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ABSTRACT

The Effect of Sentencing Reform on Crime Rates: Evidence from California's Proposition 47

We evaluate whether California's state proposition 47 impacted state violent and property crime rates. Passed by the voters in November 2014, the proposition redefined many less serious property and drug offenses that in the past could be charged as either a felony or misdemeanor to straight misdemeanors. The proposition caused a sudden and sizable decline in county jail populations, a moderate decline in the state prison population, a decrease in arrests for property and drug offenses, and a wave of legal petitions filed for retroactive resentencing and reclassification of prior convictions. We make use of multiple strategies to estimate the effect of the proposition, including state-level synthetic cohort analysis, within-state event study estimates based on state-level monthly time series, and a cross-county analysis of changes in county-level crime rates that exploit heterogeneity in the effects of the proposition on local criminal justice practices. We find little evidence of an impact on violent crime rates in the state. Once changes in offense definitions and reporting practices in key agencies are accounted for, violent crime in California is roughly at pre-proposition levels and generally lower than the levels that existed in 2010 prior to a wave major reforms to the state's criminal justice system. While our analysis of violent crime rates yields a few significant point estimates (a decrease in murder for one method and an increase in robbery for another), these findings are highly sensitivity to the method used to generate a counterfactual comparison path. We find more consistent evidence of an impact on property crime, operating primarily through an effect on larceny theft. The estimates are sensitive to the method used to generate the counterfactual, with more than half of the relative increase in property crime (and for some estimates considerably more) driven by a decline in the counterfactual crime rate rather than increases for California for several of the estimators that we employ. Despite this sensitivity, there is evidence from all methods tried that property crime increased with, a ballpark summary of five to seven percent roughly consistent with the totality of our analysis. Similar to violent crime, California property crime rates remain at historically low levels.

JEL Classification: K40, K42, H11

Keywords: incarceration, sentencing, crime, jail, prison, Proposition 47

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1. Introduction

After decades of continuous growth, the U.S. incarceration rate peaked in 2008 at 506 inmates per 100,000 before declining modestly by approximately 10 percent. While several states and the federal prison system have experienced prison population declines, a remarkable and unprecedented series of policy reforms in California contributed disproportionately to the decline in the national incarceration rate.[1] Two reforms in particular generated pronounced and fairly sudden declines in incarceration.

First, under pressure from a federal court order to relieve prison overcrowding, California passed legislation in 2011 that realigned responsibility for punishing and monitoring relatively less serious felony offenses from the state to the counties. These "realignment" reforms reduced the state prison population by roughly 34,000 inmate (a 20 percent decrease) while generating an offsetting 10,000 person increase in the average daily population of county jails.

Second, in November 2014 California voters passed state proposition 47. The proposition redefined a series of "wobbler" offenses (offenses that can be charged as either a felony or misdemeanor) as straight misdemeanors. The proposition also included a provision for the filing of resentencing petitions for those under correctional supervision at the time the proposition passed as well as a provision for prior conviction reclassification for those with prior felony convictions that would now be classified as a misdemeanor. The proposition caused a sudden decline in the state's jail population of roughly 7,600 inmates (a nine percent decline), sudden and sharp decreases in arrests for property offenses, a dramatic increase in the proportion of drug arrests that are for misdemeanors, and a further reduction in the state prison population of roughly 3.4 percent. Collectively, these reforms reduced the state's prison incarceration rate by roughly one quarter, from 442 in 2010 to 330 per 100,000 in 2016. California's

[1] Between 2008 and 2016 the U.S. prison population declined by 102,885 inmates. Comparing totals for the federal prison population and state prison populations published in Carson (2018) and West and Sabol (2010) reveals that 12 percent of this decline is attributable to a decline in the federal prison population, 40 percent is attributable to the decline in California's prison population, while 48 percent is attributable to declines in the remaining 49 state prison populations.

current incarceration rate has been rolled back to early 1990 levels (a time period that precedes "Three-Strikes" sentencing reform by several years).[2]

In prior research, we evaluated the effects of the 2011 realignment reform on California crime rates (2016a, 2016b). Our analysis revealed no evidence of an impact on violent crime rates and a relatively small effect on motor vehicle theft. The impact on auto theft and property crime more generally was small relative to the findings from comparable analyses for the United States and other nations (Levitt 1996; Liedka, Piehl, and Useem 2006; Johnson and Raphael 2012; Buonanno and Raphael 2013; Vollard 2013; Barbarino and Mastrobuoni 2014). Bartos and Kubrin (2018) provide the sole evaluation of the crime effects of proposition 47. The authors use a synthetic comparison estimator to identify a synthetic comparison group based with crime trends that parallel those for California through 2014. The authors find no effect on violent crime, but suggestive evidence of a modest impact on larceny and motor vehicle theft that are not quite statistically significant.

In this paper, we build on recent work by Bartos and Kubrin (2018) to evaluate whether the second set of reforms ushered in by the passage of proposition 47 impacted California crime rates. We extend the analysis in several direcitons. Following our earlier realignment research and the methodological strategy pursued in the evaluation of Italy's 2006 Collective Clemency in Buonanno and Raphael (2013), we employ several strategies to test for an effect of the proposition. First, we use the synthetic cohort estimator presented in Abadi and Gardeazaba (2003) and Abadi, Diamond and Hainmueller (2010) to identify a weighted average of comparison states with crime time series that best match those of California. We use these comparison states to generate a series of difference-in-difference estimates to test whether the disparities in crime rates between California and comparison states widen with the passage of proposition 47 above and beyond any measurable impact from the earlier reform.

[2] Relative to other states, California's incarceration rate ranked 31 out of 50 in 2010 and 18 out of 50 in 2016.

Second, we use higher frequency monthly data to project counterfactual crime rate trends for California based on pre-intervention momentum in the months leading up to the passage of proposition 47. We then estimate the effect of the proposition by comparing actual monthly crime rates to these projections. We present separate estimates of annualized crime effects based on discontinuous changes in monthly crime coinciding with the implementation of the proposition as well as estimates that use pre-intervention trends to project a counterfactual path for the entire post-intervention period.

Finally, we exploit variation across counties in the impact of proposition 47; in particular the fact that counties with relatively high pre-47 jail incarceration rates experienced the largest relative declines in jail incarceration rates. Using the sharp exogenous shock caused by the policy change we assess whether crime increased more in counties experiencing larger declines in jail incarceration rates, whether changes in county-level crime rates depend on the quantity per capita of resentencing and reclassification petitions filed, and whether changes in crime rates depend on county-level changes in arrest activity.

In terms of overall crime trends, crime rates in California are currently no higher than they were in 2010 prior to both policy reforms. While the official violent crime rate is slightly higher, this is due to a change in the definition of rape that mechanically increased the number of incidents and by a change in the process used to classify and record aggravated assaults in the largest police agency in the state beginning in late 2014. The overall property crime rate in 2016 is lower than the level in 2010. In general, crime rates in California remain at historically low levels despite the pronounced declines in incarceration and the downgrading of relatively less serious property and drug offenses.

Regarding the specific effects of proposition 47, we do not find consistent evidence of an effect of the reform on violent crime confirming, a finding that aligns with that of Bartos and Kubrin (2018). Violent crime overall as well as specific violent crimes do not increase relative to comparison states and do not increase in counties that were more impacted by proposition 47 relative to counties that were less impacted. We do observe some statistically significant results that are not consistent across

methodological approaches. For example, the synthetic cohort analysis reveals a statistically significant decline in murder in California relative to other states while the within state time series analysis and cross-county analysis do not. The time series analysis generates a statistically significant increase in robbery while the synthetic cohort and cross-county analyses do not. In general, the estimates regarding impacts on violent crime are close to zero, and when significant (either in the negative or positive direction) are not robust to the method used to project the counterfactual. A methodological issue of particular concern is the historically low crime rate in California in the year that largely preceded the passage of proposition 47 and the attendant risk of overfitting the pre-existing trends to an outlier pre-intervention year.

We do observe more consistent evidence of an impact of the proposition on property crime, primarily larceny theft, although the magnitude of the effect varies across methods. The synthetic cohort analysis reveals a 5 to 7.5 percent relative increase in property crime relative to comparable changes for other states that is driven largely by a relative increase in larceny theft. The magnitudes of the estimates from the within-state analysis of monthly date are quite similar to the synthetic cohort estimates when we annualize the discontinuous change coinciding with the implementation of the proposition, but considerably larger when we use pre-proposition trends to project the counterfactual over the entire first post-intervention year. This difference is due to the sizable declines in California property crime rates from 2013 and 2014 and the implications for the projected counterfactual. Moreover, this within-state monthly analysis finds significant effects on motor vehicle theft. The difference here relative to the synthetic cohort analysis is due to the within-state time series projecting sizable counterfactual decreases in motor vehicle theft rates while motor vehicle theft rates in California's synthetic comparison group increases substantially between 2014 and 2016.

The cross-county analysis yields some though not particularly robust evidence that counties that were more impacted by the proposition experienced greater increases in property crime. The largest estimates from the cross county analysis suggest an impact on overall property crime of roughly 3 percent,

though many of the model estimates (in particular those that do not weight the county-level regression by population) find no significant effects. Again, the results suggest that this was largely due to a post-proposition 47 increase in larceny theft.

2. The Potential Causal Pathways Between California Policy Reforms and State Crime Rates

Aggregate indicators of crime and punishment in California have historically moved in lock-step with those of the nation. Figure 1 presents the prison incarceration rates for California and for the U.S. from the late 1970s through 2016. Both series exhibit pronounced increases during the last two decades of the 20th century. From the early 2000s on however, there are notable departures with large relative decreases in California's incarceration rates post 2010. Figure2 presents long term trends for overall California violent and property crime rates. Similar to national trends, California's violent crime rate peaks in the early 1990s before declining to current historical lows. While the historical peak for property crime occurs in the early 1980s, the largest declines in property crime occur post 1990, with the rate declining by roughly 50 percent over the subsequent 26 years. In both figures, the years 2010 (the last pre-realignment year) and 2014 (a year mostly preceding the implementation of proposition 47) are marked with vertical lines. Notably, these reforms reduced the state's prison incarceration rate to early 1990s levels while crime rates have remained at historical lows.

Two broad factors converged to generate the reduction in the state incarceration rate depicted in Figure 1. First, decades of litigation pertaining to conditions of confinement and the availability of health and mental health services in the state prison system culminated in a federal court order to reduce state prison overcrowding. Second, public opinion pertaining to sentencing severity and the use of incarceration in particular softened, resulting in several notable ballot measures aimed at undoing many of the stringent sentencing practiced introduced in past decades.

To address the court order, California enacted broad corrections reform legislation under the banner of corrections realignment (passed in April 2011 and implemented on October 5, 2011).[3] The legislation eliminated the practice of returning parolees to state prison custody for technical parole violations for all but a small set of the most serious offenders. The legislation also defined a group of non-serious, non-sexual, non-violent offenders who upon conviction serve their sentences in county jails. The act generated an immediate reduction in weekly prison admissions from roughly 2,100 per week to 600 per week and a steady, permanent decline in the prison population. This resulted in the sharp and sudden reduction in the prison population depicted in figure 3.

Regarding the change in public opinion, in recent year California voters passed several state ballot initiatives aimed at reducing the use of prison along both the intensive and extensive margins. In 2012, voters approved a ballot measures that narrowed the definition of felonies that would qualify for second and third-strike sentence enhancements, limiting these felonies to serious and violent offenses (proposition 36). More recently, voters passed a proposition that incentivizes prison inmates to engage in rehabilitative programming and refrain from institutional misconduct in exchange for shorter prison terms (proposition 57 passed in November 2016).

The passage of proposition 47 in November 2014, however, is perhaps one of the most far-reaching sentencing reforms passed by way of ballot initiative and had immediate impacts on the operations and practices of several different arms of the state's criminal justice system. Put simply, the

[3] The legislation was prompted by pressure from a federal three-judge court overseeing the California prison system, impaneled as a result of legal decisions 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 and that the poor health and mental health care systems violated the 8th amendment prohibition against cruel and unusual punishment. Assembly Bill 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 inmates if the state failed to act on its own.

proposition redefined a sub-set of “wobbler” offenses (offenses that can be charged as either a misdemeanor or felony) as straight misdemeanors offenses. Regarding property offenses, the proposition redefined shoplifting, forgery, crimes involving insufficient funds, petty theft, and receiving stolen property offenses where the value of the property theft falls below $950 as misdemeanors. The proposition also eliminated the offense of petty theft with a prior. Regarding drug offenses, a subset of possession offenses were redefined as misdemeanors. These new charging protocols apply to all new cases with the exception of instances where the individual in question has certain prior convictions. The proposition also included a provision for individuals currently serving sentences for reclassified offenses to file a resentencing petition, as well as a provision for those convicted in the past to file a petition to have the prior conviction reclassified as a misdemeanor (California Judicial Council 2016).

The passage and implementation of proposition 47 impacted jail populations, the state prison population, policing, and the sanctions associated with specific offenses. The effect on the expected severity of punishment is self-evident and the direct intended consequence of the proposition. The impacts on correctional populations and policing however bear closer analysis. Figure 4 presents the average daily population of county jails for the 26 months preceding and the 26 months following the passage of proposition 47 as well as comparable monthly time series for the state prison population. There is an immediate decline in the average daily jail population, with the monthly average declining by 7,717 inmates (a 9.4 percent decrease) between the two periods. We also observe a discrete decrease in the state prison population of 4,570 inmates (a 3.4 percent decline). Figure 5 reveals a sharp decline in jail bookings, suggesting that the decrease in jail populations is driven by either an increase in the propensity to cite and release for offenses that in the past would have generated a jail booking, an overall reduction in arrests, or some combination thereof. The figure also reveals a discrete decrease in early

releases from jail due to capacity constraints, suggesting that proposition 47 appreciably relieved overcrowding pressure in county jails throughout the state.[4]

Figure 6 reveals the factors driving the decline in the state's prison population associated with the passage of proposition 47. First, we observe a notable decrease in prison admissions overall (displayed in the top half of the figure) driven principally by a reduction in admissions for property and drug offenses (displayed in the bottom half of the figure). Second, there is a temporary spike in releases driven by resentencing petitions as well as other population reduction measures coincidentally implemented by the California Department of Corrections and Rehabilitation (CDCR) to comply with the federal court order to reduce prison overcrowding.

