

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

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

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

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

In every year since 2007, the U.S. incarceration rate has declined. As of December 31, 2013, the overall imprisonment rate stood at 700 per 100,000, the same rate as in 1999.¹ These declines are driven by sentencing reforms at the state level explicitly designed to reduce incarceration rates. Various factors drive these reforms. First, the fiscal impacts of the recent deep economic recession have induced state leaders to scour their budgets for potential savings. While corrections expenditures are often third or fourth on the list of budget categories commanding general fund dollars, the explosive growth in these expenditures in recent decades has come under increasing scrutiny.

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

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

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

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

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

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

Prior Research on the Crime-Prison Relationship

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

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

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

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

Buonanno and Raphael (2013) use relatively high-frequency crime and incarceration data at the national level as well as province-level variation to estimate the reverse-incapacitation effects caused by the August 2006 Italian mass prisoner release. The authors find felony incapacitation effects on the order of

thirteen to seventeen serious offenses per year served. However, the authors present several sets of results indicative of diminishing marginal incapacitation effects. First, they show that incapacitation effects are the largest for those inmates who replace the pardoned and/or those who are returned to prison the soonest after the mass release. In addition, the authors find much larger incapacitation effects in provinces with lower pre-pardon incarceration rates relative to provinces with higher pre-pardon incarceration rates.

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

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

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

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

Johnson and Raphael (2012) use an instrument for incarceration based on the difference between a state's current incarceration rate and the state's steady-state

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

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

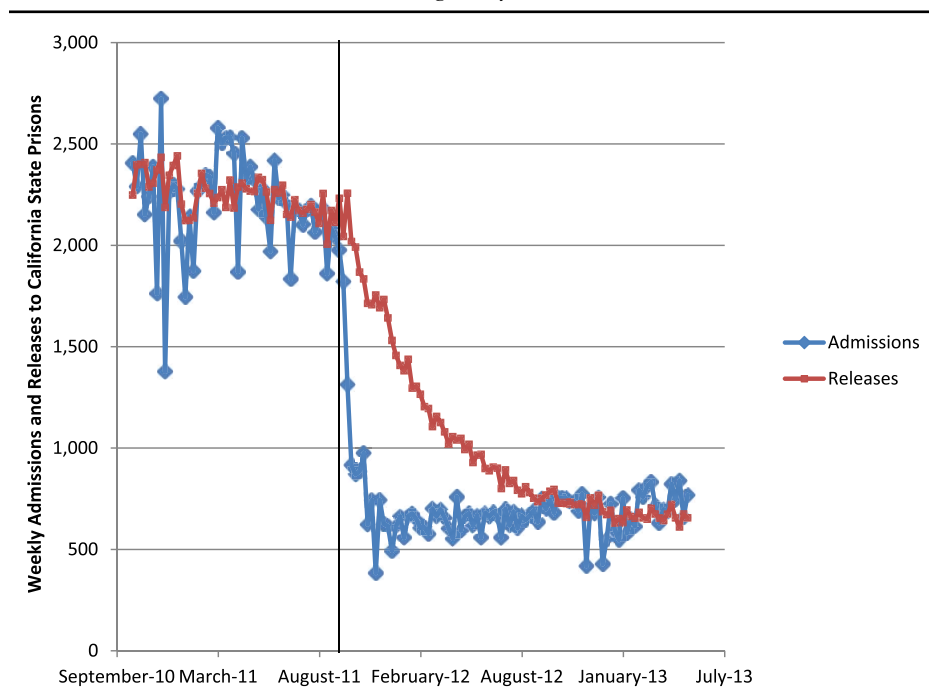
Unlike the Dutch and Italian studies, the U.S. panel data estimates represent joint incapacitation/deterrence effects associated with increases in incarceration, estimates that in theory should be larger than the estimates of pure incapacitation effects. Nonetheless, for recent years, empirical estimates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

