

ORIGINAL ARTICLE

CRIME CONTROL AND RECIDIVISM

Is life without parole an effective way to reduce violent crime? An empirical assessment

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Research Summary: By taking advantage of data published by the Sentencing Project to analyze whether states that use life without parole (LWOP) more often experience lower violent crime rates or greater reductions in violent crime, this study is the first to empirically assess the crime-reducing potential of LWOP sentences. The results suggest that LWOP might produce a small absolute reduction in violent crime but that it is no more effective than life with parole.

Policy Implications: Despite reductions in the use of the death penalty, LWOP has expanded dramatically—and at a much faster rate—over the last quarter century. This expansion has come at great financial and human costs and has not been distributed equally throughout the population. As such, the public policy debate over the use of LWOP is likely to intensify. Yet, to date, there have been no empirical assessments of LWOP's efficacy to inform this debate. This study begins to fill this gap in our knowledge, and the results, if replicated, suggest that the use of LWOP should be either scaled back or eliminated.

KEYWORDS

deterrence, incapacitation, life without parole, sentencing policy, violent crime

Over the last several decades, life-without-parole (LWOP) sentences have proliferated in the United States. In 1970, only seven states even authorized LWOP (Nellis, 2013), but by 2016, only Alaska did not have an explicit LWOP statute (Nellis, 2017). Even more recently, while the use of the formal death penalty has been in decline for the past two decades (Death Penalty Information Center, 2018), the size of the LWOP population has exploded at a rate that far surpasses the decline in death sentences so that many who were never at risk of an execution are now being sentenced to die in prison (Henry, 2012). Since 1992, the number of inmates serving LWOP has more than quadrupled (Nellis, 2013, 2017).

while the overall prison population has only increased by 71% (Carson & Mulako-Wangota, n.d.). In 2016, nearly 1 in every 28 American prisoners was serving LWOP, compared with just 1 in every 71 prisoners in 1992 (Carson & Mulako-Wangota, n.d.; Nellis, 2013, 2017). More people are now serving LWOP in the United States than there are people serving life sentences of any kind in Europe, which has more than double the population, and it has been estimated that the U.S. imprisons more than *half* of the worldwide LWOP population (van Zyl Smit & Appleton, 2019). There are, obviously, numerous reasons for this rapid expansion in the use of LWOP, including as an alternative to the death penalty (Barkow, 2012; Seeds, 2018), but one of the formal, explicitly stated goals of this sanction in at least some states was to fight crime (Broun & Allison, 2016; Gottschalk, 2014; Robinson, 2012; Spohn, 2014; Tonry, 2016; van Zyl Smit & Appleton, 2019; Yates, 2015). Nonetheless, as the use of LWOP has grown, several criticisms of LWOP have started to emerge. These sentences are extraordinarily expensive, especially considering the fact that the costs of incarceration increase dramatically as inmates age (American Civil Liberties Union, 2012; Human Rights Watch, 2012). They also have a racially disproportionate impact as the majority of those sentenced to LWOP are people of color (American Civil Liberties Union, 2014; Capers, 2012; Nellis, 2013, 2017), and because of the amount of suffering they cause and their threat to human dignity, LWOP sentences have been denounced by many as a violation of human rights (Capers, 2012; Henry, 2012; Leigey, 2015; van Zyl Smit & Appleton, 2019; Villaume, 2005).

Given this state of affairs, it would be prudent to ask whether LWOP is even effective at reducing violent crime. In fact, more than three decades ago, prior to the explosion in the use of LWOP, Cheatwood (1988) called for research into LWOP's efficacy. Yet, this question has never been examined empirically, and the evidence that does exist is equivocal on this point. On the one hand, violent crime rates have plunged since the early 1990s (Federal Bureau of Investigation, 2017), so it is entirely plausible that the growth of LWOP has contributed to this decline. On the other hand, existing criminological theory and research both provide contradictory expectations as to whether LWOP is likely to reduce violent crime. Whereas both incapacitation and deterrence theories suggest that increasingly severe sentences (like LWOP) will reduce criminal offending, both theories also point out that as sentences get overly harsh, there are likely to be diminishing returns and that at some point, the crime-reducing power of a punishment will max out. Empirically, there is a robust and consistent finding that the certainty of punishment matters more than its severity and that increasing already harsh punishments does not reduce crime (Travis, Western, & Redburn, 2014), but there is considerable debate in the academic literature over the deterrent effect of the death penalty (Nagin & Pepper, 2012). In many ways, LWOP is more similar to a death sentence than it is to a lengthy term of years (Berry, 2010; Henry, 2012; Kleinstuber, Joy, & Mansley, 2016; Leigey, 2015; Villaume, 2005), so it is not clear whether the findings of the wider literature on deterrence or the findings of the death penalty literature are more applicable to LWOP. Therefore, it is crucial to study the specific impact of LWOP to understand what role, if any, it plays in lowering violent crime rates. This study attempts to do just this by taking advantage of data collected and published by The Sentencing Project on LWOP sentences to examine whether states that use LWOP more prolifically experience lower violent crime rates or greater reductions in violent crime when compared with states that use LWOP less often.

1 | INCARCERATION AND CRIME: THEORETICAL AND EMPIRICAL OVERVIEW

The idea that harsher punishment may reduce crime is premised on two different theories: deterrence and incapacitation. Simply put, deterrence theory argues that the threat of punishment will convince

potential lawbreakers to obey the law in order to avoid the displeasure associated with the punishment (Beccaria, 1767/1963; Becker, 1968). The concept of incapacitation, meanwhile, makes no claim that punishment will alter the decision-making of potential offenders. Rather, by locking offenders in prisons, it sees punishment as making it physically impossible (or at least extremely difficult) for them to re-offend (Canelo-Cacho, 2002; Levitt, 2004).¹ According to both approaches, therefore, LWOP sentences should be highly effective at reducing crime. LWOP is one of the most severe sanctions that can be imposed, which means that it should invoke the most fear in potential offenders, and by making offenders ineligible for release, it should make their incapacitation permanent.

A few caveats in these approaches, however, cast doubt on the assertion that LWOP should reduce crime. First, deterrence theory suggests that the certainty of punishment is more important than its severity (Beccaria, 1767; 1963), which means that using LWOP without increasing the risk of apprehension is unlikely to have much of an effect on crime. Furthermore, the concept of marginal deterrence suggests that each additional increment of a punishment's severity will have less effect than the previous increment because the additional deterrent value added by making a sentence more severe is necessarily limited by the deterrent value of less severe sanctions (Stigler, 1970). In other words, increasing the severity of punishment will only be able to deter those who have not already been deterred by less serious penalties; thus, as punishment severity increases, there will likely be diminishing returns as there are fewer and fewer people left to deter (Donohue, 2009; Roeder, Eisen, & Bowling, 2015; Travis et al., 2014). Perhaps even more problematically, by increasing the severity of a punishment, the sanctions for less serious crimes become so similar to those of more serious crimes that a rationally calculating potential offender may see no reason not to commit the more serious offense (Beccaria, 1767; 1963; Shavell, 1992). Given this proposition, it is difficult to imagine that LWOP could be any more effective than life with parole or even a lengthy term of years at deterring potential offenders.

Similarly, because criminal activity tends to diminish quickly after one's mid-20s (Hirschi & Gottfredson, 1983; Siegel, 2015), there are also likely to be diminishing returns on incapacitation as punishment gets more severe. Once offenders have been incarcerated beyond the ages in which they are likely to be criminally active, further imprisonment is no longer accomplishing any incapacitation. Thus, it would again seem unlikely that LWOP could be superior to life with parole or to a decades-long prison term at incapacitation. Furthermore, because society tends to incarcerate the most severe offenders first, expanding LWOP to include less and less serious offenders is also likely to have diminishing returns. As Levitt (2004, p. 179) explained, "the two-millionth criminal imprisoned is likely to impose a much smaller crime burden on society than the first prisoner." Therefore, even if LWOP does achieve some incapacitating effect on the most severe offenders who may have continued to offend until their deaths, the expansion of LWOP to other offenders is not likely to achieve any additional crime reduction.

The existing empirical research supports the idea that punishment can reduce crime but that the certainty of punishment matters more than its severity and that the efficacy of punishment dissipates rapidly. As Nagin (2013, p. 201) has summarized, "the perceived certainty of punishment is associated with reduced self-reported or intended offending." For example, increasing police resources to increase the odds of apprehension has been shown to lower crime rates (Evans & Owens, 2007; Levitt, 1997, 2004; Marvell & Moody, 1994) and imposing short jail sentences has been shown to be a more effective deterrent than probation or fines (Hawken, 2010; Hawken & Kleiman, 2009; Kleiman, 2010; Weisburd, Einat, & Kowalski, 2008). Yet, "there is little evidence that increases in the length of already long prison sentences" produce any additional deterrent value (Nagin, 2013, p. 201)—a conclusion that has been reached by "[n]early every leading survey of the deterrence literature" (Travis et al., 2014, p. 90). These findings would suggest that LWOP is unlikely to impose any additional deterrent value that other harsh, but slightly less severe, punishments have not already achieved.

When it comes to incapacitation, one study found that after being released from prison, more than half of offenders returned to the criminal trajectory on which they had been prior to being incarcerated (Bhati & Piquero, 2008), which suggests that incarceration serves an important incapacitating role. The data also consistently indicate, however, that inmates serving life terms are comparatively well behaved (Cunningham & Sorensen, 2006; Sorensen & Reidy, 2018, 2019) and that upon release, older offenders and paroled lifers are extremely unlikely to re-offend (Advisory Committee on Geriatric and Seriously Ill Inmates, 2005; Anderson, 2019; California Department of Corrections and Rehabilitation, 2013; Justice Policy Institute, 2018; Mauer, King, & Young, 2004; Millemann, Bowman-Rivas, & Smith, 2017; Rosenfeld, Wallman, & Fornango, 2005; State of New York Department of Corrections and Community Supervision, 2016; van Zyl Smit & Appleton, 2019; Weisberg, Mukamal, & Segall, 2011, pp. 282–285). Furthermore, Roeder et al. (2015) discovered that although increasing incarceration rates were responsible for around 6% of the decline in property crime rates during the 1990s, they have had no impact since then, and they played almost no role in the decline in violent crime since 1990. All of this indicates that even though prison sentences will obviously prevent incarcerated offenders from being able to re-offend against members of the general public, LWOP is unlikely to be more effective than life with parole or even lengthy periods of incarceration at incapacitating offenders, and that as LWOP expands its reach to less and less serious offenders, its incapacitating effects will decline precipitously.

Although there is no direct test of LWOP's efficacy, there have been numerous analyses of the impact of three strikes laws. Three strikes policies, which mandate a minimum sentence for third-time felons—sometimes even life in prison—developed at the same time as LWOP expanded, were fueled by the same “tough-on-crime” mentality, and in many cases were a *cause* of LWOP's expansion (Ogletree & Sarat, 2012; Spohn, 2014; Tonry, 2014, 2016). Yet, the results of these studies have been equivocal. Some tests concluded that three strikes policies reduced crime (Chen, 2008; Helland & Tabarrok, 2007; Shepherd, 2002), whereas others found that they did not (Males & Macallair, 1999; Worrall, 2004; Zimring, Hawkins, & Kamin, 2001) and still others found that they *increased* homicide rates (Kovandzic et al., 2002; Marvell & Moody, 2001). Yet another study found that three strikes laws reduced nonviolent offending but increased violent crime (Iyengar, 2008), and a recent analysis of California's three strikes law concluded that its impact on crime was modest and that the second strike produced a greater marginal deterrent effect than the third strike (Datta, 2017). These findings would seem to indicate that although incarceration can reduce crime, the magnitude of this effect is small and that the effect of increasing sentencing severity maxes out quickly, which would suggest that LWOP is probably no more effective than life with parole or even a lengthy term of years.

