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## Sentence Length and Recidivism: A Review of the Research

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### *Abstract*

In response to increasing concerns about jail and prison overcrowding, many officials and legislatures across the U.S. have undertaken different efforts aimed at reducing the prison population, such as reduced sentence lengths and early release of prisoners. Thus, there is currently a high degree of public interest regarding how these changes in policy might affect recidivism rates of released offenders. When considering the research on the relationship between incarceration and recidivism, many studies compare custodial with non-custodial sentences on recidivism, while fewer examine the impact of varying incarceration lengths on recidivism. This article provides a review of the research on the latter. While some findings suggest that longer sentences may provide additional deterrent benefit in the aggregate, this effect is not always consistent or strong. In addition, many of the studies had null effects, while none of the studies suggested a strong aggregate-level criminogenic effect. Overall, the literature on the impact of incarceration on recidivism is admittedly limited by important methodological considerations, resulting in inconsistency of findings across studies. In addition, it appears that deterrent effects of incarceration may vary slightly for different offenders. Ultimately, the effect of incarceration length on recidivism appears too heterogenous to be able to draw universal conclusions. We argue that a deepened understanding of the causal mechanisms at play is needed to reliably and accurately inform policy.

*Keywords:* incarceration length, incarceration, prison, recidivism, sentencing policy, deterrence, custodial sentence

## Introduction

There is currently a high degree of public interest in research regarding the effect of length of incarceration on the recidivism rates of released offenders. The interest is particularly strong in Los Angeles, where the recently-elected District Attorney has adopted charging policies to sharply reduce sentence lengths by omitting allegations that would increase sentences beyond the base sentence for the crime (Gascón, 2020). This policy is justified with a statement regarding empirical research in the field: “While initial incarceration prevents crime through incapacitation, studies show that each additional sentence year causes a 4 to 7 percent increase in recidivism that eventually outweighs the incapacitation benefit” (Gascón, 2020, p. 1). Despite the plural “studies” alluded to, only one unpublished manuscript is actually cited in support (Mueller-Smith, 2015). In addition, the study methodology has some nuances that render it not fully comparable to past literature on the topic. Thus, it is concerning that such a drastic policy change is based on only one study selected from the full body of research on the topic.

Despite the fact that Mueller-Smith’s (2015) findings have not been published nor replicated in academic research, the claim that longer periods of incarceration disproportionately increase risk for recidivism has nonetheless attracted prominent support from people within the academic community. A newspaper opinion piece co-authored by the dean of U.C. Berkeley Law School asserts that sentence enhancement “approaches have exacerbated recidivism, creating more victims of crime” (Chemerinsky & Krinsky, 2021, para. 5). A hyperlink in the online version of the article links again to Mueller-Smith (2015) as authority for the assertion. A “friend of the court brief” filed in litigation over the policies, by one of the same co-authors, makes a similar assertion also citing the 2015 article (Romano & Chemerinsky, 2021).

Contrary to Gascón's (2020) stated desire to inform criminal justice policy based on empirical research, it is unclear whether the cited findings (Mueller-Smith, 2015) hold when considering the wider body of research on the topic. Specifically, there has not been much consistency supporting these claims throughout the entire breadth of research (Nagin, Cullen, & Jonson, 2009; Rhodes, Gaes, Kling, & Cutler, 2018; Tonry, 2008). Rather, the blanket assertion that longer sentences result in greater likelihood of reoffending relative to shorter sentences contrasts sharply with findings from the last thorough review of the literature on the subject (Nagin et al., 2009). When considering the consistency and strength of findings across numerous studies, Nagin and colleagues (2009, p. 183) found "little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate." This article is 12 years old, though. A review of the subsequent literature is in order to determine if convincing evidence of a strong relationship, or any relationship at all, has been discovered since.

### **I. Difficulties in Measurement.**

Nagin et al. (2009) appear to have been writing for a larger audience than just researchers in their own field, as they explained the problems of measuring effects in criminal justice that need explanation for that broader audience. This article will follow a similar path, with frequent references to Nagin et al. (2009) for the "long form" explanation.<sup>1</sup>

In the physical sciences, it is generally possible to test a hypothesis with a tightly controlled experiment that eliminates all variables except those of interest. As we move to living things, individual variation in the test subjects becomes inevitable. As we move up the evolutionary ladder, ethical constraints become more restrictive. For studies actively involving

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<sup>1</sup> Because the Nagin et al. (2009) article is long, this article will occasionally give "point pages" even where social science style manuals do not require them.

humans, informed consent of the participants is required. Past studies with unwilling or uninformed subjects, such as the infamous Tuskegee Experiment (Cobb, 1973), are regarded with horror today (Centers for Disease Control and Prevention, 2000).

In medicine and social sciences, the randomized controlled trial (RCT) is regarded as the “gold standard” for assessing the effectiveness of a policy, program, or intervention. In such a study, researchers assign participants at random to either a treatment group or a control group. The treatment group receives the intervention or “treatment” to be evaluated. The control group receives a comparison intervention – typically either through the form of a “business-as-usual” treatment or a placebo treatment, i.e., a dummy “treatment” that does nothing. In an ideal scenario, participants do not know which group they are in until the end of the trial; this concept is known as “blinding.” The element of randomization is a key strength of this design that makes it superior to most other designs (Berk, 2005; Sobel, 2006). In contrast, a retrospective observational study design would simply compare those receiving a treatment with those who did not based on retrospective data from participant records (Shadish, Cook, & Campbell, 2002). In the latter, assignment is not random nor is it controlled by the researcher, which introduces a higher probability of bias known as “selection bias” which can compromise the generalizability of results (Sampson, 2010; Wermink, Blokland, Nieuwbeerta, Nagin, & Tollenaar, 2010).