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

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

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

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



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

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

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

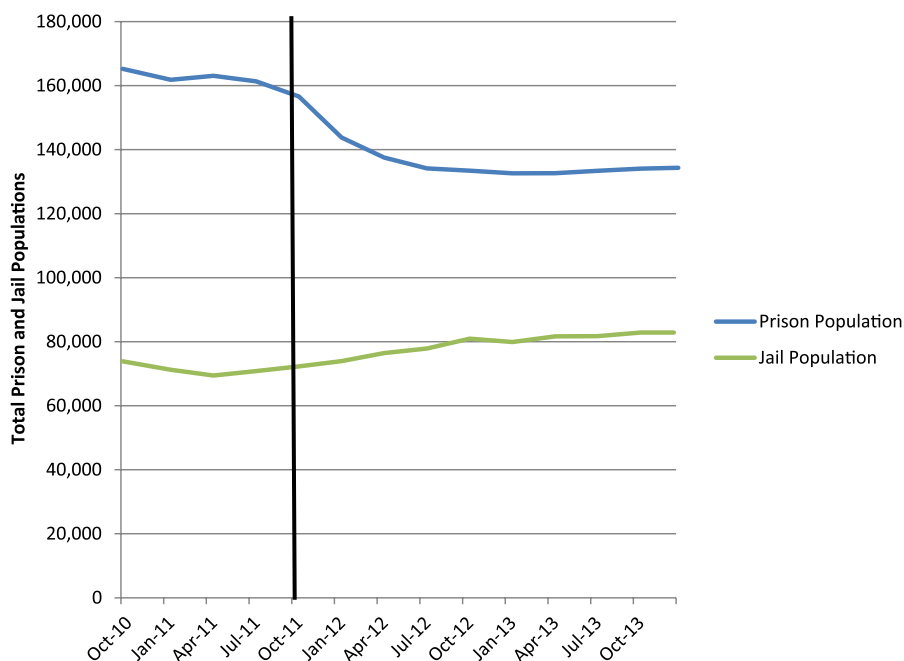
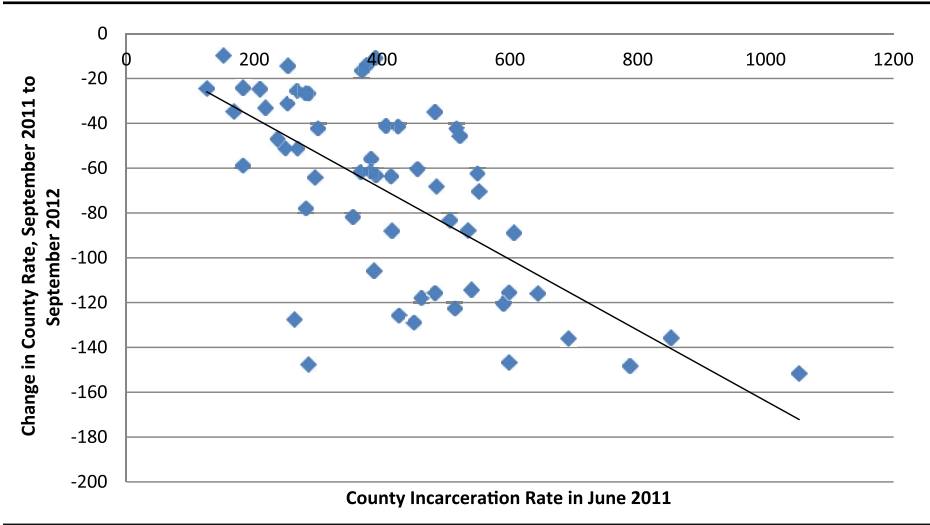


Figure 2 shows the impact of these changes in flows on the total prison population (measured at the end of each month). The figure reveals a clear drop in the state's prison population, with a total decline between September 2011 and May 2013 of 27,846. The effect of Realignment was felt immediately,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

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

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



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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

NOTE: Standard errors are in parentheses.