2 | LIFE WITHOUT PAROLE: DEATH BY ANOTHER NAME?

Unfortunately, none of the studies looking at incarceration's impact on crime have looked at the impact of LWOP sentences, which several commentators and scholars have argued are really just “semantically disguised sentences of death” (Villaume, 2005, p. 266; see also Berry, 2010; Henry, 2012; Kleinstuber et al., 2016; Leigey, 2015). In the words of one death row inmate in Delaware, “the difference between LWOP and death is like the ‘difference between drowning and suffocation,’” and another death row inmate compared the difference between LWOP and death to “‘living on the East and West sides of Hell’” (Kleinstuber et al., 2016, p. 188). Echoing these sentiments, an inmate serving LWOP indicated in an interview that inmates refer to LWOP as “the hard death penalty” (Leigey, 2015, p. 15). When viewed in this light, the dearth of any empirical assessment of LWOP's influence on crime is alarming. In essence, if LWOP is conceptualized as a type of death sentence, then the dramatic reduction in

death sentences and executions that the United States has experienced since the mid-1990s (Death Penalty Information Center, 2018) is a mirage as there are now *more* people being sentenced to die in prison—all without the benefit of the super-due process protections to which death row inhabitants are entitled (Berry, 2010; Henry, 2012). Furthermore, if LWOP is a type of death sentence, then the literature on incarceration and crime reviewed earlier may not be applicable to it, and the literature on capital punishment is mixed as to what effect, if any, the death penalty has on crime rates (Nagin & Pepper, 2012). Some studies have found a robust deterrent effect, whereas others have found either no deterrent effect or a brutalization effect (Nagin & Pepper, 2012; Shepherd, 2005).

One of the primary reasons for these contradictory findings is that these studies are plagued with methodological problems that drastically influence the outcome of their statistical models (Berk, 2005; Donohue & Wolfers, 2005, 2009; Nagin & Pepper, 2012). LWOP, however, overcomes many of these methodological pitfalls, making it less susceptible to the statistical volatility and uncertainty of death penalty studies, which makes it a potentially fruitful avenue for empirical investigation. One of the major issues death penalty studies must confront is that it is impossible to know how many death sentences will eventually lead to an execution, so there is no way to create a universally agreed upon method of calculating the risk of execution (Nagin & Pepper, 2012). LWOP, on the other hand, begins to be carried out the moment the sentence is imposed, and because LWOP sentences are not subject to any additional judicial scrutiny (like automatic review, state-funded appellate counsel, or super-due process) the way that death sentences are, LWOP sentences are rarely overturned or commuted (Barkow, 2012; Henry, 2012), which means there is no need to determine which LWOP sentences will eventually be imposed. In fact, although most death sentences imposed since 1973 have *not* resulted in an execution (Baumgartner & Dietrich, 2015), the reversal rate for noncapital cases is estimated to be around 10% to 20% (Henry, 2012).

Second, executions and formal death sentences are statistically rare events that are not imposed in a time-stable manner, which means that even slight variations in measurement practices or model specifications can yield dramatically different results and outlier cases can have an undue influence on the outcome of statistical models (Berk, 2005; Cohen-Cole, Durlauf, Fagan, & Nagin, 2009; Donohue & Wolfers, 2005; Land, Teske, & Zheng, 2009; Nagin & Pepper, 2012; Shepherd, 2005). It also means that, because executions happen so infrequently, any deterrent effect that they have, no matter how large, might simply be too small to detect (Katz, Levitt, & Shustorovich, 2003). When considering the fact that homicide rates are also highly unstable, these realities suggest that it will be challenging to distinguish between signal and noise in statistical models, which again means that the results will be highly susceptible to model specification (Katz et al., 2003). LWOP sentences, however, are not statistically rare, and because they last so long, their number is stable over time, which should also make statistical models less volatile and more reliable.

A third major problem for death penalty studies is that death sentences and executions are discrete events that occur at a single moment in history, but it is not clear how long their deterrent effects can be expected to last (Zimmerman, 2004), and the time period one chooses to investigate can have an enormous impact on the outcome of a study (Donohue & Wolfers, 2005; Land et al., 2009). In contrast, LWOP is a continuous event that lasts indefinitely, which means that its deterrent or incapacitating power—if it exists—should also be continuous, thus, eliminating this problem.

Lastly, as a practical matter, the implementation of the death penalty is neither swift nor certain—even among those who have already been sentenced to death—a fact that might wipe out or mask any deterrent value that executions might otherwise have had (Nagin & Pepper, 2012; Shepherd, 2004). Once again, LWOP overcomes this concern because it is a mandatory penalty for many crimes, it is imposed immediately upon being sentenced, it continues to be imposed even during any subsequent appeals, and it is rarely overturned or commuted (Barkow, 2012; Henry, 2012). Therefore, even though

the objective and perceived risk of being sentenced to LWOP is difficult to determine with precision, LWOP sentences are clearly far more certain than death sentences, which means that LWOP's deterrent power is less likely to be masked by a lack of certainty.

Given these concerns, the ability of LWOP to overcome them, and the proliferation of LWOP over the past quarter-century, it is imperative that the specific impact of LWOP be studied. Simply put, LWOP occupies a unique location somewhere between a harsh prison sentence and a formal death sentence, so it is impossible to generalize existing research findings to LWOP, which means that LWOP must be studied as a separate phenomenon. Even if LWOP is conceptualized as a type of death sentence, its distinctive features provide it some clear advantages relative to formal death sentences and executions for analyzing the punishment's effect on violent crime rates. If, on the other hand, LWOP is not considered a type of death sentence, then it is not clear how much more severe it is than a term of years, so its crime-fighting potential is still unknown. Thus, it is critical to subject LWOP's effectiveness as a crime-fighting tool to empirical analysis, which has yet to be done and is precisely what this study attempts to do.

3 | METHOD

3.1 | Primary independent variables

Conceptualizing exactly what is meant by a sentence of LWOP is challenging because there are ways of accomplishing *de facto* LWOP sentences without explicitly sentencing someone to LWOP. For example, Alaska does not authorize LWOP sentences, but it does permit sentences of 99 years. The question of precisely how long a fixed-term sentence must be before it should be considered a "virtual" or *de facto* LWOP sentence, however, is an open question with no easy answer (Barkow, 2012; van Zyl Smit & Appleton, 2019).² Furthermore, even if we were to arbitrarily assign a length for virtual life sentences, the only year for which there are nationwide, state-level data on the number of prisoners serving lengthy or virtual life sentences is 2016 (Nellis, 2017; van Zyl Smit & Appleton, 2019). Therefore, to ensure that we have a reliable and consistent measure of LWOP over time, we have restricted our analysis to explicit LWOP sentences.

Data on the number of inmates serving formal LWOP sentences in each state are only available in the years 1992, 1993, 2003, 2008, 2012, and 2016. Because of the close proximity of the years 1992 and 1993, we decided to omit the year 1993 from our analysis. The number of individuals serving LWOP for 1992 comes from the Bureau of Justice Statistics' (BJS) *Sourcebook of Criminal Justice Statistics*. For all other years, we rely on data published by The Sentencing Project. To explore the full range of LWOP's possible deterrent and incapacitating effects, we use four different measures of LWOP. The total number of people serving LWOP sentences in thousands ("Total LWOP")³ and the number of people serving LWOP per 10,000 population ("LWOP per capita") are both expected to capture the absolute deterrent or incapacitating effect of LWOP. We include both measures because there is disagreement over whether the sheer volume of punishment or the rate of punishment is the thing to which potential criminals respond (Kovandzic, Vieraitis, & Boots, 2009). On the one hand, as the number of people serving LWOP increases, the public is likely to hear about the sentence more often (even if the actual rate at which the sentence is imposed remains low). By way of comparison, most people consider Texas to be the capital of capital punishment because it issues the largest number of death sentences and performs the most executions. From 1977 to 1999, however, Texas ranked *sixteenth* in death sentences per known murderer, while Nevada had the highest death sentence-to-known murderer ratio (Blume, Eisenberg, & Wells, 2004). Even looking at executions, from 1976 to

2015, Texas was second to Oklahoma in executions per capita, and from 1977 to 2010, it was second to Virginia in executions per death sentence (Death Penalty Information Center, n.d.). Nevertheless, as a result of the publicity of Texas's death penalty, one might reasonably conclude that it would be the riskiest state in which to kill. The same logic might also carry over to LWOP sentences; thus, it could be the sheer volume of LWOP sentences rather than the LWOP rate that deters. On the other hand, as the rate of people serving LWOP goes up, both the likelihood of knowing someone who was sentenced to LWOP and the objective risk of receiving a LWOP sentence increase. Therefore, we examine the effect of LWOP both ways.

In addition to absolute effects, we are interested in the possibility of marginal deterrent or incapacitating effects—that is, does LWOP have any additional crime-reducing power beyond that which is achieved by life with parole or by incarceration more generally? Therefore, we also measure the proportion of all life-sentenced inmates who are ineligible for parole (“LWOP per life sentence”) and the percentage of all prison inmates who are serving LWOP (“%LWOP prisoners”) to capture the effect of LWOP beyond life with parole and imprisonment, respectively.⁴ It is important to recognize the difference in scaling for these two LWOP measures. “LWOP per life sentence” has a theoretical range of 0 to 1, whereas “%LWOP prisoners” has a theoretical range of 0 to 100.

These two measures have two additional benefits compared with our first two measures of LWOP. First, because one must first be convicted of a crime—specifically a life-eligible offense—to be sentenced to LWOP, these two variables are better measures of the objective risk of receiving a LWOP sentence compared with “LWOP per capita.” These variables also, to some extent, help to control for the possibility that the LWOP population increased *because* violent crime increased. If increasing violent crime rates are driving the size of the LWOP population, they should also be driving a comparable increase in overall prison populations and the overall population of lifers. Thus, increasing violent crime rates should not explain an increase in the proportion of inmates or lifers serving LWOP. Both of these ratios are the result of purely *political* decisions to deny a greater proportion of felons an opportunity to ever be released, and so, these two measures allow us to examine whether these political choices are an effective way to lower violent crime rates.

3.2 | Dependent variables

We measured the impact of LWOP on violent crime through two dependent variables. The first dependent variable, “Violent crime,” is the violent crime rate, excluding rape, measured at the state level as reported by the FBI's *Uniform Crime Report* (UCR). Our measure therefore includes the number of reported murders, robberies, and aggravated assaults. Our rationale for using this variable is described in the Appendix. Furthermore, to ensure time order, we measure the violent crime rate in the calendar year *after* our LWOP measures.

Although the use of the “Violent crime” variable will indicate whether states that use LWOP more often have lower violent crime rates compared with states that use it less often, there is also a potential problem with this variable. Crime rates tend to be highly correlated from year to year. Therefore, a state may have a high LWOP population or its policy makers may choose to send a greater proportion of offenders to LWOP *because* the state had a high rate of violent crime. Although we attempt to control for this concern through the use of the two proportional independent variables, these are really an imperfect means of addressing this problem. Therefore, to address the risk of reverse causation, we also measured the impact of LWOP on violent crime using “ Δ Violent crime,” which is the change in “Violent crime” from the year in which the LWOP variables are measured to the following calendar year (e.g., the change in “Violent crime” from 2016 to 2017). This variable will assess whether states

that use LWOP more often experience larger decreases in violent crime regardless of what their starting violent crime rate was.

