Study 100 June 17, 2020

Memorandum 2020-7

Sentencing Topics and Trends, Including Recent Changes to California Law and Effects on Public Safety: Overview and Panelist Materials

At its June 2020 meeting, the Committee on Revision of the Penal Code[1] will hear from three panelists about the relationship between incarceration and crime rates, including a review of changes in California law and how two of those changes — Realignment in 2011[2] and Prop 47 in 2014[3] — affected crime rates.

The purpose of this memorandum is to provide general background about the topics the panelists are expected to cover. Some relevant material authored by the panelists are attached and summarized.

BACKGROUND

In many respects, California has been a leading laboratory of democracy in reducing the number of people it incarcerates — though much of that experimentation has been prodded along by federal court orders.[4] But even with California's dramatic steps — which have reduced its prison population by more than 25% since a peak in 2006[5] — more than 100,000 people are currently incarcerated in California's prisons.[6]

1. All Committee memoranda and reports can be downloaded from the Committee's website: <www.clrc.ca.gov/CRPC.html>.
2. Realignment (also known as AB 109) specified that, among other changes, (1) sentences for certain non-serious, non-violent, and non-sex offenses were to be served in local county jails instead of state prison (Penal Code § 1170(h)), (2) most violations of parole were to be served in jail, (Penal Code § 3056(a)), and (3) custody credits for time spent in jail were to be increased (Penal Code § 4019).
3. Proposition 47, enacted in November 2014, reduced penalties for some non-serious, non-violent property and drug offenses and allowed certain people who had been previously convicted of those offenses to be resentenced.
4. See, e.g., Brown v. Plata, 563 U.S. 493 (2011).
5 Nazgol Ghandnoosh, The Sentencing Project, U.S. Prison Decline: Insufficient to Undo Mass Incarceration, May 2020 (Figure 2 noting that California's prison population has declined 27% since its peak year in 2006).
6. California Department of Corrections and Rehabilitation, Weekly Report of Population, As of Midnight June 10, 2020 <https://www.cdcr.ca.gov/research/wp-content/uploads/sites/174/2020/06/Tpop1d200610.pdf>

The issue to be discussed by the panelists is the connection between rates of incarceration and crime. Do reductions in incarceration result in increases in crime? California's recent experience is instructive.

Crime rates are not the only measure of public safety, but they are important and are the focus of this meeting.[7]

PANELISTS

The committee will hear from the following experts at its June meeting:

- **Professor Steven Raphael**, Professor of Public Policy at UC Berkeley Goldman School of Public Policy. He has published widely on the criminal legal system, including the 2013 book (with Michael A. Stoll) *Why Are So Many Americans in Prison?*

  Professor Raphael will discuss the connection between incarceration and crime rates.
- **Caitlin O'Neil**, Senior Fiscal & Policy Analyst at the Legislative Analyst's Office.

  Ms. O'Neil will present an overview of major changes to California's sentencing and related laws since 2009, and an overview of changes in California's correctional populations and spending on corrections.
- **Professor Charis E. Kubrin**, Professor of Criminology, Law and Society, at UC Irvine. She has published widely on criminal justice issues. She completed (with Bradley J. Bartos) the first analysis of Prop 47's effect on violent and property crime and edited (with Carroll Seron) a special edition of the *Annals of the American Academy of Political and Social Science* about Realignment.

  Professor Kubrin will discuss research showing that violent crime did not increase as a result of Realignment and Prop 47 and that property crime may have had a small increase in auto-theft immediately following Realignment.

ATTACHED MATERIALS

As background, the staff has attached four documents authored by the panelists. They are summarized below.

7. See, e.g., Dr. Robert K. Ross, Op-Ed: Community safety means more than guns, badges, and crime, *LA Times*, Nov. 1, 2016 ("Just as the word peace means more than the absence of war, and health means more than the absence of disease, we must come to understand that safety means more than an absence of crime.").

"Incarceration and Crime," Chapter 7 of *Why Are So Many Americans in Prison*? (2013) by Steven Raphael and Michael A. Stoll.[8]

This chapter concludes that there is a connection between incarceration and public safety — at some level incarceration does reduce crime rates. But that connection has a limit. In particular, long sentences and high rates of incarceration have diminishing returns in reducing crime rates.

The chapter examines nationwide crime and incarceration data from 1977 to 2010 and finds that "empirical research certainly suggests that there were large gains to be had in terms of crime control during the 1980s in increasing the size of the prison population. The same is not true today."[9] In particular, between 1977 and 1988 a one-person increase in the incarceration rate resulted in a reduction of 1.3 to 2.1 violent crime incidents. But from 1989 to 2010, the explosion in incarceration "had no measurable effect on overall violent crime rates."[10] There were similar results for property crimes.

The chapter also reviews research into three reasons why incarceration might reduce crime. Those reasons and the chapter's conclusions are summarized below.

Incapacitation

Incarceration may "incapacitate" people from committing future offenses because they are incarcerated.

But the research shows that — in large part because offending reduces with age — there is little incapacitative value when prison sentences are long and widely applied. In other words, because the United States incarcerates so many people for so long, we have likely gone far beyond incapacitating the population most likely to commit future offenses.

General Deterrence

The threat of incarceration may deter some people from committing offenses. However, the research suggests that the value of general deterrence is minimal. At best there are occasional "modest effects" when "targeted offenders are well aware of potential enhancements and punishments are meted out with a fair degree of certainty."[11] But in general there is almost no evidence that long sentences deter the crimes they are intended to deter.

8 See Exhibit A.
9 Ex. A at 236.
10 *Id*. at 233.
11 *Id*. at 222.

Prison Experience

The experience of serving a prison sentence may increase or decrease someone's chance of offending after release.

Because each individual's response to the programs and perils of prison varies greatly, research is difficult here, but there is "little evidence that a prison spell reduces future offending below what it would otherwise be, and perhaps weak evidence that on average the incarceration experience makes former inmates more prone to commit crimes."[12] Situations that appear to increase the likelihood of offending include harsher conditions (such as a higher security prison setting), the criminal history of the group that someone is incarcerated with, and the difficulty of obtaining employment after release because of the stigma of a conviction.

***Overview of Recent Changes Impacting Sentencing* (2020), Legislative Analyst's Office**[13]

This handout prepared by the Legislative Analyst's Office briefly describes a decade's worth of changes (from 2000 to 2019) to California law related to adult sentencing.

The handout also charts changes in the number of people in prison, jail, and on various forms of supervision during this time. Overall, the total adult state and local correctional population declined by about 170,00 people (24%) between 2009 and 2018. More specifically, between 2009 and 2019, the prison population declined by about 42,000 people (25%) and the parole population declined by about 60,000 people (54%). The prison population is expected to continue to decline by about 9,000 people (7%) in the next four years. On the county side, between 2014 (the year Prop 47 was enacted) and 2018, the jail population declined by about 9,000 people (11%) and the population under community supervision declined by about 60,000 (18%).

Despite these population reductions, spending has generally increased. Between 2009–10 and 2017–18, California Department of Corrections and Rehabilitation expenditures increased by $2.6 billion (28%). In the same time period, county correctional expenditures increased by $1.6 billion (38%).

12. *Id*. at 228.
13. See Exhibit B.

Incarceration and Crime: Evidence from California's Public Safety Realignment Reform **(2016) by Magnus Lofstrom and Steven Raphael**[14]

This article concludes that "the reduction in California's prison population caused by Realignment modestly increased property crime primarily through motor vehicle thefts but had little effect on violent crime."[15]

The article considered roughly a year and a half's worth of post-Realignment data and found that "at California's pre-Realignment incarceration rate, for an additional offender serving one year in prison, roughly one to two property crimes per year and little to no violent crime are prevented."[16] The article cast this finding in purely economic terms as well, noting that "each prison year served for those who as a result of Realignment are no longer incarcerated prevents $11,783 in crime related costs."[17] The authors suggest that this is a poor use of public resources considering how much it costs to incarcerate someone in prison.

Can We Downsize Our Prisons and Jails Without Compromising Public Safety? Findings from California's Prop 47 **(2018) by Charis E. Kubrin and Bradley J. Bartos**[18]

This article concludes that there is "very little evidence to suggest that Prop 47 caused crime to increase in California."[19] Professor Kubrin's analysis relied on a synthetic control group — a research method where decades of crime data from states similar to California were used to create a "Synthetic California" that modelled what would have happened in California if Prop 47 had not been enacted. While some crime rose in California following Prop 47, this analysis showed that, in the year following its enactment, Prop 47 was not the cause: there is "no evidence of a statistically significant robust increase for [homicide, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft] in the year after Prop 47's enactment."[20]

Respectfully submitted,

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Senior Staff Counsel

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14. See Exhibit C.
15. Ex. C at 216.
16. *Id.*
17. *Id.* at 218.
18. See Exhibit D.
19. Ex. D at 16.
20. *Id.*

Exhibit A

CHAPTER 7

Incarceration and Crime

On July 31, 2006, the Italian Parliament passed legislation that reduced the sentences of most Italian prison inmates by three years, effective August 1, 2006. The clemency applied only to inmates convicted of a subset of felonies committed prior to May of that year. The passage of the "collective clemency" bill followed a six-year debate surrounding Italian prison conditions, spurred in large part by the activism of the Catholic Church and the personal involvement of Pope John Paul II. With Italian prisons filled to 130 percent of capacity, the onetime pardon was principally motivated by the need to address prison overcrowding.

Figure 7.1 displays a scatter plot of Italian monthly incarceration rates (measured as inmates per 100,000 residents) for the period from January 2004 to December 2008. The month of August 2006 is set to zero along the horizontal axis, with all months preceding and following measured relative to that month. The incarceration rate is relatively stable between January 2004 and August 2006. Between August and September 2006, however, the collective pardon induces a sharp decline. Over this one-month period, the prison population declined by 21,863 individuals, equivalent to a 36 percent decrease, with a corresponding decrease in the national incarceration rate from 103 to 66 inmates per 100,000.

Figure 7.2 displays corresponding monthly total crimes per 100,000 Italian residents. The national crime rate increased slightly during the pre-pardon

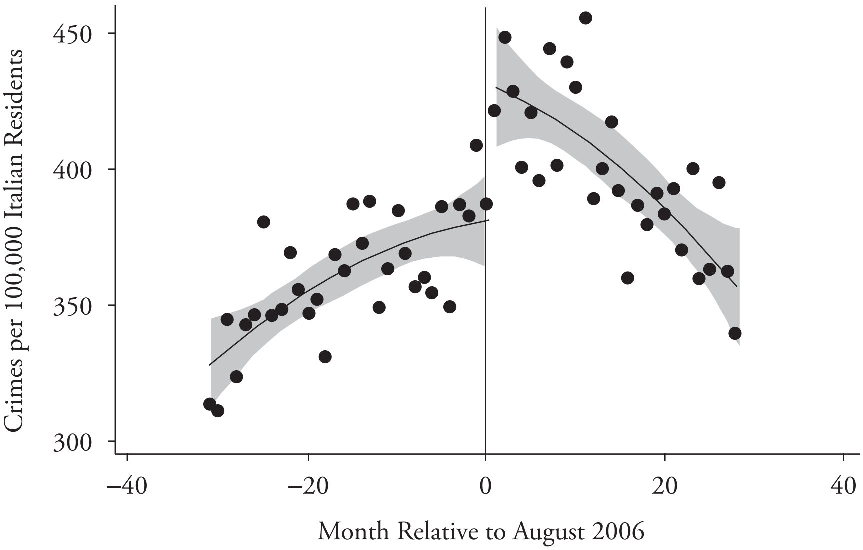
Figure 7.1 Scatter Plot of Monthly Incarceration Rate Against Month Measured Relative to August 2006

Source: Authors' compilation based on Italian Ministry of Interior (2009b), Ministero della Giustizia, Italy.

period, increased sharply between August 2006 and September 2006, and then steadily declined back to pre-pardon levels. The magnitude of the increase in crime coinciding with the mass prisoner release suggests that on average each released inmate generated fourteen felony crime reports to the police per year. Looking at variation within the country, we also note that Italian provinces that received more released inmates as a result of the pardon experienced relatively larger increases in crime. Although most of the increase in Italian crime associated with the collective clemency was attributable to theft, there was also a notable and statistically significant increase in robbery, a crime classified in most nations as a violent felony (Buonanno and Raphael, forthcoming).

Italy's experience with the 2006 collective clemency bill contrasts sharply with the recent experience of California. In April 2011, the state of California enacted broad correctional reform legislation under the banner of corrections realignment. The legislation eliminates the practice of returning parolees to

Figure 7.2 Scatter Plot of Total Monthly Crimes per 100,000 Italian Residents Against Month Measured Relative to August 2006



Source: Authors' compilation based on Italian Ministry of Interior (2009a), Ministero dell'Interno, Italy.

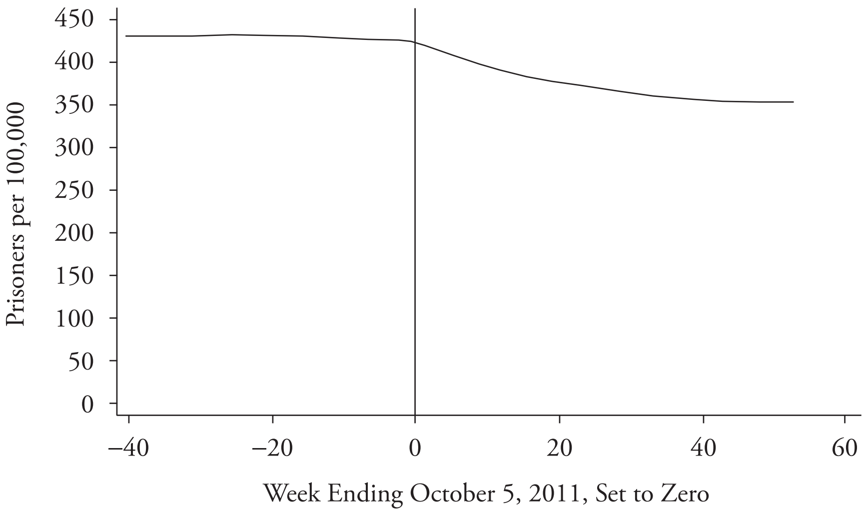
state prison custody for technical parole violation for all but a small set of the most serious and mentally ill offenders. The legislation also defines a group of nonserious, nonsexual, nonviolent offenders who upon conviction will serve their sentences in county jails. These offenders will earn good time credits more quickly than they would within the state prison system and can be given split sentences that involve alternative monitoring within the community. More generally, judges are now afforded greater discretion to devise alternatives to confinement in the sentencing of these offenders.

The legislation was prompted by an order by a federal three-judge court impanelled as a result of legal decisions in two lawsuits against the state filed on behalf of California prison inmates. In Plata v. Brown, it was alleged that California was providing inadequate health care services to its prison population. In Coleman v. Brown, it was alleged that the system was providing inadequate mental health services. These cases were consolidated and appealed to the U.S. Supreme Court. The Supreme Court affirmed the three-judge court ruling that prison overcrowding led to inadequate health and mental health care in violation of the Eighth Amendment prohibition against cruel and unusual punishment. The three-judge court ordered the California Department of Corrections and Rehabilitation (CDCR) to reduce its prison population from roughly 200 percent of design capacity to less than 137.5 percent of design capacity. Assembly Bill 109 (AB 109, referred to in the state as "corrections realignment") was passed and implemented to achieve this goal.

These reforms did not affect the California prison population as suddenly as was observed in Italy. However, realignment did result in a relatively quick reduction in the California prison population that was larger in magnitude than that experienced in Italy. Moreover, while Italy authorized a onetime release with an impact on the prison population that would be reversed by subsequent business-as-usual, California permanently altered sentencing and parole practices in a way that will lead to long-term sustained declines in its incarceration rate. Within a few weeks of the legislation's implementation on October 1, 2011, admissions to the state prison declined from roughly 2,100 per week to 1,000. Six months following the legislation, weekly admissions had settled at roughly 600 per week.

These admissions declines had immediate impacts on the state's incarceration rate. Figure 7.3 shows the California incarceration rate by week from January 2011 to the middle of October 2012. Along the horizontal axis of the graph, the week of the first post-realignment population count (the week ending October 5, 2011) is set to zero, and all other weeks are measured relative to this date. During the pre-reform period, the figure reveals a relatively stable incarceration rate that exhibited a slight downward trend. With the passage of realignment, we observe a sharp decline in the state's incarceration rate. By the end of 2011 (three months into the implementation of reforms), the incarceration rate had declined to 392 per 100,000—a decline per 100,000 relative to the last pre-reform week of 34 inmates per 100,000 (similar in magnitude to the immediate decline caused by the Italian pardon). By the end of June 2012, the state's incarceration rate had declined further, to 359 per 100,000, a level not experienced since 1992 (which predates the passage of California's "three strikes" ballot amendment). By October 2012, the incarceration rate had declined to 354 per 100,000 and appeared to have stabilized. In total, the permanent decline in California's incarceration rate by 72 per 100,000 over the first year of the reform period is nearly twice the temporary decline experienced by Italy as a result of the collective clemency bill.

Figure 7.3 California Incarceration Rates by Week, January 2011 to October 2012

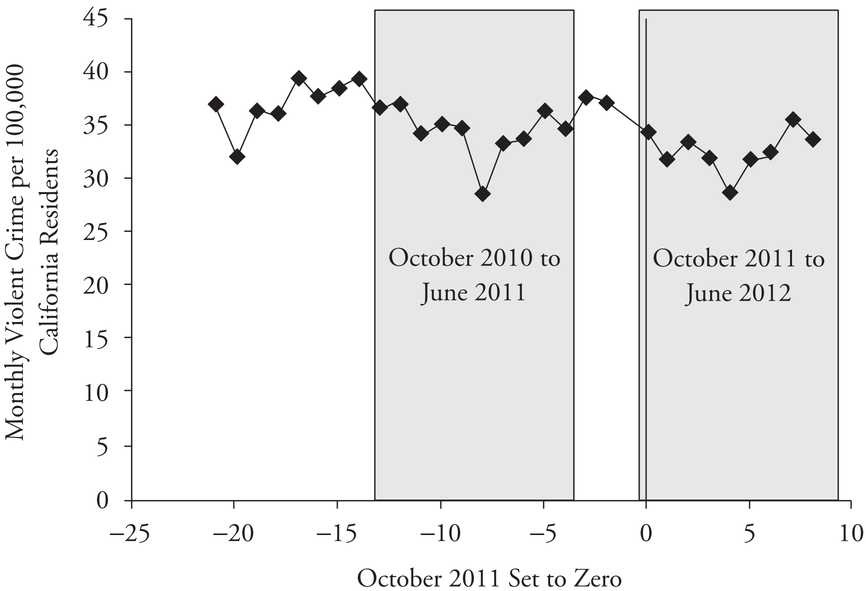


Source: Authors' compilation based on California Department of Corrections and Rehabilitation Weekly Population Reports (2011/2012).
Note: Weeks are expressed relative to first full post-realignment week.

And what has been the impact on California crime rates? Figure 7.4 displays monthly violent crime rates for the period January 2010 to June 2012. These are the most recent data available and provide coverage for nine postreform months.[1] During these months, violent crime declined relative to the months immediately preceding the reform. Crime appeared to be trending downward, however, over this time period. A better comparison is between violent crime for the period October 2011 to June 2012 and comparable crime rates a year earlier (that is to say, for the period October 2010 to June 2011). These two periods are highlighted on the figure. With the exception of February 2012, violent crime in each post-realignment month was lower than the comparable crime rate one year earlier. For February, the ratio of the violent crime rate in 2012 to the violent crime rate in 2011 is 1.006.

Figure 7.5 displays comparable data for property crime. Here we see higher property crime rates relative to one year earlier in seven of the nine postreform months for which we have data. However, we also see crime rising relative to one year previous in the two months prior to the implementation of AB 109, suggesting that property crime was trending upward for reasons other than the

Figure 7.4 California Monthly Violent Crime Rates, January 2010 to June 2012

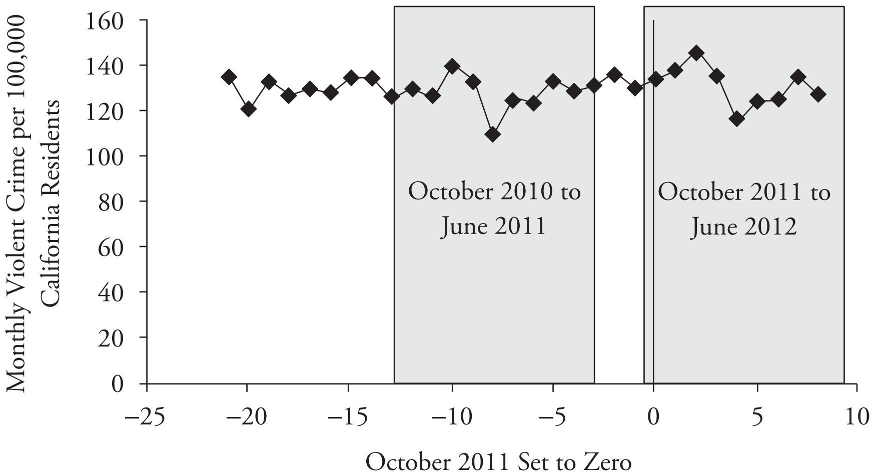


Source: Authors' compilation based on unpublished data from the California Department of Justice, Criminal Justice Information Services Division.

declining prison population. Relative to the difference in incarceration rates, the higher post-realignment crime rate suggests modest effects on crime relative to what was observed in Italy. For example, the monthly felony property crime rate in December 2011 was higher than the comparable rate in December 2010 by 5.8 incidents per 100,000. Meanwhile, the difference in incarceration rates between these two months was 39 inmates per 100,000, suggesting that each realigned inmate generated 0.14 new property offenses per month, or roughly 1.78 new offenses per year. If we account for the increase in the state's jail population equivalent to roughly 35 percent of the decline in the prison population, the figure would increase to 2.7 new offenses per year. This estimated impact on property crime is considerably smaller than the Italian estimate.

Raphael (2013) analyzes the effects of the realignment reforms on the California crime rate exploiting the fact that the effect of realignment has varied considerably across California's fifty-eight counties. Prior to realignment, there were large differences across counties in the propensity to use the state

Figure 7.5 California Monthly Property Crime Rates, January 2010 to June 2012



Source: Authors' compilation based on unpublished data from the California Department of Justice, Criminal Justice Information Services Division.

prison system to punish lower-level offenders. As a consequence, the number of inmates "realigned" to counties has tended to be larger in those counties that used the system relatively intensively prior to the reform. There is no evidence that those counties that received more realigned inmates experienced relatively greater changes in violent crime from the pre-reform period to the postreform period. Moreover, there is also no evidence in this cross-county analysis of an impact of realignment on burglary or larceny theft, though there is some evidence of a small effect on auto theft. Note that this analysis statistically accounts for the fact that some realigned offenders were being incarcerated in county jails.

What explains the difference between the experiences of Italy and California? For one, these are two very different places, with different demographics and systems of policing and criminal sentencing. Hence, the disparity may be due in part to differences in institutional and cultural factors. However, there are other key differences between the two case studies that are probably key to understanding the difference in outcomes. First, the pre-pardon incarceration rate in Italy stood at roughly 103 per 100,000 residents, which is quite close to the U.S. incarceration rates that existed prior to 1980. In California the pre-reform incarceration rate was between 425 and 430 per 100,000, more than four times that of Italy. If we add California's 75,000 jail inmates (which makes for a more appropriate comparison to Italy since Italy has a unified prison and jail system), this rate increases to 625 per 100,000. Our earlier analysis demonstrated that most of the growth in the U.S. incarceration rate in recent decades was driven by policy choices that have increased the likelihood of being sent to prison conditional on the crime committed as well as the amount of time that a person can expect to serve. Hence, one possible explanation is that California casts a much wider net in terms of who is sent to prison and for how long. Consequently, the average pre-reform inmate in California was perhaps less criminally prone than the average inmate in Italy, where prison is used more sparingly.

Second, Italy's collective pardon was broadly applied to all inmates with three years or less left on their sentence, with exceptions for inmates who had been convicted of offenses involving organized crime, felony sex offenders, and those convicted of terrorism, kidnapping, or exploitation of prostitution. California reduced its prison population more selectively, largely by discontinuing the policy of returning to custody parole violators who had not been convicted of a new felony. Hence, California's policy experiment may have been more effective at selectively reducing the prison population in a way that increased the street time of the least-serious offenders.

All of these factors are suggestive of great heterogeneity among those serving time in their propensity to offend when they are on the street. Moreover, in situations where policy choices increase the scope and scale of incarceration, this heterogeneity will increase as individuals who pose relatively little threat to society become more likely to be caught up in the criminal justice system alongside more dangerous convicted felons. Of course, it can be argued that a high incarceration rate has an impact on crime through other avenues, with deterrence of potential offenders a key consideration. To the extent that stiff sentences prevent crime through such deterrence, incarcerating someone who is convicted of a crime but poses little future threat to society may still be justified on utilitarian grounds.

In this chapter, we analyze the relationship between the use of incarceration and crime rates. We begin with a conceptual discussion of the various avenues through which crime and incarceration are linked and discuss the current state of knowledge regarding each mechanism. We then present estimates of the net effect of incarceration on crime in the United States and discuss how this relationship has changed with the growing prison population.

INCARCERATION, INCAPACITATION, AND DETERRENCE

Incarcerating a criminal offender may affect crime through several channels. First, placing a criminally active person in custody curtails that person's ability to commit crimes in non-institutional society. This "incapacitation effect" for a specific individual essentially equals the crimes that the person would have committed had he or she been free rather than in prison. There is little reason to believe that this incapacitation effect is constant for all those sent to prison. Some prison inmates are generally more criminally prone than others, an issue that we discuss at great length shortly.

Second, some potential offenders may be deterred from committing crime by the threat of a prison spell. To the extent that potential criminals consider the costs and benefits of their actions, stiffer sentences in the form of a higher likelihood of being sent to prison or receiving a longer sentence may increase costs above benefits and tip the decision-making scales in favor of behaving. Of course, such "general deterrence" requires that potential offenders accurately assess the likely consequences of their actions and that such individuals be sufficiently future-oriented to be deterred by changes in sentencing policy.

Finally, the experience of serving time in prison may alter the future offending trajectories of former prison inmates. It is important to note that for a given incarceration rate, an associated proportion of the non-institutional population has served prison time in the past. Higher incarceration rates generally correspond to a larger population of former prisoners, and their criminal offending may be affected by their experiences in prison. A prison spell may reduce offending if the experience itself deters (a factor often referred to as "specific deterrence," since the experience deters a specific person). Alternatively, education and treatment services while incarcerated may have rehabilitative effects on those who pass through prison and thus reduce future offending.

Of course, there are several ways in which a prison spell could increase an inmate's future offending. For example, he or she might adopt the behavioral norms of prison pertaining to the use of violence and approaches to conflict that are not acceptable in non-institutionalized society. Inmates may learn from other inmates, and they may pursue outside of prison the contacts they made with highly criminally active individuals while they were in prison. Finally, the stigma of having served time may limit an individual's legitimate opportunities once released and increase the relative returns to crime. When prison enhances future criminal offending, criminologists refer to prison as having a "criminogenic" effect on former inmates.

