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Public Safety Realignment and Crime Rates in California

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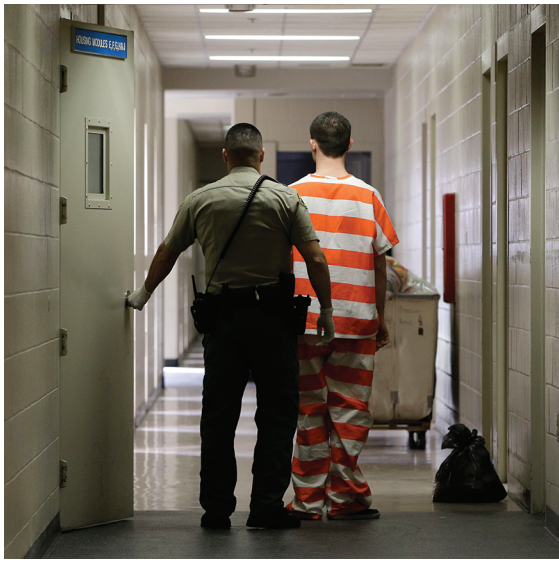

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SUMMARY

California's corrections realignment plan quickly and significantly reduced the state's prison population. The reduction, motivated by a federal mandate, was achieved by sentencing lower-level offenders to county jails rather than state prison and by giving counties, rather than the state, most of the responsibilities for parolees. Although county jails absorbed many of the offenders affected by the legislation, realignment markedly decreased the overall reliance on incarceration in California. Currently, about 18,000 offenders, who in past years would have been in either prison or jail, are not serving time behind bars (Lofstrom and Raphael 2013). This large increase in "street time" among former prison inmates has raised obvious concerns about crime.

We find that California's crime rates increased between 2011 and 2012—violent crime went up 3.4 percent and property crime went up 7.6 percent. These rates vary widely across the state, with California's ten largest counties generally seeing greater increases in crime than in the state overall. However, despite this pattern of increase, crime rates remain at historically low levels in California today.

How does realignment relate to the recent uptick in crime? Our analysis of violent crime finds no evidence that realignment has had an effect on the most serious offenses, murder and rape. The evidence on robbery is more uncertain, with a possible indication of a modest increase related to realignment. California's overall increases in violent crime between 2011 and 2012 appear to be part of a broader upward trend also experienced in other states.

By contrast, we find robust evidence that realignment is related to increased property crime. In terms of overall property crime, we estimate an additional one to two property crimes per year on average for each offender who is not incarcerated as a result of realignment. In particular, we see substantial increases in the number of motor vehicle thefts, which went up by 14.8 percent between 2011 and 2012. Our estimates translate to an increase in the motor vehicle theft crime rate of about 65 more auto thefts per year per 100,000 residents. In a comparison with other states, California had the highest increase in this area. This increase, of about 24,000 auto thefts per year, reverses a declining trend in this theft rate and brings it back to 2009 levels.

Because California still houses more prisoners than the federal mandate will ultimately allow, we also look at how further reductions in the prison population could affect crime rates. Our analysis suggests that, on average, further reductions are likely to lead to somewhat greater effects on crime, in the range of 7 to 12 percent more property crime than the property crime numbers we have estimated for 2011–2012.

When we compare the costs of incarceration to those of alternative crime-reducing strategies, we find that incarceration is an expensive way to maintain public safety. We suggest that these alternative strategies are likely to provide improved outcomes at lower costs. In particular, our analysis suggests that more crimes, between 3.5 and 7 times as many, would be prevented by spending an additional dollar on policing rather than on prison incarceration. As realignment continues to unfold, California should consider safer, smarter, and more cost-effective approaches to corrections and crime prevention.

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Introduction

The 2011 legislation commonly referred to as corrections realignment (or AB 109) substantially reduced the population of California's overcrowded and expensive prison system. Realignment was put in motion by a federal court order to reduce the state's prison population; this order had been challenged by the state but was upheld by the U.S. Supreme Court. With prison expenditures consuming 10 percent of the state budget—more than double the mid-1980s level—and state revenues in severe decline because of the Great Recession, California was in no position to relieve overcrowding through new prison construction.

Realignment sought to reduce the prison population by lowering the rate at which parolees return to state custody and by sentencing lower-level offenders to county jails rather than state prison. The state transferred substantial responsibilities to the counties for monitoring paroled inmates and punishing lower-level offenders. These new responsibilities also came with additional funding and greater discretion for localities to decide how to implement realignment. Realignment went into effect on October 1, 2011, and quickly decreased the prison incarceration rate to a level not seen since the early 1990s.

Realignment Significantly Shifted Incarceration Rates and Jail Time

Between late September 2011 and September 2013, the state prison population declined by roughly 27,000 inmates. Concurrently, the population of county jails throughout the state increased by roughly one-third this amount, or about 9,000 inmates. These trends reflect a substantial reduction in the scope of state-level corrections and an expansion of the role of counties in managing felony offenders. New county responsibilities fall into three main categories:

- First, lower-level offenders convicted of non-sexual, non-violent, and non-serious crimes (so-called triple-non offenses) with no such crimes appearing in their criminal records now serve their sentences under county supervision rather than in state prisons.
- Second, parole violators who reoffend (i.e., violate the terms of their release but are not convicted of a new felony) are no longer sent to state prison but serve short stays in county jails or face other local sanctions.
- Third, most offenders serving time in state prison for triple-non offenses will now, on release from prison, be supervised by county probation departments rather than state parole.

Although realignment has certainly increased the population of county jails, it has reduced the overall California incarceration rate . . . almost 9 percent.

Realignment affords counties considerable discretion in exercising their new responsibilities. They are free to rely heavily on their local jails or to choose from a wide variety of less severe alternatives, such as electronic monitoring, house arrest, split-sentencing (a sentence in which the offender serves a reduced jail term followed by probation), and very short jail stays (known as "flash incarcerations") for those who violate the terms of their conditional release.

Although realignment has certainly increased the population of county jails, it has reduced the overall California incarceration rate (prisons and jails combined) almost 9 percent (Lofstrom and Raphael 2013). On average, a county's jail population increases by one for every three felons no longer assigned to state prison. In other words, two out of three offenders are not serving time behind bars for their parole violations or crimes.[1] One of the most notable decreases in incarceration has occurred among parole violators. Those who would have been returned to the custody of the state prison system in the past are now spending much less time behind bars (in either prison or jail) as a result of realignment.

There is also evidence that some populations have been displaced from local jails to make way for realigned

offenders. Realignment appears to have increased the number of early releases of some jail inmates, especially in counties under court-ordered population caps (Lofstrom and Raphael 2013). For example, in these counties, one sentenced inmate per month is released early because of housing capacity constraints for every four realigned offenders. Pretrial releases caused by capacity constraints also went up at a rate of roughly one inmate for every seven fewer felons sent to prison. Counties without court-ordered population caps also appear to have responded to realignment by releasing some inmates who would have otherwise been incarcerated, especially pretrial detainees and those serving time for misdemeanors.

The evidence points toward a wide effect of realignment on incarceration, reaching beyond the targeted realigned offenders. That is, although lower-level felons face less jail time, other offender populations do as well. These large increases in "street time" among former prison inmates, and possibly some displaced jail detainees, raise obvious concerns over whether realignment has caused an increase in state crime rates. One sign of such concerns is the number of proposals in the legislature seeking to shift some county corrections responsibilities back to the state.[2]

In addition, California still houses about 8,000 prisoners over the court-mandated level. At this time, it is unclear how California will achieve further reductions in its prison population, and it is possible that the state will have to resort to early release of some inmates. In this context, it is critical to understand the effect of realignment on crime in California.

Focus of This Report

In this report, we estimate the effect on crime of the realignment-caused decrease in incarceration, focusing on the first year that the reform was implemented. First, we look at statewide crime trends and examine county-specific changes in crime rates. Next, we determine the extent to which realignment has affected crime rates in the state and compare California to other states. We then examine the effect on crime that further reductions in California's prison population may cause. Finally, in an effort to provide a context for considering ways in which California can build safer and smarter approaches to corrections and crime prevention, we look at the cost-effectiveness of prison incarceration as a crime-reducing strategy and compare it to one of many alternative strategies: increased policing.

Two potential limitations of this study are worth noting. Our specific focus on the relationship between crime and realignment-induced changes in incarceration means that our results do not speak to the potentially mitigating effects of new county approaches, introduced with funding from the state, to implement crime-prevention strategies. In addition, this study is limited to the first year of realignment—but as counties refine their strategies, the effect of realignment on crime might change.

California Crime Trends

After a prolonged period of decline, California's crime rates have recently started to increase. Both violent and property crime rates went up between 2011 and 2012—by 3.4 percent for violent crime (including murder, rape, robbery, and aggravated assault) and 7.6 percent for property crime (including burglaries, larceny, and motor vehicle theft). We also observe increases in each of the individual crimes that make up the total property and violent crime indices.

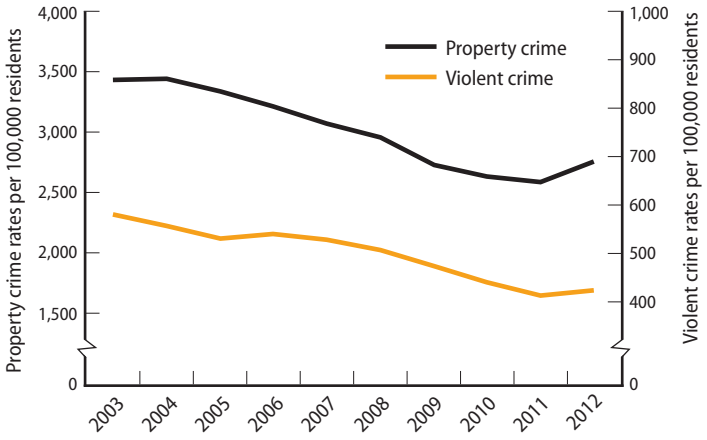
However, violent and property crime rates remain at historically low levels and are substantially below those observed a decade ago (Figure 1). The 2012 property crime rate is 20 percent below what it was in 2003, and the 2012 violent crime rate is 27 percent below the 2003 rate.

Many factors drive crime trends. How does realignment relate to these recent upticks in crime?

Increases in Some Property Crimes Coincide with Realignment

The annual changes shown in Figure 1 do not line up precisely with realignment, since implementation began in the last three months of 2011. To investigate more precisely the relationship between realignment and changes in crime

Figure 1. Despite recent upticks, crime is at historically low levels in California



SOURCE: The California Department of Justice's Criminal Justice Statistics Center, California Crimes and Clearances Files, 2003–2012.

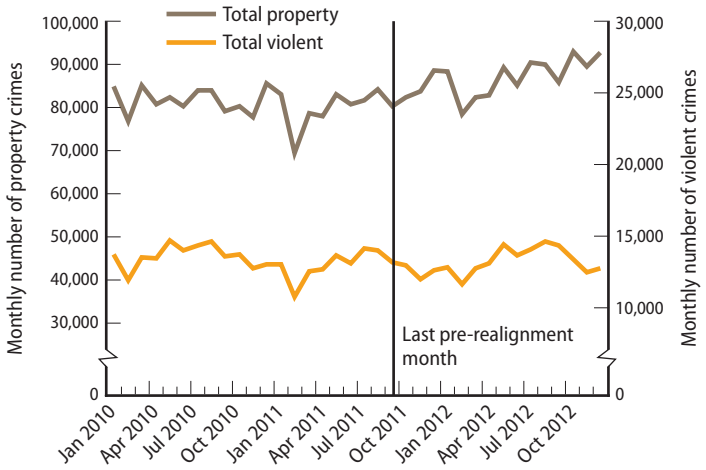
trends, we use monthly data published by the California Department of Justice's Criminal Justice Statistics Center. With these data, we can more accurately document how crime trends align with the implementation of realignment.

The monthly data on violent crime provide little evidence of an increase caused by realignment (Figure 2). The data display some monthly variation caused in part by differences in the number of days per month and other seasonal factors. But none of these changes appear to coincide with realignment—the trend line looks fairly similar before and after realignment began.

By contrast, we do see higher property crime in the post-realignment period. The property crime trend is quite flat for the period January 2010 through September 2011—with some monthly variation, as with violent crime. However, starting around the time realignment began, we see a noticeable increase in property crime, with three-year peaks observed in October 2012 and December 2012. These peaks are about 15 and 8 percent higher, respectively, than they were in the same months in 2010.