3.3 | Control variables

We have included several control variables, each of which, as we describe in the Appendix, has been theorized to, or empirically shown to, affect violent crime rates and has also previously been used to analyze the death penalty's deterrent effects. We have organized them into three major categories, all of which are measured at the state level: (1) demographic variables, (2) economic variables, and (3) criminal justice variables. The demographic variables are the percentage of the noninstitutionalized population age 25 and older, which possesses a bachelor's degree or higher ("College"), the percentage of the population aged 18–24 ("18–24"), the percentage of the population aged 25–34 ("25–34"), and the percentage of the population that does not identify as White alone ("Non-White"). Each of these measures comes from the U.S. Census Bureau. The economic variables are the percentage of the noninstitutionalized civilian population that is 16 years old or older, which is employed in a civilian job ("Employed") and the percentage of families in each state living below the poverty line ("Poverty"). These economic data come from the Bureau of Labor Statistics (BLS). The criminal justice variables come from various governmental sources. The number of sworn police officers per 1,000 persons in the population ("Police") comes from the FBI's online *Crime Data Explorer*. The percentage of all suicides carried out using a firearm ("Firearm suicide") is based on data from the Center for Disease Control and Prevention's (CDC) *National Center for Health Statistics Compressed Mortality File*. This variable is included because there is no reliable estimate of the percentage of households with a firearm at the state level after 2004, but the firearm suicide-to-suicide ratio has been shown to be a reliable proxy for the percentage of households with a firearm (Azrael, Cook, & Miller, 2004; National Research Council, 2005) and has been used extensively in the academic literature examining the relationship between firearm ownership and violent crime (Moore & Bergner, 2016; RAND, 2018). The two policy variables, presence of a three strikes law ("Three strikes law") and presence of a shall issue or right-to-carry (RTC) gun law ("Right to carry law"), are both dummy variables that indicate whether the state passed a recognizable version of the respective law. Both variables are coded such that the presence of the law equals 1 and the absence of the law equals 0. These data come from the National Criminal Justice Reference Service (NCJRS) and the authors' review of the Nexis legal database. State legislative websites were used to cross-check these sources. Finally, the exposure variable "Total population" comes from the FBI's *Uniform Crime Report*.

All control variables are measured in the same year as the LWOP variables with the exception of "College," which is measured in the year prior to the LWOP variables because no measure of the percentage of each state's population that had attained a college degree or higher could be found for 1992. Data are available, however, for each year prior to each of the five time periods. Therefore, "College" is measured at $t1 - 1$ relative to the measures of LWOP for each time period included in the analysis. Further details on how we selected our control variables are provided in the Appendix, and the summary statistics for all variables used in the analysis are presented in Table A2 in the Appendix.

3.4 | Analytic strategy

Because the data are panel data (otherwise known as "pooled time-series data"), we use a fixed-effects model to account for both time-invariant, state-level effects and time dependent, national-level trends (Allison, 2009; Rabe-Hesketh & Skrondal, 2012). Furthermore, because violent crime rates are distributed as counts, we first determine whether the Poisson or negative binomial distribution better

captures the shape of the data (Long, 1997; Long & Freese, 2006; Osgood, 2000). Chi-square analyses of the log-likelihood statistics indicate that there is overdispersion in the dependent variable, and thus, the negative binomial model better accounts for the data structure (Allison, 2009; Long & Freese, 2006). For technical details on how we performed the negative binomial analysis, refer to the Appendix.

As a result of the potential for reverse causation between our measures of LWOP and violent crime rates, we additionally analyze the change in violent crime rates, “ Δ Violent crime.” Unlike violent crime rates, which are distributed as counts, the distribution of the change in the violent crime rate is nearly normal. Therefore, we analyze “ Δ Violent crime” using ordinary least-squares (OLS) regression, with robust standard errors. We report both the unstandardized coefficients, b , and the standardized coefficients, β , so that unit-change effects and magnitude differences, respectively, can both be easily determined.

Finally, because it has been suggested that punishments are likely to have diminishing returns as they are expanded to less and less serious offenders (Levitt, 2004; Roeder et al., 2015), we re-ran all of our models with the square of the four LWOP measures included to account for the possibility that LWOP’s effect may decline as it gets used more. In essence, as society tends to punish the most serious offenders first, each additional person sentenced to LWOP is less likely than the previous person to be a re-offending risk, which means that each additional person ensnared by the punishment is likely to have less effect on the violent crime rate than the previous person. Including the squared terms allows us to account for this possibility. To test for the joint significance of the linear and quadratic effects of the LWOP variables, Wald tests for joint significance were run. A significant result indicates that the variables are jointly significant in the model being examined. Finally, to better understand what a significant joint effect means, margin plots were constructed to help visualize the change in the dependent variables being predicted according to the joint effects. Because the LWOP variables and their squares are highly correlated, we mean centered all the continuous variables in these quadratic analyses, thus, reducing the correlation between the LWOP measure and its quadratic form. To keep all of our analyses consistent, we mean centered the continuous variables in the models without the quadratic terms as well. These mean-centered analyses are what we report in the Results section.

4 | RESULTS

4.1 | Linear effect of LWOP on violent crime

We begin by analyzing the linear effect of LWOP on “Violent crime.” As discussed in the Method section, there are several plausible ways of calculating the size of the LWOP population, each of which suggests a different mechanism connecting the use of LWOP to reductions in violent crime: the raw number of prisoners serving LWOP or the proportion of all residents, all prisoners, or all lifers in a state who are serving LWOP. Therefore, we report the findings of models measuring LWOP using all four methods in Table 1. There is a bit of inconsistency in these results as the LWOP variable only achieves statistical significance in two models: Total LWOP population and LWOP as a percentage of all prisoners. Taken together these results suggest that using LWOP may produce a small reduction in violent crime but that it is no more effective than life with parole.

Recall that the “Total LWOP” variable is measured in *thousands*, so Model 1 in Table 1 indicates that for every *thousand* people serving LWOP, we would expect “Violent crime” to decline by 12%, which means that each person sentenced to LWOP should reduce “Violent crime” by 0.012%. To help put this result into perspective, in 2016 (the most recent data), only 14 states even had 1,000 prisoners serving LWOP, and the median LWOP population in 2016 was 337. So even by the most recent count,

TABLE 1 Negative binomial regression models predicting violent crime rate 1993 to 2017

Variable	Model 1		Model 2		Model 3		Model 4	
	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR
Life Without Parole (LWOP)								
Absolute Deterrence								
Total LWOP (thousands)	-.128*** (.027)	.880***						
LWOP per capita			-.032 (.039)	.969				
Marginal Deterrence								
LWOP per life sentence					.271 [†] (.163)	1.311 [†]		
%LWOP prisoners							-.032* (.014)	.969*
Demographic Controls								
College	-.003 (.013)	.997	-.002 (.015)	.998	-.002 (.014)	.998	-.001 (.015)	.999
18–24	.055 (.045)	1.057	.052 (.049)	1.053	.071 (.047)	1.073	.044 (.047)	1.045
25–34	.014 (.023)	1.014	.018 (.023)	1.018	.026 (.022)	1.026	.011 (.023)	1.011
Non-White	-.013 (.009)	.987	-.013 (.009)	.987	-.014 (.010)	.986	-.012 (.009)	.988
Economic Controls								
Poverty	.007 (.014)	1.007	.011 (.015)	1.011	.009 (.016)	1.009	.015 (.015)	1.016
Employed	.022 (.013)	1.022	.020 (.014)	1.020	.021 (.014)	1.022	.021 (.014)	1.021
Criminal Justice Controls								
Police per capita	.150* (.073)	1.161*	.160* (.077)	1.173*	.164* (.078)	1.178*	.153* (.074)	1.166*
Firearm suicide	.009 [†] (.005)	1.009 [†]	.008 (.005)	1.008	.006 (.005)	1.006	.010 [†] (.005)	1.010 [†]
Three strikes law	-.067 (.069)	.935	-.127 [†] (.069)	.880 [†]	-.147* (.070)	.863*	-.105 (.074)	.901
Right to carry law	-.101 (.076)	.904	-.069 (.082)	.933	-.061 (.080)	.940	-.063 (.077)	.939
Constants								
y-intercept	-2.085*** (.210)	.124***	-2.151*** (.233)	.116***	-2.133*** (.226)	.118***	-2.179*** (.225)	.113***
ln Total population	1	1	1	1	1	1	1	1

(Continues)

TABLE 1 (Continued)

Variable	Model 1		Model 2		Model 3		Model 4	
	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR
Observations								
State years	244		244		241		244	
α	.029*** (.003)		.033*** (.003)		.032*** (.003)		.032*** (.003)	
Log likelihood	−1345.896		−1357.269		−1339.027		−1355.141	

Notes. Negative binomial regression coefficient estimates and incidence-rate ratios (IRRs) are reported. Standard errors of the coefficients are in parentheses and are estimated using the outer product of the gradient vectors (OPG) option in Stata 15, as suggested by Allison (2009). We include 49 state dummy variables, not shown, to control for time-invariant, state-level characteristics and 4 year dummy variables, not shown, to control for national-level time trends. All independent variables are measured at *t minus* 1 relative to the dependent variable, except for College, which is measured at *t minus* 2 relative to the dependent variable. All continuous independent variables are mean centered.

[†]*p* ≤ .10. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001 (two-tailed tests).

the median state would need to increase its LWOP population by 297% to achieve a 12% reduction in “Violent crime.” In the entire data set, the state year with the median number of people serving LWOP has only 189 individuals serving LWOP, which means the median state year would need a 529% increase in its LWOP population to achieve the same 12% decline. Viewed in another way, as Table A2 in the Appendix shows, the median violent crime rate excluding rape in our study was 340.6 per 100,000 persons, so adding 1,000 people to the LWOP population in the state with the median “Violent crime” would reduce “Violent crime” by 40.9 violent crimes per 100,000 population. In Florida in 1992, the state year with the highest “Violent crime” in the following year (1,152.2 per 100,000), a 12% reduction in “Violent crime” would result in 138.3 fewer violent crimes (excluding rape) per 100,000 population.

Model 2 in Table 1 (LWOP per capita) fails to achieve statistical significance, whereas Model 3 (proportion of lifers serving LWOP), which is designed to measure the effect of LWOP beyond life with parole, is only marginally statistically significant and the magnitude of the effect is *positive*, meaning that if this effect is real, increasing the proportion of lifers who are ineligible for parole *increases* the violent crime rate (excluding rape). Lastly, Model 4 in Table 1 shows that increasing the percentage of prisoners serving LWOP has a small but statistically significant impact on violent offending. For every percentage point increase in the percentage of prisoners sentenced to LWOP, “Violent crime” is expected to decline by 3.1%. Again, to put this in perspective, the state year with the median percentage of inmates serving LWOP has 1.45% of its prisoners serving LWOP, which means that increasing the percentage of prisoners in the median state year serving LWOP by one percentage point to 2.45% would represent a 69.0% increase in the size of the LWOP population to achieve a 3.1% reduction in “Violent crime.” Even with the growth of LWOP since 1992, the state with the median percentage of inmates serving LWOP in 2016 had only 2.49% of its inmates serving LWOP, so a one percentage point increase in this state would require increasing the LWOP population by 40.2%.

