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Technical Appendix

Public Safety Realignment and Crime Rates in California

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Appendix

Data

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

Monthly data on county jail populations come from the California Jail Profile Survey, administered and maintained by the California Board of State and Community Corrections. To calculate jail incarceration rates, we use average daily population figures for each county and each month. There are 58 counties, but one small rural county, Alpine, does not maintain its own jail system. Hence, nearly all of our analyses (with the exception of the descriptive crime trends presented in the main report) focus on the 57 counties with independent jail systems.

The California Department of Corrections and Rehabilitation (CDCR) calculates prison totals by county of commitment only intermittently and hence does not publish the monthly totals we need to implement our estimation strategy. However, CDCR has provided us with weekly admissions and releases to the system by county and by controlling offense for the period from October 2010 through December of 2012. The difference between cumulative admissions and releases between any two dates for a given county provides the change in the incarceration total. We use this strategy to tabulate the change in incarceration by county between any two months, using the latest date within each month as the starting and end points. To convert to rates, we normalize by the average of the county population estimates for the two calendar years straddled by the change.

Prior Research on the Crime-Prison Relationship

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

A large body of research by criminologists using inmate interviews estimates incapacitation effects through retrospective surveys. Careful reviews of this research summarize the findings in terms of the average number of serious felonies prevented per prison year served. The corpus of this body of research finds annual incapacitation effects of 10 to 20 fewer serious felony offenses (Marvell and Moody 1994; Spelman 1994, 2000). However, most of this research employs prisoner surveys fielded during the 1970s and 1980s, years when the U.S. incarceration rate was much lower than contemporary levels. With the large increase in U.S. incarceration rates, the average age of prison inmates has increased as has the proportion of inmates convicted of less serious offenses. Given the tendency of offending to decline with age, and heterogeneity in the criminal propensities of prison inmates (Raphael and Stoll 2013, Chapter 7), one might expect lower incapacitation effects from studies employing more recent data.

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

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

Buonanno and Raphael (2013) use relatively high-frequency crime and incarceration data at the national level as well as province-level variation to estimate the reverse-incapacitation effects caused by the August 2006 Italian mass prisoner release. The authors find felony incapacitation effects on the order of 13 to 17 serious offenses per year served. However, the authors present several sets of results indicative of diminishing marginal incapacitation effects. First, they show that incapacitation effects are the largest for those inmates who replace the pardoned and/or those who are returned to prison the soonest after the mass release. In addition, the authors find much larger incapacitation effects in provinces with lower pre-pardon incarceration rates relative to provinces with higher pre-pardon incarceration rates. Both findings are consistent with great heterogeneity in offending behavior among those convicted and sent to prison and a decline in this propensity to offend among the incarcerated as the scale of incarceration increases.

Vollaard (2012) estimates incapacitation effects for repeat offenders exploiting a change in Dutch sentencing policy. The author analyzes the effect of a sentence enhancement in the Netherlands targeted at repeat offenders defined as those with over ten prior felony convictions. In 2001, the Netherlands enacted an enhanced sentence of two years for such offenders, first allowing a small number of municipalities to experiment with the enhancement before nationwide application in 2004. The author finds very large incapacitation effects, on the order of 50 to 60 reported thefts prevented per year of incarceration. However, the author also finds that those municipalities that dipped further into the repeat-offender pool in the application of the sentencing enhancement experienced significantly smaller crime reductions per additional prison-year served.

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

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

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

Johnson and Raphael (2012) use an instrument for incarceration based on the difference between a state's current incarceration rate and the state's steady-state incarceration rate implied by observable admissions and release rates. The authors derive an empirical prediction regarding the effect of this difference on next-year's change in incarceration, based on a theoretical model of the relationship between crime and incarceration, and derive the conditions under which the transitory disparity between the actual and steady state incarceration rate provides a valid instrument for one-year-lead changes in the actual incarceration rate. The authors analyze state-level panel data for two time periods: 1978 to 1990 and 1991 to 2004. The former period is characterized by a relatively low incarceration rate (186 per 100,000), and the latter period is characterized by a much a higher incarceration rate (396 per 100,000). For the early period, an additional prison year served is estimated to prevent roughly 2.5 felony violent offenses and 11.4 felony property offenses. Note that the total crimes prevented figure is quite close to the implied annual reverse incapacitation effects caused by the 2006 Italian pardon (Buonanno and Raphael 2013) and, when expressed as elasticites, is quite close to the estimates in Levitt (1996) using a much different identification strategy.