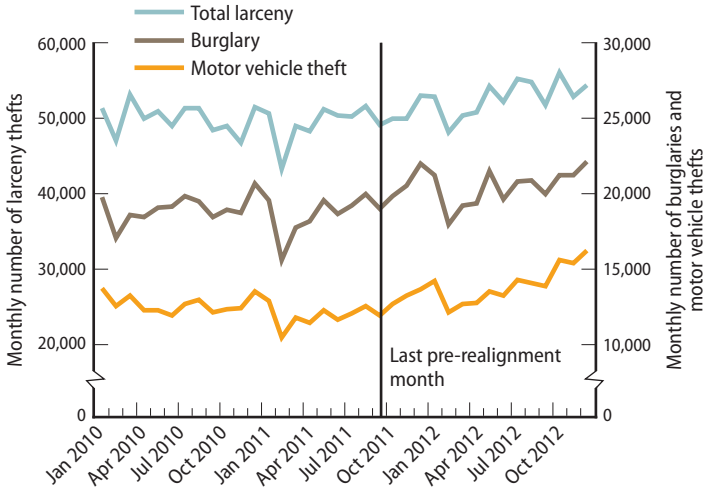
Figure 3 shows the individual property crimes that make up the overall property crime category. The trends for these offenses indicate that all three types—motor vehicle theft, larceny, and burglary—are on the uptick post-realignment. Most notable are the increases in number of motor vehicle thefts, which are up by more than 20 percent in each of the last few months of 2012 compared to the same months in 2010. Furthermore, comparing each month in 2011 to the same month in 2010 reveals that the start of the increase in motor vehicle theft coincides exactly with the implementation of realignment in October 2011.

Figure 2. Property crime increased noticeably after realignment whereas violent crime remained about the same



SOURCE: The California Department of Justice's Criminal Justice Statistics Center, California Crimes and Clearances Files, 2010–2012.

Figure 3. Motor vehicle thefts increased most after realignment



SOURCE: The California Department of Justice's Criminal Justice Statistics Center, California Crimes and Clearances Files, 2010–2012.

Changes in Crime Rates Differ Vastly across Counties

On average, then, both violent and property crime went up in California in 2012. But these increases varied widely across counties—and in some counties crime even went down. Here, we focus on the state's ten largest counties—Los Angeles, San Diego, Orange, Riverside, San Bernardino, Santa Clara, Alameda, Sacramento, Contra Costa, and Fresno—where more than 70 percent of the state's population lives.[3]

Most of these counties experienced increases in both violent and property crime that exceed comparable changes for the state (Table 1). There are two notable exceptions: Los Angeles County, where violent crime fell by 2.7 percent, and Fresno County, where it fell by 12.1 percent. Property crime also declined in Fresno, by 1.2 percent.

Elsewhere, both violent and property crime increased. Contra Costa saw the largest increase in violent crime (12.6%), followed by Orange (9.1%) and San Diego (8.8%). Property crime went up the most in Santa Clara (20.4%) and Alameda (17.1%).

The data clearly show that changes in crime rates vary substantially across counties (and even more so if we look at all counties). Why would realignment affect counties so differently? As we will discuss below, one reason is the extent to which counties relied on prison incarceration in the years before realignment began. Variation in county incarceration rates before realignment ultimately resulted in differences across counties in how realignment affected the number of offenders on the street after realignment.

Are Crime Rates Changing as a Result of Realignment?

As we have said, many factors contribute to crime trends. How do we know if realignment is the cause of the recent uptick in crime around the state? The analysis above suggests that realignment may have had an effect on property crime. The evidence with regard to violent crime is mixed, with smaller increases that do not clearly coincide with the implementation of realignment.

To be more certain about the effect of realignment on crime, we must rule out the potential effect of other factors that may also affect crime rates in the state, such as

Table 1. Violent and property crimes before and after realignment, by county

	Violent crimes (January–September)			Property crimes (January–September)		
	2011 (before)	2012 (after)	% change	2011 (before)	2012 (after)	% change
Statewide	117,578	121,934	3.7	719,646	773,148	7.4
Ten largest counties						
Los Angeles	35,018	34,067	–2.7	168,584	171,617	1.8
San Diego	8,218	8,945	8.8	47,675	51,180	7.4
Orange	4,835	5,274	9.1	45,623	50,014	9.6
Riverside	4,978	5,334	7.2	46,213	49,675	7.5
San Bernardino	6,427	6,816	6.1	41,729	46,056	10.4
Santa Clara	3,451	3,657	6.0	28,372	34,170	20.4
Alameda	8,265	8,836	6.9	39,155	45,835	17.1
Sacramento	5,652	6,130	8.5	33,545	35,965	7.2
Contra Costa	2,941	3,312	12.6	22,128	24,360	10.1
Fresno	4,455	3,918	–12.1	31,692	31,300	–1.2

SOURCE: The California Department of Justice's Criminal Justice Statistics Center, California Crimes and Clearances Files, 2011–2012.

NOTES: The table presents the number of crimes in the first nine months in each year, 2011 and 2012, as well as the percentage change over the period. The pre-realignment period is January–September 2011 and the post-realignment period is January–September 2012. October through December are excluded, since these months in 2011 are post-realignment months.

local changes in police staffing or the lingering effects of the recent recession. In addition, certain factors related to realignment must also be taken into consideration. In this section, we focus particularly on incarceration rates, which vary considerably across California's counties.

Why do county incarceration rates matter? Much of the concern about realignment and crime has to do with the rapid decline in the state's prison population—and the possibility that released offenders will return to criminal activities. Recall that in the first 12 months following the reform, the state prison population was reduced by some 27,000 inmates—and only about one-third of them can be accounted for in increases in county jail populations. In other words, the number of former inmates on the streets has grown considerably since realignment began.

Our previous work has shown that counties with very high incarceration rates before realignment experienced the largest decreases in incarceration rates after realignment (Lofstrom and Raphael 2013). By extension, these counties saw the largest increases in the number of former inmates in their communities. Here, we assess whether crime rates increased more in counties that experienced relatively larger decreases in their incarceration rates after realignment.

Analyzing Incarceration Rates, Crime Rates, and Realignment

In this section, we provide a brief synopsis of our analytical approach and our data sources. An online technical appendix provides further details on the data, along with an in-depth discussion of our methodology.

In the analysis that follows, we employ monthly crime data published by the California Department of Justice's Criminal Justice Statistics Center, aggregated to the county level and normalized by county population to measure crime rates and changes in crime rates per 100,000 county residents. We use county-level prison admissions and release data provided to us by the California Department of Corrections and Rehabilitation (CDCR) and monthly county-level jail population data from the Board of State and Community Corrections (BSCC) Jail Profile Survey to characterize corresponding changes in county-specific prison and jail incarceration rates.

Using these data, we measure changes in crime rates, prison incarceration rates, and jail incarceration rates for each month from October 2011 through September 2012 (effectively, the first 12 months following the reform) and compare these rates to the pre-realignment period. We adjust these measures for county-specific seasonal patterns in crime and incarceration to make sure that county differences in crime-seasonality that happen to coincide with the geographic distribution of realigned inmates are not biasing our results.

Much of the concern about realignment and crime has to do with the rapid decline in the state's prison population—and the possibility that released offenders will return to criminal activities.

Because there is substantial variation both within and between counties in the effects of realignment on county incarceration rates, we can assess whether a county's crime rate increases as the number of realigned offenders residing within that county increases. We can also assess whether any increases in crime rates are larger in counties that experience large increases in the number of former inmates.

We control for three broad factors. First, all of our estimates control for any reincarceration occurring at the county level, specifically, for changes in the jail incarceration rate.

Second, we adjust for broad county-specific trends that coincide with the implementation of realignment but have nothing to do with its effects.[4] Nonetheless, a few county-specific factors could potentially bias our estimates. Changes to police staffing is one such trend. Many police departments have seen cuts to their staff in recent years, which potentially exerts an upward pressure on crime. Another trend is the speed of the economic recovery across California's counties—it is possible that counties that

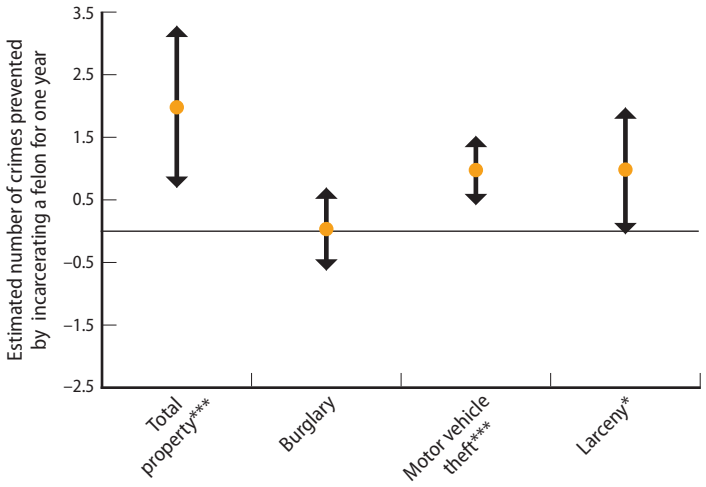
suffered the longest economically had larger increases in crime in 2012. As it turns out, we find no indication that our estimates are likely to be affected by these factors.[5]

Third, we control for overall statewide trends in crime and incarceration rates. The objective of doing so is to make sure that we do not assign changes in crime to realignment if those changes are part of broader trends also experienced in other states. The substantial variation within counties over the course of the first post-realignment year allows us to analyze whether counties that experience above average monthly declines in their incarceration rate also experience above average monthly increases in crime.[6]

In what follows, we present estimates both with and without corrections for the overall state-level trends. In a technical appendix, we provide even further variants. Our preferred estimates are those that adjust for all three factors discussed here. However, we present the alternatives to allow readers to view the sensitivity of the results to various analytical choices.

We should note that our approach provides an estimate of the effect of realignment-induced changes in incarceration on crime rates and that these estimates may differ from what one might expect from similar-sized reductions in other states or further reductions in incarceration in California.[7] The results presented here should be interpreted as the effects on crime of a change in the incarceration rate for a system with a pre-change rate hovering around 425 per 100,000 (roughly speaking, California's rate before September 2011).

Figure 4a. Realignment contributed to increases in larceny and motor vehicle thefts



SOURCE for Figures 4a–c: Authors' estimates based on monthly county-level crime, prison, and jail data obtained from the California Department of Justice's Criminal Justice Statistics Center, the CDCR, and the BSCC Jail Profile Survey.

NOTES for Figures 4a–c: The dots in the figure are based on the estimated regression coefficients, multiplied by –12 to obtain annualized estimates, from separate regressions of the difference-in-difference characterization of the change in the county's crime rate on the corresponding change in the county's prison incarceration rate controlling for changes in jail incarceration rates and county fixed effects. The length of the vertical bars represents the corresponding 95 percent confidence interval. Due to the small margin of error, the black arrows for murder cannot be seen in Figures 4b and 4c. (See the technical appendix for a detailed discussion.)

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

Realignment Affected Property Crime, but Evidence on Violent Crime Is Less Certain

Throughout this analysis, we estimate, on a per-year basis, the increase in crime rates for each one-person decrease in the rate at which county residents are incarcerated. In other words, we are analyzing whether crime increases as more former offenders are on the streets.

First, we show results that do not adjust for state-level trends. We begin with estimates for property crime (Figure 4a). The figure illustrates the number of crimes prevented per year of incarceration—this number is represented by the orange dot.[8] The black arrows represent the margin of error of our estimate. When the arrows cross the zero line along the vertical axis, this indicates that the estimate is not statistically significant (that is to say, a value of zero is within our margin of error). But when the range of the arrows lies above zero, the estimate is statistically significant.

On average, if realignment causes one less year of incarceration, then we see roughly two more property crimes per year, with the effect split between one motor vehicle theft and one larceny theft. In other words, a year in prison prevents about two property crimes a year. The estimates for total property crime and motor vehicle theft are both highly statistically significant, but the estimate for larceny is barely significant. We find no evidence of an effect on burglary.

A similar analysis of violent crime shows a different story (Figure 4b). There is no evidence that realignment resulted in an increase in murder or rape, with the estimates near zero and statistically insignificant. We find

Figure 4b. Realignment's effects on violent crime were very small

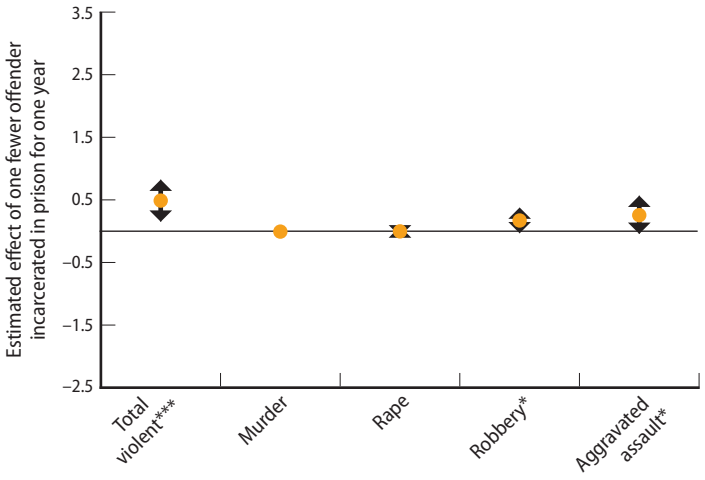
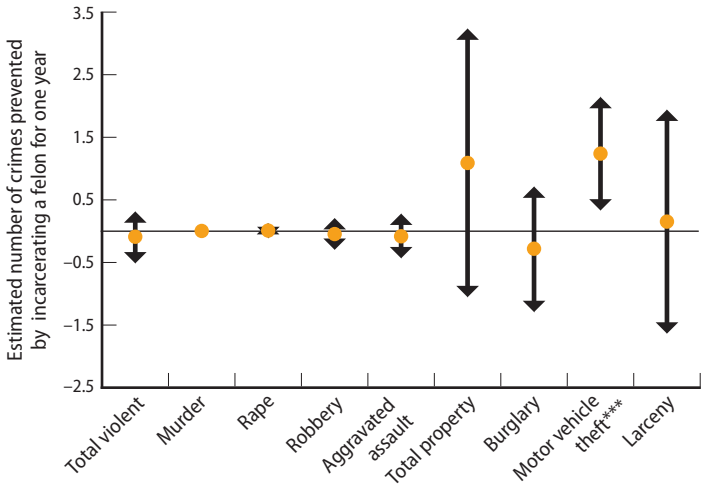


Figure 4c. When accounting for broader crime trends, realignment's effects on motor vehicle theft stand out



small and marginally significant effects on robbery and aggravated assault. These estimates suggest an increase of about 0.2 robberies and 0.3 aggravated assaults per year for each offender not incarcerated as a result of realignment. What happens to these estimates when we adjust them for underlying statewide trends, including broader trends experienced in other states? When we control for these trends, the picture changes quite a bit. All evidence of an effect of realignment on violent crime vanishes. All of the estimates are near zero and statistically insignificant.