*Statistically significant at the 1 percent level of confidence.

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

Second, equation (1) includes a complete set of month fixed effects corresponding to the end month of the change defining each observation. Including

time fixed effects effectively nets out the overall state time trends for crime changes and identifies the prison-crime effects based on variation above and beyond what occurs for the state overall. Such an adjustment may net out any state-level change in general deterrence associated with the policy reform. Hence, we will ultimately corroborate our county-level analysis with a series of synthetic cohort estimates using variation at the state level.

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

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

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

A potential weakness of this strategy is that the change over the course of a full year may reflect underlying trends in crime and corrections that predate the implementation of Realignment. This is particularly problematic for the earlier months in our analysis, such as October through December 2011, when the majority of the period over which changes are measured lies within the pre-Realignment period. Variation in crime and incarceration over this earlier time period may be driven by exogenous shocks to criminal offending that create the

simultaneity bias to which much research on the prison-crime effect has been devoted to correcting. In addition, several policy reforms in California predate AB 109, most of which were geared toward reducing the prison population to comply with the federal court order.⁴ Given the policy activity prior to Realignment's implementation and other potential sources of variation in crime and incarceration rates, one might want to focus more tightly on time periods that isolate variation in incarceration rates, and consequent impacts on crime, more clearly driven by the 2011 reforms.

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

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

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

Data Description and Basic Crime Trends

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

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

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

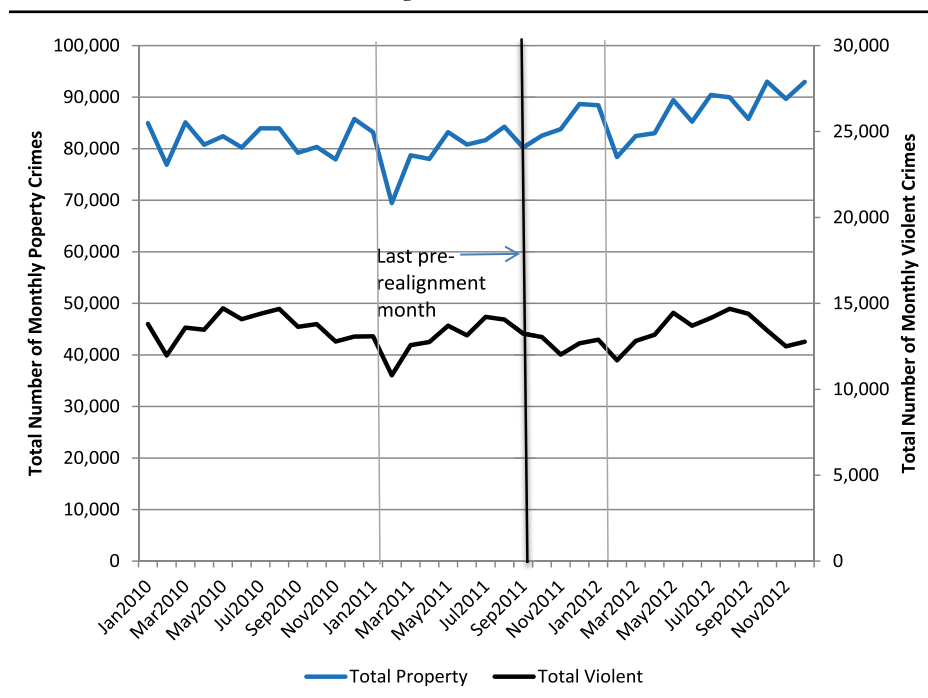
SOURCE: Crime data come from the California Criminal Justice Statistics Center. Population data are from the U.S. Census Bureau and measure the state population as of July in each calendar year.