The effectiveness of prison as a crime control device depends on these three causal channels linking incarceration and crime. Prison certainly incapacitates people. However, the amount of crime incapacitated by a year in prison most certainly varies from person to person and is likely to be very low, if not negligible, for older inmates and for many inmates convicted of less serious offenses. In a high-incarceration-rate regime, we might expect particularly low average incapacitation effects to the extent that the broad applicability of prison as punishment nets the less criminally prone individuals along with the high-risk offenders. To the extent that many are deterred by the threat of prison, however, the costs of incarcerating these low-risk individuals may be outweighed by the deterrence benefits derived from making examples of them. The long-term effects on crime of having more people funnel through prisons can go in either direction. We discuss each of these avenues in turn and offer our summary of the current state of knowledge.

Prison and Incapacitation

Individuals differ considerably in their propensity to criminally offend. Moreover, there are clear average lifetime trajectories in the propensity to engage in crime that are observed throughout the world. In general, a small number of individuals commit the lion's share of felony offenses. Moreover, the likelihood of engaging in criminal activity tends to decline sharply with age beyond the age of eighteen.

This cross-person heterogeneity in the propensity to offend is central to understanding how the magnitude of the average incapacitation effect of prison changes with policy-induced increases in incarceration. In a world where incarceration is reserved for only the most serious offenders and sentences are relatively short, the criminal justice system prosecutes and incarcerates those offenders who commit the most serious offenses, and for periods of time that will span their younger, most criminally active years. As a consequence, the average amount of crime prevented per prisoner-year served should be relatively high. By contrast, in a world where incarceration is applied liberally and long sentences are the norm, the average number of crimes prevented per prison-year served is relatively low, owing to the fact that the criminal justice system is dipping further into the criminally active population for incarcerations (and netting less serious offenders as a result) and incarcerating people who are older.

There is ample empirical evidence of incapacitation effects that are often quite substantial at low incarceration rates yet quickly diminish as the incarceration rate increases. For example, in their thorough analysis of the 2006 Italian collective clemency bill, Buonanno and Raphael (forthcoming) produced several findings consistent with diminishing incapacitation effects. First, they demonstrate that Italian provinces with high pre-pardon incarceration rates suffered smaller increases in crime per released offender than Italian provinces with relatively low pre-pardon incarceration rates, holding prepardon crime rates constant. In other words, those provinces that were incarcerating their residents at a relatively high rate given their crime rate appeared to be incarcerating less dangerous people—strong evidence of diminishing crime-fighting returns to scale. Second, the incapacitation effect associated with early returns to custody following the pardon was considerably larger than the incapacitation effect associated with later returns to custody. In other words, those pardoned inmates who fail the soonest are the most criminally active and pose the greatest risk to society.

Ben Vollaard (2013) presents additional evidence of decreasing returns to scale, albeit in an institutional context very different from and less punitive than that of the United States. Vollaard analyzes the impact of a sentence enhancement in the Netherlands targeted at repeat offenders defined as those with more than ten prior felony convictions. In 2001 the Netherlands enacted an enhanced sentence of two years for such offenders, first allowing a small number of municipalities to experiment with the enhancement before applying it nationwide in 2004. Vollaard finds very large annual incapacitation effects of this policy change, on the order of fifty to sixty reported thefts prevented per year of incarceration. He also finds, however, that those municipalities that dipped further into the repeat-offender pool when they applied the sentencing enhancement experienced significantly smaller crime reductions per additional prison-year served. This latter finding is particularly interesting since the Dutch incarceration rate as of 2004, inclusive of pretrial detainees, was 124 per 100,000, or less than one-fifth the comparable incarceration rate for the United States.[2]

Empirical research for the United States strongly suggests that the crime-preventing effects of incarceration have declined as the incarceration rate has increased. Rucker Johnson and Steven Raphael (2012) provide estimates of the effects of a one-person increase in incarceration on felony property and violent crime for the United States for two different periods of time, 1978 to 1990 and 1991 to 2004. The former period was characterized by a relatively low number of prisoners (186 per 100,000 U.S. residents), while the latter period was characterized by a much higher incarceration rate (396 per 100,000). For the early period, an additional prison-year served prevented roughly 2.5 felony violent offenses and 11.4 felony property offenses.[3] Note that the figure for total crimes prevented is quite close to the implied annual reverse incapacitation effects caused by the 2006 Italian pardon. This is particularly striking since the U.S. incarceration rate during this earlier period was much closer to that of Italy at the time of the pardon. The figures for comparable crimes prevented per prison-year served for the period 1991 to 2004 were much lower (0.3 violent felony offenses and 2.7 felony property offenses). These findings are consistent with the evidence of diminishing returns to scale reported in a study by Raymond Liedke, Anne Morrison Piehl, and Bert Useem (2006).

Emily Owens (2009) provides further evidence of relatively small incapacitation effects for recent years for one U.S. state. Owens analyzes the criminal activity of convicted felons who served less time as the result of a change in Maryland sentencing policy that eliminated the practice of considering juvenile records when sentencing adult offenders. Owens finds that these former prison inmates indeed committed additional crimes during the time period when they would have been incarcerated had they been sentenced in years past. The implied incapacitation effects are quite small, however, on the order of one-fifth the size of the incapacitation effects from earlier research conducted during the 1970s base on inmate self-reports.

The recent experience of California documented earlier provides perhaps some of the strongest evidence of the crime-preventing effects of incarceration diminishing with increases in the incarceration rate. Despite a shock to the state incarceration rate that was nearly double in magnitude that of the Italian pardon, the immediate impact on crime rates was very slight. This observation suggests that many of the inmates who would have otherwise been sent to state prison in California have generated very little in the way of crime reported to the police.

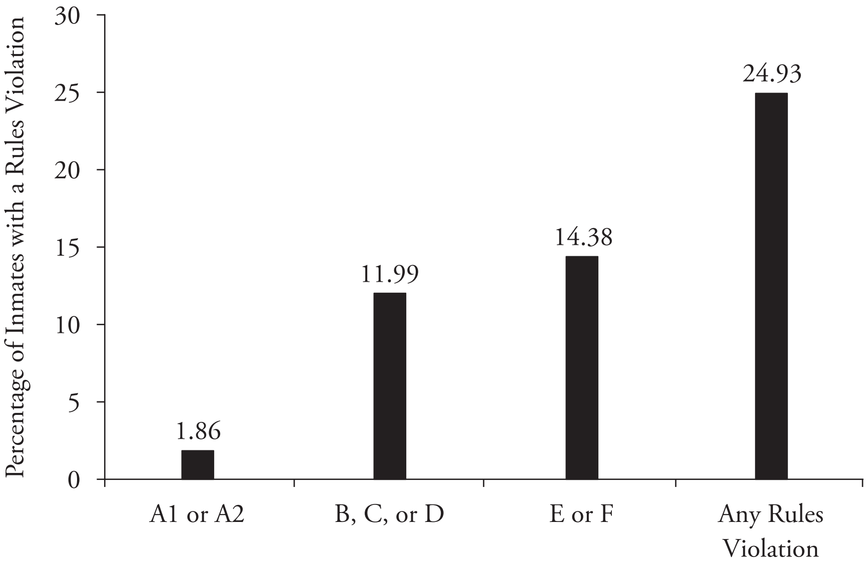
Although this may seem hard to believe, it is interesting to note that even among those doing time in California's prisons there is a great deal of heterogeneity in the propensity to offend behind bars and that the majority of the state's inmates are relatively well behaved while incarcerated. There is strong empirical evidence that the propensity to offend behind bars (especially the propensity to engage in violence) correlates with the propensity to offend when on the street.[4] In fact, recent behavior while incarcerated was a commonly used indicator of rehabilitation by parole boards under indeterminate sentencing in the past. Hence, heterogeneity in offending while incarcerated is likely to be indicative of heterogeneity in the incapacitation effects associated with incarcerating the current stock of prison inmates.

Fortunately, we have access to data that allow us to characterize such heterogeneity among the incarcerated population in California. Similar to other state prison systems, California periodically reviews the within-prison behavior of inmates for the purpose of classifying them according to security levels and then assigning them to specific institutions accordingly. Such reclassification hearings occur every six to twelve months and often result in inmate transfers between institutions with varying levels of security and inmate liberty. Some of the inmates' rules violations are quite serious, such as those involving violent assaults on other inmates and staff, while others are less serious (trafficking in contraband, possession of controlled substances, consensual sex, and so on).

We have administrative records pertaining to all serious rules violations by state inmates who served for a complete review period at any point in 2008 (that is to say, all inmates for whom we can observe two consecutive reclassification hearings).[5] We observe whether each inmate acquired an A violation (use of force or violence against another person), a B, C, or D violation (a breach or hazard to facility security; a serious disruption of facility operations; the introduction, distribution, possession, or use of controlled substances, alcohol, or dangerous contraband) or an E or F violation (an attempt or threat to commit any of the A through D violations or being under the influence or use of alcoholic beverages, controlled substances, unauthorized drugs, or intoxicants in an institution, community correctional facility, or camp).[6]

Before describing the incidence of these rules violations, we would note that the behavior of 80 percent of inmates between classification reviews was such that CDCR officially lowered their security classification score. In other words, 80 percent of the inmates were deemed to be more or less behaving between reviews. This general compliance is certainly evident in the low proportions of those who obtained official rules violation reports. Figure 7.6 shows the percentage of inmates who committed various rules violations. Roughly one-quarter of all inmates committed one serious rules violation over the course of the review period, meaning that three-quarters did not. Fewer than 2 percent committed a violent act that was met with an official

Figure 7.6 Percentage of Inmates Serving an Entire Review Period in a California State Prison in 2008 Who Acquired a Rules Violation Report Between Classification Hearings

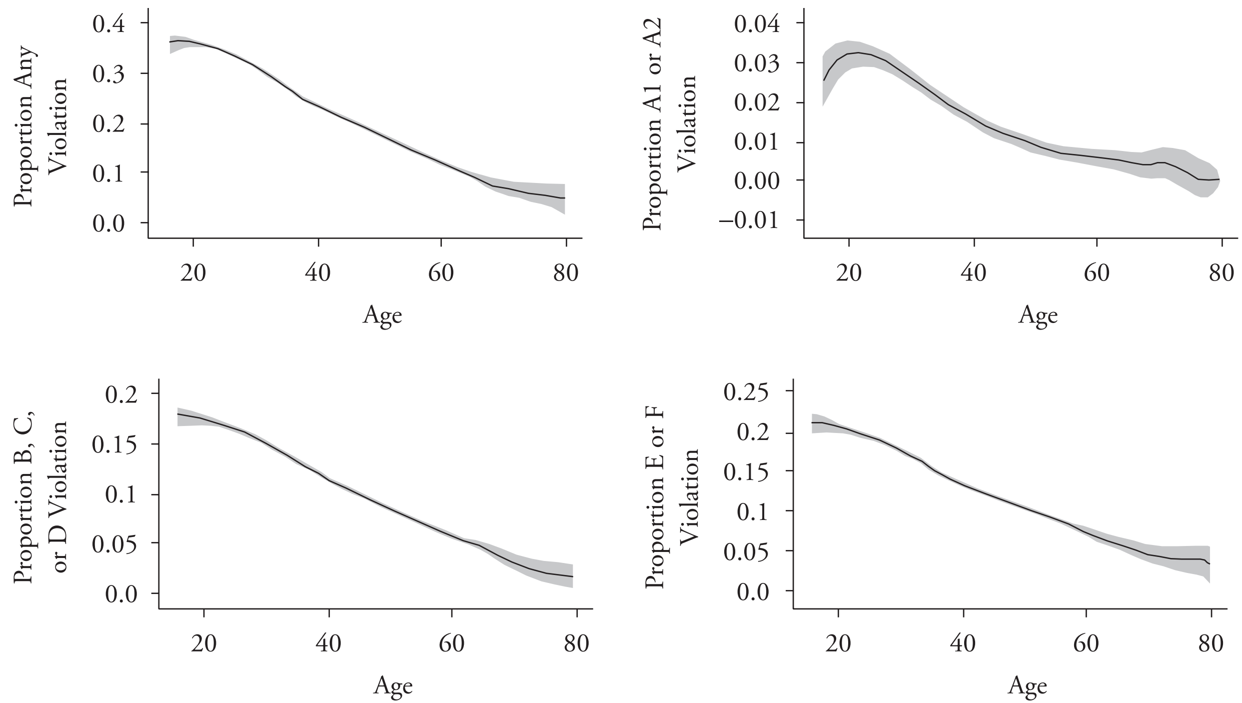


Source: Authors' tabulations of California Department of Corrections and Rehabilitation administrative records.

report and sanction. The incidence was higher for nonviolent infractions. Although these incidences were certainly high, it is notable that the majority of inmates were not engaging in these types of behaviors.[7] Figure 7.7 shows the relationship between the likelihood of committing a rules violation and age.[8] For all violations there is a very clear inverse relationship between the likelihood of committing an infraction and age, highlighting one very strong predictor of the likelihood that an individual will offend.

Hence, prison certainly incapacitates. However, the degree to which incapacitation results in lower crime varies from inmate to inmate. In general, the average incapacitation effect is lower when prison sentences are long and are applied with relative liberty. Moreover, even among those we send to prison, it is easy to document large disparities in criminal behavior and, most importantly for policy purposes, the power of at least one obvious predictor. This empirical research suggests that the United States has greater latitude for the

Figure 7.7 The Relationship Between the Likelihood of Acquiring a Rules Violation and Age Within California State Prisons, 2008



Source: Authors' tabulations of California Department of Corrections and Rehabilitation administrative data.

selective use of prison to incapacitate those who pose the greatest risk to society. To the extent that such selective incapacitation could be achieved, we could lower incarceration rates and the attendant fiscal and social costs with little impact on crime.

To be sure, even if more selective incapacitation does not necessarily increase crime by convicted offenders, it may lead to crime increases as a result of less general deterrence. We turn now to this issue.

The Threat of Prison and General Deterrence

A large and growing body of empirical research is attempting to measure general deterrence. The basic premise motivating this empirical work is that the threat of severe punishment will deter some potential offenders from committing crime. Some of the more high-profile and influential research in this domain focuses on the deterrent effect of capital punishment. Since the 1970s, several research teams have claimed to demonstrate that each execution saves numerous lives via general deterrence (Ehrlich 1975; Dezhbakhsh and Shepherd 2006). However, two reports by the National Academy of Sciences (Blumstein, Cohen, and Nagin 1978; Nagin and Pepper 2012), as well as four thorough reviews of this body of work (Donohue and Wolfers 2005, 2009; Chalfin, Haviland, and Raphael 2012; Charles and Durlauf 2012), conclude that nearly all of the research in this field is fraught with basic methodological problems that preclude any such inference.

Certainly punishment via confinement is a less drastic sanction than capital punishment. Nonetheless, prison sentences create very real personal costs for the punished offender, and thus it is theoretically plausible that the threat of a prison sentence deters crime. For example, being denied basic liberties, losing control over one's time, and having others control one's daily activities are very tangible costs of incarceration, as is the heightened risk of victimization by fellow inmates. State prisoners, and especially federal prisoners, are often housed very far from their home communities and have limited contacts with family and friends. Hence, any policy that increases either the likelihood of doing time or the effective sentence length could potentially deter.

General deterrence requires that those at risk of committing an offense be cognizant of the likelihood of being caught and the punishment that awaits them. Moreover, the extent to which one factors in the potential costs of incarceration certainly depends on the weight that one places on the costs that will be borne in the distant future (and for a long prison sentence, far into the future). In other words, a lengthy prison sentence will deter criminal activity only insofar as potential offenders take into account future costs and benefits when deciding whether to offend. Such considerations probably have little influence in determining levels of unpremeditated violent offenses that occur in emotionally charged settings. Even for premeditated offenses, the effectiveness of incarceration as a deterrent may be neutralized for those who have little knowledge of the likelihood of being caught and are extremely oriented toward the present.

There are many empirical studies of the deterrent effects of incarceration. On balance, our reading of this research is that the evidence suggestive of large general deterrence is relatively weak. Some studies find convincing evidence of general deterrence when the targeted offenders (usually repeat offenders facing very severe sanctions for subsequent crimes) are well informed regarding the consequences of their actions. The magnitudes of these effects, however, tend to be small relative to the effects of incarceration on crime operating through incapacitation. (Note that these conclusions accord with a recent review of this research presented in Nagin, 2013.)

A key challenge faced by empirical studies of general deterrence is to disentangle general deterrence effects from the effects of physically incapacitating a prison inmate. The research on general deterrence falls into broad groupings based on the methodological strategy pursued toward this end. One common strategy is to identify sentence enhancements that apply to offenses that nearly always result in a prison sentence. Since such enhancements rarely result in increased admissions to prison, any decline in crime associated with a sentencing policy change may be attributed to enhanced general deterrence caused by the longer expected sentence. The analysis of California's 1982 Proposition 8 by Daniel Kessler and Steven Levitt (1999) pursues such a strategy. The California ballot initiative enhanced sentences for a subset of violent felonies that nearly always result in a prison sentence. Hence, we would not expect any immediate impact of the proposition on crime operating through incapacitation.[9] The authors show a pre-post decline in serious violent crime in California. They also find that the targeted crimes declined relative to less serious felony offenses that were not targeted by the proposition, and that this relative decline departed from contemporaneous and comparable crime trends in the rest of the United States.

The conclusions in Kessler and Levitt (1999) have not gone uncontested. Cheryl Webster, Anthony Doob, and Franklin Zimring (2006) take issue with Kessler and Levitt's omission of even-numbered years from their descriptive crime statistics tables. Focusing on the odd-numbered years surrounding the passage of Proposition 8 shows a discrete and sustained drop in crime rates between 1981 and 1983. Adding the even-numbered years to the analysis reveals, however, that the decline in crime preceded the ballot measure's passage. Webster and her colleagues also take issue with Kessler and Levitt's choice of comparison groups, arguing that property offenses do not provide a sound benchmark against which to compare trends in violent crime.

The apparent ineffectiveness of this particular California proposition in the immediate aftermath of its implementation may have been due to poor knowledge among potential violent offenders of the provisions of the ballot initiative. All such propositions certainly receive a fair amount of press coverage, but it may be unlikely that potential violent offenders are following state electoral politics. Certain sentence enhancements have been better publicized, however, and in such a way that the specific offenders targeted by these enhancements are likely to be aware of the consequences should they reoffend. Moreover, many of these enhancements have been empirically evaluated.

Steven Raphael and Jens Ludwig (2003) analyze the general deterrence effects of a 1990s effort in Richmond, Virginia, to combat homicide by enhancing the sentences faced by felons found to be in possession of a firearm, a program called Project Exile. The central goal of Project Exile, through the coordinated efforts of Richmond law enforcement and the regional U.S. attorney's office, was to prosecute in federal courts all felon-in-possession of a firearm (FIP) cases, drug and gun cases, and domestic violence and gun cases, regardless of the number. Exile also included a massive advertising campaign intended to send the clear message of zero tolerance for gun offenses and to inform potential offenders of the swift and certain federal sentence.

Project Exile effectively enhanced sentences because the federal penalties for these firearm offenses were more severe than those in effect in Virginia at the time. Exile was announced in 1997. The disparity between the federal and state systems may have been particularly dramatic for FIP convictions for which the federal penalty was five years with no chance of early release. In addition to the differences in prison terms, gun offenders diverted into the federal system were denied bail at a higher rate than those handled in state courts. Moreover, they served time in a federal penitentiary that was likely to be located out of state. Both aspects of the program are thought to have imposed additional costs on offenders.

In their examination of the impact of Richmond's Project Exile on homicide and other crimes, Raphael and Ludwig (2003) conclude that the claims of dramatic declines in homicide rates made by supporters of the program were unfounded. Although Richmond's gun homicide rate did indeed decline during the implementation of Project Exile, the decline was in line with what was occurring across the country in comparable localities with no such programs. To be specific, cities with the largest increases in homicide rates during the 1980s and early 1990s also experienced the largest decreases during the late 1990s. Richmond happened to be among the handful of cities that had experienced unusually large increases in homicide rates during the 1980s. Consequently, nearly all of the reduction in murder rates experienced by Richmond following Project Exile may be attributed to the large increase in gun homicides that occurred prior to Exile's implementation. In other words, none of the decline in gun homicide rates in Richmond could be attributed to general deterrence.

One set of sentence enhancements for which the targeted offenders are generally made well aware of the tougher sentences they face should they reoffend are those passed through state repeat offender laws, often referred to as "three strikes" laws (see chapter 4). Such laws enhance the sentences of offenders with prior serious or violent felony convictions. California was one of the first states in the nation to adopt a "three strikes" law in 1994. The California law doubles the required sentence for any new offense for offenders with a prior conviction for one serious or violent felony (often referred to as a "second striker"). For offenders with two serious or violent felonies (potential "third strikers"), the sentence for a subsequent felony offense is an automatic life term, with a minimum sentence of twenty-five years. Prior prison inmates in the state of California with strike offenses on their criminal history records are certainly aware of the sentence enhancements they face if convicted of subsequent felonies.

Eric Helland and Alexander Tabarrok (2007) provide a quite convincing empirical assessment suggesting that general deterrence has been enhanced by the California law. For individuals released from California state prisons, they analyze postrelease arrest outcomes that vary in terms of the number of prior strikes on their criminal history records and thus the sentences they face should they reoffend. The study compares those with two prior strikes to those who had one prior strike, were charged and tried for a second-strike offense, but were convicted of a less serious felony the second time around that did not result in an increase in their strike count. Helland and Tabarrok find that within three years of release 40 percent of those with two strikes on their criminal history record were rearrested, compared with 48 percent of those in the comparison group. This eight-percentage-point differential is highly statistically significant.

Although this is pretty clear evidence of a general deterrence effect, it is noteworthy that even when facing a sentence of twenty-five years to life for any felony (even for less serious offenses such as drug felonies, receiving stolen property, larceny, and so on), 40 percent of second strikers in this study were still rearrested within three years. Moreover, this re-arrest rate is only 17 percent lower than that for offenders facing much less severe punishment for future offenses. Although the evidence certainly suggests responsiveness to incentives, managing the offending of this particular population requires interventions that extend beyond the credible threat of stiff punishment.

Francesco Drago, Roberto Galbiati, and Pietro Vertova (2009) present a similar analysis of sentence enhancements targeted at repeat offenders. These authors exploit a unique feature of the 2006 Italian mass pardon discussed earlier. To reiterate, the Italian collective clemency bill released most inmates with three years or less remaining on their sentence. Those who reoffended after release faced an enhanced sentence through the addition of the remainder of their unserved time to whatever new sentence was meted out for the new offense. Drago and his colleagues find that those inmates who faced a longer sentence enhancement (conditional on observables) were less likely to reoffend after being released. This added general deterrence effect was small, however, relative to the pure reverse incapacitation effect caused by the release of these inmates (Buonanno and Raphael, forthcoming). Moreover, as Daniel Nagin (2013) has noted, those pardoned inmates who faced longer sentence enhancements as a result of a longer unserved sentence also served less time in prison than those who faced shorter sentence enhancement. Hence, an alternative and equally plausible interpretation of the results in this study is that the more time one serves the more likely one is to commit crime in the future.

Several scholars have exploited the discontinuity in the severity of criminal sentencing upon reaching the age of majority to test for general deterrence. Since the severity of punishment increases discretely in most states at the age of eighteen, general deterrence should give rise to a corresponding discontinuous drop in criminal offending. Steven Levitt (1998) compares the change in offending between single years of age in states where the enhancement associated with being tried as an adult is relatively large and in states where the differences between juvenile and adult sentencing practices are more modest.

He finds larger declines in offending at the age of eighteen in the former states relative to the latter.

David Lee and Justin McCrary (2009), on the other hand, find no such evidence. Analyzing high-frequency arrest data with more granular information on the age of the arrestees, they find no evidence of a decline in offending associated with turning eighteen. They calibrate a dynamic model of criminal participation to forecast the expected declines in offending under various assumptions regarding the degree to which youth are present-oriented and assuming rational responses to enhanced adult sentences. The predicted declines from this model far exceed what is observed in the data, leading the authors to conclude that youthful offenders either are extremely myopic, are uninformed as to the consequences of being tried as an adult, or are making decisions to participate in crime that are not well characterized by rational choice modeling.

The findings in Lee and McCrary (2009) raise the important question of whether the extent of general deterrence depends only on the expected value of the offender's sentence (that is to say, the expected value of time served equal to the likelihood of being caught and convicted times the expected de facto sentence length). Certainly, criminal offenders' time preferences—and perhaps their time-inconsistent preferences—mediate the general deterrence effects created by enhancement of already long sentences.

Randi Hjalmarsson (2009) digs deeper into the offending behavior of youth as they pass through the age of majority. Using a nationally representative longitudinal data set, Hjalmarsson first assesses whether youth, and young men in particular, are aware of the harsher punishments that await them should they commit and be convicted of a felony offense as an adult. She documents this relatively harsher punishment using several data sources that clearly establish that offenders tried in adult criminal courts are much more likely to be punished with confinement than those tried in juvenile courts for comparable crimes. Next, Hjalmarsson shows that youth do indeed perceive a discretely higher likelihood of prison or jail time when they reach the age of majority in their respective states of residence. Interestingly, she finds surprisingly little evidence that youth criminal activity is deterred by this change in risk perceptions and only modest evidence of an effect of the threat of stiffer sanctions on the likelihood of committing larceny theft of less than $50. Hjalmarsson finds no measurable impacts on any other property, violent, or drug crimes.

In his recent treatise on crime control policy, Mark Kleiman (2009) argues for a shift toward sentencing practices that deemphasize sentencing severity yet increase the certainty of punishment. The thinking behind this proposition rests largely on the abundant empirical research (some of which we discussed earlier in the chapter) demonstrating the relative insensitivity of the behavior of the criminally active to enhanced sentences. Kleiman argues that this is to be expected given the profile of the average person who commits felonies. Kleiman also presents evidence of much greater responsiveness to sanctions that occur with great certainty. We discuss this work in more detail in the concluding chapter.

On balance, recent research on the general deterrence effects of the threat of incarceration yields evidence of modest effects in some instances and no deterrence in many others. General deterrence appears to be stronger when targeted offenders are well aware of the potential enhancements and punishments are meted out with a fair degree of certainty. There is very little evidence of an impact of extremely harsh punishment (that is, longer sentences, capital punishment) on the levels of the crimes they are intended to deter. This may reflect a tendency toward extreme present-orientation among those most likely to commit crime, poor information regarding likely sentencing outcomes, or both. Finally, in terms of overall crime control, general deterrence effects, when detectable, tend to be small relative to the effects of incarceration on crime operating through incapacitation.

The Experience of Prison and Future Offending After Release

The relatively high current incarceration rate in the United States translates directly into a larger pool of former prison inmates in non-institutional society. As we documented in chapter 1, roughly 5 percent of non-institutionalized adult males, and up to 17 percent of non-institutionalized African American males, have served time in a state or federal prison. A prison experience may either increase or decrease offending among former inmates relative to what their level of offending would otherwise have been. For example, a sufficiently harsh experience may deter future offending. Moreover, programming and services provided to inmates while incarcerated may rehabilitate them and reduce their offending.

On the negative side, prison inmates are exposed to a very criminally active peer set while institutionalized. To the extent that inmates adopt the norms and values of their peers, this may increase their criminal offending postrelease. Moreover, inmates may build stronger criminal ties behind bars and draw upon these social networks in non-institutional society. Former prison inmates also face substantial and real stigma upon release. To the extent that such stigma makes it difficult to achieve conventional markers of success (find legitimate employment, form lasting relationships), the relative attractiveness of participating in crime may be enhanced.