The estimates for property crime decline as well, with the overall estimates for total property crime dropping to 1.1 incidents per year and the estimate for larceny theft dropping to slightly greater than zero. Both of these estimates are now statistically insignificant (Figure 4c). However, the estimate for motor vehicle theft remains statistically significant. Moreover, the overall effect is somewhat greater, implying an additional 1.2 motor vehicle thefts per year for each offender not incarcerated as a result of realignment.

This analysis provides robust evidence that changes in incarceration caused by realignment have increased property crime, especially motor vehicle thefts. Our results corroborate what we observed above in the statewide monthly trend data, pointing toward realignment exerting an upward pressure on property crimes. Our results show that, at most, realignment increased the number of property crimes by two per year for each realigned offender who is no longer incarcerated—and this number is probably more on the order of 1 to 1.5 additional property crimes, limited to auto thefts.

The results for violent crime are not particularly strong. There is no evidence in any of our analyses of an effect on murder rates or the rate of sexual assault. Adjusting for state-level trends eliminates all evidence for robbery as well as aggravated assault. Given that our preferred estimates adjust for state-level trends, we conclude that there is no robust evidence of an effect of realignment to date on violent crimes within the state.

This analysis provides robust evidence that changes in incarceration caused by realignment have increased property crime, especially motor vehicle thefts.

How Does California Compare to Other States?

So far, our analysis has focused solely on California, relying on comparisons across counties. But it is also useful to look at how California fits in with other states. We find that the data from other states suggest that factors other than realignment may be at least partially behind the changes in crime we see in California.[9]

We begin by examining the 2011–2012 annual data available in the Federal Bureau of Investigation's (FBI's) Uniform Crime Report (UCR) and comparing what happened in California to what happened both in neighboring states and in other states throughout the country.

The UCR data indicate that the number of violent crimes in California rose by 3.9 percent between 2011 and 2012, greater than the nationwide increase of 0.7 percent (Table 2). At the same time, ten other states in the country experienced larger increase in violent crime. Among western states, Nevada and Arizona saw greater increases than California, by 8.5 percent and 4.9 percent, respectively. In California, increases between 2.2 percent and 5.1 percent occurred in all four violent crime offenses (murder, rape, robbery, and aggravated assault). At least 15 other states saw greater increases in each violent crime category.

When compared to other states, California's increases in the number of property crimes are greater and more noticeable than the rise in violent crimes (Table 3). In stark contrast to the nationwide decrease in property crime of 0.9 percent, overall property crime in California increased by 7.8 percent, ranking fifth among all states. This substantial annual increase is greater than the combined increase of 2.2 percent in California's neighboring states (Arizona, Nevada, and Oregon); however, it is not unique to the states in our region. Two other western states saw even greater increases in property crime rates, Nevada by 10.6 percent and Montana by 8.7 percent. California's burglary and larceny theft increases, by 6.8 and 6.4 percent, respectively, were the seventh largest in the country. California's one-year increase in motor vehicle thefts of 14.8 percent stands out more than any other—and ranks third among all states. The only western state with a greater increase in this area was Montana.

The simple comparisons of changes in crime in California to those in other states do not provide a clear and

Table 2. Changes in violent crime in California are similar to changes in many other states

	Violent crime	Murder	Rape	Robbery	Aggravated assault
California	3.9%	5.1%	2.2%	4.1%	3.8%
Rank among all states	11	18	22	16	16
Other western states					
Arizona	4.9%	–9.8%	–8.9%	3.4%	8.0%
Colorado	–0.4%	4.5%	–7.5%	2.9%	0.1%
Idaho	3.6%	–17.1%	7.9%	29.3%	1.3%
Montana	–0.7%	–6.9%	3.6%	11.7%	–2.3%
Nevada	8.5%	–10.8%	2.0%	14.2%	6.9%
New Mexico	–2.0%	–26.6%	11.8%	7.4%	–4.7%
Oregon	0.1%	9.5%	–8.0%	7.9%	–1.2%
Utah	5.9%	0.0%	4.7%	1.9%	7.6%
Washington	1.2%	26.4%	–5.5%	2.1%	1.7%
Wyoming	–6.7%	–22.2%	5.5%	–14.1%	–7.7%
Nationwide	0.7%	1.1%	0.2%	–0.1%	1.1%

SOURCE: FBI Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data.

NOTE: The percentage changes refer to the change in the annual number of crimes between 2011 and 2012.

consistent picture of whether the state's trends are truly unique. For example, for both violent and property crimes, we observe higher as well as lower increases in neighboring states compared to those observed in California. The underlying challenge, then, is to determine which state or states best represent an appropriate comparison group to California. For that, we turn to an empirical approach that lets the state-level data tell us which combination of states best represents what the crime rates would have been in California had the state not implemented realignment. For each crime category, then, we compare the pre-realignment crime trends of all states to those of California to find the combination of states that best matches California's pre-realignment trend.[10] Details of how we implement this data-driven matching strategy, known as a synthetic control method, are provided in the technical appendix.

To test whether the differences between California and the matched comparison states are statistically significant, we rerun the matching process for each of the other 49 states to generate their own set of matched states and then compare the observed 2012 differences to the pre-realignment-year differences. A ranking of the magnitude of the estimated changes, roughly realignment simulations, tells us whether California's changes stand out and provides the basis for statistical significance.

This method allows us to match California's pre-realignment violent crime rate trend closely to a set of comparison states (Figure 5). As with our examination of county differences across California, this analysis provides no convincing evidence of an effect of realignment on violent crime rates.

We do find that the 2012 violent crime rate in California is somewhat above the rate of the matched comparison states. However, judging by the statistical significance of the estimated effect of realignment, we cannot conclude that realignment is behind the uptick; the magnitude of the increase ranks 13th when we simulate a policy change in all other states.

We also analyze each of the four violent crime offense trends separately and find that changes in murder, rape, and aggravated assault in California do not stand out when compared to changes in other states (results are shown in

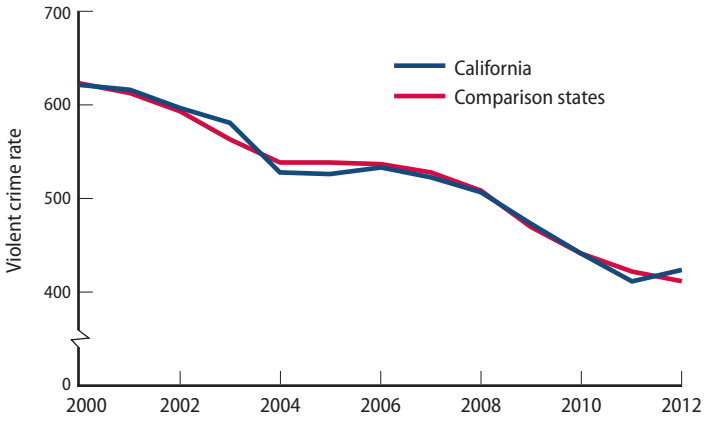
Table 3. Changes in motor vehicle theft in California stand out

	Property crime	Burglary	Larceny theft	Motor vehicle theft
California	7.8%	6.8%	6.4%	14.8%
Rank among all states	5	7	7	3
Other western states				
Arizona	0.9%	–3.2%	2.9%	–3.2%
Colorado	4.9%	1.7%	5.2%	9.9%
Idaho	–3.7%	3.9%	–6.2%	2.5%
Montana	8.7%	16.3%	6.8%	16.6%
Nevada	10.6%	8.7%	13.0%	4.6%
New Mexico	2.1%	–0.2%	3.2%	1.2%
Oregon	3.2%	5.8%	1.7%	12.0%
Utah	1.6%	–1.7%	2.7%	–3.0%
Washington	3.3%	7.4%	1.5%	6.4%
Wyoming	2.7%	14.0%	0.2%	12.1%
Nationwide	–0.9%	–3.7%	0.0%	0.6%

SOURCE: FBI Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data.

NOTE: The percentage changes refer to the change in the annual number of crimes between 2011 and 2012.

Figure 5. Violent crime rate trends in California closely track trends in other states, before and after realignment



SOURCE: Authors' estimates based on annual state-level data from the FBI Uniform Crime Reports, 2000–2012, prepared by the National Archive of Criminal Justice Data.

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).

technical appendix Table A5). For the most serious crimes—murder and rape—the post-realignment increases in California do not rank among the top ten largest increases; the increase in aggravated assaults ranks ninth. None of these changes in California is statistically significant.

The evidence of realignment's effect on robberies is mixed and depends on how the pre-realignment period is defined. If we base it on the five-year period 2006–2010 or the three-year period 2008–2010, the estimated increase of slightly more than six more robberies per 100,000 residents in California ranks fourth largest—a ranking that we would interpret as statistically significant. However, if we instead focus on the changes between 2010 and 2012, the increase of slightly less than six robberies in California ranks tenth, which leads us to conclude that the change is not statistically significant.

Increases in property crime rates in California are more noticeable than the changes in the comparison states and can be more convincingly tied to realignment. As with the violent crime trend, California's pre-realignment property crime trend closely matches a set of comparison states (Figure 6). However, these trends start to diverge in 2011, the year in which realignment was implemented, and by 2012 there is a noticeable gap.

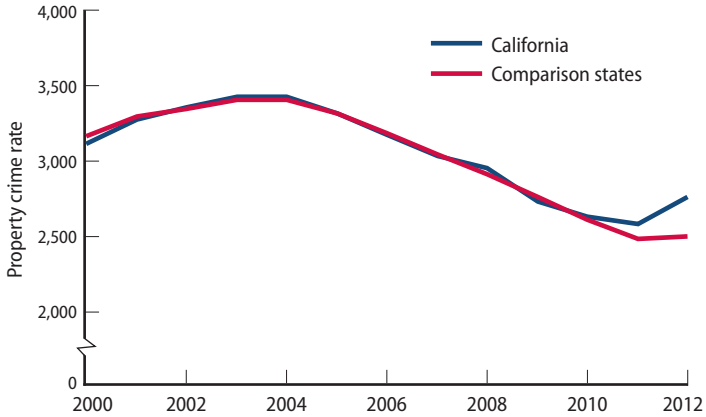
However, the estimated effect of realignment on property crime rates, of about 250 more property crimes per 100,000 residents, is not the largest we obtain, as the post-realignment increase in California ranks fifth among all states (results are shown in technical appendix Table A6).

We find no convincing evidence of an effect of realignment on violent crime, with the possible exception of an increase in robberies.

This finding can roughly be interpreted as a marginally statistically significant increase tied to realignment.

As in our analysis of the county data, the separate analysis of each property crime offense generates more precise and convincing evidence of realignment's effect. We find that increases in property crime are limited to increases in motor vehicle thefts. Our estimates indicate that the motor vehicle theft crime rate went up by about 70 thefts per 100,000 residents, just slightly above the estimate of

Figure 6. Property crime rate trends in California diverge markedly from trends in other states after realignment



SOURCE: Authors' estimates based on annual state-level data from the FBI Uniform Crime Reports, 2000–2012, prepared by the National Archive of Criminal Justice Data.

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).

65 thefts per 100,000 residents that we obtain from the analysis of county-level monthly data. California's post-realignment increase ranks first and is substantially greater than the simulation estimates obtained for any other state. The estimated effects on burglaries and larceny are much smaller (around 40 more burglaries per 100,000 residents and 23 more larceny thefts per 100,000 residents). These estimates do not stand out when compared to the estimates we obtain for other states: California's post-realignment increases rank 12th for burglaries and 20th for larceny thefts.

To summarize, the matching analysis of state-level data generates results very similar to our analysis of county monthly data. We find no convincing evidence of an effect of realignment on violent crime, with the possible exception of an increase in robberies. But we do find that property crimes, specifically motor vehicle thefts, started to increase noticeably in California around the time that realignment began.