Although these models produce inconsistent results, it must be remembered that each one is measuring a slightly different phenomenon. Looking holistically at the results of these models suggests that LWOP may produce small reductions in violent offending but that this effect is being produced by the volume of LWOP rather than by its rate of use. Furthermore, the results suggest that LWOP might be slightly more effective than a term of years but that any deterrent or incapacitating power associated with LWOP can be better achieved using life-with-parole sentences. That is, even if increasing the number or percentage of inmates serving LWOP exerts some modest downward pressure on violent offending, the proportion of lifers serving LWOP has a marginally significant but *positive* impact

on “Violent crime,” which means that any reduction in “Violent crime” attributable to increasing the number or percentage of inmates serving LWOP could also be achieved (and could potentially be better achieved) by simply increasing the number or percentage of prisoners who are sentenced to life with parole. That is, any crime reductions attributable to LWOP are being accomplished by the “life” part of the sentence, not by the “without parole” part.

4.2 | Linear effect of LWOP on changes in violent crime

As a result of the possibility that violent crime rates within a state may be serially correlated with prior years’ violent crime rates and therefore run the risk of reverse causation (that is, perhaps violent crime rates are driving LWOP populations rather than the other way around), we decided to re-run the analysis using “ Δ Violent crime,” the change in the violent crime rate (excluding rape) between the year in which the LWOP population was measured and the following calendar year, as the dependent variable to determine whether LWOP has an impact on *changes* in the violent crime rate. Obviously, the change in violent crime from t_1 to t_2 cannot impact prison populations at t_1 , so this variable eliminates the risk of reverse causation. As the results reported in Table 2 indicate, in all four models the LWOP variable fails to achieve statistical significance, which suggests that LWOP has no impact on changes in the violent crime rate (excluding rape).

4.3 | Quadratic effect of lwop on violent crime

Yet, it is possible that these results do not capture the full picture of what is going on and underestimate the impact of LWOP because, as discussed, it is possible that the crime-reducing power of a punishment diminishes as the punishment expands to ensnare less and less serious offenders. To account for this possibility, we re-ran all of our models with the addition of the square of our LWOP measures. The results of these models for estimating LWOP’s effect on “Violent crime” are reported in Table 3, and the results of the Wald tests for joint significance are reported in Table 4. The results of the Wald tests indicate that, consistent with the models without the quadratic term, the Total LWOP population (in thousands) and the percentage of all prisoners serving LWOP are statistically significant but that the other two measures of LWOP are not. To illustrate how the two significant variables are related to “Violent crime” in these models, we have graphed the predicted quadratic effect of Total LWOP and percentage of LWOP prisoners in Figures 1 and 2, respectively.

As hypothesized, Figure 1 demonstrates that increasing the size of the LWOP population leads to diminishing returns. Using the results displayed in Model 9 of Table 3, we can see that at the mean, increasing the number of persons serving LWOP by 1,000 will produce a 10.6% reduction in “Violent crime” and that for every additional 1,000 people sentenced to LWOP, this impact declines by 0.2 percentage points. Thus, increasing the LWOP population from 1,000 to 2,000 individuals beyond the mean will reduce “Violent crime” by 10.4%. In our data set, the mean number of individuals serving LWOP is only 700, so increasing the LWOP population by 1,000 individuals in the mean state year would represent a 143% increase in the size of the LWOP population for that state year. As the confidence intervals displayed in Figure 1 demonstrate, a state would need to increase its LWOP population by nearly 2,000 individuals above the mean to have a statistically perceptible reduction in “Violent crime” relative to having the mean number of persons serving LWOP. As a result of diminishing returns, however, to achieve another statistically perceptible reduction, the state would then need to increase its LWOP population from 2,000 to nearly 6,000 above the mean, which is higher than the LWOP population of any state other than Florida in any year in our data set.

TABLE 2 OLS regression models predicting change in violent crime rates

Variable	Model 5		Model 6		Model 7		Model 8	
	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β
Life Without Parole (LWOP)								
Absolute Deterrence								
Total LWOP (thousands)	-4.073 (3.689)	-.187						
LWOP per capita			-7.002 (5.103)	-.378				
Marginal Deterrence								
LWOP per life sentence					-.976 (18.159)	-.010		
%LWOP prisoners							.131 (1.782)	.013
Demographic Controls								
College	2.360 (1.562)	.489	2.095 (1.619)	.434	2.334 (1.549)	.480	2.390 (1.565)	.495
18–24	2.118 (5.035)	.054	.711 (4.668)	.018	5.353 (5.218)	.131	2.339 (4.968)	.060
25–34	7.933* (3.095)	.426*	7.778* (3.127)	.417*	8.715** (3.155)	.464**	7.977* (3.174)	.428*
Non-White	-1.839 (1.654)	-.764	-1.855 (1.651)	-.771	-2.244 (1.672)	-.930	-1.879 (1.657)	-.781
Population (thousands)	.005* (.002)	1.215*	.004* (.002)	.915*	.004† (.002)	.925†	.004† (.002)	.919†
Economic Controls								
Poverty	1.632 (2.345)	.188	1.699 (2.293)	.196	1.799 (2.345)	.206	1.617 (2.354)	.187
Employed	1.834 (2.157)	.279	1.725 (2.123)	.262	1.830 (2.112)	.278	1.728 (2.177)	.263
Criminal Justice Controls								
Police per capita	7.335 (10.472)	.148	7.122 (10.160)	.144	7.224 (10.329)	.145	7.647 (10.510)	.154
Firearm suicide	.168 (.700)	.073	.305 (.652)	.133	-.170 (.680)	-.074	.106 (.698)	.046
Three strikes law	-13.249 (10.449)	-.227	-12.515 (10.533)	-.215	-14.305 (10.680)	-.244	-14.829 (10.937)	-.254
Right to carry law	-11.608 (8.042)	-.188	-10.664 (8.177)	-.172	-9.668 (8.224)	-.156	-10.952 (8.349)	-.177
Constant								
y-intercept	42.309 (33.056)		47.632 (33.720)		49.596 (33.669)		38.200 (33.130)	

(Continues)

TABLE 2 (Continued)

Variable	Model 5		Model 6		Model 7		Model 8	
	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β
Observations								
State years	244		244		241		244	
<i>r</i> ²	.363		.371		.376		.360	

Notes. All independent variables are measured at t_1 , except for College, which is measured at t_1 minus 1. All continuous independent variables are mean centered. Standard errors are in parentheses and are estimated using robust standard errors. We include 49 state dummy variables, not shown, to control for time-invariant, state-level characteristics and 4 year dummy variables, not shown, to control for national-level time trends.

† $p \leq .10$. * $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$ (two-tailed tests)

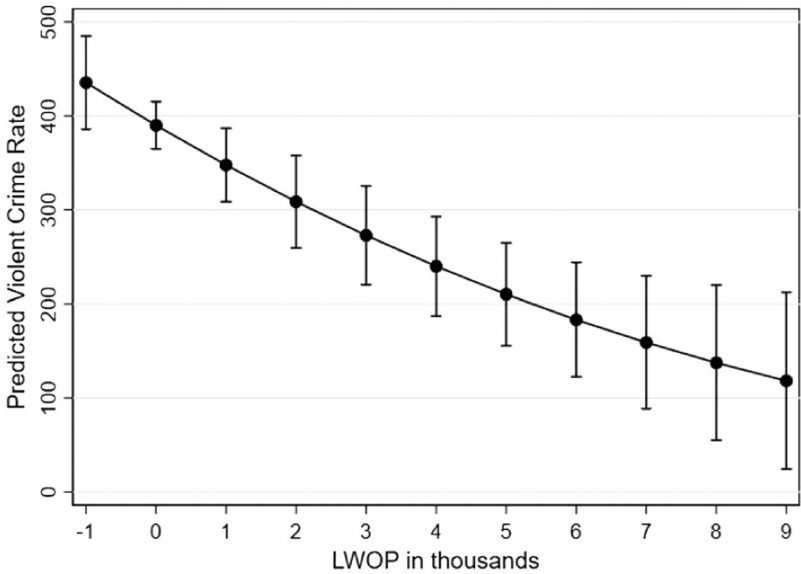


FIGURE 1 Predicted Violent Crime Rate based on Joint Effects of Total LWOP and Total LWOP²

Notes: Predictive margins with 95 percent confidence intervals. Zero on the x-axis equals the mean, since the measure of LWOP is mean centered.

Model 12 in Table 3 indicates a perplexing result. As Figure 2 illustrates, it appears that increasing the percentage of prisoners serving LWOP initially *increases* the amount of violent crime but that this effect diminishes and eventually leads to a decline in “Violent crime” that gets *more* pronounced (rather than less) as the percentage of inmates serving LWOP increases.⁵ That is, rather than having diminishing returns, this variable is predicted to have increasing returns. Using the results in Table 3, we see that at the mean, increasing the amount of prisoners serving LWOP by one percentage point is predicted to lead to a 1.0% decline in “Violent crime,” but as the mean is past the point at which the slope turns negative, this effect is predicted to grow by 0.5 percentage points for each percentage point increase in the percentage of prisoners serving LWOP.

To fully understand what this result is indicating, these numbers need to be placed into the proper context. As Figure 2 illustrates and the vertex reported in Table 4 demonstrates, increasing the percentage of inmates serving LWOP *increases* violent offending until 1.08 percentage points below the mean. As shown in Table A2 in the Appendix, the median state year in our sample has only 1.45% of

TABLE 3 Negative binomial regression models predicting violent crime rate 1993 to 2017 with quadratic lwop measures

Variable	Model 9		Model 10		Model 11		Model 12	
	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR
Life Without Parole (LWOP)								
Absolute Deterrence								
Total LWOP (thousands)	-.112*	.894*						
	(.050)							
Total LWOP (thousands) ²	-.002	.998						
	(.009)							
LWOP per capita			-.021	.979				
			(.053)					
LWOP per capita ²			-.002	.998				
			(.009)					
Marginal Deterrence								
LWOP per life sentence					.286	1.331		
					(.216)			
LWOP per life sentence ²					-.041	.960		
					(.313)			
%LWOP prisoners							-.010	.990
							(.021)	
%LWOP prisoners ²							-.005	.995
							(.004)	
Demographic Controls								
College	-.003	.997	-.003	.997	-.002	.998	-.001	.999
	(.013)		(.015)		(.015)		(.014)	
18–24	.057	1.059	.052	1.054	.071	1.074	.046	1.047
	(.045)		(.049)		(.047)		(.047)	
25–34	.015	1.015	.018	1.019	.026	1.026	.012	1.012
	(.022)		(.023)		(.022)		(.023)	
Non-White	-.013	.987	-.013	.987	-.014	.986	-.013	.987
	(.009)		(.009)		(.010)		(.009)	
Economic Controls								
Poverty	.007	1.007	.011	1.011	.009	1.009	.016	1.016
	(.014)		(.015)		(.016)		(.015)	
Employed	.022 [†]	1.022 [†]	.020	1.020	.021	1.022	.024 [†]	1.024 [†]
	(.013)		(.014)		(.014)		(.014)	
Criminal Justice Controls								
Police per capita	.150*	1.162*	.160*	1.174*	.164*	1.178*	.143 [†]	1.154 [†]
	(.073)		(.077)		(.079)		(.076)	
Firearm suicide	.009 [†]	1.009 [†]	.008	1.008	.006	1.006	.010 [†]	1.010 [†]
	(.005)		(.005)		(.005)		(.005)	

(Continues)

TABLE 3 (Continued)