It is an inherently difficult task to empirically assess whether a prison term reduces future offending or increases it. Ideally we would compare the offending trajectory of a criminal offender who is sentenced to prison to that of a comparable individual who receives a noncustodial alternative sentence. We would need to carefully align the criminal histories of these two offenders to be sure to draw comparisons where both are of similar ages and not under some form of criminal justice custody, so as to not confound differential incapacitation or age with a long-term impact of a prior prison spell. Moreover, we would need to ensure comparability along all other possible determinants of criminal activity between those who are sentenced to prison and those who are not—a particularly difficult task given that prison sentences on average tend to be applied to more serious offenders.

An additional complicating factor faced by research on this question is that actual time served—and by extension, when and under what circumstances an inmate is released—can vary greatly for inmates with similar prison sentences. Through differential accumulation of good time credits, additional sentences for felonies committed while incarcerated, or the explicit decision-making of parole authorities in indeterminate sentencing states, those released from prison may be less likely to reoffend than inmates with similar offenses who are retained for longer periods. This consideration may create both heterogeneity in the effect of a prison sentence on future offending and a possible sample selection problem for empirical analyses of the long-term effects of incarceration on future offending. Regarding the first factor, states where release dates depend to a greater extent on good behavior and markers of rehabilitation may incentivize rehabilitation and lead to better postrelease outcomes.

Indeed, there is empirical evidence consistent with this line of reasoning. In an analysis of prison releases in the state of Georgia, Ilyana Kuziemko (2013) shows that when afforded discretion the state parole board manages to selectively release those inmates with a relatively low risk of offending. Moreover, Raphael and Stoll (2004) find evidence that the effect of prison releases on crime is lower in states with discretionary parole systems relative to states with mandatory parole systems.

Regarding the selection problem, the well-behaved may be disproportionately represented among releases. Studying the effect of prison on long-term offending must necessarily focus on those who are released. If the most criminally active are more likely to be retained, such research may overstate the salutary effects of a prison spell on future offending by focusing on those releases who managed to earn release. Moreover, if release dates are measured inaccurately (for example, if the research uses the date equal to the admissions date plus the minimum sentence as a proxy for release), the most criminally active among those sent to prison are likely to still be in custody and incapacitated. The apparent "good behavior" of those who are still in custody will artificially suppress average criminal activity among those sent to prison relative to a chosen comparison group.

A large body of research focuses on evaluating the net effect of these mechanisms on offending levels after release. Daniel Nagin, Frances Cullen, and Cheryl Lero-Johnson (2009) provide a very thorough review of this research. These authors review several groups of studies, including a handful of randomized control experiments in which the prison sentences are effectively randomly assigned; a set of studies that construct comparison samples for prison inmates using various matching techniques; and a series of studies that attempt to achieve comparability between those who have been sent to prison and those who have not by controlling for observable characteristics using multivariate regression modeling. Nagin and his colleagues also provide a review of research on the effects of time served on future offending behavior. Although the reviewed body of empirical work does not consistently point in one specific direction, the findings regarding the net effects of having served a prison sentence tend to point toward a slightly criminogenic impact of having served time on future offending.

Of course, given the multiple avenues linking serving time to future offending, the ultimate effect of a prison sentence is likely to vary greatly from one inmate to the next, since each will respond differently to the influences and incentives faced while incarcerated. For example, there is evidence that inmates held in harsher conditions become more hardened and criminally prone. Keith Chen and Jesse Shapiro (2007) analyze the effects of serving time in higher-security facilities relative to lower-security facilities in the federal prison system. Like many state systems, the federal prison system employs a numeric security classification score based on factors that predict behavioral risks and escape risk. (In many states, age, sentence length, prior misconduct, and gang affiliation are often key determinants.) When security scores rise above predetermined cutoffs, inmates are assigned to higher-security institutions, where there is considerably less freedom of movement and peer inmates are on average a more serious group of offenders. Chen and Shapiro find that inmates who just miss the cutoff to be assigned to lower-security institutions are more likely to recidivate after release than inmates placed in prisons that are less harsh. In a similar analysis, Steven Raphael and Sarah Tahamont (2012) find similar effects of high-security placement on the likelihood that inmates will commit serious behavioral infractions while incarcerated in a California state prison.

Further evidence of the effect of the conditions of confinement on within-prison behavior is presented by Sarah Tahamont (2012). Using a nationally representative survey of prison inmates, she assesses the impact of a connection to family and friends, operating through within-prison visits, on the likelihood of misconduct while incarcerated (much of which involves quite serious incidents, including assault on fellow inmates and corrections officers). Exploiting the fact that inmates housed very far from their home communities are considerably less likely to receive family visits, Tahamont finds very strong salutary effects of visits on inmate misconduct, although she also finds some evidence that the likelihood of a drug violation increases with family visits. To the extent that these behavior patterns extend beyond prison release, such variation in the conditions of confinement will cause variation in the long-term effects of incarceration on future offending trajectories.

A particularly novel demonstration of the heterogeneous impacts of incarceration on future offending behavior is presented by Patrick Bayer, Randi Hjalmarsson, and David Pozen (2009). Using administrative data on the Florida juvenile justice system, these authors assess whether the criminal histories of an inmate's fellow inmates have an impact on the likelihood that the inmate will offend in the future and on the types of future offenses committed. The study provides quite convincing evidence of adverse peer effects that tend to reinforce (or perhaps aggravate is a more appropriate word) the offending tendencies of incarcerated youth. For example, they find that youth who are serving time for burglary and whose peers are disproportionately made up of those convicted of burglary are more likely to commit a new burglary after being released. Bayer and his colleagues find similar patterns for youth convicted of larceny, drug offenses, aggravated assault, and felony sex offenses. Interestingly, peers are found to have a strong reinforcing effect on offending behavior (a burglar housed with other burglars is more likely to commit more burglary in the future), but not for youth with no history of committing a specific offense.

There is also evidence suggesting that the stigmatizing effect of incarceration may differ, with particularly serious effects for African American men. Stigma poses very real challenges for former inmates, especially when they seek legitimate employment. Employers often actively screen out those with prior convictions and prior time served for a number of reasons. First, employers may consider prior criminality a predictor of important unobservable traits, such as honesty or dependability. This may be particularly important to employers filling positions where monitoring by management is imperfect and where it may be difficult or costly to readily observe worker productivity.

Second, employers may fear being held liable for any criminal actions committed by their employees on the company's time. In negligent hiring and negligent retention cases, an employer may be sued for monetary damages caused by the criminal actions of any employee who the employer either knew or should have known had committed prior crimes that made the employee unsuitable for the position in question. Not surprisingly, research analyzing employer-stated preferences with regard to hiring those with criminal histories consistently finds that employers filling positions that require substantial contact with customers are among the most reluctant to hire former prison inmates (Holzer, Raphael, and Stoll 2006, 2007).

Finally, employers may be prohibited under local ordinances, state law, and sometimes federal law from hiring convicted felons into specific occupations. According to Shawn Bushway and Gary Sweeten (2007), ex-felons are barred from employment in roughly eight hundred occupations across the country, with the composition of these bans varying across states and in some instances localities. Occupations covered by such bans range from barber shop owners to emergency medical technicians to cosmetologists.

How important is prior criminal history to the screening and hiring practices of employers? Unfortunately, there are no national-level surveys of employer practices that can be used to answer this question. However, a handful of surveys of employers in various metropolitan areas and one state survey have queried respondents about their willingness to hire convicted felons. The most recent effort was carried out in 2003 by the Survey Research Center at the University of California at Berkeley. The sample includes California business and nonprofit establishments with at least five employees, excluding gov-

Table 7.1 Indicators of Employer Willingness to Hire Workers with Specific Characteristics into Nonprofessional, Nonmanagerial Jobs, 2003

Degree of Acceptability for the Most Recently Filled Position	Has a Criminal Record	Unemployed for a Year or More	Minimal Work Experience
Definitely accept	0.018	0.077	0.090
Probably accept	0.271	0.538	0.318
Probably not accept	0.339	0.368	0.454
Definitely not accept	0.371	0.018	0.137

Source: Authors' tabulations from Institute for Research on Labor and Employment (2003).

ernment agencies, public schools and universities, and establishments in the agricultural, forestry, or fisheries industries.[10] Table 7.1 presents tabulations regarding employer responses to queries about the acceptability of certain types of applicants for the most recently filled nonmanagerial, nonprofessional position. Employers were asked to think of the most recent position filled that met these criteria. They were then asked whether they would definitely accept, probably accept, probably not accept, or definitely not accept a specific type of applicant. The survey inquired about three applicant traits: an applicant with a criminal record, an applicant who had been unemployed for a year or more, and an applicant with minimal work experience.[11]

Fully 71 percent of employers indicated that they would probably not or definitely not hire a worker with a criminal record. The comparable figure for a worker who had been unemployed for a year is 38.6 percent, while the comparable figure for a worker with minimal experience is 59.1 percent. In prior research with Harry Holzer (Holzer, Raphael, and Stoll 2006), using data from an older establishment survey, we found a comparable reluctance to hire those with criminal records and much less reluctance to hire workers who had been unemployed, who were current welfare recipients, or who had little experience. The one category of applicants for whom employers exhibited a comparable (yet still less severe) reluctance to hire was applicants with gaps in their employment history. Certainly, prior criminal history and unaccounted-for gaps in one's résumé may be related in reality and in the minds of employers. In all, the California data and prior research clearly indicate a particular reluctance to hire workers with a criminal past.

The stigmatization associated with prior convictions and prison time most certainly inhibits a successful transition upon release to productive, law-abiding roles in non-institutional society. Although many former inmates overcome this challenge, employer inhibitions probably throw sand in the gears for many others. There is some evidence from audit studies that these barriers pose particular problems for African American men. Devah Pager (2003) provides evidence that employers have complicated perceptions of the relationship between race and criminality. Pager conducted an audit study in Milwaukee in which pairs of auditors of the same race were sent to apply for the same jobs, one with a spell in prison listed on his résumé and one with no such signal. Among the white auditors, 34 percent of the non-offenders received a callback, in contrast to 17 percent of ex-offenders. The comparable figures for blacks were 14 and 5 percent. Although all of the African American auditors experienced very low callback rates, the extremely low callback rate for African Americans with a criminal history (5 percent) on their résumé is particularly salient.

To summarize, in our reading of this body of research, we find little evidence that a prison spell reduces future offending below what it would otherwise be, and perhaps weak evidence that on average the incarceration experience makes former inmates more prone to commit crime. However, the mechanisms through which these effects operate certainly admit much heterogeneity, and thus the impact of the experience on specific individuals is likely to vary greatly. There is certainly evidence that prior convictions and incarceration create stigma and that the criminal propensities of some individuals are enhanced by their time behind bars. However, we can also imagine that, among others, access to drug treatment, education, vocational training, and consistent health care, as well as the experience itself, would reduce future offending. This is an area of inquiry greatly in need of further research.

EMPIRICAL ESTIMATES OF THE EFFECT OF PRISON ON CRIME

The three channels through which incarceration may affect crime rates—through incapacitation, general deterrence, and its long-term impact on offending—ultimately cumulate to a net effectiveness of incarceration as a crime control device. We have already discussed the evidence of diminishing incapacitation effects. We have said little about how the marginal effects through general deterrence and long-term effects on offending trajectories change with the scale of incarceration, as there is no empirical evidence specifically addressing this issue. There is, however, evidence on how the net effect on crime operating through the first two channels changes as the incarceration rate increases.[12]

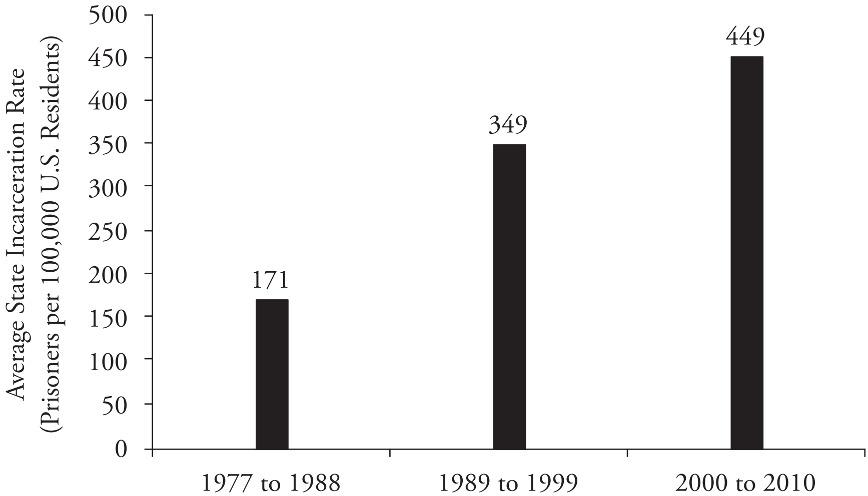
Specifically, a number of studies estimate the net effectiveness of incarceration as a crime control device using state-level data (Levitt 1996; Liedke, Piehl, and Useem 2006; Johnson and Raphael 2012). This research analyzes the relationship between year-over-year changes in state incarceration rates and corresponding changes in crime. Ultimately, the research produces estimates of the effect of a one-person change in the incarceration rate on crime rates. The two studies that assess how these effects change over time estimate this relationship for time periods with different incarceration levels. To the extent that the net effects of incarceration on crime are lower during time periods when the incarceration rate is higher, the data suggest diminishing net effects of incarceration on crime.[13]

Here we present estimates of the net effects of incarceration on crime, making use of the methodological strategy presented in Johnson and Raphael (2012). Specifically, we assembled a data set that measured crime rates, incarceration rates, and a number of average socioeconomic characteristics for each state and each year for the period 1977 to 2010.[14] We then used these data to estimate the average effects of a one-person increase in a state's incarceration rate on crime rates using crime data from the FBI Uniform Crime Reports.

We estimate average effects for the entire thirty-three-year period. We also estimate average effects for three subperiods: 1977 to 1988, 1989 to 1999, and 2000 to 2010. Figure 7.8 displays the average state incarceration rate during these three periods. Clearly, the earliest has the lowest average incarceration rate (171 per 100,000) compared to the middle period (349 per 100,000) and the most recent period (449 per 100,000). To the extent that the average effectiveness of incarceration in controlling crime changes with scale, we should see different estimates for these three periods. The appendix to this chapter provides a more detailed description of data sources and estimation methodology.

Table 7.2 presents estimates of the average effect of incarceration on crime for the entire thirty-three-year period. We present estimates for overall violent crime rates (the sum of murder, rape, robbery, and aggravated assault) and overall property crime rates (the sum of burglary, larceny, and motor vehicle theft), as well as estimates for each individual felony offense. For each crime rate, the table presents results from four models that estimate the effect of a change in a state's incarceration rate on the change in the state's crime rate.

Figure 7.8 Average State Incarceration Rates for 1977 to 1988, 1989 to 1999, and 2000 to 2010



Source: Authors' tabulations from Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.
Note: Averages are weighted by state population.

The first model does not control for any additional variables and the results simply reflect the relationship between year-over-year changes in incarceration and corresponding changes in crime rates. The second model adds controls for changes in state-level demographics and standard socioeconomic characteristics that are likely to have an impact on crime. The third model statistically adjusts for all factors that change from year to year for all states in a common manner. The final model adds a control for state linear time trends. We present this level of detail so that readers can gauge for themselves the sensitivity of the results to specification choices.

The results in table 7.2 yield fairly consistent evidence of sizable average effects of incarceration on crime for most of the crime categories analyzed in the table. For example, there is consistent evidence of a negative and statistically significant average effect of incarceration on the murder rate and the rate of sexual assault. There is also very strong evidence of significant negative effects on the overall property crime rate, the burglary rate, and the larceny rate.

How large are these effects? Some simple back-of-the-envelope calculations reveal the meaning of the magnitudes presented in table 7.2 The most de-

Table 7.2 Estimates of the Average Effect of a One-Person Increase in State Prison Incarceration Rates on Various Crime Rates, 1977 to 2010

	(1) No controls		(2) Controlling for State Demographics		(3) Controlling for State Demographics and Year Effects		(4) Controlling for State Demographics, Year Effects, and State Time Trends	
Violent crime	–0.299	(0.184)	–0.378	(0.199)***	–0.119	(0.171)	–0.195	(0.175)
Murder	–0.013	(0.004)*	–0.014	(0.005)*	–0.009	(0.005)***	–0.009	(0.005)***
Rape	–0.027	(0.007)*	–0.033	(0.009)*	–0.024	(0.011)**	–0.025	(0.013)***
Robbery	–0.299	(0.090)*	–0.300	(0.122)**	–0.177	(0.121)	–0.229	(0.140)
Assault	0.040	(0.152)	–0.030	(0.115)	0.089	(0.122)	0.068	(0.138)
Property crime	–3.4178	(0.899)*	–3.798	(1.302)*	–2.353	(0.731)*	–2.887	(0.905)*
Burglary	–1.884	(0.551)*	–1.806	(0.597)*	–0.970	(0.354)*	–1.159	(0.492)**
Larceny	–1.251	(0.417)*	–1.467	(0.649)**	–1.097	(0.332)*	–1.281	(0.357)*
Motor vehicle theft	–0.280	(0.197)	–0.524	(0.189)*	–0.286	(0.255)	–0.445	(0.289)

Source: Authors' tabulations from FBI Uniform Crime Reports (various years) and Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.

Note: Standard errors are in parentheses and account for clustering in the error term variance-covariance matrix within states. The coefficient estimates come from an instrumental variables regression of the year-to-year change in the given crime rate on the year-to-year change in the incarceration rate measured at the state level. The difference between the actual incarceration rate and the steady-state incarceration rate implied by the state's prison admissions and release rates forms the basis for the instrumental variable for the change in incarceration rates. This methodology is discussed in greater detail in the chapter 7 appendix. The regression models for the second column of results include controls for changes in per-capita income, state unemployment rates, state poverty rates, state age structure, and the proportion of the state population that is African American. The third column adds a complete set of year fixed effects, while the final column adds a complete set of state fixed effects. Since the regressions are based on changes in the dependent and explanatory variables rather than levels, the inclusion of state fixed effects in the specification is equivalent to adjusting each model for state-specific linear time trends.

*Statistically significant at the 1 percent level of confidence; **statistically significant at the 5 percent level of confidence; ***statistically significant at the 10 percent level of confidence tailed specification (the last column) for the murder rate model yields an average effect of a one-person increase in the incarceration rate on the murder rate of −0.009 (statistically significant at the 10 percent level of confidence). With an average incarceration rate in the states over the period analyzed of 329.8 per 100,000, this implies that incarceration in general reduced the murder rate over this period by 2.97 (0.009 × 329.8). The average murder rate over this period was 7.43 homicides per 100,000. Hence, our estimates imply that if we were to have abolished prisons with no compensatory social investment, homicide rates would have been 40 percent higher over this period. Comparable back-of-the-envelope calculations for rape, robbery, and overall property crime suggest that these crime rates would be 23, 39, and 22 percent higher with a zero incarceration rate.

Although these effects may seem large and perhaps suggestive that reducing the incarceration rate would result in large increases in crime, the logic behind such back-of-the-envelope calculations does not apply to partial reductions in the prison population and the manner in which a selective scaling back is likely to occur. First, we would expect that a deliberate policy choice to reduce the use of incarceration would also involve an expansion in the use of some other form of criminal justice intervention or crime-preventing social investments that would compensate. This is clearly what is unfolding in California, where the reduction in the state's incarceration rate is coinciding with expanded investment in community corrections.

Second, and more important for policy, these back-of-the-envelope calculations applied the average effect of incarceration on crime to project the effects of reducing the prison population. This would be appropriate if the policy under consideration was to abolish prisons. However, if the policy under consideration is to pare back but not eliminate prison populations, applying the average effects would be inappropriate.

The reason why is shown in tables 7.3 and 7.4, which present comparable estimates of the effects of incarceration on crime rates for the three subperiods corresponding roughly with the 1980s, the 1990s, and the 2000s. Here we present results only for the first three model specifications used in table 7.2.[15] Beginning with the violent crime results in table 7.3, we observe clear differences in the effects of incarceration on crime rates across the subperiods. Between 1977 and 1988, the average effect of a one-person increase in the incarceration rate on overall violent crime was a reduction of 1.3 to 2.1 incidents (depending on the model specification). Note that these estimated impacts on violent crime are substantially larger than the average effects for the entire period that we presented in table 7.2. All of these estimates are at least marginally statistically significant.

For the two latter time periods, however, the estimated effects are either the wrong sign (slightly positive for the period 1989 to 1999) or negative and very small (for the period 2000 to 2010). None of them are statistically significant, suggesting that growth in incarceration rates during these latter two time periods had no measurable effect on overall violent crime rates. We observe substantial and statistically significant effects of incarceration growth on rape and robbery crime rates for the earliest period, but very little evidence of an effect for the latter two periods. The results for murder are mixed, with significant coefficients during the 1990s and insignificant coefficients during the earliest and latest periods.

Table 7.4 reveals very similar results for property crime. For the earliest time period, the results suggest that each one-person increase in the incarceration rate reduced the overall felony property crime rate by between 9 and 19 incidents per 100,000 residents. Again, these estimates are much larger than average effects estimated in table 7.2 using data from all thirty-three years. During the last period, these estimates drop to between 2 and 3 incidents per 100,000 and are all statistically insignificant. Similar cross-period differences are observed for burglary and larceny theft.

These results are strongly suggestive of diminishing marginal returns to incarceration. Moreover, they provide a coherent explanation for the differences in the outcomes of the two case studies discussed at the beginning of this chapter. California, with its very high pre-reform incarceration rate and selective reduction in the incarceration rate through reduced admissions, saw very little impact of its prison reforms on crime rates, and any such impact was concentrated on less serious property crime. Italy, on the other hand, with its very low (by U.S. standards) pre-pardon incarceration rate and broadly applied pardon, released very criminally active people into non-institutionalized society and suffered a crime spike as a result.

CONCLUSION

The proposition that the use of incarceration reduces crime is not controversial. The high incidence of serious misconduct behind bars, the average socioeconomic and demographic characteristics of current prison inmates, and the criminal histories of inmates all suggest that prisons do indeed house many

Table 7.3 Estimates of the Effect of a One-Person Increase in the Prison Incarceration Rate on Violent Crime for 1977 to 1988, 1989 to 1999, and 2000 to 2010

	No Controls		Controlling for State Demographics		Controlling for State Demographics and Year Effects	
Violent crime						
1977 to 1988	–2.110	(1.079)***	–2.262	(1.044)**	–1.298	(0.724)***
1989 to 1999	0.061	(0.388)	0.106	(0.288)	0.034	(0.177)
2000 to 2010	–0.012	(0.304)	–0.069	(0.256)	–0.177	(0.309)
Murder						
1977 to 1988	–0.022	(0.037)	–0.021	(0.036)	–0.006	(0.037)
1989 to 1999	–0.011	(0.005)***	–0.012	(0.004)*	–0.010	(0.004)**
2000 to 2010	–0.009	(0.007)	–0.009	(0.007)	–0.009	(0.008)
Rape						
1977 to 1988	–0.158	(0.059)*	–0.151	(0.053)*	–0.104	(0.038)*
1989 to 1999	–0.016	(0.015)	–0.017	(0.012)	–0.019	(0.010)***
2000 to 2010	0.001	(0.021)	0.003	(0.019)	–0.004	(0.026)
Robbery						
1977 to 1988	–1.811	(0.784)**	–1.814	(0.758)**	–1.171	(0.545)**
1989 to 1999	–0.014	(0.166)	–0.001	(0.124)	–0.048	(0.087)
2000 to 2010	–0.012	(0.149)	–0.034	(0.129)	–0.151	(0.145)
Assault						
1977 to 1988	–0.119	(0.337)	–0.276	(0.288)	–0.016	(0.271)
1989 to 1999	0.102	(0.235)	0.135	(0.197)	0.112	(0.155)
2000 to 2010	0.008	(0.171)	–0.030	(0.147)	–0.020	(0.194)

Source: Authors' tabulations from FBI Uniform Crime Reports (various years) and Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.

Note: Robust standard errors that cluster on states are reported in parentheses. The coefficient estimates come from an instrumental variables regression of the year-to-year change in the given crime rate on the year-to-year change in the incarceration rate measured at the state level for the indicated time periods. The difference between the actual incarceration rate and the steady-state incarceration rate implied by the state's prison admissions and release rates forms the basis for the instrumental variable for the change in incarceration rates. This methodology is discussed in greater detail in the chapter 7 appendix. The regression models for the second column of results include controls for changes in per-capita income, state unemployment rates, state poverty rates, state age structure, and the proportion of the state population that is African American. The third column adds a complete set of year effects.

*Statistically significant at the 1 percent level of confidence; **statistically significant at the 5 percent level of confidence; ***statistically significant at the 10 percent level of confidence

Table 7.4 Estimates of the Effect of a One-Person Increase in the Prison Incarceration Rate on Property Crime for 1977 to 1988, 1989 to 1999, and 2000 to 2010

	No Controls		Controlling for State Demographics		Controlling for State Demographics and Year Effects	
Property crime						
1977 to 1988	–18.096	(8.087)**	–19.267	(7.968)**	–8.640	(4.777)***
1989 to 1999	0.319	(0.943)	–0.245	(0.680)	–1.289	(0.654)***
2000 to 2010	–2.822	(1.696)	–2.134	(1.414)	–2.049	(1.527)
Burglary						
1977 to 1988	–8.694	(3.606)**	–9.176	(3.572)**	–4.437	(2.076)**
1989 to 1999	–0.277	(0.195)	–0.409	(0.284)	–0.477	(0.253)***
2000 to 2010	–0.390	(0.419)	–0.241	(0.387)	–0.342	(0.462)
Larceny						
1977 to 1988	–8.701	(4.182)**	–8.954	(4.161)**	–3.278	(2.437)
1989 to 1999	0.678	(0.629)	0.302	(0.424)	–0.526	(0.401)
2000 to 2010	–1.726	(0.994)***	–1.108	(0.822)	–1.178	(0.925)
Motor vehicle theft						
1977 to 1988	–0.702	(0.815)	–1.137	(0.747)	–0.924	(0.926)
1989 to 1999	–0.081	(0.318)	–0.139	(0.262)	–0.284	(0.202)
2000 to 2010	–0.703	(0.489)	–0.785	(0.428)***	–0.526	(0.444)

Source: Authors' tabulations from FBI Uniform Crime Reports (various years) and Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.
Note: See table 7.3 note.
*Statistically significant at the 1 percent level of confidence; **statistically significant at the 5 percent level of confidence; ***statistically significant at the 10 percent level of confidence

highly criminally active individuals. That being said, prisons also house many older inmates, many inmates who have not been convicted of serious violent felonies, and many inmates who manage to do their time without getting into further trouble. Abolishing prisons would certainly increase the nation's crime rates. That might not be the outcome, however, of selectively scaling back the use of incarceration with an eye to reserving it for those who pose the greatest risk to society.

We have presented a detailed discussion of large bodies of research that suggest that the crime-preventing effects of incarceration vary quite a bit from inmate to inmate and that on average the effectiveness of prison in reducing crime rates diminishes as the incarceration rate increases. Evidence of such diminishing returns is clear in the United States, where the incarceration rate has grown to unprecedented levels, but it has also been documented in other nations with much lower incarceration rates. Our empirical research certainly suggests that there were large gains to be had in terms of crime control during the 1980s in increasing the size of the prison population. The same is not true today. In fact, a full accounting of the on-budget costs and off-budget social costs of incarceration is likely to reveal that today, with our much higher incarceration rate relative to the 1980s, we are in the mirror-opposite position.

