Prison Downsizing and Public Safety

Evidence From California

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S ince the mid-1970s, the United States has experienced explosive growth in the incarceration rate and now incarcerates adults at a higher rate than any other country in the world (Raphael and Stoll, 2013). State and local budgets primarily carry the economic burden as most inmates are held in state prisons and local jails. The social costs of incarceration are largely borne by poor and minority households whose members disproportionately experience incarceration directly or indirectly through the incarceration of a family member. Not surprisingly, many states, as well as the federal government, are actively seeking alternative strategies to manage public safety. Recent reforms have put California at the forefront of broad efforts across the country to address the reliance on costly incarceration.

California's recent history presents unique opportunities to study large, exogenous changes in incarceration rates. The 2011 public safety realignment (or AB 109) examined by Jody Sundt, Emily Salisbury, and Mark Harmon (2016, this issue) caused one of the largest declines in a state's incarceration rate in U.S. history with the absolute magnitude of the decline comparable with those caused by the Italian periodic collective amnesties.

The article by Sundt et al. (2016) addresses one of the key issues and challenges facing efforts to reduce reliance on costly incarceration; can it be done without jeopardizing public safety? The Sundt et al. article complements earlier studies, and the findings are, broadly speaking, consistent with the existing research that has employed California's realignment reform to study the relationship between incarceration and crime (Lofstrom and Raphael, 2013b, 2015, 2016). Several different empirical strategies focusing on realignment now paint a picture of noticeable declines in incarceration rates with no measurable effect on

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violent crime and only a modest effect on property crime, which is driven by an increase in motor vehicle thefts. These findings are also consistent with a growing body of research showing diminishing marginal crime preventive effects of incarceration (Johnson and Raphael, 2012; Liedka, Piehl, and Useem, 2006).

We generally agree with the empirical findings in Sundt et al. (2016) and, in particular, with the magnitudes of the effect estimates. However, we do have a few areas of subtle disagreement with Sundt et al. that warrant additional discussions. In particular, Sundt et al. assert that the reform they study should not be characterized as a decarceration effort but as a decentralization of incarceration responsibilities. This characterization by Sundt et al. conflicts with the sharp overall decrease in incarceration in the state and fails to appreciate the particular population dynamics unleashed by the 2011 reforms. Second, Sundt et al. argue that auto theft rates are highly volatile, that one should not interpret their findings as evidence of an impact of the reform on auto theft rates, and that the effect they do find disappears by 2014. We disagree. The evidence of an impact in the first few post-realignment years is strong, although the effect is arguably small. The increase in auto theft rates relative to a carefully chosen set of comparison states is robust, is evident in cross-county analysis as well as in interstate comparisons, and persists through 2014. That being said, we do observe that the auto theft effect narrows by 2014, which suggests that local law enforcement or local probation departments were able in part to address this impact of realignment 3 years into the reform. In any case, the effects are small. Finally, the findings and conclusions from our earlier work, in particular, how they situate within the larger prison–crime literature, are slightly mischaracterized. When situated within a broad set of research studies, the results presented by Sundt et al., as well as the findings from our research, are strongly suggestive of diminishing marginal effects of incarceration on crime levels. This in turn means that the marginal benefits associated with the relatively high pre-realignment incarceration levels are also small and likely do not justify the high fiscal and social costs of typical U.S. incarceration levels.

California's Decarceration Path

After decades of dramatic growth in its state prison population—primarily driven by "tough-on-crime policies"—California faced severe overcrowding and lawsuits alleging inadequate mental health care (*Coleman v. Brown*, originally filed in 1991) and later inadequate medical care (*Plata v. Brown*, originally filed in 2001). These lawsuits resulted in the appointment by a federal court of a Special Master and a Receiver with budgetary authority to oversee mental health care and medical care, respectively. As a result of a lack of progress under this remedy, a federal three-judge panel was convened in 2007 to oversee prison reforms in the state with the aim of bringing the state system into compliance with the federal court orders. In 2009, the three-judge panel ordered the state to reduce the prison population to a level that it deemed would be consistent with adequate mental health and medical care. The order effectively required a reduction in the population from close to 190.0% of

design capacity to 137.5% (a reduction at that time of almost 40,000 inmates). The state appealed the mandate to the U.S. Supreme Court, but it was rebuffed in May 2011.

During the appeals process, the state introduced several policy changes intended to reduce the prison population. For example, the 2009 Senate Bill 678 provided financial incentives to counties to reduce the number of offenders sent to prison for probation failures. In addition, in 2009, the state created a "nonrevocable parole" designation that removed some lower level offenders from parole supervision while maintaining their active criminal justice status (allowing, for example, warrantless searches by law enforcement). Both reforms modestly reduced the state's prison population prior to the introduction of realignment (from the peak of roughly 172,000 in 2006 to 165,000 in 2010). Nonetheless, on the eve of the implementation of realignment, California's prison population stood at 179.5% of design capacity, requiring a further reduction of roughly 33,000 inmates by June 2013 to comply with the federal court order. Fiscal, political, and logistical constraints made prison expansion an impractical answer to the problem, and releasing tens of thousands of prisoners in a one-time amnesty was out of the question.