Variable	Model 9		Model 10		Model 11		Model 12	
	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR	Coefficient	IRR
Three strikes law	−.070 (.069)	.932	−.128 [†] (.069)	.880 [†]	−.149* (.076)	.862*	−.099 (.075)	.905
Right to carry law	−.102 (.076)	.903	−.069 (.082)	.934	−.062 (.081)	.940	−.071 (.080)	.931
Constants								
y-intercept	−2.088*** (.209)	.124***	−2.156*** (.234)	.116***	−2.130*** (.226)	.119***	−2.165*** (.220)	.115***
In Total population	1	1	1	1	1	1	1	1
Observations								
State years	244		244		241		244	
α	.029*** (.003)		.033*** (.004)		.032*** (.003)		.032*** (.004)	
Log likelihood	−1345.837		−1357.202		−1339.014		−1353.346	

Notes. Negative binomial regression coefficient estimates and incidence-rate ratios (IRRs) are reported. Standard errors of the coefficients are in parentheses and are estimated using the outer product of the gradient vectors (OPG) option in Stata 15, as suggested by Allison (2009). We include 49 state dummy variables, not shown, to control for time-invariant, state-level characteristics and 4 year dummy variables, not shown, to control for national-level time trends. All independent variables are measured at *t minus* 1 relative to the dependent variable, except for College, which is measured at *t minus* 2 relative to the dependent variable. All continuous independent variables are mean centered

[†]*p* ≤ .10. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001 (two-tailed tests)

TABLE 4 Postnegative binomial regression diagnostics for Joint Quadratic Effects

Joint Variable	Wald Test for Joint Significance			Estimated Vertex		
	d.f.	χ ²	<i>p</i>	Coefficient	SE	<i>p</i>
Total LWOP (thousands) and						
Total LWOP (thousands) ²	2	18.79	.0001	−25.165	107.457	.815
LWOP per capita and						
LWOP per capita ²	2	.53	.7657			
LWOP per life sentence and						
LWOP per life sentence ²	2	2.75	.2523			
%LWOP prisoners and						
%LWOP prisoners ²	2	6.01	.0496	−1.079	2.998	.719

its inmates serving LWOP, which is 1.09 percentage points below the mean of 2.54. Therefore, for the majority of state years in our data set, increasing the percentage of inmates serving LWOP would lead to an initial *increase* in “Violent crime.” Furthermore, even for many of those states on the downside of the curve, these reductions in “Violent crime” would be canceled out by the predicted increases in “Violent crime” caused by earlier expansions of LWOP. Thus, the same reductions would be predicted to occur if their LWOP population was *lowered*.

Consistent with the models without quadratic effects, these results suggest that the size of the LWOP population rather than the LWOP per-capita rate is the phenomenon responsible for any reductions

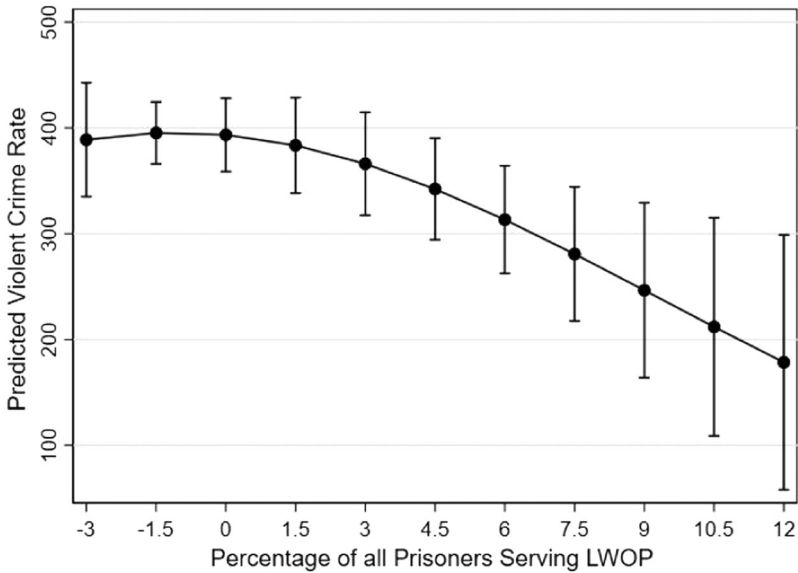


FIGURE 2 Predicted Violent Crime Rate based on Joint Effects of %LWOP prisoners and %LWOP prisoners²
Notes: Predictive margins with 95 percent confidence intervals. Zero on the x-axis equals the mean, since the measure of LWOP is mean centered.

in crime attributable to LWOP and that increasing the proportion of prisoners serving LWOP may also produce some modest reductions in violent offending. Again, however, it is worth noting that the nonsignificance of the joint effect of the variables measuring the marginal impact of increasing the proportion of lifers serving LWOP seen in Table 4 suggests that even if LWOP does achieve some modest reductions in violent offending, it is no more effective than life with parole. Therefore, both the models with and without the quadratic term suggest that even though “life” might be lowering crime rates, “without parole” is not.

4.4 | Quadratic effect of lwop on changes in violent crime

Again, in an effort to address the risk of serial correlation and reverse causation, we re-ran these models using “Δ Violent crime” as the dependent variable. Table 5 reports the results of these analyses, and Table 6 reports the results of the Wald tests for joint significance. In this analysis, as in the analysis without the quadratic term, the variables measuring the number of persons serving LWOP and the variables measuring the proportion of all lifers serving LWOP are jointly nonsignificant. Unlike the earlier analysis, however, the variables measuring LWOP per capita and the percentage of prisoners serving LWOP are now both jointly significant. Again, to illustrate the predicted quadratic effects of the significant variables, we have graphed the relationship between LWOP per capita and the change in the violent crime rate (excluding rape) in Figure 3 and the relationship between the percentage of inmates serving LWOP and the change in the violent crime rate (excluding rape) in Figure 4. In both cases, the data produced the perplexing conclusion that increasing the LWOP variable produces *increasing*, rather than decreasing, returns.

Turning to the effect of LWOP per capita, it should be reiterated that this variable is measuring the number of persons serving LWOP *per 10,000* residents. Thus, the results reported in Model 14 in Table 5 indicate that for each additional person serving LWOP per 10,000 residents beyond the mean, the crime rate is expected to decline by 2.6 crimes per 100,000 (or 0.26 crimes per 10,000). As the mean

TABLE 5 OLS regression models predicting change in violent crime rates with quadratic LWOP measures

Variable	Model 13		Model 14		Model 15		Model 16	
	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β
Life Without Parole (LWOP)								
Absolute Deterrence								
Total LWOP	.144	.007						
(thousands)	(6.784)							
Total LWOP	-.633	-.145						
(thousands) ²	(.682)							
LWOP per capita			-2.574	-.139				
			(6.739)					
LWOP per capita ²			-.748	-.245				
			(.474)					
Marginal Deterrence								
LWOP per life sentence					25.946	.258		
					(27.916)			
LWOP per life sentence ²					-70.008 [†]	-.300 [†]		
					(40.150)			
%LWOP prisoners							3.374	.325
							(2.983)	
%LWOP prisoners ²							-.687*	-.356*
							(.320)	
Demographic Controls								
College	2.358	.489	1.953	.405	2.275	.468	2.449	.507
	(1.558)		(1.602)		(1.499)		(1.513)	
18–24	2.631	.067	.996	.025	6.048	.147	2.558	.065
	(5.081)		(4.680)		(5.108)		(4.991)	
25–34	8.119**	.436**	7.965*	.428*	8.769**	.467**	8.163*	.438*
	(3.126)		(3.111)		(3.106)		(3.192)	
Non-White	-1.854	-.771	-1.898	-.789	-2.358	-.977	-2.011	-.836
	(1.660)		(1.662)		(1.677)		(1.663)	
Population	.005*	1.238*	.004 [†]	.848 [†]	.004 [†]	.883 [†]	.004 [†]	.896 [†]
(thousands)	(.002)		(.002)		(.002)		(.002)	
Economic Controls								
Poverty	1.655	.191	1.511	.174	1.402	.160	1.675	.193
	(2.354)		(2.314)		(2.323)		(2.343)	
Employed	1.914	.291	1.872	.285	1.950	.296	2.092	.318
	(2.190)		(2.152)		(2.156)		(2.248)	
Criminal Justice Controls								
Police per capita	7.483	.151	7.460	.150	6.487	.130	6.247	.126
	(10.504)		(10.165)		(10.117)		(10.277)	
Firearm suicide	.136	.059	.224	.098	-.144	-.063	.085	.037
	(.699)		(.653)		(.665)		(.694)	

(Continues)

TABLE 5 (Continued)

Variable	Model 13		Model 14		Model 15		Model 16	
	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β	<i>b</i>	β
Three strikes law	-14.003 (10.704)	-.240	-12.436 (10.570)	-.213	-17.887 (11.272)	-.305	-13.997 (10.686)	-.240
Right to carry law	-12.009 (8.179)	-.194	-10.536 (8.104)	-.170	-11.391 (8.461)	-.184	-12.262 (8.383)	-.198
Constant								
y-intercept	41.637 (32.897)		45.054 (33.684)		53.757 (33.562)		39.496 (32.998)	
Observations								
State years	244		244		241		244	
<i>r</i> ²	.365		.376		.390		.373	

Notes. All independent variables are measured at *t*₁, except for College, which is measured at *t*₁*minus* 1. All continuous independent variables are mean centered. Standard errors are in parentheses and are estimated using robust standard errors. We include 49 state dummy variables, not shown, to control for time-invariant, state-level characteristics and 4 year dummy variables, not shown, to control for national-level time trends.

[†]*p* ≤ .10. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001 (two-tailed tests).

TABLE 6 PostOLS regression diagnostics for joint quadratic effects

Joint Variable	Wald Test for Joint Significance			Estimated Vertex		
	d.f.	F	<i>p</i>	Coefficient	SE	<i>p</i>
Total LWOP (thousands) and						
Total LWOP (thousands) ²	2 and 177	2.19	.114			
LWOP per capita and						
LWOP per capita ²	2 and 177	5.52	.005	-1.722	5.438	.752
LWOP per life sentence and						
LWOP per life sentence ²	2 and 174	1.81	.168			
%LWOP prisoners and						
%LWOP prisoners ²	2 and 177	3.53	.031	2.455	1.292	.057

is beyond the vertex, however, as shown in Figure 3, this effect increases by 0.7 crimes per 100,000 for each additional person per 10,000 residents added to LWOP. As Table 6 indicates, the vertex of this effect occurs at 1.72 inmates per 10,000 below the mean, but as the mean is only at 1.10 inmates per 10,000, this means that the slope is negative for *all* observations in our data set and getting more negative. If these predictions are accurate, it would suggest that increasing the proportion of residents who are sentenced to LWOP should lead to a small but increasing decline in the violent crime rate (excluding rape). In essence, at the mean (which is more than double the median), sentencing four additional persons to LWOP is predicted to prevent one nonrape violent crime.

