

Short-Term Effects of Imprisonment Length on Recidivism in the Netherlands

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Abstract

This article assesses the relationship between imprisonment length and recidivism. The data come from a unique longitudinal and nationwide study of Dutch prisoners, serving an average of 4.1 months of confinement ($N = 1,467$). A propensity score methodology is used to examine the dose–response relationship for three types of registered recidivism (i.e., reoffending, reconviction, and reincarceration) within a 6-month follow-up period. Findings indicate that length of imprisonment exerts an overall null effect on future rates of recidivism and that this conclusion holds across the various types of recidivism. These findings contribute to continuing scholarly debates over the social and economic costs of imprisonment.

Keywords

imprisonment, dose–response, recidivism, propensity score methodology

An important question for criminal justice policy is whether imprisonment can reduce crime in society. In many Western countries, imprisonment is the

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most severe sanction that can be imposed. Worldwide imprisonment rates are high—at present more than 10 million people are confined in penal institutions (Walmsley, 2015)—and societies spend a lot of money on incarcerating people (Henrichson & Delaney, 2012; Nauta, Moolenaar, & van Tulder, 2011). For instance, in the Netherlands prosecution, sentencing, and the execution of punishment cost the Dutch taxpayer approximately 3.3 billion Euros each year, of which the largest part is spent on the execution of prison sentences. These high costs of placing so many individuals in custody are usually legitimized by the expectation that imprisonment controls crime, and therefore will contribute to public safety. Indeed, next to retribution, there are different goals of imprisonment that intend to reduce crime in society, such as rehabilitation, incapacitation, general deterrence, and specific deterrence (Von Hirsch, Ashworth, & Roberts, 2009). Hence, for informed justice policy debate empirical knowledge on the actual influence of imprisonment on crime is of direct importance, especially because recidivism rates among released offenders tend to be high, reaching 47% within 2 years (Wartna, Tollenaar, Verweij, Alberda, & Essers, 2016). This knowledge is also important for legal actors to make sentencing decisions that are just and that yield the highest crime-control benefits.

In the current study, we limit our attention to the mechanism of specific deterrence and focus on the relationship between time served and recidivism. Empirical investigations of imprisonment and reoffending represent a long-standing research tradition in criminology. Most of this research focused on the effects of custodial versus noncustodial sanctions (for instance, Bales & Piquero, 2012; Nieuwbeerta, Nagin, & Blokland, 2009), and reports mixed evidence, with some studies finding no effect of prison on recidivism, some finding that incarceration reduces recidivism, and others finding that it is criminogenic (Mears, Cochran, & Cullen, 2015). While essential “. . . vital for both public policy and science” (Nagin, Cullen, & Jonson, 2009, p. 49), the relationship between length of stay and reoffending has received far fewer scholarly attention. This is unfortunate because more solid knowledge about the level of confinement that may produce the highest public safety benefits is of paramount importance for policy and society (Loughran et al., 2009). Although the few empirical studies that have been conducted have provided important and substantial contributions to our understanding of the crime-preventive effects of different lengths of confinement, and have led to more critical views toward the idea that prisons reduce recidivism, they are also characterized by a number of limitations. Perhaps, the most important is the inability to assure adequate comparability between prisoners who serve different lengths of imprisonment due to reliance on problematic study designs and data limitations (Nagin et al., 2009). The magnitude of imprisonment

length's contribution to recidivism outcomes thus remains an unsettled area of research.

To our knowledge, only four studies have compared recidivism after different lengths of imprisonment using a quasi-experimental design (Loughran et al., 2009; Meade, Steiner, Makarios, & Travis, 2012; Rydberg & Clark, 2016; Snodgrass, Blokland, Haviland, Nieuwbeerta, & Nagin, 2011). Yet, the focus on specific contexts and offender populations compromises the generalizability of previous results, and the sole reliance on administrative data increases the possibility that results are biased because certain key constructs remain unobserved. The current study therefore aims to extend prior research in multiple ways. This study uses longitudinal data and analyzes recidivism outcomes by imprisonment length in a nationwide sample of Dutch offenders. Relying on Dutch data provides the opportunity to assess the generalizability of previous dose-response research findings from studies conducted mostly in the United States to contemporary non-American correctional systems and societies. We use a propensity score methodology to minimize concerns about selection bias (also see Shadish, 2013), and are able to control for many possible confounding variables (informal social control factors, substance abuse, criminal career information) because unique interview data were combined with administrative data.

The Dose-Response Relationship in Criminological Perspective

The central question in this study is to what extent longer imprisonment length is more, or less, effective in controlling recidivism. From a theoretical perspective, it is difficult to determine the direction of this relationship because different theories result in contrasting expectations. On one hand, based on deterrence theory it should be expected that a longer stay results in *less* recidivism (Von Hirsch et al., 2009). Deterrence theory is an economic approach to criminal behavior in which individuals are assumed to make rational calculations of the costs and benefits of criminal behavior. These calculations are based on individuals' estimates of the certainty and severity of punishment as a consequence of their criminal behavior (Becker, 1968; Ghali, 1982). The costs associated with serving time reduce the likelihood of reoffending, because of the fear of a future sentence or serving time in the future (Nagin et al., 2009; Spohn & Holleran, 2002). It is expected that the deterrent effect is stronger for longer periods of imprisonment because they provide an increased cost for future involvement in crime compared with shorter periods (Loughran et al., 2009; Nagin et al., 2009).¹

Other theoretical arguments, however, suggest that longer prison terms may result in *higher* recidivism rates. For instance, control theories postulate that bonds to conventional society, i.e., belief in societal norms, attachment to nondeviant others, and involvement and commitment in conventional activities, can encourage individuals to refrain from committing crime (Hirschi, 1969; Sampson & Laub, 1995). Longer imprisonment length removes individuals for a more extended period of time from the law-abiding community, and as a consequence deteriorates social bonds more than shorter prison sentences will do. For instance, long prison terms may have detrimental effects on family and work relationships. Moreover, human capital, such as work-related skills and experience, goes unused due to longer confinement which may negatively affect the chances of a conventional life after release from prison. In addition, differential association and learning theories assume that criminal behavior is learned from deviant others (Sutherland, 1947). From these theories, it can be derived that offenders with longer prison spells are more exposed to deviant values, and as a consequence can become more deeply embedded in criminal networks (Akers, 1997; Sutherland, 1947). Finally, labeling theory suggests that legal sanctions contribute to the development of a criminal career rather than prevent offenders' involvement in crime (Becker, 1963). According to this theory, individuals are being treated as offenders by others, and subsequently the offender internalizes this label and starts to behave in accordance with this label. Longer prison terms may result in a criminal identity that is more strongly internalized, which in turn leads to increased offending after release. Longer prison terms interrupt with conventional activities in such a way that labeling effects tend to be stronger. For instance, more relatives and (ex-)colleagues will be informed about the individual's imprisonment as it gets increasingly difficult to use excuses for their absence.

Summarizing, criminological theories result in contradicting hypotheses about how longer prison sentences will affect recidivism. Sound empirical research on the dose–response relationship between time served and recidivism is therefore needed.

Two Generations of Empirical Research on Time Served and Recidivism

In this section, we review past empirical efforts to estimate the effect of time served on recidivism. We do not attempt to give an exhaustive review of the literature but rather focus primarily on a number of recent studies that help contextualize the current study. Interested readers may consult reviews that

were published elsewhere (see Durlauf & Nagin, 2011; Gendreau, Goggin, & Cullen, 1999; Nagin et al., 2009; Tonry, 2011).

Two generations of studies can be distinguished in the research tradition in examining effects of time served on offending. The first generation of studies typically uses regression-based techniques to estimate the dose–response relationship. This empirical literature shows mixed results. While some studies found evidence for either negative or positive effects of length of stay on recidivism, other studies did not find a significant relationship. Not only is it difficult to draw firm conclusions based on these mixed results, it is also questionable whether these studies were able to approximate a *causal* effect of length of stay (Nagin et al., 2009).²

Most of these first-generation studies did not specifically focus on the dose–response relationship but rather included time served together with many other characteristics as controls. This is problematic for the interpretation of the causal effects of time served because selection processes may occur that are not accounted for by these studies. For instance, selection effects may occur because judges already consider offender dangerousness in their sentencing decisions to protect the community from criminal behavior (Steffensmeier, Ulmer, & Kramer, 1998). Empirical evidence indicates that characteristics related to recidivism, such as criminal history (Farrington, 1992; Gendreau, Little, & Goggin, 1996), are also robust predictors of length of imprisonment (Spohn, 2000; Zatz, 2000). When studies fail to account for such selection effects, the result might be the by-product of lengthier terms of imprisonment being assigned to the more crime-prone offenders. Consequently, it is difficult to determine whether the effects are actually caused by different sentence lengths or by preexisting differences in offenders' crime proneness.