The state's solution to this problem was Assembly Bill 109, which is commonly referred to as "public safety realignment." The historic reform went from proposal to implementation quickly: It was proposed by Governor Jerry Brown in January 2011, passed by the legislature in March 2011, and went into effect October 1, 2011. As pointed out by Sundt et al. (2016), the reform was partly motivated by decentralization or the idea that "locals can do a better job" and, hence, shifted the incarceration and supervision responsibilities of many lower level felons from the state's prison system to county sheriff and probation departments.
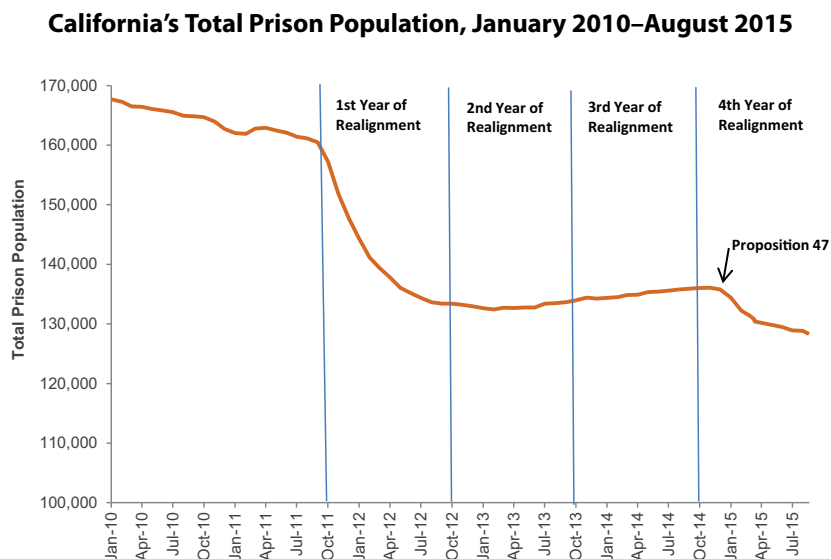
Two features in particular of the reform aimed to reduce the prison population quickly. First, most parole violators are no longer sent to state prison but serve short stays in county jails or face alternative local sanctions. Second, most low-level offenders convicted of nonsexual, nonviolent, and nonserious crimes (so-called "triple nonoffenses") with no such crimes listed on their criminal records now serve their sentences under county supervision rather than in state prison.

The legislation also aimed to reduce California's high rates of recidivism. As suggested by Sundt et al. (2016), this aspiration was based on the notion that counties, by using evidence-based practices, would be able to reduce the reoffending rates of lower level offenders more effectively than the state parole system. Therefore, supervision of lower level offenders released from state prison was shifted from the state to county probation departments (on so-called "post release community supervision" or PRCS for short).

The reform quickly and substantially reduced the prison population with nearly all of the reduction occurring during the first post-realignment year (Figure 1). In addition, much of the decline within the first year was front-loaded within the first 6 months. By the end of September 2012, the total prison population declined by approximately 27,400.

The lion's share of this decline was driven by a sharp reduction in admissions resulting from parole violations, the category of admissions that accounted for the overwhelming

California's Total Prison Population, January 2010–August 2015

Notes. California Department of Corrections and Rehabilitation, Monthly Population Report, January 2010–August 2015. Total prison population as of the last day of the month.
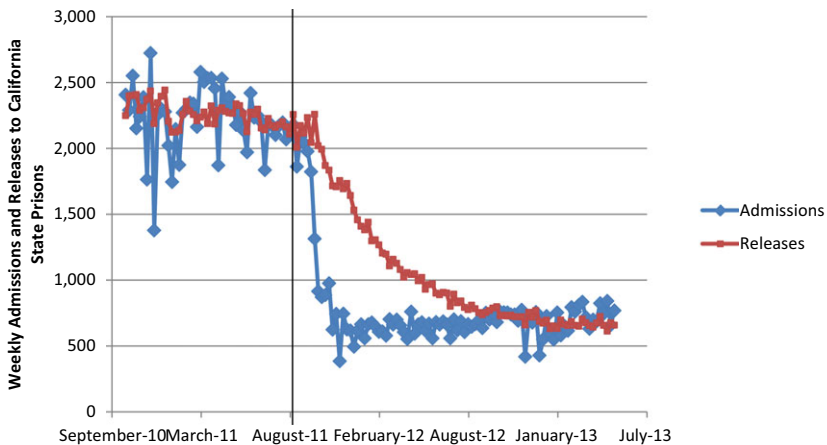
majority of annual prison admission in California prior to the reform. Figure 2 compares weekly prison admissions and release totals for a period surrounding the reform. The figure shows an immediate and sharp reduction in prison admissions with a lagging comparable reduction in releases. Ultimately, these combined changes caused the permanent decline in the prison population evident in Figure 1.[1] This decline in admissions was driven for the most part by a sharp decline in the return to custody rate for parolees (a community corrections population that in California is monitored by Adult Parole Operations, a division of the California Department of Corrections and Rehabilitation [CDCR]). In some of our previous work, we estimated that realignment reduced the 1-year return to custody rate for released inmates by more than 30 percentage points with little impact on rearrest rates and only a slight increase in rates of convictions for new crimes (Lofstrom, Raphael, and Grattet, 2014).[2]