The variables measuring the percentage of inmates serving LWOP demonstrate the same basic pattern, but as can be seen in Figure 4, the point at which the curve turns negative is in a much different location, which leads to vastly different conclusions. Model 16 in Table 5 shows that at the mean, increasing the percentage of inmates who are serving LWOP is predicted to lead to an *increase* in violent crime. At the mean, a one-percentage-point increase in inmates serving LWOP is predicted to increase the violent crime rate (excluding rape) by 3.4 crimes per 100,000. This effect, however, is

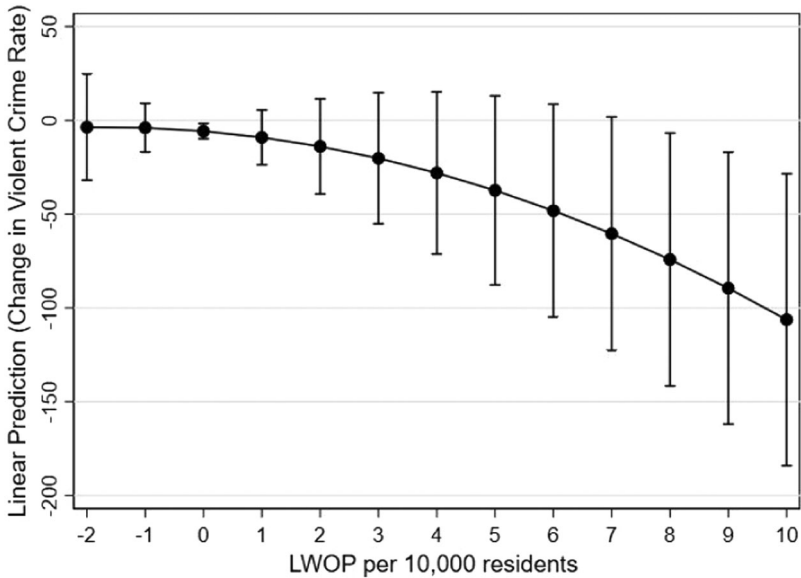


FIGURE 3 Predicted Change in Violent Crime Rate based on Joint Effects of LWOP per capita and LWOP per capita²

Notes: Predictive margins with 95 percent confidence intervals. Zero on the x-axis equals the mean, since the measure of LWOP is mean centered.

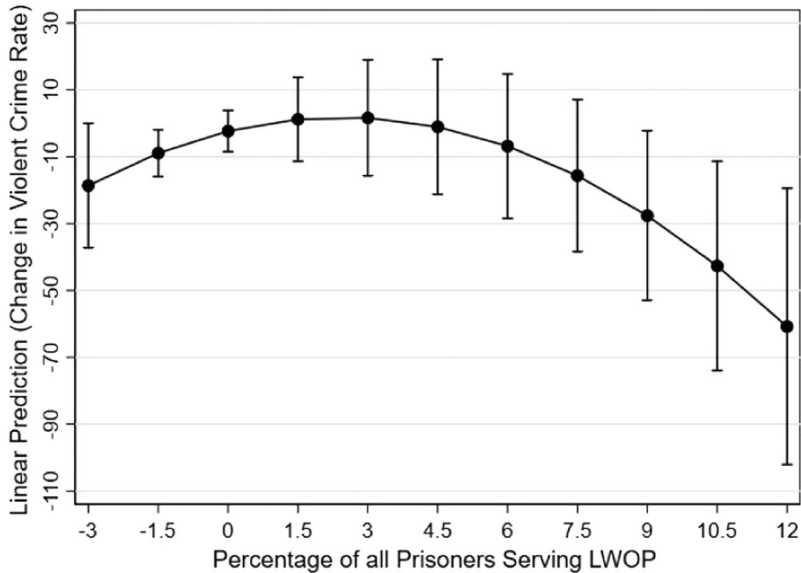


FIGURE 4 Predicted Change in Violent Crime Rate based on Joint Effects of %LWOP prisoners and %LWOP prisoners²

Notes: Predictive margins with 95 percent confidence intervals. Zero on the x-axis equals the mean, since the measure of LWOP is mean centered.

expected to get smaller by 0.7 crimes per 100,000 for each percentage point increase in the percentage of prisoners serving LWOP, which as indicated by the vertex reported in Table 6, leads to the curve reaching zero at 2.46 percentage points *above the mean* and then turning negative at an increasing rate. As mentioned, though, most state years are *below* the mean, so for most state years in our data set, Model 16 predicts that increasing the percentage of prisoners serving LWOP will increase violent crime. Given the mean of 2.54 percent of prisoners serving LWOP, a state would need to have 1 in every 20 inmates (5.0 percent) serving LWOP to be on the downside of this curve. In our entire data set, only 38 state years have at least 5.0 percent of their inmates serving LWOP, and in 2016, only 10 states did. Even for most of those states, however, any reductions in crime they achieved by increasing the percentage of prisoners serving LWOP would likely be canceled out by earlier increases in the percentage of inmates serving LWOP and, thus, could likely also be achieved by *reducing* the percentage of inmates serving LWOP. It is also important to note that although the significance of these two variables in the quadratic models seems inconsistent with the findings in the models without the quadratic terms, the shape of these curves (increasing and then decreasing) are consistent with a finding of no linear effect (that is, an overall slope of close to zero).

5 | POLICY DISCUSSION

Whereas many nations around the world have already outlawed the use of LWOP (Henry, 2012; Nellis, 2013; van Zyl Smit & Appleton, 2019), the use of LWOP has continued unabated in the United States. Although the United States has reduced its use of the death penalty over the last two decades (Death Penalty Information Center, 2018), its use of “death-in-prison” sentences has been expanding at a far faster rate (Henry, 2012). From 1992 to 2016, the number of inmates serving LWOP quadrupled (Nellis, 2017). The United States now has more people sentenced to die in prison than the rest of the world combined (van Zyl Smit & Appleton, 2019). As the use of LWOP continues to expand and the death penalty continues to decline, the public policy debate over LWOP is likely to intensify. Indeed, in 2014, Pope Francis called for its abolition, referring to LWOP as “a hidden death penalty” (Gibson & McKenna, 2014), and in 2019, Pennsylvania’s Lieutenant Governor, John Fetterman, began a campaign to encourage those sentenced to LWOP to apply for commutations in an effort to reduce that state’s LWOP population (Melamed, 2019). As this debate heats up, it is critical that there is high-quality empirical research to help inform it; yet, to date, there has been no empirical assessment of LWOP’s efficacy as a crime-fighting tool. Given the human rights concerns raised by LWOP, the high financial cost of LWOP sentences, and the racially disproportionate impact of LWOP, this is a glaring oversight. Such a dramatic expansion of the state’s power to deprive its citizens of liberty should be rooted in evidence that such a practice is effective.

This study begins filling this gap by analyzing whether states that use LWOP more prolifically experience less violent crime or greater reductions in violent crime. Overall, the results suggest that LWOP may produce modest reductions in violent crime but that it is no more effective than life with parole. Specifically, in the models without a quadratic term, the data indicate that having more people serving LWOP—but not a greater proportion of the population serving LWOP—and that having a greater percentage of inmates serving LWOP are both associated with less violent crime. Neither variable was associated with *changes* in violent crime, however. Importantly, when assessing the *marginal* crime-reducing power of LWOP *relative to* life with parole, we found that the proportion of lifers who are ineligible for parole was not associated with changes in violent crime and was even associated with *higher* rates of violent crime (albeit the relationship was only marginally significant). These findings indicate that to the extent that incarceration can produce lower crime rates, the effect of increasing

sentencing severity maxes out at some point prior to LWOP. Thus, LWOP does not seem to produce any additional crime reduction beyond that which is produced by parole-eligible life sentences (and possibly by other long-term sentences).

Out of a concern that the initial models were failing to account for the potential of diminishing returns, however, we re-ran them with a quadratic term included. The quadratic models examining the relationship between LWOP and violent crime rates seem to confirm the findings of the initial models: Increases in the raw number of people serving LWOP and the percentage of prisoners serving LWOP are both associated with modest reductions in violent crime, but the *rate* at which LWOP is used and the proportion of lifers serving LWOP are not. The quadratic models looking at the relationship between LWOP and changes in violent crime, however, produced different results. Whereas none of the LWOP measures were significant in the initial model, the quadratic model suggests that the LWOP rate (but not its raw volume) and the percentage of prisoners serving LWOP are both associated with reductions in violent crime. Importantly, however, the variable measuring the proportion of lifers who are ineligible for parole was again not significant.

It is worth noting that the results of three of the four quadratic models that achieved statistical significance lead to the questionable conclusion that the states that use LWOP the most and the states that use it the least should have the least violent crime and experience the greatest reductions in violent crime, whereas the states that use LWOP moderately will experience the most violent crime and the smallest reductions in violent crime. The implausibility of this outcome leads us to conclude that the models without the quadratic terms are probably more accurate representations of reality. Finally, and perhaps most importantly, the variable measuring the proportion of lifers who are ineligible for parole failed to achieve traditional levels of statistical significance in any of our models, which indicates that even if LWOP is capable of reducing crime, it does not seem to have any marginal crime-reducing power beyond that which is accomplished by parole-eligible life sentences. The effect size in all of our models also indicates that if the effect of the LWOP to life proportion were real, it would lead to an *increase* rather than to a decrease in violent offending.

These conclusions are in line with what we would expect from the deterrence literature, which indicates that certainty of punishment is more important than its severity and that increasing already lengthy prison terms is not an effective way to fight crime (Nagin, 2013; Travis et al., 2014). They are also in line with what we would expect from the research indicating that both older inmates who are released and paroled lifers have extremely low recidivism rates (Advisory Committee on Geriatric and Seriously Ill Inmates, 2005; Anderson, 2019; California Department of Corrections and Rehabilitation, 2013; Justice Policy Institute, 2018; Mauer et al., 2004; Millemann et al., 2017; Rosenfeld et al., 2005; State of New York Department of Corrections and Community Supervision, 2016; Weisberg et al., 2011). Given LWOP's unique status as a sort of "semantically disguised" death sentence (Villaume, 2005, p. 266) or "death by incarceration" (see Gross, 2019), however, it was not clear whether the wider deterrence literature or the more specific death penalty literature was more applicable to LWOP. Unlike the deterrence literature, the death penalty literature has produced equivocal results and is beset with methodological pitfalls that prevent any academic consensus from forming over its efficacy (Nagin & Pepper, 2012). Because of the relative size and stability of the LWOP population relative to the number of death sentences and executions, LWOP overcomes many of these hurdles and thus offers an opportunity to test the efficacy of "the new death penalty" (Ogletree & Sarat, 2012) with more stable and reliable statistical models. The results seem to confirm the "no marginal deterrence" perspective. That is, even though LWOP sentences might be producing modest reductions in crime, they do not seem to be any more effective than life with parole, and given the existing literature on deterrence and incapacitation, we suspect that the effect of increasing sentencing severity likely maxes out at some point less than life with parole. Therefore, to the extent that LWOP can be conceptualized as a type

of death sentence, our findings seem to support the findings reported in most of the sociological—but not the economic—literature on the death penalty that the death penalty is unlikely to be a more effective deterrent than LWOP or even life with parole—but based on a more stable statistical model than previous tests of the death penalty's deterrent effect.

Thus, when considering the racially disparate impact of LWOP and the tremendous financial and human costs associated with LWOP, LWOP does not seem to be a justifiable crime reduction strategy. As is true with most other aspects of the U.S. justice system, the use of LWOP is not equally distributed throughout society. Slightly less than three fifths of those serving LWOP in 2012 were African American (Nellis, 2013). Overall, in 2016, nearly half of all life sentenced inmates (both with and without parole) were African American, and two thirds were people of color (Nellis, 2017). Even more strikingly, 82.2% of those serving LWOP for a nonviolent offense are non-White, and 65.4% of them are Black (American Civil Liberties Union, 2014). Furthermore, this punishment comes with a hefty price tag. It costs more than \$30,000 per year to incarcerate an inmate, and this cost more than doubles for inmates older than the age of 50 (American Civil Liberties Union, 2012; Erger & Beger, 2002; Henrichson & Delaney, 2012). There are more cost-effective means of reducing crime than locking up people into old age, such as investing in education, job training, and poverty-reduction programs (Beckett & Sasson, 2004). Lastly, LWOP comes at the cost of tremendous human suffering. In fact, as a result of concerns about human rights and respect for human dignity, many nations and international bodies have been debating the acceptability of life and LWOP sentences for decades (van Zyl Smit & Appleton, 2019), and in 2013, the European Court of Human Rights declared LWOP sentences to be a violation of fundamental human rights (*Case of Vinters and Others v. United Kingdom*, 2013). Similarly, many Latin American nations have outlawed LWOP on human rights grounds (Henry, 2012; van Zyl Smit & Appleton, 2019).

