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Can We Downsize Our Prisons and Jails Without Compromising Public Safety?

Findings from California's Prop 47

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Research Summary

Our study represents the first effort to evaluate systematically Proposition 47's (Prop 47's) impact on California's crime rates. With a state-level panel containing violent and property offenses from 1970 through 2015, we employ a synthetic control group design to approximate California's crime rates had Prop 47 not been enacted. Our findings suggest that Prop 47 had no effect on homicide, rape, aggravated assault, robbery, or burglary. Larceny and motor vehicle thefts, however, seem to have increased moderately after Prop 47, but these results were both sensitive to alternative specifications of our synthetic control group and small enough that placebo testing cannot rule out spuriousness.

Policy Implications

As the United States engages in renewed debates regarding the scale and cost of its incarcerated population, California stands at the forefront of criminal justice reform. Although California reduced its prison population by 13,000 through Prop 47, critics argue anecdotally that the measure is responsible for recent crime upticks across the state. We find little empirical support for these claims. Thus, our findings suggest that California can downsize its prisons and jails without compromising public safety.

Keywords

criminal justice reform, crime, prison downsizing, decarceration, California

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Speaking on the promise of downsizing prisons, Joan Petersilia (2016) recently distinguished between symbolic speechmaking, which is easy, and actual reform, which “is about as easy as bending granite” (p. 9; see also Petersilia and Cullen, 2015). Indeed, scholars have long made the distinction between “policy talk” and “policy action” (Tyack and Cuban, 1995), especially in the context of criminal justice reform in the United States. Yet in recent years, policy action, in fact, may be a good way to characterize many of the changes that have occurred in America’s criminal justice system. Indeed, Petersilia (2016: 8) also noted:

We are very likely at a transformative moment in criminal justice reform. There is great optimism that the United States is making a decisive move away from the harsh punishment policies that characterized the last 30 years. Prison growth has largely stopped, some states are closing prisons, and Congress and most legislatures are enacting policies that reduce prison sentences for drug crimes and other nonviolent offenses.

California has been at the epicenter of these changes. Perhaps more than any other state, California is immersed in a period of fundamental reform to its criminal justice system. In just a few short years, the state has passed a series of senate bills and propositions, most of which are intended to reduce its massive prison population. So far, they seem to be working. A recent report published by the Public Policy Institute of California, *California’s Historic Corrections Reforms*, concluded: “Since reaching a peak in 2006 of almost 256,000 inmates, the total population incarcerated in California’s state prisons and county jails has dropped by roughly 55,000. The incarceration rate has fallen from 702 to 515 per 100,000 residents—a level not seen since the early 1990s” (Lofstrom, Bird, and Martin, 2016:3).

One of the most recent of these reforms that has garnered significant attention is Proposition 47 (Prop 47), which requires that certain drug and property offenses be charged as misdemeanors rather than as felonies, as had previously been the case. Since the enactment of Prop 47 on November 14, 2014, the number of people incarcerated in California’s prisons and jails has decreased by approximately 13,000 inmates, helping alleviate crowding conditions in those institutions (Romano, 2015). Proponents of Prop 47 hail it a success, yet critics charge that the measure is mainly responsible for recent upticks in the state’s crime rates.

Despite these contradictory claims, to date there has been no systematic analysis of Prop 47’s impact on crime rates throughout the state, leaving Californians in the dark about the policy’s effectiveness. We address this research lacuna in this study. With a synthetic control group design, we conduct the first evaluation of Prop 47’s impact on violent and property crime rates in the year after its implementation. By using a state-level panel containing UCR index 1 offense frequencies from 1970 through 2015, we employ a synthetic control group design to approximate California’s crime rates had Prop 47 not been enacted. We perform this analysis for each offense category and interpret the gap between California’s

2015 crime rate and our constructed counterfactual approximation as Prop 47's impact. As with other recent criminal justice reforms in California, the implementation of Prop 47 is "a natural experiment that allows us to test one of the most important crime policy questions of our time" (Sundt, Salisbury, and Harmon, 2016: 316). At the same time, the findings have implications well beyond Prop 47 and California as other states encounter similar pressures to downsize their prisons and jails and seek examples of successful reform.

In the remainder of the article, we first describe Prop 47 in the broader context of criminal justice reform in California. We then summarize key arguments made by both Prop 47 proponents and opponents with respect to its hypothesized impact on crime throughout the state. We then describe our data and methodological approach, followed by a discussion of the findings. We conclude by reviewing the key findings, noting some limitations with the study, identifying future avenues of research, and discussing the implications of the findings for state systems across the country.

Prop 47 and Criminal Justice Reform in California

For years, California was home to the nation's largest state prison system. At its apex in 2006, the state prison population peaked at more than 170,000 inmates (West and Sabol, 2008), despite the fact that California prisons were designed to hold a maximum of 79,858 inmates. Critics charged that California was incarcerating too many people for too long. Starting in 2011, the state began to implement a series of criminal justice reforms, one of which is Prop 47.

What led to these reforms? Several factors were at play. First, fiscal impacts of the recent economic recession induced state leaders to scour their budgets for potential savings (Lofstrom and Raphael, 2015: 197; see also Aviram, 2016). At a cost of approximately \$52,000 per year per inmate (Lofstrom and Raphael, 2016: 218), the state was footing an enormous bill to incarcerate so many offenders, a sizable portion of whom were low-level, nonviolent offenders and parole violators.

Second, California experienced a bipartisan shift in public opinion regarding the use of prison as a tool for crime control and punishment (Lofstrom and Raphael, 2016: 197; but see Beckett, Reosti, and Knaphus, 2016), a trend that paralleled what was happening at the national level (Petersilia, 2016: 8). This shift occurred, in part, after the realization that increased sentences did not seem to budge California's stubbornly high recidivism rate, which at close to 70% was among the highest in the nation (California Department of Corrections and Rehabilitation, 2010: 11). Evidence of dissatisfaction with the status quo could be seen in public opinion polls, which overwhelmingly reflected support for policy changes that reduced incarceration. Prop 47, for example, passed by a wide margin, with 60% of California residents voting in favor of it.