The second-generation dose–response studies were specifically designed to overcome some of the shortcomings of the first-generation studies by using stratification on the propensity score as methodology. Up to date, empirical investigations of this ilk remain limited, with a total of four studies that applied this methodology to estimate the effect of length of imprisonment on recidivism. Stratification on propensity scores can be used to control for preexisting differences between offenders serving different confinement lengths as much as possible, and is better able than other studies of the first generation to make a causal interpretation of the results regarding sanction effectiveness. In Table 1, we present the main characteristics of the second-generation dose–response studies.

Loughran and colleagues (2009) were the first of this new generation who applied this methodology to estimate the dose–response

relationship. Data on 419 serious juvenile offenders in Maricopa and Philadelphia counties were used who served an average of 11 months. The follow-up period started after the baseline interview that was completed within 3 months after the juvenile had been adjudicated delinquent or found guilty. Prior to stratification, more than 40% of all covariates were imbalanced, and after stratification only 1 out of 66 covariates remained out of balance. Their results show neither a criminogenic nor a preventive effect of longer imprisonment length for terms between 3 and 13 months on rearrest and self-reported reoffending.³ Snodgrass et al. (2011) used administrative data on 4,096 juvenile and adult offenders incarcerated in the Netherlands in 1997. More than 85% of the suspects were sentenced to prison for a maximum of 1 year. After stratification on propensity scores, the imbalance between groups was reduced from 34 to 4 out of 45 covariates. The results showed no significant differences between imprisonment length and the reconviction rate and the proportion of offenders who were reconvicted. The relationship between imprisonment length and future incarceration length was not significant for offenders of whom the difference in time served was less than 10 months. Meade and colleagues (2012) used administrative data of 1,989 adult offenders in the state of Ohio. After applying the stratification on propensity score, the imbalance was reduced from 12 to one out of 20 covariates. The median length of stay was 24 months for released offenders, and rearrest for a new felony offense was used to establish the dose–response relationship. Significant effects of length of stay were only found for offenders who served at least 5 years—this prisoner group recidivated the least. The most recent study in this tradition was conducted by Rydberg and Clark (2016), who used administrative data of 103,438 adult offenders released under supervision in four states in the United States. The median imposed prison sentence was 5 years. Although imbalance was reduced after propensity score stratification, a significance test per covariate was not provided. Results showed that longer imprisonment in general increased revocations and reduced reincarceration. Because considerable heterogeneity was observed across offense types, they conclude that no clear support was found for either suppressive or criminogenic effects of imprisonment length.

Overall, findings of the limited number of second-generation studies suggest that there is little evidence of a relationship between time served and recidivism. Some caution is still warranted, though, because the focus on specific offender populations (juveniles, offenders released under supervision, or offenders sentenced in specific court systems) limits the generalizability of previous second-generation findings, and it remains unknown

what the effects of prison length are in a contemporary legal context with relatively short prison sentences and an extensive welfare system. Moreover, previous second-generation studies remain vulnerable to sources of unobserved bias because they rely predominantly on administrative datasets. The extent to which propensity score methodologies can reduce bias depends to a large extent on the degree to which the variables in the propensity score model capture the process of selection into treatment (Shadish, 2013). Administrative datasets, in particular, are not well suited to ascertain comparability between groups of prisoners who serve different imprisonment lengths as certain key constructs, such as informal social control factors and substance abuse, are often unavailable to researchers. A final omission is that virtually no research attention has been devoted to the fact that imprisonment may have heterogeneous effects.

This study extends previous research in four ways. First, this study examines a more representative sample of cases originating from a nationwide setting. Second, the study measures the effects of imprisonment length in a contemporary international context, focusing on the Netherlands. The penal climate in the Netherlands is considered to be relatively mild with a prison population that decreased unintermittedly after 2005 and sentences that tend to be short (Beijersbergen, Dirkzwager, van der Laan, & Nieuwbeerta, 2016). Although short terms of incarceration are the norm in the United States (in jail terms) and in Western Europe (in prison terms), very little contemporary work examines their consequences. For instance, most of the previous second-generation dose–response studies are conducted using a sample in which prison terms approximate 1 year or much longer. This study, therefore, offers a valuable opportunity to assess the generalizability of prior research. Third, detailed interview data were coded and combined with administrative data to better capture the process of selection into treatment and reduce omitted variable bias. Finally, the study investigates whether imprisonment length exerts a variable effect for specific prison populations. Most second-generation studies of dose–response effects have examined only an average treatment effect treating imprisonment as a uniform experience. Yet, imprisonment constitutes a heterogeneous type of sanction, and it is important to empirically address incarceration heterogeneity to provide greater insight into the effects of incarceration (see also Mears et al., 2015).

In the next section, we further discuss how in-prison experiences may vary among offenders, and elaborate on sentencing practices in the Netherlands.

Table 1. Existing Research on Imprisonment Length and Recidivism Using a Propensity Score Methodology.

Author	Country	Length of confinement	Population	Recidivism	Covariates in PS model	Outcome
Loughran et al. (2009)	United States (Maricopa, Philadelphia)	Time served (including detention prior to trial) measured in two ways: 1: 0-6 months; 6-10 months; >13 months. 2: 0-3 months; 3-6 months; 6-9 months; 9-12 months; >12 months.	Serious juveniles aged 14 through 17 placed in institution (N = 419) between November 2000 and January 2003. Majority were placed in contracted residential setting	Rate of rearrest (standardized to yearly rate) Self-reported offending (standardized to yearly rate) F-U: Maximally 48 months	66 from administrative and self-report data (including age, sex, prior criminal offending, and situational factors, such as substance use and social bonds; but no information about offense severity)	N.S. for offenders serving 3 to 13 months ^a
Snodgrass et al. (2011)	The Netherlands	Imposed prison length (including detention prior to trial): <1 month; 1-2 months; 2-3 months; 3-6 months; 6-12 months; >12 months.	Felony offenders aged 12 to 40 who committed a violent, property, or drug offense and were sentenced to prison for the first time in 1997 (N = 4,096)	Rate of recidivations Recidivism Length of reincarceration F-U: At least 3 years	44 from administrative data (including age, sex, offense severity, and prior criminal offending)	Rate of recidivations: N.S. Proportion reconvicted: N.S. Length of reincarceration: N.S. ^b
Meade et al. (2012)	United States (Ohio)	Time served in prison: ≤12 months; 13-24 months; 25-36 months; 37-60 months; ≥61 months.	Offenders released from prison under supervision (N = 1,989) in October–December 2003 or August–October 2005	Felony rearrest F-U: 1 year	20 from administrative data (including age, sex, offense severity; but no direct measure of prior criminal offending)	N.S. for offenders serving 0 to 60 months S. lower for offenders serving >60 months
Rydberg and Clark (2016)	United States (Michigan, New York, Missouri, Utah)	Time served: <12 months; 12-23 months; 24-35 months; 36-48 months; >48 months.	Offenders released from prison under supervision (N = 104,447) between 2004 and 2008	Parole revocations Reincarceration F-U: Maximally 3 years	~12 from administrative data (including age, sex, race, limited controls for offense severity, and criminal history)	Parole revocations: S. higher with incremental increase in prison length Reincarceration: Lower with incremental increase in prison length

Note. PS = propensity score; F-U = follow-up; N.S. = not significant.

^aThe authors have argued that it was not possible to make solid inferences outside this range.

^bNot significant, if offenders are at most three dose levels apart.

The Netherlands as Context

Since the 1970s, the prison population in the Netherlands has grown almost fourfold between 1975 and 2005, and at the end of that period it had one of the highest incarceration rates in Western Europe. However, a prolonged period of decarceration lasted from 2005 and 2014 in which the prison population halved. Currently, approximately 32,000 persons enter a Dutch detention facility each year, of which half enter a pretrial detention facility and half enter prison (Linckens & de Loeff, 2013). Of the prison population, 55% are born in the Netherlands and 92% are male.