The results of this study thus have implications for policy makers in the United States and abroad as the status of LWOP is debated. Over the last quarter century, while many countries have abolished or reduced their use of LWOP, the United States has expanded its use of LWOP exponentially (Henry, 2012; Nellis, 2013, 2017; van Zyl Smit & Appleton, 2019). Yet, until now, there had been no empirical assessment of LWOP's efficacy. Although just a first step, the findings of this study suggest that policy makers should consider dramatically scaling back the use of LWOP unless and until more convincing evidence of the punishment's effectiveness can be produced. Before implementing a policy that is extremely costly to taxpayers, considered by many to be excessively cruel and inhumane, and racially disproportionate in its impact, there should at least be some evidence that the policy is effective. Our study fails to produce any compelling evidence that LWOP is more effective than lesser penalties. Simply put, states that deny a greater percentage of lifers an opportunity for parole do not experience measurably lower violent crime rates or greater reductions in violent crime as a result. As such, our study suggests that LWOP inflicts unnecessary suffering while exacerbating racial inequalities and wasting millions of taxpayer dollars per year.⁶ Additionally, by eliminating an incentive to behave while in prison, LWOP sentences have the potential to increase violence in prisons, thus, endangering inmates and correctional staff. In fact, the limited empirical evidence that exists on this question suggests that although inmates serving LWOP are well behaved and less dangerous relative to those sentenced to short terms, they are slightly more violent than lifers who are eligible for parole and those serving sentences of 30 years or longer (although the differences are small) (Cunningham & Sorensen, 2006; Sorensen & Reidy, 2018, 2019). Coupled with the results of the present study, these findings suggest that policy makers should strongly consider eliminating or greatly reducing the use LWOP sentences.

Of course, crime reduction is not the only justification for LWOP. For example, some death penalty abolitionists have embraced LWOP as a palatable alternative to the death penalty (Barkow, 2012; Kleinstuber et al., 2016; Ogletree & Sarat, 2012; Seeds, 2018), whereas others are interested in LWOP

for retributive purposes (Robinson, 2012). These alternative rationales notwithstanding, LWOP's lack of effectiveness relative to life with parole at lowering crime rates should give policy makers a reason to reconsider the expanded use of LWOP. Although LWOP may sound like an appealing vehicle for reducing the use of the death penalty, LWOP is now being used far more expansively than the death penalty ever was and for crimes that have never been death eligible—including nonviolent crimes and crimes committed by juveniles (Henry, 2012; Kleinstuber et al., 2016; *Montgomery v. Louisiana*, 2016; Nellis, 2017; Ogletree & Sarat, 2012). Moreover, the use of LWOP as an alternative to the death penalty ignores the human rights concerns associated with death-in-prison sentences (Henry, 2012; Kleinstuber et al., 2016; Ogletree & Sarat, 2012; van Zyl Smit & Appleton, 2019).

The retributive value of a punishment, on the other hand, is not something that can be empirically measured as this rests on value judgments. There are, however, also reasons to question the use of LWOP for retributive purposes. For one, because LWOP punishes someone for the rest of their life, it can only be justified on retributive grounds for homicide offenses; yet, as we have already mentioned, LWOP is even being used for nonviolent crimes. Second, a truly retributive sanction would need to consider potential mitigating factors as well, but unlike the death penalty, most states impose LWOP automatically with no consideration of mitigating circumstances for some offenses (Kleinstuber et al., 2016; Nellis, 2010). These mandatory LWOP sentences, especially when used for nonhomicide offenses, disturb the proportionality balance required for retributive or “just desserts” models of punishment (van Zyl Smit & Appleton, 2019). Third, imposing LWOP on such a wide range of offenses and on such a large proportion of offenders trivializes “important differences in the moral blameworthiness among serious cases” and “undermines the criminal law’s moral credibility” (Robinson, 2012, p. 139). As Robinson (2012) explained, from a retributive point of view, punishment should be designed to punish more serious and more blameworthy offenders more severely, but as LWOP is currently imposed in the United States, it treats a wide range of offenders with vastly different levels of culpability and moral blameworthiness the same. Therefore, even from a retributive perspective, LWOP should be limited only to those convicted of capital offenses and only after a sentencing hearing that allows for true consideration of both aggravating and mitigating circumstances. As such, regardless of what rationale one uses to justify the use of LWOP sentences, the evidence suggests that their use should be scaled back dramatically.

6 | LIMITATIONS AND FUTURE RESEARCH

It is important to note, however, that this study should not be considered the definitive word on this matter. It is just the first effort to analyze LWOP empirically. First, it must be pointed out that non-significant findings should not be confused with finding that there is no association. Failure to reject the null hypothesis does not imply that the null hypothesis is true. It simply means we cannot rule out the possibility that LWOP has no impact on violent crime rates beyond that which is accomplished by life with parole. Furthermore, our article only examines the impact of formal LWOP sentences, not virtual life sentences where the prisoner cannot possibly live long enough to become eligible for release (e.g., a 200-year sentence). Unfortunately, as we point out, there is no agreed upon cut-off for how long a sentence must be to qualify as a virtual life sentence, and there are no data on the number of inmates serving virtual life sentences prior to 2016 (Barkow, 2012; Nellis, 2017; van Zyl Smit & Appleton, 2019). Therefore, it is not yet possible to analyze the effect of virtual life sentences, but it might be in the near future. Similarly, as a result of a lack of available state-level data on lengthy prison sentences, we were only able to assess the marginal impact of LWOP relative to life with parole and relative to prison in general, but we could not analyze the impact of LWOP relative to other lengthy

prison sentences. Therefore, although we could demonstrate that LWOP does not seem to be any more effective than life with parole, determining whether life-with-parole sentences are any more effective than a lengthy term of years is beyond the scope of the present analysis.

Another shortcoming of this analysis is that data on LWOP sentences are only available periodically rather than every year. Even though the data do show substantial growth in the use of LWOP at the state level over time, these increases, while dramatic, are orderly, meaning there are no erratic swings in the data and the states remain ranked in approximately the same order from one time period to the next. Given the consistent growth observed in the LWOP measures over time, we suspect that the missing years would not substantively alter the results, but we cannot know this for sure. As a result, the findings of this study need to be interpreted with some caution.

Additionally, there are alternative ways of analyzing the data, specifying the models, and measuring whether crime has been reduced, which could impact the results. For example, our analysis focuses on whether *using* LWOP more often reduces violent crime (via either deterrence or incapacitation), but it is possible that the presence of a LWOP statute or the publicity surrounding LWOP is the thing that reduces crime rather than the actual use of the sanction. In the death penalty literature, for instance, several studies examine the impact of the presence of a death penalty law or the amount of news media coverage of executions rather than (or in addition to) the objective risk of execution (Bailey, 1998; Dezhbakhsh & Shepherd, 2006; Kovandzic et al., 2009; Stolzenberg & D'Alessio, 2004). Although we encourage such an analysis to be performed, as the results would clearly be useful and informative, knowing the effect of a penalty's presence or absence does not indicate how often that sanction should be applied (if at all). Additionally, it is possible that the presence or publicity of the threat of a sanction, even one as severe as LWOP, is not enough to deter unless or until the punishment is imposed. Therefore, even if future research focuses on the passage of LWOP statutes or on media publicity, it also needs to remain focused on the implementation of LWOP sentences. Furthermore, future analyses should consider using different types of statistical models and different control variables as this could potentially lead to different results and thus to a fuller and more nuanced understanding of LWOP's effects.

We strongly believe all of these alternative analyses should be done. As mentioned, the debate over LWOP is likely to intensify, so it is important to develop an extensive and robust body of research on LWOP to help inform both theory and policy. It is only by subjecting the data to multiple tests by different researchers using different plausible models that we can attain a degree of confidence in the findings and hope to achieve scientific consensus and influence lawmakers.

CONFLICT OF INTEREST STATEMENT

The authors confirm that they have no conflict of interest to declare.

ENDNOTES

¹ Although violent crime clearly occurs inside prison as well, these crimes are rarely reported in official crime data (Voorhees, 2014), and incapacitation theory and research are primarily concerned with crimes "outside the prison walls" (Canelo-Cacho, 2002, p. 809) rather than with violence inside prison.

² For example, The Sentencing Project defines a virtual life sentence as a sentence of 50 years or more (Nellis, 2017), the U.S. Sentencing Commission (2015) sets the cut-off at 470 months, and van zyl Smit and Appleton (2019) use 35 years.

³ Given the range of LWOP sentenced prisoners in our data set, which can be seen in Table A2 in the Appendix, it would have been reasonable to measure LWOP as a straight count of people currently serving LWOP sentences. We chose,

however, to represent this variable in thousands so that the magnitude of the coefficient for this variable would appear in the quadratic models we run, thus, making it easier to illustrate the size of this variable's effect to the reader.

⁴ Ideally, we would prefer to use the percentage of inmates serving long prison terms who are serving LWOP rather than the percentage of all inmates as it would better capture the relative impact of LWOP in comparison with a more similarly sentenced set of prisoners. Yet, as mentioned, data on the number of inmates serving lengthy prison terms are only available at the state level in 2016 (Nellis, 2017; van zyl Smit & Appleton, 2019).

⁵ Running the regressions without mean-centering the variables indicates that "%LWOP prisoners" has an IRR (incident-rate ratio) greater than 1, which indicates that increasing the percentage of prisoners serving LWOP increases crime but at a decreasing rate because the squared term is negative. Once the effect becomes 0, LWOP causes crime to decline at an increasing rate.

⁶ At a cost of more than \$68,000 per year to house prisoners older than the age of 50 (American Civil Liberties Union, 2012), every 15 inmates older than the age of 50 released from prison would save \$1 million per year.

⁷ In an effort to get data on the legacy definition of rape for 2017, we requested and received the 2017 Uniform Crime Reports (UCR) master file from the FBI. Because the codebook included with the file was from 1990, however, which is prior to the establishment of the revised definition of rape, we were unable to decipher which variable (if any) represented the legacy rape definition.

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APPENDIX A: FURTHER DETAILS ON MODEL SPECIFICATION

Dependent Variable Selection

We use violent crime rates as the dependent variable for two reasons. First, from a statistical perspective, violent crime rates are far more stable than homicide rates. As mentioned in the main text, the standard deviation for homicide rates is large, which makes it difficult to distinguish between signal and noise and leads to highly erratic and unstable statistical estimates (Katz et al., 2003). Second, for the following three reasons, we see no logical reason to disaggregate homicide and violent crime rates: (1) Disaggregating violent crime from homicide assumes a concordance between the crime committed and the offender's intent that simply does not exist. Many aggravated assaults are the result of attempted murders in which the victim survived, and many homicides are the result of other felonies (such as robbery) that went wrong and in which death was never intended. In fact, interviews with incarcerated persons who fired a gun during their offense reveal that most did not intend to fire their gun prior to the moment they fired and did not shoot with the intention to kill (Wright & Rossi, 1986, pp. 92–93), and research by Zimring (1968) suggests that most homicides result from ambiguously motivated attacks rather than from a single-minded intention to kill. (2) Even if there was concordance between outcome and intent, there is little reason to assume potential criminals are aware of the nuances in criminal law that make some offenses eligible for LWOP and other similar offenses not. (3) Unlike the death penalty, LWOP is an available sentencing option for nonhomicide offenses, so it seems unlikely that any deterrent or incapacitating power that it possesses would be restricted to homicides.