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

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

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

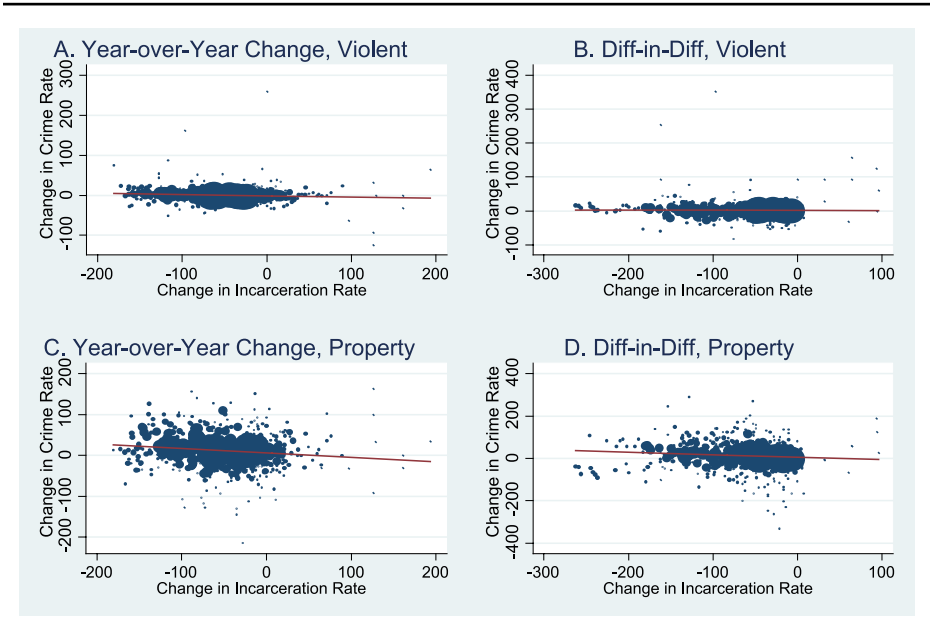
FIGURE 4
Total Monthly Violent and Property Crimes in California, January 2010
through December 2012



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

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

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



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

Cross-County Empirical Results

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

TABLE 4
Regression Estimates of the Prison-Crime Effects for Overall and Individual Property and Violent Crimes

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

NOTE: Standard errors are in parentheses. Standard errors are calculated assuming clustering by county. Each regression contains 684 county-month observations. See main text for description of the alternative characterizations of the dependent and explanatory variables.
*Statistically significant at the 1 percent level of confidence. **Statistically significant at the 5 percent level of confidence. ***Statistically significant at the 10 percent level of confidence.

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

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

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

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

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

To render the results in Table 4 comparable, we must multiply each coefficient by 12 since the estimation results here pertain to monthly crime. The largest point estimate for violent crime suggests that each prison month served prevents 0.041 violent incidents, implying that each prison year served prevents 0.5 violent incidents. For property crime, the largest point estimate suggests that each prison

month served prevents 0.183 property crimes, implying that 2.2 reported property crimes per year are prevented per prison year served. Note, both estimates are likely too high as we have selectively chosen the largest coefficients from the table, neither of which is adjusted for state-level crime trends. Nonetheless, the implied effect sizes are consistent with those from previous research and suggest impacts per prison year served that are considerably lower than estimates from time periods in the United States when the incarceration rate was much lower.

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

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

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

Cross-State Comparison Using Synthetic Cohort Analysis

Thus far, we have relied on cross-county differences in the impact of Realignment on county-specific prison incarceration rates to study the effects on crime. One might contend that focusing on the effects at the county level may be controlling away any change in general deterrence statewide associated with the change in the penal code. While the great county-level heterogeneity in the application of the common penal code prior to Realignment might call such concerns into question, it certainly is possible that the very public and high-publicity proceedings

surrounding the reform may have altered expectations regarding punishment severity and altered criminal behavior as a result. An additional concern involves the relatively small geographic units of analysis (counties) and the fact that in many urban areas, county borders are relatively arbitrary boundaries that do not demarcate meaningful social ecological divisions. To the extent that one county's Realignment caseload spills over into another county's crime rate, our county-level regression analysis may underestimate the effects of the reform on crime rates.