Finally, Figure 7 displays trends in monthly arrest totals tabulated from the California Monthly Arrest and Citation Register data. The figure displays monthly arrests through the end of 2015 inclusive of booked arrests and arrests that result in citation and release. The figure presents monthly arrest totals by whether the arrest is for a person offense, a property offense, a drug offense, or a catch-all "other arrests" category. Within each arrest category, arrests are further subdivided by whether the offense is classified as a felony or a misdemeanor. There are several notable patterns in Figure 7. First, arrests for person offenses and other offenses are essentially stable and not visibly impacted by proposition 47. For drug offenses however, we observe a sharp decline in felony arrests and a sharp increases in misdemeanor arrests. Comparing the 14 months prior to proposition 47 to the 14 post months, average monthly drug arrests overall (felony and misdemeanors combined) decline by 15 percent. Property arrests, also decline discretely. Here, however, there is no apparent increase in misdemeanor arrests. Again, comparing

[4] One consequence of the 2011 realignment reform was an increase in the average daily population of county jails of approximately 10,000 inmates (essentially undoing one third of the prison population decline). This increase was driven mostly by the fact that parolees who violated the terms of their supervised release were sanctioned with relatively short local jail spells or some other community sanction rather than prison. Since nearly half of county jail systems in the state were operating under independent court orders to reduce overcrowding, realignment increased emergency releases due to capacity constraints (Lofstrom and Raphael 2013b). Proposition 47 relieved many of these pressures on county sheriff departments across the state.

average monthly arrests in the 14 months preceding and following the passage of proposition 47, there is a 20 percent decline in average monthly property crime arrests.

Given the impact of the proposition on jail populations, prison populations, the likelihood of being arrested, and the sanctions one faces in the event of arrest and conviction, there are multiple avenues through which the proposition may impact crime rates. Beginning with incapacitation, there is a sizable body of research estimating the amount of officially recorded crime prevented by a year of detention, with most of the research focusing on prison incarceration. Methodologically, these studies range from surveys of prison inmates regarding past offending (reviewed in Spelman 1994, 2000), state panel data analyses that use various instrumental variables strategies to identify the causal connection from changes in incarceration to crime (Levitt 1996, Liedke, Piehl, and Useem 2006, Johnson and Raphael 2012), studies that exploit sentencing reforms that either enhance (Vollaard 2012) or reduce (Owens 2009) sentences for admittedly criminally active people, to studies that evaluate the effects of sudden, discrete, and policy-induced changes in correctional populations (Buonanno and Raphael 2013, Barbarino and Mastrobuoni 2014, Lofstrom and Raphael 2016a). While these studies vary greatly methodologically, this corpus of work yields several findings that inform our priors.

First, most careful analyses reveal significant incapacitation effects associated with prison incarceration, and to a lesser extent jail incarceration. Second, these incapacitation effects tend to be much higher in low incarceration settings. That is to say, the marginal effect on crime of a one person increase in the incarceration rate tends to be much higher in countries with low incarceration rates or in periods of time in the United States when the incarceration rate was low, a pattern suggestive of diminishing crime-fighting returns to scale. For example, our prior analysis of the effect of the 2011 realignment reform revealed no impact on violent crime and a modest effect on property crime (Lofstrom and Raphael 2016a). Note, the decline in incarceration caused by realignment occurred in a state with a total incarceration rate (prison plus jail) that exceed 700 per 100,000. By contrast, evaluation of a

similarly-sized prison decline caused by Italy's 2006 collective clemency in Buonanno and Raphael (2013) revealed considerably larger incapacitation effect. Italy's total incarceration rate on the eve of the clemency was roughly one-seventh that of California's. Nonetheless, the study also found smaller effects of prison releases on crime rates in Italian provinces with relatively high incarceration rates, despite the generally low Italian incarceration rate. Liedke, Piehl, and Useem (2006) find evidence in state panel data indicating that incapacitation effects in the United States diminish with the incarceration rate, as do Johnson and Raphael (2012).

These findings suggest that incapacitation effects are inherently heterogeneous –e.g., likely to be larger for the young than the old, likely to vary with prior criminal history, and likely to vary on average with the extensiveness with which a given society deploys incarceration in an attempt to control crime. On the eve of the passage of proposition 47, the California incarceration was considerably lower than in years past, yet still high by historical standards in the U.S. and relative to other nations.

Beyond incapacitation, the decline in property and drug arrests as well as the downgrading of several offenses to misdemeanors translates into both a lower likelihood of apprehension conditional on committing one of the reclassified crimes as well as less severe punishment if convicted. Both factors lower the expected value of the sanction one is likely to face for committing a crime impacted by the change in policy, yet may exhibit differential effects on offending to the extent that individuals who criminally offend have high discount rates or are myopic. The results from research on whether changes in sanction severity deters criminal offending tend to be mixed. Among the studies finding evidence of general deterrence, Drago, Galbiati, and Vertova (2009) find that individuals released from Italian prisons under the 2006 Italian Collective Clemency who faced larger sentence enhancements for re-offending tended to recidivate at a lower rate. Similarly, Helland and Tabarrok (2007) find that individuals facing the prospect of a sentence enhancement due to state three-strikes laws recidivate at relatively lower rates. In contrast, research exploiting the discontinuous increase in sentencing severity at the age of

majority tends to find little evidence of an impact of the stiffer sentencing on offending (see Hjalmarsson 2009, Lee and McCrary 2009, but Levitt 1998 for a comparable analysis with contrary findings) as does much of the research on the deterrent effects of capital punishment (Blumstein, Cohen, Nagin 1978; Nagin and Pepper 2012). A thorough review of the deterrence effects of sanction severity concludes that general deterrence effects associated with stiffer penalties tend to be small (Nagin 2013; also see the review in Raphael and Stoll 2013, chapter 7).

There is considerably stronger evidence that changes in the likelihood of apprehension, proxied by either changes in police staffing levels or enforcement surges, impacts criminal offending. A recent review of experimental research conducted as a part of a National Academies of Sciences consensus panel on proactive policing concluded that hot spots policing, where concentrated enforcement and patrol efforts are targeted towards high crime city blocks, appreciably reduces crime with little evidence of displacement (National Academies of Sciences 2017). Several quasi-experimental studies of policing and enforcement surges yield similar conclusions. For example, Di Tella and Schargrodsky (2004) analyze an exogenous increase in police presence outside Jewish institutions in Buenos Aires, Argentina. Following a 1994 terrorist attack on an Argentine Jewish center that killed 85 people and wounded 300, Argentina increased police presence to 24 hours per day outside all Jewish and Muslim institution throughout the country. Comparing monthly auto thefts before and after the terrorist attack, the authors document a sharp decline (on the order of 75 percent) in auto thefts on blocks experiencing increased enforcement, with no measurable effect (negative or positive) in neighboring blocks, or those that are two or more away.

In a similar vein, Klick and Tabarrok (2005) exploit changes in the terror-alert levels under the Homeland Security Advisor System established by the Department of Homeland Security in the wake of the September 11 terrorist attack. Analyzing a 500 day period in 2002 and 2003, the authors exploit the fact that during high-alert time periods the Washington D.C. police increase policing resources by roughly

50 percent in key destinations such as the mall. The authors compare average daily crime on days during high alert periods to other days, and find significant reductions in daily crime when the high alert system was activated.

There is also robust evidence of sizable effects of police staffing levels on crime. Chalfin and McCrary (*forthcoming*) estimate the effect of changes in city-level police staffing levels on crime rates using panel data for the period 1960 through 2010, with a correction for attenuation bias associated with measurement error in police staffing data. Accounting for attenuation bias leads to quite large (negative) crime-police level elasticities, with sizable and significant effects of the police in reducing homicide, robbery, burglary, and motor vehicle theft. Based on these findings and an accompanying cost-benefit analysis, the authors conclude that many cities in the United States are under-policed. Evans and Owens (2007) analyze city-level panel data for the period 1990 to 2001 to estimate the effect of changes in police staffing levels caused by the receipt of federal grant funds from the COPS program created by the 1994 Violent Crime Control and Law Enforcement Act. The study found that the hiring of new police officers generated statistically significant reductions in robbery, aggravated assault, auto theft and burglary. In accord with Chalfin and McCrary, the authors conclude the dollar value of the crime reduction exceeded budgetary outlays for the new officers.

Finally, Cook and MacDonald (2011) found a direct dose-response effect of private security in the context of business improvement districts (BID) created in Los Angeles during the 1990s, with each dollar spent on private security preventing $20 worth of crime and no evidence of displacement to other areas. The creation of a BID had no measureable effect on crime in the absence of security expenditures.

This body of research articulates the possible mechanisms that may link the proposition 47 reforms to changes in crime rates. The proposition clearly reduced incarceration rates. Given heterogeneity in the inmate population, the concentration of the impact on jail populations, and the still high incarceration rate in California relative to years past and other nations, prior research findings

suggest that any reverse incapacitation effect is likely to be modest. Nonetheless, the incarceration decline caused by the proposition begins from a starting incarceration level that is considerably lower than the rate that existed before the 2011 realignment reform. Depending on the shape of the relationship between the overall incarceration rate and the marginal crime-preventing impact, incapacitation effects of the latest reform may plausibly be larger than the per-unit effects estimated for realignment.

The proposition also scaled back the sanctions associated with targeted offenses, eliminating prison time for most. Existing research suggests that such changes have little impact on behavior. However, proposition 47 was well publicized and thus may have impacted the behavior of some.

The decline in arrests clearly indicate that the likelihood of being caught for a property or drug offense declined, and by a sizable amount. To the extent that those who commit or are predisposed to committing the crimes targeted by proposition 47 are short sighted, the realized change in enforcement (as evidenced in the change in the arrest probability) may lead to less overall general deterrence. We now turn to our strategy for estimating the cumulative impact of these three causal channels on state crime rates.

3. Methodological Strategy and Data Description

Quasi-experimental research investigating the relationship between crime rates and changes in criminal justice policy levers such as incarceration, policing, or sentencing severity faces several methodological challenges. First, one must identify exogenous variation in the policy lever to be able to estimate true causal effects from data where both the dependent variable and the explanatory variable are simultaneously determined. Take for example the crime-incarceration relationship. The level of incarceration will certainly depend on the crime rate, with exogenous shocks to crime due to say changes in the economy, changing demographics, changing environmental determinants such as average blood-

lead levels, or shocks to drug markets, simultaneously generating coincident changes in incarceration and crime. In the other direction, incarceration will incapacitate, may generate a criminogenic or specific deterrence effects on prior inmates in steady-state, and may have a general deterrence effect on crime. With causation running in both directions, one must articulate an identification strategy breaking the simultaneity.[5] Similar problems apply to empirical attempts to evaluate the relationship between policing and enforcement on crime or changes in sentencing severity on crime.

Second, even in the face of an arguably exogenous shock there are many alternative strategies for estimating the counterfactual crime rates that can be used to benchmark realized outcomes and characterize the magnitude of the treatment effect. For example, one could simply use crime levels in the treated area in the pre-period, generate estimates of counter-factual crime rates from pre-intervention trends, look to non-treated areas, such as other states for comparison, or assess whether the dose associated with a policy change varies across sub-geographic units.

The major policy reforms in California are arguably exogenous. Realignment was forced upon the state by the federal courts. In fact, the state Attorney General resisted the initial order to reduce the population-to-capacity ratio, an appeal that was eventually heard and overruled by the U.S. Supreme Court. Similarly, proposition 47 passed against a policy backdrop where the state had not yet met the targeted population-to-capacity-ratio ordered by the court. The proposition was opposed by many district attorneys across the state as well as law enforcement. Moreover, as we documented in the previous section both realignment and proposition 47 generate clear breaks from pre-intervention trend in prison and jail incarceration and (in the case of proposition 47) arrest activity. Hence, we feel confident that changes to correctional population aggregates as well as arrest totals are attributable to the proposition and that the proposition in turn was not somehow a function of crime rates.

[5] See Johnson and Raphael (2012) and Buonanno and Raphael (2013) for a formal model of the determination of crime and incarceration rates.

Regarding characterization of the counterfactual path, the raw data on monthly crime rates reveal that estimates of the effect of the proposition are likely to be sensitive to this particular choice. Figure 8 displays monthly violent and property crimes per 100,000 for the period from January 2010 through December 2016. Months are indexed along the horizontal axis relative to November 2014. For reference, the annual average for each calendar year is marked with separate horizontal lines. The figure presents separate time series with and without the crime rate for the city of Los Angeles.[6] We present separate series without Los Angeles due to the introduction of a data integrity unit by the Los Angeles Police Department (LAPD) in November 2014 and a concerted effort in this agency to address a sizable undercounting of aggravated assaults by the department. We discuss this issue in greater detail below.

The figures reveal annual cyclicality in crime rates with what appear to be downward trends in violent and property crime in the two years preceding the passage of the proposition. For violent crime, there is a clear increase when Los Angeles is included, but not when Los Angeles is excluded. For property crime, the pre-proposition downward trend is more pronounced and is not sensitive to the inclusion of Los Angeles. However, the year 2014 (the first 10 months of which precede the passage of the proposition) appears to be a particularly low crime year (in fact, the year with the lowest recorded crime rates since 1969). The data suggests that a linear trend based on property crimes rates in 2012 and 2013 would project directly through the crime rate outcomes for 2015 and 2016, with 2014 being an outlier year. Alternatively, the low property crime rates in 2014 may be reflective of the true underlying trend that would have continued in the absence of the proposition (suggestive of a visible increase in property crime).

To address these issues, we pursue multiple imperfect, yet complementarity strategies for generating counterfactual crime rates for the state to triangulate the impacts of the proposition on state

[6] The series without Los Angeles adjusts both the numerator (reported crimes) and the denominator (population) for the omission of the city of Los Angeles.

crime rates. First, we use synthetic cohort analysis to generate comparison annual time series based on other states with pre-reform trends matching those of California. Second, we use monthly data for California during the pre-proposition period to project counterfactual crime paths in the post period and then compare actual crime to these projections. Finally, we explore whether cross county variation in the dose created by the proposition explains cross county variation in crime rate changes.

A. Difference-in-Difference Estimates from Synthetic Cohort Analysis

Our first strategy analyzes annual state-level crime data from the Federal Bureau of Investigation's Uniform Crime Report for the period 2000-2016. We employ the synthetic control approach of Abadie, Diamond, and Hainmueller (2010) to identify a convex combination of states with pre-intervention crime trends that closely match those of California. We then use this synthetic comparison group to chart out the counterfactual path for California, using this as a benchmark against which actual California crime trends can be compared. Given the amount of policy activity in California since 2011 (realignment in 2011, proposition 36 in 2012), we use the period from 2000 to 2010 to identify the synthetic comparison states. We measure the effect of proposition 47 by assessing the degree to which the difference in crime rates relative to the synthetic comparison cohort widens in the latter years of our panel above and beyond differences that emerge in the immediate pre-proposition 47 period due to earlier reforms.