Although the prison population is small compared with the United States, sanction policies in the Netherlands are in many ways reflective of practices in most of Western Europe. In particular, throughout Western Europe, the prison incarceration rate is much lower than that in the United States, and prison terms tend to be shorter. For example, the prison population rate is 84 per 100,000 in the Netherlands (Aebi et al., 2014). Other Western European regions, such as Scandinavia and Germany, have a fairly similar prison population rate; for instance, 72 per 100,000 in Sweden and 87 per 100,000 in Germany. In contrast, with an imprisonment rate of 612 per 100,000, the United States is unique in its scale of imprisonment (Carson, 2015). Moreover, the Dutch average prison term of approximately 4 months (Linckens & de Loeff, 2013) is short compared with the United States. All other countries in Europe, except Moldova and Azerbaijan, also have (much) shorter average prison terms compared with the United States, and two thirds of the 47 surveyed countries have prison terms that are on average shorter than 1 year (Aebi, Tiago, & Burkhardt, 2015). In the United States, 97% of the prisoners were sentenced to more than 1 year (Carson, 2015), with state prisoners serving on average 2 years (Guerino, Harrison, & Sabol, 2011). These aspects of the Dutch legal context make it a particularly instructive context for studying the dose–response relationship, in particular because it can fill an important empirical gap where short prison terms are concerned.

As in many Western legal systems, criminal suspects in the Netherlands can either be detained following their arrest prior to conviction or after their conviction. Pretrial detainees account for a relatively large portion of the prison population in the Netherlands. Suspects can first be detained prior to conviction for a maximum period of 90 days, and this term can be extended twice by a maximum of 90 days. Suspects in pretrial detention can be released prior to their conviction if the grounds for pretrial detention are no longer valid (i.e., flight risk or public safety concerns) or if the prison sentence is probably not going to exceed the time already served in pretrial detention. Courts in the Netherlands are legally required to take the term of pretrial

detention into account in sentencing decisions (see Art. 27 Sv). When suspects are still held in pretrial detention when the sentence is imposed, judges may impose a less severe type of sanction, a “time served,” or a prison sentence that exceeds the time served in pretrial detention.

Although terms of incarceration often involve some portion of time served in pretrial detention, it is not always clear how these subtleties are handled in extant prison effectiveness research. In-prison differences among prisoners occur because pretrial detainees may or may not be released prior to conviction, and the final sentence either exceeds or does not exceed the term already served in pretrial detention. Mears and colleagues (2015) argue that effects of incarceration may vary depending on such differences in specific in-prison experiences. For instance, the experienced severity of imprisonment depends on the deprivations of prison life that prisoners are facing (Liebling, 2011; Pogrebin & Dodge, 2001). A longer term in pretrial detention may therefore result in a stronger deterrent effect, because inmates have very limited opportunities to participate in in-prison activities and spend the vast majority of their time alone in their cell. The latter aspect of pretrial detention also results in less opportunities to adopt deviant attitudes or learn criminal behavior from other inmates. Such mechanisms may result in effects of imprisonment length that differ among distinct groups of prisoners. In addition, according to Zimring and Hawkins (1973), the deterrent effect of imprisonment not only depends on the experienced severity during incarceration, but also on the remembered severity after release. Receiving a time served may therefore lead to an adjustment of offender’s assessment of sanction severity, because the final sentence may have turned out better than expected.

To provide insight into potential heterogeneity in dose-relationship estimates, we stratify the dose-response relationship for four groups of prisoners based on their pretrial release status and sentence.

Data

The data used in this study were collected as part of the Prison Project, a longitudinal study in which individuals entering pretrial detention in all penitentiary institutions in the Netherlands were interviewed and followed over time. The project targeted male prisoners who entered a Dutch pretrial detention facility between October 2010 and March 2011, were born in the Netherlands, and were between 18 and 65 years old (Dirkzwager & Nieuwbeerta, 2015). Suspects who met our selection criteria were interviewed about 3 weeks after their arrival at pretrial detention.

Of the 3,983 prisoners who met our selection criteria, 71% could be approached to participate ($N = 2,841$). We were mainly unable to approach those prisoners who were already released before the interview was conducted. Of all prisoners who were contacted by us, we obtained interview data from 1,904 prisoners (67%), and of them, 1,764 completed an additional written questionnaire. The inmates who participated in the Prison Project are generally representative of all prisoners who met our selection criteria in terms of age, marital status, receiving an unconditional prison sentence, and committing a violent crime. Participants and nonparticipants differ in age of onset (18.6 vs. 18.2) and being employed before imprisonment (45.6% vs. 41.4%). The duration of actual time served was longer for participants than for nonparticipants (6.6 vs. 3.5 months), because many nonparticipants were released before the interview was conducted. A smaller difference in time served is found between the 2,841 participants and the 937 refusers (6.63 vs. 6.47 months). In addition, a comparison of criminal history measures revealed that participants have a less extensive criminal history than refusers (3.5 vs. 4.5 previous spells; 8.8 vs. 10.5 previous convictions).

The Prison Project dataset was extended with multiple types of administrative data to obtain information on full criminal careers, offense- and case characteristics, and other life circumstances. Data on life circumstances, such as marriage and number of children, were retrieved from the decentralized (municipal) population registers (GBA). In addition, data on officially recorded criminal careers were retrieved from the general documentation files (GDF) and were made available by the Research and Documentation Centre (WODC) of the Netherlands Ministry of Security and Justice. The entire officially recorded criminal history was compiled beginning with age 12 and ending at July 11, 2012. At that date, more than 78% of all respondents had been released for a minimum of 6 months. For the current study, we only selected those offenders with a valid follow-up period of 6 months ($N = 1,484$). Subsequently, 1.1% of the observations were dropped due to missing data, resulting in a final sample that consists of 1,467 defendants.⁴

The Prison Project data are well suited for the proposed analyses because they offer extensive information on an individual's criminal career and on other individual-level life circumstances, attitudes, and offense characteristics. Based on these data, we meet the requirements raised by Shadish (2013), such as using a sample of at least 500 persons, using information of the same "behavior" in our propensity model as the outcome variable (i.e., criminal history characteristics), and using key variables in our propensity model that are part of the selection of offenders into different lengths of imprisonment

(e.g., offense type, sociodemographics, and lifestyle characteristics), and we may therefore “yield accurate effect estimates in non-randomized experiments” by applying a propensity score methodology (p. 129).

Measures

The Outcome Variables

We examine future engagement in criminal behavior during the first 6 months after release using three indicators: reoffending, reconviction, and reincarceration. We use binary recidivism indicators because of the highly skewed distributions. First, the outcome variable reoffending is coded dichotomously, with 0 representing those of whom no charges were recorded after release and 1 representing those of whom at least one charge was recorded. Second, reconviction is coded dichotomously, with 0 representing those of whom no reconviction was recorded and 1 representing those of whom one or more reconvictions were recorded. Finally, reincarceration is coded using a dichotomous variable, with 0 representing those not reincarcerated and 1 representing those reincarcerated.⁵

To reach a sufficiently large sample size from the data available, we restrict our focus to a 6-month period after release to estimate future engagement in criminal behavior. As this is a relatively short period after release, we recognize that it is not possible to draw conclusions about the long-term recidivism patterns of ex-prisoners in the current study. That said, these first months can be identified as essential for postrelease success in the community. Ex-prisoners experience difficulties at various life domains within the first 6 months of release (Beerthuis, Beijersbergen, Noordhuizen, & Weijters, 2015), and important differences in postrelease labor market success are found in this period (Ramakers, Apel, Nieuwebeerta, Dirkzwager, & van Wilsem, 2014). In addition, statistics seem to suggest that the risk of reoffending is highest shortly after release. For example, while the recidivism rate of all adult prisoners released in the Netherlands in 2010 reached 47% after 2 years (Wartna et al., 2016), the recidivism rate of the ex-inmates in our sample already reached 33% after 6 months.

Independent Variable: Actual Length of Imprisonment

In the current study, we wish to assess the effect of length of imprisonment. All types of detentions, including pretrial detention, were counted as imprisonment. Length of imprisonment refers to the actual time between the first

day of pretrial detention and the date of first release from confinement (either suspension of pretrial detention, “time served” in pretrial detention, or end of prison sentence), as administered by the Judicial Institutions Department of the Netherlands. Figure 1 shows the original distribution of prison length. Detention spells are indeed relatively short: The mean number of days imprisoned is 122, and only 20% of the prisoners serve more than 6 months. In the present study, we measure different categories of length of stay based on the actual distribution of the length of imprisonment as well as judicial practice (i.e., the fixed decision moments about extended placement by judges). Discrete categories are preferred over a continuous measure because hearings regarding extended stay occur at regular intervals resulting in specific imprisonment lengths rather than continuous outcomes. A continuous measure does also not address the fact that additional days of incarceration may become less consequential for longer terms (Johnson, 2006). In addition, prior research showed that results using a continuous treatment are comparable with the results yielded by an ordered response model (Ramakers et al., 2014).