And third, California experienced federal court intervention as a result of the conditions of confinement in its state prisons (Lofstrom and Raphael, 2016: 197; see also Kubrin and

Seron, 2016; Sundt et al., 2016: 316–317). Extreme overcrowding led the U.S. Supreme Court to take a historic step, ordering the state to reduce its prison population to comply with constitutional standards. In *Brown v. Plata*, the Supreme Court ruled that overcrowding in California’s prisons resulted in cruel and unusual punishment in violation of the Constitution’s Eighth Amendment. The decision, handed down on May 23, 2011, was the result of nearly 20 years of litigation (Schlanger, 2016) in which the lower federal court found that the “convergence of tough-on-crime policies and an unwillingness to expend the necessary funds to support the population growth has brought California’s prisons to the breaking point” (*Plata/Coleman v. Brown* 2009: 182). The Supreme Court’s decision required the California Department of Corrections and Rehabilitation (CDCR) to reduce the state prison population by approximately 33,000 people (to 137.5% of design capacity) over a 2-year timeframe—no small feat.

AB 109, Public Safety Realignment

California responded to the Court’s mandate by enacting the first of several controversial reforms: “Public Safety Realignment” (Assembly Bill [AB] 109). Realignment made fundamental changes to California’s correctional system, including realigning from state to local jurisdictions certain responsibilities for lower level nonviolent offenders and parolees. Specifically, AB 109 required nonviolent, nonserious, and nonsex offenders (“the triple nons”) to serve their sentences in county jails instead of in state prisons, thus, shifting responsibility for punishment from prisons, which in the United States are state or federal operations, to jails, which are run by counties and their elected sheriffs. A similar change applied to everyone released from state prison. Before implementation, these individuals were automatically on “parole” (a state term), which was then replaced by local “post-release community supervision.”

Governor Edmund G. Brown Jr. proposed Realignment in January 2011, the legislature approved it in March, and it took effect in October of that year—an unusually fast track for a major policy shift described as “the biggest criminal justice experiment ever conducted in America” (Petersilia, 2012). The outcome was a sharp and permanent reduction in the state’s incarceration rate, driven mainly by a reduction in new prison admissions (Lofstrom and Martin, 2016). In a very short time, Realignment substantially reduced California’s prison population.¹ Yet almost all of the decline took place in the first year, and more importantly, it was not sufficient to meet the judicial target.

1. Some question whether reductions in state-level prison admissions were simply offset with increased jail populations at the local level. Notably, the county jail population did not rise nearly as much as the prison population fell, reducing the total number of people incarcerated in California. In particular, the jail population rose by only about one inmate for every three fewer offenders in state prison (Lofstrom and Martin, 2016).

Proposition 47

The judicial target was, in fact, met a few years later, in part, as a result of Proposition 47, approved by California voters on November 4, 2014. Also known as the “Reduced Penalties for Some Crimes Initiative,” Prop 47 changed the lowest level nonviolent drug possession and petty theft crimes from felonies to simple misdemeanors. In particular, Prop 47 reduced certain drug possession felonies to misdemeanors and required misdemeanor sentencing for a variety of crimes, including shoplifting, where the value of stolen property does not exceed \$950; grand theft, where the value of the stolen property does not exceed \$950; receiving stolen property, where the value of the property does not exceed \$950; forgery, where the value of a forged check, bond, or bill does not exceed \$950; fraud, where the value of the fraudulent check, draft, or order does not exceed \$950; and writing a bad check, where the value of the check does not exceed \$950.² Prop 47 was intended to impact future convictions and sentencing but also allowed for individuals incarcerated at the time for crimes covered by the measure to petition for resentencing. Notably, Prop 47 required thorough review of an individual’s criminal history and proper risk assessment before (re)sentencing to ensure public safety.

A unique component of Prop 47 is its additional focus on crime prevention. As state prison and jail population numbers were predicted to fall (some projected by as much as several thousand inmates or ~40,000 felony convictions a year [Watson, 2017]), it was estimated that state savings would grow by millions and would be reinvested in prevention efforts. In fact, through the creation of a Safe Neighborhoods and School Fund, the measure required money saved as a result of Prop 47 to be spent on “school truancy and dropout prevention, victim services, mental health and drug abuse treatment, and other programs designed to keep offenders out of prison and jail” (Legislative Analyst’s Office, 2014: Section “Summary of Legislative Analyst’s Estimate of Net State and Local Government Fiscal Impact,” bullet 1).

After the passage of Prop 47, California was finally able to reach the court-mandated prison population target. California’s jail population, in particular, dropped dramatically in the first few months after Prop 47’s passage. Bird, Tafoya, Grattet, and Nguyen (2016) identified four mechanisms that drove this decline: (1) immediate decline in new bookings on arrests and warrants for Prop 47 offenses, (2) decline in number of convictions for these individuals, (3) share of Prop 47 defendants receiving pretrial releases increased, and (4) decline in average length of stay for sentenced offenders (i.e., less custody time). In just a few short years as a result of these significant reforms, California has done an about-face. With an incarceration rate of 329 (per 100,000), California is now well below the national average of 458 per 100,000 (The Sentencing Project, n.d.).³

2. Except for shoplifting, property values for these offenses were previously set at \$450.

3. For a more complete discussion of California’s various contemporary criminal justice reforms (beyond Realignment and Prop 47), see Gardiner and Spiropoulos (2018).

Crime in the Wake of Prop 47

Proponents of Prop 47, which include a wide-ranging list of supporters (Ballotpedia, 2014b) have been vocal about the measure's benefits. Those in favor point out that punishment is now more commensurate with crime. They also emphasize that Prop 47 is helping the state make smarter use of its criminal justice and incarceration resources by no longer wasting prison space on low-level, nonviolent offenders, which frees up space for violent criminals. Relatedly, reductions in jail populations, induced by Prop 47, have allowed counties with court-capped jails to reduce their use of capacity releases substantially. For these counties, Prop 47 presented the opportunity to decrease custody time for lower level drug and property offenders and, in exchange, increase custody time for more serious offenders (at least some of whom would otherwise have been released early because of jail capacity constraints; Grattet, Tafoya, Bird, and Nguyen, 2016). All of these changes, proponents suggest, are likely to increase public safety and lower crime rates throughout the state. And Prop 47's reallocation of resources to prevention efforts, they further argue, should significantly improve public safety in the longer term. As evidence in support for some of these claims, proponents turn to scientific evaluations of Realignment, which found that it had no impact on violent crime rates and only a small impact on property crime rates, mainly auto-theft (Lofstrom and Raphael, 2016; Sundt et al., 2016; see also Bird and Grattet, 2016, for findings related to Realignment's impact on recidivism).