To be specific, let the index j =(0,1,…,J) denote states. The value j=0 corresponds to California and j=(1,…,J) correspond to each of the other J states that are candidate contributors to the control group (or in the language of Abadie et. al, the donor pool). Define F_0 as a 11x1 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 11xJ matrix F_1 as the collection of comparable time series for each of the 49 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 Jx1 weighting vector $W = (w_1, w_2, ..., w_J)'$ such that $\sum_{j=1}^{J} w_j = 1$, and $w_j \geq 0$ for j=(1,…,J). The product $F_1 W$ then gives a weighted average of the pre-intervention time series for all states omitting California, with the difference between California and this average given by $F_0 - F_1 W$. The synthetic control method essentially chooses a value for the weighting vector, W, that yields a synthetic comparison group (consisting of an average of some subset of donor states) that best approximates the pre-intervention path for California. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

$$W^* = \arg\min_{W} (F_0 - F_1 W)' V (F_0 - F_1 W)$$
$$s.t. \quad (1)$$
$$W' i = 1, w_j \geq 0, j = (1, ...J)$$

where V is a 11x11, diagonal positive-definite matrix with diagonal elements providing the relative weights for the contribution of the square of the elements in the vector $F_0 - F_1 W$ to the objective function being minimized. Once an optimal weighting vector W^* is chosen, both the pre-intervention path as well as the post-intervention values for the dependent variable in "synthetic California" can be tabulated by calculating the corresponding weighted average for each year using the donor states with positive weights. The post-intervention values for the synthetic control group serve as our counterfactual outcomes for California.

Our principal estimate of the impacts of proposition 47 on crime uses the synthetic control group to generate a series of difference-in-difference estimate. Specifically, define $Outcome^{CA}_{2009-2010}$ as the average value of the outcome of interest for California for the pre-intervention years 2009 and 2010, $Outcome^{CA}_{2012-2014}$ as the average value of the outcome during the post-realignment/pre-47 period, and $Outcome^{CA}_{2015-2016}$ as the average value for the outcome in the post-47 period. Similarly, define

$Outcome^{Synth}_{2009-2010}$, $Outcome^{Synth}_{2012-2014}$, and $Outcome^{Synth}_{2015-2016}$ as the comparable averages for the synthetic control group. With these averages, we define and estimate the following three alternative difference-in-difference estimates:

(2)

$$\Delta^2_{realignment} = \left[Outcome^{CA}_{2012-2014} - Outcome^{Synth}_{2012-2014}\right] - \left[Outcome^{CA}_{2009-2010} - Outcome^{Synth}_{2009-2010}\right]$$

$$\Delta^2_{real.+prop47} = \left[Outcome^{CA}_{2015-2016} - Outcome^{Synth}_{2015-2016}\right] - \left[Outcome^{CA}_{2009-2010} - Outcome^{Synth}_{2009-2010}\right]$$

$$\Delta^2_{proposition\ 47} = \left[Outcome^{CA}_{2015-2016} - Outcome^{Synth}_{2015-2016}\right] - \left[Outcome^{CA}_{2012-2014} - Outcome^{Synth}_{2012-2014}\right]$$

The first difference-in-difference estimator identifies the effect of the realignment reforms on crime rates. The second measures the cumulative effects of realignment and proposition 47. The final estimator measured the differential effect of proposition 47 above and beyond the lasting effects the realignment reforms.

To formally test the significance of any observed relative increase in California's crime rates, we apply the permutation test suggested by Abadie et. al. (2010) to the difference-in-difference estimator discussed above.[7] Specifically, for each state in the donor pool, we identify synthetic comparison groups

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

based on the solution to the quadratic minimization problem. We then estimate the three difference-in-difference estimators for each state as if we were testing for comparable policy impacts in these states. The distribution of these "placebo" difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimates of $\Delta^2_{realignment}$, $\Delta^2_{real.+prop\ 47}$, and $\Delta^2_{proposition\ 47}$. For example, if the cumulative empirical density function of the complete set of estimates of $\Delta^2_{proposition\ 47}$ is given by F(.) the p-value from a one-tailed test of the hypothesis that $\Delta^2_{proposition\ 47}$ >0 is given by 1-F($\Delta^2_{proposition\ 47}$).

Our principal synthetic cohort analysis uses state-level crime rate data for the period 2000 through 2016 tabulated by the FBI from agency-level data reported through the Uniform Crime Reports program. The main benefit of using the FBI tabulations of state-level crime rates rather than tabulating them directly from agency level data provided in the annual Offenses Known and Cleared by Arrests computer files concerns the handling of rape. On January 1, 2013, the FBI changed the official definition of rape towards a more inclusive definition that mechanically increased the reported rate for this particular crime. The FBI still collects information on the legacy definition in addition to crime totals using the new definition and reports crime rates by state using both measures. However, the data in the Offenses Known and Cleared by Arrests files are based on the legacy definition prior to adoption of the new definition by each agency and the new definition thereafter. The California Department of Justice officially adopted the new rape definition in 2014, though many police agencies throughout the state including large agencies such as the Los Angeles Police Department (LAPD) did not adopt the new definition until 2015. To avoid a mechanical increase in this crime in 2015 and 2016, we use the state level rates as tabulated by the FBI where rape is consistently measured with the legacy definition and the total violent crime rate (which includes rape as a component crime) is not impacted by the definitional change.

We also present a parallel series of synthetic control results where we tabulate California crime rates omitting crime reported by LAPD and the population covered by LAPD from crime and population

totals for the state. LAPD came under press scrutiny in 2014 for under-reporting aggravated assaults.[8] Aggravated assaults account for nearly 60 percent of all violent crimes and is the largest contributor to the violent crime index, followed by robbery (33 percent of the total). A subsequent audit of crime report narratives and arrest charges by the LAPD Office of the Inspector General revealed that between 2008 and 2014 aggravated assaults were underreported by between 30 and 39 percent in each year, with many aggravated assaults involving brandishing a weapon and domestic violence being incorrectly recorded as simple assault (a part II crime not included in official crime rate totals). To address this issue the LAPD created a data integrity unit in November 2014 (the exact month when proposition 47 went into effect) that closely monitors crime reporting, performs targeted audits, and conducts widespread training on crime recording. The data reveal a near 40 percent increase in reported aggravated assaults in Los Angeles between 2014 and 2016.[9] The LAPD has jurisdiction over roughly 10 percent of the state's population. Given the size of the area policed by this agency, the fact that the observed increase in aggravated assaults is likely due to changes in how aggravated assaults are being classified, and the compositional importance of assaults as a contributor to total violent crime, it is important to assess whether results are sensitivity to the inclusion of Los Angeles. While the data integrity unit appears to have concentrated their efforts on increasing the accuracy of aggravated assault totals, we adjust all other crimes as well in the event that the enhanced training and monitoring impact the degree of under-reporting of other part I offenses.

To estimate the alternative crime rates for California, we tabulate total crimes in the state using the Offenses Known and Cleared by Arrests files for the years 2000 through 2016 excluding crimes report by LAPD from the numerator and the population policed by LAPD from the denominator. Doing so creates the new issue of the change in the rape definition and the fact that these agency-level data do not include

[8] See Poston, Ben and Joel Rubin "LAPD Misclassified Nearly 1,200 Violent Crimes as Minor Offenses," *Los Angeles Times*, August 9, 2014.

[9] For 2010 through 2016, the number of aggravated assaults reported by LAPD are 9,344, 8,843, 8,329, 7,624, 9,836, 13,713, and 15,874, respectively.

totals for the legacy definition once an agency switches over. Hence, our synthetic cohort estimates using the "LA-adjusted" California time series omits a separate estimate for rape.[10]

B. Projecting a Counterfactual Based on Higher-Frequency California Data

Our second strategy involves a univariate analysis of the monthly crime rate time series for California. We focus on the 24 months preceding November 2014 and the first 24 post-proposition months inclusive of November 2014. Figures 3 and 4 reveal relatively stable prison and jail populations for the 24 months preceding November 2014, while Figure 7 shows relatively stable arrest rates. Moreover, the beginning of the period 24 months prior to November 2014 (November 2012) is one full year following the implementation of realignment. Hence, one would expect little effect of realignment on crime rates by that time and little impact of the earlier reform on crime trends during this specified pre-period.

We conduct a univariate analysis of violent and property crime rates overall and for the component part 1 offenses that comprise the aggregate crime indices. Define *t* as an index measuring month relative to November 2014 (-1 in October 2014, 0 in November 2014, 1 in December 2014, and so on). For each crime rate we estimate the following model,

(3)

$$Crime_t = \alpha_0 + \alpha_1 t + \alpha_2 t^2 + \beta_0 After_t + \beta_1 After_t.t + \beta_2 After_t.t^2 + \varepsilon_t,$$

where $After_t$ is a dummy variable indicating t>-1, ε_t is an error term, and α_0, α_1, α_2, β_0, β_1, and β_2 are parameters to be estimated. Equation (3) effectively fits a quadratic trend to the 24 pre-intervention months and separate quadratic trend to the 24 post intervention months with a discontinuous break at

[10] We do however present an estimate for overall violent crime which is inclusive of rape. As the California rape rate for some agencies in 2014 and most if not all agencies in 2015 and 2016 will be based on the new more inclusive definition, the estimate of proposition 47 on violent crime will be upwardly biased. This bias, however is likely to be negligible as rape accounts for only 6 percent of violent crime in California.

t=0. The equation is comparable to the model used in Buonanno and Raphael (2013) to test for a discontinuous effect of the Italian Collective Clemency on Italian crime rates. Equation (3) can be used to project the counterfactual crime rate based on the estimated pre-intervention quadratic trend and to then measure the difference between the crime predicted by the full equation and the counterfactual. Specifically, for any t>-1, the difference in the crime rate predicted by equation (3) and the counterfactual crime rate predicted by the pre-intervention trends is given by

(4)

$$Diff\ relative\ to\ cf_t = \beta_0 After_t + \beta_1 After_t.t + \beta_2 After_t.t^2 + \varepsilon_t,$$

with the difference in the first post-intervention month simply equal to the coefficient on the variable $After_t$. We use equation (4) to generate several alternative estimates of the annualized effect of proposition 47 on specific crime rates. First, following Buonanno and Raphael (2013), we simply use twelve times the estimate of the discontinuity at November 2014 as an annualized crime effect estimate. Second, we tabulated the difference in equation (4) for each of the first twelve post-intervention months and then sum these estimates. The first estimate is based on the most precise definition of the counterfactual yet may miss impacts of the proposition that occur beyond the first month. On the other hand, the second estimate will be overly sensitive to over-projection of what may be a temporary downward trend in crime during the pre-intervention period, to the extent that property crime levels in California in 2014 were outliers.

We apply the model in equation (3) to the individual part 1 offenses using monthly data tabulated by the California Department of Justice (DOJ). For the major part 1 offenses, again we present analysis with and without crimes reported by LAPD. We pull monthly crime totals from the Offenses Known and Cleared by Arrests file for LAPD and subtract them from the monthly crime totals provided by the California DOJ. We should also note that the monthly data provided by the California DOJ records rape

totals based on the definition in use at the time of reporting and does not report a consistent total for the legacy definition.

C. Testing Whether Cross-County Heterogeneity in the Proposition 47 Dose Predicts Cross-County Heterogeneity in Crime Trends

Two prior analyses of discrete changes in correctional policy tested for changes in aggregate incarceration rates as well as whether variation in the regional impact of the shock explains regional variation in crime rates. Specifically, in our prior work on realignment (Lofstrom and Raphael 2016a) we found that the statewide increase in auto theft coincided with relatively larger increases in auto theft in counties that experienced greater reductions in county-specific prison incarceration rates and no evidence of an impact for other crimes. Buonanno and Raphael (2013) found a discrete increase in Italian crime rates coinciding with a mass prisoner release with larger increases in Italian provinces experiencing larger reductions in local incarceration rates. In both instances, the magnitude of the effects implied by the cross-regional analysis matched the magnitude of the impacts implied by the change in aggregate crime trends.

Our final strategy exploits cross-county variation in the impact of proposition 47 on local incarceration, resentencing and reclassification activity, and arrest rates on local crime rates. Specifically, we calculate the change in average monthly crime rates by county and regress these changes on the change in local jail incarceration rates, the amount of resentencing and case reclassification per capita in the county, and changes in arrests rates for property and drug offenses. Monthly data on local jail populations comes from the Jail Profile Survey maintained by the California Board of State and Community Corrections. Data on resentencing and reclassification totals by counties comes from a survey administered to counties by the Judicial Council of California. Finally, we tabulated average changes in arrest activity by county using data from the California Monthly Arrest and Citation Register files.

D. The Relative Strengths and Weaknesses of these Three Approaches

Our three strategies are certainly imperfect and each individually carries particular weaknesses. The synthetic cohort analysis matches on the years 2000 through 2010. Given the size and scope of realignment reforms, the states that match California from these earlier time periods may no longer be an appropriate gauge of counterfactual crime paths. Our higher-frequency analysis based on monthly data may be overfitting pre-existing trends to an unusual year. The property crime rate in California recorded in 2014 is literally the lowest rate on record since 1969 and notably lower than the immediately preceding years. Hence, estimates based solely on pre-existing trends run the risk of over-projecting the counterfactual crime decline. The cross county analysis estimates the effects of the proposition based on heterogeneity across counties in the differential impact of the proposition on arrests and jail populations. Any general deterrence effect that impacts state crime levels overall washes out in the analysis.

Nonetheless, the relative strengths of these strategies complement one another. Though problematic, the synthetic cohort and time series strategy will capture statewide general deterrent effects that the cross-county analysis may miss. The clear differences in dose across county permit analysis of the proposition's effect that does not depend on a potentially problematic pre-proposition year. Moreover, whether the projected counterfactuals from the within state analysis reflect over-fitting to an outlier pre-intervention year can be verified by comparison to other states from the synthetic cohort estimator. Our strategy is to present estimation result from all three approaches and to interpret overlapping results that accord with one another as evidence of an effect of the proposition.

4. Results from the State-Level Synthetic Cohort Analysis

We begin with a graphical analysis of the total violent crime rate (the sum of the rates of murder, rape, robbery, and aggravated assault) and the total property crime rates (the sum of burglary, larceny, and motor vehicle theft). Figure 9 graphically displays the violent crime rate for California and for the synthetically matched comparison group for the period 2000 through 2016. Recall our synthetic cohort

match is based on pre-intervention values for the years 2000 through 2010. The figures on the left display violent crime rates when crime reported by LAPD are included in the total. The figures on the right display the time series when the LAPD contribution to state crime rates is omitted. In addition to the overall time series, we graphically depict the difference between the "treated state" and the synthetic comparison group for all 49 states used to generate the placebo estimates and for California (marked by the thicker dashed line) in the bottom two figures. Vertical lines denote the years 2011 (the year in which realignment goes into effect) and 2014 (the year when proposition 47 passes) in all of the figures. Figure 10 presents comparable graphs for property crime.

Beginning with the results for violent crime, there is little evidence of an increase in violent crime following the implementation of realignment, but an increase in violent crime rate in 2015 and 2016 following the implementation of proposition 47. However, the increase (both in terms of the level for California as well as the difference relative to the synthetic comparison group) disappears when the Los Angeles crime rate is excluded from the overall tabulation. In fact, the figure on the right suggests that violent crime in California falls slightly below the value for the comparison group post-proposition 47 when Los Angeles is omitted from the aggregate crime tabulation. The lower figures reveal that the annual differences in violent crime rates between California and synthetic California do not visibly widen post 2011 especially when LA is dropped from the analysis. Moreover, the plotted time series differences for California lie well within the distribution of placebo estimates.