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

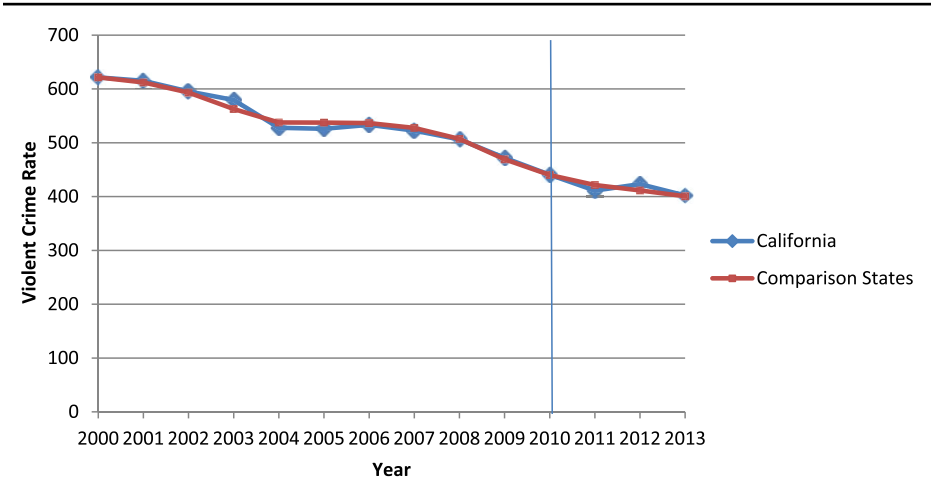
Using data from the Federal Bureau of Investigation's Uniform Crime Report for the period 2000 to 2013, we employ the synthetic control approach of Abadie, Diamond, and Hainmueller (2010). The synthetic control approach identifies a group of states that when averaged have crime trends that are as close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

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

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

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

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

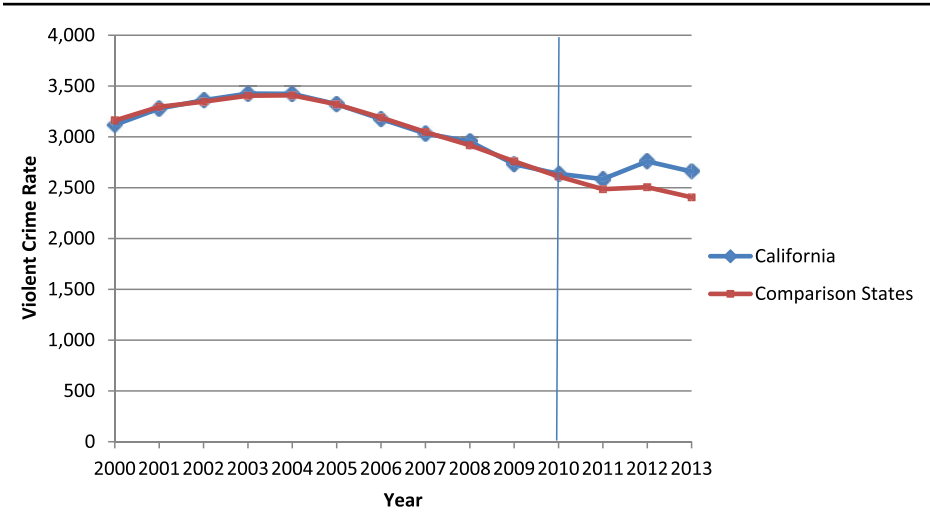


NOTE: The matched comparison states (with estimated weights in parentheses) are Florida (0.338), Maryland (0.161), Montana (0.068), New York (0.214), Rhode Island (0.191), and South Carolina (0.029).