An optimal crime control policy geared toward minimizing crime for a given level of public expenditures would invest further public resources in interventions for which the return per dollar spent is the highest. An optimal policy would also scale back any intervention that resulted in the returns per dollar spent falling short of comparable returns from alternative interventions. Are there alternative crime control policy tools that yield a larger bang for the buck than additional prison beds? Moreover, are there other policy tools that could enable us to use current prison capacity more efficiently while reducing the scale of incarceration? We turn to these questions in our final chapter.

APPENDIX

In this appendix, we describe our methodology for estimating the effects of a one-person change in a state's incarceration rate on crime rates. We apply a two-stage-least-squares estimator to a state-year-level panel data set. Our data set covers the period from 1977 to 2010. In all models, the explanatory and dependent variables are measured as year-over-year changes (that is, the state-year panel data are first-differenced).

A key methodological issue in panel data studies of the effects of incarceration on crime is the likely simultaneous relationship between incarceration and crime rates. To break the simultaneity and measure causal effects, we employ the instrumental variables strategy developed by Johnson and Raphael (2012), who developed an instrumental variable that can succinctly be described as a prediction of the change in a state's incarceration rate between two years, based on the disparity between the state's actual incarceration rate and the steady-state incarceration rate implied by the admissions and release rates in the base year. When the state's actual incarceration rate is below the implicit steady-state rate, the instrument predicts an increase. Conversely, when a state's incarceration rate is above the implicit steady-state rate of the state, the instrument predicts that the incarceration rate will decrease. The absolute value of the magnitude of the predicted change in the incarceration rate increases with the absolute value of the difference between the actual and implicit steady-state rates in the base year of the change. Johnson and Raphael derive the theoretical conditions under which the instrument identifies exogenous variation in incarceration usable for identifying the causal effects of an incarceration change on crime rates.

We obtained data on aggregate flows into and out of prison by state and year from the National Prison Statistics (NPS) database. These data provide the total admissions and total releases from prison within a calendar year. Data on the stock of prison inmates under each state's jurisdiction come from the Bureau of Justice Statistics; these data measure the stock of inmates as of December 31 of the stated calendar year. State-level population and demographic data as well as data on state-level poverty come from the U.S. Census Bureau. State-level unemployment rates come from the Bureau of Labor Statistics while state-level data on per-capita income are drawn from the Bureau of Economic Analysis.

In the original analysis, Johnson and Raphael (2012) construct the instrumental variable predicting changes in incarceration rates from annual estimates of prison admissions and release rates that were smoothed across years with a high-order, state-specific polynomial regression. The results were not sensitive to this smoothing of the underlying data. In the current application, we employ the unsmoothed transition probabilities as the instrument; using the unsmoothed data provides us with stronger first-stage relationships between the predicted change in incarceration and the actual change.

Our panel data set covers the period from 1977 to 2010 and the fifty states and Washington, D.C.[16] All models are weighted by state population. Table 7A.1 displays the first-stage relationship between the predicted change and actual changes in incarceration rates from several specifications when the model is fit to the entire time period. The first column presents the results from a bivariate regression. The second column adds demographic and socioeconomic covariates that vary at the state-year level. The third column adds a complete set of year fixed effects. Finally, the fourth column adds a complete set of state fixed effects. Note that since the models are estimated in first-difference form, state-level fixed effects are already differenced out of the data.

Table 7A.1 First-Stage Effect of the Predicted Change in Incarceration Rates Based on the Last Period Shock on the Current Change in Incarceration Rates

	(1)	(2)	(3)	(4)
Predicted Δ incarceration	0.670	0.637	0.542	0.519
	(0.125)	(0.134)	(0.139)	(0.157)
Δ percentage in population age zero to seventeen	—	–2.418	–0.426	–1.007
		(2.272)	(2.394)	(2.419)
Δ percentage in population age eighteen to twenty-four	—	–4.669	3.272	2.649
		(3.787)	(3.773)	(3.798)
Δ percentage in population age twenty-five to forty-four	—	–2.011	–1.194	–2.217
		(3.281)	(3.711)	(3.787)
Δ percentage in population age forty-five to sixty-four	—	–3.433	2.343	1.705
		(3.693)	(4.561)	(4.624)
Δ unemployment rate	—	–2.018	–1.759	–1.699
		(0.641)	(1.017)	(1.009)
Δ poverty rate	—	–0.966	0.347	0.376
		(0.518)	(0.598)	(0.607)
Δ percentage black	—	0.902	0.787	0.840
		(0.153)	(0.246)	(0.241)
Δ per-capita income	—	–0.002	–0.001	–0.001
		(0.0001)	(0.002)	(0.002)
Year effects	No	No	Yes	Yes
State effects	No	No	No	Yes
R^2	0.122	0.137	0.277	0.286
N	1,621	1,621	1,621	1,621
F-statistic[a]	28.54	22.34	15.25	10.84
(*P*-value)	(0.000)	(0.000)	(0.000)	(0.002)

Source: Authors' tabulations from FBI Uniform Crime Reports (various years) and Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.

Note: Robust standard errors clustered by state are reported in parentheses. All models include constant terms and are weighted by the state-year populations. Dependent variable = ΔIncarceration Rate.

[a]F-test from a test of the significance of the instrumental variable.

Table 7A.2 Results from F-Tests of the Instrumental Variable from the First-Stage Regressions of the Two-Stage-Least-Squares Models of the Effects of Changes in Incarceration Rates on Crime for Three Subperiods

	1977 to 1988	1989 to 1999	2000 to 2010
No controls	18.50	11.74	13.91
	(0.000)	(0.001)	(0.001)
Demographic and economic covariates	21.38	10.38	14.16
	(0.000)	(0.002)	(0.000)
Covariates plus year effects	18.14	10.66	10.54
	(0.000)	(0.002)	(0.002)
Covariates plus year effects and state effects	1.13	5.21	1.06
	(0.292)	(0.27)	(0.309)

Source: Authors' tabulations from FBI Uniform Crime Reports (various years) and Bureau of Justice Statistics (various years), *National Prisoner Statistics* data series, BJ5.
Note: The figures in the table are F-statistics from a test of the statistical significance of the instrumental variable predictor of changes in incarceration rates from the first-stage regression of the change in incarceration rate on the instrument and other covariates. Associated P-values are presented in parentheses below each F-statistic.

The inclusion of the state fixed effects in the first-difference models is equivalent to controlling for state-specific linear time trends. These four specifications correspond to the model specifications of the two-stage-least-squares models underlying the main empirical results presented in the main text of the chapter. In all models, the predictive power of the instrumental variable is respectable, with the F-statistic on a test of the exclusion restriction in excess of 10 in all specifications.

Table 7A.2 presents the results from an F-test of the first-stage relationships for the same four specifications applied to the three subperiods that we analyze in chapter 7. The instrument performs well for all specifications except for the most detailed. When state fixed effects are added for the three subperiods, the explanatory power of the instrument is diminished considerably. In fact, the instrument is statistically insignificant for the earliest and latest subperiods for this specification. Hence, the estimation results for the three subperiods employ the first three model specifications only.

Exhibit B

JUNE 24, 2020

Overview of Recent Changes Impacting Sentencing

PRESENTED TO: Committee on Revision of the Penal Code

LEGISLATIVE ANALYST'S OFFICE

Selected Changes Impacting Sentencing in Past Decade

Chapter 28 of 2009 (SB3X 18, Ducheny)

- Increased credits prison inmates earn to reduce their sentences (such as for completion of rehabilitation programs) and made certain lower-level parolees ineligible for revocation to prison for parole violations by the California Department of Corrections and Rehabilitation (CDCR).
- Reduced felony convictions by increasing the dollar thresholds that allow various theft crimes to be punished as felonies as opposed to misdemeanors.

Chapter 608 of 2009 (SB 678, Leno)

- Created a fiscal incentive for counties to reduce the number of felony probationers that fail on probation and are sent to state prison.

2011 Realignment

- Limited who could be sent to state prison by requiring that certain lower-level felons serve their incarceration terms in county jail or a combination of jail and county community supervision—referred to as mandatory supervision.
- Required that counties, rather than the state, supervise certain lower-level felons released from state prison—referred to as Post-Release Community Supervision (PRCS).

Proposition 36 (2012): Changes to "Three Strikes" Law

- Eliminated life sentences for certain offenders with two or more prior serious or violent felony convictions whose most recent offenses are nonserious, nonviolent felonies.
- Allowed offenders who were serving these sentences at the time to apply for reduced sentences.

Selected Changes Impacting Sentencing in Past Decade

(Continued)

Proposition 47 (2014): Sentencing for Nonserious, Nonviolent Felons

- Reduced penalties for certain offenders convicted of nonserious and nonviolent property and drug crimes.
- Allowed certain offenders who had been previously convicted of such crimes to apply for reduced sentences.

Various Court-Ordered Population Reduction Measures (2014)

- Reduced the prison population primarily by increasing credits certain inmates earn for maintaining good behavior and creating a release consideration process for certain nonviolent inmates sentenced under the three strikes law.

Proposition 57 (2016): Parole Consideration, Credits, and Juveniles Charged as Adults

- Reduced the prison population primarily by expanding inmate eligibility for release consideration and increasing CDCR's authority to reduce inmates' sentences through credits (such as for completion of rehabilitation programs).

Expanded Authority for Courts to Resentence Inmates

- Chapter 36 of 2018 (AB 1812, Committee on Budget) allowed courts to consider post-conviction factors (such as inmates' disciplinary records) in determining whether to reduce an inmate's sentence upon recommendation by a CDCR or jail administrator.
- Chapter 1001 of 2018 (AB 2942, Ting) authorized district attorneys to recommend inmates to the courts for resentencing under this process.

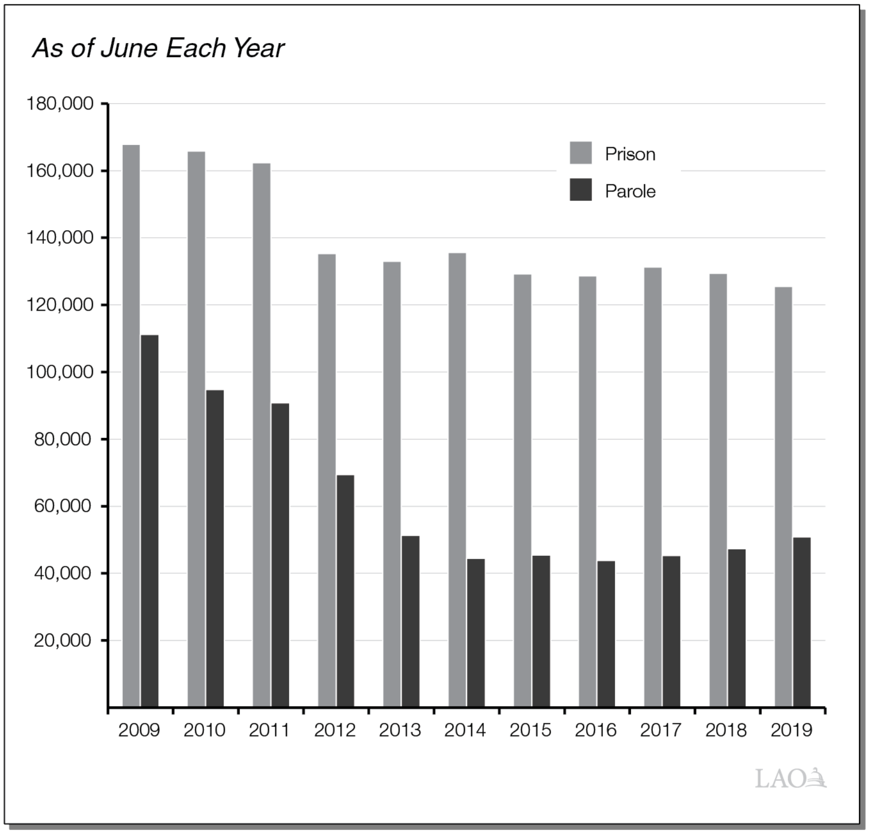
Selected Changes Impacting Sentencing in Past Decade

(Continued)

Various Modifications to Sentencing Enhancements

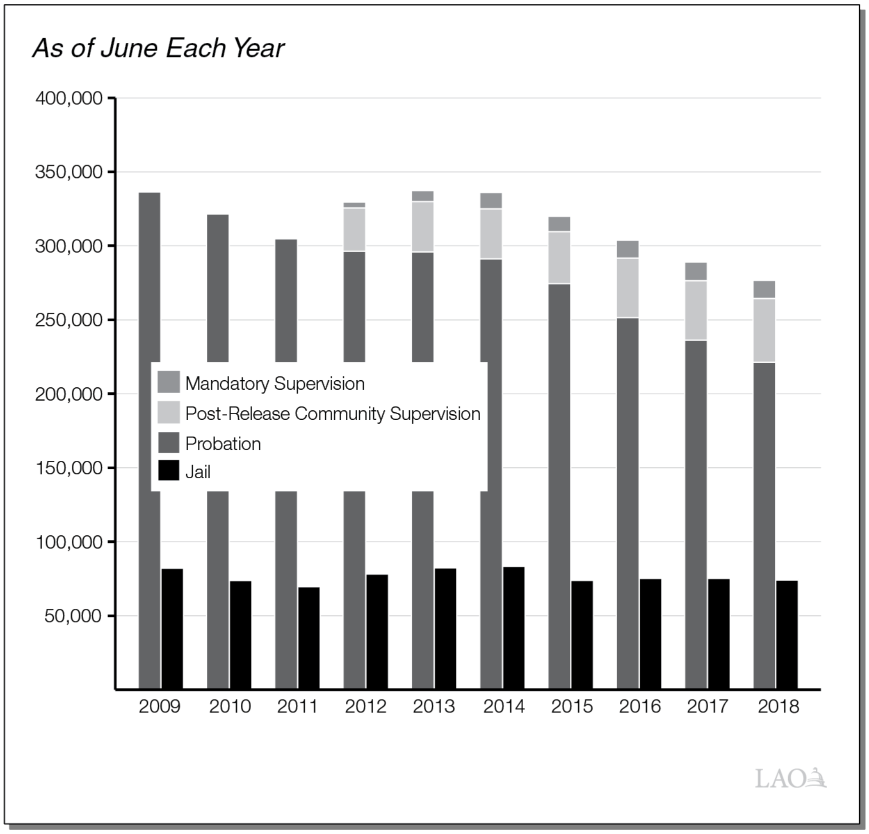
- Felony offenders may be required to serve additional time in jail or prison due to circumstances surrounding their crime (such as if they used a firearm) or their criminal history. This additional time is known as an "enhancement."
- Various recent sentencing changes have reduced enhancements that offenders receive:
  - Chapter 677 of 2017 (SB 180, Mitchell) generally eliminated a three-year enhancement imposed on people convicted of drug offenses who also have prior drug offenses.
  - Chapter 682 of 2017 (SB 620, Bradford) allowed judges to choose not to impose certain enhancements in cases where a firearm is used in the commission of a crime.
  - Chapter 1013 of 2018 (SB 1393, Mitchell) allowed judges to choose not to impose a five-year enhancement for those convicted of a serious felony who also had a prior serious felony conviction.
  - Chapter 590 of 2019 (SB 136, Wiener) generally eliminated a one-year enhancement for offenders who have previously served a prison or jail term for a felony.

State Correctional Populations Have Declined Significantly



- Between 2009 and 2019 the prison population declined by about 42,400 (25 percent) and the parole population declined by about 60,400 (54 percent).
- The most significant reductions occurred between 2011 and 2014 when the prison population declined by about 26,800 inmates (16 percent) and the parole population declined by about 46,300 (51 percent)—primarily due to the effects of the 2011 realignment.
- In addition to further reducing the prison population, the various policy changes occurring after the 2011 realignment also offset underlying projected growth in the prison population. Some of the changes, such as Proposition 57, have temporarily increased the parole population by accelerating releases from prison.

County Correctional Populations Have Generally Declined

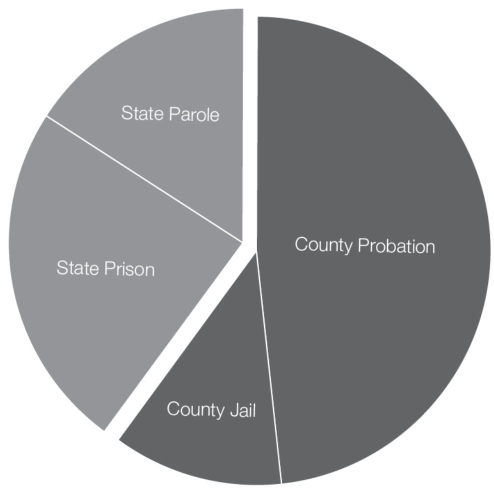


- Between 2009 and 2011, the probation population decreased by about 31,600 (9 percent) and the jail population declined by about 12,500 (15 percent)—likely primarily due to the effects of Chapter 28.
- Between 2011 and 2014, the population under county community supervision (which includes mandatory supervision, PRCS, and probation) increased by about 31,100 (10 percent) and the jail population increased by about 13,600 (19 percent)—primarily as a result of the 2011 realignment.
- Between 2014 and 2018, the population under county community supervision declined by about 59,300 (18 percent) and the jail population declined by about 9,100 (11 percent)—likely primarily due to Proposition 47.

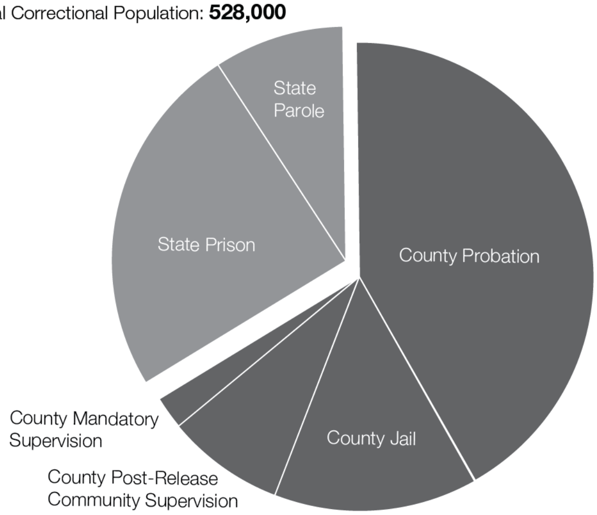
Total Adult Correctional Population Declined and Shifted to Counties

As of June Each Year

2009
Total Correctional Population: **698,000**



2018
Total Correctional Population: **528,000**

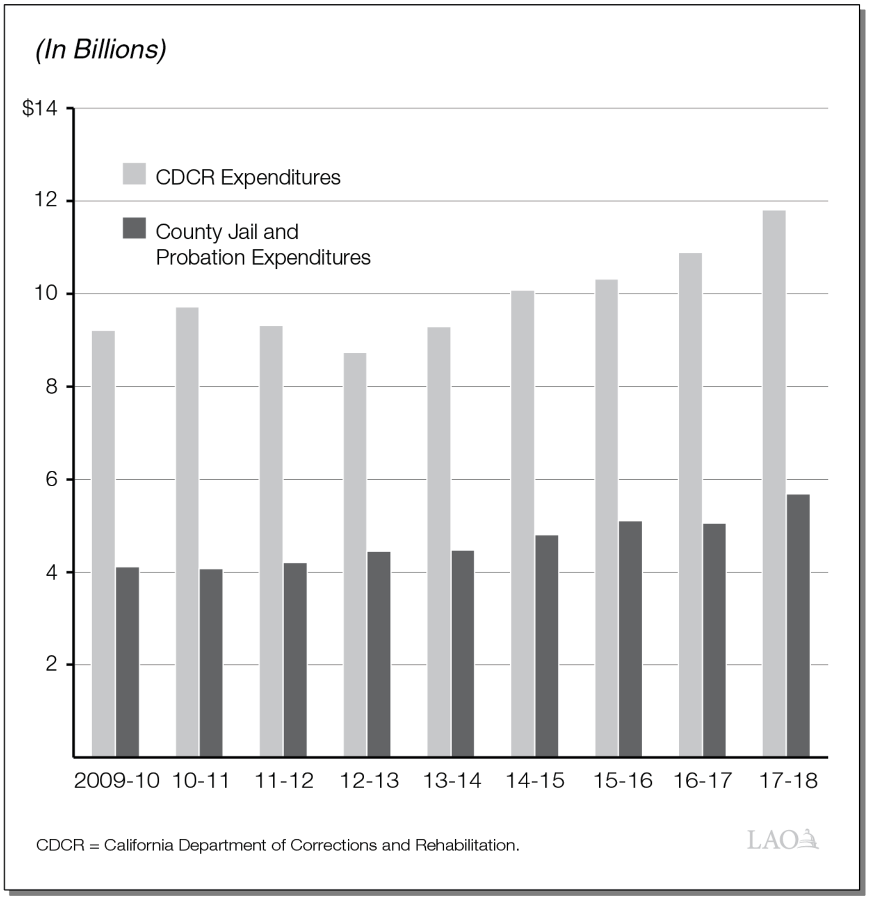


Total Adult Correctional Population Declined and Shifted to Counties

(Continued)

- Between 2009 and 2018, the total adult state and local correctional population declined by about 170,000 (24 percent).
- While the 2011 realignment shifted certain offenders from the state to the counties, the resulting increase in county populations was smaller than the corresponding decrease in the state population for various reasons. For example, Proposition 47 reduced the time that some realigned offenders serve at the county level.
- On net, the portion of the correctional population under county jurisdiction increased from 60 percent in 2009 to 66 percent in 2018.

Despite Population Declines, Spending has Generally Increased



- Between 2009-10 and 2017-18, CDCR expenditures increased by about $2.6 billion (28 percent)—twice the rate of inflation—primarily driven by three factors:
  - ***Compliance With Court Orders.*** The state had to: (1) expand prison capacity, in order to meet a court-ordered overcrowding limit and (2) make substantial improvements to inmate health care to comply with court orders.
  - ***Increased Employee Compensation Costs.*** Increases in pension costs and raises given to employees caused employee compensation costs to grow substantially.

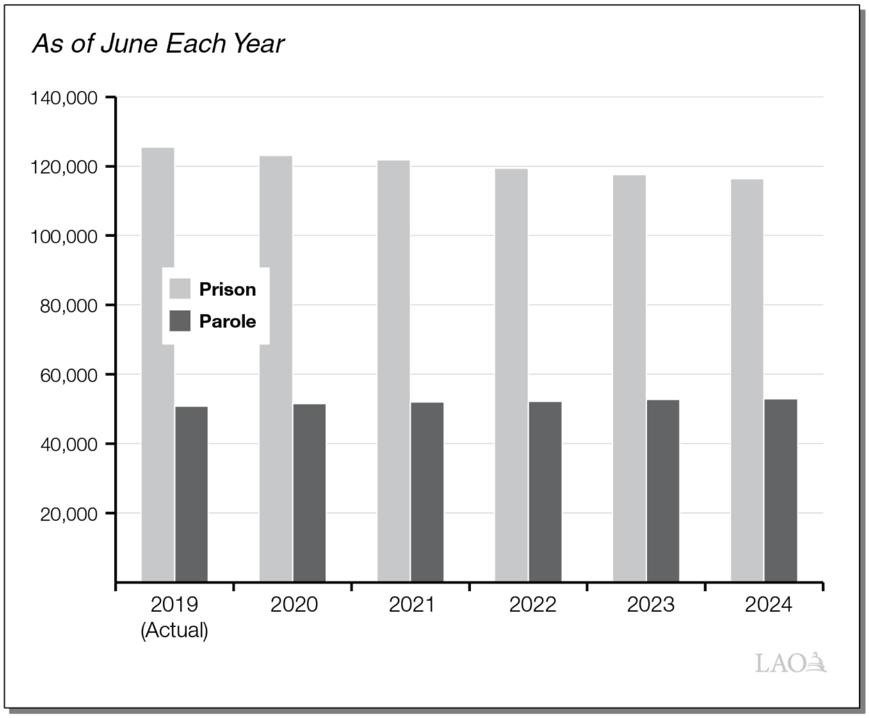
Despite Population Declines, Spending has Generally Increased

(Continued)

- – ***Spending on Costs Deferred During Fiscal Crisis.*** The state is now paying for costs that were deferred during the fiscal crisis, such as furloughing of correctional officers.

- Between 2009-10 and 2017-18, county correctional expenditures increased by about $1.6 billion (38 percent)—nearly three times the rate of inflation. This could be for various reasons, including the factors similar to those that increased CDCR spending. We note that some of these expenditures are supported by funds provided by the state, such as funding provided as part of the 2011 realignment.

State Prison Population Projected to Continue to Decline



- Between 2019 and 2024, the prison population is projected to decrease by about 9,100 (7 percent) and the parole population is expected to increase by about 2,100 (4 percent), primarily due to the effects of Proposition 57.
- We note, however, there is considerable uncertainty around these projections as they do not reflect various factors, including:
  - The effects of the novel coronavirus 2019 pandemic, which has reduced arrests and crime but also prompted CDCR to suspend rehabilitation programs through which inmates earn time off of their prison sentences.

State Prison Population Projected to Continue to Decline

(Continued)

- Proposals to: (1) further expand inmate credit earning, which could reduce the prison population by about 9,600 inmates by 2023-24 and (2) cap parole terms at 24 months for most parolees and create a parole earned discharge process, which could reduce the parole population by about 15,400 by 2023-24.

- Future population declines could have major implications for state spending on corrections. For example, the administration plans to close two prisons by 2022-23, which would create hundreds of millions of dollars in savings.

Exhibit C

Incarceration and Crime: Evidence from California's Public Safety Realignment Reform

By
MAGNUS LOFSTROM
and
STEVEN RAPHAEL

Recent reforms in California caused a sharp and permanent reduction in the state's incarceration rate. We evaluate the effects of that incarceration decline on local crime rates. Our analysis exploits the large variation across California counties in the effect of this reform on county-specific prison incarceration rates. We find very little evidence that the large reduction in California incarceration had an effect on violent crime, and modest evidence of effects on property crime, auto theft in particular. These effects are considerably smaller than existing estimates based on panel data for periods of time when the U.S. incarceration rate was considerably lower. We corroborate these cross-county results with a synthetic-cohort analysis of state crime rates in California. The statewide analysis confirms our findings from the county-level analysis. In line with with previous research, the results from this study support the hypothesis of a crime-prison effect that diminishes with increased reliance on incarceration.

Keywords: crime; incapacitation; incarceration; prison; Realignment; reform

Since the 1970s, the United States has experienced a pronounced increase in its incarceration rate. Between 1975 and 2007, the U.S.

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incarceration rate grew from 111 inmates per 100,000 residents to 506. If one adds the 773,000 jails inmates to the total for 2007, the overall incarceration rate increases to 762 per 100,000. This is by far the highest incarceration rate in the world (Raphael and Stoll 2013).

In every year since 2007, the U.S. incarceration rate has declined. As of December 31, 2013, the overall imprisonment rate stood at 700 per 100,000, the same rate as in 1999.[1] These declines are driven by sentencing reforms at the state level explicitly designed to reduce incarceration rates. Various factors drive these reforms. First, the fiscal impacts of the recent deep economic recession have induced state leaders to scour their budgets for potential savings. While corrections expenditures are often third or fourth on the list of budget categories commanding general fund dollars, the explosive growth in these expenditures in recent decades has come under increasing scrutiny.