Prop 47 critics (also a wide-ranging group; Ballotpedia, 2014a) have been equally vocal. They argue that felony arrests throughout the state have plummeted, emboldening would-be criminals. They also claim that drug and theft offenders who previously were arrested and held in jail pending trial are now simply receiving citations and orders to appear in court, and that few actually show up for their court dates. As a consequence, "When you don't jail these people on drug and other relatively minor charges, they are free to commit all manner of more serious crimes, including murder, rape and robbery, and they do" (Greene, 2015).

Critics believe they have data on their side. After a decades-long decline in violent and property crime throughout the state, California's crime rate saw an uptick in 2015 after Prop 47's implementation. The violent crime rate increased by 8.4% in 2015 and the property crime rate went up by 6.6% (Lofstrom et al., 2016). Concentrating on California's largest cities, violent crime jumped 11% in the first 6 months of 2015 compared with the same period in 2014. Among major U.S. cities, three California cities saw the largest increase in property crime in the country (Levin, 2016). And, from 2015 to 2016, violent crime grew 4.1% (Miller, 2017). Law enforcement officials and others have voiced concern that Prop 47 is to blame for rising crime rates throughout the state.

Predictions aside, theory on the crime-prison relationship offers several important (if contradictory) predictions. Some theories suggest that prison is crime-suppressive, whereas others suggest it is criminogenic (Harding, Morenoff, Nguyen, and Bushway, 2017). Regarding the former, it has been argued, for example, that prisons incapacitate the criminally active and that the threat of prison may deter criminal activity; at the same time, prison

may be transformative through rehabilitation (Lofstrom and Raphael, 2016: 198). If these arguments are correct, we would expect a negative relationship between incarceration levels and criminal offending. Regarding the latter, however, it has been argued that incarceration may be associated with increasing crime levels, in part through a hardening of prison inmates (Lofstrom and Raphael, 2016: 198).

What do the researchers find? At early levels, incarceration does seem to reduce crime; however, diminishing crime-abating returns set in at low incarceration rates (King, Mauer, and Young, 2005: 6). Stated alternatively, scholars have reported very small crime-prevention effects of even marginal increases in incarceration. Moreover, in the context of the recent steep rise in U.S. incarceration rates, some researchers have found a criminogenic effect: “[O]ur results demonstrate that imprisonment leads to future imprisonment. In other words, prison’s figurative revolving door has real causal force, rather than being the simple consequence of imprisonment of individuals at higher risk for future offending. These results imply that the rise in incarceration was to some degree self-generating, as imprisonment creates more imprisonment” (Harding et al., 2017: 4). Notably, the relationship between incarceration and crime is almost always examined at the individual level. Moreover, this relationship is overwhelming in its complexity.⁴

Returning to the focus of the study, what impact has Prop 47 had on crime rates in California? Are Prop 47 and the state’s rising crime rates connected? At this point, we do not know. Since its implementation on November 4, 2014, there has not been even *one* systematic analysis of Prop 47’s impact on crime in California. For this reason, researchers continually warn against premature conclusions when they claim, “[I]t is too early to conclusively determine whether or not Prop. 47 has had an impact on crime” (Males, 2016: 5) and “caution should be used in drawing strong conclusions about Prop 47 from the comparison of California to the rest of the country” (Lofstrom et al., 2016: 14), a sentiment echoed by some reporters who remind readers, “[T]here has been no definitive research to date showing a relationship between crime trends and Proposition 47” (Levin, 2016). Indeed, several critical questions about Prop 47 remain unanswered, as Lofstrom and Martin (2017) recently reminded us: “How have reforms affected factors such as arrests and incarceration? Do these differ across counties and what is their relationship to crime rates? Also, California’s crime trends may be affected by factors unrelated to recent reforms. How do statewide trends compare to what other states are seeing?” We begin to address some of these critical questions here.

This study represents the first effort to evaluate systematically the causal effect of Prop 47’s enactment on UCR part 1 violent (homicide, rape, aggravated assault, and robbery) and property (burglary, larceny, and motor vehicle theft) crime rates throughout California. As we discuss in detail in the Conclusion, the findings of this study have implications well

4. For an excellent review of research on the imprisonment–crime nexus, see King et al. (2005) and Raphael and Stoll (2009).

beyond both Prop 47 and California, as states across the country consider reforming their criminal justice systems and face similar pressures to downsize their prisons and jails.

Data and Method

With a quasi-experimental design, we examine the impact of Prop 47 on crime in the year after its enactment (i.e., 2015). Employing a synthetic control group design, described in detail as follows, we aim to identify Prop 47's causal effect on crime throughout the state. Through our analysis, we utilize a state-level panel dataset (including the District of Columbia) containing annual Uniform Crime Report Part 1 offense frequencies spanning 1970–2015. In particular, we examine the crimes of homicide, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle thefts. We transform statewide crime frequencies into per-capita rates to facilitate comparisons between states of different sizes (i.e., allow large states like California to be compared with small states like Delaware without extrapolating).⁵ Without this transformation, the state with the highest observed crime frequency could not be approximated by a linear combination of the other states, as their weighted average would fall short of the highest observed frequency without extrapolating.