These seemingly minute differences in study procedures have important ramifications for policy, because studies on similar topics with different methodological approaches often find contrasting results. This is also typical of the research assessing the relationship between imprisonment and recidivism, with different methodological procedures often resulting in mixed findings (Mears, Cochran, Bales, & Bhati, 2016; Nagin et al., 2009; Roodman, 2017; Tonry,

2008) as well as the research on deterrence theory more broadly (Apel & Nagin, 2011; Braga, Weisburd, & Turchan, 2018; Nagin, Solow, & Lum, 2015; Tyler, 1990). There are several challenges in studying the relationship between imprisonment and recidivism. First, in circumstances such as incarceration, it is not ethical to consider RCTs (Weisburd, 2010). Thus, any approach to studying the relationship needs to be quasi-experimental, which has inherent challenges in being able to ascertain causality. In the absence of randomization, it becomes more challenging to sufficiently control for the impact of outside factors, making it more difficult to ascertain a clear causal relationship between any factor and its direct impact on recidivism (Mears et al., 2016; Tonry, 2008; Sampson, 2010). Further, there is considerable variation in the approaches used to methodologically control for the impact of important characteristics (e.g. offending history), which can result in mixed findings across approaches (Nagin et al., 2009; Roodman, 2017). Finally, there are many different ways of measuring the outcome of interest (in this case, recidivism) that make it more complicated to compare results across studies (Andersen & Skardhamar, 2017; Maltz, 1984). We explain this in more detail below followed by examples from research.

The differences between an observational study and a randomized study were dramatically demonstrated to the general public during the Covid-19 pandemic (Wilson, 2020). Early observational studies raised hopes that a widely available and inexpensive drug, hydroxychloroquine, might be a promising treatment (Chen et al., 2020; Gautret et al., 2020), though multiple RCTs have since demonstrated otherwise (Kashour et al., 2020; Singh, Ryan, Kredon, Chaplin, & Fletcher, 2021). Nonetheless, preliminary findings were touted in an unpublished manuscript (Todaro & Rigano, 2021) and circulated to millions on social media

(Gould & Norris, 2021), eventually leading to a rapid increase in off-label use of the drug for Covid-19 patients (Gyselinck, Janssens, Verhamme, & Vos 2021).<sup>2</sup> However, because the earlier studies failed to consider the impact of important patient-level differences (Axfors et al., 2021; Yang et al., 2020), suspicions increased regarding the true efficacy of the drug as well as the potential unintended effects (Gyselinck et al., 2021; Singh et al., 2021). This example is one of many where policy has outpaced empirical research, only to be met with lackluster results (Saltelli & Giampietro, 2017).

To summarize, a key benefit of the RCT is that it greatly enhances the ability to infer a causal relationship between two factors, rather than just being able to detect a mere association (Farrington, Lösel, Braga, Mazerolle, Raine, Sherman, & Weisburd, 2020). To argue causality between two things, such as incarceration and recidivism, one must eliminate every alternative explanation for that relationship. Randomization limits the possibility that alternative explanations exist, because the design ensures that unmeasured factors will be randomly distributed and therefore not affect results. In nonrandomized studies, the treatment group and the control group may be different, and efforts to control for those differences are hampered by the reality that the differing factors may be numerous, unmeasurable, or even unknown. With truly random assignment and a large sample, differences among individuals aren't as critical because any differences will average out and therefore not impact results (Berk, 2005; Shadish et al., 2002; Sobel, 2006; Wilson, 2020).

The same is true in the study of crime and punishment (Nagin et al., 2009, p. 135; Sampson, 2010). A well-conducted randomized experiment with a large enough sample provides

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<sup>2</sup> For a detailed timeline of events regarding the hydroxychloroquine controversy see Gould & Norris (2021).

an assurance that the experimental (or treatment) group and the control group differ in no way other than the treatment, an assurance that observational studies cannot provide. Unfortunately, RCTs are not always feasible or ethical (Sampson, 2010; Shadish et al., 2002; Weisburd, 2010). For one, there would be an obvious ethical problem in assigning people to an arbitrary sentence length at random, particularly for serious crimes deserving severe punishment. How many people would give informed consent to being sentenced to five or ten years in prison, at random? Even if consenting defendants could be found, the individuals consenting would likely be so atypical that it would introduce additional bias to the design (Sampson, 2010; Shadish et al., 2002). Further, one must consider how the public, and especially the victims, would react. With this in consideration, it is not surprising that Nagin et al. (2009, pp. 144-145) found only four actual experiments doing so, mostly using old data, and only one of which involved serious adult offenders. They found the evidence from this group of studies weak due to the data and sampling constraints, coupled with the fact that many of the findings were not statistically significant (Nagin et al., 2009).

One step down from the true randomized experiment or RCT is a “quasi-experiment,” which is similar to an RCT but without random assignment. Compared with an RCT, the risk for selection bias sans random assignment is higher and thus, it is more likely that treatment and control groups will look different from each other (Shadish et al., 2002). However, researchers can take steps to mitigate this risk by strategically assigning subjects to groups based on certain criteria with the ultimate goal of minimizing observable differences between groups (Sampson, 2010; Shadish et al., 2002).