At the fixed decision moments in the Dutch sentencing process, judges may lengthen pretrial detention, impose a sentence, or end pretrial detention. Almost half of all pretrial detainees do not receive a prison sentence that exceeds the time already served in pretrial detention, resulting in release from confinement (Linckens & de Looff, 2013). In the Netherlands, sentences for simple cases tried by single sitting judges are typically imposed within 6 weeks (Zuiderwijk, Cramer, Leertouwer, Temürhan, & Busker, 2012). The first prison length category defined is, therefore, 1 to 6 weeks imprisonment. These offenders mostly served their time in pretrial detention only. Furthermore, in the first instance, individuals can be held in pretrial detention for a maximum of 90 days or 3 months, after which a judicial decision is taken (Ausma, 2009). The second prison length category created is, therefore, 6 weeks to 3 months. For cases that are more complicated and settled by a panel of judges, it takes 4 months before a final decision is reached (Zuiderwijk et al., 2012). The third prison length category is, therefore, set at 3 to 4 months. Finally, because only 20% of the sample serves more than 6 months and because this group is considered a long-term prisoner in the Dutch context, a distinction is made between prisoners who served 4 to 6 months and prisoners who served more than 6 months.

Five imprisonment length “doses” are created, and each dose includes a sufficient number of prisoners: 1 to 6 weeks ($N = 284$), 6 weeks to 3 months ($N = 396$), 3 to 4 months ($N = 266$), 4 to 6 months ($N = 224$), and 6 to 15 months ($N = 297$).

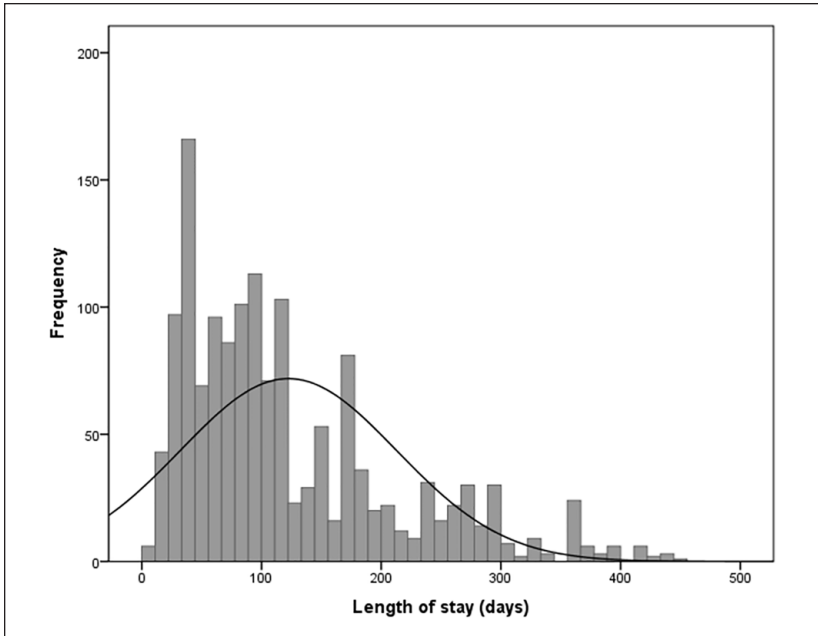


Figure 1. Distribution of length of stay (in days; $N = 1,467$).

Control Variables

Although the list of potential confounding factors is infinite, Nagin et al. (2009) identified those necessary to reach an acceptable base of comparison between groups of individuals who served different types of sanctions or different lengths of confinement. We control for those characteristics in great detail by accounting for the type of the offense committed and the offender's criminal history, age, race, and sex. The latter characteristic was held constant as only males were targeted in our study (see Table 2).

In addition, based on the focal concerns theory (Steffensmeier et al., 1998) and findings from previous studies we identified other characteristics that may influence imprisonment length and/or recidivism. First, we control for problematic alcohol and drug use, and homelessness because these factors have been linked to future offending and length of imprisonment. Dowden and Brown (2002), for instance, showed in their meta-analysis a predictive relationship between substance abuse and recidivism. These factors may also be related to legal actors' perceptions of suspects' dangerousness, a concern that is theorized to be taken into account by legal actors when making decisions in the sentencing process (Steffensmeier et al., 1998). Second, we control for conventional bonds

of suspects because they are typically theorized to produce law-abiding behavior. Following the focal concerns theory, bonds to family members are also related to the third focal concern: the practical constraints and consequences of sentencing decisions. Legal actors are theorized to consider concerns about the disruption of social bonds (Steffensmeier et al., 1998) and the social costs of imprisonment on removal of family members (Bond & Jeffries, 2012). In addition, conventional bonds to work can be linked to court actors' perceptions of dangerousness. Unemployed individuals, for instance, are perceived to be more dangerous and threatening than individuals with a job (Spohn & Holleran, 2000). Third, we included measures of criminal thinking because it has been identified as one of the main predictors of recidivism (Gendreau et al., 1996). Finally, two additional characteristics about the seriousness of the case and pretrial release are included because they are related to imprisonment length.

Propensity Score Methodology and Covariate Balance

General advantages of propensity score methodologies over the commonly used multivariate regression models are (a) that they are more robust concerning model misspecification and (b) that they have the ability to identify those subjects who almost always receive one specific treatment (dose), and who thus should be excluded in an analysis of comparative effectiveness because they cannot be compared with offenders who receive a different treatment (dose) (Nagin et al., 2009; Zanutto, Lu, & Hornik, 2005). In general, propensity score methodologies attempt to approximate the conditions of a randomized experiment, and prior research suggests that several types of propensity score analyses, including stratification, can result in findings similar to a randomized experiment, especially when an extensive set of covariates is included (Shadish, Clark, & Steiner, 2008). As such, it can be assured that the current study—using the detailed information in the Prison Project—accounts for many important confounders. Table 2 contains descriptive statistics on these confounders measured at baseline.

In line with previous research, we assess initial imbalance by performing two-way ANOVAs for each covariate. In these analyses, the ordinal measure of imprisonment length, the balancing score, and their interaction are included as independent variables, and each covariate serves as the dependent variable. Prior to stratification on propensity scores, 13 of the 40 covariates are out of balance (see Table 2). In line with more recent studies, we also assess the magnitude of group differences in covariates by using the root of the *R*-square (.1 = small; .3 = medium; .5 = large; see Connelly, Sackett, & Waters, 2013). Here, a covariate is considered out of balance when this effect size is 0.10 or higher. With this method, only seven covariates are initially imbalanced. Propensity score methodologies are specifically designed to further reduce bias in observational studies (Rosenbaum & Rubin, 1984).

Table 2. Descriptive Statistics and Initial Balance ($N = 1,467$).

	<i>M</i>	<i>SD</i>	<i>p</i> value	<i>R</i>
Sociodemographics				
Dutch	0.63	0.48	0.40	0.05
Moroccan	0.12	0.33	0.19	0.06
Surinamese	0.06	0.24	0.43	0.05
Turkish	0.05	0.22	0.19	0.06
Other ethnicity	0.14	0.34	0.65	0.04
Age (/10)	3.02	1.06	0.71	0.04
Lifestyle				
Problematic use of alcohol	0.16	0.37	0.86	0.03
Problematic use of drugs	0.26	0.44	0.45	0.05
Homelessness	0.07	0.25	0.01 ^a	0.09
Conventional bonds				
Marriage/registered partner	0.06	0.23	0.75	0.04
Children (y/n)	0.28	0.45	0.85	0.03
Not in labor force	0.21	0.40	0.42	0.05
Unemployed	0.41	0.49	0.95	0.02
Part-time employment	0.07	0.26	0.44	0.05
Full-time employment	0.18	0.38	0.69	0.04
Self-employed	0.13	0.34	0.32	0.06
Never active on labor market	0.08	0.27	0.23	0.06
Full-time education	0.04	0.20	0.82	0.03
Criminal thinking				
Tolerance law violation	3.02	0.57	0.11	0.07
Identifying with criminal others	2.64	0.56	0.33	0.06
Criminal history				
Number of recorded crimes	16.33	20.10	0.18	0.06
Number of convictions	8.70	9.77	0.09	0.07
Previous prison spell	0.58	0.49	0.52	0.05
Number of property convictions (past 5 years)	1.33	1.92	0.02 ^a	0.09
Number of violent convictions (past 5 years)	0.59	0.91	0.04 ^a	0.08
Number of other convictions (past 5 years)	1.03	1.36	0.56	0.04
Number of unknown convictions (past 5 years)	0.01	0.10	0.36	0.05
Age of onset (/10)	1.87	0.71	0.03 ^a	0.08

(continued)

Table 2. (continued)

	<i>M</i>	<i>SD</i>	<i>p</i> value	<i>R</i>
Offense type				
Threatening	0.05	0.21	0.54	0.05
Crime against human life	0.06	0.24	0.01 ^a	0.10
Assault	0.11	0.32	0.01 ^a	0.09
Violent theft	0.14	0.35	0.00 ^a	0.15
Theft	0.07	0.25	0.00 ^a	0.20
Aggravated theft	0.25	0.43	0.00 ^a	0.12
Other	0.21	0.40	0.69	0.04
Hard drugs	0.09	0.29	0.01 ^a	0.09
Soft drugs	0.03	0.17	0.59	0.04
Case				
Number of crimes in case of conviction	2.59	1.96	0.00 ^a	0.16
Severity of the offense	6.43	3.24	0.00 ^a	0.29
Pretrial release	0.47	0.50	0.00 ^a	0.36

^aCovariate is initially out of balance.