Methodological Approach

To evaluate the impact of Prop 47 on crime rates, we use a synthetic control group design to construct a comparison unit that approximates California had it not enacted Prop 47 (i.e., “Counterfactual California”; Abadie, Diamond, and Hainmueller, 2010; Lofstrom and Raphael, 2015). This quasi-experimental design is an extension of “difference-in-differences” models, which are aimed at estimating the causal effect of an intervention as the change in the distance between two time series that emerges after an intervention. In standard difference-in-differences (DiD) designs, it is assumed that the treated unit and its untreated comparison unit follow “parallel trends” prior to the intervention. When examining state-level interventions, however, neither the nation as a whole nor any individual state is likely to follow the treated state’s long and jagged preintervention time series. By assuming “parallel trends” prior to the intervention, DiD designs are used to interpret any change in the gap between the treated and comparison units after the intervention as the effect of the treatment on the outcome. To better satisfy the “parallel trends” assumption, we construct a synthetic control group for California, “Counterfactual California,” as a weighted combination of “donor pool” states that optimally fits California’s crime trends from 1970 to 2014, the preintervention period. By fitting our synthetic control groups over preintervention time series containing 44 years of pre-Prop 47 crime rate observations, we go beyond selecting the most appropriate control time series for California and instead construct a better comparison unit than any individual unit available that exists. Matching on a long

5. We examined alternative transformations of the dependent variables. Results, available upon request, were mainly consistent.

($n = 44$) preintervention time series also greatly reduces our likelihood of identifying a spurious effect compared with synthetic control group models matched on fewer preintervention observations (Abadie et al., 2010; McCleary, McDowall, and Bartos, 2017).

We populate our “donor pool” with states whose time series do not reflect the impact of a Prop 47-style intervention within our analysis frame. It is important to exclude all states that experienced criminal justice interventions similar to California’s Prop 47 from the donor pool; otherwise, the constructed synthetic control may be contaminated by the contribution of a treated donor pool state (i.e., Synthetic California’s time series would also reflect Prop 47’s impact to some degree). Since Prop 47 was intended, in part, to ameliorate California’s lingering post-Realignment overcrowding issues (Romano, 2015: 3) and the sentence reductions it carried apply to a select subgroup of property and drug offenders, in fact, no other states experienced a comparable criminal justice intervention. Therefore, we include the remaining 49 states in our donor pool from which Synthetic California is constructed.

An important step in the process of synthetic control group construction involves choosing an optimal combination of donor pool weights. We employ the data-driven approach for assigning donor pool weights (time-invariant,⁶ non-negative,⁷ and sum to one⁸) described in Abadie et al. (2010, 2015) so as to minimize the distance between California and “Counterfactual California’s” crime trends throughout the preintervention time series. When a gap emerges between California and its synthetic counterpart after the enactment of Prop 47, the difference between the two time series can be interpreted as the causal effect of Prop 47 on the crime rate examined. Causal interpretations of the gap are predicated on the quality of the match between California and Synthetic California across the preintervention period.

We describe the quality of our preintervention fit using the conventional root mean squared prediction error (RMSPE) term, as discussed in the Findings section. If the gap between California and its constructed “Counterfactual” that emerges post-Prop 47 is within the range of the preintervention RMSPE, no effect beyond what is attributable to matching error can be identified. Identifying a post-Prop 47 gap greater than the observed pre-Prop 47 RMSPE does not mean the estimated effect is of practical significance, however. When the precision of the preintervention fit between California and Synthetic California is very good, a postintervention gap that is small relative to the observed variation in the preintervention time series can result in an effect size that is an order of magnitude greater than the preintervention RMSPE. Thus, when the preintervention fit is more precise, smaller treatment effects can be identified.

6. $\omega_{(1|t=1)} = \omega_{(1|t=2)} = \omega_{(1|t=n)}$

7. $(\omega_1, \omega_2, \dots, \omega_n \geq 0)$

8. $(\omega_1 + \omega_2 + \dots + \omega_n = 1)$

To fit our models, we use the “Synth” routine written for Stata by Jans Heinmuller and Aberto Abadie (available at web.stanford.edu/~jhain/synthpage.html). We include all available preintervention observations (e.g., crime in California 1970–2014) of the outcome of interest as predictors (Lofstrom and Raphael, 2015; McCleary et al., 2017). By fitting our models on longer ($n = 44$, 1970–2014) time series that exhibit a great deal of white-noise variation, the optimization routine is less likely to converge on a perfect approximation of pre-Prop 47 California, but we are much less likely to identify a spurious effect than models fit on shorter and/or smoother preintervention time series (McCleary et al., 2017).

Postestimation tests. We conduct a series of postestimation tests to enhance our confidence in the reported findings. In particular, the postestimation tests allow us to address questions of spuriousness as well as to determine the extent to which our findings may be sensitive to model specification.

Concerning the former, to determine whether the estimated impact is large relative to the unidentified/exogenous variation observed among untreated (i.e., donor pool) states, in-sample placebo tests are conducted, providing a type of randomization inference (Abadie et al., 2010; Abadie and Gardeazabal, 2003; Fisher, 1922; McCleary et al., 2017). We iteratively reassign the treatment condition to each donor pool state and construct a synthetic control group. The states are then ranked by a ratio of 2015 gap to pre-2015 RMSPE. If California ranks highest among our pool of 50 states, then the estimated effect is larger than the unidentified variation observed in the donor pool states. If California does not rank highly, however, then the estimated effect is not large relative to the white noise exhibited by non-Prop 47 states. This randomization-inference procedure determines the probability of estimating an effect with an equal or greater ratio than California in any of the other donor states. Put another way, pretending that we don’t know which state enacted Prop 47, we construct synthetic control groups for every donor pool state and estimate the effect of Prop 47 on crime in 2015. Because California is the only state that enacted Prop 47, it should produce a larger ratio than any state in the donor pool.⁹

Another important postestimation test, known as the “Leave One Out” test, evaluates whether an estimated effect is sensitive to changes in Synthetic “Counterfactual” California’s composition. We achieve this by iteratively excluding the donor pool unit contributing the largest weight to Synthetic California until all of the original donor pool units with non-zero weights are excluded from the matching algorithm. At the end of this process, Synthetic

9. Whereas an in-sample placebo test compares the effect of Prop 47 in California with nontreated states, an in-time placebo test would compare Prop 47’s estimated effect in the year it was enacted to random effects in nonenacted years. In-time placebo tests assume, however, that no structural shocks to California’s crime rate occurred prior to Prop 47. Yet as our previous discussion on criminal justice reform in California reveals, recent reforms make this assumption untenable. For example, we would expect an in-time placebo test performed in 2011 to produce a larger effect estimate than Prop 47 in 2014 due to the enactment of AB109, making the in-time placebo test uninterpretable. Thus, in-time placebo tests are not applicable in this context (see McCleary et al., 2017: Ch. 7).