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

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

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



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

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

Conclusion

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

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

TABLE 5
Estimated Impact of Realignment on Violent and Property Crimes Using the Synthetic Control Method

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

Property Crimes											
Burglary				Motor Vehicle Theft				Larceny Theft			
California	Synthetic California	Difference	California	Synthetic California	Difference	California	Synthetic California	California	Synthetic California	Difference	
2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010	2006–2010	2008–2010	2010
Pre-period											
Prereform diff	1.24	1.22	1.19	32.33	60.22	72.56	–7.95	–9.97	0.13		
Postreform diff, 2012–2013	45.83		144.45			20.66					
Diff-in-diff	44.59	44.61	44.64	112.12	84.22	71.89	28.61	30.63	20.53		
Placebo test, rank	11	11	10	1	1	1	15	15	18		
p-value (one tail)	.224	.224	.204	.020	.020	.020	.306	.306	.367		

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

An alternative manner of characterizing these results would be to ask whether the returns in terms of crimes prevented outweigh the budgetary or, better yet, the complete social costs of incarcerating these marginal offenders. Heaton (2010) provides a summary of the findings from research on the costs of crime. With our estimates of the effect of Realignment on crime, estimates of the costs of crime summarized in Heaton, and estimates of the costs of incarceration in California, we can perform an analysis of the returns on the state's incarceration investment. Our preferred empirical results suggest that each prison year served prevents 1.2 auto thefts. Heaton's summary implies that each auto theft costs on average \$9,533 (in 2013 dollars). This suggests that each prison year served for those who as a result of Realignment are no longer incarcerated prevents \$11,783 in crime related costs. The California Legislative Analyst's Office estimates that the annual cost of incarcerating a prison inmate in California is \$51,889.⁶ This suggests a return of 23 cents on the dollar. Incorporating some of the more difficult to price social costs in the calculation would certainly lower the return even further.

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

In characterizing the magnitude of our results, we could ask whether there are other interventions that generate a higher return per dollar spent. Perhaps the most obvious policy tool with the strongest research base regarding impacts on crime concerns the expansion of local police forces. There is considerable empirical evidence of the general effectiveness of higher police staffing levels on crime (Chalfin and McCrary 2012; Di Tella and Schargrodsky 2004; Evans and Owens 2007; Corman and Mocan 2000). These studies consistently find relatively large effects of expanding city police forces on local crime rates. Perhaps the most

rigorous analysis of the effects of additional police on crime is provided in a recent study by Chalfin and McCrary (2013). In an analysis of the period 1960 through 2010 of medium to large U.S. cities, the authors find substantial and sizable effects of hiring additional police officers on crime rates, with notably statistically significant effects on very serious violent crimes. The empirical results in their analysis imply that each additional police officer reduces annual crime by 1.3 violent crimes and 4.2 property crimes. In an analysis of the costs and benefits of police expansion, the authors conclude that each dollar invested in additional policing generates \$1.60 in crime savings.

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

Notes

1. Bureau of Justice Statistics, Key Statistics, "Estimated supervision rate for persons under state, federal, and local correctional supervision, 1980–2013." Available from www.bjs.gov.
2. For example, see Gingrich and Nolan (2013).
3. The pre-2006 pardon Italian incarceration rate stood at roughly 100 per 100,000 (Buonanno and Raphael 2013). The Dutch incarceration rate in 2004 stood at roughly 124 per 100,000 (International Centre for Prison Studies, <http://www.prisonstudies.org/info/worldbrief/>, accessed June 15, 2012). Both are inclusive of pretrial populations and inmates serving short sentences.
4. For example, in January 2010, California instituted financial incentives for localities to reduce probation revocation rates. In late 2009 the state made it more difficult to revoke the parole of low-risk parolees. Combined, these two reforms reduced the prison population between January 2010 and September 2011 from 167,694 to 160,482.
5. See Bohn, Lofstrom, and Raphael (2013) for an application of this method to an immigration policy intervention.
6. Given the extreme capacity constraints faced by the state and the standing court order, this average cost is likely below marginal cost, given that increasing the population clearly requires new facilities at this point.

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