The propensity score represents the probability of receiving treatment or a certain treatment level, conditional on observed pretreatment covariates. Propensity score methods are most often applied when two treatment groups are studied, and offenders from the experimental group are then matched to offenders from the control group based on their propensity score (1-1 propensity score matching). In the current study, we applied the stratification on propensity score methodology rather than 1-1 propensity score matching because a total of five “treatment” groups were distinguished based on the different “doses” of prison: 1 to 6 weeks; 6 weeks to 3 months; 3 to 4 months; 4 to 6 months; and 6 to 15 months. An ordered logit model is used to generate the propensity score (Loughran et al., 2009; see Lu, Zanutto, Hornik, & Rosenbaum, 2001; Zanutto et al., 2005 for other applications of the generalized propensity score). This model is reported in Appendix A.

The stratification on propensity scores is a technique to group subjects into strata, or equal-sized groups, determined by observed background characteristics. After estimating the propensity scores and the model assumption checks,⁶ subjects were, therefore, grouped into strata within a defined range of propensity scores. The procedure of creating five strata can eliminate 90% of the differences in observed pretreatment covariates between treatment groups (Rosenbaum & Rubin, 1984). The objective is to compare the outcomes for subjects who have a

similar distribution of observed baseline covariates but belong to a different treatment group, that is, groups of prisoners who differ on imprisonment length. Hence, we exclude those prisoners who have incomparable covariate patterns: all subjects who have a propensity score lower than the minimum propensity score among the highest dose prisoners ($N = 130$), or a propensity score higher than the maximum propensity score among the lowest dose prisoners ($N = 4$; see Appendix B). These prisoners were so different that a meaningful comparison of recidivism outcomes between the prisoner groups is not possible. These restrictions result in an estimation sample comprising 1,333 prisoners, representing 91% of the original sample. After these restrictions, results indicate that there is now overlap in predicted propensity scores: Every quintile of predicted scores includes prisoners of the different lengths of observed imprisonment (Appendix C).

After the estimation of the propensity score and prior to assessing the outcome of interest, we checked the covariate balance within each stratum. When using stratification on the propensity score, it is possible to maximize covariate balance by modifying the initial propensity score model. This iterative approach for model selection can, for instance, exist of adding interactions between covariates that are already in the model, using log transformation for continuous predictors of the treatment dose, and modeling relationships using nonlinear terms (also see Rosenbaum & Rubin, 1984; Rubin, 1997). The process of cycling between checking for covariate balance and modifying the propensity score model reduced the initial imbalance to three covariates, i.e., theft, offense severity, and pretrial release. The balance achieved in the current study approximates the balance that we would expect from randomization, because at $\alpha = .05$ we would expect two of the 40 covariates imbalanced. Overall, mean differences in the imbalanced covariates were rather small, with only a few differences between prison lengths being substantial.⁷ We therefore argue that no meaningful differences were observed in any covariate, and are confident that our model handled the selection bias problem by eliminating a substantial number of covariates as potential confounders.

Now, a dose–response curve can be created to compare the recidivism outcomes of prisoners based on their length of imprisonment. In the stratification approach, treatment effects are estimated within each propensity score stratum, as a weighted sum of the differences of sample means across strata. We weight by the proportion of observations within each stratum by taking the proportion in each stratum divided by the total number of subjects. By using these weights, we average across treated subjects (see Loughran et al., 2009). After providing the estimates of the treatment effects of length of imprisonment on various types of recidivism, we perform two sensitivity analyses by using the original sample without restrictions and a more restrictive sample to check the robustness of the average treatment effect.

Table 3. Unadjusted Estimates of Length of Imprisonment on Recidivism ($N = 1,467$).

	Reoffending (%)	Reconviction (%)	Reincarceration (%)
1-6 weeks	37.3	26.8	16.9
6 weeks-3 months	36.1	21.2	11.9
3-4 months	29.7	16.2	7.9
4-6 months	29.9	18.3	11.2
>6 months	27.9	14.1	6.7
Cramer's V	0.08	0.11	0.11
Total	32.58	19.50	10.97

Results

Unadjusted Estimates of the Treatment Effect

For comparative purposes, we first show the unadjusted dose–response relationship between length of stay and recidivism (i.e., the naïve comparison), and then present the results based on the propensity score methodology to adjust for preexisting differences between groups of offenders that were confined for different lengths.

Table 3 shows the results for the naïve comparison of our measures of reoffending, reconviction, and reincarceration. The results show fairly consistently that the recidivism risk is lower after serving longer terms of imprisonment. For instance, 37% of the short-term prisoners (1-6 weeks) reoffend in the 6 months after release compared with 28% of the long-term prisoners (6-15 months). The remaining groups fall between these two percentages. Thus, the likelihood of reoffending decreases for individuals who stayed in prison for a longer period of time. This effect is linear, $\chi^2(1) = 8.60, p < .05$, and statistically significant, $\chi^2(4) = 9.79, p = .05$. For the two other recidivism outcomes, a similar pattern emerges. For instance, 27% of the short-term prisoners are reconvicted compared with 14% of the long-term prisoners. With respect to reconviction, $\chi^2(1) = 14.81, p < .01$, and reincarceration, $\chi^2(1) = 13.41, p < .01$, a linear relationships is observed as well. These effects are statistically significant, and the effect sizes are fairly modest (see Table 3). Our unadjusted dose–response estimates are in line with previous second-generation dose–response studies (Meade et al., 2012; Rydberg & Clark, 2016; Snodgrass et al., 2011, but see Loughran et al., 2009 who found no significant effect).

These unadjusted results should, however, not be interpreted as a test of the dose–response relationship but rather as a description of recidivism patterns.

Adjusted Estimates of the Treatment Effect

Figure 2 presents the adjusted findings (weighted by propensity score strata) for the relationship between imprisonment length and recidivism. The likelihood that offenders with different lengths of confinement reoffend in the 6 months after their release varies between 28% and 38%. Prisoners who served 1 to 6 weeks or 6 weeks to 3 months seem more likely to reoffend (38% and 34%, respectively) than prisoners who served 3 to 4 months (28%). No meaningful differences in the likelihood to reoffend are found, however. The curve for reoffending seems linear, $\chi^2(1) = 4.65$, $p < .05$, but is not statistically significant, $\chi^2(4) = 7.97$, $p = .09$. Standardized residuals further indicate that the reoffending rate for all specific dose groups is never significantly different compared with the average reoffending rate of the other dose groups.

Although less prevalent, similar patterns are observed for reconviction and reincarceration. For instance, while 12% of the short-term prisoners were reincarcerated in the first 6 months after their release, this percentage was approximately 7 for those who were imprisoned for 3 to 4 months and for long-term prisoners. The relationships between time served on one hand and reconviction, $\chi^2(4) = 9.15$, $p = .06$, and reincarceration, $\chi^2(4) = 6.25$, $p = .18$, on the other hand are, however, not significant.

The high resemblance in patterns across the different outcome measures strengthens the finding of a null effect in the current study. The most pronounced difference between the reoffending pattern and the reconviction and reincarceration patterns is visible in the long-term prisoner group as we find relatively low

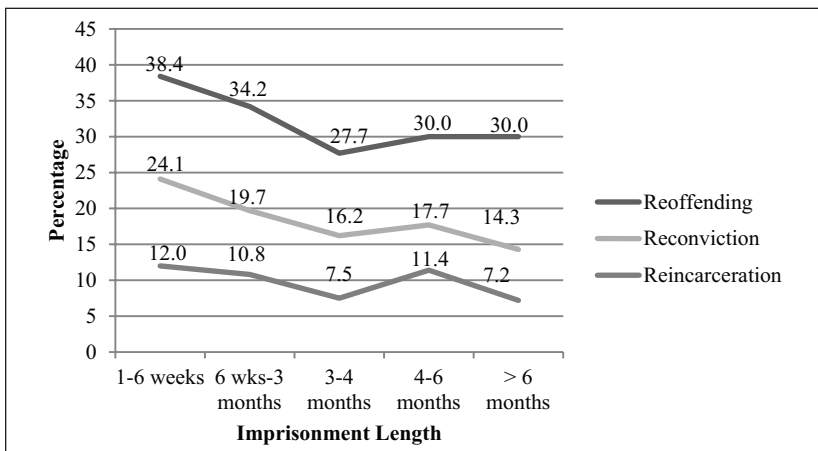


Figure 2. Adjusted estimates of length of imprisonment on recidivism ($N = 1,333$).

percentages for this group based on the latter two recidivism types. This difference may represent a registration effect and may be caused by the fact that new charge(s) may not have led to a conviction yet during the first 6 months after release, resulting in an underestimation of reconviction and reincarceration rates.