California is comprised of a completely different set of donor pool units than it was in the original model. If the original effect persists in sign and magnitude once all of the original contributors to Synthetic California have been excluded, then we can be confident that this effect is insensitive to changes in Synthetic California's composition. In other words, we can be confident that our interpretation of Prop 47's effect on crime does not change even when substantial changes are made to Synthetic California.

Results

To estimate the impact of Prop 47's enactment on crime rates in California, we construct synthetic control groups for homicide, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft. Figure 1 displays California (solid black line) and our constructed synthetic control (dashed black line) for each offense category. The gray dashed reference line reflects the 2014 enactment of Prop 47.

For homicide, rape, aggravated assault, robbery, and burglary, we find no evidence that the impact of Prop 47 was any different from zero. In other words, Prop 47 appears to have a null effect on these offenses. In particular, the gap that emerges after Prop 47's enactment was smaller than the model's preintervention RMSPE. Therefore, Prop 47's impact on these offense categories was within the range attributable to matching error and cannot be distinguished from zero.

For larceny and motor-vehicle theft, on the other hand, the gap that emerged in 2015 (i.e., post-Prop 47) was more than twice the size of the model's preintervention RMPSE, suggesting that Prop 47 did have an impact on these offenses. With California's actual time series falling above the synthetic control group estimate, the size and direction of the gap suggest that both larceny and motor vehicle theft experienced a nontrivial increase post-Prop 47. Although it is premature to draw conclusions about these effects prior to postestimation testing (see subsequent discussion), the postintervention gaps suggest that larceny and motor vehicle thefts were less than 10% and roughly 20% higher, respectively, in 2015 than they would have been without Prop 47.

Sensitivity/robustness tests. To determine whether the estimated effects of Prop 47 are large relative to the unidentified annual variation observed in states that did not experience Prop 47, we perform in-sample placebo tests (a type of randomization inference used to estimate the exact probability of identifying a treatment effect of equal or greater magnitude if the treatment were randomly assigned to each donor pool unit). Put another way, this test determines the probability of identifying California as the state that experienced Prop 47 effects if we began our analysis not knowing which state had enacted Prop 47. If we identify more than five donor pool states that produce larger treatment effects than California, then the probability of identifying an effect equal or greater in magnitude than California is greater than .1 (i.e., $p = 5 / 50 = .10$, $p = 6 / 50 = .12$) and would not be significant. Figure 2

FIGURE 1

(a) Synthetic Control Group Estimates for Violent Offenses and (b) Synthetic Control Group Estimates for Property Offenses

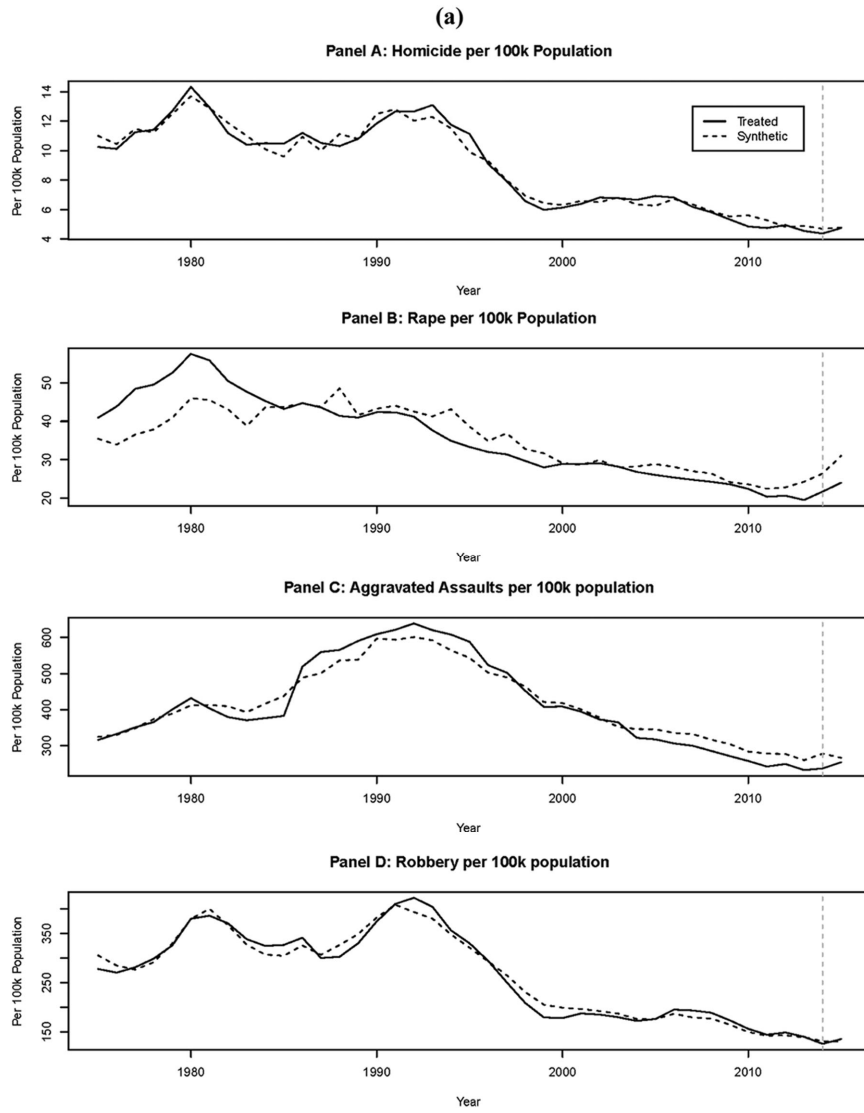
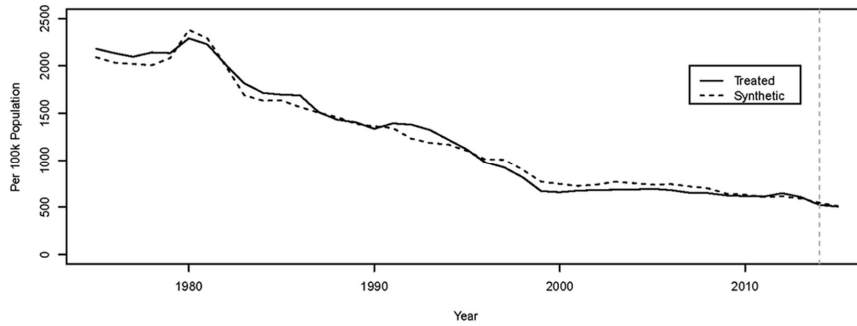