How Might Further Reductions in Incarceration Affect Crime?

Although the total prison population has declined by about 27,000 since the enactment of realignment, California still finds itself housing about 8,000 inmates over the federally mandated limit of 110,000. In the short run, the state may choose to meet the mandate by transferring some inmates to other facilities, but it may also have to rely on non-incarceration solutions, including early releases.

The effect of further reductions in the prison population surely depends on the specific affected offender population, which, depending on the implemented strategy chosen, may be different from the affected realignment population. Nonetheless, are there lessons to be learned from the realignment experience that can help us anticipate the effects on crime rates of such further reductions? To help answer this question, we again use county differences in pre-realignment prison incarceration rates to examine whether the crime-prevention effects of incarceration differ depending on these rates.

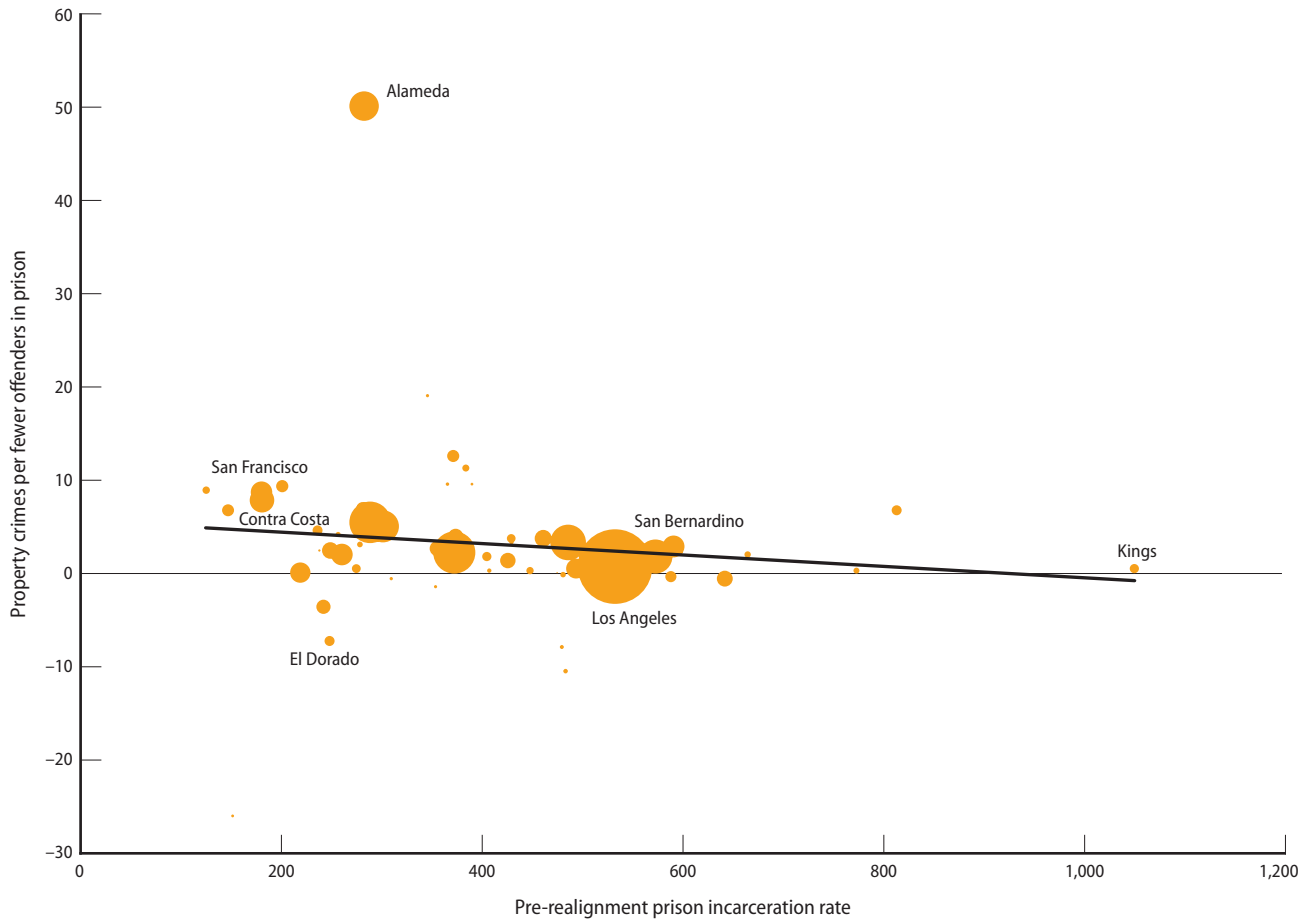
More specifically, we analyze the estimated number of property crimes per realigned offender.[11] On the one hand, if we see that the number of property crimes per realigned offender is the same in a low-incarceration county as in a high-incarceration county, this observation would suggest that the preventive effect of incarceration does not depend on the level of incarceration. On the other hand, and along the findings of existing research, if we find that the number of crimes per realigned offender is lower in high-incarceration counties than in low-incarceration counties, this would suggest that the crime-prevention benefits of incarceration diminish as incarceration rates increase. In the context of further reductions in the prison population, the latter scenario implies that as the prison incarceration rate drops further, we would also expect that those reductions, on a per-offender basis, would result in higher crime rates than those we obtain for the realignment-induced decrease in the prison population we rely on in this report.

The data suggest that as incarceration rates increase, fewer property crimes per realigned offender are actually prevented. This relationship is shown in Figure 7, in which the downward sloping line represents the estimated association between county crime and pre-realignment prison incarceration rates. The dots illustrate the size of the county population and represent the data analyzed, specifically, the number of property crimes per realigned offender

Although the total prison population has declined by about 27,000 since the enactment of realignment, California still finds itself housing about 8,000 inmates over the federally mandated limit of 110,000.

in the first year of realignment against pre-realignment prison incarceration rates. As we follow the line from left to right, the incarceration rate increases and—since the line is downward sloping—the number of crimes prevented

Figure 7. Property crimes per fewer offenders incarcerated in prison



SOURCE: Authors' estimates based on monthly county-level crime, prison, and jail data obtained from the California Department of Justice's Criminal Justice Statistics Center, the CDCR, and the BSCC Jail Profile Survey.

NOTES: The dots represent seasonally adjusted changes in the number of property crimes in the first year of realignment per offender not incarcerated as a result of realignment against pre-realignment prison incarceration rates. The size of the dot indicates size of the county population. The line represents the predictions resulting from the regression, shown in the top row of technical appendix Table A8. Prison incarceration rates represent the number of county residents in state prison per 100,000 residents.

per realigned offender decreases. More specifically, the downward sloping line indicates that incarceration in high-incarceration counties such as Kings and San Bernardino Counties prevented relatively few property crimes compared to low-incarceration counties such as San Francisco and Contra Costa. Alameda County is clearly an outlier.[12]

A number of relevant factors are not accounted for in this simple illustration of the relationship between the level of incarceration rates and crime, including county differences in jail incarceration after realignment, county differences in pre-realignment crime rates, or the sensitivity of outliers. (See the online technical appendix for an account of these factors and an explanation of alternative modeling assumptions.)[13]

A flexible model that accounts for the various factors mentioned above predicts that at the statewide pre-realignment prison incarceration rate of 435 inmates per 100,000 county residents, the incarceration of a realigned offender prevents about 2.1 property crimes per year (similar to the predictions seen in Figure 4a). The model predicts that, in counties with high prison incarceration rates, around the 75th percentile, for example, the incarceration of a realigned offender prevents about 1.6 property crimes per year. Del Norte County, with a

pre-realignment incarceration rate of 516 per 100,000 residents, is a good example of this scenario. The incarceration of a realigned offender in low-incarceration counties, at the 25th percentile, prevents about 4.3 property crimes per year. San Benito County, with a pre-realignment prison incarceration rate of 280, illustrates this example.

The results suggest that were the state to achieve the federal mandated reduction of the prison population of about 8,000 inmates by lowering incarceration, as opposed to transferring inmates to other facilities, the effect on property crime would be somewhat larger than shown in our analysis of realignment's current effect. On average, the property crime effect would be between 7 and 12 percent greater than the property crime effects we have estimated for 2011–2012.

Putting the Results in Perspective

Our examination of realignment's effect on crime raises a central question about corrections strategies: How effective is incarceration at preventing crime? Here, we look at this question from several angles. First, to assess the magnitude of our estimates, we compare our results to those from previous research. Second, we compare the costs of incarceration to its crime-prevention effects. Third, we explore alternative crime-control strategies that may yield crime reductions at lower costs.

Our Findings Echo Other Crime Studies

Not surprisingly, given the magnitude of the quick and substantial drawdown in the California's prison population (about 17 percent during the first year of realignment), there are no comparable studies for other states. However, there are several studies of the relationship between crime and incarceration that employ large data sets for all 50 states and track incarceration and crime over multiple years (see the technical appendix for a more detailed discussion of this line of research).

These studies generate estimates of the number of crimes prevented per year of incarceration that are comparable to our estimates for California. For example, one recent study finds that at the high incarceration rates observed in the United States over the last two decades, the average prison year served prevents between 1.3 and 2 property crimes, but also that marginal increases of incarceration do not have a statistically significant preventive effect on violent crime (Raphael and Stoll 2013). This closely parallels our findings that in the context of realignment, about 1.2 motor vehicle thefts on average were prevented for each additional year of prison incarceration, but that there was no robust and convincing evidence of an effect on violent crime.

Our examination of realignment's effect on crime raises a central question about corrections strategies: How effective is incarceration at preventing crime?

In addition, existing research provides evidence that crime-prevention effects decline with the scale of incarceration (see, for example, Liedka, Piehl, and Useem 2006). That is, when incarceration rates are low, such as they were in the United States as recently as in the 1970s and 1980s (averaging around 166 per 100,000 residents), increases in incarceration tend to generate large reductions in both property and violent crime.[14] Conversely, when incarceration rates are higher, at the recent nationwide levels as well as in pre-realignment California (around 450 per 100,000 residents), small increases in incarceration generate quite small reductions in crime. This analysis is in line with our findings in this study and is a textbook example of what economists refer to as diminishing returns to scale.

Costs of Incarceration and Potential for Alternative Crime-Reduction Strategies

Although inherently difficult and controversial, the crime prevention associated with incarceration can be put in

the context of cost-benefit analysis. Assuming that costs associated with crime can be measured reliably, the cost of crimes avoided because of incarceration can be juxtaposed against the costs of incarceration itself. Clearly, the costs associated with violent crime are both more controversial and more difficult to ascertain than are the costs associated with property crimes. Nonetheless, there is a growing body of research that places a dollar value on the social costs of specific criminal offenses. The general approach is to obtain estimates of so-called "willingness-to-pay" to reduce the probability of experiencing an undesirable outcome, such as having one's car stolen (the approach is similar to what is used to generate estimates of other difficult-to-obtain costs, such as those associated with pollution).[15]

Our findings suggest that each prison year served prevents 1.2 auto thefts. These estimates suggest that one prison year for a realigned offender would prevent $11,783 in crime-related costs.

Our findings suggest that each prison year served prevents 1.2 auto thefts. One important study by the RAND Corporation has suggested that an auto theft today costs on average $9,533 (Heaton 2010). Put together, these estimates suggest that one prison year for a realigned offender would prevent $11,783 in crime-related costs. The annual cost of incarcerating a prison inmate in California is $51,889, according to the Legislative Analyst's Office (2013). According to these numbers, then, the state receives only about 23¢ return on each $1 spent on incarceration.

Above, we noted that there is no robust evidence of an effect of realignment on violent crime—but there is some evidence that the robbery rate might have increased because of the policy (by slightly more than six more robberies per 100,000 residents). Applying and adding the RAND estimated cost of a robbery today at $70,641 to our calculations generates an estimated return of about 48¢ in terms of crime prevention for each dollar spent on prison incarceration. Hence, the benefits in terms of prison expenditure savings outweigh the costs in terms of somewhat higher property crimes, and this holds true even if we account for the possible increase in robberies resulting from realignment.

This simple cost-benefit analysis is useful for thinking about whether on the margin the social expenditures we are making are justified. However, this analysis considers the effectiveness of a particular policy intervention in isolation, without considering what could be achieved by reallocating the saved resources to other uses. For example, it may be the case that a reduction in incarceration without 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 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.

Perhaps the most obvious approach with the strongest research base is the expansion of local police forces. There is considerable empirical evidence on the general effectiveness of higher police staffing levels on crime (see, for example, Levitt 1997, 2002; Chalfin and McCrary 2012; Di Tella and Schargrodsky 2004; Evans and Owens 2007; and Corman and Mocan 2000). These studies consistently find relatively large effects on local crime rates from expanding city police forces. One study estimates that the benefits in terms of reduced crime from hiring an additional police officer exceed $300,000 per year in several cities; this figure substantially exceeds the annual cost of an additional officer (Heaton 2010). Although some of the benefits from expanding local policing most certainly derive from apprehending and incarcerating highly criminally active individuals, a more consistent police presence is also likely to deter criminal activity, especially among those who may be transitionally passing through a

high-offending age range and whose future life in crime is certainly not a preordained outcome.[16]

Of course, we have discussed only one possible alternative intervention, but many policy options could and should be explored by researchers and policymakers. Short-term approaches include alternative systems of managing probationers and parolees, including swift-and-certain yet moderate sanctions systems, such as Hawaii's Opportunity Probation with Enforcement (HOPE), or high-quality cognitive-behavioral therapy interventions for adult offenders. Longer-term interventions include investments in early childhood programs and targeted interventions for high-risk youth. In sum, a variety of policy interventions can likely be deployed to combat crime in California—interventions that would not require California's past incarceration rates to maintain low crime rates.

