched sample.

[16] Note that, consistent with prior research, our current aim is to provide a single estimate of the average incapacitation effect for the entire population of those imprisoned for the first time—stratified by age period and sex. As one anonymous reviewer noted from a (selective) policy perspective it is also important to gain insight in the extent to which incapacitation effects are concentrated in the most active offenders. While we value this suggestion, it is to be noted that the current focus is on the incapacitation effects of first-time imprisonment and that variation in the number of prior offenses in our sample is thereby limited as those most active are also more likely to have been previously sentenced to prison. Effects of various selective scenarios using Dutch data were estimated in Blokland and Nieuwbeerta (2007) and Vollaard (2011). In the current study, we control for heterogeneity in the between-person counterfactual approach, because offenders are matched on pre-existing rates of offending. Pre-existing rates of offending at the time of conviction is said to be a perfect control for population heterogeneity (Bushway et al. 2004). For future research that focuses on selective incapacitation it would also be interesting to study the incapacitation effect stratified by propensity score. Such stratification gives insight in whether the highest (say) 10 percent of propensity scores can account for a disproportionate share of the total incapacitation effect.

Table 2 Differences between imprisoned and the convicted but not imprisoned—full and matched sample

	Panel A: full sample			Panel B: matched sample		
	Difference between imprisoned (N = 6,260) and the convicted but not imprisoned (N = 104,776)			Difference between imprisoned (N = 5,707) and convicted but not imprisoned (N = 5,707)		
	Absolute difference	Two-sample t statistic	D	Absolute difference	Two-sample t statistic	D
Social demographics						
Female	−0.05**	−12.92	−15.48	0.00	0.00	0.00
Age (/10)	−0.16**	−14.52	−17.31	0.00	0.18	0.34
Age_squared (/10)	−1.21**	−18.11	−21.26	0.01	0.12	0.23
Non-native	0.34**	53.17	71.73	−0.02	−1.67	−3.12
Offense characteristics						
Type of offense/conviction						
Other crimes	−0.10**	−34.47	−33.96	0.00	0.00	0.00
Trespass	0.00**	−3.78	−3.90	0.00	0.00	0.00
Public violence	−0.03**	−14.67	−15.18	0.00	0.00	0.00
Offense against authority	0.00**	−6.32	−5.88	0.00	0.00	0.00
Public order	0.01**	3.29	4.58	0.00	0.00	0.00
Aggressive severe injury	0.05**	15.75	26.15	0.00	0.00	0.00
Insulting	0.00**	−12.82	−8.65	0.00	0.00	0.00
Threatening, assault	−0.04**	−16.66	−17.93	0.00	0.00	0.00
Aggravated assault	0.01**	4.86	7.31	0.00	0.00	0.00
Arson endangering human life	0.01**	7.16	12.07	0.00	0.00	0.00
Other, cruelty to animals	−0.03**	−21.35	−19.99	0.00	0.00	0.00
Violent sexual	0.02**	9.37	15.54	0.00	0.00	0.00
Fornication < 16 years	0.02**	9.10	14.87	0.00	0.00	0.00
Fencing	0.05**	11.64	16.57	0.00	0.00	0.00
Property	−0.04**	−13.01	−15.20	0.00	0.00	0.00
Theft	0.12**	22.60	33.71	0.00	0.00	0.00
Violent theft	0.11**	26.00	44.32	0.00	0.00	0.00
Traffic	−0.26**	−97.12	−75.78	0.00	0.00	0.00
Opium act	0.16**	31.67	51.99	0.00	0.00	0.00
Weapons act	−0.01**	−8.56	−9.50	0.00	0.00	0.00
N. of crimes in case of conviction	0.62**	42.98	69.69	0.02	0.81	1.52
Severity of offense	2.71**	55.52	83.12	0.03	0.41	0.77
Criminal history						
Property crimes past year	0.10**	16.05	27.94	0.01	0.91	1.70
Violent crimes past year	0.04**	11.53	20.08	0.01	1.11	2.08
Other crimes past year	0.06**	14.88	25.74	−0.01	−1.05	−1.97
Property crimes past 10 years	0.33**	27.20	47.22	0.02	1.21	2.26

Table 2 continued

	Panel A: full sample			Panel B: matched sample		
	Difference between imprisoned (N = 6,260) and the convicted but not imprisoned (N = 104,776)			Difference between imprisoned (N = 5,707) and convicted but not imprisoned (N = 5,707)		
	Absolute difference	Two-sample t statistic	D	Absolute difference	Two-sample t statistic	D
Violent crimes past 10 years	0.11**	19.54	33.96	0.01	0.89	1.66
Other crimes past 10 years	0.31**	27.87	47.68	−0.02	−1.11	−2.08

* $p < .05$; **$p < .01$

criminal behavior of the matched non-incarcerated sample (N = 5,707) to estimate the number of registered crimes that would have been committed by incarcerated individuals had they not been imprisoned.