FIGURE 1

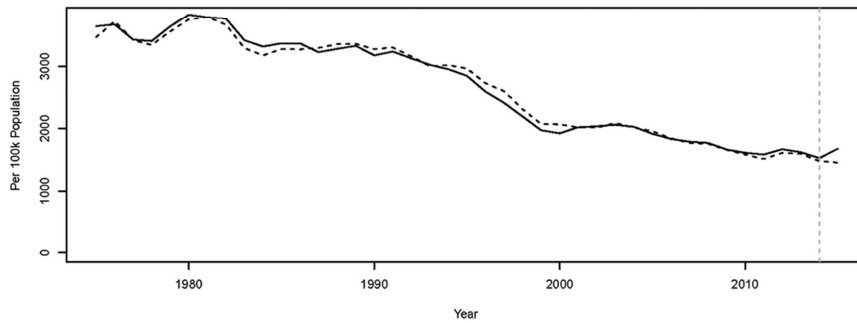
Continued

(b)

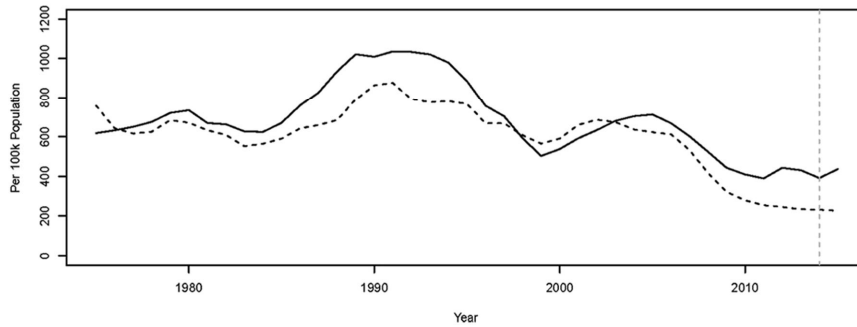
Panel A: Burglary per 100k population



Panel B: Larceny Thefts per 100k population



Panel C: Motor Vehicle Thefts per 100k pop

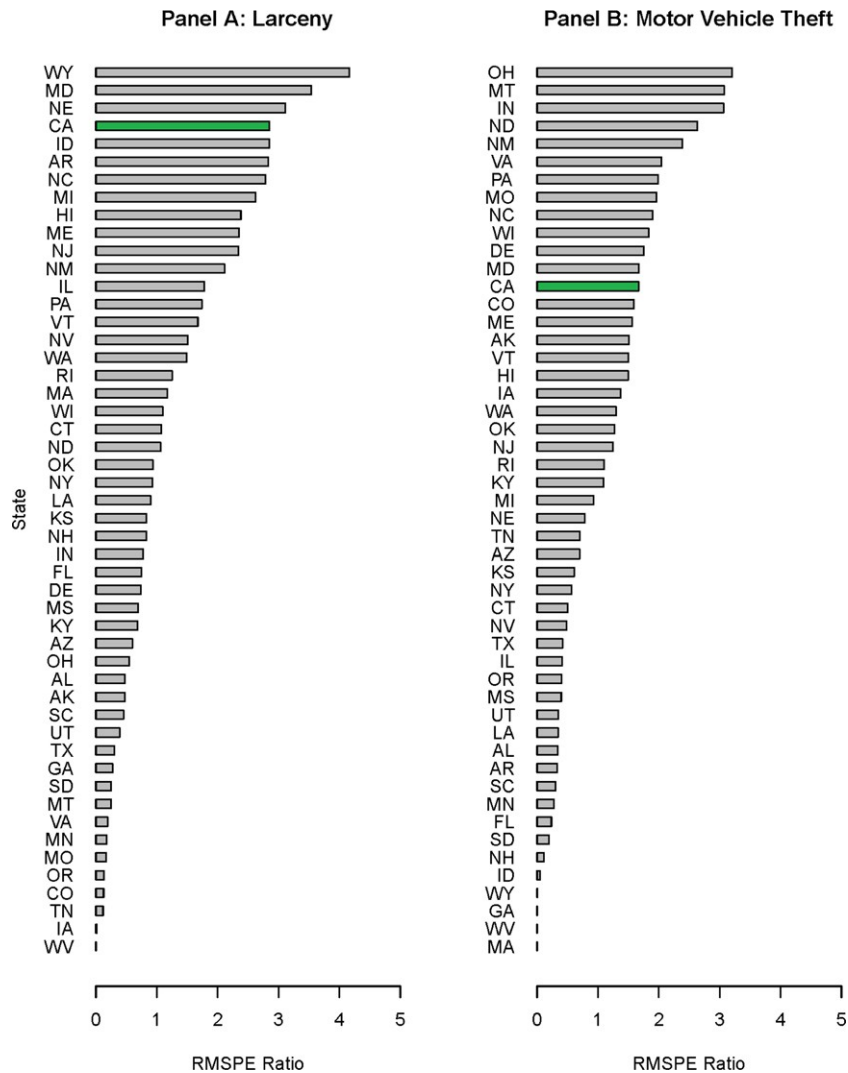


displays California's ratio of postintervention gap to preintervention RMSPE relative to the donor pool states for larceny and motor-vehicle theft, the two offenses that did not produce null effects.

As Figure 2 shows, California did not rank particularly highly for motor-vehicle thefts (13 out of 50; $p = \sim .26$), suggesting that the estimated effect appears smaller in California

FIGURE 2

In-Sample Placebo Test RMSPE Ratios Ranked [Color figure can be viewed at wileyonlinelibrary.com]



than the random variation observed in donor pool states. Larceny ranked 4 out of 50 ($p = \sim.08$), however, suggesting that the estimated larceny increase is not trivially small relative to changes in larceny observed in non-Prop 47 states.