One approach to strategic assignment is the “matched-pairs” design, where similar participants are paired based on relevant factors, and one person from each pair is randomly assigned to each group. For example, some studies of imprisonment have used a variation of this process in randomly assigning cases to judges (Garner, Maxwell, & Lee, 2021; Roach & Schanzenbach, 2015). A common way this is done in imprisonment research is through use of a “propensity score” or “instrumental variable” that acts as a proxy to control for important factors such as offense severity, prior offenses, age, and the like (Mears et al., 2016; Rosenbaum & Rubin, 1983). While matched-pairs designs are not completely random, they tend to generate treatment and control groups that are more similar than would often be possible in a quasi-experimental framework (Rosenbaum & Rubin, 1983). However, it is important to note that this does not completely eliminate selection bias because the sample is still quite selective – i.e., those individuals involved in the criminal justice system are not totally random (Sampson, 2010).

A “natural experiment” is one type of quasi-experiment where researchers have no control over assignment at all – rather, they take advantage of some outside event that occurs (such as a natural disaster, policy change, economic change, etc.) that acts as a natural “treatment,” producing some type of measurable impact(s). One example of a natural experiment is the impact of 2009 revisions to New York’s “Rockefeller Drug Laws” on recidivism rates for drug offenders (Parsons et al., 2015). A more relevant example is a study by Drago and colleagues (2009) that examined recidivism patterns among offenders released from prison as a result of a bill passed by the Italian Parliament. We discuss this study in more detail in section II.

Quasi-experiments are usually superior to observational studies, and Nagin et al. (2009, p. 184) encourage researchers to seek such opportunities. However, in terms of assessing

causality, a quasi-experiment can only minimize differences that are *observable*, while unobservable differences likely remain unknown (Shadish et al., 2002). In contrast, a well-executed RCT will ensure that both *unobservable and observable* characteristics are randomly balanced out between groups. In either case, close scrutiny is required to guard against hidden biases when evaluating the effectiveness of any potential policy or program (Cook, Shadish, & Wong, 2008; Shadish et al., 2002). Importantly, recall how difficult causality is to prove. It is easier to demonstrate that two things are *correlated*; it is much more difficult to demonstrate that one *caused* the other (Sampson, 2010). When thinking about evidence-based policy, we have to consider the *quality* of the research evaluations being used to determine effectiveness, how effectiveness is being *defined* and measured, and how *consistent* the results are across a variety of methodologies, geographies, and contexts (Sampson, 2010).

Even as evidence-based policy has gained some acceptance in the field, policies such as Gascón's (2020) are too often based on selectively cited research rather than the full breadth of research as a whole. In regard to the impact of imprisonment on recidivism, rather than having an empirical straightforward explanation, it is more likely that people respond to policy changes in a variety of ways that may or may not be directly or indirectly related to recidivism risk (Mears et al., 2016; Miles & Ludwig, 2007; Tonry, 2008). The U.S. criminal justice system has a lengthy history of rapidly implementing sweeping policy change without comprehensively considering the potential effects, often resulting in damaging consequences that are difficult if not impossible to reverse.

While reliance on empirical research is critical for effective policymaking, the reliance on research, especially single studies, should not be oversold (Lempert, 2008). It is just as critical to

consider the *quality* of such findings and the *consistency* of results when replicated (Cook et al., 2008). When empirical results are not critically examined and cautiously interpreted, the policy relevance of research findings declines rapidly (Cook et al., 2008; Lempert, 2008). Further, no matter how sound a study methodology is, no one study on its own provides a basis for policy transformation, though a series of consistent findings across studies with sound methodologies may do so (Cook et al., 2008; Lempert, 2008).

## **II. Effects of Punishment on Crime.**

Punishment is thought to affect crime in various ways, which are often referred to as “ideologies” or “purposes” of punishment.

*Deterrence.* One key paradigm that underlies much of our penal policy and crime control efforts is deterrence (Andenaes, 1968; Bentham, 1823). This can be in the form of deterring would-be offenders from committing crime (general deterrence), or it can be in the form of deterring current offenders from committing future crime (specific deterrence). Per general deterrence theory, knowledge that a crime is generally punished by society may convince a would-be offender not to commit crime (Durham, 1994; Spohn & Holleran, 2002). Per specific deterrence, the painful experience of being punished would make a person refrain from crime in the future to avoid repeating the experience (Paternoster & Piqueuro, 1995; Spohn & Holleran, 2002).

*Incapacitation.* Incapacitation, another key paradigm in our criminal justice system, refers to the use of punishment as a means to physically prevent a person from committing crimes (or at least limit his targets to those inside the prison), typically via long-term confinement or death (Blumstein et al., 1978; Spohn & Holleran, 2002).

*Rehabilitation.* Rehabilitative efforts refer to any experiences (e.g., drug treatment programs, vocational training) that transform the offender into a better person who will refrain from crime for reasons other than incapacitation or fear of punishment (Ellison, Horan, & Fox, 2017; King, 2018). The inverse of rehabilitation is a criminogenic effect, or the notion that prison is a school for crime and people come out more crime-prone than when they went in (Clemson, 1958; Kirk, 2009; Sampson & Laub, 1995). Many of these theories argue that interactions and socialization within prisons can lead to the learning of criminal behavior from fellow inmates (Akers 1998; Sutherland 1947). However, it is believed that this likelihood is more relevant to less serious offenders who are often similar to offenders sentenced to probation (Harris & Lo, 2002; Wermink et al., 2010). All of these effects are possible, and sorting them out is one of the major challenges of research in this area (Abrams, 2012; Mears, Cochran, & Cullen, 2015; Tonry, 2008).