Sundt et al. (2016) question whether realignment should be characterized as a decarceration effort by citing the following language on the CDCR website: "No inmates

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1. For a discussion of the dynamics of prison populations, as well as of the determinants of incarceration levels, see Raphael and Stoll (2013).

2. The increase in conviction rates is explained in its entirety by an increase in the likelihood of being convicted conditional on arrest. This is consistent with local prosecutors pursuing more cases that in the past they would have simply permitted to default to the board of parole hearings for a quick and frequently used parole revocation and subsequent return to state custody.

FIGURE 2

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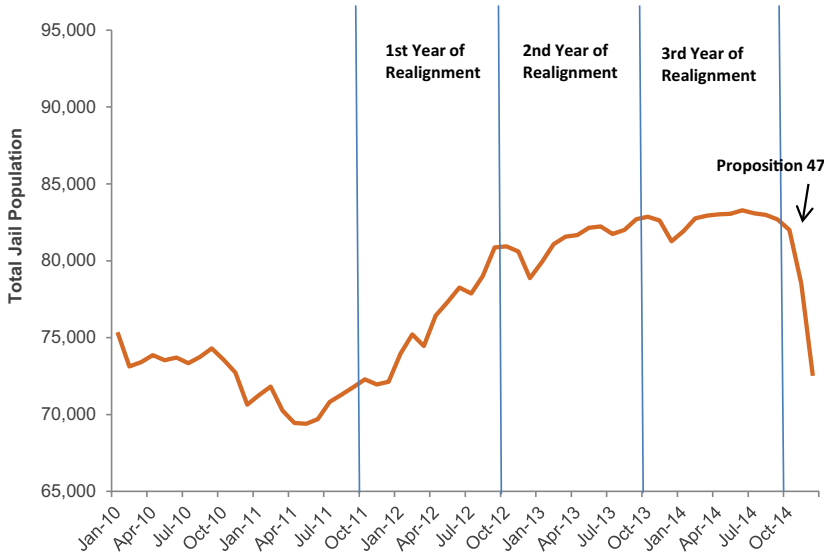

Note. Data from special tabulations provided by the California Department of Corrections and Rehabilitation.

in state prison have been or will be transferred to county jails or released early." Although technically correct, the reform greatly reduced the rate at which former inmates were returned to custody and diverted some of this reduction to local county jails. In other words, individuals who in the past would have been returned to custody were not. Moreover, as we will soon discuss, the observed increase in county jail populations was roughly one third of the decline in the prison population. Hence, on net, the reforms effectively resulted in reduced incarceration and in an increase in noninstitutional time for a fairly large group of individuals (roughly 18,000 former inmates) who absent the reform would have been serving time in a California state prison or county jail.[3]

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3. Figure 1 reveals several important post-realignment changes in the state prison population. The figure shows that after the first postreform year, the population leveled off and even rose slightly, but with increased use of in-state contract beds and the opening of a new health-care facility, the state was able to move closer to the mandated population target. Proposition 36, passed by voters in November 2012, revised California's three-strikes law to impose a life sentence only when a third felony conviction is for a serious or violent offense, which also resulted in fewer inmates in the state's prisons. Nonetheless, by October 2014 (3 years into realignment), the prison population stood at 140.9% of design capacity, still roughly 2,850 inmates above the mandated target. It was not until a referendum passed by California voters in November 2014—Proposition 47—that the prison population fell below the mandated target. Proposition 47 reduced the penalty for several drug and property offenses by reclassifying them from felonies to misdemeanors. Since November 2014, the prison population has declined noticeably (by almost 7,800 as of August 31, 2015) and has stayed below the mandated target since January 2015.