In short, our results show no evidence for a relationship between time served and each recidivism outcome, and therefore seem to suggest that, based on the first high-risk months after release, there are no crime-control benefits in terms of recidivism of imprisoning individuals for a longer period.

Stratification of Treatment Effects—Four Groups of Prisoners

Next, the relationship between length of confinement and recidivism is examined for different groups of prisoners. We do so because terms of incarceration can involve different portions of time served, and it remains unclear whether such subtleties may lead to (dis)similarities in dose–response curves among diverse groups of prisoners. These additional calculations were performed on a subset of the full sample because sentencing information was available for 1,295 individuals.

We distinguish four groups of prisoners: (a) detainees who are released prior to conviction and receive extra time upon conviction ($N = 162$), (b) detainees who are released prior to conviction and do not receive extra time upon conviction ($N = 363$), (c) detainees who are confined until their conviction and who then receive extra time resulting in a consecutive prison sentence ($N = 415$), and (d) detainees who are confined until their conviction and who then receive “time served” (no extra time) ($N = 355$). Even though the sample size of these groups of prisoners is somewhat small for specific doses, the results of this stratification provide some preliminary insight into potential heterogeneity in dose–relationship estimates.

The results of these analyses show no clear pattern of a dose–response relationship for the distinct groups of prisoners for each recidivism outcome (see Table 4 for the 12 comparisons). The shape of the dose–response relationships does seem to vary somewhat between the different groups of prisoners. It is difficult however to identify a specific prisoner group in which a clear (either positive or negative) dose–response relationship is found for all recidivism outcomes. A distinction that can be made based on these findings is that recidivism seems to be less prevalent for pretrial releasees than for suspects who have been detained up until the sentence was imposed. For instance, when we look at the short-term prisoners, we see that 19.6% of those who were released prior to conviction and received “time served” reoffended in the first 6 months after their release, while this percentage was 50 for those who were not released prior to conviction and got “time served.”

Table 4. Adjusted Estimates of Length of Imprisonment on Recidivism—Four Groups of Prisoners.

	Pretrial release and extra time ^a (%)	Pretrial release and no extra time ^b (%)	Not pretrial released and extra time ^c (%)	Not pretrial released and no extra time ^d (%)
Reoffending				
1-6 weeks	40.0	19.6	62.5	50.0
6 weeks-3 months	25.0	34.3	39.4	40.2
3-4 months	27.6	25.9	37.0	31.8
4-6 months	20.0	17.5	30.0	38.2
6-12 months	33.3	18.4	26.7	35.9
Reconviction				
1-6 weeks	22.5	13.0	31.3	41.1
6 weeks-3 months	17.3	18.5	21.2	25.6
3-4 months	6.9	10.6	26.1	22.7
4-6 months	15.0	10.0	21.0	20.0
6-12 months	19.0	7.9	14.4	12.8
Reincarceration				
1-6 weeks	10.0	7.6	25.0	25.0
6 weeks-3 months	9.6	7.4	9.1	17.1
3-4 months	0.0	2.4	15.2	13.6
4-6 months	10.0	5.0	12.0	14.5
6-12 months	4.8	5.3	7.5	2.6

^a 1 to 6 weeks ($N = 40$); 6 weeks to 3 months ($N = 52$); 3 to 4 months ($N = 29$); 4 to 6 months ($N = 20$); >6 months ($N = 21$).

^b 1 to 6 weeks ($N = 92$); 6 weeks to 3 months ($N = 108$); 3 to 4 months ($N = 85$); 4 to 6 months ($N = 40$); >6 months ($N = 38$).

^c 1 to 6 weeks ($N = 16$); 6 weeks to 3 months ($N = 66$); 3 to 4 months ($N = 46$); 4 to 6 months ($N = 100$); >6 months ($N = 187$).

^d 1 to 6 weeks ($N = 56$); 6 weeks to 3 months ($N = 117$); 3 to 4 months ($N = 88$); 4 to 6 months ($N = 55$); >6 months ($N = 39$).

The results further show that the relatively high level of recidivism among short-term prisoners observed in Figure 2, is mainly due to the group of suspects who were in pretrial detention up until the sentence was imposed. It is possible that this group consists of high-rate offenders who are typically involved in (simple) property crimes resulting in high scores on our outcome measures. Additional analyses (not shown) confirm this argument as the majority of the short-term prisoner group who score highest on reoffending consists of offenders convicted for a property crime. Although it is apparent that there may be some interesting

mechanism at work here, we are restricted in our ability to interpret it fully as the number of observations in this group is too small to draw sound inferences.

Overall, these results seem to suggest that heterogeneity in dose–response estimates might be present because the shape of the dose–response relationship seems to differ between groups of prisoners. This exploration shows that paying more detailed attention to these groups can be valuable for the interpretation of findings. However, when keeping in mind the main goal of the present study it may perhaps be more important to note that, again—even when the dose–response relationship was stratified—no clear effect of length of imprisonment on recidivism can be identified.

Sensitivity Analyses

To gain more insight into the robustness of our results, two sensitivity analyses for the total group of prisoners were performed (see Appendix D). First, we performed the previously mentioned analyses on the original sample. In this analysis, we also included those subjects who were excluded in the initial analyses because an appropriate match was not available: detainees with a propensity score lower than the minimum propensity score of the longest imprisonment length group, or with a propensity score higher than the maximum propensity score among the shortest imprisonment length group ($N = 1,467$; see Appendix B for the distribution of propensity scores). Second, we performed the analyses using a more restrictive sample to maximize comparability on confounders between prisoners serving different lengths. In this case, we excluded all individuals with a propensity score higher than the upper whisker (in Appendix B) of the shortest imprisonment length group and those with a propensity score lower than the lower whisker of the longest imprisonment length group ($N = 939$). These sensitivity analyses all show similar results for each recidivism outcome: The relationship between length of imprisonment and recidivism outcomes is not significant. Therefore, these additional analyses seem to strengthen the reliability of the previously described null effect.

Discussion and Conclusion

This research has examined the relationship between length of imprisonment and recidivism. The strengths of our study lie in the reliance on both administrative and survey data, the very detailed information on offenders' life circumstances, the precise measurement of length of imprisonment, and the methods used to minimize selection effects. Our findings suggest that length of imprisonment does not have a significant effect on recidivism in the first 6 months after release,

and that this conclusion holds across various measures and types of recorded recidivism, that is, reoffending, reconviction, and reincarceration. Our sensitivity analyses further confirm this pattern, which increases our confidence in the findings. The findings of the present study are largely in line with those of previous studies. The few existing new-generation studies examining the effects of length of imprisonment have reported little evidence of a relationship between length of stay and recidivism as well (Loughran et al., 2009; Meade et al., 2012; Rydberg & Clark, 2016; Snodgrass et al., 2011). Together, these new-generation studies seem to suggest that length of imprisonment does not influence recidivism. In addition, the current study suggests the absence of such a relationship in a context with relatively mild penal climate and a highly developed welfare regime. Based on our findings, we can neither confirm theoretical arguments that increased lengths of imprisonment are criminogenic nor can we confirm that longer stays deter individuals from recidivating.

After providing a single estimate of the average effect of length of imprisonment on recidivism, the dose–response relationship was examined for four groups of prisoners based on their release status prior to conviction and the sentencing decision (either “time served” or additional time). These preliminary analyses show no clear dose–response pattern as well. However, the dose–response relationship seems to differ between groups of prisoners. For instance, the shape of the dose–response curve differs between detainees who were and those who were not released prior to conviction, suggesting heterogeneity in dose–response estimates.⁸ The results regarding the four groups of prisoners should be regarded as preliminary as the sample sizes of some of the groups were small. An important avenue for future research is to further explore the issue that effects of imprisonment can vary between different groups of offenders to increase our understanding of differences in responding to imprisonment; for instance by individuals’ level of motivation or propensity to commit crime, or by characteristics of the institution, such as the level of investments by institutions on reintegration (Nagin et al., 2009; Wright, Caspi, Moffitt, & Paternoster, 2004).