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

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

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

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

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

where i = (1, …, 57) indexes counties, t = (October 2011, …, September 2012) indexes the end month of the change, $\Delta Crime_{it}$ is the pre-post realignment change in monthly crime rates, $\Delta Prison\ Incarceration\ Rate_{it}$ is the pre-post realignment change in the number of county residents incarcerated in a state prison, $\Delta Jail\ Incarceration\ Rate_{it}$ is the pre-post realignment change in the number of county residents incarcerated in a local county jail, α_i and β_t are county and month fixed effects, respectively, δ and γ are parameters to be estimated, and ε_{it} is a mean-zero error term. Before discussing how we characterize the pre-realignment change, we offer some general comments about the specification of equation (1) and the variation used to identify the key coefficient of interest, δ.

First, the reform explicitly provides for the incarceration of non-violent, non-sexual, and non-serious offenders in local jails as well as for discretion for local criminal justice officials to punish released prison inmates who violate the terms of their conditional releases with jail spells. In practice, this has led to the reduction in the prison population being partially offset by an increase in the population of county jails. Figure 5 illustrates this fact. The state's total jail population was roughly 72,000 in the months before realignment and then gradually increased to over 80,000 over the first post-reform year. Lofstrom and Raphael (2013) find a cross-institution substitution rate of about one for three—that is to say, each three-person reduction in a county's prison incarceration rate resulted on average in a one-person increase in the local jail incarceration rate. Moreover, most of this increase reflected increases in the number of convicted felons serving time in local jails, rather than an increase in jail incarceration for parole violators. Ultimately, we wish to answer whether an increase in the number of convicted offenders not in custody (i.e., on the street) affects crime. Hence, it is vital that we control for the corresponding changes in jail incarceration rates in equation (1).

Second, equation (1) includes a complete set of month fixed effects corresponding to the end month of the change defining each observation. Including time fixed effects effectively nets out the overall state time trends for crime changes and identifies the prison-crime effects based on variation above and beyond what occurs for the state overall. This is a particular important adjustment, as preliminary crime data published by the FBI for 2012 suggests that among western states (and, in particular, states that share a border with California), crime increased in 2012.

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

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

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

A potential weakness of this strategy is that the change over the course of a full year may reflect underlying trends in crime and corrections that pre-date the implementation of realignment. This is particularly problematic for the earlier months in our analysis, such as October through December 2011 when the majority of the period over which changes are measured lies within the pre-realignment period. Variation in crime and incarceration over this earlier time period may be driven by exogenous shocks to criminal offending that create the simultaneity bias to which much research on the prison-crime effect has been devoted to correcting. In addition, several policy reforms in California predate AB 109, most of which were geared toward reducing the prison population to comply with the federal court order. For example, in January 2010, California implemented probation reform, with the primary change being the implementation of performance incentive funding that granted local probation departments 40 to 45 percent of state corrections savings for reducing admissions to prison from the stock of convicted offenders on probation. In addition, in late 2009, the state implemented a reform to state parole, officially creating a new class of low-risk prison releases for less intensive post-release supervision. Combined, these two reforms reduced the prison population between January 2010 and September 2011 from 167,694 to 160,482. Given the policy activity before realignment's implementation and other potential sources of variation in crime and incarceration rates, one might want to focus more tightly on time periods that isolate variation in incarceration rates, and consequent effects on crime, more clearly driven by the 2011 reforms.

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

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

In the presentation of our empirical results, we estimate various specifications of equation (1) using both characterizations (the year-over-year changes and the difference-in-difference changes) of the dependent and explanatory variables. We test for the sensitivity of our results to inclusion of the month and county fixed effects and also test for heterogeneity in the prison-crime effects by pre-realignment incarceration rates. Finally, all models estimation results presented below calculate robust standard errors that permit clustering by county.