As mentioned, some of the explanation for inconsistency of findings is 1) heterogeneity in response to punishment for different types of offenders often conflate results; 2) different methodologies often find different results based on differences in study design, measurement, geography, and other factors; and 3) the degree to which policies were actually enforced/implemented (Sampson, 2010; Shadish et al., 2002). Not surprisingly, crime reduction impacts tend to be more apparent when programs are successfully implemented as intended, something that is not always easy to control or measure.

The next section will briefly describe what is known about general deterrence and incapacitation, followed by a review of the literature on the post-release effects specific to the individual.

### **A. General Deterrence.**

General deterrence is based on the basic principle of human behavior that if the cost of doing something outweighs the reward, then fewer people will do it (Andenaes, 1968; Spohn & Holleran, 2002). The principle is so basic that the debatable question is not whether a deterrent effect exists but only how strong it is (Becker, 1974). There can also be little doubt that the deterrent effect includes components of both how severe the punishment is and how likely it is to be imposed (Tonry, 2008). Obviously, a punishment would have no significant deterrent effect if it was so mild as to be inconsequential or if it was never imposed. Considering the empirical evidence from a wide range of studies on deterrence theory, research has consistently shown presence of a deterrent effect of punishment in at least some contexts (Levitt, 2002; Tonry, 2008).

Some studies have explored the effect of the length of incarceration on recidivism. Kessler and Levitt (1999) examined the changes in California crime rates following the passage of a voter initiative in 1982 that provided enhanced sentences for repeat offenders of certain crimes.<sup>3</sup> By comparing changes over time in California with the overall national trend, Kessler and Levitt (1999) determined that enhancement-eligible crimes dropped four percent in the first year after enactment. This drop could not be an incapacitative effect because the persons sentenced for these crimes would not have begun the enhanced part of the sentence, thus supporting the conclusion that it was a deterrent effect. Similar legislation on sentencing enhancements for gun crimes was studied by Abrams (2012), who found that gun use enhancements reduced gun-related robberies by an average of 6.6%, 14.8%, and 17.9% when

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<sup>3</sup> This is not the better known and more severe “Three Strikes” law. That law came 12 years later.

examined one, two, and three years after enactment, respectively. Abrams (2012) also found small reductions in gun-related assaults, an average of 1.81% and 0.82% after two and three years, respectively.

Drago, Galbiati, and Vertova (2009) studied an unusual type of natural experiment in which sentence enhancements for recidivism were applied irrespective of criminal history and current offense, meaning that the impact of sentence enhancements could be examined without being conflated with criminal history and offense characteristics. Under an Italian clemency law passed in 2006, a cohort of inmates were released early on the condition that if they offended again within five years, the time subtracted from their sentence would be added on to a new sentence for any subsequent crime. Because this cohort had varying lengths of time remaining (ranging from one month to three years) they were subject to sentence enhancements of various lengths regardless of all other factors. Drago and colleagues (2009) examined database records for 25,800 individuals to study the deterrent effect on future offending, finding that those facing more severe enhancements were somewhat less likely to reoffend, at least within the seven months initially following release.<sup>4</sup> Specifically, researchers found that a one-month increase in anticipated punishment lowered the probability of subsequent charges and/or re-imprisonment by about 0.16 percent (Drago et al., 2009).

There is considerable room for disagreement about deterrence (Nagin & Paternoster, 1991), but the legitimate disagreement is about the magnitude and conditioning of the effect, not the existence (Nagin, 2013; Tonry, 2008). Arguments that punishments always deter and never

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<sup>4</sup> Unfortunately, the researchers did not examine long-term impacts on recidivism rates. Future research would benefit from a replication study with a longer follow-up period to ascertain sustainability of benefits. Sustainability is another key consideration when thinking about a policy's "effectiveness," however that is out of the scope of this work.

deter are equally and oppositely wrong. Given that sanctions do have some deterrent effects, eliminating them altogether would produce some increase in crime. A policy argument for eliminating them would require justification that the elimination would produce benefits sufficient to offset the additional crimes. Any discussion relevant to evidence-based policy change should specify an alternative to the current system, something that is often not considered until after the fact in most real-world situations. For example, mass release of prisoners will cause the number of people on probation to increase, a phenomenon known as “net-widening.” Considering the increased probationary caseload that would inevitably result from mass release of prisoners, it stands to reason that it would be more difficult to implement probation restrictions with a high degree of certainty, an unintended consequence that could potentially impact the effectiveness of this policy change from both a recidivism and cost-savings perspective (Rhodes et al., 2018).

### **B. Incapacitation.**

Incapacitation is the most obvious effect of punishment on crime. In most cases, everyone outside the prison walls will be safe from any further crimes by a given criminal so long as he is inside the walls.<sup>5</sup> The existence of an incapacitative effect is not debatable. Estimating the magnitude is not a simple task, and requires predicting the crimes that would be committed by the prisoners if they were either released or never incarcerated for their crimes. Needless to say, there are significant methodological challenges in constructing an adequate comparison group in this instance.

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<sup>5</sup> There are, of course, notorious exceptions. Clarence Allen, convicted of murder and sentenced to life in prison, sent a recently released cohort out with a list of witnesses to his first murder. Allen’s hit man killed one of the witnesses and two other bystanders, a crime for which Allen was executed (California Department of Corrections & Rehabilitation, 2006).

Piquero and Blumstein (2007, p. 270) note that estimates of the incapacitative effect “vary markedly from study to study,” which may be an understatement. The primary factor in dispute is the estimated number of crimes per year committed by a criminal who would have otherwise been imprisoned. The rate is represented in mathematical models by the Greek letter lambda. However, the overall estimate is largely irrelevant to questions of sentence enhancement policy because individual rates of crime commission vary widely.