In short, out of seven crime categories examined, our findings suggest Prop 47 had nonzero effects on larceny and motor vehicle thefts; however, only the larceny effect appears

significant (at the $p < .10$ level, akin to Fischer's "exact test"). In other words, larceny is the only offense category that has an exact probability of identifying a larger effect in the donor pool states of less than .10 ($4 / 50 = \sim .08$). Our estimate of Prop 47's effect on the rate of motor vehicle theft in California did not rank highly compared with the estimated effects for the donor pool. As such, if we did not know which state enacted Prop 47 in 2014, and we tried to identify it by looking at the state with the largest ratio of 2015 effect to preintervention RMSPE, our chances of correctly identifying California would be 26% (i.e., a 1 out of 4 chance of identifying the wrong state). Because a quarter of the donor pool produced larger RMSPE ratios than California for motor vehicle theft, California's RMSPE ratio is not an outlier. Therefore, Prop 47's estimated effect on motor vehicle thefts in California is likely to be a spurious result. In sum, although our findings identified nonzero Prop 47 effects for larceny and motor-vehicle thefts, only larceny appears to have an impact that is large relative to the unidentified variation observed in donor pool states.

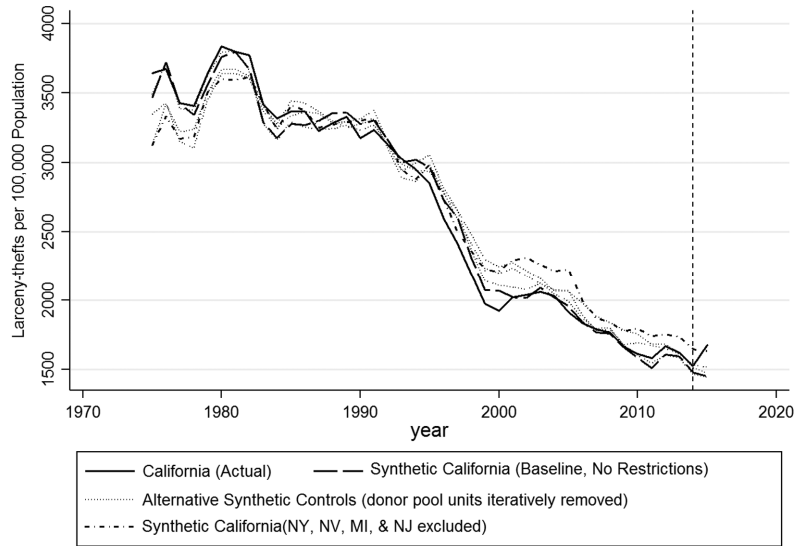
To determine whether the estimated larceny effect is sensitive to changes in Synthetic California's composition (i.e., different donor pool weights), we iteratively exclude the donor pool state with the greatest weight (ω) until all of the original donor pool states with nonzero weights have been removed. Synthetic California is composed of four donor pool states with weights that are greater than zero: New York, Michigan, Nevada, and New Jersey. The version of Synthetic California that results from this procedure is composed of a set of donor pool states that are entirely different than our original model. If the estimated impact of Prop 47 on California's crime rate persists under both compositions, we can be confident that our larceny estimate is not dependent on the contribution of certain donor pool states to Synthetic California. If our interpretation changes under Synthetic California's new composition, however, the estimated effect is dependent on the contribution of certain donor pool states and the finding should be interpreted cautiously.

The results of our Leave One Out sensitivity test are displayed in Figure 3. In addition to California and unrestricted Synthetic California (as seen in Panel B of Figure 1b), Figure 3 also displays a series of alternative specifications for Synthetic California as donor pool states are iteratively excluded (gray dashed lines). For larceny, we find that Synthetic California requires at least one of the following states be included in the donor pool in order to sustain the effect: New York, Michigan, Nevada, and New Jersey (the dashed red time series reflects Synthetic California when these four states are excluded from the donor pool). When these four donor pool units are excluded, the postintervention gap disappears. This suggests that our valid causal interpretation of the Prop 47 effect on larceny rests on the validity of including these four states in our donor pool. Thus, larceny, our only nonzero, nontrivial effect estimate, appears to be dependent on the contribution of four specific states from our donor pool. This finding, therefore, should be interpreted with caution.

To summarize our findings, although our initial synthetic control estimates suggested increases in larceny and motor-vehicle thefts after Prop 47's enactment, none of these effects

FIGURE 3

Leave One Out Plot for Larceny



survive both significance testing (randomization inference) and sensitivity testing. At the same time, null effects were identified for homicide, rape, robbery, aggravated assault, and burglary. Thus, we find no evidence of a statistically significant robust increase for any of the seven UCR index 1 offense categories in the year after Prop 47's enactment.

Conclusion and Discussion

This study represents the first systematic analysis of Prop 47's impact on violent and property crime rates throughout California in the year after the measure's implementation. With state-level panel data from 1970 through 2015, we employed a synthetic control group design to approximate California's crime rates had Prop 47 not been enacted. Our findings reveal that Prop 47 had no effect on homicide, rape, aggravated assault, robbery, and burglary. At the same time, we find that larceny and motor vehicle thefts appear to have increased moderately after Prop 47—yet these results are both sensitive to alternative specifications of our synthetic control group and are too small to rule out spuriousness. Overall, then, we find very little evidence to suggest that Prop 47 caused crime to increase in California.

The findings from our analysis have implications well beyond Prop 47 and California. Although Prop 47 is specific to California, the steps taken by the state to reform its criminal justice system are being closely watched by other states also confronting similar fiscal and

legal challenges related to overcrowding. As commentators have noted, “[P]olicymakers in different criminal justice systems across the country, from the federal courts down to the local justice systems, might be inspired to look in new directions” for criminal justice reform (Strutin, 2012: 1342). These states are asking whether the large-scale prison downsizing in California will compromise public safety or whether they can look to reforms such as Prop 47 as a possible solution to replicate in their own states. Even though speculation abounds, rigorous, high-quality scientific research has not been conducted; indeed, no scholarly empirical, peer-reviewed research on Prop 47 has been published since the measure’s enactment in 2014. As such, policy makers and the public lack the knowledge they need to make informed decisions about the futures of their criminal justice systems. The findings from this study begin to address this gap in knowledge.