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

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

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

The synthetic control method identifies a convex combination of the *J* states in the donor pool that best approximates the pre-intervention time series for the treated state. Define the *J* x 1 weighting vector $W = (w_1, w_2, ..., w_J)'$ such that $\sum_{j=1}^{J} w_j = 1$, and $w_j \geq 0$ for *j* = (1, …, *J*). The product F_1W then gives a weighted average of the pre-intervention time series for all states omitting California, with the difference between California and this average given by $F_0 - F_1W$. The synthetic control method essentially chooses a value for the weighting vector, W, that yields a synthetic comparison group (consisting of an average of some subset of donor states) that best approximates the pre-intervention path for California. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

$$
\begin{aligned}
&W^* = \arg\min_{W}(F_0 - F_1W)'V(F_0 - F_1W) \\
(2) \qquad &s.t. \\
&W'i = 1, w_j \geq 0, \; j = (1, ...J)
\end{aligned}
$$

where V is an 11 x 11, diagonal positive-definite matrix with diagonal elements providing the relative weights for the contribution of the square of the elements in the vector $F_0 - F_1W$ to the objective function being minimized.

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

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

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

To formally test the significance of any observed relative increase in California's crime rates, we apply the permutation test suggested by Abadie Diamond, and Hainmueller (2010) to the difference-in-difference estimator discussed above.[1] Specifically, for each state in the donor pool, we identify synthetic comparison groups based on the solution to the quadratic minimization problem. We then estimate the difference-in-difference for each state as if these states had enacted the equivalent of California's realignment with comparable timing. The distribution of these "placebo" difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimate DD_{CA}. To be specific, if the cumulative density function of the complete set of DD estimates is given by $F(.)$, the p-value from a one-tailed test of the hypothesis that $DD_{CA} < 0$ is given by $F(DD_{CA})$.

Empirical Results

Table A1 presents estimation results for various specifications of equation (1). Panel A presents results where the dependent variable is the change in the total violent crime rate, and panel B presents results for the change in the total property crime rate. Within each panel, we present results for each of our characterizations of the pre-post change in the dependent and explanatory variables. Within each characterization, we present results for five specifications, varying whether we control for the contemporaneous change in the local jail population and the mix of county and month fixed effects. The final specification corresponds to the full model specified in equation (1). All regressions are weighted by county population and the calculated standard errors are clustered by county.

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

The models employing the difference-in-difference characterization yield weaker evidence of an effect of variation in county incarceration rates on county violent crime rates. In several specifications, the coefficients are the wrong sign and in four of the five specifications are statistically insignificant. The one specification where the coefficient has the correct sign and is significant is when we control for county fixed effects only and the change in the local jail incarceration rate. However, adding month effects to the specification, essentially adjusting for state-level trends, reduces the coefficient to zero.

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

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

The magnitudes of these results are consistent with those from previous research on the prison-crime effects. For example, Johnson and Raphael's (2012) analysis of state-level panel data provides estimates of the effect of a one-person increase in the prison incarceration rate for the period 1978 to 1990 and 1991 and 2004. During the earlier period, state incarceration rates averaged 186 per 100,000, whereas during the later period, state incarceration rates averaged 396 per 100,000. The authors find that, for the earlier period, each prison-year served reduced annual reported property crimes by 11.4 incidents and annual violent crimes by 2.5 incidents. During the later period, each prison-year served reduced annual reported property crime by 2.6 incidents and annual reported violent crime by 0.4 incidents. Raphael and Stoll (2013) update these results for a more recent period, from 2000 to 2010, when the average state incarceration rate was 449 per 100,000. They estimate that each prison-year served prevented 2.05 reported property crimes per year and 0.18 reported violent crimes.

To render the results in Table A1 comparable, we must multiply each coefficient by 12, since the estimation results here pertain to monthly crime. The largest point estimate in Panel A for violent crime suggests that each prison-month served prevents 0.041 violent incidents per month (the specification from the difference-in-difference models controlling for the change in the jail incarceration rate and county fixed effects). This coefficient estimate implies that each prison-year served prevents 0.5 violent incidents. For property crime, the largest coefficient comes from the difference-in-difference model adjusting for the change in the jail population and not controlling for month or county fixed effects (with a coefficient on the change in the prison incarceration rate of –0.183). This estimate implies that 2.2 reported property crimes per year are prevented per prison-year served. Note that both estimates are likely too high, as we have selectively chosen the largest coefficients from the table, neither of which is adjusted for state-level crime trends. Nonetheless, the implied effect sizes are consistent with those from previous research and suggest per prison-year served effects that are considerably lower than estimates from time periods in the United States when the incarceration rate was much lower.