Research shows that a small percentage of habitual offenders are likely responsible for a large portion of crime (Lussier & Davies, 2011), and their offending trajectory may differ from nonhabitual offenders (Hunt, Iaconetti, & Maass, 2019). Specifically, high-rate chronic offenders appear to be just a small percentage of offenders, whose offending trajectory seems to generally follow an age-crime curve (Gottfredson & Hirschi, 2016; 1990), but with a higher likelihood to recidivate (Hunt et al., 2019; Lussier & Davies, 2011). Other research suggests this effect may be pronounced for violent offenders, including those who tend to use weapons, who have been found to be re-arrested at higher rates and for more serious crimes than nonviolent offenders (Hunt et al., 2019; Iaconetti, Kyckelhahn, & McGilton, 2019). This difference was the most pronounced for those identified as “career offenders” (Hunt et al., 2019), supporting the idea that a small number of offenders are likely responsible for a large portion of crime. Data from the U.S. Sentencing Commission has found that seriousness of offense is also linked to increase recidivism rates (Hunt & Easley, 2017; Hunt et al., 2019; Iaconetti et al., 2019). One implication for future research would be to learn to better identify high-rate chronic offenders.

In theory, substantial crime reduction could be achieved by incarcerating a relatively small number of prolific offenders, a phenomenon known as “selective incapacitation.” In

practice, identifying those most likely to reoffend is not a precise exercise (Eckhouse, Lum, Conti-Cook, & Ciccolini, 2019; Goel, Shroff, Skeem, & Slobogin, 2018). Further, to the extent that the factors used to identify prolific offenders are not related to culpability, longer sentences for those identified may be unjust (Eckhouse, et al., 2019; Piquero & Blumstein, 2007).

Despite these issues, there is no doubt that incapacitation plays an important role in public safety, as even the foremost opponents of “mass incarceration” agree. According to Blumstein (2002, p. 480), “Incapacitation through imprisonment is probably the only effective means of restraining the violent crimes committed by some individuals otherwise out of social control.” The key question, then, is whether the benefit achieved by targeted extension of the sentences of “individuals otherwise out of social control” is outweighed by a criminogenic effect – i.e., that longer sentences will lead to increased recidivism rates. There is no straightforward answer, particularly when considering how much variation there is when quantifying benefits in cost-benefit analyses (Clear & Austin 2021; Cohen & Farrington, 2021; Zedlewski, 2009). Given what we have discussed regarding the variety of research methodologies, lack of consistency in findings, and general challenges in determining causality, it becomes less clear whether studies really do show strong evidence of a criminogenic effect resulting from longer prison sentences, in contrast to what Gascón (2020) has claimed.

### **III. Incarceration and Post-Release Recidivism.**

The effects of incarceration on the individual prisoner’s post-release recidivism includes at least three conceptually different mechanisms: rehabilitation, specific deterrence, and the criminogenic effects of prison, as described in section II. For empirical research examining length of incarceration on recidivism, though, it is not necessary to separate these mechanisms.

The research often seeks to examine the overall correlation between incarceration and subsequent offending while attempting to control for other factors.<sup>6</sup>

### **A. The Nagin 2009 Review.**

For research before 2009, we already have the benefit of a thorough review (Nagin, et al., 2009). Nagin and colleagues (2009) reviewed five studies that used random assignment, 11 studies with strong quasi-experimental designs<sup>7</sup> and 31 regression-based studies. This review highlights a key distinction between two sentencing decisions: (1) whether to sentence the defendant to prison/jail at all, rather than a noncustodial sentence such as a fine or probation; and (2) for those sentenced to incarceration, how long the sentence will be. The two are not the same, each with a different causal mechanism (Lempert, 2008), and studies of these different effects should be considered separately (Nagin et al., 2009; p. 143). Only the second of these is relevant to the topic of this article.

The first topic of inquiry involves offenders who are on the “policy margin between prison and probation sentences” (Harding et al., 2019, p. 1). These are the offenders who, on average, have a less serious current conviction and fewer if any prior convictions, compared with those more serious offenders for whom probation is clearly not an appropriate sentence. The former are more likely people for whom a crime serious enough to warrant imprisonment is a deviation from a normally law-abiding life. Going to jail for any time at all disrupts family, social, and employment relationships followed by social stigma, all of which is more likely to

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<sup>6</sup> A controlled study on rehabilitation might identify a rehabilitative effect of a particular program by comparing participants with a control group of nonparticipants. That is an important area of research but outside the scope of the present review.

<sup>7</sup> These studies relied on “matching designs” where relevant factors were used to strategically assign people to groups. This generates treatment and control groups that are more similar to each other than would normally be feasible without random assignment.

interfere with resumption of legal employment for a first-time offender than it is for repeat offender (Kruttschnitt, Uggen, & Shelton, 2000; Tonry, 2008; Uggen & Manza, 2002).

Offenders who are not candidates for probation typically have committed especially grave crimes or are often already repeat offenders (Hunt & Easley, 2017; Hunt et al., 2019). Sentence enhancements, the subject of greatest interest here, are for offenders with prior felony convictions or those who are particularly culpable, wanton, or cruel (Cal. Pen. Code §§ 1170.12(a), 12022.5). For example, robbery with a gun presents a greater threat to safety than a robbery committed without a weapon or with a less lethal weapon, and thus is eligible for an enhanced sentence (Cal. Pen. Code § 12022.5). In California, one who commits a crime on the “serious felony” list after one or more previous convictions for crimes on the same list presents a higher risk of recidivism (Hunt & Easley, 2017; Lussier et al., 2011), warranting a possible enhanced sentence (Cal. Pen. Code § 1170.12(a)).<sup>8</sup>

For the purpose of this article, then, the studies in the portion of the Nagin (2009) review discussing “the effect of custodial versus noncustodial sanctions” (Nagin et al., 2009, p. 143) are pertinent only to the extent they have other implications. The studies discussing “the effect of sentence length on reoffending” are more directly relevant, though there are fewer of them (Nagin et al., 2009, p. 167).