Figure 10 reveals stronger evidence of a departure from trends for property crime. Moreover, the results are not sensitive to the inclusion of Los Angeles in the total property crime rate. California property crime rates increase relative to the synthetic comparison groups in 2012, remain above the trend for other states in 2013, and 2014, and widen again in 2015 and 2016. The later widening is due in part to a decline in property crime in the comparison states and in part due to an increase in property crime levels in 2015. The time series of the difference between California and its synthetic comparison group

visibly widens post 2011 and appears large relative to the distribution of placebo differentials. Interestingly, we do observe comparable proportionate declines of roughly eight percent in the property crime rate for both California and the synthetic comparison group between 2013 and 2014. In the years following however (between 2014 and 2016), property crime in the synthetic comparison group declines by only two percent. That is to say, the sharp decline between 2013 and 2014 does not continue in the comparison states.

Table 1 presents the average annual difference in violent crime rates between California and the synthetic comparison group for the pre-period 2009-2010, the post-realignment period 2012-2014, and the post-prop 47 period 2015-2016. In addition to presenting the results for violent crime overall, we also present results for the individual violent crime rates. We omit results for rape and overall violent crime in the bottom panels since rape is measured using the new definition for Los Angeles in 2015 and 2016 and the legacy definition in the FBI crime totals for the states. When Los Angeles is included we observe a slight increase in relative violent crime in the 2012-2014 period, and a larger increase in 2015-2016 period. This is driven primarily by a relative increase in aggravated assault, and smaller relative increases in robbery and rape. The murder rate in California declines relative to comparison states in both post periods. Again, dropping Los Angeles greatly narrows the increase in aggravated assault especially between the years 2012-2014 and 2015-2016.

Table 2 presents estimates of the difference-in-difference equations specified in (4) from the previous section for violent crime. The first column presents the estimate for the period 2012-2014 relative to 2009-2010 and is akin to the estimated impact of realignment. The second column presents estimates for the period 2015-2016 relative to 2009-2010 and amounts to our estimates of the cumulative effects of both reforms on crime. The final column presents the difference-in-difference estimates for the period 2015-2016 relative to 2012-2014. These are our principal estimates of the effect of proposition 47 on violent crime from the synthetic cohort analysis. For each estimate, we also display California's rank

in the distribution of estimates for all states (California plus the estimates for the remaining 49 placebo estimates). We also present the proportion of states with a higher value than that for California (in essence, the p-value for a one-tailed test of whether California's violent crime rate increases in the post-proposition period). When Los Angles is included, we observe a relative increase in overall violent crime in the post-proposition period driven primarily by the increase in aggravated assault. While the p-values do not fall below 0.10, California's relative increase in violent crime is in the right tail of the distribution of placebo estimates. Interestingly, we observe a statistically significant relative decline in the murder rate for California post-proposition 47 when Los Angeles is included, with the probability of experiencing a decline larger than California's equal to 0.06 (which our method would indicate is a statistically significant relative decline in homicide).

Omitting Los Angeles from the overall violent crime rate tabulation eliminates any evidence of an effect of the proposition on violent crime. The relative changes in violent crime in 2015-2016 are negative for murder, and robbery, and relatively small for aggravated assault. All of the estimates lie either close to the placebo estimate for the median state or well within the left tail of the placebo distribution (for murder in particular). Our reading of the results in Figure 9 and Tables 1 and 2 are that proposition 47 had no measurable adverse impact on violent crime rates in the state.

Tables 3 and 4 present a comparable analysis for overall property crime and the individual components that constitute property crime. Beginning with the results for overall property crime in Table 3, we observe an increase in the difference between California and synthetic California in 2012-2014, and a further increase in 2015-2016. Interestingly, this is driven mostly by declining crime in the synthetic comparison states. Specifically, between 2009-2010 and 2012-2014, average annual property crime rates in California decline by 64 incidents per 100,000. The comparable decline in synthetic comparison states is 234 per 100,000. Between 2012-2014 and 2015-2016, average annual property crime in California decline by 34 per 100,000. The comparable decline in the comparison states is 194 per 100,000. In terms

of the specific crime categories, we observe a sizable increase in the relative rate of auto theft in the post-realignment period but no further widening of this differential in 2015-2016. The relative larceny theft rate however, increases in 2015-2016. The exclusion of Los Angeles does not impact the basic patterns in Table 3.

Table 4 presents our difference-in-difference estimates for property crime along with our test for statistical significance using the placebo distributions of estimates for the other 49 states. The structure of the table is comparable to that of table 2 for violent crime. Again our principal estimates of the effect of proposition 47 on property crime are in the final column of the table. For property crime overall, we observe a statistically significant increase in property crime in the post proposition 47 period of roughly 194 incidents per 100,000 in the estimates excluding Los Angeles. Relative to property crime rates for the state during the period 2012-2014, this represent a 7.5 percent increase. Most of this effect (83 percent) is driven by the decline in average crime rates in the comparison states rather than an actual increase in property crime in California. When Los Angeles is omitted from the analysis, relative property crime rates increase by 134 incidents per 100,000. Here, however, California ranks 41/50 and thus the increase is no longer statistically significant. Again, the lion's share of this effect is generated by the decline in crime rates in comparison states. At face value, an increase of 134 incidents per 100,000 represents a five percent increase in property crime relative to property crime levels during the period 2012-2014. Regardless of whether Los Angeles is included in the analysis, the post-proposition 47 effect is driven compositionally by increases in larceny theft relative to comparison states.

To summarize the results of this section, the synthetic comparison analysis of state level data yields little evidence of an impact of proposition 47 on violent crime. The results for violent crime are particularly sensitive to the inclusion of crimes reported by LAPD, where we know a priori that changes in reporting have mechanically increased the rate of aggravated assault. Even when Los Angeles is included in the tabulation of crime rates, we fail to find statistically significant increase in violent crime.

For property crime, however, we observe relative increases in California in the post-proposition 47 period. The magnitude of the estimates suggest increases in property crime ranging from five to 7.5 percent of pre-proposition 47 levels, with the effect driven mostly by a relative increase in larceny. The relative increase in property crime actually reflects declines in average annual property crime in California that are considerably smaller than the coinciding decline in comparison state. Hence, most of the five to 7.5 percent relative increase reflects crime trends in the comparison states rather than absolute increase in California.

5. Results from the Within-State Trends Analysis

Figure 11 graphically depicts the estimation procedure we deploy to use within-state time trends to project counterfactual crime rates. Specifically, for 24 pre-47 and 24 post-47 months, we fit a simple regression model of crime rates on time measured relative to November 2014, time squared, an indicator variable for time>-1, and interactions between the quadratic function and the indicator variable (equation (3) above). The models allow for first-order serial correlation in the residuals.[11] Figure 11 plots the actual monthly crime rate against time (denoted with dots), the fitted values from the interacted quadratic function in time, and the projected counterfactual values for the post-period (the predicted value less the post-period differential given by equation (4) above). Our estimates of the effect of the proposition are based on either twelve times the discontinuous break in the crime rate time series in November 2011 or the difference between the predicted value and the counterfactual value over the first post-proposition 47 year. While we are also able to generate estimates for the second post-proposition 47 year, these estimates are quite imprecise (with standard errors larger than the point estimates in each case) and thus we focus on various estimates of the annualized crime effect over the first year.

[11] Model estimates using OLS with Newey-West standard errors specifying one, two or three lags yield similar results.

For violent crime, the predicted and counterfactual values are visibly similar regardless of whether crime reported by LAPD is included in the state monthly crime totals. For property crime however, there is a visible difference between the projected counterfactual and predicted crime rate, with the pre-intervention trends predicting substantial subsequent declines in crime in 2015 and 2016. In fact, the difference between the two series increases with time. The average monthly counterfactual crime rate declines by approximately 6 percent when comparing the first twelve post-proposition months to the 12 preceding months and declines by another two percent between the first and second post-proposition years. Similar to the results for violent crime, these estimates are not sensitive to the exclusion of offenses reported by LAPD.

Table 5 presents annualized estimates for total violent crime, total property crime, and for each part 1 offense making up the violent and property crime indices. For each crime rate we present estimates including and excluding crime reported by LAPD in the aggregate crime series. Within each of these groups we first present annualized estimated based on the discontinuity and then an annual estimate based on the sum of the estimated treatment effects over the first post-proposition year. The patterns roughly conform to the findings from the state synthetic cohort analysis, with a few key differences. First, while we find no significant effect on violent crime overall in three of the four specifications, we do find a significant coefficient based on the predicted discontinuity when we omit crime reported by LAPD. For all four specifications we find statistically significant increases in robbery amounting to roughly 10 percent of base levels in 2012 through 2014. We should note that the robbery rate in 2014 was particularly low and the decline in robbery from 2013 to 2014 unusually large both relative to past changes for California as well as relative to the changes observed for synthetic comparison matches discussed in the previous

section.[12] To the extent that our method is over-projecting the counterfactual decline based on an unusual year, this estimate may be unreliable.

Regarding property crime, we again find significant effects for property crime overall as well as for larceny. Here, however, we also find a significant effect on motor vehicle theft in three of the four model estimates. These motor vehicle theft results contrast sharply with the results from the synthetic comparison group analysis, and thus we should identify the source of the contrast in results. Our time series projections generate a counterfactual decrease in motor vehicle theft between the last pre-proposition year and the first post-proposition year of 7.4 percent and a further decrease of 2.9 percent between the first and second post years. Moreover, the predicted value from the full model projects an 8.5 percent increase in motor vehicle theft which, when combined with the decline in the counterfactual crime rate, generates the statistically significant relative increases in motor vehicle theft presented in the table. The difference here relative to the synthetic comparison analysis is due entirely to the behavior of the counterfactual crime rate from these two estimators. For our synthetic comparison states for California, motor vehicle theft increases by 9 percent between 2014 and 2015 (an increase larger than the 8.5 percent predicted absolute increase for this period generated by our full time series model), and increases by another 10 percent between 2015 and 2016. Hence, the difference in results is due in its entirety to the different counterfactual predictions generated by the two estimators.

More generally, when we estimate based on the discontinuity the magnitudes better align with the results from the synthetic cohort analysis. For example, the estimated effects on overall property crime from the synthetic cohort analysis was an increase in property crimes per 100,000 of between 134 to 193 incidents. The results based on the discontinuous change in property crime are increases of 152

[12] The robbery rate increased by 3 percent from 2011 to 2012, declined by 5.8 percent from, 2012 to 2013, declined by 10.2 percent from 2013 to 2014, and then increased by 7.6 percent from 2014 to 2015. For comparison, the robbery rate in our synthetic comparison group inclusive of LA declined by half a percent between 2011 and 2012, declined by 2.3 percent between 2012 and 2013, declined by 3.7 percent between 2013 and 2014, declined by 0.6 percent between 2014 and 2015, and the increased by 1.7 percent between 2015 and 2016.

to 155, lying within the range of these estimates. Similarly, the synthetic cohort analysis yielded estimated increases in the larceny theft rate of between 124 and136 incidents per 100,000. The annualized estimates based on the discontinuous change are comparable (roughly 129 whether or not LAPD crimes are included). In contrast, when we estimate the effect based on the sum of the first twelve treatment effects, the effect size grow considerably, with the overall property crime effect 72 percent larger and the overall larceny effect roughly 50 percent larger. This disparity is driven by the fact that the pre-existing trends predict continuous declines in crime over the subsequent two years largely due to the very low property crime rate in 2014.

6. Cross-County Analysis

Thus far we have tested for breaks in trends and deviation of California's crime rate from those of comparison states. While the results from these two exercises do not entirely line up, we find consistent evidence of an impact on property crime, larceny theft in particular, on the order of 5 to 7 percent relative to pre-proposition 47 levels. In this section, we exploit a different source of variation. Specifically, we assess whether crime rates increased by more in counties that experienced larger proportional declines in their average daily jail population, larger declines in arrest rates for drug and property offenses, and larger volumes of resentencing and reclassification petitions. In our prior research on realignment (Lofstrom and Raphael 2016), we found comparable results from analysis that exploited cross-county variation in the policy dose and a state-level synthetic cohort analysis. Similarly, Buonanno and Raphael (2013) found larger increases in crime in Italian provinces that received more reentering inmates per capita as a result of the Collective Clemency. Moreover, the magnitude of the effect implied by the cross-province relationship was similar to the estimates based on the discontinuous breaks in crime trends. Here, we assess whether the cross-county relationship accords with the findings thus far.

Figure 12 presents scatter plots of the pre-post change in the average monthly crime rate for the 52 month period surrounding the implementation of proposition 47 against county-level changes in the jail incarceration rate and the quantity per 100,000 of resentencing and reclassification petitions filed between November 2014 and June 2017.[13] Separate scatter plots are presented for overall violent crime and overall property crime. There is quite a bit of variance across counties in the change in jail incarceration rates. The mean change is a decline of 15 per 100,000 with an inter-quartile range of approximately 28 per 100,000. This is relative to an unweighted pre-intervention average of 239 per 100,000. The figure also reveals substantial variation across counties in the volume per capita of resentencing and reclassification petitions, a factor likely correlated with prison releases to the county and the extent to which the county aggressively prosecuted proposition 47 crimes in the past.[14] There is little evidence in the scatter plots that counties that experienced larger declines in the jail population or larger quantities of resentencing and reclassification proceedings experience larger increases in crime for either violent or property crime.

Figure 13 presents comparable scatter plots of changes in county-level average crime rates against change in combined property and drug arrests per 100,000.[15] Again we observe substantial cross-county heterogeneity in the change in arrest rates. The unweighted average decline in combined property and drug arrests per 100,000 is 16 per 100,000 relative to a pre-intervention level of 99. The standard

[13] The period for resentencing and reclassification petitions is based on survey results of county courts conducted by the California Judicial Council. According to Judicial Council tabs, most of the petitions are filed within the first year and half of November 2014. While several counties report separate totals for resentencing and reclassification petitions, a handful of key California counties (inclusive of San Diego and Alameda) only report the total. Hence, we do not break up petitions by type. The scatter plots in Figures 12 and 13 are based on 56 of California's 58 counties. We omit Alpine and Sierra counties as these two counties do not operate independent county jail systems for the entire period that we analyze (Alpine does not at all and Sierra for only the beginning of the period).

[14] We have requested high-frequency admissions and releases data from the California Department of Corrections and Rehabilitation (CDCR). In future work, we will add the change in prison releases by county caused by proposition 47 to the list of covariates analyzed.