Before discussing the implications of our study, some limitations need to be addressed. First, it should go without saying that an experimental design would be the ideal design to assess the causal effects of length of imprisonment on recidivism. Only in a randomized experiment, would selection bias be completely ruled out, and therefore the effect of imprisonment length could be isolated. Although a propensity score methodology was used to minimize selection bias, the present study solely relied on observational data, and as a consequence remains vulnerable to sources of unobserved bias. This is a common limitation in (the new generation of) studies assessing the dose–response

relationship between length of imprisonment and recidivism. In the current study, we specifically aimed to reduce this problem by combining administrative data with rich survey data, thereby accounting for many potential confounders. Moreover, the observational data used in the current study allowed us to investigate short-term recidivism patterns. Continued follow-up is needed to draw conclusions about long-term effects.

A second concern refers to the mechanism underlying the effect of imprisonment length on recidivism. Although stratification on propensity scores was applied to increase comparability on pre-prison covariates between prisoners serving different lengths, post-prison differences may occur. For instance, longer prison terms not only imply that an offender has served more time but also that an offender is older when released. Long-term imprisonment, in particular, will make it difficult to disentangle effects of specific deterrence and incapacitation as offenders may have “aged out” of crime during their detention (Meade et al., 2012). In the current study, we only included short prison terms, that is, up to 15 months with an average of 4.1 months, resulting in relatively small differences in imprisonment length between the selected prisoner groups. Therefore, the issue of maturation may be less of a concern for the results in the current study. Other causal structures, such as learning, social control, and labeling processes, could be significant contributors to the null effect observed. For future research, it is highly relevant to test these underlying mechanisms.

A third concern is that we solely relied on official measures of recidivism. Although official measures by definition underestimate actual criminal behavior, they have been considered valid indicators of offending behavior (Farrall, 2005). Self-reported offenses that more closely align with actual criminal behavior usually contain more offenses of a less serious nature than offenses derived from official data (Blokland, 2005). The measures used in the current study should therefore be perceived as more serious types of recidivism than measures based on self-reported criminal behavior. Although self-report and official offending records are not perfect substitutes, previous research shows that dose–response estimates for both outcomes are substantially similar (see Loughran et al., 2009). An important direction for future research is to further study recidivism of ex-prisoners by combining administrative data with self-report data on recidivism.

A final concern refers to the generalizability of our findings. In the current study, all subjects experienced relatively short prison terms. An advantage of this approach is that groups that differ in imprisonment length will be more similar based on preprison (un)observables than when prisoners with very long terms would have been captured as well. This is also evidenced by the fact that the initial imbalance in the current study was not

that high to begin with. In similar vein, Loughran et al. (2009) showed a high level of heterogeneity in recidivism for offenders who served more than 15 months of imprisonment. The focus on effects of imprisonment length up to 15 months also did not dramatically decrease the generalizability of the results to the wider population of prisoners in the Dutch context because for the vast majority of the individuals who entered pretrial detention the prison term does not exceed 1 year (Linckens & de Loeff, 2013). This restriction perhaps does reduce the generalizability to other sentencing contexts, like the United States, where imprisonment sentences are typically much longer and prison conditions are harsher. Findings based on Dutch data would, therefore, be more reflective of practices in most of Western Europe and may translate to the U.S. jail population but less so to U.S. federal and state prisons. Another notable limit to the generalizability of the findings is that they pertain only to compliant men aged 18 to 65 who were born in the Netherlands and who entered detention in a pretrial detention facility. It would, for instance, be valuable to examine effects of length of imprisonment on women, who are a growing proportion of the prison population, and on juveniles and foreign-born prisoners. From a policy and scientific perspective, it would also be interesting to determine whether the overall null effect would apply in a population who enter detention in a prison facility rather than a pretrial detention facility. That said, the consistency in findings in various second-generation dose–response studies focusing on diverse offender populations and contexts strengthens our conclusions.

Notwithstanding the limitations noted above, our study adds to a growing number of second-generation dose–response studies that use data on different populations and correctional contexts but find similar results. In general, it must be noted that relatively very few studies in the criminological literature focus on the effects of length of imprisonment and recidivism, and most of this work has been conducted in the United States. Although short terms of imprisonment are common practice in Western European and American contexts (i.e., jail terms), previous studies mostly include only relatively long terms of imprisonment. The current study thereby fills an important gap in the empirical literature, and our findings indicate that perhaps a general trend is present: When adequately controlling for preexisting differences, there is little evidence for a dose–response relationship between imprisonment length and recidivism.

In sum, the findings of the current study may raise questions about the effectiveness of specific deterrence. For legal actors who take specific deterrence into account when deciding which sanction they will impose, it

is important to know that longer terms of imprisonment do not necessarily reduce recidivism in the first months after release. From a policy perspective, the results of the current study suggest that—within the context of imprisonment length up to 15 months—investing in longer prison sentences is not likely to yield higher crime-control benefits. Public resources might therefore be better spent to achieve such benefits. This is especially important, given the high costs associated with prison sentences in the Netherlands (Custodial Institutions Agency, 2013: “Masterplan Prison Service 2013-2018”) and in other societies such as the United States (Henrichson & Delaney, 2012). Alternative ways of punishment, such as electronic monitoring and community service, could be more promising in terms of spending and of lowering recidivism (see, for instance, Robert, Maes, Blokland, & Wermink, 2016; Wermink, Blokland, Nieuwbeerta, Nagin, & Tollenaar, 2010). Prior research has shown more robust evidence for a deterrent effect of the certainty of punishment than for a deterrent effect of the severity of punishment (see Nagin & Pogarsky, 2001). These research findings make it necessary to reexamine the role of imprisonment in contemporary justice policy.

Appendix A

Ordinal Logit Model of Imprisonment Length ($N = 1,467$).

	Coefficient	SE	<i>p</i> value
Sociodemographics			
Dutch	-0.26	0.22	0.24
Moroccan	-0.24	0.25	0.33
Surinamese	-0.08	0.29	0.77
Turkish	Ref.	Ref.	Ref.
Other ethnicity	-0.07	0.24	0.77
Age (/10)	-0.09	0.09	0.32
Lifestyle			
Problematic use of alcohol	-0.02	0.14	0.88
Problematic use of drugs	0.12	0.12	0.33
Homelessness	-0.14	0.73	0.85
Conventional bonds			
Marriage/registered partner	0.17	0.23	0.46
Children (y/n)	0.00	0.13	0.99
Not in labor force	-0.15	0.21	0.46
Unemployed	0.06	0.19	0.76
Part-time employment	Ref.	Ref.	Ref.

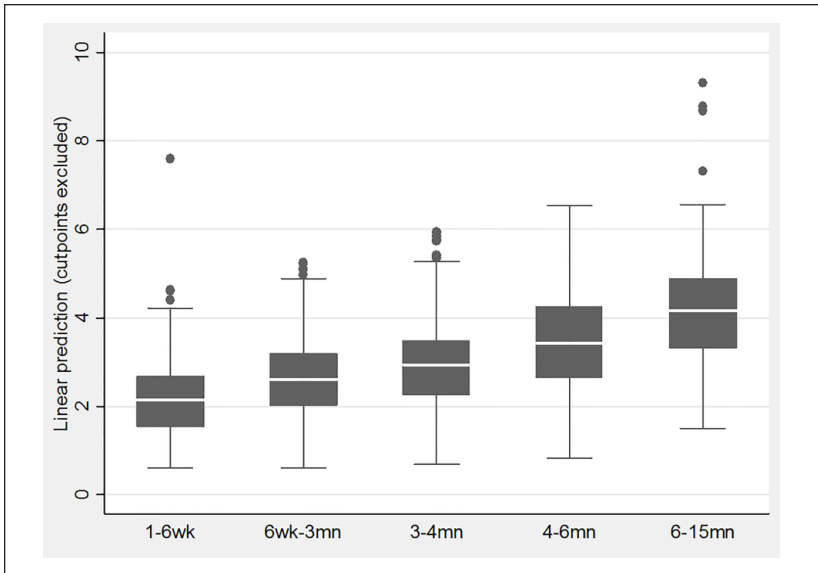
(continued)

Appendix A (continued)

	Coefficient	SE	p value
Full-time employment	-0.09	0.21	0.67
Self-employed	0.24	0.23	0.30
Never active on labor market	-0.09	0.19	0.63
Full-time education	0.19	0.24	0.44
Criminal thinking			
Tolerance law violation	-0.33	0.31	0.29
Identifying with criminal others	-0.20	0.37	0.59
Criminal history			
Number of recorded crimes	0.01	0.01	0.18
Number of convictions	-0.02	0.02	0.39
Previous prison spell	0.25	0.13	0.06
Number of property convictions (past 5 years)	-0.05	0.04	0.17
Number of violent convictions (past 5 years)	-0.04	0.06	0.47
Number of other convictions (past 5 years)	0.06	0.04	0.14
Number of unknown convictions (past 5 years)	0.21	0.50	0.67
Age of onset (/10)	0.62	0.26	0.02
Offense type			
Threatening	1.76	0.96	0.07
Crime against human life	Ref.	Ref.	Ref.
Assault	1.21	0.95	0.20
Violent theft	1.57	0.98	0.11
Theft	-0.20	0.98	0.84
Aggravated theft	0.86	0.95	0.37
Other	1.51	0.95	0.11
Hard drugs	1.70	0.96	0.08
Soft drugs	2.10	0.98	0.03
Case			
Number of crimes in case of conviction	0.21	0.03	0.00
Severity of the offense	.21	0.03	0.00
Pretrial release	-.88	0.24	0.00
Pseudo R ²	.11		

Note. To optimize balance, a nonlinear term of age onset was added to the model, and five interaction terms were added: simple theft × pretrial release; crime against human life × severity of the offense; age × homelessness; tolerance law violation × identifying with criminal others; severity of the offense × pretrial release.