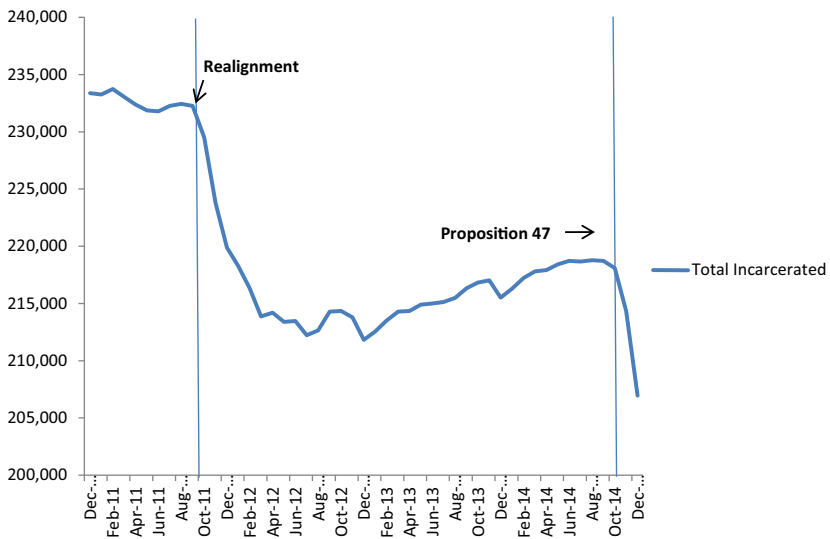**California's Average Daily Jail Population, January 2010–December 2014**

Note. Board of State and Community Corrections, Monthly Jail Profile Survey January 2010—December 2014.

The effects of the reform on California's jail population are evident in Figure 3. With the diversion of both newly sentenced lower level felons and parole violators to county jail, California's average daily jail population (ADP) increased significantly by approximately 9,000 by the end of the first year of realignment (from ~71,800 to 80,900). With several jails at full capacity and operating under court-ordered population caps, the increases in the jail population do not fully reflect realignment's total impact on incarceration. Capacity-constrained (i.e., emergency) releases also increased noticeably in the first year of realignment, from a monthly average of approximately 10,700 the year immediately before realignment to 12,300 during its first year, which is an increase of roughly 15% (Lofstrom and Raphael, 2013a).[4]

When combined, the prison and jail population data clearly show that realignment substantially reduced the state's overall incarceration rate (Figure 4). This decline was relatively quick and concentrated in the first year of the reform. The net decline amounted to a decrease in incarceration of roughly 60 per 100,000 California residents (a decline of

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4. Similar to the state prison population, the jail population dropped dramatically with the passing of Proposition 47. In the first 5 months, the average daily population of California county jails declined by approximately 9,000 inmates.

FIGURE 4

Combined State Prison and County Jail Population

approximately 90 per 100,000 in the state prison incarceration rate offset by an increase of 30 per 100,000 in the jail incarceration rate). To the extent that the overall decline in the incarceration rate was the main determinant behind the first-year increase in property crime (Lofstrom and Raphael, 2013b), this suggests no further impact of realignment on crime in 2013 and 2014 unless the impact occurred with a delay.[5]

Estimating Realignment's Impact on Crime Rates

Realignment creates a unique opportunity to study the consequences of variation in incarceration rates. Arguably, the change was policy driven, and in particular, it was forced on the state by a federal court threatening to emergency-release tens of thousands of prison inmates. The change was big, amounting to a roughly 17% decline in the state's prison population and an approximately 10% decline in the state's overall incarceration rate (prison and jail combined). Third, the change was sudden with nearly all of the decrease occurring within the first year.

Sundt et al. (2016) evaluated the effect of realignment by using a regression point displacement strategy. This estimator basically analyzes the bivariate scatter plot of state-level crime rates in post-realignment years against state-level crime rates in the

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5. Figure 4 reveals the large effect of California's Proposition 47 on both the jail and prison populations. Clearly this is a policy shock that deserves further research and careful analysis.

pre-realignment year of 2010, with the explicit aim of assessing whether California's data point deviates unusually from the estimated regression line. In other words, Sundt et al. use the relationship between pre- and post-realignment crime rates for the 49 states to generate a counterfactual prediction for California and then assess whether the magnitude of the deviation from the prediction is statistically significant.