We have chosen to exclude rape from our measure of violent crime rates for two reasons. First, rape is, by far, the most underreported violent crime. Whereas most aggravated assault and robbery victims report their victimization to police, less than a quarter of rape survivors identified in the National Crime Victimization Survey (NCVS) indicated that they reported the crime to police (Morgan & Kena, 2018, p. 7), and according to the National Intimate Partner and Sexual Violence Survey, the number of annual rape victims is nearly eight times greater than that reported by the NCVS (Breiding et al., 2014; Truman & Planty, 2012). Therefore, the number of reported rapes per state is extremely unreliable and subject to a high degree of randomness and measurement error. Second, beginning in 2013, the FBI revised its definition of rape. Although for 2013–2016, they continued to report the rate and number of rapes using both the old definition (known as the “legacy” definition) and the new definition (known as the “revised” definition) at the state level, in 2017, the FBI stopped releasing data on the number or rate of rapes at the state level using the legacy definition. Therefore, to ensure that we used a consistent measure of the violent crime rate throughout the sample period of our study, we were forced to exclude the rape rate from our calculation of the violent crime rate.⁷

Control Variable Selection

We selected our control variables because they are standard control variables used in macro-level analyses of crime rates—especially in studies of the death penalty. The age variables were chosen because, as we discussed in the literature review in the main text, there is a strong correlation between age and crime with teens and young adults being disproportionately involved in criminal offending and involvement in crime declining precipitously after one's mid-20s. Thus, the proportion of the population in the high-crime age range of 18–24 and the immediately older age range of 25–34, when people are beginning the desistance process, may impact the violent crime rate independent of any policy decisions. Additionally, although researchers do not agree on the reasons for this finding, UCR data consistently indicate that criminal offending is not evenly distributed among racial groups (Rosich, 2007; Schleiden et al., 2019; Siegel, 2015). Therefore, nearly all death penalty studies include some

variable that measures racial composition (Dezhbakhsh & Shepherd, 2006; Dezhbakhsh, Rubin, & Shepherd, 2003; Fagan, Zimring, & Geller, 2006; Katz et al., 2003; Shepherd, 2005; Zimmerman, 2004, 2006). To be consistent with those studies and to avoid misspecifying our models, we also control for racial composition by using percent non-White. Furthermore, we have included the proportion of the population with a college degree because, in addition to appearing in the death penalty literature (Kovandzic et al., 2009), increasing education levels have been empirically associated with reductions in violent crime (Groot & van den Brink, 2010; Lochner, 2004).

The poverty rate is included as a control variable because, as Hipp and Yates (2011, p. 956) pointed out, “[O]ne bedrock conclusion [across the field of criminology] is that the presence of more poverty is associated with more crime” (see also Hsieh & Pugh, 1993). We have decided to include a measure of employment because the link between unemployment and crime has been commonly examined in the academic literature, albeit with mixed results (Aaltonen, MacDonald, Martikainen, & Kivivuori, 2013; Chiricos, 1987). We have also decided to include the employment rate rather than the unemployment rate as this is a more accurate measure of the proportion of the population that is employed because the unemployment rate only includes people who are actively looking for work as unemployed (Bureau of Labor Statistics, 2015).

The number of police officers per capita is included because, as we discussed in the literature review, increasing police resources has been linked to reductions in crime. We chose to include a dummy variable measuring the presence or absence of a “three strikes” law because, as discussed in the literature review, there is a body of research linking these laws to changes in violent crime rates. We have also included the measures of firearm laws and presence because there is a large body of research analyzing the effect of gun control policies and of the presence of firearms on violent crime (National Research Council, 2005; Santaella-Tenorio, Cerdá, Villaveces, & Galea, 2016). The literature on RTC laws has produced inconsistent conclusions. For example, Lott and Mustard (1997) found that RTC laws reduce violent crime, but re-analysis of their data concluded that RTC laws have no effect on violent crime (Black & Nagin, 1998). More recently, Cook and Donohue (2017) and Donohue, Aneja, and Weber (2019) concluded that RTC laws *increase* violent crime rates. Therefore, although we are not certain what effect (if any) to expect from RTC laws, to avoid underspecifying our model, like Kovandzic et al.’s (2009) test of the death penalty’s deterrent effect, we chose to err on the side of caution and include RTC laws as a control variable.

As discussed in the Method section, we include the “firearm suicide” variable as a proxy for the firearm ownership rate. The relationship between gun prevalence and violent crime is a controversial issue in American society, and the empirical literature reflects that uncertainty. This has been a heavily researched question, however, with most studies concluding that increases in firearm ownership are associated with higher rates of violent crime (Moore & Bergner, 2016; National Research Council, 2005; RAND, 2018). Although it cannot be determined whether this relationship is causal, the likely presence of this relationship suggests that we need to include a measure of firearm prevalence as a control variable. Out of a concern that the RTC and “firearm suicide” variables could act as confounds to each other as both could be indicative of an increased prevalence of firearms, we did extensive testing to ensure that this was not the case. The results are not reported here but are available upon request.

Although often included in models of violent crime, we chose to omit a measure of population density because it is not clear what this variable is capturing at the state level as states that have both large urban and expansive rural counties might end up with a middling overall population density that approximates a more suburban state. Furthermore, the population of the nation has increased over time, which means that including a measure of population density might really be capturing a time rather than a population density trend. A more appropriate measure would be the percentage of the population living in urban areas, but we could not use this variable because the U.S. Census Bureau ceased reporting this measure in 2012. Many analyses of crime and the death penalty also include a

TABLE A1 Correlation coefficients between LWOP and death penalty measures

Variable	Life Without Parole (LWOP)			
	Total	Per Capita	Per Life Sentence	Percent Prisoners
Death Penalty				
Total				
Sentences	.282***	.033	-.129	-.008
Population	.589***	.167**	-.046	.134*
Executions	.013	-.026	-.132*	-.092
Per Capita				
Sentences	.028	.107	-.073	.038
Population	.321***	.385***	.027	.239***
Executions	-.026	.077	-.009	-.009
Percent of Life and				
Death Sentence				
Sentences	-.065	-.061	-.072	-.089
Population	.024	-.005	-.058	-.087
Executions	-.065	-.027	-.053	-.076
Percent of Prisoners				
Sentences	-.019	.006	-.108	-.006
Population	.262***	.217***	-.042	.160
Executions	-.048	.012	-.056	-.054

* $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$ (two-tailed tests).

measure of income (Dezhbakhsh & Shepherd, 2006; Dezhbakhsh et al., 2003; Enter Katz et al., 2003; Kovandzic et al., 2009; Shepherd, 2005; Wright et al., 1999; Zimmerman, 2006); however, in our data set, this variable was highly correlated with "Poverty" and "College," so to avoid multicollinearity issues, we omitted this variable from the model.

As a result of the myriad concerns with measuring the death penalty noted earlier, we have also chosen not to include any measures of the death penalty as control variables in our model. As many states have been replacing the death penalty with LWOP, however, we were concerned that there may be an inverse relationship between a state's use of LWOP and its use of the death penalty that might mask LWOP's crime-reducing power (that is, the effects of increasing the use of LWOP and reducing the use of the death penalty might cancel each other out). Therefore, we ran a series of correlations between our measures of LWOP and 12 measures of a state's use of the death penalty: total number of death sentences imposed, total death row population, total executions, death sentences per capita, death sentences as a percentage of the total prison population, death sentences as a percentage of all life and death sentences imposed, the death row population per capita, the death row population as a percentage of the prison population, the death row population as a percentage of all life and death sentences imposed, the number of executions per capita, the number of executions as a percentage of the prison population, and the number of executions as a percentage of all life and death sentences imposed. The results, reported in Table A1, suggest that there are no correlations between use of the death penalty and use of LWOP that are both meaningful and significant, except for the correlation between size of the LWOP population and size of the death row population. Yet, this bivariate relationship is positive, suggesting that the effects (if any) of these variables would complement each other, not cancel each other out. Thus, there is no concern that excluding the measures of the death penalty might downwardly bias or suppress the results for our LWOP measures.

We recognize that by excluding the death row population variable, the models that use the size of the LWOP population as the independent variable run the risk of overestimating the effect of LWOP. To avoid the problems associated with including measures of the death penalty and to provide a test with the most favorable possible conditions for the LWOP variable as a guard against criticisms of trying to ensure the failure of LWOP as a policy, however, we decided excluding this variable was the best strategy. Nevertheless, out of an overabundance of caution, we re-ran Models 1 and 9 twice, once with the significant and meaningful death row population variable included and once with the significant but not meaningful death row per capita variable included, and the results (not reported here but available upon request) had only a negligible impact on the magnitude of the LWOP variable and did not affect its significance level.

Descriptive Statistics

The descriptive statistics for all variables used in our analysis are presented in Table A2.

Technical Details on Computing Negative Binomial Models

We use Stata 15TM to conduct all of our analyses. Because the time-series commands built into Stata have a known error when the NB2 form of the negative binomial distribution is run (Allison, 2009), we run our models using the *nbg* command and include dummy variables for 49 of the 50 states and 4 of the 5 years to remove time-invariant state effects and time-varying national level trends. In addition, as this procedure produces confidence intervals that are too broad (Allison & Waterman, 2002), we rely on Stata’s *vce (opg)* option to inflate our standard errors (Allison, 2009). To make interpretation easier, we report our results as both regression coefficients and incidence-rate ratios (IRRs), which are the exponentiated coefficients (Long & Freese, 2006; Rabe-Hesketh & Skrondal, 2012).

TABLE A2 Descriptive statistics of dependent and independent variables

Continuous Variables	Median	Mean	SD	Range		<i>n</i>	
Violent crime	340.55	377.07	199.12	58.7	to	1152.2	250
Change in violent crime	−6.80	−7.11	29.96	−126.3	to	180.3	250
Total LWOP (thousands)	.19	.70	1.34	0	to	8.92	245
LWOP per capita	.51	1.10	1.57	0	to	10.41	245
LWOP per life sentence	.22	.31	.29	0	to	1	241
%LWOP prisoners	1.45	2.54	2.81	0	to	13.66	245
College	26.45	27.07	6.04	11.9	to	45.6	250
18–24	9.92	9.98	.76	8.17	to	13.36	250
25–34	13.49	13.83	1.61	8.92	to	18.91	250
Non-White	16.96	19.44	12.57	1.42	to	75.06	250
Population (thousands)	4,127.0	5,935.7	6,606.2	466	to	39,250	250
Poverty	12.55	12.98	3.44	5.8	to	24.6	250
Employed	61.90	61.99	4.44	48.3	to	72.5	250
Police per capita	2.32	2.42	.59	1.49	to	4.52	250
Firearm suicide	55.22	53.96	12.96	14.29	to	77.18	249
Discrete Variables	Percentage “Yes”			<i>n</i>			
Three strikes law	51.20			250			
Right to carry law	66.00			250			