Second, there is a palpable, bipartisan shift in public opinion regarding the use of prison as a tool for crime control and punishment. While liberal scholars and think tanks have deemed the use of incarceration in the United States as excessive for years (Jacobson 2006; Mauer 2006; Western 2006), several prominent conservatives have recently voiced similar opinions.[2]

Third, one large state, California, was forced by a federal court to reduce the size of its prison population. The reform driving the reduction went into effect in late 2011 and, by the end of 2012, decreased the state's incarceration rates to early-1990s levels.

A key question of interest to both policy-makers and criminal justice researchers concerns the effects of these reforms on crime rates. Existing research finds that incarcerating a convicted criminal offender does on average reduce crime through incapacitation and deterrence, with the lion's share of the reduction operating through incapacitation. However, these effects exhibit diminishing returns to scale that set in at quite low levels of incarceration and very small incapacitation/deterrence effects at the incarceration rates that currently characterize most U.S. states.

In this article, we assess the effects of a recent reform in California that caused a sharp and permanent reduction in the state's incarceration rate. Implemented on October 1, 2011, California's Public Safety Realignment (Assembly Bill [AB] 109) eliminated the practice of sending technical parole violators back to state prison, defined a series of offenses and offenders that are now punished with jail sentences rather than prison sentences, and greatly increased the ability and incentives for local criminal justice systems to make use of alternative sentences that rely less heavily on incarceration.

We exploit the large variation across California counties in the effect of this reform on county-specific prison incarceration rates. We find very little evidence of an effect of these reforms on violent crime and evidence of modest effects on property crime, auto theft in particular. These effects are considerably smaller than existing estimates in the literature based on panel data for periods of time when the U.S. incarceration rate was considerably lower. We corroborate these cross-county results with a synthetic-cohort analysis of state crime rates in

California relative to other states with comparable preintervention crime trends. This statewide analysis confirms our findings.

Prior Research on the Crime-Prison Relationship

The relationship between incarceration and crime is driven by three primary causal channels. First, prisons incapacitate the criminally active. Second, the threat of prison may deter criminal activity. Finally, prison may be transformative, either through rehabilitation or through a hardening of prison inmates, factors likely to alter future offending of former prison inmates. While the first two factors theoretically induce a contemporaneous negative relationship between criminal offending and incarceration levels, the latter channel likely induces a distributed lagged effect of incarceration on crime that can be either positive or negative.

A large body of research by criminologists based on inmate interviews estimates incapacitation effects through retrospective surveys. Careful reviews of this research summarize the findings in terms of the average number of serious felonies prevented per prison year served. The corpus of this body of research finds annual incapacitation effects of ten to twenty fewer serious felony offenses per prison year served (Marvell and Moody 1994; Spelman 1994, 2000). Most of this research, however, employs prisoner surveys fielded during the 1970s and 1980s when the U.S. incarceration rate was quite low. With the large increase in U.S. incarceration rates, the average age of prison inmates has increased, as has the proportion of inmates convicted of less serious offenses. Given the tendency of offending to decline with age, and heterogeneity in the criminal propensities of prison inmates (Raphael and Stoll 2013, chap. 7), one might expect lower incapacitation effects from studies employing more recent data.

A study by Owens (2009) suggests that this is the case. Owens analyzes the criminal activity of convicted felons who serve less time as the result of the 2001 discontinuance of the practice of considering juvenile records when sentencing adult offenders in the state of Maryland. Owens finds incapacitation effects roughly one-fifth the size of the incapacitation effects from earlier research.

Several studies exploit the unusual Italian practice of periodic, large, and sudden prisoner releases through collective clemencies and collective pardons to study the relationship between incarceration and crime. Barbarino and Mastrobuoni (2012) construct a panel dataset of crime and incarceration rates that vary by year and by Italian province and exploit province-level variation in pardon totals for all prisoner releases occurring between 1962 and 1995. The authors find sizable impacts of prison on crime of an order of magnitude similar to the early incapacitation research in the United States.

Buonanno and Raphael (2013) use relatively high-frequency crime and incarceration data at the national level as well as province-level variation to estimate the reverse-incapacitation effects caused by the August 2006 Italian mass prisoner release. The authors find felony incapacitation effects on the order of thirteen to seventeen serious offenses per year served. However, the authors present several sets of results indicative of diminishing marginal incapacitation effects. First, they show that incapacitation effects are the largest for those inmates who replace the pardoned and/or those who are returned to prison the soonest after the mass release. In addition, the authors find much larger incapacitation effects in provinces with lower pre-pardon incarceration rates relative to provinces with higher pre-pardon incarceration rates.

Vollaard (2012) estimates incapacitation effects for repeat offenders exploiting a change in Dutch sentencing policy. The author analyzes the impact of a sentence enhancement in the Netherlands targeted at repeat offenders defined as those with over ten prior felony convictions. In 2001, the Netherlands enacted an enhanced sentence of two years for such offenders, first allowing a small number of municipalities to experiment with the enhancement before nationwide application in 2004. The author finds very large incapacitation effects, on the order of fifty to sixty reported thefts prevented per year of incarceration. However, the author also finds that those municipalities that dipped further into the repeat-offender pool in the application of the sentencing enhancement experienced significantly smaller crime reductions per additional prison-year served.

The findings from these European studies suggest that diminishing crime-abating returns may set in at relatively low incarceration rates. For both countries, incapacitation effect estimates are comparable to or larger than estimates for the United States for data collected when the U.S. incarceration rate was comparable to that of Italy and the Netherlands. Most notably, this research finds incapacitation effects setting in quite quickly, with Buonanno and Raphael (2013) finding substantial declines in incapacitation effects at levels below 200 per 100,000 and Vollaard (2012) finding declining marginal criminality even among offenders with ten or more prior convictions.[3]

There are several studies of the crime-prison relationship based on U.S. panel data regressions. A key methodological hurdle that these studies must address concerns the likely simultaneous relationship between incarceration and crime. Specifically, while exogenous increases in the use of incarceration will incapacitate more people and perhaps provide a greater deterrent effect (creating a negative relationship between incarceration and crime), exogenous increases in crime for reasons unrelated to criminal justice policy will cause incarceration rates and crime to positively covary. Failing to account for the endogeneity of incarceration rates likely leads to crime-prison effects biased toward zero.

Levitt (1996) was the first to point out this identification problem and to propose a formal identification strategy. Using U.S. state panel data, Levitt exploits the fact that in years when states are under a court order to relieve prisoner overcrowding, state prison populations grow at relatively low rates, and finds two-stage least squares (2SLS) estimates of crime-prison elasticities that are considerably larger than comparable estimates from ordinary least squares (OLS) with a corrected property crime-prison elasticity of –0.3 and a violent crime-prison elasticity of –0.4.

Johnson and Raphael (2012) use an instrument for incarceration based on the difference between a state's current incarceration rate and the state's steady-state incarceration rate implied by observable admissions and release rates. The authors derive an empirical prediction regarding the impact of this difference on next-year's change in incarceration based on a theoretical model of the relationship between crime and incarceration. The authors analyze state level panel data for two time periods: 1978 to 1990 and 1991 to 2004. The former period is characterized by a relatively low incarceration rate (186 per 100,000), while the latter period is characterized by a much higher incarceration rate (396 per 100,000). For the early period, for an additional offender serving one year in prison, an estimated 2.5 felony violent offenses and 11.4 felony property offenses were prevented in that year.

The estimates for the latter time period, however, are considerably smaller. The comparable figures for crimes prevented per prison year served for the period 1991 through 2004 are 0.3 violent felony offenses and 2.7 felony property offenses. Raphael and Stoll (2013) reproduce this analysis with updated data for three time periods: 1977 through 1988, 1989 through 1999, and 2000 through 2010, with corresponding weighted-average state incarceration rates of 171, 349, and 449. This reanalysis finds very small prison-crime effects for the latter two time periods, but fairly large effects for the earliest time period, strongly suggestive of diminishing returns to scale. Liedke, Piehl, and Useem (2006) provide similar evidence with U.S. panel data.

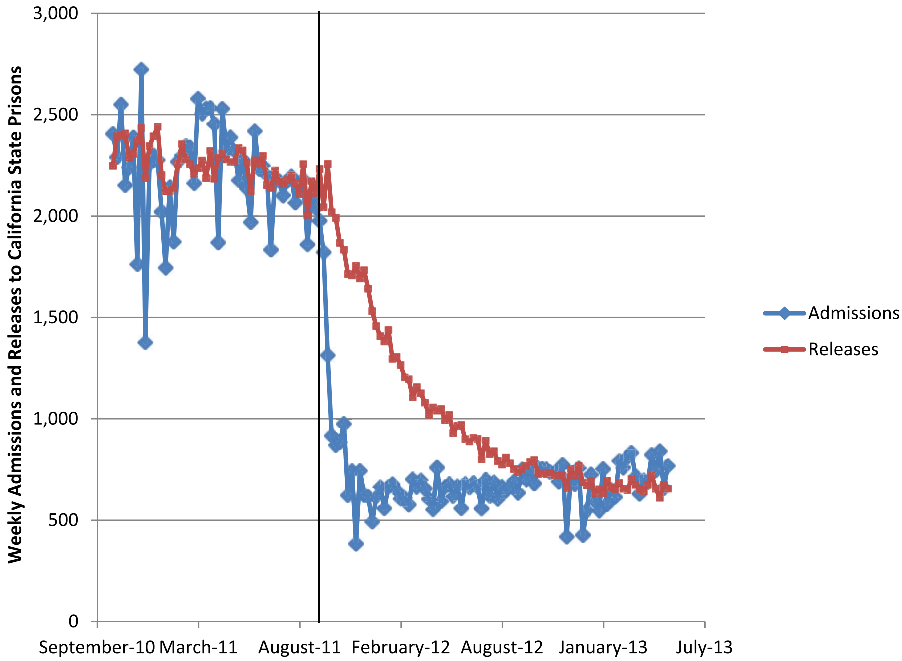
Unlike the Dutch and Italian studies, the U.S. panel data estimates represent joint incapacitation/deterrence effects associated with increases in incarceration, estimates that in theory should be larger than the estimates of pure incapacitation effects. Nonetheless, for recent years, empirical estimates find very small crime-prevention effects of marginal increases in incarceration. Given the trajectory of U.S. incarceration rates over the past three decades, this research has been based largely on variation within and between states in the rate of positive incarceration growth. In what follows, we present results from analysis of a single state experiencing a very large and relatively sudden decline in incarceration rates.

Description of California's Public Safety Realignment Reform and Our Empirical Strategy

In April 2011, the state of California enacted broad correctional reform legislation under the banner of Corrections Realignment. The legislation eliminates the practice of returning parolees to state prison custody for technical parole violation for all but a small set of the most serious offenders. The legislation also defines a group of nonserious, nonsexual, nonviolent offenders who upon conviction will serve their sentences in county jails. These offenders earn good-time credits at faster rates than they would within the state prison system and can be given split sentences that involve alternative monitoring within the community. More generally, judges are now afforded greater discretion to devise alternatives to confinement in the sentencing of these offenders.

The legislation was prompted by pressure from a federal three-judge court overseeing the California prison system, impaneled as a result of legal decisions

FIGURE 1
Weekly Admissions and Releases to California State Prisons, October 2010 through May 2013



in two lawsuits against the state filed on behalf of California prison inmates. In one (*Coleman v. Brown*), it was alleged that California was providing inadequate health care services to its prison population. In the other (*Plata v. Brown*), it was alleged that the system was providing inadequate mental health services. Both resulted in rulings in favor of the plaintiffs finding that prison overcrowding was the primary cause of the inadequate services. AB 109 (referred to in the state as "Corrections Realignment") was passed and implemented under threat of a federal court order to release up to 35,000 of the then 165,000 state prison inmates.

Realignment caused a relatively quick and large reduction in the California prison population driven primarily by a reduction in prison admissions. Figure 1 presents weekly admissions and releases to the California state prison system from October 2010 through May 2013. Through September 2011, weekly admissions oscillate around twenty-two hundred. With the implementation of Realignment, admissions drop discretely and permanently to roughly six hundred per week. Prior to Realignment's implementation, admissions and releases are in rough balance. Following the policy reform, releases fall as well, yet more slowly than the drop in admissions. The slower drop effectively created a period where admissions fell far short of releases, causing the overall prison population to decline.

FIGURE 2
California Total Prison and Jail Populations by Month, October 2010 through December 2013

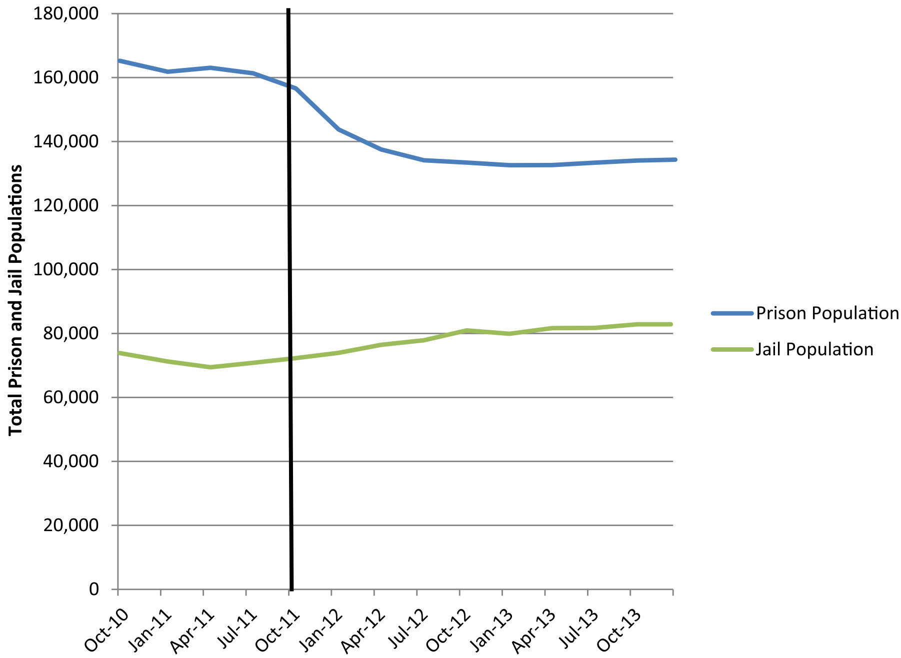
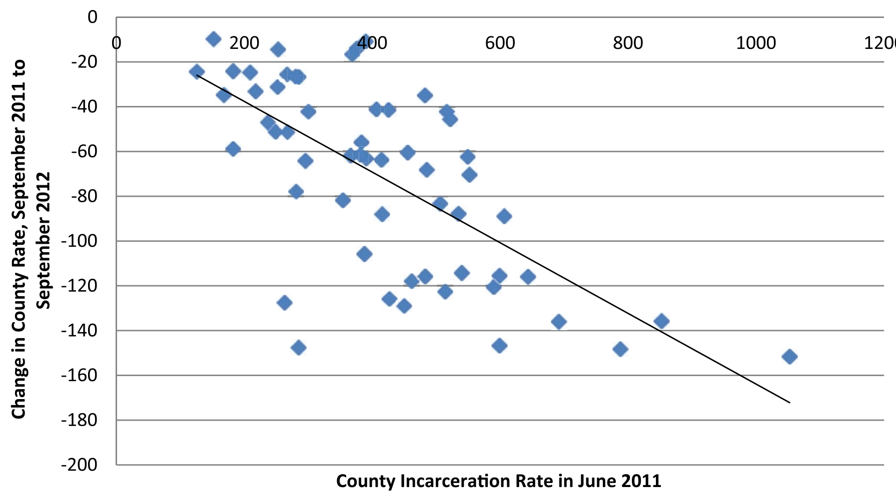


Figure 2 shows the impact of these changes in flows on the total prison population (measured at the end of each month). The figure reveals a clear drop in the state's prison population, with a total decline between September 2011 and May 2013 of 27,846. The effect of Realignment was felt immediately, with 12 percent of the decline occurring within one month of implementation, 46 percent within three months, 70 percent within five months, and 82 percent within seven months. The prison population stabilizes within one year. Expressed per 100,000 California residents, the prison incarceration rate declined from 426 to 348 (comparable to the 1991 rate).

Despite operating under a common state penal code, California counties vary considerably in their use of the state prison system. Not surprisingly, this variance in pre-Realignment incarceration rates naturally led to large cross-county differences in the impact of Realignment. Figure 3 documents this fact. The figure presents a scatter plot of the change in county-specific incarceration rates between September 2011 and September 2012 (roughly the first year of Realignment) against the county's incarceration rate in June 2011. The variation in both starting incarceration rates as well as the change in incarceration rate is quite remarkable. Regarding pre-Realignment incarceration rates, these rates vary from below 200 per 100,000 to over 1,000 per 100,000. Naturally, counties with lower prereform incarceration rates experienced much smaller declines in

FIGURE 3
Scatter Plot of the Pre-Post Realignment Change in County-Level Prison Incarceration Rates against the County-Level Prison Incarceration Rate in June 2011



their prison incarceration rates as a result of the reform. The changes in county-specific incarceration rates range from slight pre-post Realignment increases to declines of over 160 inmates per 100,000 residents.

Since our identification strategy exploits the intercounty variation displayed on the vertical axis of Figure 3, here we document the prereform differences between high-incarceration and low-incarceration counties. Table 1 presents some basic descriptive statistics for counties stratified into thirds according to pre-Realignment prison incarceration rates. The differences in the average number of county residents in a state prison per 100,000 county residents across these three strata are striking, with average rates of 234, 402, and 612. Moreover, these counties differ along several other notable dimensions. Poverty rates increase uniformly as we move from low- to high-incarceration rate counties. Moreover, counties with relatively high incarceration rates also have relatively high pre-Realignment violent and property crime rates.

We also find some evidence that public opinion in high-incarceration counties tends to be relatively less supportive of recent sentencing reforms, suggesting potential important ideological differences across counties. Specifically, the table shows the average proportion of county voters that support the 2012 California State Proposition 36, a proposed amendment that essentially scales back the scope of California's three-strikes law. Support for Proposition 36 is notably lower in high-incarceration-rate counties, with average values of 70.2, 65.2, and 62.4 percent in the bottom, middle, and top third counties, respectively.

Table 2 presents results from a simple linear regression of prereform county incarceration rates on prereform poverty rates, the proportion of county

TABLE 1
Characteristics of Counties Stratified by Their Pre-Realignment (June 2011) County-Specific Prison Incarceration Rate

	Bottom Third	Middle Third	Top Third
Incarceration rate, June 2011	234.1	401.7	611.7
Poverty rate, 2006 to 2010	10.9	14.3	17.6
Percentage voting for Proposition 36	70.2	65.2	62.4
Property crime rate 2011	1,292.8	1,354.8	1,768.1
Violent crime rate 2011	331.4	356.8	467.4

SOURCE: Data on county poverty rates come from the U.S. Census Bureau American Community Survey. The percent of county voters supporting proposition 36 comes from the California Secretary of State. Data on property and violent crime for 2011 come from agency-level crime counts provided by the California State Attorney General's office.

residents supporting Proposition 36, and the prereform violent and property crime rates. Both poverty as well as criminal justice ideology exhibit significant partial correlations with prereform incarceration rates. Interestingly, after accounting for these two factors, there is no partial correlation between crime and incarceration. The table also illustrates the magnitude of the coefficient estimates by calculating the implied effect of a variation in the explanatory variable equal to its interquartile difference. The implied effects suggest that both crime fundamentals as well as ideology were important independent determinants of county incarceration rates prior to the reform.

Our principal estimation strategy exploits this cross-county variation in the impact of Realignment. Specifically, we assess whether counties that have experienced larger declines in county-specific prison incarceration rates experience relatively large increases in crime rates. This analysis relies on estimation of various specification of the regression

$$\begin{aligned}\Delta Crime_{it} &= \alpha_i + \beta_t + \delta\Delta\, \mathrm{Pr}ison\ Incarceration\ Rate_{it} \\ &\quad + \gamma\Delta Jail\ Incarceration\ Rate_{it} + \varepsilon_{it},\end{aligned} \tag{1}$$

where i = (1, . . . ,57) indexes counties; t = (Oct 2011, . . .,Sep 2012) indexes the end month of the change; ΔCrime_{it} is the pre-post Realignment change in monthly crime rates; $\Delta\text{Prison Incarceration Rate}_{it}$ is the pre-post Realignment change in county prison incarceration rate; $\Delta\text{Jail Incarceration Rate}_{it}$ is the pre-post Realignment change in the county jail incarceration rate; α_i and β_t are county and month fixed effects, respectively; δ and γ are parameters to be estimated; and ε_{it} is a mean-zero error term. Before discussing how we characterize the pre-Realignment change, we offer some general comments about the variation used to identify the key coefficient of interest, γ.

First, the reform explicitly provides for the incarceration of nonviolent, nonsexual, and nonserious offenders in local jails as well as for discretion for local

TABLE 2
Cross-County Regression Examining the Correlates of the Pre-Realignment County-Specific Prison Incarceration Rate

Explanatory Variable	Regression Coefficients	25th Percentile of the Explanatory Variable	75th Percentile of the Explanatory Variable	Effect on Incarceration Rate of a Variation Equal to the Interquartile Range
Poverty rate, 2006 to 2010	18.24* (5.34)	10.9	17.5	120.4
Percent voting for Proposition 36	–7.11* (2.58)	60.7	70.9	–72.5
Property crime rate 2011	–0.023 (0.050)	1,134.2	1,782.3	–14.9
Violent crime rate 2011	0.168 (0.155)	258.2	479.1	37.1
R^2	.463	—	—	—
N	57	—	—	—

SOURCE: Data on county poverty rates comes from the U.S. Census Bureau American Community Survey. The percentage of county voters supporting Proposition 36 comes from the California Secretary of State. Data on property and violent crime for 2011 come from agency-level crime counts provided by the California State Attorney General's office.
NOTE: Standard errors are in parentheses.
*Statistically significant at the 1 percent level of confidence.

criminal justice officials to punish released prison inmates who violate the terms of their conditional releases with jail spells. In practice, this has led to the reduction in the prison population being partially offset by an increase in the population of county jails. Returning to Figure 2, we can observe this fact. The state's total jail population was roughly seventy-two thousand in the months prior to Realignment and then gradually increased to over eighty thousand over the first postreform year. Lofstrom and Raphael (2013) find a cross-institution substitution rate of about one for three—that is to say, each three-person reduction in a county's prison incarceration rate resulted on average in a one-person increase in the local jail incarceration rate. Moreover, most of this increase reflected increases in the number of convicted felons serving time in local jails, rather than an increase in jail incarceration for parole violators. Ultimately, we wish to answer whether an increase in the number of convicted offenders not in custody (i.e., on the street) impacts crime. Hence, it is vital that we control for the corresponding changes in jail incarceration rates in equation (1).

Second, equation (1) includes a complete set of month fixed effects corresponding to the end month of the change defining each observation. Including time fixed effects effectively nets out the overall state time trends for crime changes and identifies the prison-crime effects based on variation above and beyond what occurs for the state overall. Such an adjustment may net out any state-level change in general deterrence associated with the policy reform. Hence, we will ultimately corroborate our county-level analysis with a series of synthetic cohort estimates using variation at the state level.

Third, equation (1) includes a complete set of county fixed effects. Counties in California, and cities within the counties, vary considerably with regard to demographics, economic conditions, and local fiscal conditions. Most notably, several California cities vary in terms of their law enforcement staffing levels as well as changes in staffing levels over the time period we study here. By adding county fixed effects, we adjust for general trends in changes and identify the prison-crime effect based on within-county variation above and beyond county overall averages for the dependent and explanatory variables.

Our characterization of the change in the dependent and explanatory variables merits a detailed discussion. Absent a policy-induced shock to the prison population, such as the implementation of Realignment, one might expect prison incarceration and crime rates to positively covary. For example, higher crime leads to more arrests, which leads to a larger jail population of inmates awaiting trial and transfer to prison, which in turn leads to a larger prison population. The reform in question, however, should identify the causal link running from prison to crime rates, as the prison reduction is driven by a policy reform that, in turn, is driven by forces having nothing to do with state crime trends. Hence, our analysis must focus on isolating variation in the prison population that is attributable to the reforms ushered in by AB 109.

One possible manner of characterizing the change in crime and incarceration rates would be to calculate the changes for a given post-Realignment month relative to the comparable month a year previous. September 2012 is the last month for which the base month of the change would still lie within the prereform period. Focusing on the change relative to one year previous ensures that we are making comparisons relative to the same time last year and that any association we observe between the prison incarceration rate and crime is not being driven by particular effects of calendar month and potential heterogeneity in these effects across counties. Moreover, focusing on pre-post Realignment changes ensures that variation in the incarceration rate is primarily driven by the policy reform. Hence, our first strategy is to analyze the relationship between the change in county crime rates and county prison incarceration rates using the change in monthly rates for October 2011 through September 2012 relative to monthly crime rates one year previous.

A potential weakness of this strategy is that the change over the course of a full year may reflect underlying trends in crime and corrections that predate the implementation of Realignment. This is particularly problematic for the earlier months in our analysis, such as October through December 2011, when the majority of the period over which changes are measured lies within the pre-Realignment period. Variation in crime and incarceration over this earlier time period may be driven by exogenous shocks to criminal offending that create the simultaneity bias to which much research on the prison-crime effect has been devoted to correcting. In addition, several policy reforms in California predate AB 109, most of which were geared toward reducing the prison population to comply with the federal court order.[4] Given the policy activity prior to Realignment's implementation and other potential sources of variation in crime and incarceration rates, one might want to focus more tightly on time periods that isolate variation in incarceration rates, and consequent impacts on crime, more clearly driven by the 2011 reforms.

Hence, we also characterize the changes in crime and incarceration rates focusing on the more narrow time windows using the pre-post changes in monthly crime and incarceration rates relative to September 2011. Of course, focusing on changes relative to a single month introduces a new measurement problem. Namely, changes between September in one year and all subsequent calendar months from October through September of the following year may introduce bias associated with cross-county variation in the seasonality of crime. For example, tourist visits to San Francisco and Southern California beaches increase during the summer, a factor likely increasing crime rates. On the other hand, tourist visits to desert cities such as Palm Springs decline greatly during summer months, as does time out of doors among local residents due to the extreme heat. Hence, one might expect different monthly patterns in crime across California counties.

To address this issue, we modify our tightly focused change calculations to account for underlying seasonal variation in crime specific to counties. Specifically, we calculate the pre-post Realignment changes in incarceration and crime rates relative to September 2011 net of the comparable change in crime one year earlier. For example, our pre-post change ending in, say, April 2012 is calculate by first measuring the difference in crime and incarceration rates between April 2012 and September 2011 (the last prereform month) for each county, then calculating comparable differences for the period from September 2010 and April 2011, and then subtracting the latter change from the former. We refer to this characterization of the dependent and explanatory variables as our difference-in-difference specification.

In the presentation of our empirical results, we estimate various specifications of equation (1) using both characterizations (the year-over-year changes and the difference-in-difference changes) of the dependent and explanatory variables. We test for the sensitivity of our results to inclusion of the month and county fixed effects and the inclusion of the jail incarceration variable.