Conclusions

Corrections realignment, California's answer to a federal court order to substantially reduce its prison population, quickly shrank the state's overcrowded and expensive prison system. Although still short of the mandated target, realignment has so far reduced the prison population by about 27,000 inmates. This quick and significant decline was achieved by limiting parolee returns to state custody and by sentencing lower-level offenders to county jails rather than state prison. Although county jails absorbed many of the offenders affected by the legislation, a recent PPIC report shows that realignment markedly decreased the overall reliance on incarceration in California (Lofstrom and Raphael 2013). The estimates in that report reveal that about 18,000 lower-level offenders are now on the streets who in past years would have been in either prison or jail.

We find little evidence that the substantial reductions in the state's prison population caused by realignment have increased violent crime. Violent crime rates remain at historically low levels—they are not higher than comparable rates for 2010—and the slight increase that occurred between 2011 and 2012 appears to parallel what has happened in comparison states. We find no sign that the most serious crimes—murder and rape—have increased as a result of realignment. However, there is some indication that robberies may have increased, on the order of around six more robberies per year per 100,000 residents, as a result of realignment.

ISTOCKPHOTO

Expanding local police forces has proven to be cost-effective in reducing local crime rates.

We do find convincing and robust evidence of an effect on property crime. We observe that property crime increased with the implementation of realignment by a rate that exceeds the rate nationwide—and, more important, by a rate that exceeds that of a group of states with pre-realignment crime trends similar to those in California. We also find that counties with larger increases in the number of realigned offenders per capita also experienced larger increases in property crime rates. For the most part, this effect appears to operate entirely through growth in auto thefts. We estimate an average increase of about 1.2 auto thefts per year for each realigned offender who is not incarcerated as a result of realignment. Overall, this translates to an increase in the motor vehicle theft crime rate, caused by realignment, of about 65 more auto thefts per year per 100,000 residents. This increase, of about

24,000 auto thefts per year, reverses a declining trend in this theft rate and brings it back to 2009 levels.

Because California is still housing about 8,000 more inmates than the federally mandated limit of 110,000, we analyzed the effect that further reductions in the prison population might have on crime rates, keeping in mind that this effect would depend on the specific offender population involved. We find that if the state were to achieve the federal mandated reduction by lowering incarceration, as opposed to transferring inmates to other facilities, the effect on property crime would be somewhat larger than in our analysis of realignment's current effect. We estimate that, on average, property crime would be between 7 and 12 percent greater than the property crime numbers we have estimated for 2011–2012.

We also find evidence that the crime-reducing benefits of incarceration decrease as incarceration rates rise. Our analysis suggests that incarceration prevented fewer crimes in counties that had relatively high pre-realignment prison incarceration rates, such as Kings, Kern, and Fresno Counties, than in counties with low pre-realignment reliance on prison incarceration, such as San Francisco and Marin Counties.

Taken together, our findings indicate that the state has not been receiving a very good return on its prison incarceration investments. We suggest that alternative crime-reducing strategies—for example, increased policing—could likely provide improved outcomes at lower costs. Our cost-benefit analysis suggests that, on average, $1 spent on the incarceration of realigned offenders generates between 23¢ and 48¢ in terms of the value of crimes averted. However, credible existing research finds that each $1 invested in policing generates $1.6 in crime savings. Put differently, between 3.5 and 7 as many crimes would be prevented by spending an additional $1 on police rather than spending it on prison incarceration.

Realignment has brought enormous changes to California's corrections system, and it appears to have affected some crime rates as well—motor vehicle thefts in particular. Any increase in crime is cause for concern. But safer and smarter approaches to corrections and crime prevention are within reach. As the realignment process continues to unfold, the state—and the counties—should look to a variety of ways to effectively, and cost-efficiently, handle their public safety responsibilities. ●

A technical appendix to this report is available on the PPIC website:
www.ppic.org/content/pubs/other/1213MLR_appendix.pdf

Notes

[1] In the case of pretrial detainees, these would be alleged crimes (Lofstrom and Raphael 2013).

[2] The proposals include AB 2 (which proposes to send sex offenders who violate their parole back to state prisons instead of county jails), AB 605 (which would send sex offenders who violate any provision of their parole back to state prison), AB 63 (which would make it a felony for individuals released on parole, post-release community supervision, or mandatory supervision to remove court-ordered GPS monitoring devices), SB 57 (which would send sex offenders to county jail for a period of 180 days for removing court-ordered GPS monitoring devices), and AB 601 (which would allow parole violators to be returned to state prison for up to one year). Of these proposals, SB 57 is the only proposal to pass the legislature and be signed by Governor Jerry Brown during the 2013 session.

[3] To make the pre- and post-realignment comparison cleaner, we compare the first nine months of 2011 to the first nine months of 2012 (as the legislation was enacted as of October 1, 2011, the following three months in 2011 are post-realignment months).

[4] Two rather technical points warrant a brief clarifying discussion here. First, the data we use measure seasonally adjusted changes in the crime and incarceration rates between September 2011 (the last pre-realignment month) and the subsequent 12 post-realignment months. That is, we focus on correlation in the *changes* of these rates, as opposed to comparisons of levels. Second, this means that in our preferred identification strategy, which includes county fixed effects, we identify crime effects based on the deviations from the seasonally adjusted within-county average changes in post-realignment crime and incarceration rates. Put slightly differently, it is not the post-realignment levels of incarceration and crime across counties that identify the effects; instead, effects are estimated from the within-county changes in crime and incarceration rates' deviations from the within-county post-realignment period average. This modeling decision has the advantage that the potential effects of important county-specific realignment strategies, such as the initial funding allocation decisions, have been purged from the data.

[5] Although no suitable data are available that can be directly incorporated into our estimation, we turn to the California Department of Justice's publicly available annual Law Enforcement Personnel file to examine whether recent changes in the number of law enforcement personnel is correlated with the "realignment dose" (that is, the change in county prison incarceration rates caused by realignment, which is driven by the reliance on prison incarceration before the policy implementation) (as measured in September 2012). However, this reveals a weak, statistically non-significant, negative relationship between the changes in the number of sworn officers in law enforcement agencies and the realignment dose. The Law Enforcement Personnel file contains county-level information on the number of sworn officers in law enforcement agencies as of October 31 of each year. We explore specifically the sensitivity of our preferred estimates to the inclusion of monthly county unemployment rates and find that the estimated effects are robust and, furthermore, that there is no evidence in these specifications of a statistically significant relationship between unemployment and changes in crime rates.

[6] One might contend that purging the data of the overall state-level trends may effectively throw out any general deterrent effects caused by realignment that are affecting crime statewide. To the extent that this is the case, our estimates controlling for state trends may be underestimating the effects of realigned offenders on crime. However, prior research on the prison-crime relationship has found that nearly all of the contemporaneous effect of prison on crime operates through incapacitation (see, in particular, the discussion in Buonanno and Raphael 2013 and Raphael and Stoll 2013). Moreover, as we will show, the violent crime trends in neighboring states strongly suggest the need for such controls. Finally, the estimated crime effects for the offense for which we see the strongest evidence of an effect of realignment (motor vehicle theft) exhibit little sensitivity to this control, suggesting that true realignment-induced effects survive this statistical trend adjustment.

[7] A large body of research assessing the effects of changes in incarceration on crime tends to find that incarcerating a convicted criminal offender does, on average, reduce crime through incapacitation (essentially reduced "street time") and deterrence, with the lion's share of the reduction operating through incapacitation. However, this research also documents a decreasing crime-prison effect as incarceration rates increase (what economists refer to as diminishing returns to scale) at quite low levels of incarceration and very small crime effects at the incarceration rates that currently characterize most U.S. states, including California. See the technical appendix for a discussion of the existing relevant literature.

[8] By realigned offenders, we mean individuals who, before realignment, would have been locked up in either state prison or county jail but as a result of the legislation are now not incarcerated.

[9] In addition to changes in incarceration, factors that affect crime trends include demographic shifts (for example, in age and race/ethnicity), economic conditions, the dynamics of illegal drug markets, law enforcement personnel, and policing strategies. The influence of these factors on crime trends varies with type of crime. Changes in sentencing laws are further contributors, although these effects may well be through incarceration.

[10] Technical appendix Table A7 shows which specific states and weights generate the best match for each of California's nine crime rate trends analyzed.

[11] Specifically, we analyze changes in the number of seasonally adjusted property crimes during the first year of realignment in relation to the decrease in the number of offenders incarcerated in prison by the end of the first year of the legislation.

[12] Although it is not entirely clear what explains Alameda's high rate, a closer look at the data reveals that the county saw one of the lowest drops in the prison population per 100,000 residents (that is, it received one of the smallest realignment doses) while also experiencing a substantial increase in property crimes (twice the state average, or about 16 percent) in the first year of realignment. This suggests that non-realignment crime-related factors are at play, of which a reduced number of police officers is one plausible factor. Countywide, Alameda has seen a continued decrease in the number of police officers since 2008 and by 2012 had lost more than 200 officers, a decrease of about 11 percent.

[13] We find that although the magnitude of our estimates is sensitive to modeling assumptions, the estimates consistently reveal that as incarceration increases, there is a smaller effect on property crimes, consistent with existing research (see, for example, Liedka, Piehl, and Useem 2006).

[14] For example, for this time period, Raphael and Stoll (2013) estimate that each prison year served prevented roughly 1.2 violent felony offenses and 8.6 property offenses, roughly in line with Levitt's (1996) estimates.

[15] A RAND Corporation study (Heaton 2010) summarizes the approaches used and generates societal crime cost estimates based on the relevant literature. It is also worthwhile to note that cost-benefit calculations attempting to identify cost-effective crime-prevention strategies are used to calculate returns on investment estimates such as those generated by the Washington Legislature created Washington State Institute for Public Policy.

[16] Perhaps the most rigorous analysis of the effects of additional police on crime is provided in a recent study by Aaron Chalfin at the University of Cincinnati and Justin McCrary at the UC Berkeley Law School (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 $1.6 in crime savings. The authors conclude from these findings that the level of police staffing levels in the United States is too low. Note that our cost-benefits analysis for prison suggests that $1 of additional incarceration generates between 23¢ and 45¢ in crime savings. In other words, the average benefit-cost ratio for incarcerating those who are now on the street as a result of realignment falls far short of one.

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Technical Appendix

Public Safety Realignment and Crime Rates in California

Magnus Lofstrom and Steven Raphael

with research support from Brandon Martin

Supported with funding from the Smith Richardson Foundation

Appendix

Data

The data for this project come from several sources. Crime data are provided by the California Department of Justice's Criminal Justice Statistics Center, within the Office of the California State Attorney General. Crime totals for part 1 felony offenses are reported by month and police agency (these data are eventually released by the FBI, along with data for other states, in the Uniform Crime Report Series). The data include county identifiers that permit summing total offenses by county and month. Data are currently publicly available for the period 2003 through 2012. We use data for 2010 through 2012 for this project.

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. There are 58 counties, but one small rural county, Alpine, does not maintain its own jail system. Hence, nearly all of our analyses (with the exception of the descriptive crime trends presented in the main report) focus on the 57 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 from October 2010 through December of 2012. 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.

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 the future offending of former prison inmates. Although 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 using 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 10 to 20 fewer serious felony offenses (Marvell and Moody 1994; Spelman 1994, 2000). However, most of this research employs prisoner surveys fielded during the 1970s and 1980s, years when the U.S. incarceration rate was much lower than contemporary levels. 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, Chapter 7), one might expect lower incapacitation effects from studies employing more recent data.

The 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. Barbarino and Mastrobuoni (2012) construct a panel data set 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 effects 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 13 to 17 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. Both findings are consistent with great heterogeneity in offending behavior among those convicted and sent to prison and a decline in this propensity to offend among the incarcerated as the scale of incarceration increases.

Vollaard (2012) estimates incapacitation effects for repeat offenders exploiting a change in Dutch sentencing policy. The author analyzes the effect 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 50 to 60 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 diminishing marginal incapacitation effects setting in quite quickly, with Buonanno and Raphael finding substantial declines in incapacitation effects at levels below 200 per 100,000 and Vollaard finding declining marginal criminality even among offenders with ten or more prior convictions.