[15] Separate analysis for drug and property arrests yields similar results. Here we calculate the change in average arrest rates using the 26 pre-proposition months and 14 post-proposition months. This difference is due to the fact that our arrest data ends in 2015.

deviation or the change is 15.4. There is no evidence in the scatter plots that counties that experienced greater declines in arrest experienced relative increase in either property crime rates of violent crime rates.

Table 7 presents the results from county-level regressions of changes in monthly crime rates on changes in the jail average daily population per 100,000, the number of resentencing/reclassification petitions per 100,000, and the change in drug and property arrests per 100,000. Each row corresponds to a separate regression model. For each crime we present separate estimates for unweighted models and regression models that are weighted by county-level population. We interpret a significant negative coefficient on the change in jail incarceration rates, a significant positive coefficient on petition rates, and a significant negative coefficient on the change in arrest rates as evidence of an adverse effect of the proposition 47 shock to these variables on crime rates. The models in table 7 are based on changes for the 24 months preceding and following the passage of proposition 47. In appendix table A3 we present comparable model estimates where the changes are calculated using the 12 months preceding and 12 months following the passage of the proposition.

Beginning with the results for violent crime rates, we find no evidence of a relative increase in violent crime overall or on any of the individual violent crime rates of prop-47 induced changes in jail incarceration, resentencing/reclassification petitions, or changes in arrests activity. All of the coefficient estimates on the change in the jail population are all small and statistically insignificant and often the wrong sign. We do see significant negative coefficients in several models on the number of resentencing/reclassification petitions per 100,000 residents. However, the estimates suggest that crime fell by more in counties with more petition activity, suggestive of a crime-abating effect of the proposition. Regarding the estimates for change in arrest activity on violent crime, there is one significant negative coefficient on robbery when the model is weighted by county populations. Statewide the arrest rate declined by roughly 16 per 100,000. Taking the one significant coefficient estimate for robbery at face

value (-0.03) suggests an annualized effect of the decline in arrest activity of 5.76 incidents per 100,000 (0.03x16x12). When we estimate these models using the year-over-year changes in crime to construct the dependent variable for the regression rather than the average change over two pre and two post years (presented in table A3), again we find little evidence of any effects on violent crime. The negative effect of the change in arrests on robbery does not appear in these models.

Turning to the results for property crime, there are notable differences between the models that weight the regressions by county population and those that do not. None of the unweighted models yield evidence of a proposition 47 effect on property crime overall or the individual property crime rates. In the weighted models however, we find a nearly significant negative coefficient on the change in jail incarceration rates for property crime overall (with a p-value of 0.105) and marginally significant coefficients on the jail incarceration rate for larceny and motor vehicle theft. The coefficient estimate for property crime overall in the weighted model is consistent with an annualized effect of 71.6 additional property crimes per 100,000 (calculated by the multiplying the coefficient estimate (0.284), by thest statewide decline in the jail incarceration rate (21), by the number of months in a year). The comparable implied annualized estimates for larceny theft and motor vehicle theft are 48.9 and 25.7, respectively. These estimates are considerably smaller than the results from the within-state time series analysis as well as the results from the synthetic cohort analysis. These estimates suggest a 2.8 percent increase in property crime rates overall, a 3.1 percent increase in larceny theft and a 7 percent increase in auto theft. We find no evidence of an effect of resentencing/reclassification petitions activity nor of the change in arrest rates in any of the models.

The results for property crime using year-over-year changes presented in appendix Table A3 are roughly consistent with the findings in Table 7, though the point estimates on the change in the jail incarceration rate imply much smaller effects on property crime overall and larceny theft and little effect

on motor vehicle theft. None of the coefficient on the change in the jail incarceration rate are statistically significant in these models.

To summarize the results in this section, we find very little evidence that cross-county variation in the effect of proposition 47 on jail populations, resentencing and reclassification petitions, and on arrest activity predicts inter-county variation in the pre-post 47 change in violent crime rates in a manner consistent with an adverse effect of the proposition. There is some evidence of an impact on property crime overall and on larceny and motor vehicle theft, though the estimates are sensitive to whether the models are weighted by population and by the time periods used to calculate the changes in crime and jail incarceration rates. Moreover, the largest estimates from this analysis imply property crime effects that are considerably smaller than those implied by the synthetic cohort analysis and the within-state time series results.

7. Discussion

To summarize the results of our empirical analysis, we find little evidence that the changes in correctional populations, arrests, and convictions reclassifications ushered in by California's proposition 47 impacted violent crime rates in the state. Once changes in offense definitions and reporting practices in key agencies are accounted for, violent crime in California are roughly at pre-proposition levels and generally lower than the levels that existed in 2010 prior to major reforms to the state's criminal justice system. While our analysis of violent crime rates yields a few significant point estimates (a decrease in murder for one method and an increase in robbery for another), these findings are highly sensitivity to the method used to generate a counterfactual comparison path.

We find more consistent evidence of an impact on property crime, operating primarily through an effect on larceny theft. The magnitudes of the property crime effect range from an increase of zero to three percent when we analyze cross-county crime patterns, an increase of 5 to 7.5 percent for our

synthetic cohort analysis, and an increase of 6 to 10 percent increase for our within-state time series analysis. The estimates are sensitivity to the manner used to generate the counterfactual, with more than half of the relative increase in property crime (and for some estimate considerably more) driven by a decline in the counterfactual crime rate rather than increases for California. Despite this sensitivity, there is evidence from all methods that larceny theft increase with, a ballpark summary of 5 percent roughly consistent with the totality of our analysis.

We are able to say very little about specific mechanisms that may be driving these results. Our cross-county analysis directly tests whether change in incarceration (imperfectly proxied by change in the jail incarceration rate and the rate at which resentencing and reclassification petitions are filed) or change in enforcement (measured by changes in arrest activity) increase crime rates. We find relatively weak evidence that these factors impacted the cross-county changes in crime rates. In the models for property crime where we do see an impact, only changes in jail incarceration have the theoretically predicted effect. Of course, this conclusion should be tempered by the fact that in many models we see that counties with higher reclassification and resentencing petitions per capita experienced significantly larger decrease in crime.

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Figure 1: Long-term Trends for California and U.S. Prison Incarceration Rates

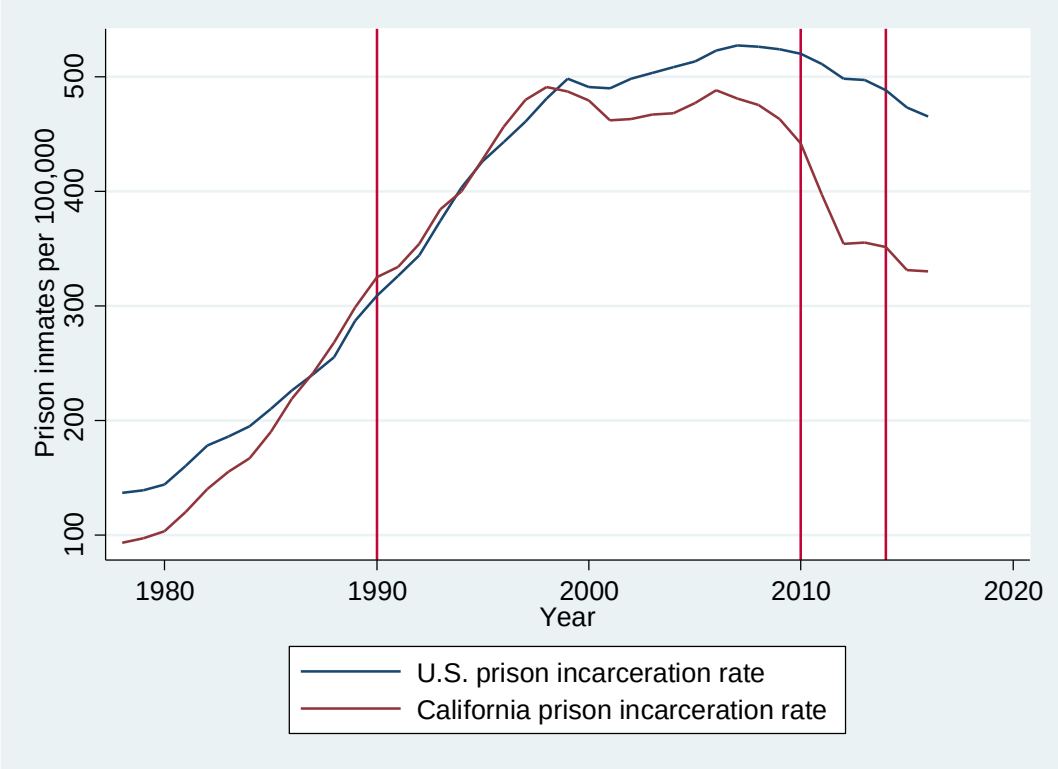


Figure 2: Long-term Trends for California Violent and Property Crime Rates

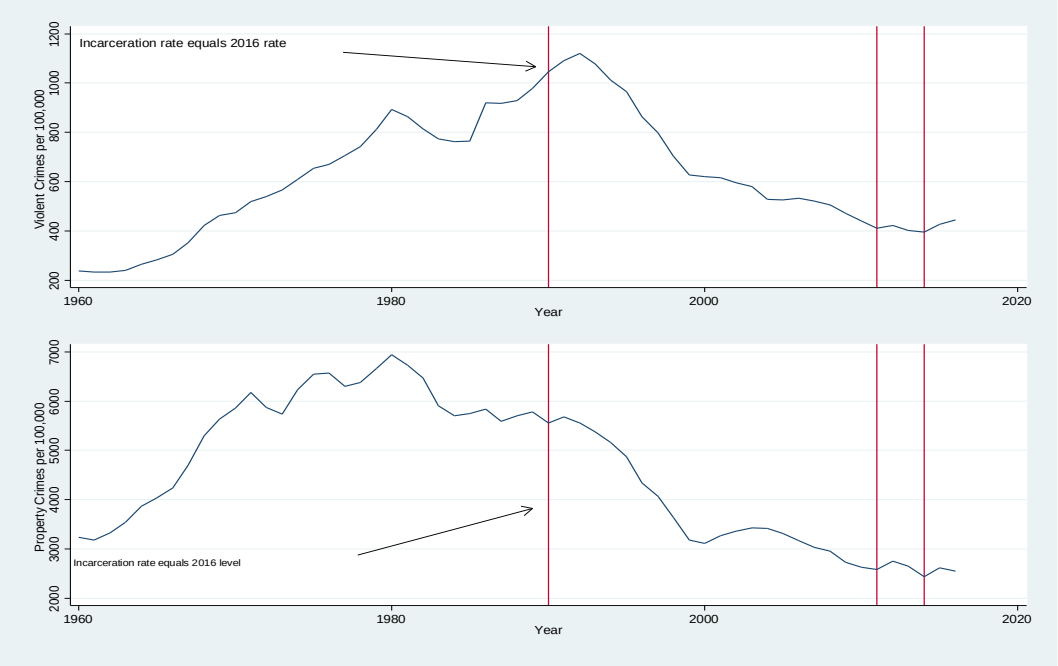


Figure 3: Weekly Prison Population Trends Surrounding the Implementation of Realignment and Proposition 47

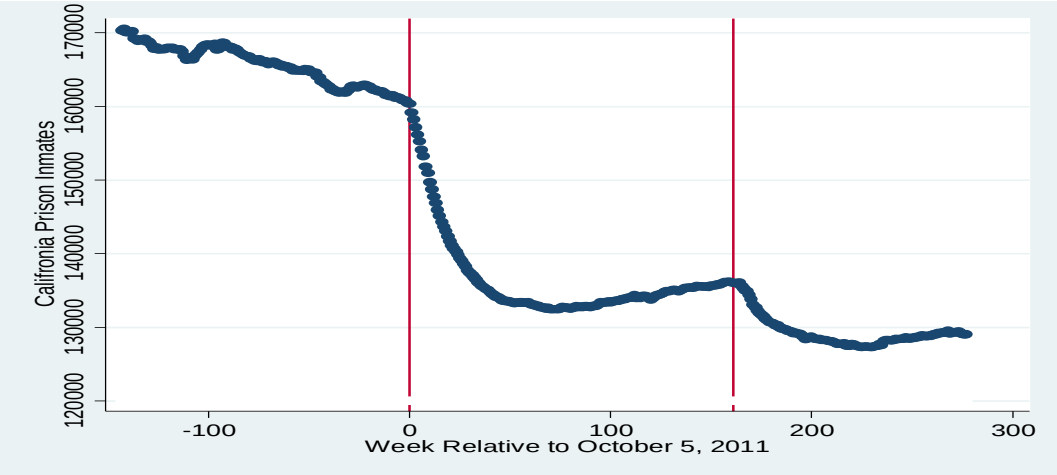


Figure 4: Pre-Post Proposition 47 Trends in Monthly California Jail and Prison Populations

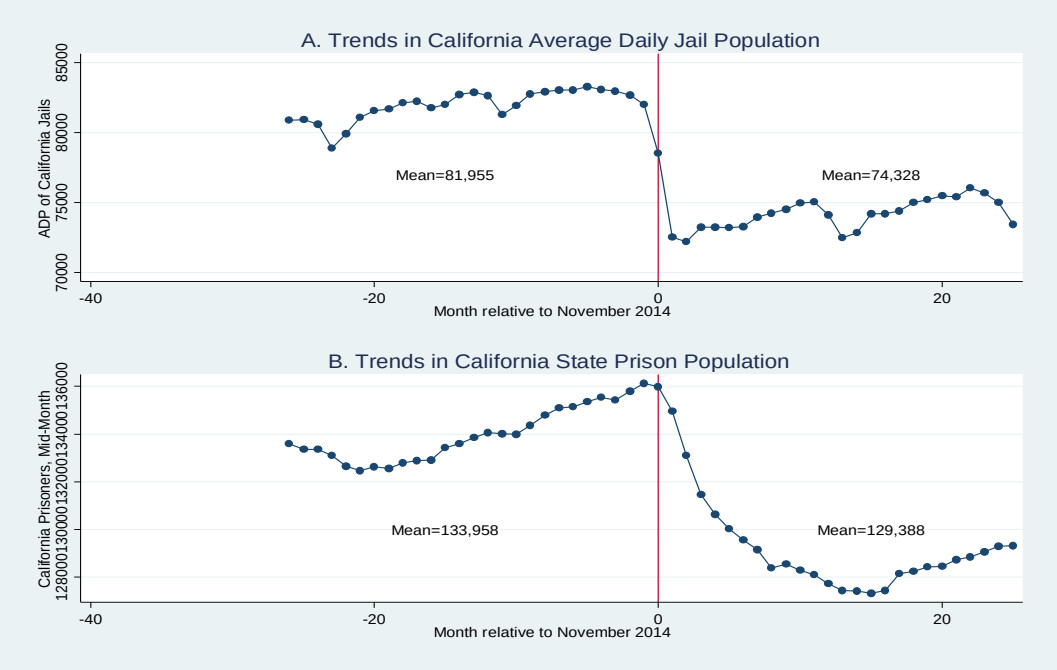


Figure 5: Pre-Post Proposition 47 Trends in Monthly Jail Bookings and Releases due to Capacity Constraints.