In our previous research on the crime effects of realignment (Lofstrom and Raphael, 2013b, 2016), we pursued two alternative estimation strategies, one of which is comparable in spirit with that of Sundt et al. (2016). First, we exploited the enormous heterogeneity in the effect of realignment on different California counties. California's counties vary considerably with respect to their use of the state prison system with county-specific incarceration rates varying from below 200 to more than 1,200.[6] Naturally, the incarceration declines caused by realignment were considerably greater in high incarceration counties. Hence, we estimated a series of models that regress county-specific changes in crime rates against county-specific changes in prison incarceration rates exploiting both heterogeneity across counties in the shock, as well as variation within county over time over the first year or so of the reform. These models also explicitly control for the change in county jail populations, which is a factor that may clearly confound change in prison incarceration rates.
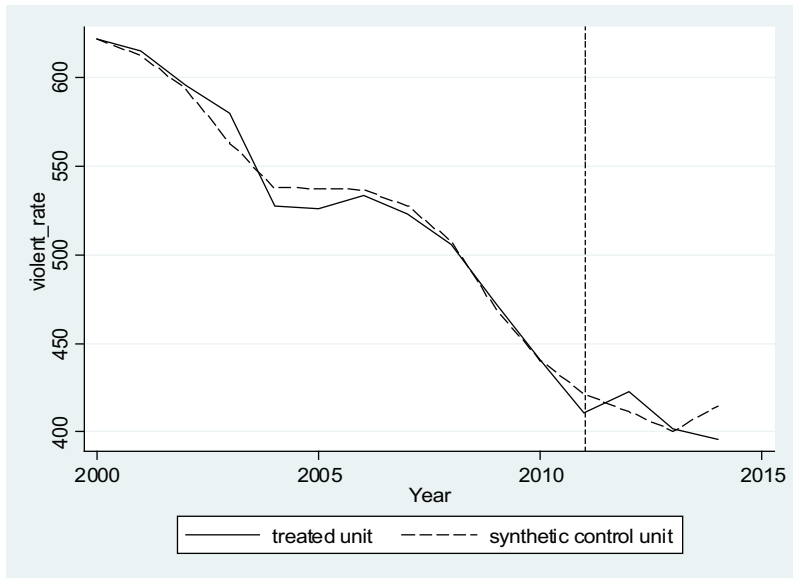
Our second strategy employed the synthetic cohort method of Abadie, Diamond, and Hainmueller (2010). In the present context, the synthetic cohort method basically identifies a convex combination of nontreated states (from among the pool of all possible donor states) that best matches the pre-intervention characteristics of the treatment state. In our application (Lofstrom and Raphael, 2013b, 2016), we used the method to identify those states for whom a weighted average of crime rates minimizes the mean-squared difference between California and "synthetic California" for the pre-intervention period 2000 to 2010. This "synthetic California" is then used to chart the counterfactual path for actual California. We used this counterfactual to calculate difference-in-difference estimates of the pre–post reform change in crime and to draw inferences by generating a sampling distribution from a full set of placebo estimates from the remaining 49 states not experiencing realignment reform.

Are these three strategies in agreement (that of Sundt et al. [2016] and our two strategies [Lofstrom and Raphael, 2013b, 2016])? For the most part, they are. None of these strategies finds evidence of an impact on violent crime. All three find robust evidence of an impact on motor vehicle theft. In addition, the magnitudes are all close to one another. Sundt et al. (2016) estimate an increase in auto thefts per 100,000 in 2012 and 2013 of 74 and 66, respectively. Our synthetic comparison estimates (Lofstrom and Raphael, 2013b, 2016) suggest an average increase in auto theft rates for the period 2012–2013 of 72 per 100,000. With a corresponding overall net decline of 60 per 100,000, our estimate suggests that each prison

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6. As there is no connection between where one is from and where one is incarcerated (i.e., one is highly likely not to be physically incarcerated in one's home county), these figures are referring to the number of each county's residents in the state prison system per 100,000.

FIGURE 5

Violent Crime Rate Trends in California and Synthetic California 2000–2014, with Synthetic Comparison Group and Weighted Identified by Matching on Violent Crime Rates for Each Year Between 2000 and 2010
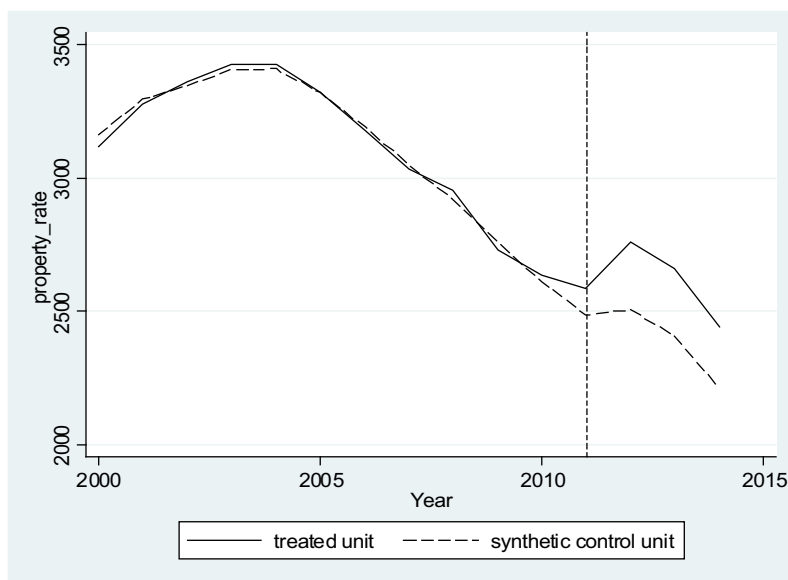

Note. The matched comparison states (with estimated weights in parentheses) are Florida (0.338), Maryland (0.161), Montana (0.068), New York (0.214), Rhode Island (0.191), and South Carolina (0.029).

year not served as a result of realignment resulted in 1.2 additional auto thefts. If we take the average of the two estimates for Sundt et al. (70), their results suggest a comparable increase of 1.06 per prison year not served. Our cross-county results that adjust for county-specific fixed effects and state-level time trends and that exploit variation within county in the realignment dose, so to speak (our preferred specification), similarly yield an estimate of roughly 1.2 additional auto thefts per year not served and no measureable evidence for other offenses.