Several studies of the crime-prison relationship are 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, although 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. Using a series of variables measuring the status of prisoner overcrowding lawsuits as instruments for state-level incarceration rates, Levitt finds two state least squares estimates of crime-prison elasticities that are considerably larger than comparable estimates from ordinary least squares, 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 effect of this difference on next-year's change in incarceration, based on a theoretical model of the relationship between crime and incarceration, and derive the conditions under which the transitory disparity between the actual and steady state incarceration rate provides a valid instrument for one-year-lead changes in the actual incarceration rate. 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), and the latter period is characterized by a much a higher incarceration rate (396 per 100,000). For the early period, an additional prison year served is estimated to prevent roughly 2.5 felony violent offenses and 11.4 felony property offenses. Note that the total crimes prevented figure is quite close to the implied annual reverse incapacitation effects caused by the 2006 Italian pardon (Buonanno and Raphael 2013) and, when expressed as elasticites, is quite close to the estimates in Levitt (1996) using a much different identification strategy.

However, the estimates for the latter time period 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 find 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. Liedka, 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 an analysis of a single state experiencing a very large and relatively sudden decline in incarceration rates.

Empirical Strategy for Estimating Cross-County Relationships Between Realignment and Crime

Our principal estimation strategy exploits this cross-county variation in the effect 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

$$(1)\quad \Delta Crime_{it} = \alpha_i + \beta_t + \delta \Delta Prison\ Incarceration\ Rate_{it} + \gamma \Delta Jail\ Incarceration\ Rate_{it} + \varepsilon_{it},$$

where i = (1, …, 57) indexes counties, t = (October 2011, …, September 2012) indexes the end month of the change, $\Delta Crime_{it}$ is the pre-post realignment change in monthly crime rates, $\Delta Prison\ Incarceration\ Rate_{it}$ is the pre-post realignment change in the number of county residents incarcerated in a state prison, $\Delta Jail\ Incarceration\ Rate_{it}$ is the pre-post realignment change in the number of county residents incarcerated in a local county jail, 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 specification of equation (1) and the variation used to identify the key coefficient of interest, δ.

First, the reform explicitly provides for the incarceration of non-violent, non-sexual, and non-serious offenders in local jails as well as for discretion for local 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. Figure 5 illustrates this fact. The state's total jail population was roughly 72,000 in the months before realignment and then gradually increased to over 80,000 over the first post-reform 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) affects 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. This is a particular important adjustment, as preliminary crime data published by the FBI for 2012 suggests that among western states (and, in particular, states that share a border with California), crime increased in 2012.

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 lead to a larger jail population of inmates awaiting trial and transfer to prison, which in turn leads to a large prison population. However, the reform in question should identify the causal link running from to prison and crime rates, as the legislation and 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 way to characterize 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 earlier. Although we have data through December 2012, September 2012 is the last month for which the base month of the change would still lie within the pre-reform period. Focusing on the change relative to one year earlier 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 earlier.

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 pre-date 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. For example, in January 2010, California implemented probation reform, with the primary change being the implementation of performance incentive funding that granted local probation departments 40 to 45 percent of state corrections savings for reducing admissions to prison from the stock of convicted offenders on probation. In addition, in late 2009, the state implemented a reform to state parole, officially creating a new class of low-risk prison releases for less intensive post-release supervision. Combined, these two reforms reduced the

prison population between January 2010 and September 2011 from 167,694 to 160,482. Given the policy activity before 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 effects on crime, more clearly driven by the 2011 reforms.

As an alternative, 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 to increase crime rates. On the other hand, tourist visits to desert cities such as Palm Springs decline greatly during summer months, as does the time local residents spend outdoors because of 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 calculated by first measuring the difference in crime and incarceration rates between April 2012 and September 2011 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 also test for heterogeneity in the prison-crime effects by pre-realignment incarceration rates. Finally, all models estimation results presented below calculate robust standard errors that permit clustering by county.

Empirical Strategy for Estimating Realignment's Effect on Crime Using the Synthetic Control Method

To assess the effect of realignment on crime, we also analyze annual state-level data from the FBI Uniform Crime Report for the period 2000–2012. To estimate the effect of realignment on crime using the state-level data, we employ the synthetic control approach of Abadie, Diamond, and Hainmueller (2010). Key to the identification strategy is charting the appropriate counterfactual path for California in the absence of realignment. One could employ a number of approaches. One is to select states that presumably share characteristics and trends similar to California's—for example, the neighboring states (a traditional difference-in-difference approach). Another would be to employ a data-driven search for comparison states based on pre-realignment crime trends (the synthetic control method of Abadie Diamond, and Hainmueller 2010). Here we employ the latter strategy because it is arguably the most reliable and essentially incorporates the first strategy. It allows the data to tell us which states best match California's pre-realignment experience.

The synthetic control method allows for robust analysis in the single policy change—single state context. Here, we summarize the methodology for charting a counterfactual post-realignment path for California, limited to 2012. The basic idea is to generate a comparison group from a convex combination of states drawn from a large donor pool. Let the index j = (0,1, …, J) denote states. The value j = 0 corresponds to California and j = (1, …, J) corresponds to each of the other J states that are candidate contributors to the control group (or in the language of Abadie Diamond, and Hainmueller, the donor pool). Define F_0 as an 11 x 1 vector with elements equal to the offense-specific crime rates in California in years 2000 through 2010 (the 11 years we use here as our pre-intervention period). Similarly, define the 11 x J matrix F_1 as the collection of comparable time series for each of the J states in the donor pool (with each column corresponding to a separate state-level time series for the period 2000 through 2010).

The synthetic control method identifies a convex combination of the J states in the donor pool that best approximates the pre-intervention time series for the treated state. Define the J x 1 weighting vector $W = (w_1, w_2, ..., w_J)'$ such that $\sum_{j=1}^{J} w_j = 1$, and $w_j \geq 0$ for j = (1, …, J). The product $F_1 W$ then gives a weighted average of the pre-intervention time series for all states omitting California, with the difference between California and this average given by $F_0 - F_1 W$. The synthetic control method essentially chooses a value for the weighting vector, W , that yields a synthetic comparison group (consisting of an average of some subset of donor states) that best approximates the pre-intervention path for California. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

$$W^* = \arg\min_{W} (F_0 - F_1 W)' V (F_0 - F_1 W)$$

(2) $$s.t.$$

$$W' i = 1, w_j \geq 0, \; j = (1, ...J)$$

where V is an 11 x 11, diagonal positive-definite matrix with diagonal elements providing the relative weights for the contribution of the square of the elements in the vector $F_0 - F_1 W$ to the objective function being minimized.

Once an optimal weighting vector W^* is chosen, both the pre-intervention path as well as the post-intervention values for the dependent variable in "synthetic California" can be tabulated by calculating the corresponding weighted average for each year using the donor states with positive weights. The post-intervention values for the synthetic control group serve as our counterfactual outcomes for California.

Our principal estimate of the effects of realignment on crime uses the synthetic control group to calculate a simple difference-in-difference estimate. Specifically, define $Outcome_{pre}^{CA}$ as the average value of the outcome of interest for California for the pre-intervention period 2000 through 2010 and $Outcome_{post}^{CA}$ as the corresponding average for the only post-treatment year currently available in the UCR data, 2012. Define the similar averages $Outcome_{pre}^{synth}$ and $Outcome_{Post}^{synth}$ for the synthetic control group. Our difference-in-difference estimate subtracts the pre-intervention difference between the averages for California and synthetic California from the comparable post-intervention difference, or

$$(3) \qquad DD_{CA} = (Outcome_{post}^{CA} - Outcome_{post}^{synth}) - (Outcome_{pre}^{CA} - Outcome_{pre}^{synth})$$

To formally test the significance of any observed relative increase in California's crime rates, we apply the permutation test suggested by Abadie Diamond, and Hainmueller (2010) to the difference-in-difference

estimator discussed above.[1] Specifically, for each state in the donor pool, we identify synthetic comparison groups based on the solution to the quadratic minimization problem. We then estimate the difference-in-difference for each state as if these states had enacted the equivalent of California's realignment with comparable timing. The distribution of these "placebo" difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimate DD_{CA}. To be specific, if the cumulative density function of the complete set of DD estimates is given by $F(.)$, the p-value from a one-tailed test of the hypothesis that $DD_{CA} < 0$ is given by $F(DD_{CA})$.

Empirical Results

Table A1 presents estimation results for various specifications of equation (1). Panel A presents results where the dependent variable is the change in the total violent crime rate, and panel B presents results for the change in the total property crime rate. Within each panel, we present results for each of our characterizations of the pre-post change in the 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. However, this estimate 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 yields only 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 effect of variation in county incarceration rates on county violent crime rates. In several specifications, the coefficients are the wrong sign and in four of the five specifications are statistically insignificant. The one specification where the coefficient has the correct 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 specification 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 reflects broader forces influencing the entire state. The difference-in-difference models yields 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.

[1] Buchmueller, DiNardo, and Valletta (2009) use a permutation test similar to that described here to test for an effect of Hawaii's employer mandate to provide health insurance benefits to employees on benefits coverage, health care costs, wages, and employment.

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 provides estimates of the effect of a one-person increase in the prison incarceration rate for the period 1978 to 1990 and 1991 and 2004. During the earlier period, state incarceration rates averaged 186 per 100,000, whereas during the later period, state incarceration rates averaged 396 per 100,000. The authors find that, for the earlier period, each prison-year served reduced annual reported property crimes by 11.4 incidents and annual violent crimes by 2.5 incidents. During the later period, each prison-year served reduced annual reported property crime by 2.6 incidents and annual reported violent crime by 0.4 incidents. Raphael and Stoll (2013) update these results for a more recent period, from 2000 to 2010, when the average state incarceration rate was 449 per 100,000. They estimate 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 A1 comparable, we must multiply each coefficient by 12, since the estimation results here pertain to monthly crime. The largest point estimate in Panel A for violent crime suggests that each prison-month served prevents 0.041 violent incidents per month (the specification from the difference-in-difference models controlling for the change in the jail incarceration rate and county fixed effects). This coefficient estimate implies that each prison-year served prevents 0.5 violent incidents. For property crime, the largest coefficient comes from the difference-in-difference model adjusting for the change in the jail population and not controlling for month or county fixed effects (with a coefficient on the change in the prison incarceration rate of –0.183). This estimate implies that 2.2 reported property crimes per year are prevented per prison-year served. Note that 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 per prison-year served effects that are considerably lower than estimates from time periods in the United States when the incarceration rate was much lower.

Table A2 presents comparable estimation results for individual violent crimes. We present results for the same set of model specifications reported in Table A1. However, here we report only coefficients on the change in the incarceration rate to conserve space. For murder and rape, we find no evidence of an effect of realignment-induced declines in incarceration on these crimes. There is not a single negative and statistically significant coefficient in any of the specifications. For robbery, we find some evidence of a small adverse effect in some specifications. However, the robbery coefficient never survives controlling for month fixed effects, suggesting that factors influencing crime statewide explain these negative coefficients in the more parsimoniously specified models. 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. However, the difference-in-difference models generally find no effect of realignment on aggravated assault.

Table A3 presents 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 is the weakest. Note that the increase in larceny accounts for nearly 50 percent of the increase in property crime rates between 2011 and 2012 for the state, suggesting that the recent prison reforms provide only a partial explanation for the increase in property crime statewide. 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 coefficient 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.

Diminishing Incarceration Effects on Crime

To investigate whether the realignment effect on property crimes varies depending on pre-realignment prison incarceration rates, we analyze the number of seasonally adjusted property crimes per fewer offenders incarcerated in prison by the end of the first year of the legislation. We focus on property crimes, since our analysis only points toward a robust relationship between these types of crime and realignment.

The data suggests a negative relationship between pre-realignment prison incarceration rates and the number of property crimes per realigned offender. That is, as incarceration rates increase, fewer crimes are prevented by incarcerating an additional offender. More specifically, the downward sloping line in Figure 9 in the main report indicates that low incarceration counties, such as San Francisco and Contra Costa, saw more property crimes per realigned offenders (8.8 and 7.9, respectively, significantly above the statewide average of 2.6) than relative high prison incarceration counties such as Kings and San Bernardino (0.5 and 1.9, respectively). However, this analysis does not account for county differences in jail incarceration responses to realignment or county differences in pre-realignment crime rates. The negative relationship is not greatly affected by these factors, as our regression results presented in Table A9 show, and, if anything, becomes somewhat stronger. The latter estimates suggest that counties with a pre-realignment prison incarceration rate of 280 per 100,000 residents (which corresponds to San Benito County at the pre-realignment 25th percentile in the prison incarceration distribution) saw an additional 7.6 property crimes per fewer offenders not incarcerated in prison compared to a county with a pre-realignment incarceration rate of 516 (corresponding to Del Norte County at the 75th percentile).[2] This does not imply that low incarceration counties saw greater increases in crime rates than high incarceration counties as a result of realignment. The latter group of counties experienced greater decreases in incarceration rates, that is, more offenders on the street, than low incarceration counties and hence may have seen greater increases in crime rates.

The magnitude of the estimates, but not the statistical significance, is influenced by whether Alameda County is included in the regressions, however. As the figure clearly shows, Alameda County stands out with more crimes per fewer offenders incarcerated than any other county (about 2.5 times more than the second highest county).[3] Without Alameda County, the estimated difference for counties with incarceration rates of 280 and 516 drops to 2.8 fewer property crimes per non-incarcerated offender in the higher pre-incarceration county.