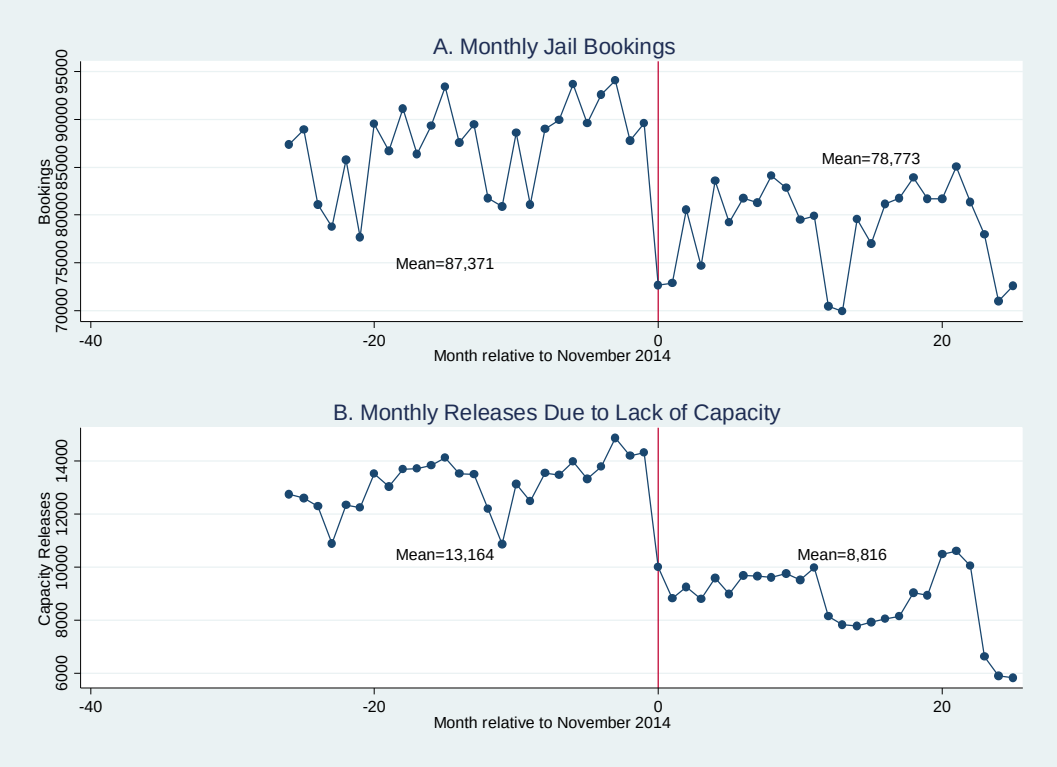


Figure 6: Pre-Post Proposition 47 Trends in Monthly Prison Admissions and Releases

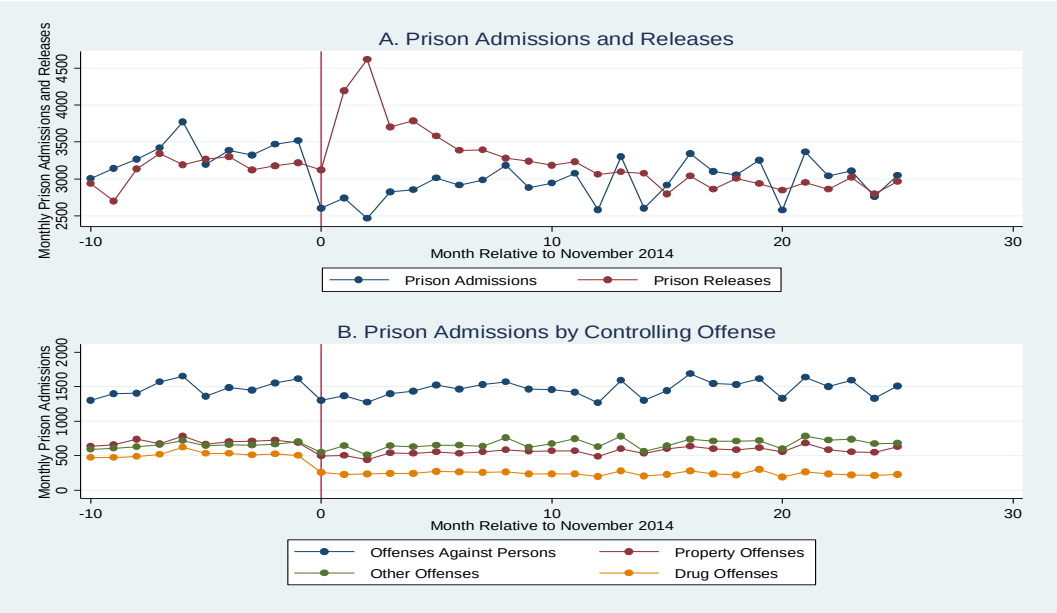
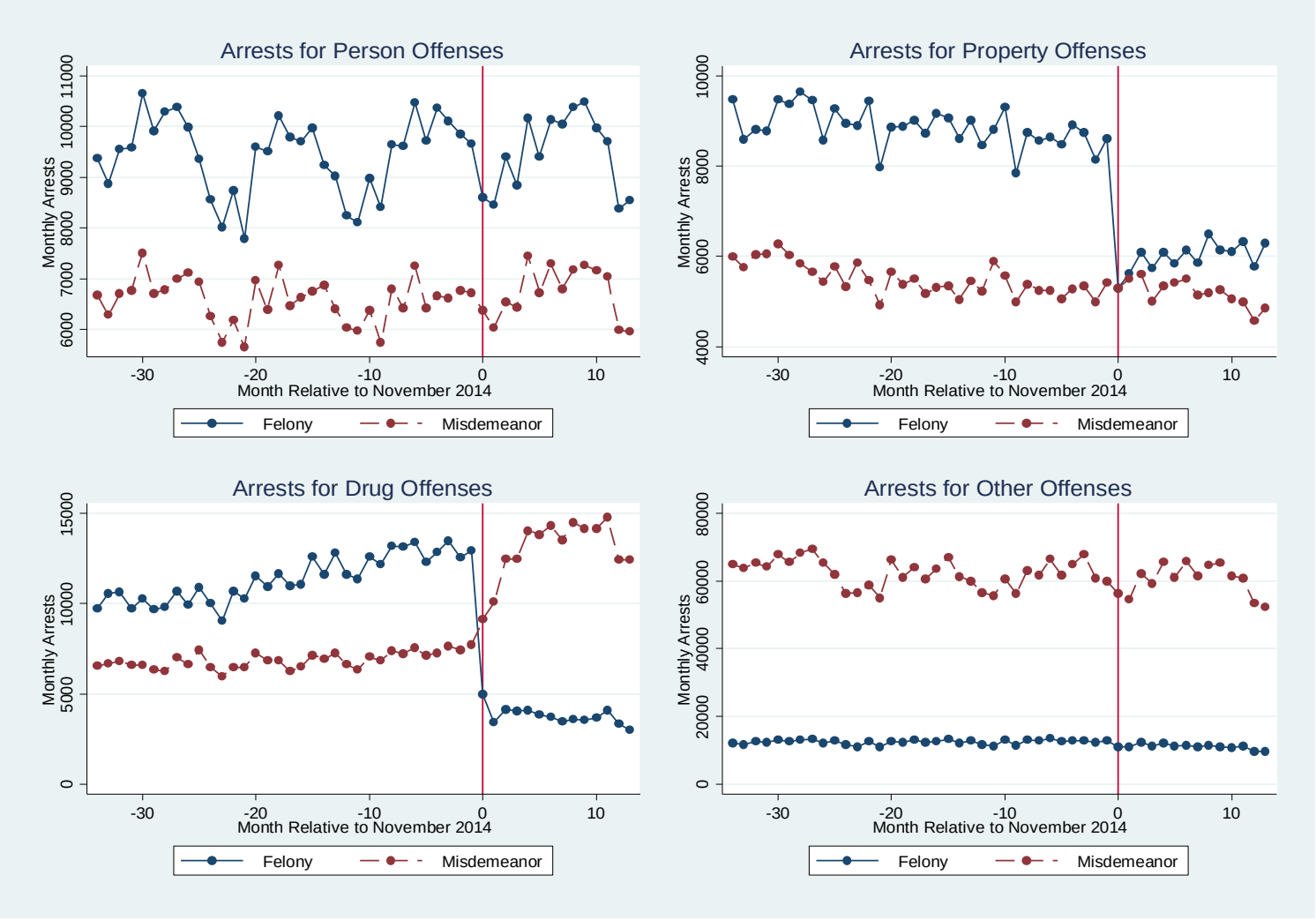
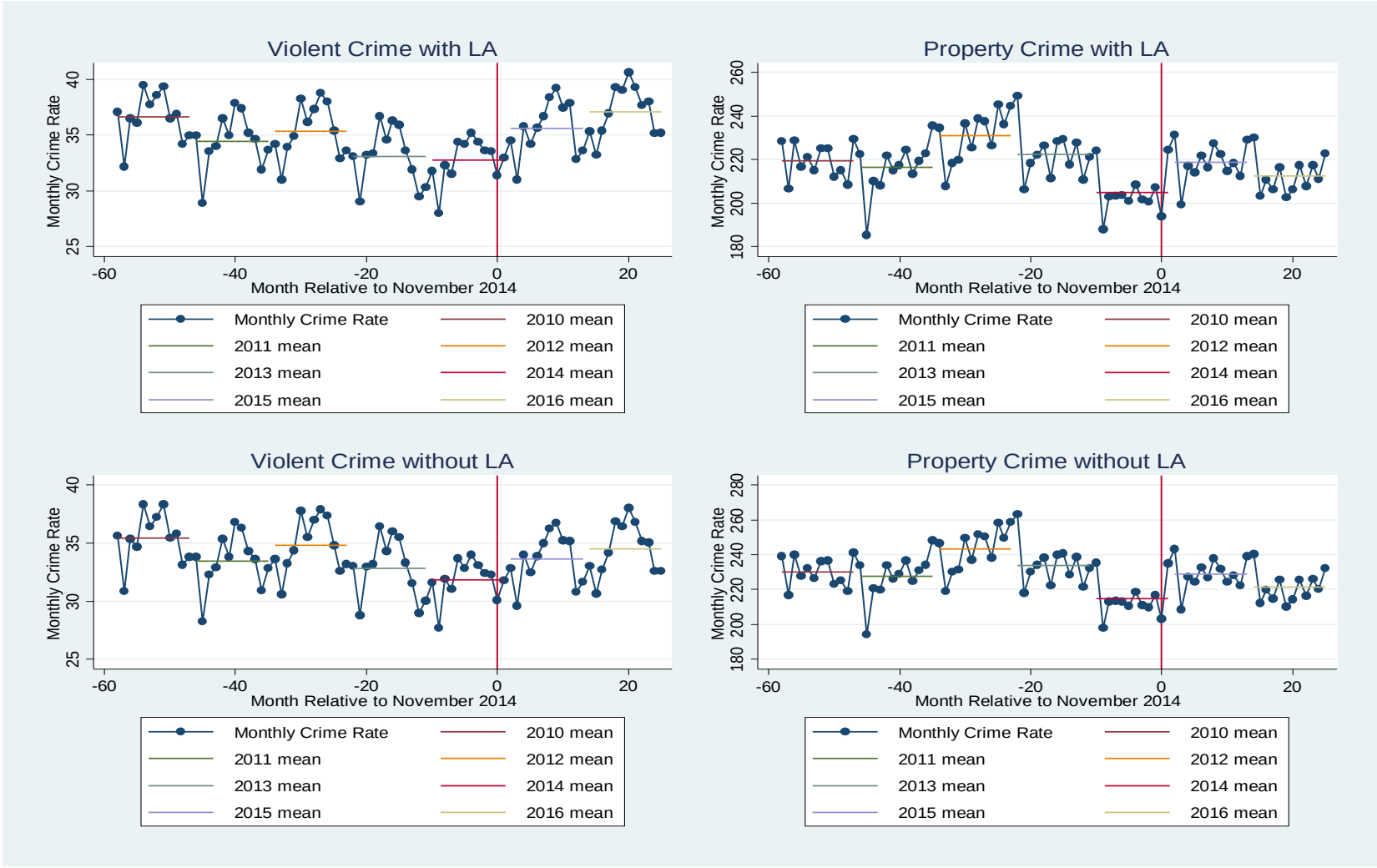


Figure 7: Pre and Post Proposition 47 Trends in Monthly Arrest by Offense Type



Monthly Violent and Property Crimes Per 100,000 With and Without Crimes Reported by LAPD in the Total, January 2010 through December 2016



Violent Crime Rates in California and Synthetic California as well as Placebo Time Series of Treatment-Control Differences for Two Alternative California Crime Time Series (With and Without LA in the Tabulation of California Violent Crime Rates)