Here, the annual estimate of the incapacitation effect refers to the number of registered crimes committed by the person in the control group in the year following the conviction to imprisonment of the matched person in the experimental group. Our baseline between-person estimate of incapacitation, which assumes that incarcerated individuals are no different from the non-incarcerated in terms of expected offending, yield a total incapacitation effect of 0.21 (see Table 3). This is to say that each year of first-time incarceration avoids 0.21 convictions. Similar to the incapacitation estimates of the within-person counterfactual approach, the incapacitation effect is higher for male offenders (0.22) compared to female offenders (0.12) and the effect is higher for juveniles (0.32) compared to adults (0.20).

When controlling for the length of imprisonment, the total between-person effect of incapacitation is 0.07—first-time incarceration thus avoids 0.07 convictions. Therefore, in total almost 400 convictions are avoided by incarcerating the 5,707 matched offenders for the first time in 1997.

Table 3 Estimates of crimes averted through incapacitation (between and within person estimates) including 95 % confidence intervals

	Within-person approach (N = 6,260)		Between-person approach (N = 5,707)	
	Per year	Per incarceration	Per year	Per incarceration
Group				
Male	0.18(0.17–0.19)	0.06(0.05–0.06)	0.22(0.21–0.24)	0.08(0.07–0.09)
Female	0.15(0.11–0.18)	0.03(0.02–0.04)	0.12(0.08–0.15)	0.03(0.02–0.04)
Juvenile (12–17)	0.29(0.24–0.34)	0.07(0.06–0.09)	0.32(0.27–0.37)	0.08(0.06–0.09)
Adult (18–50)	0.16(0.15–0.18)	0.05(0.05–0.06)	0.20(0.18–0.22)	0.07(0.06–0.08)
Total	0.17(0.16–0.19)	0.05(0.05–0.06)	0.21(0.20–0.23)	0.07(0.06–0.08)

Discussion and Conclusion

One possible way in which imprisonment can reduce crime is through incapacitation. Here a period of "time out" is imposed from the criminal career, thereby removing the opportunity for an individual to commit crime in the community for the duration of his or her sentence. The aim of this study has been to provide estimates of the degree to which first-time imprisonment incapacitates offenders from committing crime in the community.

We relied on two distinct approaches using a sample of all offenders convicted of a crime in The Netherlands in 1997. The first is a within-person counterfactual approach in which the incapacitation effect is estimated by investigating the numbers of crime committed by persons prior to their first incarceration in 1997. The second, inspired by the recent work of Sweeten and Apel (2007), is a between-person counterfactual approach in which offenders who were convicted but not-imprisoned in 1997 were matched to those individuals convicted to imprisonment in 1997. To estimate the incapacitation effect we investigate the criminal behavior of the matched controls after their matched counterpart was sent to prison.

Using the within and between-person counterfactual approach, our best estimate is that between 0.17 and 0.21 convictions are prevented per year of first-time incarceration. Distinguishing for different social groups, our results show larger first-time incapacitation effects for male offenders compared to female offenders and for juveniles compared to adults. Our incapacitation estimates are considerably smaller than those of earlier studies. For example, in the Rand studies the mean offending rate was estimated to be between 187 and 278 crimes per year (e.g. Chaiken and Chaiken 1982) and in a more recent study by Sweeten and Apel (2007) the incapacitation effect was estimated to be 9 offenses per year among 16–17 year olds, and about 6 offenses per year among 18–19 year olds.

Our study differs in a number of important ways from earlier studies addressing the incapacitation effect, so it is difficult to isolate what exactly accounts for the large difference between the estimates. There are at least two important differences compared to the Rand study. The first important difference is that we used conviction rates to estimate the incapacitation effect instead of self-reported criminal behavior, which by definition will lead to a lower incapacitation effect. The second important difference is that unlike the Rand study we focus solely on first-time imprisonment, while the Rand study focused on all incarcerated offenders. Therefore, the offenders included in the Rand study are more crime prone compared to offenders in our study. The treated group in the Rand study is more serious with regard to criminal history and it includes persons who were not deterred by their first prison sanction. The study by Sweeten and Apel (2007) also made use of self-report data to measure the incapacitation effect. This, at least partly, could explain the difference between incapacitation estimates. Another important difference with Sweeten and Apel (2007) is the age range of the treated. Their focus was on adolescents at near the peak age of offending while the current study included offenders aged 12–50. Additional research using charges or self-reported criminal behavior and studies on the incapacitative effects of following prison terms would help to resolve this issue.

To provide estimates that more closely allign with actual criminal behavior prevented by first-time incarceration we performed additional calculations using the European Sourcebook (Killias et al. 2003). In 1997, a total number of 7,807 criminal offenses per 100,000 were recorded by the police in the Netherlands. These offenses led to a total number of convictions of 648.4 per 100,000. As such, roughly 12 criminal offenses recorded by the police led to one conviction in 1997. The results of the current study showed that the annualized estimate of the incapacitation effect is between 0.17 and 0.21

convictions. Multiplying that with factor 12 suggests that between 2.04 and 2.52 criminal recorded offenses were avoided per year of first-time incarceration. Although these are back-of-the-envelope estimates, they indicate that our results are fairly in line with prior findings of the new generation of incapacitation studies (see e.g. Owens 2009; Sweeten and Apel 2007).