Appendix B



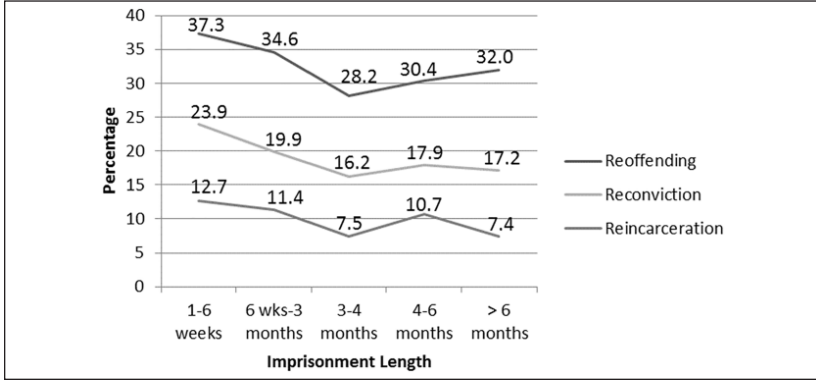
Propensity score distribution, by imprisonment length (N = 1,467).

Appendix C

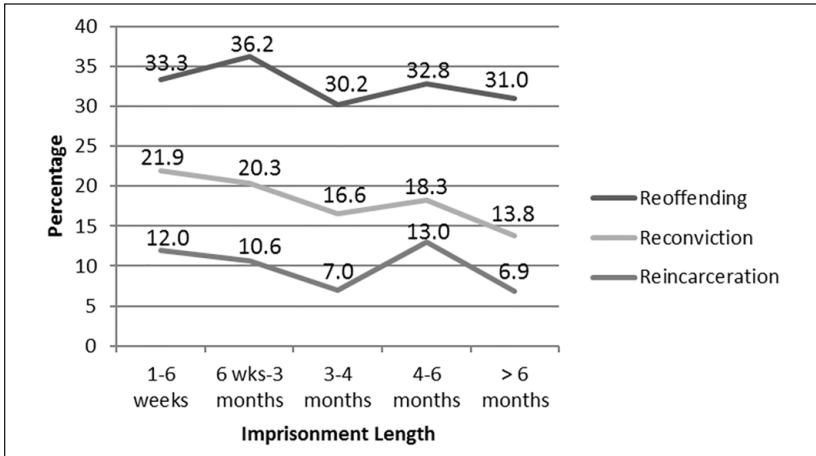
The Distribution of Imprisonment Length Group Over Propensity Quintiles (N = 1,333).

Predicted scores	Observed imprisonment length				
	1-6 weeks	6 weeks-3 months	3-4 months	4-6 months	6-15 months
First quintile	54	54	29	17	9
Second quintile	82	101	52	34	25
Third quintile	54	89	76	40	35
Fourth quintile	22	76	63	60	72
Fifth quintile	4	31	33	69	152
N of respondents	216	351	253	220	293

Appendix D



Adjusted estimates of length of imprisonment on recidivism.
 Panel A: In less restricted sample (N = 1,467).



Panel B: In more restricted sample (N = 939).

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Notes

1. In the current study, length of stay includes time served in pretrial detention. We argue that the specific deterrence argument is also valid for time served in pretrial detention, because it can, similar to prison sentences, be defined as an unpleasant experience which is characterized by loss of liberty and autonomy. Moreover, especially in pretrial detention, activities such as education and work are hardly offered, and people spend most of their day in their own cell (Dirkzwager, Nieuwbeerta, & Fiselier, 2009). Furthermore, approximately 90% of all criminal suspects are found guilty by a judge in the Netherlands (Statistics Netherlands, 2013), meaning that the time in pretrial detention becomes a punishment for most inmates.
2. Two studies of the first generation, above 30 years old, have been conducted using a variable-by-variable matching technique rather than regression-based techniques. Jaman, Dickover, and Bennett (1972) examined released imprisoned males in California convicted of first-degree robbery or second-degree burglary, and found no significant differences at 6 and 12 months after release. Kraus (1981) examined juvenile boys in New South Wales who were put in a special school for truancy reasons and were released in 1975-1977, and found that differences in recidivism were not significant after different lengths of stay. Despite the attempt of these studies to methodologically address the problem of selection more rigorously than regression-based studies, these studies have limitations as well. With every match on a certain characteristic, there will be subjects who are not matched. Variable-by-variable matching on a relatively broad set of confounders thus increases internal validity but very quickly reduces the generalizability of the study. Also, it was not possible to thoroughly control for some confounding variables, such as criminal history or offense type.
3. Sound inference for two groups of offenders could not be drawn, because (a) the number of observations in the 0-3 months prison length group was too small to provide adequate statistical power and (b) high levels of heterogeneity in the rate of rearrest were observed among offenders with prison lengths beyond 15 months, mainly because the outcome may be confounded with length of stay as the exposure time is limited for these offenders.
4. This sample was generally representative of all participants in the Prison Project in terms of age, age of onset, partner status, being employed, previous convictions, and previous prison spells. Participants in the Prison Project and men in our analysis sample differ in committing a violent offense (0.46 vs. 0.40) and duration of actual time served (6.6 vs. 4.4 months). This average time served among the prisoners

in our analysis sample is comparable with the national average. It must further be noted that although we have quite extensive data available to describe the representativeness of our sample, it is a matter of speculation whether participants and nonparticipants differ on other unobserved characteristics.

5. Further adjusting for exposure time as individuals may have been incarcerated during the follow-up period is not necessary for those who score 1 at the outcome variables because all outcome variables are dichotomous. Results of additional analyses (not shown) suggest that there is substantial overlap between those who are incarcerated during the follow-up period and those who reoffend within the 6-month follow-up period; 75% of those who are incarcerated during the follow-up (and who thereby have less exposure time) score 1 at reoffending. This high level of overlap can be expected considering that the registration of a new charge can lead to confinement.
6. The proportional odds assumption of consistency of effects across categories—one of the assumptions underlying ordered logistic regression—is met in this study, $\chi^2(120) = 138.3, p = .21$, meaning that a single set of coefficients can be estimated for each of the covariates.
7. First, the imbalance with regard to offense severity (range = 0-30 years) was mainly caused by a mean difference between men who served 4 to 6 months and men who served 6 weeks to 3 months. The absolute mean difference in offense severity between those groups was 7 months. Second, the imbalance in theft was mainly caused by a relatively high mean value for long-term prisoners (6-15 months). Moreover, only few offenders committed a theft offense ($N = 59$). Third, the imbalance in pretrial release was mainly caused by a relatively high mean value among short-term prisoners (1-6 weeks). Not surprisingly, these men were more often released pretrial compared with men who were longer imprisoned.
8. Also, recidivism rates seem lower for suspects released prior to their trial than for suspects who stayed in custody upon conviction. These released offenders may be more reluctant to commit crimes because they are still awaiting trial. The perceived costs of such criminal involvement may be higher in that period because the certainty of punishment could be (perceived to be) higher. An alternative explanation may be that these offenders, who are released pretrial and who are eventually sentenced to additional prison time may be detained in the follow-up period which reduces their exposure time. However, additional analyses (not shown) revealed that the latter explanation seems not plausible, because these persons are not more likely to be detained in the follow-up period compared with other offenders (17% vs. 21%, respectively). Another alternative explanation may be a selection effect because detainees who are confined until their conviction may be offenders with higher (recidivism) risks.

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