In the studies specifically examining the impact of sentence length on recidivism, there are only two actual experimental designs. One had mixed results, and the other showed increased recidivism in the group of inmates randomly selected for a shorter sentence (Berecochea, Jaman, & Jones, 1981; Deschenes, Turner, & Petersilia, 1995). That is, one experimental study showed

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<sup>8</sup> “Prior record of convictions is also generally relevant to the sentencing decision and is perhaps the best predictor of reoffending rate” (Nagin et al., 2009, p. 137).

no sizable effect either way, while one suggested that longer sentences reduce recidivism. There were 17 nonexperimental studies, most of which were conducted for another purpose and have only incidental results on this point. Three of these employed quasi-experimental designs (Jaman, Dickover, & Bennett, 1972; Kraus, 1974; Loughran, Mulvey, Schubert, Fagan, Piquero, & Losoya, 2009), though the overall “results of these studies are quite varied” (Nagin et al., 2009, p. 169). Indeed. The estimates of “criminogenic” or “preventative” effects are scattered with similar numbers of each, and most of the results are not statistically significant. That is, the observed correlation between sentence length and recidivism is so weak that the researchers cannot say with confidence that it is real rather than just random variation. Part of this reason is likely due to variation in study methodologies, particularly in the 17 nonexperimental studies, which are at higher risk for selection bias effects (Cook et al., 2008; Shadish et al., 2000). Thus, it is hard to know whether these findings would extend to broader groups of offenders (Sampson, 2010; Wermink et al., 2010).

The bottom line is that as of 2009, “there [was] little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate” (Nagin et al., 2009, p. 183) That is, studies did not clearly demonstrate that longer prison sentences increased recidivism.<sup>9</sup>

## **B. Subsequent Research.**

As discussed above, estimating the causal relationship between length of incarceration and recidivism is difficult for a variety of reasons (Rhodes et al., 2018). Since the 2009 review

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<sup>9</sup> They did not show that sentence length reduces recidivism, either, but advocates of enhanced sentences have not generally advanced specific deterrence as a principal reason for such sentences. Such calls have been based primarily on incapacitation and retribution (i.e., justice). (Wilson, 1975; Scheidegger & Rushford, 1999).

by Nagin et al., there have only been a handful of methodologically rigorous studies that have attempted to do so. Taken together, the findings are still very mixed, providing little conclusive evidence for or against the specific deterrent effects of imprisonment (Rhodes et al., 2018), similar to conclusions reached by Nagin et al. (2009). Two of the studies used variations of judge-randomization strategies and examined data prospectively (Green & Winik, 2010; Roach & Schanzenbach, 2015). Six studies exploited natural experiments and analyzed data retrospectively, attempting to control for selection effects (Kuziemko, 2012; Rhodes et al., 2018; Meade, Steiner, Makarios, & Travis, 2013; Mears, Cochran, Bales, & Bhati, 2016; Snodgrass, Blokland, Haviland, Nieuwbeerta, & Nagin, 2011), though one of these is not peer-reviewed (Cotter, 2020). Finally, one study re-examined Kuziemko's (2012) and Green and Winik's (2010) data using new techniques (Roodman, 2017).

When considering the following studies, recall the potential limitations of selection bias that are inherent in many of these designs (Shadish et al., 2002; Wermink et al., 2010), particularly those relying on retrospective data that attempt to control for selection bias effects. Attempts to control for selection bias can increase risk of error, increasing the likelihood of finding mixed effects (Cook et al., 2008; Sampson, 2010). High levels of selection bias are not desirable because it limits how well the findings extend to other people beyond the study (Shadish et al., 2002; Wermink et al., 2010).

Meade and colleagues (2013) compared offenders that differentiated in terms of sentence length (i.e., less than one year, between one and two years, and between two and six years) and relationships with one-year felony re-arrest rates. The authors used propensity scores to account for important factors (such as prior convictions) when examining felony re-arrest rates among a

sample of nearly 2,000 parolees in Ohio who were released following statewide changes in parole statutes. Meade et al. (2013) found that only one category of ‘sentence length’ – the longest sentence length (i.e., two to six years) – was statistically significant in lowering one-year felony re-arrest rates. However, these reductions were minimal. Meade et al. (2013) suggest that potentially longer prison terms may deter people from committing *more serious* crimes to avoid receiving a lengthier prison term, as opposed to being deterred from committing crime altogether.