Our findings consistently show slightly higher incapacitation estimates using the between-person counterfactual approach compared to the within-person counterfactual approach. On the one hand, this is surprising given the possible presence of "crime spurts" which would lead to higher incapacitation effects when use is made of the within-person counterfactual approach. On the other hand, unlike the within-person approach estimate, the between person estimate of the incapacitation effect accounts for the possible criminogenic effects of the 1997 conviction. Prior research has shown that a prior conviction increases the odds of a future conviction (Blokland and Nieuwbeerta 2010). The extent that criminogenic effects are present in the control group will only lead to higher estimates of the incapacitation effect in the between-person approach. We feel this is an additional strength of the between-person approach since those incarcerated in 1997 would in all likelihood have received a different type of sanction if not sentenced to prison. The convicted not incarcerated offenders thus constitute the proper counterfactual. That said, the gap between the estimates generated by both methods is narrow, increasing the validity of our results. Results of any study may be highly sensitive to the analytical technique applied to the question (Sweeten and Apel 2007). Every technique has its limitations and therefore its results are sensitive to bias. While more complicated does not necessarily equal better, the strength of the technique is in the extent to which it is able to minimize sources of possible bias. While the strength of the within person method may be its clarity, the strengths of the between person method is that it accounts for sources of bias such as effect, crime spurts, and the contagion effects of conviction.

In addition, a few further comments should be made. First, our analyses focus only on the incapacitation effect of imprisonment and do not take into account general or specific deterrent effects. If these effects are large, the crime control benefits of imprisonment may prove much more beneficial. However, our within and between-person approaches did not include the possible criminogenic influence of imprisonment either. One objective of incapacitation is a safe society, however, previous research has shown that imprisonment can have the potential to exacerbate long-term criminal offending (Nagin et al. 2009; Wermink et al. 2010).

In conjunction with these prior findings, the current results inform both penal policy and the public discourse surrounding the use of imprisonment as the societal reaction to crime. The current results suggest that in as far as imprisonment is used with the primary goal of reducing crime, a general increase of imprisonment as the sanction of choice is not likely to yield high crime control benefits. While the incapacitative effects of imprisonment are likely to be larger when specific offender groups are selectively targeted, studies examining the effects of such selective policies mostly find rather disappointing effects (e.g. Blokland and Nieuwbeerta 2007, but see: Vollaard 2011). In addition, prior studies indicate that even a relatively short prison sentence may have long-lasting detrimental effects in terms of increased offending (Nieuwbeerta et al. 2009; Wermink et al. 2010). Given the incapacitation effect of first imprisonment is not that large to begin with, it is therefore likely to be offset—if not overruled—by the long-term collateral effects of incarceration. This is not to say that imprisonment does not have any function in the criminal justice system. It does however clearly point to its limitations and downsides, underscoring that imprisonment is to be regarded not as a panacea to the crime problem, but merely as an ultimum remedium.

Appendix

See Table 4

Table 4 Determinants of receiving imprisonment in 1997 (N = 111,036 of which 6,260 coded 'imprisonment')

	Coefficient	SE	Sign.	Exp(B)
Social demographics				
Female	−0.555	0.047	**	0.574
Age (/10)	2.851	0.118	**	17.301
Age_squared (/10)	−0.426	0.019	**	0.653
Non-native	1.290	0.032	**	3.633
Type of offense/conviction				
Other crimes	(ref.)	(ref.)		(ref.)
Trespass	0.276	0.381		1.318
Public violence	0.133	0.124		1.142
Offense against authority	−0.292	0.366		0.747
Public order	1.249	0.118	**	3.486
Aggressive severe injury	1.558	0.118	**	4.750
Insulting	−0.848	0.584		0.428
Threatening, assault	0.180	0.095		1.197
Aggravated assault	1.004	0.157	**	2.730
Arson endangering human life	1.042	0.218	**	2.836
Other, cruelty to animals	−0.357	0.132	*	0.700
Violent sexual	1.512	0.159	**	4.534
Fornication younger than 16 years	1.846	0.142	**	6.334
Fencing	0.590	0.085	**	1.804
Property	0.113	0.086		1.120
Theft	1.141	0.085	**	3.131
Violent theft	1.946	0.124	**	7.002
Traffic	−0.810	0.092	**	0.445
Opium act	2.769	0.072	**	15.943
Weapons act	0.420	0.123	**	1.522
Number of crimes in case of conviction	0.559	0.016	**	1.748
Severity of offense	0.168	0.010	**	1.183
Criminal history				
Number of property crimes past year	0.318	0.031	**	1.374
Number of violent crimes past year	0.282	0.065	**	1.326
Number of other crimes past year	0.066	0.046		1.068
Number of property crimes past 10 years	0.191	0.016	**	1.211
Number of violent crimes past 10 years	0.231	0.035	**	1.260
Number of other crimes past 10 years	0.123	0.016	**	1.131
Constant	−9.848	0.188	**	
R2 (Nagelkerke)—Pseudo	34.6 %			

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

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