Data Description and Basic Crime Trends

The data for this project come from several sources. Crime data are provided by the California Criminal Justice Statistics Center (CJSC) within the Office of the California State Attorney General. Crime totals for part 1 felony offenses are reported by month and police agency. The data include county identifiers that permit summing total offenses by county and month.

TABLE 3
Annual Crimes per 100,000 State Residents for the Entire State of California, 2010, 2011, and 2012

Crime Category	2011	2012	2012	Δ, 2010–2011 (percent Δ)	Δ, 2011–2012 (percent Δ)	Δ, 2010–2012 (percent Δ)
Total violent	439.2	412.1	422.2	–27.0 (–6.2)	10.1 (2.5)	–16.9 (–3.9)
Homicide	4.8	4.8	4.9	–0.1 (1.7)	0.2 (3.7)	0.1 (1.9)
Rape	22.3	20.4	20.6	–1.9 (–8.6)	0.2 (1.0)	–1.7 (–7.7)
Robbery	155.6	144.2	148.5	–11.4 (–7.3)	4.3 (2.9)	–7.1 (–4.6)
Aggravated assault	256.4	242.8	248.2	–13.6 (–5.3)	5.5 (2.3)	–8.2 (–3.2)
Total property	2,692.0	2,586.4	2,756.9	–42.6 (–1.6)	170.5 (6.6)	127.9 (4.9)
Burglary	612.5	611.2	645.6	–1.3 (–0.2)	34.4 (5.6)	33.1 (5.4)
Larceny	1,608.1	1,585.0	1,668.3	–23.0 (1.4)	83.3 (5.3)	60.3 (3.7)
Vehicle theft	408.5	390.2	443.0	–18.3 (–4.5)	52.8 (13.5)	34.5 (8.5)

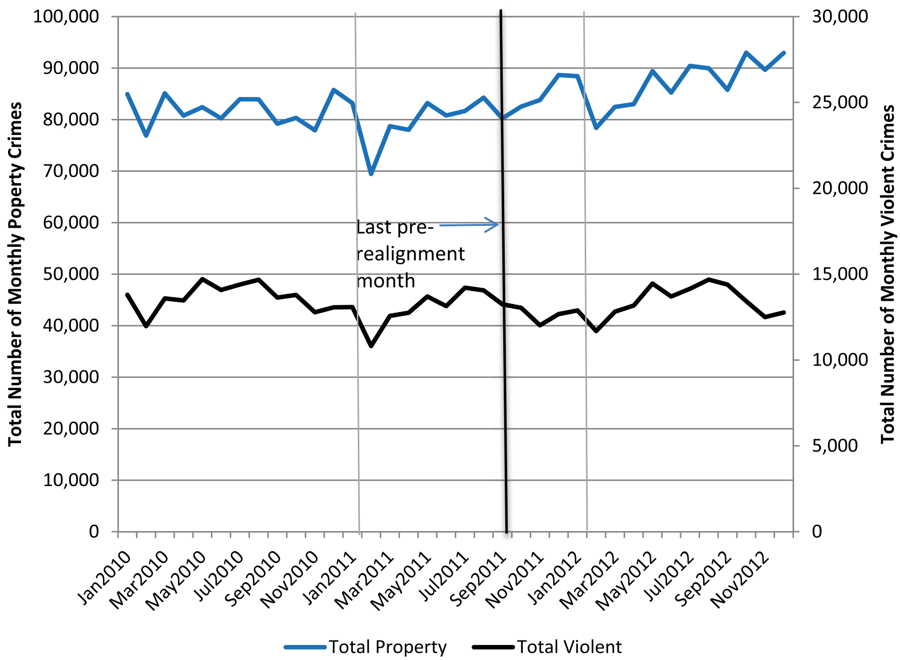
SOURCE: Crime data come from the California Criminal Justice Statistics Center. Population data are from the U.S. Census Bureau and measure the state population as of July in each calendar year.

Monthly data on county jail populations come from the California Jail Profile Survey, administered and maintained by the California Board of State and Community Corrections. To calculate jail incarceration rates, we use average daily population figures for each county and each month. While there are fifty-eight counties, one small rural county (Alpine) does not maintain its own jail system. Hence, nearly all of our analyses (with the exception of the aggregate trends presented shortly and our synthetic cohort analysis to follow) focus on the fifty-seven counties with independent jail systems.

The California Department of Corrections and Rehabilitation (CDCR) calculates prison totals by county of commitment only intermittently and hence does not publish the monthly totals we need to implement our estimation strategy. However, CDCR has provided us with weekly admissions and releases to the system by county and by controlling offense for the period ranging from October 2010 through May 2013. The difference between cumulative admissions and releases between any two dates for a given county provides the change in the incarceration total. We use this strategy to tabulate the change in incarceration by county between any two months, using the latest date within each month as the starting and end points. To convert to rates, we normalize by the average of the county population estimates for the two calendar years straddled by the change.

Before presenting our formal estimation results, we first present some basic descriptive statistics describing recent crime trends in the state. Table 3 presents annual crime rates for 2010, 2011, and 2012. Between 2010 and 2011, all crime rates decline, with fairly large percentage declines in overall violent crime and a more modest decline in property crime. From 2011 to 2012, crime rates increase

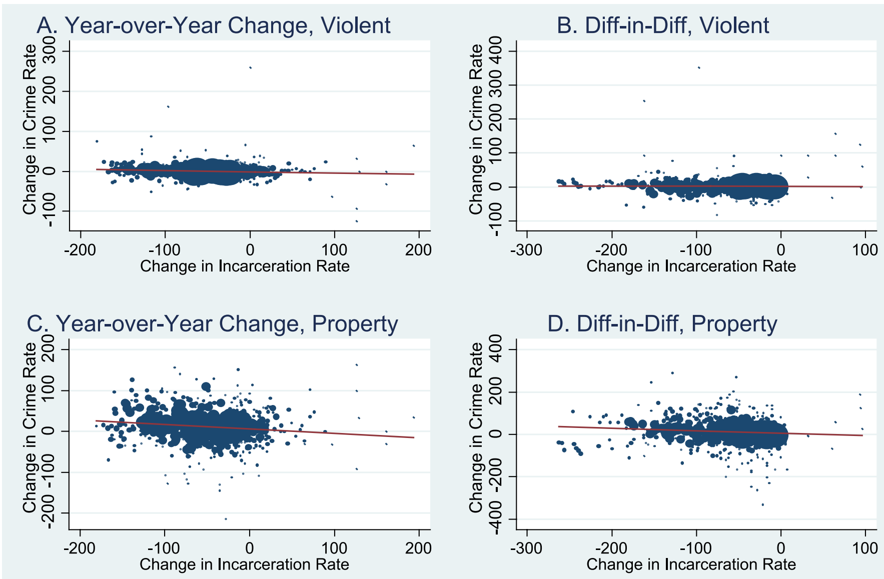
FIGURE 4
Total Monthly Violent and Property Crimes in California, January 2010 through December 2012



uniformly, with relatively small increases in violent crime but more pronounced increases in property crime. The overall violent crime rate increases by 2.5 percent while the overall property crime rate increases by 6.6 percent. We observe the largest percentage increase in motor vehicle theft (13.5 percent). Comparing 2012 crime rates to 2010 crime rates, violent crime in 2012 is generally lower than violent crime in 2010, though murder rates are slightly higher. Property crime rates, however, are uniformly higher with the percentage difference relative to 2010 greatest for auto theft (8.5 percent).

Figure 4 displays monthly total crime for January 2010 through December 2012, a comparison that permits a tighter visual depiction of the timing of the reform. For reference, the figures include two thin vertical lines marking the beginning of each calendar year and a thick vertical line marking the last pre-reform month (September 2011). Figure 4 does not reveal any visible increase in violent crime with the timing of Realignment's implementation, though the annual monthly violent crime totals in 2012 appear slightly elevated relative to comparable months in 2011. However, Figure 4 reveals a gradual and sustained increase in total property crimes following the reform. During the prereform period, monthly property crime totals oscillate around eighty thousand incidences per month. Following Realignment, monthly incidents drift upward toward ninety thousand incidents per month.

FIGURE 5
Scatter Plots of Two Alternative Measures of the Change in Violent and Property Crime at the County Level against Corrsponding Changes in County-Specific Prison Incarceration Rates



Finally Figure 5 visibly depicts the cross-county relationship between changes in crime rates and changes in prison incarceration rates. For our two alternative characterizations of the changes in incarceration and crime, the figure presents scatter plots of the crime changes against the incarceration changes for each of the fifty-seven counties and for each of the twelve postreform months in our analysis period. The scatter plots are weighted by county population and include a line depicting a population-weighted bivariate regression between the two variables. Figures 5A and 5B depict the change in violent crime rates against the change in incarceration rates. Despite very large changes in incarceration rates, we observe little evidence of relative increases in violent crime. The fitted regression for the year-over-year change exhibits a modest negative slope, while the regression line using the difference-in-difference characterization has a slope that is basically zero. The scatter plots for property crime rates exhibit more evidence of a negative slope.

Cross-County Empirical Results

Table 4 presents estimations results for various specifications of equation (1). We present results for each of our characterizations of the pre-post change in the

TABLE 4

Regression Estimates of the Prison-Crime Effects for Overall and Individual Property and Violent Crimes

	No Fixed Effects, no Control for Jail Change	No Fixed Effects, Control for Jail Change	Month Effects	County Effects	Month and County Effects
Violent (overall)					
Year-over-Year	–0.033* (0.008)	–0.034* (0.007)	–0.019** (0.009)	–0.040* (0.013)	0.009 (0.016)
Diff-in-Diff	–0.006 (0.022)	–0.017 (0.019)	0.005 (0.025)	–0.041* (0.014)	0.009 (0.017)
Murder					
Year-over-Year	–0.0004 (0.0004)	–0.0004 (0.0004)	–0.0005 (0.0005)	–0.001 (0.001)	–0.001 (0.001)
Diff-in-Diff	–0.001 (0.001)	–0.0017 (0.0012)	–0.0016 (0.0014)	–0.001 (0.001)	–0.001 (0.002)
Rape					
Year-over-Year	–0.001 (0.001)	–0.001 (0.001)	0.000 (0.001)	–0.002 (0.002)	–0.002 (0.004)
Diff-in-Diff	0.003 (0.002)	0.004*** (0.003)	0.006*** (0.003)	–0.002 (0.002)	–0.001 (0.003)
Robbery					
Year-over-Year	–0.009*** (0.005)	–0.012** (0.005)	–0.005 (0.005)	–0.015*** (0.008)	0.004 (0.010)
Diff-in-Diff	0.001 (0.010)	–0.003 (0.010)	0.006 (0.013)	–0.016*** (0.008)	0.004 (0.010)
Aggravated assault					
Year-over-Year	–0.022* (0.007)	–0.021* (0.006)	–0.014*** (0.008)	–0.022*** (0.012)	0.008 (0.014)
Diff-in-Diff	–0.008 (0.015)	–0.017 (0.012)	–0.004 (0.015)	–0.023*** (0.012)	0.007 (0.015)
Property (overall)					
Year-over-Year	–0.107*** (0.053)	–0.164* (0.056)	–0.122*** (0.070)	–0.162* (0.053)	–0.089 (0.087)
Diff-in-Diff	–0.117** (0.068)	–0.183** (0.073)	–0.159*** (0.086)	–0.165* (0.054)	–0.091 (0.089)
Burglary					
Year-over-Year	–0.005 (0.019)	–0.012 (0.022)	–0.013 (0.030)	–0.004 (0.027)	0.019 (0.041)
Diff-in-Diff	–0.032 (0.031)	–0.079* (0.024)	–0.076* (0.028)	–0.003 (0.027)	0.024 (0.042)
Larceny					
Year-over-Year	–0.045 (0.033)	–0.073*** (0.039)	–0.047 (0.040)	–0.078*** (0.042)	–0.010 (0.073)
Diff-in-Diff	–0.016 (0.038)	–0.035 (0.039)	–0.014 (0.044)	–0.081*** (0.043)	–0.013 (0.075)
Motor vehicle theft					
Year-over-Year	–0.057* (0.018)	–0.080* (0.017)	–0.062* (0.020)	–0.080* (0.023)	–0.099* (0.036)
Diff-in-Diff	–0.068* (0.024)	–0.069** (0.029)	–0.069*** (0.037)	–0.081* (0.023)	–0.103* (0.037)

NOTE: Standard errors are in parentheses. Standard errors are calculated assuming clustering by county. Each regression contains 684 county-month observations. See main text for description of the alternative characterizations of the dependent and explanatory variables.

*Statistically significant at the 1 percent level of confidence. **Statistically significant at the 5 percent level of confidence. ***Statistically significant at the 10 percent level of confidence.

dependent and explanatory variables. Within each characterization, we present results for five specifications, varying whether we control for the contemporaneous change in the local jail population and the mix of county and month fixed effects. The final specification corresponds to the full model specified in equation (1). All regressions are weighted by county population and the calculated standard errors are clustered by county.

Beginning with the results for violent crime, the year-over-year change models yield some evidence of an adverse effect of the decline in the prison population on crime rates. Adjusting for the change in the jail population but not controlling for month or county fixed effects yields a statistically significant estimate of 0.034 violent crimes per 100,000 prevented per month for each one-person increase in the prison incarceration rate. This estimate, however, is quite sensitive to adjusting for month fixed effects, with the magnitude of the effect dropping by nearly half when month effects are added to the specification. The model controlling for county fixed effects only yields a slightly higher and significant estimate of 0.04 crimes prevented per 100,000. The final specification controlling for both month and county fixed effects yields a positive statistically insignificant coefficient.

The models employing the difference-in-difference characterization yield weaker evidence of an impact of variation in county incarceration rates on county violent crime rates. In several specifications, the coefficients are not the expected sign and in four of the five specifications are statistically insignificant. The one specification where the coefficient has the expected sign and is significant is when we control for county fixed effects only and the change in the local jail incarceration rate. However, adding month effects to the specification, essentially adjusting for state-level trends, reduces the coefficient to zero.

The results for property crime reveal more consistent evidence of a prison-crime effect. Beginning with the year-over-year models, the coefficient estimates in all five specifications are roughly consistent with one another (ranging from –0.089 to –0.164) and statistically significant in four of the five specifications. Controlling for month effects does attenuate the coefficient somewhat, suggesting that part of the relationship in the unadjusted data reflect broader forces influencing the entire state. The difference-in-difference models yield slightly higher estimates (ranging from –0.091 to –0.183) and are all statistically significant with the exception of the results from the full specification.

The magnitudes of these results are consistent with those from previous research on the prison-crime effects. For example, Johnson and Raphael's (2012) analysis of state-level panel data found that for the period 1992 to 2004, each prison year served prevented 2.6 property crimes and 0.4 violent crimes. Raphael and Stoll (2013) update these results for the more recent period from 2000 to 2010 and find that each prison year served prevented 2.05 reported property crimes per year and 0.18 reported violent crimes.

To render the results in Table 4 comparable, we must multiply each coefficient by 12 since the estimation results here pertain to monthly crime. The largest point estimate for violent crime suggests that each prison month served prevents 0.041 violent incidents, implying that each prison year served prevents 0.5 violent incidents. For property crime, the largest point estimate suggests that each prison month served prevents 0.183 property crimes, implying that 2.2 reported property crimes per year are prevented per prison year served. Note, both estimates are likely too high as we have selectively chosen the largest coefficients from the table, neither of which is adjusted for state-level crime trends. Nonetheless, the implied effect sizes are consistent with those from previous research and suggest impacts per prison year served that are considerably lower than estimates from time periods in the United States when the incarceration rate was much lower.

Table 4 also presents comparable estimation results for individual violent crimes. For murder and rape, we find no evidence of an effect of Realignment. There is not a single negative and statistically significant coefficient. For robbery, we find some evidence of small adverse effects in some specifications. However, the robbery coefficient never survives controlling for month fixed effects, our preferred specification. We do find more consistent evidence of relative increases in aggravated assault rates in counties experiencing relatively large reductions in incarceration rates in the year-over-year change models. The difference-in-difference models, however, generally find no effect of Realignment on aggravated assault.

In Table 4 we also present comparable results for individual property crime rates. For burglary and larceny, results are inconsistent across specifications and across our alternative characterization of pre-post reform change. Surprisingly, the evidence of an effect of the reform on larceny theft is the weakest. In contrast, we find robust evidence that Realignment-induced declines in the prison population have caused increases in motor vehicle theft. For both change characterizations, the coefficients estimates are statistically significant in each specification, do not appear to be sensitive to controlling for month effects, and are comparable in magnitude across specifications. Interestingly, the largest point estimates come from the complete model specifications inclusive of county and year fixed effects. The complete model results suggest each prison year served prevents roughly 1.2 motor vehicle thefts.

To summarize, the cross-county results suggest that at most each prison year served among those not incarcerated as a result of Realignment prevent on average of 0.5 violent felony offenses and roughly 2 property offenses. Our complete model specifications that adjust for time trends and county-specific factors suggest even smaller effects, with no impact on violent crime and an effect on property crime limited to auto theft of 1.2 incidents per year.

Cross-State Comparison Using Synthetic Cohort Analysis

Thus far, we have relied on cross-county differences in the impact of Realignment on county-specific prison incarceration rates to study the effects on crime. One might contend that focusing on the effects at the county level may be controlling away any change in general deterrence statewide associated with the change in the penal code. While the great county-level heterogeneity in the application of the common penal code prior to Realignment might call such concerns into question, it certainly is possible that the very public and high-publicity proceedings surrounding the reform may have altered expectations regarding punishment severity and altered criminal behavior as a result. An additional concern involves the relatively small geographic units of analysis (counties) and the fact that in many urban areas, county borders are relatively arbitrary boundaries that do not demarcate meaningful social ecological divisions. To the extent that one county's Realignment caseload spills over into another county's crime rate, our county-level regression analysis may underestimate the effects of the reform on crime rates.

To complement our county-level analysis, here we present results using state-level crime data for California and the rest of the nation. Analyzing state-level data will minimize the bias associated with deterrence, as we are looking for an overall effect for the state in question. Moreover, California's major population centers do not cross state boundaries, and hence issues of spillover are less of a concern.

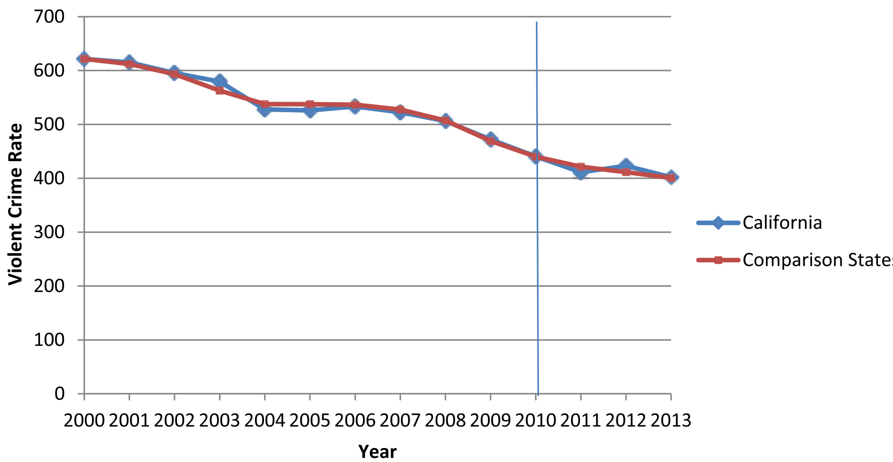
Using data from the Federal Bureau of Investigation's Uniform Crime Report for the period 2000 to 2013, we employ the synthetic control approach of Abadie, Diamond, and Hainmueller (2010). The synthetic control approach identifies a group of states that when averaged have crime trends that are as close as possible to that of California for the preintervention period. Comparison of the pre-post reform change in crime rates in California against the comparable change for "synthetic California" provides a difference-in-difference estimate of the effect of Realignment on crime. Below we present such comparison using several alternative definitions of the pre and post period.

To draw a statistical inference from this exercise, we estimate a comparable difference-in-difference for each state in the nation as if the state had experienced a Realignment reform in 2011. Since the other forty-nine states did not implement such reforms, these forty-nine estimates provide a sampling distribution of "placebo" estimates against which the estimate for California can be compared. If the difference-in-difference for crime in California is positive and in the extreme tail of the distribution of estimates from the placebo distribution, we would conclude that Realignment indeed impacted state incarceration rates.[5]

Figure 6 presents violent crime rate trends for California and for our "synthetic California." The synthetic comparison estimator yields a very good match for violent crime rates in the pretreatment period, with little visible difference between California and its synthetic comparison group. Most notably, there is little evidence of a relative increase in violent crime in California in 2012 and 2013. Figure 7 provides a comparable figure for property crime. Again, the pretreatment crime trends for California and synthetic California are quite similar. Here, however, we observe divergence in crime trends, with slightly higher crime in California in 2011 and a wider differential in 2012 and 2013.

Our formal estimates from this analysis for overall violent and property crime rates (not shown but available upon request) reveal that the relative increases in violent crime in California rank at most fourteenth in the placebo distribution and never yield a p-value less than .28. The relative increase in property crime consistently scores among the top seven estimates in the placebo distribution, yielding a marginally significant effect in the comparison relative to 2010. The point estimates are generally insensitive to the chosen preintervention comparison period, though the comparisons relative to 2010 yield the smallest estimates.

FIGURE 6
Violent Crime Rate Trends in California and Synthetic California, with Synthetic Comparison Group and Weighted Identified by Matching on Violent Crime Rates for Each Year between 2000 and 2010

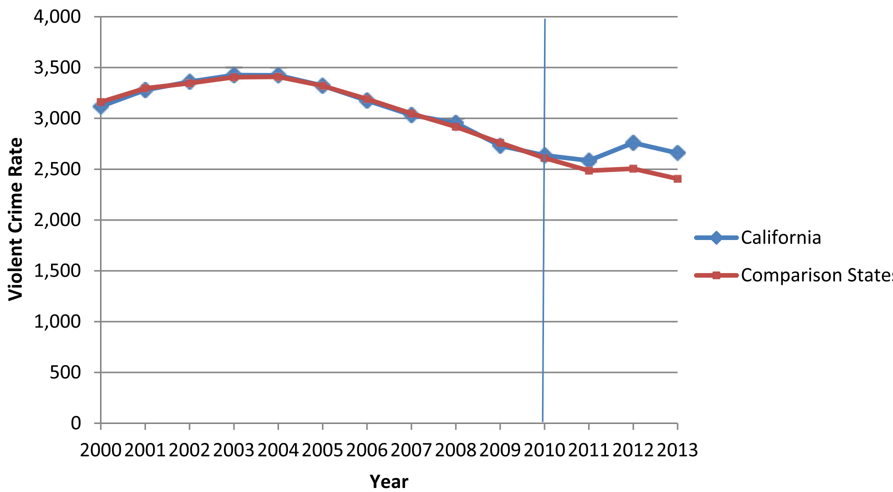


NOTE: The matched comparison states (with estimated weights in parentheses) are Florida (0.338), Maryland (0.161), Montana (0.068), New York (0.214), Rhode Island (0.191), and South Carolina (0.029).

Table 5 presents results for individual violent crimes. We show the difference-in-difference estimates using the average of 2012 and 2013 as the post-period and three different pre-periods: 2006 to 2010, 2008 to 2010, and 2010 alone. Similar to the findings from our cross-county analysis, we find no evidence of a relative increase in murder rates or the rate of rape/sexual assault. While the point estimates for aggravated assault and robbery are positive, the California estimates lie well within the distribution of placebo estimates for the other forty-nine states. Hence, consistent with our cross-county results, we find no evidence in the state-level analysis of the large reduction in California incarceration rates on violent crime. For property crime, there is no evidence of a statistically significant relative increase in burglary or larceny theft. However, the relative increase in motor vehicle theft is pronounced and the largest among the distribution of placebo estimates.

How do these results compare to our findings from the cross-county analysis? Consistent with our county analysis, we find no evidence of an increase in violent crime in the synthetic cohort results. We do, however, find quite robust evidence of an impact on motor vehicle theft. When expressed as an impact per prison year not served, the results from the synthetic cohort estimator are remarkably close to those from the cross-county analysis. In 2010, the California state prison incarceration rate stood at approximately 444 per 100,000. For 2012 and 2013, the state's incarceration rate was roughly 354 per 100,000. Hence, for the pre-post comparison period with 2010 as a base year, the state's incarceration rate declined

FIGURE 7
Property Crime Rate Trends in California and Synthetic California, with Synthetic Comparison Group and Weighted Identified by Matching on Violent Crime Rates for Each Year between 2000 and 2010



NOTE: The matched comparison states (with estimated weights in parentheses) are Colorado (0.033), Georgia (0.001), Kentucky (0.133), Massachusetts (0.032), Nevada (0.163), Tennessee (0.075), West Virginia (0.041), and Wyoming (0.522).

by 90 per 100,000. Lofstrom and Raphael (2013) find that one of every three realigned inmates is reincarcerated in county jails. Taking this into account would yield an increase in the number of former inmates "on the street" of roughly 60 per 100,000. The estimate in Table 5 for motor vehicle theft suggests that relative to 2010, motor vehicle thefts per 100,000 in California increase by 72 relative to synthetic California. This implies an auto thefts per year of prison not served of 1.2, a figure remarkably close to our estimates from the cross-county analysis.

Conclusion

We find that the reduction in California's prison population caused by Realignment modestly increased property crime primarily through motor vehicle thefts but had little effect on violent crime. Results from cross-county analyses are roughly consistent with a synthetic-cohort analysis of aggregate state crime trends. Our estimates suggest that at California's pre-Realignment incarceration rate, for an additional offender serving one year in prison, roughly one to two property crimes per year and little to no violent crime are prevented.

Are these effects large? There are a number of ways to answer this question. First, we can compare our results to those from previous research. Not

TABLE 5

Estimated Impact of Realignment on Violent and Property Crimes Using the Synthetic Control Method

	Violent Crimes											
	Murder			Rape			Robbery			Aggravated Assault		
	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference
Pre-period	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Prereform diff	–0.05	–0.04	–0.04	0.02	0.01	0.00	–0.01	0.36	1.10	–5.46	–6.32	–5.32
Postreform diff, 2012–2013	0.03			–0.71			5.14			5.44		
Diff-in-Diff	0.08	0.07	0.07	–0.73	–0.72	–0.71	5.16	4.78	4.05	10.91	11.77	10.76
Placebo test, rank	17	18	16	28	26	29	12	15	18	13	12	10
p-value (one tail)	.347	.367	.327	.571	.531	.592	.245	.306	.367	.267	.245	.204

	Property Crimes								
	Burglary			Motor Vehicle Theft			Larceny Theft		
	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference
Prereform diff	1.24	1.22	1.19	32.33	60.22	72.56	–7.95	–9.97	0.13
Postreform diff, 2012–2013	45.83			144.45			20.66		
Diff-in-diff	44.59	44.61	44.64	112.12	84.22	71.89	28.61	30.63	20.53
Placebo test, rank	11	11	10	1	1	1	15	15	18
p-value (one tail)	.224	.224	.204	.020	.020	.020	.306	.306	.367

surprisingly given the magnitude of the quick and substantial drawdown in California's prison population (of about 17 percent during the first year of Realignment), there are no comparable single-state studies for the United States. Our review of panel data research in the United States using different methods and different time periods of analysis suggests that the amount of crime prevented per prison year served during the 1970s and 1980s is many multiples the effect sizes that we document here. By contrast, more recent panel data research using post-1990 data finds effect sizes in line with our findings for California. Hence, relative to the effect sizes from times past, the estimated prison-crime effects here are small.