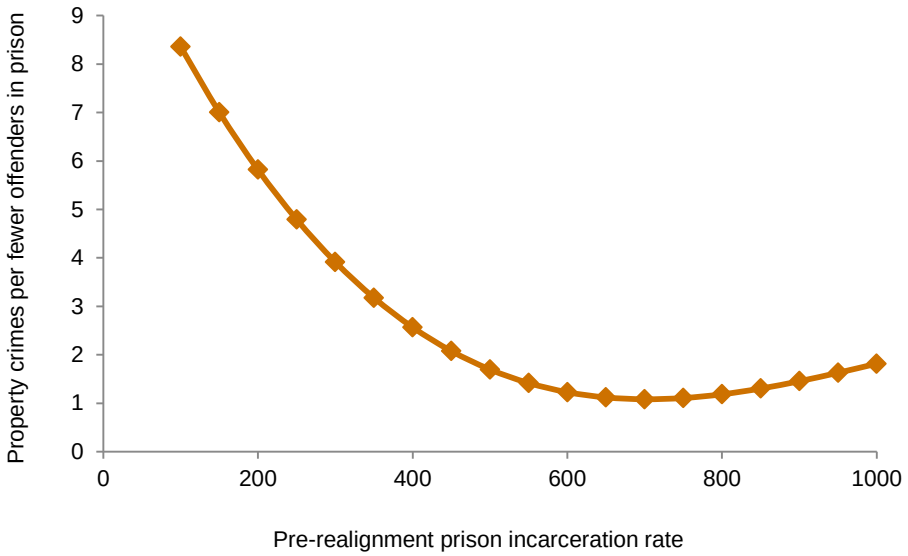
So far, we have assumed that the relationship between property crimes per fewer offenders incarcerated and reliance on prison incarceration is linear. However, both the data displayed in Figure 9 and existing research suggest that this may not be the case. Instead, a more likely relationship is one where a reduction in incarceration at low incarceration rates has a greater effect on crime than a reduction in incarceration at a high level of incarceration (that is, there are "diminishing marginal returns" to incarceration). Numerous functional

[2] In other words, the incarceration rate in San Benito at the 25th percentile exceeded that of only 25 percent of California counties, whereas the incarceration rate in Del Norte County at the 75th percentile is greater than that of 75 percent of the counties.

[3] Although it is not entirely clear what explains Alameda County's high rate, a closer look at the data reveals that the county saw one of the lowest drops in the prison population per 100,000 residents (that is, it received one of the smallest "realignment doses") while also experiencing a substantial increase in property crimes (twice the state average, or about 16 percent) in the first year of realignment. This suggests that non-realignment crime-related factors are at play, of which the reduced number of police officers is one plausible factor. Countywide, Alameda County has seen a continued decrease in the number of police officers since 2008 and, by 2012, it had lost more than 200 officers, a decrease of about 11 percent.

forms can capture this possible feature of the relationship, including natural logs and quadratic and higher-order polynomials. We explored a number of these possibilities and consistently found that the data support a non-linear relationship consistent with diminishing marginal returns to scale. One of these functional forms is a third-order polynomial, for which we present the results in Figure A1.[4]

FIGURE A1
Predicted number of property crimes per fewer offenders incarcerated as a result of realignment



SOURCE: Authors' estimates based on monthly county-level crime, prison, and jail data obtained from the California Department of Justice's Criminal Justice Statistics Center, the CDCR, and the BSCC Jail Profile Survey.

NOTE: The line represents the estimates based on a third-order polynomial regression of the relationship between the number of property crimes in the first year of realignment per offender not incarcerated as a result of realignment against pre-realignment prison incarceration rates.

The estimates suggest a sharp decline in the crime-reducing benefits of incarceration as incarceration rates increase. At the statewide pre-realignment prison incarceration rate of 435, the model predicts an increase of about 2.1 property crimes per year by decreasing prison incarceration by one inmate (similar to the predictions seen in Figure 4). Counties with high prison incarceration rates, around the 75th percentile, are predicted to see an increase of about 1.6 property crimes, whereas the model estimates that low-incarceration counties at the 25th percentile would see an increase of 4.3 property crimes. Although the magnitude of our estimates is sensitive to modeling assumptions, the estimates consistently reveal that as incarceration increases, there is a smaller effect on property crimes, consistent with existing research (see, for example, Liedka, Piehl, and Useem 2006 and Raphael and Stoll 2013).

The results suggest that were the state to achieve the federal mandated reduction of the prison population, of about 8,000 inmates, by lowering incarceration, as opposed to transferring inmates to other facilities, the effect on property crime would be somewhat larger per non-incarcerated offender than what we estimate for

[4] Alameda County is excluded from the regression because of its influence as an outlier in these data. The consequence of the exclusion is that the function shifts down and estimates smaller effects for lower incarceration rates, from about 150 to 500, but larger effects for incarceration rates up to around 1,000. A steep decline in the effects of incarceration on crime in the range that almost all counties operate, between 200 and 800 prisoners per 100,000 residents, remains.

the realignment-induced decrease in incarceration rates. Although the specific estimated effect of such a reduction (lowering the prison incarceration rate from about 360 to 335) is sensitive to how we specify our models, the range is relatively tight, suggesting that, on average, the property crime effect would be between 7 and 12 percent greater than the estimated property crime effects of the realignment-induced decrease in incarceration.

Comparing Our Estimates to Those in the Existing Research

Regarding results from previous research, the speed and size of the reduction in California's incarceration rate is unprecedented in the United States, and thus it is impossible to find a comparable evaluation conducted within the United States. However, there is a relevant example from another country. 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 before 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 one-time pardon was principally motivated by the need to address prison overcrowding.

The legislation caused an immediate and large reduction in the Italian prison population. Within one month of implementation, that population declined by roughly 22,000 inmates, equivalent to a 36 percent decrease, with a corresponding decrease in the national incarceration rate from 103 to 66 inmates per 100,000. Buonanno and Raphael (2013) evaluated the effects of the massive prisoner release on crime using empirical methods quite similar to those employed here for California. The magnitude of the increase in crime coinciding with the mass prisoner release suggests that, on average, each released inmate generates 14 reported felony crime reports to the police per year. Although most of the increase in Italian crime associated with the collective clemency is attributable to theft, there was also a notable and statistically significant increase in robbery, a crime classified in most nations as a violent felony.

Why was the effect on crime so much larger in Italy than in 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, other key differences between the two case studies are likely key to understanding the difference in outcomes. First, the pre-pardon incarceration rate in Italy stood at roughly 103 per 100,000 residents, quite close to the U.S. incarceration rates that existed before 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 roughly 75,000 jail inmates (a more appropriate comparison to Italy, since Italy has a unified prison and jail system), this rate increases to 625 per 100,000. 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 is perhaps less criminally prone than the average inmate in Italy where prison is used more sparingly. Moreover, the Italian Collective Clemency affected a broader base of prison inmates, whereas California's realignment reforms were much more selectively focused on non-violent offenders and parole violators.

Steven Levitt (1996) provides one of the most widely cited studies in this vein. Levitt analyzes data for U.S. states covering 1971 through 1993, a period over which the average state in his sample had an incarceration rate of 166 per 100,000. The estimates in the study imply that each prison-year served prevented approximately one violent offense and roughly seven property offenses. Raphael and Stoll (2013) provide a

similar analysis yet for separate time periods in the United States across which incarceration rates differ greatly. Specifically, Raphael and Stoll estimate the average number of crimes prevented for each prison-year served for three time periods: 1977 to 1988, 1989 to 1999, and 2000 to 2010. Average state incarceration rates during these three time periods were 171, 349, and 449, respectively. The authors estimate that during the earliest period when incarceration rates were the lowest, each prison-year served prevented roughly 1.2 violent felony offenses and 8.6 property offenses (roughly in line with Levitt's estimates). For the latter two periods with higher incarceration rates, the average effect on violent crime falls to zero. The authors find effects on property crime of roughly 1.3 crimes per prison year served during the 1990s and roughly 2 crimes per prison year served for the period from 2000 to 2010. Liedka, Piehl, and Useem (2006) provide an additional analysis of state-level crime and incarceration data, with an explicit focus on how the effect of incarceration on crime varies with the overall incarceration rate. The authors present strong evidence that the effectiveness of incarceration as a crime-control device declines as the incarceration rate grows.

Our estimates for California line up quite closely with those from Raphael and Stoll (2013) for the United States for more recent years and are certainly in line with the results presented in Liedka, Piehl, and Useem (2006). Moreover, when contrasted with the very large effects on crime of the Italian mass prisoner release, the estimates presented in this report strongly reinforce the finding from prior research that the effectiveness of prison as a crime-control device is subject to diminishing returns to scale.

Appendix Tables

TABLE A1
Regression estimates of the prison-crime effects for overall violent crime and overall property crime

Total Violent Crime					
Year-over-Year Changes	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
ΔPrison	-0.033*** (0.008)	-0.034*** (0.007)	-0.019** (0.009)	-0.040*** (0.013)	0.009 (0.016)
ΔJail	-	-0.011 (0.016)	-0.027 (0.0019)	0.013 (0.022)	-0.029 (0.022)
Difference-in-Difference Changes	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
ΔPrison	-0.006 (0.022)	-0.017 (0.019)	0.005 (0.025)	-0.041*** (0.014)	0.009 (0.017)
ΔJail	-	-0.047* (0.026)	-0.069** (0.027)	0.013 (0.022)	-0.029 (0.022)
Total Property Crime					
Year-over-Year Changes	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
ΔPrison	-0.107* (0.053)	-0.164*** (0.056)	-0.122* (0.070)	-0.162*** (0.053)	-0.089 (0.087)
ΔJail	-	-0.317*** (0.079)	-0.348*** (0.081)	-0.149* (0.086)	-0.162* (0.097)
Difference-in-Difference Changes	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
ΔPrison	-0.117** (0.068)	-0.183** (0.073)	-0.159* (0.086)	-0.165*** (0.054)	-0.091 (0.089)
ΔJail	-	-0.285*** (0.095)	-0.299*** (0.101)	-0.152* (0.086)	-0.163* (0.097)

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

*** Coefficient statistically significant at the 1 percent level of confidence.

** Coefficient statistically significant at the 5 percent level of confidence.

* Coefficient statistically significant at the 10 percent level of confidence.

TABLE A2
Regression estimates of the prison-crime effects for individual 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
Murder					
Year-over-year changes	–0.0004 (0.0004)	–0.0004 (0.0004)	–0.0005 (0.0005)	–0.001 (0.001)	–0.001 (0.001)
Difference-in-difference changes	–0.001 (0.001)	–0.0017 (0.0012)	–0.0016 (0.0014)	–0.001 (0.001)	–0.001 (0.002)
Rape					
Year-over-year changes	–0.001 (0.001)	–0.001 (0.001)	0.000 (0.001)	–0.002 (0.002)	–0.002 (0.004)
Difference-in-difference changes	0.003 (0.002)	0.004* (0.003)	0.006* (0.003)	–0.002 (0.002)	–0.001 (0.003)
Robbery					
Year-over-year changes	–0.009* (0.005)	–0.012** (0.005)	–0.005 (0.005)	–0.015* (0.008)	0.004 (0.010)
Difference-in-difference changes	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 changes	–0.022*** (0.007)	–0.021*** (0.006)	–0.014* (0.008)	–0.022* (0.012)	0.008 (0.014)
Difference-in-difference changes	–0.008 (0.015)	–0.017 (0.012)	–0.004 (0.015)	–0.023* (0.012)	0.007 (0.015)

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

*** Coefficient statistically significant at the 1 percent level of confidence.

** Coefficient statistically significant at the 5 percent level of confidence.

* Coefficient statistically significant at the 10 percent level of confidence.

TABLE A3
Regression estimates of the prison-crime effects for individual property crimes

	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
Burglary					
Year-over-year changes	–0.005 (0.019)	–0.012 (0.022)	–0.013 (0.030)	–0.004 (0.027)	0.019 (0.041)
Difference-in-difference changes	–0.032 (0.031)	–0.079*** (0.024)	–0.076*** (0.028)	–0.003 (0.027)	0.024 (0.042)
Larceny					
Year-over-year changes	–0.045 (0.033)	–0.073* (0.039)	–0.047 (0.040)	–0.078* (0.042)	–0.010 (0.073)
Difference-in-difference changes	–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 changes	–0.057*** (0.018)	–0.080*** (0.017)	–0.062*** (0.020)	–0.080*** (0.023)	–0.099*** (0.036)
Difference-in-difference changes	–0.068*** (0.024)	–0.069** (0.029)	–0.069* (0.037)	–0.081*** (0.023)	–0.103*** (0.037)

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

*** Coefficient statistically significant at the 1 percent level of confidence.

** Coefficient statistically significant at the 5 percent level of confidence.

* Coefficient statistically significant at the 10 percent level of confidence.