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

Table A3 presents comparable results for individual property crime rates. For burglary and larceny, results are inconsistent across specifications and across our alternative characterization of pre-post reform change. Surprisingly, the evidence of an effect of the reform on larceny is the weakest. Note that the increase in larceny accounts for nearly 50 percent of the increase in property crime rates between 2011 and 2012 for the state, suggesting that the recent prison reforms provide only a partial explanation for the increase in property crime statewide. In contrast, we find robust evidence that realignment-induced declines in the prison population have caused increases in motor vehicle theft. For both change characterizations, the coefficient estimates are statistically significant in each specification, do not appear to be sensitive to controlling for month effects, and are comparable in magnitude across specifications. Interestingly, the largest point estimates come from the complete model specifications inclusive of county and year fixed effects.

Diminishing Incarceration Effects on Crime

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

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

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

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

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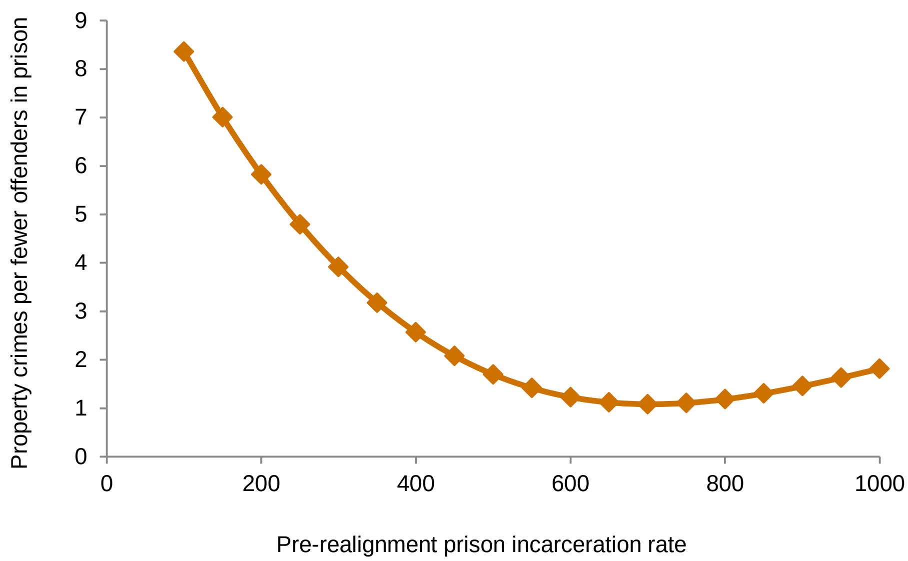
[2] In other words, the incarceration rate in San Benito at the 25th percentile exceeded that of only 25 percent of California counties, whereas the incarceration rate in Del Norte County at the 75th percentile is greater than that of 75 percent of the counties.

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

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

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



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

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

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

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

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

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

Comparing Our Estimates to Those in the Existing Research

Regarding results from previous research, the speed and size of the reduction in California's incarceration rate is unprecedented in the United States, and thus it is impossible to find a comparable evaluation conducted within the United States. However, there is a relevant example from another country. On July 31, 2006, the Italian Parliament passed legislation that reduced the sentences of most Italian prison inmates by three years effective August 1, 2006. The clemency applied only to inmates convicted of a subset of felonies committed before May of that year. The passage of the Collective Clemency bill followed a six-year debate surrounding Italian prison conditions, spurred in large part by the activism of the Catholic Church and the personal involvement of Pope John Paul II. With Italian prisons filled to 130 percent of capacity, the one-time pardon was principally motivated by the need to address prison overcrowding.

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

Why was the effect on crime so much larger in Italy than in California? For one, these are two very different places with different demographics and systems of policing and criminal sentencing. Hence, the disparity may be due in part to differences in institutional and cultural factors. However, other key differences between the two case studies are likely key to understanding the difference in outcomes. First, the pre-pardon incarceration rate in Italy stood at roughly 103 per 100,000 residents, quite close to the U.S. incarceration rates that existed before 1980. In California, the pre-reform incarceration rate was between 425 and 430 per 100,000, more than four times that of Italy. If we add California's roughly 75,000 jail inmates (a more appropriate comparison to Italy, since Italy has a unified prison and jail system), this rate increases to 625 per 100,000. Hence, one possible explanation is that California casts a much wider net in terms of who is sent to prison and for how long. Consequently, the average pre-reform inmate in California is perhaps less criminally prone than the average inmate in Italy where prison is used more sparingly. Moreover, the Italian Collective Clemency affected a broader base of prison inmates, whereas California's realignment reforms were much more selectively focused on non-violent offenders and parole violators.