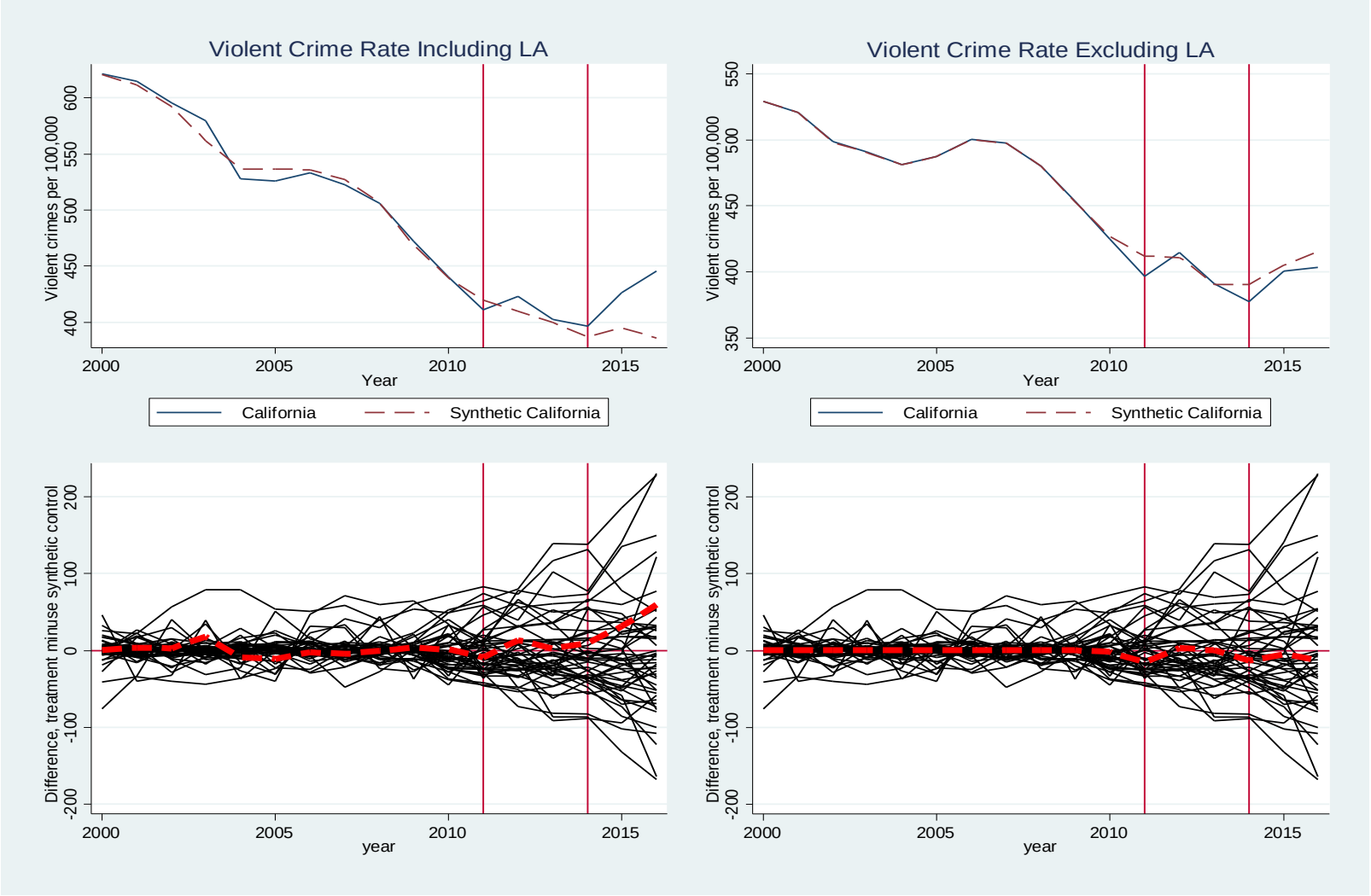


Figure 10: Property Crime Rates in California and Synthetic California as well as Placebo Time Series of Treatment-Control Differences for Two Alternative California Crime Time Series (With and Without LA in the Tabulation of California Property Crime Rates)

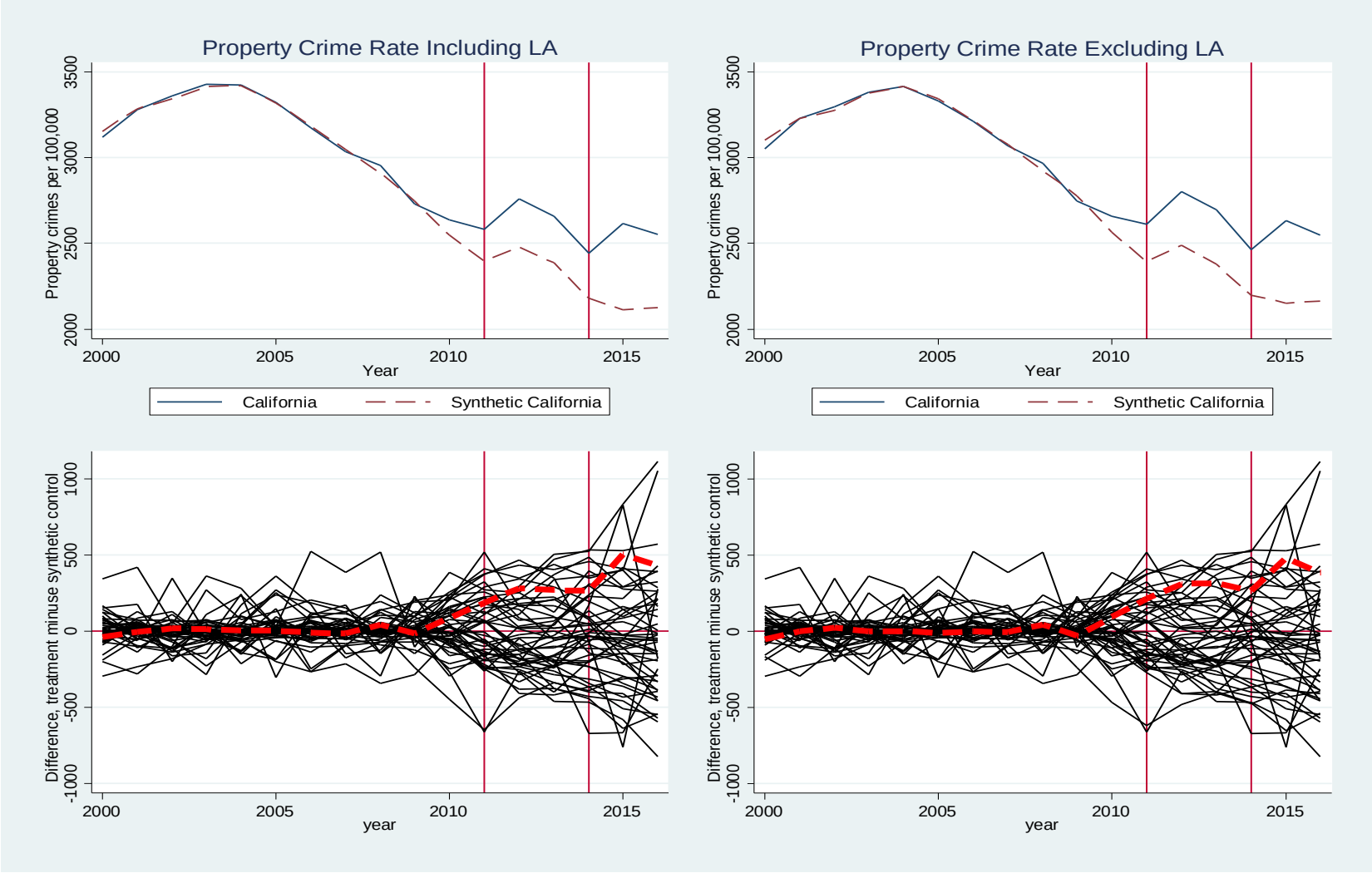


Figure 11: Actual and Projected Monthly Violent and Property Crime Rates for the two Pre-47 Years and two Post-47 Year, Rates Tabulated With and Without Los Angeles County

Figure 12: Scatter Plots of County-Level Pre-Post 47 Changes in Average Monthly Crime Rates Against Change in Average Monthly Jail ADP per 100,000 and the Number of Proposition 47 Resentencing/Reclassifications Activity per 100,000

Figure 13: Scatter Plots of County-Level Pre-Post 47 Changes in Average Monthly Crime Rates Against Change in Average Monthly Property and Drug Crime Arrests per 100,000

Table 1
Different in Violent Crime Rates Between California and Synthetic California for the Pre-Intervention Period and two Post-Intervention Periods

	Difference, CA-Synthetic CA 2009-2010	Difference, CA-Synthetic CA 2012-2014	Difference, CA-Synthetic CA 2015-2015
With LA			
Total Violent Crime	2.40	8.37	45.45
Murder	-0.17	-0.15	-1.13
Rape	-0.09	-0.87	1.66
Robbery	2.55	1.19	4.64
Aggravated Assault	-12.65	-6.59	18.46
Without LA			
Total Violent Crime	-0.73	-3.10	-8.38
Murder	-0.16	-0.26	-0.98
Rape[a]	-	-	-
Robbery	1.58	4.61	4.09
Aggravated Assault	-0.14	7.04	9.88

The figures in the table are the average of the difference in annual crime rates between California and the synthetic comparison group for the noted time period. Actual values for the violent crime rate levels are provided in appendix Tables A1.

a. We omit tabulations for rape without Los Angeles due to the fact that the FBI UCR agency level data do not include rape tabulation for recent years using the legacy definition. Our state level panel data set produced by the FBI uses the legacy definition for rape through 2016. Tabulating the rate of rape per 100,000 for California from agency level data yields a mechanical increase in rape in 2015 and 2016 due to the adoption of the new rape definition in several large agencies in California.

Table 2
Difference-in-Difference Estimates of the Effects of the Estimates of Sentencing Reforms on Violent Crime Rates Along with Statistical Inference from the Distribution of Placebo Estimates

	Diff-in-diff, 2012-2014 minus 2009-2010		Diff-in-diff, 2015-2016 mins 2009-2010		Diff-in-diff, 2015-2016 minus 2012-2014	
	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)
With LA						
Violent	5.97	34/50 (0.32)	43.05	44/50 (0.12)	37.09	44/50 (0.12)
Murder	0.02	30/50 (0.40)	-0.96	10/50 (0.80)	-0.98	4/50 (0.92)
Rape	-0.78	23/50 (0.54)	1.74	36/50 (0.28)	2.53	43/50 (0.14)
Robbery	-1.35	25/50 (0.50)	2.08	31/50 (0.38)	3.44	32/50 (0.36)
Assault	6.07	37/50 (0.26)	31.11	44/50 (0.12)	25.05	42/50 (0.16)
Without LA						
Violent	-2.37	33/50 (0.34)	-7.66	26/50 (0.48)	-5.92	23/50 (0.54)
Murder	-0.10	27/50 (0.46)	-0.82	10/50 (0.80)	-0.72	8/50 (0.84)
Rape[a]	-	-	-	-	-	-
Robbery	3.02	33/50 (0.34)	2.50	32/50 (0.36)	-0.51	27/50 (0.46)
Assault	7.17	37/50 (0.26)	10.02	34/50 (0.32)	2.84	33/50 (0.34)

Δ^2 statistics present the difference-in-difference between California and synthetic California in the given crime rate for the two noted time periods. The rank indicates where California's estimate sits within the distribution of placebo estimates for all 50 states. The probability value estimate provides the empirical probability that a placebo difference-in-difference crime rate effect exceeds the estimate for California. We interpret this figure as the p-value from a one-tailed test of the significance of the California crime effect.

a. We omit tabulations for rape without Los Angeles due to the fact that the FBI UCR agency level data do not include rape tabulation for recent years using the legacy definition. Our state level panel data set produced by the FBI uses the legacy definition for rape through 2016. Tabulating the rate of rape per 100,000 for California from agency level data yields a mechanical increase in rape in 2015 and 2016 due to the adoption of the new rape definition in several large agencies in California.

Table 3
Difference in Property Crime Rates Between California and Synthetic California for the Pre-Intervention Period and two Post-Intervention Periods

	Difference, CA-Synthetic CA 2009-2010	Difference, CA-Synthetic CA 2012-2014	Difference, CA-Synthetic CA 2015-2015
With LA			
Total Property Crime	37.02	270.83	464.68
Burglary	2.16	41.91	42.65
Larceny	12.89	20.75	157.51
Motor Vehicle Theft	76.88	141.32	128.33
Without LA			
Total Property Crime	31.68	297.13	431.27
Burglary	11.47	63.12	58.46
Larceny	15.33	43.51	168.48
Motor Vehicle Theft	69.51	129.17	110.79

The figures in the table are the average of the difference in annual crime rates between California and the synthetic comparison group for the noted time period. Actual values for the violent crime rate levels are provided in appendix Tables A2.

Table 4
Difference-in-Difference Estimates of the Effects of the Estimates of Sentencing Reforms on Property Crime Rates Along with Statistical Inference from the Distribution of Placebo Estimates

	Diff-in-diff, 2012-2014 minus 2009-2010		Diff-in-diff, 2015-2016 mins 2009-2010		Diff-in-diff, 2015-2016 minus 2012-2014	
	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)	Δ^2	Rank ($P[\Delta^2 > \Delta^2_{CA}]$)
With LA						
Property	233.79	46/50 (0.08)	427.63	46/50 (0.08)	193.85	42/50 (0.16)
Burglary	39.75	37/50 (0.26)	40.49	36/50 (0.28)	0.74	28/50 (0.44)
Larceny	7.86	29/50 (0.42)	144.62	38/50 (0.24)	136.76	45/50 (0.10)
MVT	64.43	49/50 (0.02)	51.45	40/50 (0.20)	-12.98	20/50 (0.60)
Without LA						
Property	256.46	47/50 (0.06)	399.60	45/50 (0.10)	134.13	41/50 (0.18)
Burglary	51.65	42/50 (0.16)	46.98	36/50 (0.28)	-4.67	26/50 (0.48)
Larceny	28.18	33/50 (0.34)	153.16	38/50 (0.24)	124.98	43/50 (0.14)
MVT	59.76	48/50 (0.04)	41.28	38/50 (0.24)	-18.48	18/50 (0.64)

Δ^2 statistics present the difference-in-difference between California and synthetic California in the given crime rate for the two noted time periods. The rank indicates where California's estimate sits within the distribution of placebo estimates for all 50 states. The probability value estimate provides the empirical probability that a placebo difference-in-difference crime rate effect exceeds the estimate for California. We interpret this figure as the p-value from a one-tailed test of the significance of the California crime effect.

Table 5
Estimates of the Effect of Proposition 47 on Crime Rates Based on the Discontinuous Change in Crime as well as the Difference Between the Model Predicted Value and the Counterfactual Values Over the First Post-47 Year

	Including LAPD Crime Reports		Excluding LAPD Crime Reports	
	Annualized estimates based on discontinuous break at t=0	Difference between prediction and counterfactual over first twelve months	Annualized estimates based on discontinuous break at t=0	Difference between prediction and counterfactual over first twelve months
Violent	19.81	11.01	21.07[c]	17.43
	(13.70)	(22.94)	(10.88)	(17.69)
Murder	0.24	0.34	0.15	0.43
	(0.57)	(0.79)	(0.40)	(0.50)
Rape	0.07	-2.81	0.05	-2.84
	(1.48)	(3.49)	(1.31)	(2.64)
Robbery	14.29[a]	14.79[a]	14.74[a]	17.14[a]
	(2.77)	(4.14)	(2.60)	(3.64)
Assault	6.05	-0.62	7.89	4.48
	(12.12)	(20.75)	(10.86)	(16.33)
Property	152.14[a]	266.43[b]	155.41[a]	267.23[b]
	(56.72)	(104.75)	(61.18)	(113.55)
Burglary	-9.30	-1.27	-10.79	6.88
	(10.08)	(18.28)	(9.92)	(17.09)
Larceny	128.94[a]	193.85[b]	129.33[a]	186.44[b]
	(43.27)	(66.84)	(46.43)	(86.90)
Motor Vehicle	24.86[c]	63.10[a]	21.78	61.99[b]
Theft	(13.13)	(23.30)	(14.35)	(25.34)

Standard errors are in parentheses. Estimates are based on estimation of equations (3) and (4) from the main text inclusive of calendar month fixed effects. Regression models allow for an AR(1) error structure.

a. Statistically significant at the one percent level of confidence.

b. Statistically significant at the five percent level of confidence.

c. Statistically significant at the ten percent level of confidence.