In our prior work (Lofstrom and Raphael, 2013b, 2016), we did not extend data through 2014. It may certainly be the case that 3 years out, local probation departments gain their sea legs and can better manage their new caseloads, as well as perhaps mitigate the modest effect on auto theft. Alternatively, adverse effects of the incarceration decline may occur with delay. To assess whether our results still accord with Sundt et al. (2016), we extended our synthetic cohort analysis through 2014. Figures 5, 6, and 7 present these results graphically, whereas Table 1 presents the actual values for California and synthetic California, the difference for each year, and the difference-in-difference calculations for the

Property Crime Rate Trends in California and Synthetic California 2000–2014, with Synthetic Comparison Group and Weighted Identified by Matching on Property Crime Rates for Each Year Between 2000 and 2010

Note. The matched comparison states (with estimated weights in parentheses) are Colorado (0.033), Georgia (0.001), Kentucky (0.133), Massachusetts (0.032), Nevada (0.163), Tennessee (0.075), West Virginia (0.041), and Wyoming (0.522).
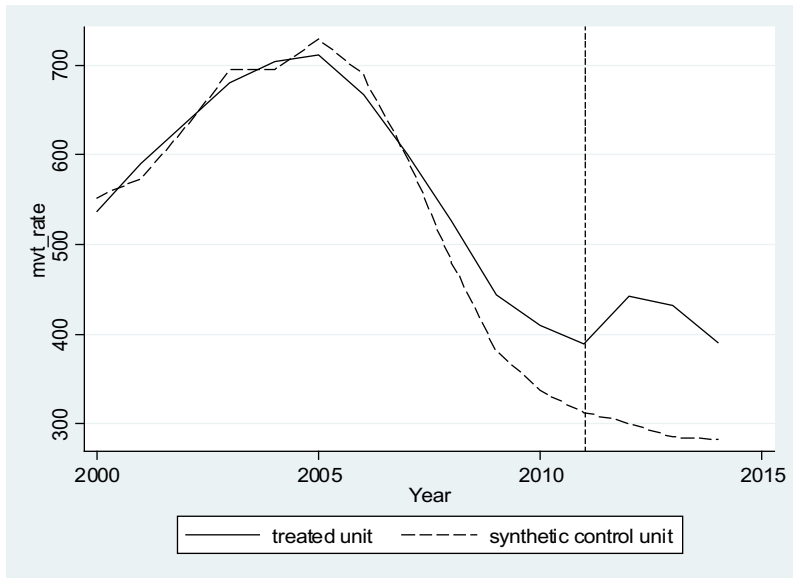
post-period 2012–2014 relative to three alternative pre-period benchmarks (2006–2010, 2008–2010, and 2010 only). The table also presents the results from a test of the significance of the difference-in-difference for California basically locating California's estimate in the ranking of the 49 placebo estimates for the remaining states.[7] In each figure, the solid line shows California crime rates for 2000 through 2014, whereas the dashed figure shows the crime rates in our synthetic comparison group. Note that we match on 2000 through 2010 only as 2011 is a partial treatment year.

Figure 5 confirms our conclusion that there is no measurable effect on violent crime, immediate or delayed, as of 2014. Figure 6 shows that California's property crime rate trend started to diverge from the comparison states in 2011 with a difference between California and synthetic California in property crime rates of 99. This gap grows in 2012

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7.   See Lofstrom and Raphael (2016) for the details behind these tabulations.

FIGURE 7

Motor Vehicle Theft Rate Trends in California and Synthetic California 2000–2014, with Synthetic Comparison Group and Weighted Identified by Matching on Motor Vehicle Theft Rates for Each Year Between 2000 and 2010

Note. The matched comparison states (with estimated weights in parentheses) are Arizona (0.011), Georgia (0.368), Hawaii (0.069), Maryland (0.248), and Nevada (0.304).

and 2013 to approximately 250 and narrows slightly in 2014 to 229. The comparisons of these differentials relative to pre-realignment benchmarks yield estimates that are not statistically significant for the 2006–2010 and 2008–2010 benchmarks, but it is marginally significant when compared with the difference in 2010 (with a *p* value of .061). Finally, for auto theft, the difference in crime rates between California and synthetic California widens from 72 per 100,000 in 2010, to 77 in 2011, to 142 in 2012, to 146 in 2013, and then it narrows to 108 in 2014. Averaged across the three post-treatment years,[8] the relative increase in auto theft rates in California is statistically significant.

Sundt et al. (2016) find evidence that motor vehicle thefts increased in 2012 as a result of realignment. They also find evidence that the effect dissipated in 2013 and that by 2014 the estimated effect is no longer statistically significant. Figure 7 shows that when the comparison is refined to those states that had the most similar motor vehicle theft rate

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8. Note that 2011 is a partial treatment year as realignment is implemented in October 2011. We do not include 2011 as either a pre- or a post-treatment year in the difference-in-difference calculations.