An alternative manner of characterizing these results would be to ask whether the returns in terms of crimes prevented outweigh the budgetary or, better yet, the complete social costs of incarcerating these marginal offenders. Heaton (2010) provides a summary of the findings from research on the costs of crime. With our estimates of the effect of Realignment on crime, estimates of the costs of crime summarized in Heaton, and estimates of the costs of incarceration in California, we can perform an analysis of the returns on the state's incarceration investment. Our preferred empirical results suggest that each prison year served prevents 1.2 auto thefts. Heaton's summary implies that each auto theft costs on average \$9,533 (in 2013 dollars). This suggests that each prison year served for those who as a result of Realignment are no longer incarcerated prevents \$11,783 in crime related costs. The California Legislative Analyst's Office estimates that the annual cost of incarcerating a prison inmate in California is \$51,889.[6] This suggests a return of 23 cents on the dollar. Incorporating some of the more difficult to price social costs in the calculation would certainly lower the return even further.

The simple cost-benefit analysis discussed above is useful for thinking about whether on the margin the social expenditures we are making are justified. However, such analysis considers the effectiveness of a particular policy intervention in isolation, without considering what could be achieved by reallocating the saved resources toward other uses. For example, it may be the case that a reduction in incarceration absent some other policy intervention may generate small increases in property crime. However, if the money saved from reduced prison expenditures were channeled into alternative and perhaps more cost-effective crime control strategies, increases in crime need not be the end result. Moreover, to the extent that alternative crime-control tools are at least as effective as incarceration, maintaining low crime rates would not require additional public expenditures.

In characterizing the magnitude of our results, we could ask whether there are other interventions that generate a higher return per dollar spent. Perhaps the most obvious policy tool with the strongest research base regarding impacts on crime concerns the expansion of local police forces. There is considerable empirical evidence of the general effectiveness of higher police staffing levels on crime (Chalfin and McCrary 2012; Di Tella and Schargrodsky 2004; Evans and Owens 2007; Corman and Mocan 2000). These studies consistently find relatively large effects of expanding city police forces on local crime rates. Perhaps the most rigorous analysis of the effects of additional police on crime is provided in a recent study by Chalfin and McCrary (2013). In an analysis of the period 1960 through 2010 of medium to large U.S. cities, the authors find substantial and sizable effects of hiring additional police officers on crime rates, with notably statistically significant effects on very serious violent crimes. The empirical results in their analysis imply that each additional police officer reduces annual crime by 1.3 violent crimes and 4.2 property crimes. In an analysis of the costs and benefits of police expansion, the authors conclude that each dollar invested in additional policing generates $1.60 in crime savings.

Of course, we have discussed only one possible alternative intervention (higher police staffing), but many alternative policy tools could and should be explored by researchers and policy-makers. Such alternatives that may pay immediate returns include alternative systems of managing probationers and parolees, including swift-and-certain yet moderate alternative sanctions systems such as Hawaii's Opportunity Probation with Enforcement (HOPE) intervention, or high-quality cognitive-behavioral therapy interventions for adult offenders. Interventions that may take a few years to bear fruit yet ultimately result in less crime and fewer offenders include early childhood human capital interventions and targeted interventions for high-risk youth.

Notes

1. Bureau of Justice Statistics, Key Statistics, "Estimated supervision rate for persons under state, federal, and local correctional supervision, 1980–2013." Available from www.bjs.gov.

2. For example, see Gingrich and Nolan (2013).

3. The pre-2006 pardon Italian incarceration rate stood at roughly 100 per 100,000 (Buonanno and Raphael 2013). The Dutch incarceration rate in 2004 stood at roughly 124 per 100,000 (International Centre for Prison Studies, http://www.prisonstudies.org/info/worldbrief/, accessed June 15, 2012). Both are inclusive of pretrial populations and inmates serving short sentences.

4. For example, in January 2010, California instituted financial incentives for localities to reduce probation revocation rates. In late 2009 the state made it more difficult to revoke the parole of low-risk parolees. Combined, these two reforms reduced the prison population between January 2010 and September 2011 from 167,694 to 160,482.

5. See Bohn, Lofstrom, and Raphael (2013) for an application of this method to an immigration policy intervention.

6. Given the extreme capacity constraints faced by the state and the standing court order, this average cost is likely below marginal cost, given that increasing the population clearly requires new facilities at this point.

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Exhibit D

Can We Downsize Our Prisons and Jails Without Compromising Public Safety?

Findings from California's Prop 47

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Research Summary

Our study represents the first effort to evaluate systematically Proposition 47's (Prop 47's) impact on California's crime rates. With a state-level panel containing violent and property offenses from 1970 through 2015, we employ a synthetic control group design to approximate California's crime rates had Prop 47 not been enacted. Our findings suggest that Prop 47 had no effect on homicide, rape, aggravated assault, robbery, or burglary. Larceny and motor vehicle thefts, however, seem to have increased moderately after Prop 47, but these results were both sensitive to alternative specifications of our synthetic control group and small enough that placebo testing cannot rule out spuriousness.

Policy Implications

As the United States engages in renewed debates regarding the scale and cost of its incarcerated population, California stands at the forefront of criminal justice reform. Although California reduced its prison population by 13,000 through Prop 47, critics argue anecdotally that the measure is responsible for recent crime upticks across the state. We find little empirical support for these claims. Thus, our findings suggest that California can downsize its prisons and jails without compromising public safety.

Keywords

criminal justice reform, crime, prison downsizing, decarceration, California

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Speaking on the promise of downsizing prisons, Joan Petersilia (2016) recently distinguished between symbolic speechmaking, which is easy, and actual reform, which "is about as easy as bending granite" (p. 9; see also Petersilia and Cullen, 2015). Indeed, scholars have long made the distinction between "policy talk" and "policy action" (Tyack and Cuban, 1995), especially in the context of criminal justice reform in the United States. Yet in recent years, policy action, in fact, may be a good way to characterize many of the changes that have occurred in America's criminal justice system. Indeed, Petersilia (2016: 8) also noted:

> We are very likely at a transformative moment in criminal justice reform. There is great optimism that the United States is making a decisive move away from the harsh punishment policies that characterized the last 30 years. Prison growth has largely stopped, some states are closing prisons, and Congress and most legislatures are enacting policies that reduce prison sentences for drug crimes and other nonviolent offenses.

California has been at the epicenter of these changes. Perhaps more than any other state, California is immersed in a period of fundamental reform to its criminal justice system. In just a few short years, the state has passed a series of senate bills and propositions, most of which are intended to reduce its massive prison population. So far, they seem to be working. A recent report published by the Public Policy Institute of California, *California's Historic Corrections Reforms*, concluded: "Since reaching a peak in 2006 of almost 256,000 inmates, the total population incarcerated in California's state prisons and county jails has dropped by roughly 55,000. The incarceration rate has fallen from 702 to 515 per 100,000 residents—a level not seen since the early 1990s" (Lofstrom, Bird, and Martin, 2016: 3).

One of the most recent of these reforms that has garnered significant attention is Proposition 47 (Prop 47), which requires that certain drug and property offenses be charged as misdemeanors rather than as felonies, as had previously been the case. Since the enactment of Prop 47 on November 14, 2014, the number of people incarcerated in California's prisons and jails has decreased by approximately 13,000 inmates, helping alleviate crowding conditions in those institutions (Romano, 2015). Proponents of Prop 47 hail it a success, yet critics charge that the measure is mainly responsible for recent upticks in the state's crime rates.

Despite these contradictory claims, to date there has been no systematic analysis of Prop 47's impact on crime rates throughout the state, leaving Californians in the dark about the policy's effectiveness. We address this research lacuna in this study. With a synthetic control group design, we conduct the first evaluation of Prop 47's impact on violent and property crime rates in the year after its implementation. By using a state-level panel containing UCR index 1 offense frequencies from 1970 through 2015, we employ a synthetic control group design to approximate California's crime rates had Prop 47 not been enacted. We perform this analysis for each offense category and interpret the gap between California's

2015 crime rate and our constructed counterfactual approximation as Prop 47's impact. As with other recent criminal justice reforms in California, the implementation of Prop 47 is "a natural experiment that allows us to test one of the most important crime policy questions of our time" (Sundt, Salisbury, and Harmon, 2016: 316). At the same time, the findings have implications well beyond Prop 47 and California as other states encounter similar pressures to downsize their prisons and jails and seek examples of successful reform.

In the remainder of the article, we first describe Prop 47 in the broader context of criminal justice reform in California. We then summarize key arguments made by both Prop 47 proponents and opponents with respect to its hypothesized impact on crime throughout the state. We then describe our data and methodological approach, followed by a discussion of the findings. We conclude by reviewing the key findings, noting some limitations with the study, identifying future avenues of research, and discussing the implications of the findings for state systems across the country.

Prop 47 and Criminal Justice Reform in California

For years, California was home to the nation's largest state prison system. At its apex in 2006, the state prison population peaked at more than 170,000 inmates (West and Sabol, 2008), despite the fact that California prisons were designed to hold a maximum of 79,858 inmates. Critics charged that California was incarcerating too many people for too long. Starting in 2011, the state began to implement a series of criminal justice reforms, one of which is Prop 47.

What led to these reforms? Several factors were at play. First, fiscal impacts of the recent economic recession induced state leaders to scour their budgets for potential savings (Lofstrom and Raphael, 2015: 197; see also Aviram, 2016). At a cost of approximately $52,000 per year per inmate (Lofstrom and Raphael, 2016: 218), the state was footing an enormous bill to incarcerate so many offenders, a sizable portion of whom were low-level, nonviolent offenders and parole violators.

Second, California experienced a bipartisan shift in public opinion regarding the use of prison as a tool for crime control and punishment (Lofstrom and Raphael, 2016: 197; but see Beckett, Reosti, and Knaphus, 2016), a trend that paralleled what was happening at the national level (Petersilia, 2016: 8). This shift occurred, in part, after the realization that increased sentences did not seem to budge California's stubbornly high recidivism rate, which at close to 70% was among the highest in the nation (California Department of Corrections and Rehabilitation, 2010: 11). Evidence of dissatisfaction with the status quo could be seen in public opinion polls, which overwhelmingly reflected support for policy changes that reduced incarceration. Prop 47, for example, passed by a wide margin, with 60% of California residents voting in favor of it.

And third, California experienced federal court intervention as a result of the conditions of confinement in its state prisons (Lofstrom and Raphael, 2016: 197; see also Kubrin and

Seron, 2016; Sundt et al., 2016: 316–317). Extreme overcrowding led the U.S. Supreme Court to take a historic step, ordering the state to reduce its prison population to comply with constitutional standards. In *Brown v. Plata*, the Supreme Court ruled that overcrowding in California's prisons resulted in cruel and unusual punishment in violation of the Constitution's Eighth Amendment. The decision, handed down on May 23, 2011, was the result of nearly 20 years of litigation (Schlanger, 2016) in which the lower federal court found that the "convergence of tough-on-crime policies and an unwillingness to expend the necessary funds to support the population growth has brought California's prisons to the breaking point" (*Plata/Coleman v. Brown* 2009: 182). The Supreme Court's decision required the California Department of Corrections and Rehabilitation (CDCR) to reduce the state prison population by approximately 33,000 people (to 137.5% of design capacity) over a 2-year timeframe—no small feat.

AB 109, Public Safety Realignment

California responded to the Court's mandate by enacting the first of several controversial reforms: "Public Safety Realignment" (Assembly Bill [AB] 109). Realignment made fundamental changes to California's correctional system, including realigning from state to local jurisdictions certain responsibilities for lower level nonviolent offenders and parolees. Specifically, AB 109 required nonviolent, nonserious, and nonsex offenders ("the triple nons") to serve their sentences in county jails instead of in state prisons, thus, shifting responsibility for punishment from prisons, which in the United States are state or federal operations, to jails, which are run by counties and their elected sheriffs. A similar change applied to everyone released from state prison. Before implementation, these individuals were automatically on "parole" (a state term), which was then replaced by local "post-release community supervision."

Governor Edmund G. Brown Jr. proposed Realignment in January 2011, the legislature approved it in March, and it took effect in October of that year—an unusually fast track for a major policy shift described as "the biggest criminal justice experiment ever conducted in America" (Petersilia, 2012). The outcome was a sharp and permanent reduction in the state's incarceration rate, driven mainly by a reduction in new prison admissions (Lofstrom and Martin, 2016). In a very short time, Realignment substantially reduced California's prison population.[1] Yet almost all of the decline took place in the first year, and more importantly, it was not sufficient to meet the judicial target.

1. Some question whether reductions in state-level prison admissions were simply offset with increased jail populations at the local level. Notably, the county jail population did not rise nearly as much as the prison population fell, reducing the total number of people incarcerated in California. In particular, the jail population rose by only about one inmate for every three fewer offenders in state prison (Lofstrom and Martin, 2016).

Proposition 47

The judicial target was, in fact, met a few years later, in part, as a result of Proposition 47, approved by California voters on November 4, 2014. Also known as the "Reduced Penalties for Some Crimes Initiative," Prop 47 changed the lowest level nonviolent drug possession and petty theft crimes from felonies to simple misdemeanors. In particular, Prop 47 reduced certain drug possession felonies to misdemeanors and required misdemeanor sentencing for a variety of crimes, including shoplifting, where the value of stolen property does not exceed $950; grand theft, where the value of the stolen property does not exceed $950; receiving stolen property, where the value of the property does not exceed $950; forgery, where the value of a forged check, bond, or bill does not exceed $950; fraud, where the value of the fraudulent check, draft, or order does not exceed $950; and writing a bad check, where the value of the check does not exceed $950.[2] Prop 47 was intended to impact future convictions and sentencing but also allowed for individuals incarcerated at the time for crimes covered by the measure to petition for resentencing. Notably, Prop 47 required thorough review of an individual's criminal history and proper risk assessment before (re)sentencing to ensure public safety.

A unique component of Prop 47 is its additional focus on crime prevention. As state prison and jail population numbers were predicted to fall (some projected by as much as several thousand inmates or ~40,000 felony convictions a year [Watson, 2017]), it was estimated that state savings would grow by millions and would be reinvested in prevention efforts. In fact, through the creation of a Safe Neighborhoods and School Fund, the measure required money saved as a result of Prop 47 to be spent on "school truancy and dropout prevention, victim services, mental health and drug abuse treatment, and other programs designed to keep offenders out of prison and jail" (Legislative Analyst's Office, 2014: Section "Summary of Legislative Analyst's Estimate of Net State and Local Government Fiscal Impact," bullet 1).

After the passage of Prop 47, California was finally able to reach the court-mandated prison population target. California's jail population, in particular, dropped dramatically in the first few months after Prop 47's passage. Bird, Tafoya, Grattet, and Nguyen (2016) identified four mechanisms that drove this decline: (1) immediate decline in new bookings on arrests and warrants for Prop 47 offenses, (2) decline in number of convictions for these individuals, (3) share of Prop 47 defendants receiving pretrial releases increased, and (4) decline in average length of stay for sentenced offenders (i.e., less custody time). In just a few short years as a result of these significant reforms, California has done an about-face. With an incarceration rate of 329 (per 100,000), California is now well below the national average of 458 per 100,000 (The Sentencing Project, n.d.).[3]

2. Except for shoplifting, property values for these offenses were previously set at $450.
3. For a more complete discussion of California's various contemporary criminal justice reforms (beyond Realignment and Prop 47), see Gardiner and Spiropoulos (2018).

Crime in the Wake of Prop 47

Proponents of Prop 47, which include a wide-ranging list of supporters (Ballotpedia, 2014b) have been vocal about the measure's benefits. Those in favor point out that punishment is now more commensurate with crime. They also emphasize that Prop 47 is helping the state make smarter use of its criminal justice and incarceration resources by no longer wasting prison space on low-level, nonviolent offenders, which frees up space for violent criminals. Relatedly, reductions in jail populations, induced by Prop 47, have allowed counties with court-capped jails to reduce their use of capacity releases substantially. For these counties, Prop 47 presented the opportunity to decrease custody time for lower level drug and property offenders and, in exchange, increase custody time for more serious offenders (at least some of whom would otherwise have been released early because of jail capacity constraints; Grattet, Tafoya, Bird, and Nguyen, 2016). All of these changes, proponents suggest, are likely to increase public safety and lower crime rates throughout the state. And Prop 47's reallocation of resources to prevention efforts, they further argue, should significantly improve public safety in the longer term. As evidence in support for some of these claims, proponents turn to scientific evaluations of Realignment, which found that it had no impact on violent crime rates and only a small impact on property crime rates, mainly auto-theft (Lofstrom and Raphael, 2016; Sundt et al., 2016; see also Bird and Grattet, 2016, for findings related to Realignment's impact on recidivism).

Prop 47 critics (also a wide-ranging group; Ballotpedia, 2014a) have been equally vocal. They argue that felony arrests throughout the state have plummeted, emboldening would-be criminals. They also claim that drug and theft offenders who previously were arrested and held in jail pending trial are now simply receiving citations and orders to appear in court, and that few actually show up for their court dates. As a consequence, "When you don't jail these people on drug and other relatively minor charges, they are free to commit all manner of more serious crimes, including murder, rape and robbery, and they do" (Greene, 2015).

Critics believe they have data on their side. After a decades-long decline in violent and property crime throughout the state, California's crime rate saw an uptick in 2015 after Prop 47's implementation. The violent crime rate increased by 8.4% in 2015 and the property crime rate went up by 6.6% (Lofstrom et al., 2016). Concentrating on California's largest cities, violent crime jumped 11% in the first 6 months of 2015 compared with the same period in 2014. Among major U.S. cities, three California cities saw the largest increase in property crime in the country (Levin, 2016). And, from 2015 to 2016, violent crime grew 4.1% (Miller, 2017). Law enforcement officials and others have voiced concern that Prop 47 is to blame for rising crime rates throughout the state.

Predictions aside, theory on the crime–prison relationship offers several important (if contradictory) predictions. Some theories suggest that prison is crime-suppressive, whereas others suggest it is criminogenic (Harding, Morenoff, Nguyen, and Bushway, 2017). Regarding the former, it has been argued, for example, that prisons incapacitate the criminally active and that the threat of prison may deter criminal activity; at the same time, prison may be transformative through rehabilitation (Lofstrom and Raphael, 2016: 198). If these arguments are correct, we would expect a negative relationship between incarceration levels and criminal offending. Regarding the latter, however, it has been argued that incarceration may be associated with increasing crime levels, in part through a hardening of prison inmates (Lofstrom and Raphael, 2016: 198).

What do the researchers find? At early levels, incarceration does seem to reduce crime; however, diminishing crime-abating returns set in at low incarceration rates (King, Mauer, and Young, 2005: 6). Stated alternatively, scholars have reported very small crime-prevention effects of even marginal increases in incarceration. Moreover, in the context of the recent steep rise in U.S. incarceration rates, some researchers have found a criminogenic effect: "[O]ur results demonstrate that imprisonment leads to future imprisonment. In other words, prison's figurative revolving door has real causal force, rather than being the simple consequence of imprisonment of individuals at higher risk for future offending. . . . These results imply that the rise in incarceration was to some degree self-generating, as imprisonment creates more imprisonment" (Harding et al., 2017: 4). Notably, the relationship between incarceration and crime is almost always examined at the individual level. Moreover, this relationship is overwhelming in its complexity.[4]

Returning to the focus of the study, what impact has Prop 47 had on crime rates in California? Are Prop 47 and the state's rising crime rates connected? At this point, we do not know. Since its implementation on November 4, 2014, there has not been even *one* systematic analysis of Prop 47's impact on crime in California. For this reason, researchers continually warn against premature conclusions when they claim, "[I]t is too early to conclusively determine whether or not Prop. 47 has had an impact on crime" (Males, 2016: 5) and "caution should be used in drawing strong conclusions about Prop 47 from the . . . comparison of California to the rest of the country" (Lofstrom et al., 2016: 14), a sentiment echoed by some reporters who remind readers, "[T]here has been no definitive research to date showing a relationship between crime trends and Proposition 47" (Levin, 2016). Indeed, several critical questions about Prop 47 remain unanswered, as Lofstrom and Martin (2017) recently reminded us: "How have reforms affected factors such as arrests and incarceration? Do these differ across counties and what is their relationship to crime rates? Also, California's crime trends may be affected by factors unrelated to recent reforms. How do statewide trends compare to what other states are seeing?" We begin to address some of these critical questions here.

This study represents the first effort to evaluate systematically the causal effect of Prop 47's enactment on UCR part 1 violent (homicide, rape, aggravated assault, and robbery) and property (burglary, larceny, and motor vehicle theft) crime rates throughout California. As we discuss in detail in the Conclusion, the findings of this study have implications well

4. For an excellent review of research on the imprisonment–crime nexus, see King et al. (2005) and Raphael and Stoll (2009).

beyond both Prop 47 and California, as states across the country consider reforming their criminal justice systems and face similar pressures to downsize their prisons and jails.

Data and Method

With a quasi-experimental design, we examine the impact of Prop 47 on crime in the year after its enactment (i.e., 2015). Employing a synthetic control group design, described in detail as follows, we aim to identify Prop 47's causal effect on crime throughout the state. Through our analysis, we utilize a state-level panel dataset (including the District of Columbia) containing annual Uniform Crime Report Part 1 offense frequencies spanning 1970–2015. In particular, we examine the crimes of homicide, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle thefts. We transform statewide crime frequencies into per-capita rates to facilitate comparisons between states of different sizes (i.e., allow large states like California to be compared with small states like Delaware without extrapolating).[5] Without this transformation, the state with the highest observed crime frequency could not be approximated by a linear combination of the other states, as their weighted average would fall short of the highest observed frequency without extrapolating.

Methodological Approach

To evaluate the impact of Prop 47 on crime rates, we use a synthetic control group design to construct a comparison unit that approximates California had it not enacted Prop 47 (i.e., "Counterfactual California"; Abadie, Diamond, and Hainmueller, 2010; Lofstrom and Raphael, 2015). This quasi-experimental design is an extension of "difference-in-differences" models, which are aimed at estimating the causal effect of an intervention as the change in the distance between two time series that emerges after an intervention. In standard difference-in-differences (DiD) designs, it is assumed that the treated unit and its untreated comparison unit follow "parallel trends" prior to the intervention. When examining state-level interventions, however, neither the nation as a whole nor any individual state is likely to follow the treated state's long and jagged preintervention time series. By assuming "parallel trends" prior to the intervention, DiD designs are used to interpret any change in the gap between the treated and comparison units after the intervention as the effect of the treatment on the outcome. To better satisfy the "parallel trends" assumption, we construct a synthetic control group for California, "Counterfactual California," as a weighted combination of "donor pool" states that optimally fits California's crime trends from 1970 to 2014, the preintervention period. By fitting our synthetic control groups over preintervention time series containing 44 years of pre-Prop 47 crime rate observations, we go beyond selecting the most appropriate control time series for California and instead construct a better comparison unit than any individual unit available that exists. Matching on a long

5. We examined alternative transformations of the dependent variables. Results, available upon request, were mainly consistent.

($n = 44$) preintervention time series also greatly reduces our likelihood of identifying a spurious effect compared with synthetic control group models matched on fewer preintervention observations (Abadie et al., 2010; McCleary, McDowall, and Bartos, 2017).

We populate our “donor pool” with states whose time series do not reflect the impact of a Prop 47-style intervention within our analysis frame. It is important to exclude all states that experienced criminal justice interventions similar to California’s Prop 47 from the donor pool; otherwise, the constructed synthetic control may be contaminated by the contribution of a treated donor pool state (i.e., Synthetic California’s time series would also reflect Prop 47’s impact to some degree). Since Prop 47 was intended, in part, to ameliorate California’s lingering post-Realignment overcrowding issues (Romano, 2015: 3) and the sentence reductions it carried apply to a select subgroup of property and drug offenders, in fact, no other states experienced a comparable criminal justice intervention. Therefore, we include the remaining 49 states in our donor pool from which Synthetic California is constructed.

An important step in the process of synthetic control group construction involves choosing an optimal combination of donor pool weights. We employ the data-driven approach for assigning donor pool weights (time-invariant,[6] non-negative,[7] and sum to one[8]) described in Abadie et al. (2010, 2015) so as to minimize the distance between California and “Counterfactual California’s” crime trends throughout the preintervention time series. When a gap emerges between California and its synthetic counterpart after the enactment of Prop 47, the difference between the two time series can be interpreted as the causal effect of Prop 47 on the crime rate examined. Causal interpretations of the gap are predicated on the quality of the match between California and Synthetic California across the preintervention period.

We describe the quality of our preintervention fit using the conventional root mean squared prediction error (RMSPE) term, as discussed in the Findings section. If the gap between California and its constructed “Counterfactual” that emerges post-Prop 47 is within the range of the preintervention RMSPE, no effect beyond what is attributable to matching error can be identified. Identifying a post-Prop 47 gap greater than the observed pre-Prop 47 RMSPE does not mean the estimated effect is of practical significance, however. When the precision of the preintervention fit between California and Synthetic California is very good, a postintervention gap that is small relative to the observed variation in the preintervention time series can result in an effect size that is an order of magnitude greater than the preintervention RMSPE. Thus, when the preintervention fit is more precise, smaller treatment effects can be identified.

6. $\left(\omega(_{1|t=1}) = \omega(_{1|t=2}) = \omega(_{1|t=n})\right)$

7. $(\omega_1, \omega_2, \ldots \omega_n \geq 0)$

8. $(\omega_1 + \omega_2 + \ldots \omega_n = 1)$

To fit our models, we use the "Synth" routine written for Stata by Jans Heinmuller and Aberto Abadie (available at web.stanford.edu/~jhain/synthpage.html). We include all available preintervention observations (e.g., crime in California 1970–2014) of the outcome of interest as predictors (Lofstrom and Raphael, 2015; McCleary et al., 2017). By fitting our models on longer ($n = 44$, 1970–2014) time series that exhibit a great deal of white-noise variation, the optimization routine is less likely to converge on a perfect approximation of pre-Prop 47 California, but we are much less likely to identify a spurious effect than models fit on shorter and/or smoother preintervention time series (McCleary et al., 2017).

Postestimation tests. We conduct a series of postestimation tests to enhance our confidence in the reported findings. In particular, the postestimation tests allow us to address questions of spuriousness as well as to determine the extent to which our findings may be sensitive to model specification.