TABLE A4
Table Estimated effect of realignment on crime using the synthetic control method

	Violent Crime Rate			Property Crime Rate		
Year	California	Synthetic California	Difference	California	Synthetic California	Difference
2000	621.6	621.7	–0.06	3118.2	3161.0	–42.77
2001	615.2	612.2	3.01	3278.0	3295.7	–17.72
2002	595.4	593.3	2.07	3361.2	3346.1	15.07
2003	579.6	562.8	16.77	3426.4	3405.2	21.23
2004	527.8	537.8	–9.99	3423.9	3408.6	15.31
2005	526.0	537.6	–11.55	3321.0	3320.6	0.40
2006	533.3	536.6	–3.31	3175.2	3190.0	–14.80
2007	522.6	527.7	–5.13	3032.6	3048.4	–15.76
2008	506.2	507.3	–1.08	2954.5	2917.5	37.01
2009	472.0	469.3	2.73	2731.5	2759.5	–27.95
2010	440.6	440.3	0.33	2635.8	2610.2	25.64
2011	411.2	421.5	–10.28	2584.2	2485.5	98.70
2012	423.1	411.4	11.72	2758.7	2505.5	253.15
Pre-period	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Pre-AB 109	–1.29	0.66	0.33	0.83	11.57	25.64
Post-AB 109	11.72			253.15		
Difference-in-difference changes	13.01	11.06	11.39	252.32	241.58	227.51
Placebo test, rank	14	13	13	5	5	5
P-value (one tail)	0.286	0.265	0.265	0.102	0.102	0.102

SOURCE: FBI Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data, 2000–2012.

NOTES: The data show crime rates and refer to the number of crimes per 100,000 residents. The donor pool includes all states except California and the District of Columbia.

TABLE A5
Estimated effect of realignment on violent crimes using the synthetic control method

	Murder			Rape			Robbery			Aggravated Assault		
Year	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference
2000	6.1	6.0	0.06	28.9	28.9	0.03	177.9	179.1	–1.17	408.7	395.3	13.39
2001	6.4	6.5	–0.05	28.8	28.8	0.03	186.7	185.1	1.56	393.3	387.2	6.05
2002	6.8	6.8	0.03	29.1	29.0	0.09	185.6	185.9	–0.32	373.8	377.7	–3.87
2003	6.8	6.8	–0.03	28.2	28.2	0.02	179.8	180.2	–0.43	364.8	338.2	26.62
2004	6.7	6.6	0.14	26.8	26.8	0.05	172.3	172.9	–0.58	322.0	336.7	–14.72
2005	6.9	6.8	0.11	26.0	26.0	–0.03	176.0	176.1	–0.12	317.1	321.0	–3.91
2006	6.8	6.8	0.02	25.3	25.3	0.01	195.0	196.3	–1.26	306.2	312.1	–5.95
2007	6.2	6.4	–0.16	24.7	24.6	0.06	193.0	192.9	0.10	298.8	301.2	–2.41
2008	5.9	5.9	0.00	24.3	24.3	0.00	189.7	189.5	0.24	286.3	291.5	–5.17
2009	5.3	5.4	–0.08	23.6	23.6	0.04	173.4	173.6	–0.25	269.7	278.2	–8.48
2010	4.9	4.9	–0.04	22.4	22.4	0.00	156.0	154.9	1.10	257.4	262.7	–5.32
2011	4.8	4.9	–0.09	20.3	22.3	–2.01	144.1	143.4	0.73	242.0	243.9	–1.93
2012	5.0	4.6	0.42	20.6	21.5	–0.88	148.6	141.6	6.97	248.9	237.8	11.11
Pre-period	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Pre-AB 109	–0.05	–0.04	–0.04	0.02	0.01	0.00	–0.01	0.36	1.10	–5.46	–6.32	–5.32
Post-AB 109	0.42			–0.88			6.97			11.11		
Difference-in-difference changes	0.47	0.46	0.46	–0.89	–0.89	–0.87	6.99	6.61	5.87	16.57	17.43	16.43
Placebo test, rank	11	10	11	30	28	27	4	4	10	9	9	9
P-value (one tail)	0.224	0.204	0.224	0.612	0.571	0.551	0.082	0.082	0.204	0.184	0.184	0.184

SOURCE: FBI Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data, 2000–2012.

NOTES: The data show crime rates and refer to the number of crimes per 100,000 residents. The donor pool includes all states except California and the District of Columbia.

TABLE A6
Estimated effect of realignment on property crimes using the synthetic control method

	Burglary			Motor Vehicle Theft			Larceny Theft		
Year	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference
2000	656.3	655.0	1.26	537.4	552.3	–14.86	1924.5	2001.7	–77.20
2001	671.3	670.0	1.27	590.1	573.8	16.26	2016.6	2010.0	6.59
2002	681.2	680.0	1.21	635.3	630.7	4.62	2044.7	2020.7	24.03
2003	683.2	682.0	1.23	680.5	695.0	–14.54	2062.7	2049.8	12.95
2004	686.1	684.9	1.25	704.8	695.2	9.57	2033.1	1990.2	42.89
2005	692.9	691.6	1.30	712.0	729.4	–17.38	1915.0	1934.6	–19.57
2006	676.9	675.6	1.30	666.8	689.3	–22.55	1831.5	1837.1	–5.62
2007	648.4	647.2	1.23	600.2	596.7	3.51	1784.1	1788.3	–4.22
2008	649.9	648.7	1.23	526.3	480.1	46.17	1778.3	1781.0	–2.67
2009	622.6	621.4	1.25	443.8	381.9	61.95	1665.1	1692.5	–27.38
2010	614.3	613.1	1.19	409.4	336.8	72.56	1612.1	1612.0	0.13
2011	610.5	623.5	–12.97	389.7	312.4	77.35	1584.0	1551.2	32.81
2012	646.1	603.7	42.42	443.2	300.6	142.55	1669.5	1646.4	23.06
Pre-period	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Pre-AB 109	1.24	1.22	1.19	32.33	60.22	72.56	–7.95	–9.97	0.13
Post-AB 109	42.42			142.55			23.06		
Difference-in-difference changes	41.18	41.19	41.23	110.22	82.33	69.99	31.01	33.03	22.92
Placebo test, rank	13	13	12	1	1	1	15	15	20
P-value (one tail)	0.265	0.265	0.245	0.020	0.020	0.020	0.306	0.306	0.408

SOURCE: FBI Uniform Crime Reports, prepared by the National Archive of Criminal Justice Data, 2000–2012.

NOTES: The data show crime rates and refer to the number of crimes per 100,000 residents. The donor pool includes all states except California and the District of Columbia

TABLE A7
Estimated state weights, synthetic control method, matching on 2000–2010 annual UCR data

State	Violent	Murder	Rape	Robbery	Aggravated Assault	Property	Burglary	Motor Vehicle Theft	Larceny Theft
Alabama	0	0	0	0	0	0	0.003	0	0
Alaska	0	0	0	0	0	0	0.002	0	0
Arizona	0	0	0	0.016	0	0	0.002	0.011	0
Arkansas	0	0	0	0	0	0	0.015	0	0
Colorado	0	0.278	0	0	0	0.033	0.213	0	0.12
Connecticut	0	0	0	0	0	0	0.003	0	0
Delaware	0	0	0	0.041	0	0	0.002	0	0
Florida	0.338	0	0.117	0.135	0	0	0.004	0	0
Georgia	0	0	0	0.048	0	0.001	0.032	0.368	0
Hawaii	0	0	0.14	0	0	0	0.002	0.069	0
Idaho	0	0	0	0	0	0	0.024	0	0
Illinois	0	0	0	0.114	0.086	0	0.003	0	0
Indiana	0	0	0	0	0	0	0.102	0	0
Iowa	0	0	0	0	0	0	0.004	0	0
Kansas	0	0.033	0	0	0	0	0.001	0	0
Kentucky	0	0	0	0	0	0.133	0.006	0	0
Louisiana	0	0	0.003	0	0	0	0.001	0	0
Maine	0	0	0	0	0	0	0.003	0	0
Maryland	0.161	0.186	0.043	0.079	0.539	0	0.004	0.248	0
Massachusetts	0	0	0	0	0	0.032	0.012	0	0.217
Michigan	0	0	0	0	0	0	0.006	0	0
Minnesota	0	0	0.03	0	0	0	0.01	0	0
Mississippi	0	0.035	0	0	0.225	0	0.002	0	0
Missouri	0	0	0	0	0	0	0.143	0	0
Montana	0.068	0	0	0	0.046	0	0.004	0	0
Nebraska	0	0	0	0	0	0	0.001	0	0
Nevada	0	0.234	0	0.135	0	0.163	0.003	0.304	0.353
New Hampshire	0	0	0	0	0	0	0.002	0	0
New Jersey	0	0	0.388	0	0	0	0.003	0	0
New Mexico	0	0	0	0	0	0	0.004	0	0
New York	0.214	0	0	0	0.104	0	0.001	0	0
North Carolina	0	0.229	0	0	0	0	0.007	0	0
North Dakota	0	0	0	0	0	0	0.001	0	0.007
Ohio	0	0	0	0.326	0	0	0.008	0	0
Oklahoma	0	0	0	0	0	0	0.024	0	0
Oregon	0	0	0	0	0	0	0.107	0	0

TABLE A7 *(continued)*

State	Violent	Murder	Rape	Robbery	Aggravated Assault	Property	Burglary	Motor Vehicle Theft	Larceny Theft
Pennsylvania	0	0	0	0	0	0	0.183	0	0
Rhode Island	0.191	0.005	0.024	0	0	0	0.012	0	0.072
South Carolina	0.029	0	0.086	0	0	0	0.01	0	0
South Dakota	0	0	0	0	0	0	0.001	0	0.114
Tennessee	0	0	0	0	0	0.075	0.003	0	0
Texas	0	0	0	0.107	0	0	0.005	0	0
Utah	0	0	0.126	0	0	0	0.001	0	0
Vermont	0	0	0	0	0	0	0.002	0	0
Virginia	0	0	0	0	0	0	0.006	0	0
Washington	0	0	0	0	0	0	0.002	0	0
West Virginia	0	0	0	0	0	0.041	0.002	0	0.107
Wisconsin	0	0	0	0	0	0	0.003	0	0
Wyoming	0	0	0.042	0	0	0.522	0.005	0	0.011

TABLE A8
Regression estimates of Pre-realignment prison incarceration rates on crimes per fewer offenders incarcerated in prison

	Violent	Murder	Rape	Robbery	Aggravated Assault	Property Crimes	Burglary	Motor Vehicle Theft	Larceny Theft
Pre-prison incarceration rate	–0.0019**	0.0000	0.0000	–0.0014**	–0.0005	–0.024***	–0.0037***	–0.0074***	–0.0130**
	(0.0009)	(0.0000)	(0.0001)	(0.0007)	(0.0007)	(0.0089)	(0.0014)	(0.0024)	(0.0055)
Add control for changes in jail incarceration rates									
Pre-prison incarceration rate	–0.0014	0.0000	0.0000	–0.0010	–0.0004	–0.0180*	–0.0031**	–0.0057**	–0.0091
	(0.0010)	(0.0000)	(0.0001)	(0.0007)	(0.0007)	(0.0091)	(0.0014)	(0.0025)	(0.0057)
Realignment change In jail incarceration rate	–0.0080	0.0001	–0.0002	–0.0061*	–0.0018	–0.0925**	–0.0088	–0.0246*	–0.0591**
	(0.0049)	(0.0001)	(0.0004)	(0.0035)	(0.0036)	(0.0458)	(0.0072)	(0.0125)	(0.0284)
Add controls for pre-realignment crime rates									
Pre-prison incarceration rate	–0.0025**	0.0000	–0.0000	–0.002***	–0.0003	–0.032***	–0.0051***	–0.0102***	–0.017***
	(0.0010)	(0.0000)	(0.0001)	(0.0006)	(0.0008)	(0.0077)	(0.0014)	(0.0019)	(0.0050)
Realignment change In jail incarceration rate	–0.0101**	0.0001	–0.0004	–0.009***	–0.0010	–0.129***	–0.0130**	–0.0356***	–0.0813***
	(0.0046)	(0.0001)	(0.0004)	(0.0027)	(0.0037)	(0.0364)	(0.0064)	(0.0092)	(0.0236)
Pre-property crime rate	0.0007*	–0.0000	–0.0000	0.0005**	0.0002	0.0032	0.0010*	0.0013	0.0009
	(0.0004)	(0.0000)	(0.0000)	(0.0002)	(0.0003)	(0.0031)	(0.0006)	(0.0008)	(0.0020)
Pre-violent crime rate	0.0011	0.0000	0.0002*	0.0022***	–0.0012	0.0365***	0.0030*	0.0102***	0.0232***
	(0.0012)	(0.0000)	(0.0001)	(0.0007)	(0.0009)	(0.0092)	(0.0016)	(0.0023)	(0.0059)

NOTE: Standard errors are in parentheses

*** Coefficient statistically significant at the 1 percent level of confidence.

** Coefficient statistically significant at the 5 percent level of confidence.

* Coefficient statistically significant at the 10 percent level of confidence.

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