TABLE 1

Estimated Impact of Realignment on Crime Using the Synthetic Control Method

Year	Violent Crime Rate			Property Crime Rate			Motor Vehicle Theft		
	California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	Difference
2000	621.6	621.7	−0.06	3118.2	3161.0	−42.77	537.4	552.3	−14.86
2001	615.2	612.2	3.01	3278.0	3295.7	−17.72	590.1	573.8	16.26
2002	595.4	593.3	2.07	3361.2	3346.1	15.07	635.3	630.7	4.62
2003	579.6	562.8	16.77	3426.4	3405.2	21.23	680.5	695.0	−14.54
2004	527.8	537.8	−9.99	3423.9	3408.6	15.31	704.8	695.2	9.57
2005	526.0	537.6	−11.55	3321.0	3320.6	0.40	712.0	729.4	−17.38
2006	533.3	536.6	−3.31	3175.2	3190.0	−14.80	666.8	689.3	−22.55
2007	522.6	527.7	−5.13	3032.6	3048.4	−15.76	600.2	596.7	3.51
2008	506.2	507.3	−1.08	2954.5	2917.5	37.01	526.3	480.1	46.17
2009	472.0	469.3	2.73	2731.5	2759.5	−27.95	443.8	381.9	61.95
2010	440.6	440.3	0.33	2635.8	2610.2	25.64	409.4	336.8	72.56
2011	411.2	421.5	−10.28	2584.2	2485.5	98.70	389.7	312.4	77.35
2012	423.1	411.4	11.72	2758.7	2505.5	253.15	443.2	300.6	142.55
2013	402.1	400.6	1.50	2658.1	2405.9	252.18	431.2	284.9	146.35
2014	396.1	414.5	−18.43	2441.1	2211.5	229.58	391.3	283.0	108.27
Pre-period benchmark	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Pre-AB109 difference	−1.29	0.66	0.33	0.83	11.57	25.64	32.33	60.22	72.56
Post-AB109 difference		−1.74			244.97			132.39	
Difference-in-difference	−0.45	−2.40	−2.06	244.14	233.40	219.33	100.06	72.17	59.83
Placebo test, rank	18	21	24	7	7	3	1	2	1
p value (one tailed)	0.367	0.429	0.490	0.143	0.143	0.061	0.020	0.041	0.020

trends to California 2000–2010, the comparison states experienced noticeably lower auto theft rates 2012–2014. The results do not show any evidence that the gap decreased in 2013, but it seems to have done so to some extent in 2014. By using the average for the 2012–2014 post-period, we find that the auto theft impact stays statistically at the 5% significance level, independent of the three alternative pre-periods used (Table 1).

Table 2 presents tabulations of the difference-in-difference estimates and tests of their significance that individually use the single years 2012, 2013, and 2014 as the post-period. Across all three comparisons, the effect sizes are largest relative to the 2006–2010 base period and smallest when 2010 is used as the pre-realignment benchmark. All tabulations for 2012 and 2013 are statistically significant and are comparable in magnitude with our original estimates (Lofstrom and Raphael, 2013b, 2016) and the estimates in Sundt et al.

T A B L E   2

Difference-in-Difference Estimates of the Effect of Realignment on Auto Theft Rates for the Individual Post-Treatment Years 2012, 2013, and 2014

	Pre-Realignment Benchmark Period		
	2006–2010	**2008–2010**	**2010**
Pre-period difference	32.33	60.22	72.56
Difference, 2012	142.55	142.55	142.55
Difference-in-difference	110.22	82.33	69.99
Placebo test rank	1	1	1
p value (one-tailed)	0.020	0.020	0.020
Pre-period difference	32.33	60.22	72.56
Difference, 2013	146.35	146.35	146.35
Difference-in-difference	114.02	86.12	73.79
Placebo test rank	1	2	1
p value (one-tailed)	0.020	0.040	0.020
Pre-period difference	32.33	60.22	72.56
Difference, 2014	108.27	108.27	108.27
Difference-in-difference	75.95	48.05	35.72
Placebo test rank	2	4	9
p value (one-tailed)	0.041	0.082	0.182

(2016). By confirming the pattern documented by Sundt et al. (2016), we also observe a narrowing in 2014. For 2014, the effect estimate is still significant relative to the 2006 to 2010 benchmark, is marginally significant relative to the 2008 to 2010 benchmark, and becomes insignificant when we compare it with the 2010 benchmark.

Are These Effects Large?

With these three sets of findings largely in agreement with one another, this raises the obvious questions regarding whether these effects are large, how the results of this natural experiment fit within the larger literature regarding the prison–crime relationship, and whether we can infer something about the cost-effectiveness of prison as a crime-control tool.