Mears and colleagues (2016) also used propensity scores when studying the impact of months served on one-, two-, and three-year felony re-conviction rates among a cohort of more than 90,000 inmates released from Florida prisons. In response to concerns regarding heterogeneity in response to incarceration (Abrams, 2012; Nagin et al., 2009; Snodgrass et al., 2011), the authors used statistical models to estimate and compare multiple offending trajectories in an attempt to better understand the functional relationship(s) between time served and recidivism. Trajectories were estimated to represent various positive, negative, and curvilinear relationships between time served and recidivism (i.e., felony re-conviction rates). While controlling for important pre-incarceration differences, the authors found similar results across the three models, with a few interesting caveats. Overall, the average length of time served was 24 months, and the impact on recidivism seemed to vary. Prison terms of less than one year were associated with increased recidivism rates consistently across models, suggesting a potential criminogenic affect among this category. This effect also was pronounced for offenders under the age of 23. However, this effect plateaued after one-year post-release. In contrast, those serving prison terms of one to two years had slightly decreased recidivism rates than those

serving six to 12 months, though this effect plateaued after approximately two years post-release. Terms of three to five years had no effect on recidivism, and terms of six years or more appeared to result in a slow and monotonical decline in recidivism, though the latter model was riddled with large amounts of error (Mears et al., 2016). The results are consistent with past literature in that they point to both a potential for deterrent effects as well as criminogenic effects, with the implication that a deepened understanding of the causal mechanisms at play is warranted, particularly in regard to younger offenders. According to Mears and colleagues (2016), lengthier sentences may be justified in some circumstances to either achieve retributive goals or to achieve a stronger deterrent benefit.

Similar to the propensity score approach, Rhodes and colleagues (2018) used an instrumental identification strategy to examine the dose-response relationship between prison length of stay and recidivism among a large sample of federal offenders. The U.S. Sentencing Guidelines recommend how to quantify offense seriousness and offender criminal history, resulting in numerous determinative sentence ranges that tend to increase with offense seriousness and criminal history. Using this information, the researchers created an instrumental variable that accounted for each individual's offense seriousness and criminal history; this variable was then used to ensure that treatment and control groups looked similar enough to each other for adequate quasi-experimental comparisons (Rhodes et al., 2018). The findings revealed that longer prison terms may modestly reduce rates of recidivism beyond incapacitative benefits, though this effect was small. Specifically, the recidivism appeared to decrease from 20% to 19% relative to a 7.5-month increase in incarceration term (Rhodes et al., 2018).

Roach and Schanzenbach (2015) employed a quasi-experiment with a cohort of nearly 8,000 lower-level felony offenders and 25 judges within a Seattle courthouse. All offenders in the sample pled guilty under one judge, and then were randomly assigned to a new judge for sentencing. Sentencing hearings were scheduled once per week, each with one judge that was pre-assigned weeks in advance (Roach & Schanzenbach, 2015). Under state sentencing guidelines, judges have a fair amount of discretion to depart from recommended sentencing ranges for crimes of low severity and for first-time offenders (Revised Code Washington § 9.94A.020(5)). Considering that the average offense seriousness level was fairly low (two out of a possible 16) among offenders in Roach and Schanzenbach's (2015) sample, they argued that variation in judicial discretion resulted in seemingly random variation in prison sentences. The majority of imposed sentences (74%) were less than 12 months, with an overall average of nine months. Recidivism outcomes were measured based on whether an offender was sentenced for any new felony offense within three years post-release. Overall, Roach and Schanzenbach (2015) found that sentence length actually decreased three-year felony recidivism rates (i.e., re-sentencing rates) by approximately one percent per each additional month of incarceration (Roach and Schanzenbach, 2015). However, this study uses a very specific sample of lower-level offenders with relatively short sentences, all of whom had entered a guilty plea, which introduces an element of selection bias.

Green and Winik (2010) used a similar judge pseudo-randomization strategy using 1,000 defendants and nine judge/courtroom assignments from within District of Columbia superior courts. When examined at four-years post-release, they found that each additional month of incarceration appeared to increase both re-arrest rates and felony re-conviction rates by about

1.79% per month and 1.4% per month, respectively. However, they did not control for the impact of courtroom/judge assignment which may have affected results. Later analysis by Roodman (2017) with the same data accounted for courtroom/judge assignment and found results to be less robust, finding each additional month of incarceration to be associated with increases of about one percent in four-year arrest rates and increases of about 0.81 percent in four-year felony re-conviction rates. Overall, the effect sizes were minimal in all variations of both Green and Winik (2010) and Roodman's (2017) analyses, particularly when considering felony re-conviction rates, which are the more comparable outcome in relation to past research.

Kuziemko (2012) attempted to identify the causal impact of length of stay on recidivism using multiple methodologies as part of a natural experiment using parolees released in Georgia. First, Kuziemko (2012) took advantage of state parole guidelines that release prisoners based on a "recidivism risk" calculation. Kuziemko (2012) compared more than 17,000 similar nonviolent convicts entering prison on either side of the calculated cutoff between medium- and high-risk in regards to their recidivism (i.e., return to prison) rates. Parole guidelines were used to construct an instrumental variable that would account for prior incarcerations (0.8 on average) and offense severity (ranging from levels one through four on a scale of 20), among other factors. The average time served was approximately 33 months, ranging from seven months to ten years. Overall, 25% of individuals ended up returning to prison for a new crime (i.e., not a parole violation) at some point during the three-year follow-up period. When measured at three-years post-release, findings revealed that lengthier sentences were associated with a decreased percentage of prisoners returning to prison for a new crime by approximately 1.3% per additional month served. However, there are some concerns that the cutoff between high- and medium-risk per the risk calculation might be associated with potential third factors that could explain recidivism

rates.

Second, Kuziemko (2012) examined a smaller subgroup of 519 nonviolent offenders that were released as a result of the 1981 changes to the state statute in an effort to reduce overcrowding. The author constructed an instrumental variable that would account for prior incarcerations (0.53 on average), original length of sentence (36 months on average), and number of months released early (approximately five months), among other factors. The average time served for this subgroup was approximately 13 months, ranging from one month to six years. Overall, 36% ended up returning to prison for a new crime at some point during the three-year follow-up period. When measured at three-years post-release, findings revealed that lengthier sentences were associated with a decreased percentage of prisoners returning to prison for a new crime by approximately 3.2% per extra month served.