Concerning the former, to determine whether the estimated impact is large relative to the unidentified/exogenous variation observed among untreated (i.e., donor pool) states, in-sample placebo tests are conducted, providing a type of randomization inference (Abadie et al., 2010; Abadie and Gardeazabal, 2003; Fisher, 1922; McCleary et al., 2017). We iteratively reassign the treatment condition to each donor pool state and construct a synthetic control group. The states are then ranked by a ratio of 2015 gap to pre-2015 RMSPE. If California ranks highest among our pool of 50 states, then the estimated effect is larger than the unidentified variation observed in the donor pool states. If California does not rank highly, however, then the estimated effect is not large relative to the white noise exhibited by non-Prop 47 states. This randomization-inference procedure determines the probability of estimating an effect with an equal or greater ratio than California in any of the other donor states. Put another way, pretending that we don't know which state enacted Prop 47, we construct synthetic control groups for every donor pool state and estimate the effect of Prop 47 on crime in 2015. Because California is the only state that enacted Prop 47, it should produce a larger ratio than any state in the donor pool.[9]

Another important postestimation test, known as the "Leave One Out" test, evaluates whether an estimated effect is sensitive to changes in Synthetic "Counterfactual" California's composition. We achieve this by iteratively excluding the donor pool unit contributing the largest weight to Synthetic California until all of the original donor pool units with non-zero weights are excluded from the matching algorithm. At the end of this process, Synthetic

9. Whereas an in-sample placebo test compares the effect of Prop 47 in California with nontreated states, an *in-time* placebo test would compare Prop 47's estimated effect in the year it was enacted to random effects in nonenacted years. In-time placebo tests assume, however, that no structural shocks to California's crime rate occurred prior to Prop 47. Yet as our previous discussion on criminal justice reform in California reveals, recent reforms make this assumption untenable. For example, we would expect an in-time placebo test performed in 2011 to produce a larger effect estimate than Prop 47 in 2014 due to the enactment of AB109, making the in-time placebo test uninterpretable. Thus, in-time placebo tests are not applicable in this context (see McCleary et al., 2017: Ch. 7).

California is comprised of a completely different set of donor pool units than it was in the original model. If the original effect persists in sign and magnitude once all of the original contributors to Synthetic California have been excluded, then we can be confident that this effect is insensitive to changes in Synthetic California's composition. In other words, we can be confident that our interpretation of Prop 47's effect on crime does not change even when substantial changes are made to Synthetic California.

Results

To estimate the impact of Prop 47's enactment on crime rates in California, we construct synthetic control groups for homicide, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft. Figure 1 displays California (solid black line) and our constructed synthetic control (dashed black line) for each offense category. The gray dashed reference line reflects the 2014 enactment of Prop 47.

For homicide, rape, aggravated assault, robbery, and burglary, we find no evidence that the impact of Prop 47 was any different from zero. In other words, Prop 47 appears to have a null effect on these offenses. In particular, the gap that emerges after Prop 47's enactment was smaller than the model's preintervention RMSPE. Therefore, Prop 47's impact on these offense categories was within the range attributable to matching error and cannot be distinguished from zero.

For larceny and motor-vehicle theft, on the other hand, the gap that emerged in 2015 (i.e., post-Prop 47) was more than twice the size of the model's preintervention RMPSE, suggesting that Prop 47 did have an impact on these offenses. With California's actual time series falling above the synthetic control group estimate, the size and direction of the gap suggest that both larceny and motor vehicle theft experienced a nontrivial increase post-Prop 47. Although it is premature to draw conclusions about these effects prior to postestimation testing (see subsequent discussion), the postintervention gaps suggest that larceny and motor vehicle thefts were less than 10% and roughly 20% higher, respectively, in 2015 than they would have been without Prop 47.

Sensitivity/robustness tests. To determine whether the estimated effects of Prop 47 are large relative to the unidentified annual variation observed in states that did not experience Prop 47, we perform in-sample placebo tests (a type of randomization inference used to estimate the exact probability of identifying a treatment effect of equal or greater magnitude if the treatment were randomly assigned to each donor pool unit). Put another way, this test determines the probability of identifying California as the state that experienced Prop 47 effects if we began our analysis not knowing which state had enacted Prop 47. If we identify more than five donor pool states that produce larger treatment effects than California, then the probability of identifying an effect equal or greater in magnitude than California is greater than .1 (i.e., $p = 5 / 50 = .10, p = 6 / 50 = .12$) and would not be significant. Figure 2

FIGURE 1

(a) Synthetic Control Group Estimates for Violent Offenses and (b) Synthetic Control Group Estimates for Property Offenses

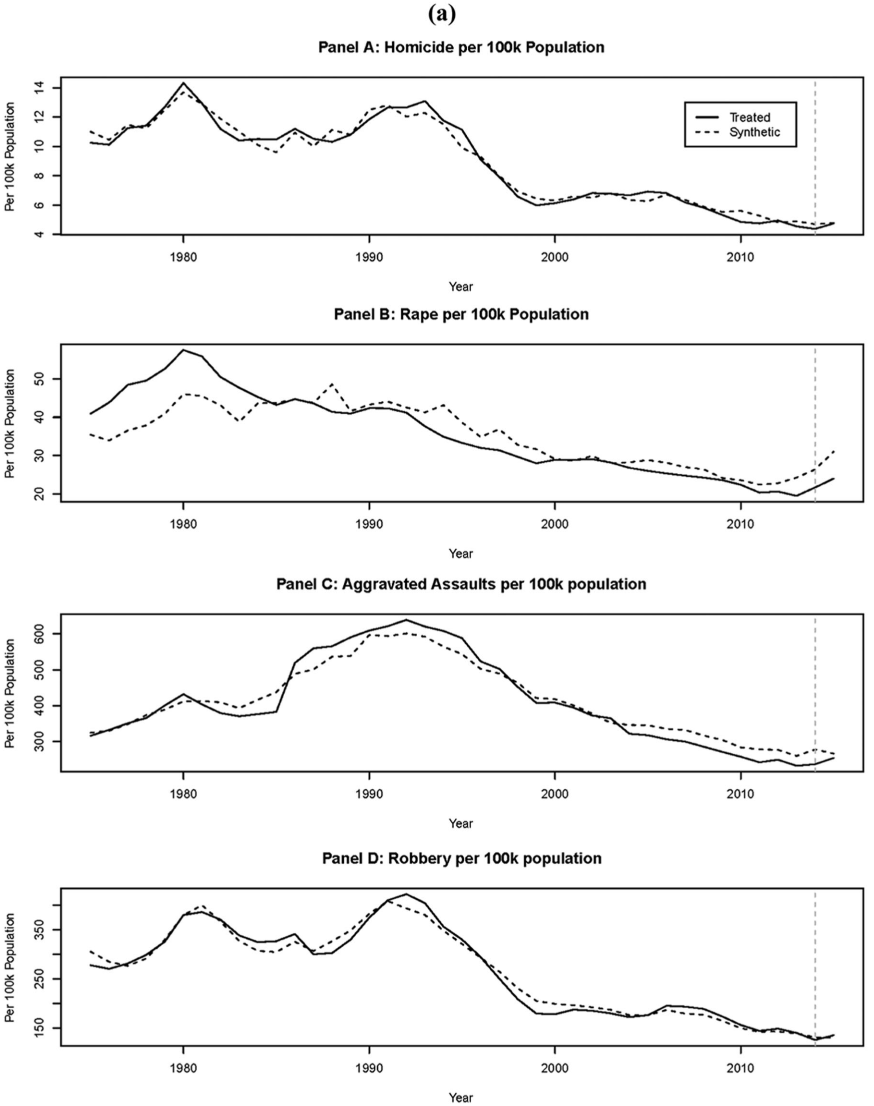
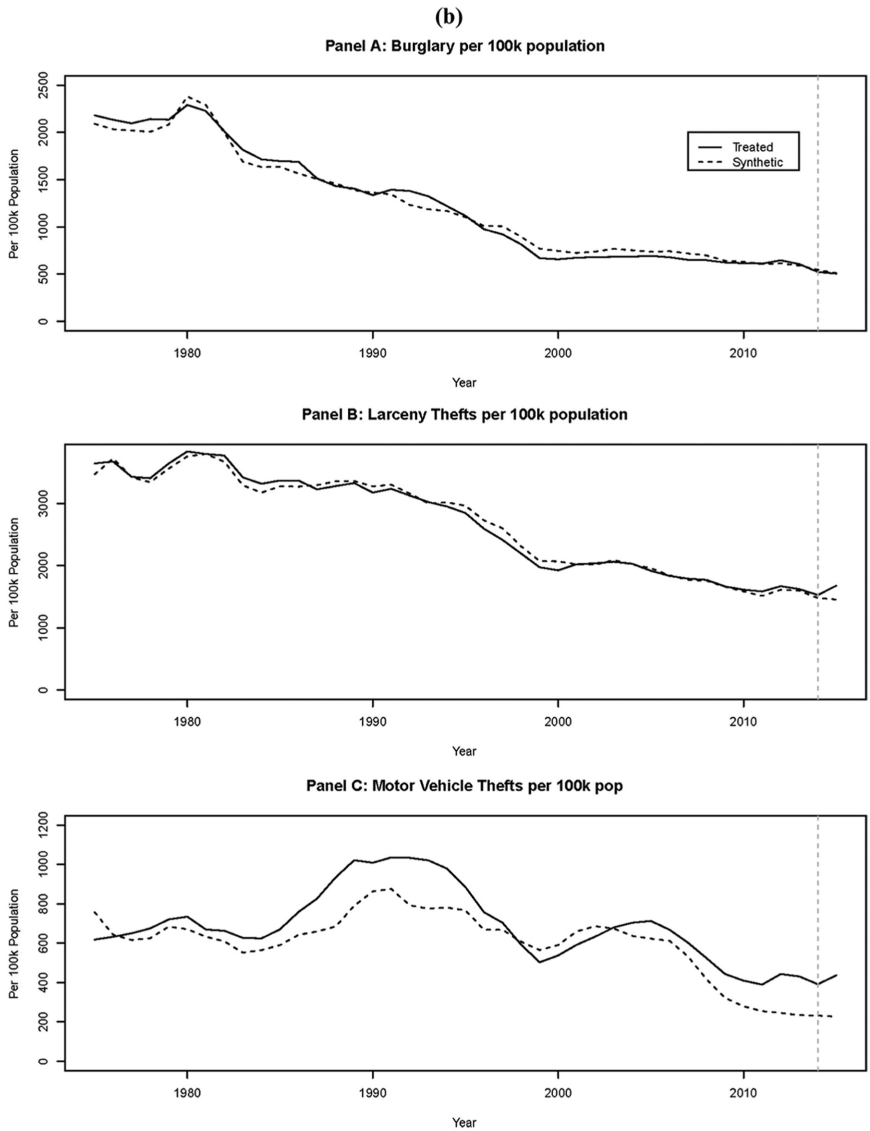


FIGURE 1

Continued

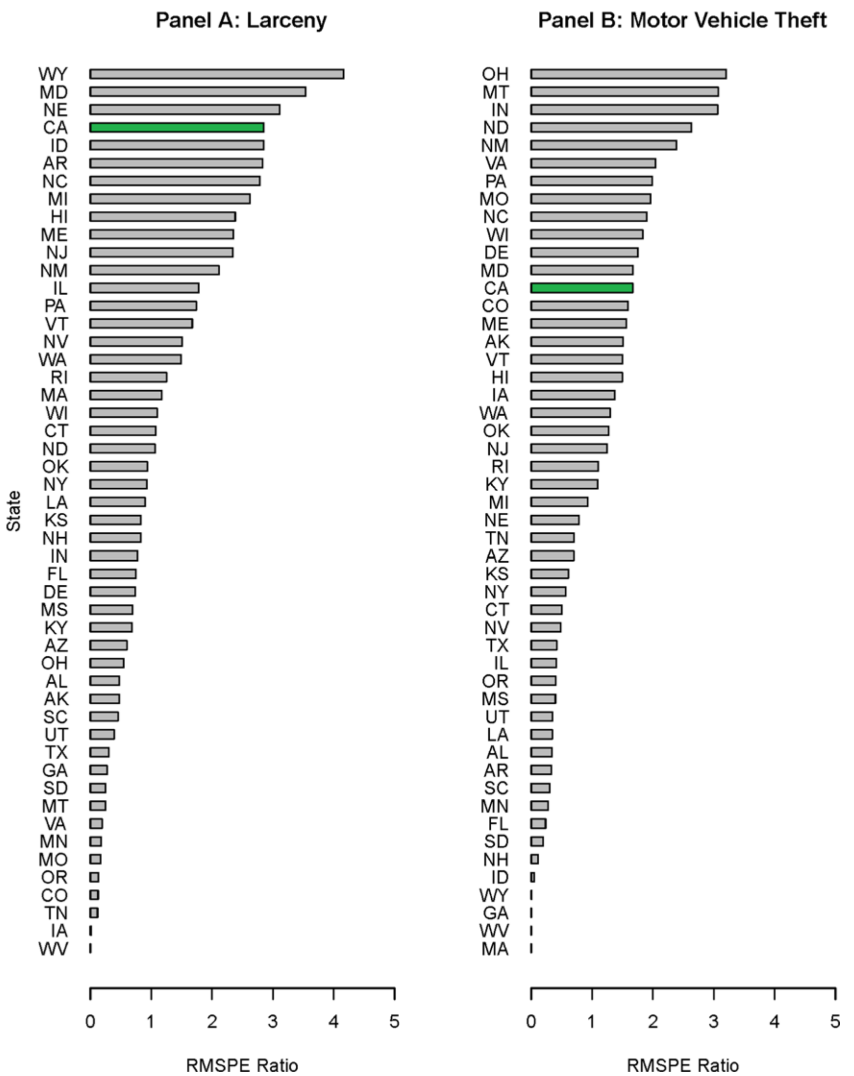


displays California's ratio of postintervention gap to preintervention RMSPE relative to the donor pool states for larceny and motor-vehicle theft, the two offenses that did not produce null effects.

As Figure 2 shows, California did not rank particularly highly for motor-vehicle thefts (13 out of 50; $p = \sim .26$), suggesting that the estimated effect appears smaller in California

FIGURE 2

In-Sample Placebo Test RMSPE Ratios Ranked [Color figure can be viewed at wileyonlinelibrary.com]



than the random variation observed in donor pool states. Larceny ranked 4 out of 50 ($p = \sim.08$), however, suggesting that the estimated larceny increase is not trivially small relative to changes in larceny observed in non-Prop 47 states.

In short, out of seven crime categories examined, our findings suggest Prop 47 had nonzero effects on larceny and motor vehicle thefts; however, only the larceny effect appears significant (at the $p < .10$ level, akin to Fischer's "exact test"). In other words, larceny is the only offense category that has an exact probability of identifying a larger effect in the donor pool states of less than .10 (4 / 50 = ~.08). Our estimate of Prop 47's effect on the rate of motor vehicle theft in California did not rank highly compared with the estimated effects for the donor pool. As such, if we did not know which state enacted Prop 47 in 2014, and we tried to identify it by looking at the state with the largest ratio of 2015 effect to preintervention RMSPE, our chances of correctly identifying California would be 26% (i.e., a 1 out of 4 chance of identifying the wrong state). Because a quarter of the donor pool produced larger RMSPE ratios than California for motor vehicle theft, California's RMSPE ratio is not an outlier. Therefore, Prop 47's estimated effect on motor vehicle thefts in California is likely to be a spurious result. In sum, although our findings identified nonzero Prop 47 effects for larceny and motor-vehicle thefts, only larceny appears to have an impact that is large relative to the unidentified variation observed in donor pool states.

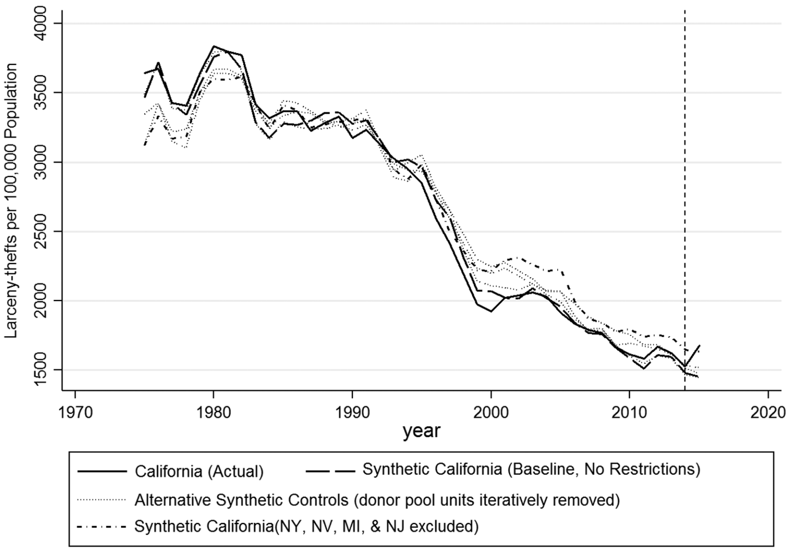
To determine whether the estimated larceny effect is sensitive to changes in Synthetic California's composition (i.e., different donor pool weights), we iteratively exclude the donor pool state with the greatest weight (ω) until all of the original donor pool states with nonzero weights have been removed. Synthetic California is composed of four donor pool states with weights that are greater than zero: New York, Michigan, Nevada, and New Jersey. The version of Synthetic California that results from this procedure is composed of a set of donor pool states that are entirely different than our original model. If the estimated impact of Prop 47 on California's crime rate persists under both compositions, we can be confident that our larceny estimate is not dependent on the contribution of certain donor pool states to Synthetic California. If our interpretation changes under Synthetic California's new composition, however, the estimated effect is dependent on the contribution of certain donor pool states and the finding should be interpreted cautiously.

The results of our Leave One Out sensitivity test are displayed in Figure 3. In addition to California and unrestricted Synthetic California (as seen in Panel B of Figure 1b), Figure 3 also displays a series of alternative specifications for Synthetic California as donor pool states are iteratively excluded (gray dashed lines). For larceny, we find that Synthetic California requires at least one of the following states be included in the donor pool in order to sustain the effect: New York, Michigan, Nevada, and New Jersey (the dashed red time series reflects Synthetic California when these four states are excluded from the donor pool). When these four donor pool units are excluded, the postintervention gap disappears. This suggests that our valid causal interpretation of the Prop 47 effect on larceny rests on the validity of including these four states in our donor pool. Thus, larceny, our only nonzero, nontrivial effect estimate, appears to be dependent on the contribution of four specific states from our donor pool. This finding, therefore, should be interpreted with caution.

To summarize our findings, although our initial synthetic control estimates suggested increases in larceny and motor-vehicle thefts after Prop 47's enactment, none of these effects

FIGURE 3

Leave One Out Plot for Larceny



survive both significance testing (randomization inference) and sensitivity testing. At the same time, null effects were identified for homicide, rape, robbery, aggravated assault, and burglary. Thus, we find no evidence of a statistically significant robust increase for any of the seven UCR index 1 offense categories in the year after Prop 47's enactment.

Conclusion and Discussion

This study represents the first systematic analysis of Prop 47's impact on violent and property crime rates throughout California in the year after the measure's implementation. With state-level panel data from 1970 through 2015, we employed a synthetic control group design to approximate California's crime rates had Prop 47 not been enacted. Our findings reveal that Prop 47 had no effect on homicide, rape, aggravated assault, robbery, and burglary. At the same time, we find that larceny and motor vehicle thefts appear to have increased moderately after Prop 47—yet these results are both sensitive to alternative specifications of our synthetic control group and are too small to rule out spuriousness. Overall, then, we find very little evidence to suggest that Prop 47 caused crime to increase in California.

The findings from our analysis have implications well beyond Prop 47 and California. Although Prop 47 is specific to California, the steps taken by the state to reform its criminal justice system are being closely watched by other states also confronting similar fiscal and legal challenges related to overcrowding. As commentators have noted, "[P]olicymakers in different criminal justice systems across the country, from the federal courts down to the local justice systems, might be inspired to look in new directions" for criminal justice reform (Strutin, 2012: 1342). These states are asking whether the large-scale prison downsizing in California will compromise public safety or whether they can look to reforms such as Prop 47 as a possible solution to replicate in their own states. Even though speculation abounds, rigorous, high-quality scientific research has not been conducted; indeed, no scholarly empirical, peer-reviewed research on Prop 47 has been published since the measure's enactment in 2014. As such, policy makers and the public lack the knowledge they need to make informed decisions about the futures of their criminal justice systems. The findings from this study begin to address this gap in knowledge.

Of course these findings should be interpreted within the context of the study's potential limitations. First, although no other state enacted a sentencing reform that is wholly comparable to Prop 47 within our analysis timeframe, a diverse body of state-level sentencing reforms has been enacted across the United States since the 2008 financial crisis. It is likely that at least some states have enacted sentencing reforms that are comparable, in some part, to Prop 47. If Synthetic California is constructed with a donor pool unit that partially experienced a Prop 47-like intervention, both trends would reflect the impact of the shared aspect of Prop 47. The gap would then reflect Prop 47's effect on the outcome beyond what was caused by the shared aspect of Prop 47, producing a more conservative estimate of the effect.

Second, even though our long preintervention time series (1970–2014) makes a spurious result less likely, our single postintervention observation (i.e., 2015) leaves us unable to assess whether Prop 47's estimated effects are permanent, temporary, accruing, or decaying. As more postintervention observations become available, this question can be addressed through replication and extensions using updated time series.

Finally, anecdotal reports of Prop 47's effect on crime often focus on increased drug offenses and other social ills (e.g., homelessness) after its enactment. These offenses, however, are not captured by our UCR Part 1 crime measures. Thus, further research is needed to address these claims.

Beyond these recommendations, nagging issues related to Prop 47 remain. For example, corrections spending in California remains high and continues to pose fiscal challenges for the state (California Department of Corrections and Rehabilitation, 2017). One anticipated benefit from Prop 47 is that the state will save money on corrections as a result of fewer individuals being sentenced to prison. These savings have not fully materialized. Still, despite greater original estimates, the state savings ($67 million in 2016–17 and $46 million in 2017–18) is to be redirected to local mental health and substance abuse programs, K–12 education, and services for victims of crime (Public Policy Institute of California, 2018). Given that money not spent on state prisons in the wake of Prop 47 is directed at increasing evidence-based programming to reduce recidivism and overall incarceration, it is critical to determine how these investments will impact crime rates in the longer term.

Also, apart from Prop 47's impact on crime, some question how Prop 47 has impacted recidivism rates throughout the state. Prior to both Realignment and Prop 47, recidivism rates in California were quite high, as noted earlier. Unfortunately, they remain stubbornly high today, even as prison and state parole populations have dropped dramatically (Lofstrom et al., 2016). What explains this trend? And more to the point, what is the recidivism rate of Prop 47ers?

Finally, there is little doubt that our statewide analysis masks important variation at the local level. In particular, it is worth determining whether Prop 47's impact on crime (and recidivism for that matter) varies across California's 58 counties, each with different socioeconomic, demographic, and criminal justice profiles. Prior research findings on Realignment reveal that, in fact, its impact on crime and recidivism varies significantly by county (Bird and Grattet, 2016; Lofstrom and Raphael, 2016) so future research should be aimed at both documenting and attempting to explain this variation. A critical challenge here involves evaluating the effects of policy or practice changes across California counties under conditions of limited data (see Bird and Grattet, 2016).

Future directions aside, we conclude with a few comments regarding criminal justice reform more broadly—that is, both beyond California and beyond prison downsizing. Although reforms such as Realignment and Prop 47 have shown us we can, in fact, downsize our prisons without comprising public safety (see also Kubrin and Seron, 2016; Sundt et al., 2016), solutions to America's "crime problem" should not be limited to "back-end" efforts at reform, or efforts that focus solely on sentencing and incarceration. "Front-end" solutions—primarily those aimed at crime prevention—also deserve a seat at the table. Whether we're talking about civic participation, housing stability, strong police–community relations, poverty alleviation, drug and alcohol treatment, or addressing challenges related to homelessness and mental health, public health researchers and criminologists alike have long clamored for more attention to be directed toward prevention. Unfortunately, prevention routinely takes a back seat to efforts focused on punishment, which helps explain the incredible growth of incarceration in the United States (Travis, Western, and Redburn, 2014).

At the same time, we must resist the politicization of criminal justice reform. In the case of Prop 47, almost from the start, strong claims have been made regarding the measure's impact on crime rates throughout the state—in the absence of any data or analysis to back those claims up. Opponents routinely cite rising crime rates as "proof" that Prop 47 is harming public safety, prompting repeated calls to repeal the measure (LA Times Editorial Board, 2017). Yet crime rates going up (or down for that matter) tell us nothing about the source of those trends, and studies such as this one are necessary to determine any link between criminal justice reform and crime rates. Absent those studies, claims about a reform's impact should be strongly tempered.

In closing, the California case is instructive. As Petersilia (2016: 9) recently reminded us, “A crisis is a terrible thing to waste in that it allows you to get things done that you could otherwise not get done in a saner atmosphere.” Indeed, California witnessed such a crisis, which ultimately led to historic corrections reforms, including Prop 47. Although more research is necessary, initial findings from a handful of studies—including this one—suggest that these reforms are not associated with meaningful increases in crime. As the nation debates prison downsizing, clearly the experience of California must be front and center.

Appendix: Donor Pool Weights by Crime Type

State	State Name	Homicide	Rape	Aggravated Assault	Robbery	Burglary	Larceny	Motor Vehicle Theft
AL	Alabama	0	0	.108	0	0	0	0
AK	Alaska	0	0	0	0	0	0	0
AZ	Arizona	.124	0	0	0	.14	0	.335
AR	Arkansas	.039	0	0	0	0	0	0
CO	Colorado	.014	0	0	0	.098	.095	0
CT	Connecticut	0	0	0	.072	0	0	0
DE	Delaware	0	0	0	0	0	0	0
FL	Florida	0	0	.333	0	.063	0	0
GA	Georgia	0	0	0	0	0	0	0
HI	Hawaii	0	0	0	0	0	0	0
ID	Idaho	0	0	0	0	0	0	0
IL	Illinois	0	0	.21	.182	0	0	0
IN	Indiana	0	0	0	0	0	0	0
IA	Iowa	0	0	0	0	0	0	0
KS	Kansas	0	0	0	0	0	0	0
KY	Kentucky	0	0	0	0	0	0	0
LA	Louisiana	.098	0	0	.208	0	0	0
ME	Maine	0	0	0	0	0	0	0
MD	Maryland	0	0	0	.172	0	0	0
MA	Massachusetts	0	0	0	0	0	.019	.27
MI	Michigan	0	0	0	0	0	0	0
MN	Minnesota	0	0	0	0	0	0	0
MS	Mississippi	0	0	0	0	0	0	0
MO	Missouri	0	0	0	0	0	0	0
MT	Montana	0	0	0	0	0	0	0
NE	Nebraska	0	0	0	0	0	0	0
NV	Nevada	.156	.428	0	.067	.363	.479	.101
NH	New Hampshire	0	0	0	0	0	0	0
NJ	New Jersey	0	0	0	0	0	0	0
NM	New Mexico	0	0	0	0	0	0	0
NY	New York	.462	.572	.349	.225	.335	.406	0
NC	North Carolina	0	0	0	0	0	0	0

(Continued)

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State	State Name	Homicide	Rape	Aggravated Assault	Robbery	Burglary	Larceny	Motor Vehicle Theft
ND	North Dakota	0	0	0	0	0	0	0
OH	Ohio	0	0	0	0	0	0	0
OK	Oklahoma	.039	0	0	0	0	0	0
OR	Oregon	0	0	0	0	0	0	0
PA	Pennsylvania	0	0	0	0	0	0	0
RI	Rhode Island	0	0	0	0	0	0	0
SC	South Carolina	0	0	0	0	0	0	0
SD	South Dakota	0	0	0	0	0	0	0
TN	Tennessee	0	0	0	0	0	0	0
TX	Texas	.067	0	0	.075	0	0	.295
UT	Utah	0	0	0	0	0	0	0
VT	Vermont	0	0	0	0	0	0	0
VA	Virginia	0	0	0	0	0	0	0
WA	Washington	0	0	0	0	0	0	0
WV	West Virginia	0	0	0	0	0	0	0
WI	Wisconsin	0	0	0	0	0	0	0
WY	Wyoming	0	0	0	0	0	0	0

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