Before proceeding, we do feel the need to correct the characterization of the conclusions that we drew from our other work. In reviewing our Lofstrom and Raphael (2016) article, Sundt et al. (2016) indicate that we suggested that "2.2 property crimes and 0.5 violent crimes were avoided as a result of incarcerating realigned offenders." These figures were pulled from a discussion of the range of estimates from our cross-county analysis. To quote our summary of these results directly:

> The largest point estimate in Panel A for violent crime suggests that each
> prison month served prevents 0.041 violent incidents, implying that each

prison year served prevents 0.5 violent incidents. For property crime, the largest point estimate suggests that each prison month served prevents 0.183 property crimes, implying that 2.2 reported property crimes per year are prevented per prison year served. Note, both estimates are likely too high as we have selectively chosen the largest coefficients from the table, neither of which are adjusted for state level crime trends. (Lofstrom and Raphael, 2016)

The quote goes on to note that these estimates are at the low end of prison–crime empirical research and are suggestive of relatively small crime-mitigating benefits at high incarceration levels. In fact, the conclusion to that particular section reads as follows:

To summarize, the cross county results suggest that at most each prison year served among those not incarcerated as a result of realignment prevents on average half of a violent felony offense and roughly 2 property offenses. Our complete model specifications that adjust for time trends and county specific factors suggest even smaller effects, with no impact on violent crime and an effect on property crime limited to auto theft of 1.2 incidents per year. (Lofstrom and Raphael, 2016)

Hence, we are basically in agreement when it comes to effect size.

Regarding whether the effects estimated here are large, there are several ways to answer this question.[9] First, we can compare our results with those from previous research. Not surprisingly given the magnitude of the quick and substantial drawdown in California's prison population (of approximately 17% during the first year of realignment), there are no comparable single-state studies for the United States. In Lofstrom and Raphael (2013b, 2016), we reviewed panel data research for the United States by using different methods and different time periods of analysis, as well as the research pertaining to large exogenous shocks to incarceration in several European countries. This body of research suggests that the amount of crime prevented per prison year served during the 1970s and 1980s is many multiples the effect sizes that we document here, and that more generally, effect sizes are larger in low-incarceration-rate settings. By contrast, more recent panel data research using post-1990 data found effect sizes in line with our findings and the findings in Sundt et al. (2016) for California. Hence, relative to the effect sizes from times past, the estimated prison–crime effects here are small, in fact, very small.

An alternative manner of characterizing these results would be to ask whether the returns in terms of crimes prevented outweigh the budgetary or, better yet, the complete social costs of incarcerating these marginal offenders. Heaton (2010) provided a summary of the findings from research on the costs of crime. With our estimates of the effect of realignment on crime, estimates of the costs of crime summarized in Heaton (2010), and

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9.   The following draws heavily from the conclusion in Lofstrom and Raphael (2016).

estimates of the costs of incarceration in California, we can perform an analysis of the returns on the state's incarceration investment. Heaton's summary implies that each auto theft costs on average $9,533 (in 2013 dollars). By assuming an effect size for auto thefts of 1.2, this suggests that each prison year served for those who as a result of realignment are no longer incarcerated prevents $11,783 in crime-related costs. The California Legislative Analyst's Office estimates that the annual cost of incarcerating a prison inmate in California is $51,889,[10] which suggests a return of 23 cents on the dollar. Incorporating some of the more difficult-to-price social costs in the calculation would certainly lower the return even further.

The simple cost–benefit analysis discussed earlier is useful for thinking about whether on the margin the social expenditures we are making are justified. However, such analysis considers the effectiveness of a particular policy intervention in isolation without considering what could be achieved by reallocating the saved resources toward other uses. For example, it may be the case that a reduction in incarceration absent some other policy intervention may generate small increases in property crime. However, if the money saved from reduced prison expenditures were channeled into alternative and perhaps more cost-effective, crime-control strategies, then increases in crime need not be the end result. For example, Durlauf and Nagin (2011) showed that policy levers that increase the likelihood of being caught for an offense, if exercised, can lead to both reductions in crime and incarceration. This requires of course that the behavior of potential offenders be sufficiently responsive to this shift, or that the elasticity of crime with respect to the apprehension is sufficiently large (greater than one in Durlauf and Nagin's model). Moreover, to the extent that alternative crime-control tools are at least as effective as incarceration, maintaining low crime rates would not require additional public expenditures.

In characterizing the magnitude of these results, we could ask whether other interventions generate a higher return per dollar spent. Perhaps the most obvious policy tool (and that discussed at length in Durlauf and Nagin [2011]) with the strongest research base regarding impacts on crime concerns the expansion of local police forces. There is considerable empirical evidence of the general effectiveness of higher police staffing levels on crime (Chalfin and McCrary, 2012; Corman and NaciMocan, 2000; Di Tella and Schargrodsky, 2004; Evans and Owens, 2007). These studies consistently have found relatively large effects of expanding city police forces on local crime rates. Perhaps the most rigorous analysis of the effects of additional police on crime was provided in a recent study by Chalfin and McCrary (2013). In an analysis of the period 1960 through 2010 of medium-to-large U.S. cities, the authors found 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

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

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, Chalfin and McCrary conclude that each dollar invested in additional policing generates $1.6 in crime savings.

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

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