However, later re-analysis of Kuziemko's original data by Roodman (2017) found different results – i.e., the latter found an insignificant impact of length of stay on recidivism. Roodman (2017) presents several potential reasons why this may be the case, such as variable construction, measurement error, and parole bias. The contrasting results found by Kuziemko (2012) and Roodman (2017) based on the same data truly shows how different methodologies can find sharp differences in results, and speaks to why we need to use multiple studies (rather than just one) to justify new policy.

Snodgrass and colleagues (2011) examined the relationship between length of time served and three-year felony re-conviction rates among more than 4,500 prisoners in the Netherlands. Similar to some of the methods discussed above (Meade et al., 2013; Mears et al., 2016), the authors used a variation of propensity score matching to retrospectively create groups that looked roughly similar to each other in terms of incarceration length of stay, criminal

history, offense severity, and the like. Similar to Roach and Schanzenbach (2015), the average length of incarceration for this sample was relatively short, with an average of seven months and majority (86%) of sentences being less than one year. Based on the interquartile ranges of the incarceration lengths, offenders were classified as “low-dose” or “high-dose.” On average, “low-dose” offenders were convicted of .033 more felonies per year than comparable “high-dose” offenders, suggesting a slight deterrent effect of longer sentences. However, this relationship was not statistically significant. Further, there was no ability to examine how results varied across subclassifications of offenders because data were only available in the aggregate.

Most recently, a report released by the U.S. Sentencing Commission used four different modeling approaches to estimate the impact of various incarceration lengths (i.e., 2-3 years, 3-4 years, 4-5 years, 5-10 years, and more than 10 years) on eight-year re-arrest rates among a sample of 25,400 offenders released from federal prison in 2005 (Cotter, 2020). Consistently across all four models, Cotter (2020) found that incarceration terms of more than 10 years were associated with lower eight-year re-arrest rates (30-45% decrease depending on the model). Across two models, incarceration terms of more than five years were associated with lower (approximately 17% decrease) eight-year re-arrest rates. When examining incarceration lengths of 2 to 5 years, there was no significant criminogenic or deterrent effect on recidivism when examined at eight-years post-release. Incarceration lengths of 1 to 2 years were not always consistent with reductions in recidivism, however, this varied across research designs and findings were not statistically significant (Cotter, 2020). In addition, it is important to note that this measure for recidivism is any re-arrest, as opposed to the many other studies that measure recidivism based on re-conviction or felony re-conviction.

Considering the rigorous research published since the Nagin et al. (2009) review, the literature regarding length of stay on recidivism is still somewhat inconsistent, with many studies claiming no recidivism effects and some showing that increased prison length reduces recidivism slightly (Rhodes et al., 2018). However, just like the rest of the research examined thus far, the study methodologies vary in terms of their limitations, which could explain some of the mixed results (Rhodes et al., 2018; Roodman, 2017). In addition, studies span different geographical jurisdictions, which can also condition findings.

It is clear that research has not fully unpacked the quantifiable costs and benefits regarding the *length* of incarceration, particularly in regards to recidivism. Overall, some important considerations remain unclear. First, it remains unknown whether shorter prison sentences would result in the same sense of retribution for the victim as well as society. Second, it is unclear whether shorter prison sentences would negatively impact public safety gains achieved from incapacitation or deterrence. Third, identifying individuals more or less likely to recidivate remains a daunting and complex task (Eckhouse et al., 2019; Goel et al., 2018). Many factors independent of sentence length may influence one's likelihood to recidivate, such as age (Mears et al., 2016), offense history (Rhodes et al., 2018; Tonry, 2008), post-release social supports (Hirshchi, 1969; Sampson & Laub, 1995), experiences in prison (Chen & Shapiro, 2007; Cochran, Mears, Bales, & Stewart, 2014; Gaes & Camp, 2009), and post-release supervision, among others (Sampson & Laub, 2003; Spohn & Holleran, 2002). Lastly, at present there is no substantial evidence that a criminogenic effect exists in the aggregate. Thus, it remains unclear whether criminogenic effects exist, and if so, under what circumstances.

The literature on the impact of incarceration on recidivism is admittedly limited by important methodological considerations, resulting in inconsistency of findings across studies. Perhaps the most important implication from the research is best summarized by Mears, Cochran, and Cullen (2015, p. 691):

“We argue that a better understanding of the heterogeneity of incarceration—including the types and sequences of sanctions and experiences that occur before, during, and after imprisonment—and of incarceration effects among different groups is important for two reasons. First, it can assist with assessing the salience of prior research on the effects of incarceration on recidivism. Second, it serves to identify conceptual and methodological challenges that must be addressed to provide credible assessments of incarceration effects.... [I]ncarceration likely exerts a variable effect depending on the nature of the prison experience,...including prior sanction history, and the specific populations subject to imprisonment.”

Evidence-based practice should involve a critical examination of the breadth and depth of the existing empirical research before enacting sweeping policy changes with potentially irreversible effects. When considering the entire body of research, there is some evidence that suggests that certain punishments may be an effective deterrent to crime, though this effect is not always consistent or strong. Further, among the substantial number of published studies with varying methodologies, not one has found a large aggregate-level criminogenic effect. Once again, this highlights the need to consider the totality of findings across research studies and the contexts to which they apply when enacting policy change.

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