Steven Levitt (1996) provides one of the most widely cited studies in this vein. Levitt analyzes data for U.S. states covering 1971 through 1993, a period over which the average state in his sample had an incarceration rate of 166 per 100,000. The estimates in the study imply that each prison-year served prevented approximately one violent offense and roughly seven property offenses. Raphael and Stoll (2013) provide a similar analysis yet for separate time periods in the United States across which incarceration rates differ greatly. Specifically, Raphael and Stoll estimate the average number of crimes prevented for each prison-year served for three time periods: 1977 to 1988, 1989 to 1999, and 2000 to 2010. Average state incarceration rates during these three time periods were 171, 349, and 449, respectively. The authors estimate that during the earliest period when incarceration rates were the lowest, each prison-year served prevented roughly 1.2 violent felony offenses and 8.6 property offenses (roughly in line with Levitt's estimates). For the latter two periods with higher incarceration rates, the average effect on violent crime falls to zero. The authors find effects on property crime of roughly 1.3 crimes per prison year served during the 1990s and roughly 2 crimes per prison year served for the period from 2000 to 2010. Liedka, Piehl, and Useem (2006) provide an additional analysis of state-level crime and incarceration data, with an explicit focus on how the effect of incarceration on crime varies with the overall incarceration rate. The authors present strong evidence that the effectiveness of incarceration as a crime-control device declines as the incarceration rate grows.

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

Appendix Tables

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

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

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

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

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

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

TABLE A2
Regression estimates of the prison-crime effects for individual violent crimes

	No fixed effects, no control for jail change	No fixed effects, control for jail change	Month effects	County effects	Month and county effects
Murder					
Year-over-year changes	–0.0004 (0.0004)	–0.0004 (0.0004)	–0.0005 (0.0005)	–0.001 (0.001)	–0.001 (0.001)
Difference-in-difference changes	–0.001 (0.001)	–0.0017 (0.0012)	–0.0016 (0.0014)	–0.001 (0.001)	–0.001 (0.002)
Rape					
Year-over-year changes	–0.001 (0.001)	–0.001 (0.001)	0.000 (0.001)	–0.002 (0.002)	–0.002 (0.004)
Difference-in-difference changes	0.003 (0.002)	0.004* (0.003)	0.006* (0.003)	–0.002 (0.002)	–0.001 (0.003)
Robbery					
Year-over-year changes	–0.009* (0.005)	–0.012** (0.005)	–0.005 (0.005)	–0.015* (0.008)	0.004 (0.010)
Difference-in-difference changes	0.001 (0.010)	–0.003 (0.010)	0.006 (0.013)	–0.016* (0.008)	0.004 (0.010)
Aggravated assault					
Year-over-year changes	–0.022*** (0.007)	–0.021*** (0.006)	–0.014* (0.008)	–0.022* (0.012)	0.008 (0.014)
Difference-in-difference changes	–0.008 (0.015)	–0.017 (0.012)	–0.004 (0.015)	–0.023* (0.012)	0.007 (0.015)

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

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

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

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

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

	No fixed effects	No fixed effects	Month effects	County effects	Month and county effects
Burglary					
Year-over-year changes	–0.005 (0.019)	–0.012 (0.022)	–0.013 (0.030)	–0.004 (0.027)	0.019 (0.041)
Difference-in-difference changes	–0.032 (0.031)	–0.079*** (0.024)	–0.076*** (0.028)	–0.003 (0.027)	0.024 (0.042)
Larceny					
Year-over-year changes	–0.045 (0.033)	–0.073* (0.039)	–0.047 (0.040)	–0.078* (0.042)	–0.010 (0.073)
Difference-in-difference changes	–0.016 (0.038)	–0.035 (0.039)	–0.014 (0.044)	–0.081* (0.043)	–0.013 (0.075)
Motor vehicle theft					
Year-over-year changes	–0.057*** (0.018)	–0.080*** (0.017)	–0.062*** (0.020)	–0.080*** (0.023)	–0.099*** (0.036)
Difference-in-difference changes	–0.068*** (0.024)	–0.069** (0.029)	–0.069* (0.037)	–0.081*** (0.023)	–0.103*** (0.037)

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

TABLE A7 *(continued)*

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

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

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

NOTE: Standard errors are in parentheses

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

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

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

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