Table 6
Regression Pre-Post Changes in Average County-Level Monthly Crime Rates on Corresponding Changes in Average Jail DP per 100,000, Average Arrests for Drug and Property Offenses per 100,000, and the Number of Prop. 47 Resentencing and Reclassification Petitions per 100,000 Using 26 Pre and Post-Prop. 47 Months

	Change in Jail ADP Rate	Resentencing/Reclassification Petitions per 100,000	Change in drug and property arrests per 100,000
Violent			
Unweighted	0.005 (0.034)	-0.001 (0.002)	0.027 (0.076)
Weighted	0.051[c] (0.030)	-0.003[a] (0.001)	-0.030 (0.038)
Murder			
Unweighted	0.002 (0.002)	0.0000 (0.0001)	0.0005 (0.003)
Weighted	-0.002 (0.002)	-0.0000 (0.0001)	0.004[b] (0.002)
Rape			
Unweighted	0.009 (0.007)	-0.0002 (0.0004)	0.026[c] (0.015)
Weighted	0.008 (0.006)	-0.0004[b] (0.0002)	0.008 (0.007)
Robbery			
Unweighted	0.006 (0.008)	-0.0005 (0.0006)	-0.023 (0.018)
Weighted	0.009 (0.011)	-0.0009[b] (0.0003)	-0.030[b] (0.025)
Assault			
Unweighted	-0.011 (0.032)	0.000 (0.002)	0.023 (0.072)
Weighted	0.036 (0.025)	-0.001[c] (0.0007)	-0.009 (0.032)
Property			
Unweighted	0.032 (0.093)	-0.005 (0.006)	0.015 (0.211)
Weighted	-0.284 (0.172)	-0.011 (0.004)	-0.015 (0.221)
Burglary			
Unweighted	-0.003 (0.035)	-0.003 (0.002)	0.083 (0.079)
Weighted	0.012 (0.045)	-0.002[c] (0.001)	-0.090 (0.059)
Larceny			
Unweighted	0.051 (0.069)	-0.003 (0.005)	-0.183 (0.154)
Weighted	-0.194[c] (0.119)	-0.005 (0.003)	-0.061 (0.143)
Motor Veh. Theft			
Unweighted	-0.015 (0.024)	0.001 (0.002)	0.115 (0.055)[b]
Weighted	-0.102[c] (0.053)	-0.003[b] (0.001)	0.136[c] (0.068)

Standard errors are in parentheses. Each row presents the results from a separate regression of the change in monthly crime rates pre-post proposition 47 on the change in the jail incarceration rate, the quantity of resentencing/reclassification petitions filed per capita, and the change in property and drug crime arrests per 100,000. Each regression has 56 observations. We omit observations for Alpine and Sierras counties since they do not operate independent jail systems for the entire period analyzed.

a. Statistically significant at the one percent level of confidence.

b. Statistically significant at the five percent level of confidence.

c. Statistically significant at the ten percent level of confidence.

Appendix Table A1
Violent Crime Rate Levels in California and Synthetic California from 2000-2016 With and Without Los Angeles in the Tabulation of California's State Level Crime Rates

Year	Violent, CA	Violent, Synth. CA	Murder, CA	Murder Synth. CA	Rape, CA	Rape, Synth. CA	Robbery, CA	Robbery, Synth. CA	Assault, CA	Assault, Synth. CA
Panel A: Including Los Angeles in the tabulation of State Leve Crime Rates										
2000	621.60	620.68	6.1	6.1	28.90	28.87	177.90	178.05	408.70	395.85
2001	615.20	611.68	6.4	6.4	28.80	28.75	186.70	185.80	393.30	389.87
2002	595.40	592.49	6.8	6.8	29.10	29.02	185.60	185.69	373.80	379.23
2003	579.60	561.88	6.8	6.8	28.20	28.19	179.80	179.73	364.80	338.62
2004	527.80	536.76	6.7	6.6	26.80	26.78	172.30	172.34	322.00	335.00
2005	526.00	536.68	6.9	6.8	26.00	26.00	176.00	175.48	317.10	320.66
2006	533.30	535.94	6.8	6.8	25.30	25.27	195.00	195.43	306.20	312.27
2007	522.60	527.11	6.2	6.3	24.70	24.68	193.00	192.54	298.80	302.81
2008	506.20	506.86	5.9	5.9	24.30	24.28	189.70	189.34	286.30	287.49
2009	472.00	468.51	5.3	5.4	23.60	23.58	173.40	172.28	269.70	281.15
2010	440.60	439.29	4.9	5.1	22.40	22.59	156.00	152.02	257.40	271.27
2011	411.20	419.99	4.8	4.8	20.30	22.44	144.10	141.39	242.00	255.43
2012	423.10	409.83	5.0	4.7	20.60	21.87	148.60	140.71	248.90	252.75
2013	402.10	399.45	4.6	4.9	19.50	20.53	139.90	137.37	232.30	246.48
2014	396.40	387.23	4.4	4.8	21.60	21.91	125.50	132.35	236.70	238.44
2015	426.30	394.75	4.8	5.8	24.00	22.81	135.00	131.56	253.80	238.72
2016	445.30	385.95	4.9	6.1	25.90	23.78	139.60	133.77	265.90	244.06
Panel B: Excluding Los Angeles in the tabulation of State Leve Crime Rates										
2000	529.14	529.07	5.0	5.0	27.50	-	147.68	147.89	348.91	349.02
2001	520.68	520.57	5.2	5.2	27.72	-	153.83	152.86	333.89	333.95
2002	498.41	498.28	5.5	5.6	27.92	-	152.06	152.43	312.88	313.75
2003	490.56	490.38	6.0	6.0	27.37	-	148.08	150.84	309.15	308.66
2004	481.14	481.04	5.8	5.8	26.33	-	147.44	147.08	301.54	302.27
2005	487.54	487.42	6.2	6.2	25.45	-	153.30	152.14	302.56	302.49
2006	500.38	500.13	6.1	6.1	24.94	-	173.17	173.91	296.15	296.21

2007	497.55	497.30	5.7	5.7	24.42	-	173.94	171.95	293.51	294.13
2008	480.29	480.05	5.3	5.3	24.08	-	169.45	168.80	281.43	281.84
2009	453.30	453.06	5.0	5.0	23.47	-	155.93	154.87	268.90	269.20
2010	424.93	426.64	4.5	4.8	22.06	-	140.60	138.49	257.75	257.73
2011	396.75	411.84	4.4	4.7	20.10	-	130.04	131.21	242.20	244.88
2012	414.64	411.00	4.6	4.5	20.09	-	138.42	129.10	251.51	243.60
2013	390.90	390.74	4.3	4.6	19.35	-	132.14	123.64	235.09	227.43
2014	377.57	390.66	4.1	4.7	23.59	-	116.14	120.13	233.73	228.18
2015	400.35	404.87	4.5	5.3	29.94	-	124.04	119.96	241.90	237.27
2016	403.52	415.77	4.6	5.7	31.59	-	125.45	121.35	249.41	234.27

Crime rates for California in Panel A are official UCR crime rates for California tabulated by the FBI. The crime rates in panel A for synthetic California are the comparison crime rates for the weighted averaged of control states matching California crime rates from 2000 through 2010. The California crime rates for panel B are tabulated from the agency-level UCR Offenses Known and Cleared by Arrests Files for 2000 through 2016. These tabulations drop crimes reported by the Los Angeles Police Department from the state total. Rates are tabulated adjusting the population for the omission of the city of Los Angeles. The synthetic California crime rates in panel B are based on a synthetic control match to these alternative time series.

Appendix Table A2
Property Crime Rate Levels in California and Synthetic California from 2000-2016 With and Without Los Angeles in the Tabulation of California's State Level Crime Rates

Year	Property, CA	Property, Synth. CA	Burglary, CA	Burglary, Synth. CA	Larceny, CA	Larceny, Synth. CA	MVT, CA	MVT, Synth. CA
Panel A: Including Los Angeles in the tabulation of State Leve Crime Rates								
2000	3,118.20	3,155.00	656.30	655.95	1,924.50	1,997.44	537.40	549.80
2001	3,278.00	3,284.32	671.30	670.98	2,016.60	2,009.52	590.10	585.58
2002	3,361.20	3,342.86	681.20	680.89	2,044.70	2,013.07	635.30	632.81
2003	3,426.40	3,414.20	683.20	682.82	2,062.70	2,046.29	680.50	687.47
2004	3,423.90	3,420.11	686.10	685.90	2,033.10	1,985.34	704.80	696.15
2005	3,321.00	3,319.00	692.90	692.82	1,915.00	1,929.44	712.00	720.23
2006	3,175.20	3,185.25	676.90	676.88	1,831.50	1,835.26	666.80	686.22
2007	3,032.60	3,047.74	648.40	648.27	1,784.10	1,785.46	600.20	600.82
2008	2,954.50	2,912.40	649.90	649.83	1,778.30	1,779.62	526.30	485.56
2009	2,731.50	2,746.67	622.60	622.45	1,665.10	1,683.80	443.80	376.71
2010	2,635.80	2,546.55	614.30	610.13	1,612.10	1,567.61	409.40	322.72
2011	2,584.20	2,398.57	610.50	621.69	1,584.00	1,520.06	389.70	297.02
2012	2,758.70	2,478.74	646.10	601.95	1,669.50	1,648.63	443.20	289.78
2013	2,658.10	2,388.14	605.40	553.40	1,621.50	1,596.34	431.20	276.05
2014	2,441.70	2,179.13	522.40	492.82	1,527.80	1,511.57	391.40	276.01
2015	2,618.30	2,114.31	504.30	459.26	1,677.10	1,488.08	436.80	300.84
2016	2,553.00	2,127.64	479.80	439.55	1,623.00	1,497.00	450.30	329.59
Panel B: Excluding Los Angeles in the tabulation of State Leve Crime Rates								
2000	3,050.74	3,101.59	653.41	652.81	1,900.64	1,978.34	496.68	531.89
2001	3,228.36	3,228.19	669.60	668.53	2,002.99	1,991.59	555.77	555.86
2002	3,298.23	3,276.34	676.88	676.11	2,022.07	1,996.42	599.28	600.72
2003	3,379.85	3,378.59	678.90	678.51	2,048.34	2,033.93	652.60	654.39
2004	3,416.45	3,415.17	689.92	689.08	2,036.47	1,988.42	690.06	666.60
2005	3,330.31	3,341.89	701.13	699.81	1,924.02	1,942.43	705.16	709.65
2006	3,213.21	3,214.94	691.54	690.82	1,857.01	1,861.81	664.66	687.67
2007	3,070.37	3,075.19	662.72	661.63	1,810.56	1,810.92	597.08	598.75
2008	2,967.18	2,923.81	660.07	659.07	1,792.65	1,793.78	514.47	483.51

2009	2,747.64	2,777.86	635.71	634.89	1,674.85	1,701.39	437.08	376.81
2010	2,658.39	2,564.82	629.89	607.76	1,624.81	1,567.61	403.70	324.93
2011	2,610.21	2,395.82	625.89	620.13	1,598.31	1,511.67	386.01	307.76
2012	2,800.92	2,491.03	667.84	601.50	1,686.05	1,639.20	447.03	302.88
2013	2,694.59	2,378.10	624.73	551.38	1,634.03	1,583.81	435.82	291.47
2014	2,463.76	2,198.74	534.92	485.25	1,535.16	1,501.72	393.68	294.36
2015	2,632.04	2,152.46	512.19	453.44	1,682.36	1,473.31	437.50	314.31
2016	2,547.49	2,164.54	486.75	428.59	1,615.03	1,487.10	446.41	348.00

Crime rates for California in Panel A are official UCR crime rates for California tabulated by the FBI. The crime rates in panel A for synthetic California are the comparison crime rates for the weighted averaged of control states matching California crime rates from 2000 through 2010. The California crime rates for panel B are tabulated from the agency-level UCR Offenses Known and Cleared by Arrests Files for 2000 through 2016. These tabulations drop crimes reported by the Los Angeles Police Department from the state total. Rates are tabulated adjusting the population for the omission of the city of Los Angeles. The synthetic California crime rates in panel B are based on a synthetic control match to these alternative time series.

Appendix Table A3
Regression Pre-Post Changes in Average County-Level Monthly Crime Rates on Corresponding Changes in Average Jail DP per 100,000, Average Arrests for Drug and Property Offenses per 100,000, and the Number of Prop. 47 Resentencing and Reclassification Petitions per 100,000 Using 12 Pre and Post-Prop. 47 Months

	Change in Jail ADP Rate	Resentencing/Reclassification Petitions per 100,000	Change in drug and property arrests per 100,000
Violent			
Unweighted	-0.020 (0.031)	-0.001 (0.002)	0.066 (0.070)
Weighted	0.041 (0.036)	-0.003[a] (0.001)	0.082[b] (0.036)
Murder			
Unweighted	0.003[c] (0.001)	0.0000 (0.0001)	0.002 (0.004)
Weighted	-0.001 (0.003)	-0.00002 (0.00006)	0.005[c] (0.003)
Rape			
Unweighted	0.008 (0.006)	-0.0004 (0.0004)	0.024 (0.014)
Weighted	0.008 (0.007)	-0.004[b] (0.002)	0.009 (0.007)
Robbery			
Unweighted	0.013 (0.009)	-0.001 (0.001)	-0.013 (0.020)
Weighted	0.014 (0.015)	-0.001[a] (0.0003)	-0.0007 (0.015)
Assault			
Unweighted	-0.044 (0.030)	0.0003 (0.002)	0.054 (0.067)
Weighted	0.019 (0.031)	-0.001[b] (0.0007)	0.069[b] (0.031)
Property			
Unweighted	0.042 (0.085)	-0.006 (0.006)	0.120 (0.188)
Weighted	-0.158 (0.181)	-0.014[a] (0.005)	0.133 (0.182)
Burglary			
Unweighted	0.010 (0.036)	-0.003 (0.002)	0.139[c] (0.081)
Weighted	0.019 (0.052)	-0.003[b] (0.001)	-0.077 (0.052)
Larceny			
Unweighted	0.036 (0.061)	-0.003 (0.004)	-0.195 (0.137)
Weighted	-0.143 (0.123)	-0.007[b] (0.003)	0.021 (0.125)
Motor Veh. Theft			
Unweighted	-0.005 (0.025)	0.0002 (0.002)	0.175[a] (0.056)
Weighted	-0.034 (0.065)	-0.004[a] (0.001)	0.189[a] (0.065)

Standard errors are in parentheses. Each row presents the results from a separate regression of the change in monthly crime rates pre-post proposition 47 on the change in the jail incarceration rate, the quantity of resentencing/reclassification petitions filed per capita, and the change in property and drug crime arrests per 100,000. Each regression has 56 observations. We omit observations for Alpine and Sierras counties since they do not operate independent jail systems for the entire period analyzed.

a. Statistically significant at the one percent level of confidence.

b. Statistically significant at the five percent level of confidence.

c. Statistically significant at the ten percent level of confidence.