Of course these findings should be interpreted within the context of the study’s potential limitations. First, although no other state enacted a sentencing reform that is wholly comparable to Prop 47 within our analysis timeframe, a diverse body of state-level sentencing reforms has been enacted across the United States since the 2008 financial crisis. It is likely that at least some states have enacted sentencing reforms that are comparable, in some part, to Prop 47. If Synthetic California is constructed with a donor pool unit that partially experienced a Prop 47-like intervention, both trends would reflect the impact of the shared aspect of Prop 47. The gap would then reflect Prop 47’s effect on the outcome beyond what was caused by the shared aspect of Prop 47, producing a more conservative estimate of the effect.

Second, even though our long preintervention time series (1970–2014) makes a spurious result less likely, our single postintervention observation (i.e., 2015) leaves us unable to assess whether Prop 47’s estimated effects are permanent, temporary, accruing, or decaying. As more postintervention observations become available, this question can be addressed through replication and extensions using updated time series.

Finally, anecdotal reports of Prop 47’s effect on crime often focus on increased drug offenses and other social ills (e.g., homelessness) after its enactment. These offenses, however, are not captured by our UCR Part 1 crime measures. Thus, further research is needed to address these claims.

Beyond these recommendations, nagging issues related to Prop 47 remain. For example, corrections spending in California remains high and continues to pose fiscal challenges for the state (California Department of Corrections and Rehabilitation, 2017). One anticipated benefit from Prop 47 is that the state will save money on corrections as a result of fewer individuals being sentenced to prison. These savings have not fully materialized. Still, despite greater original estimates, the state savings (\$67 million in 2016–17 and \$46 million in 2017–18) is to be redirected to local mental health and substance abuse programs, K–12 education, and services for victims of crime (Public Policy Institute of California, 2018). Given that money not spent on state prisons in the wake of Prop 47 is directed at increasing

evidence-based programming to reduce recidivism and overall incarceration, it is critical to determine how these investments will impact crime rates in the longer term.

Also, apart from Prop 47's impact on crime, some question how Prop 47 has impacted recidivism rates throughout the state. Prior to both Realignment and Prop 47, recidivism rates in California were quite high, as noted earlier. Unfortunately, they remain stubbornly high today, even as prison and state parole populations have dropped dramatically (Lofstrom et al., 2016). What explains this trend? And more to the point, what is the recidivism rate of Prop 47ers?

Finally, there is little doubt that our statewide analysis masks important variation at the local level. In particular, it is worth determining whether Prop 47's impact on crime (and recidivism for that matter) varies across California's 58 counties, each with different socioeconomic, demographic, and criminal justice profiles. Prior research findings on Realignment reveal that, in fact, its impact on crime and recidivism varies significantly by county (Bird and Grattet, 2016; Lofstrom and Raphael, 2016) so future research should be aimed at both documenting and attempting to explain this variation. A critical challenge here involves evaluating the effects of policy or practice changes across California counties under conditions of limited data (see Bird and Grattet, 2016).

Future directions aside, we conclude with a few comments regarding criminal justice reform more broadly—that is, both beyond California and beyond prison downsizing. Although reforms such as Realignment and Prop 47 have shown us we can, in fact, downsize our prisons without comprising public safety (see also Kubrin and Seron, 2016; Sundt et al., 2016), solutions to America's "crime problem" should not be limited to "back-end" efforts at reform, or efforts that focus solely on sentencing and incarceration. "Front-end" solutions—primarily those aimed at crime prevention—also deserve a seat at the table. Whether we're talking about civic participation, housing stability, strong police–community relations, poverty alleviation, drug and alcohol treatment, or addressing challenges related to homelessness and mental health, public health researchers and criminologists alike have long clamored for more attention to be directed toward prevention. Unfortunately, prevention routinely takes a back seat to efforts focused on punishment, which helps explain the incredible growth of incarceration in the United States (Travis, Western, and Redburn, 2014).

At the same time, we must resist the politicization of criminal justice reform. In the case of Prop 47, almost from the start, strong claims have been made regarding the measure's impact on crime rates throughout the state—in the absence of any data or analysis to back those claims up. Opponents routinely cite rising crime rates as "proof" that Prop 47 is harming public safety, prompting repeated calls to repeal the measure (LA Times Editorial Board, 2017). Yet crime rates going up (or down for that matter) tell us nothing about the source of those trends, and studies such as this one are necessary to determine any link between criminal justice reform and crime rates. Absent those studies, claims about a reform's impact should be strongly tempered.

In closing, the California case is instructive. As Petersilia (2016: 9) recently reminded us, “A crisis is a terrible thing to waste in that it allows you to get things done that you could otherwise not get done in a saner atmosphere.” Indeed, California witnessed such a crisis, which ultimately led to historic corrections reforms, including Prop 47. Although more research is necessary, initial findings from a handful of studies—including this one—suggest that these reforms are not associated with meaningful increases in crime. As the nation debates prison downsizing, clearly the experience of California must be front and center.

Appendix: Donor Pool Weights by Crime Type

State	State Name	Homicide	Rape	Aggravated Assault	Robbery	Burglary	Larceny	Motor Vehicle Theft
AL	Alabama	0	0	.108	0	0	0	0
AK	Alaska	0	0	0	0	0	0	0
AZ	Arizona	.124	0	0	0	.14	0	.335
AR	Arkansas	.039	0	0	0	0	0	0
CO	Colorado	.014	0	0	0	.098	.095	0
CT	Connecticut	0	0	0	.072	0	0	0
DE	Delaware	0	0	0	0	0	0	0
FL	Florida	0	0	.333	0	.063	0	0
GA	Georgia	0	0	0	0	0	0	0
HI	Hawaii	0	0	0	0	0	0	0
ID	Idaho	0	0	0	0	0	0	0
IL	Illinois	0	0	.21	.182	0	0	0
IN	Indiana	0	0	0	0	0	0	0
IA	Iowa	0	0	0	0	0	0	0
KS	Kansas	0	0	0	0	0	0	0
KY	Kentucky	0	0	0	0	0	0	0
LA	Louisiana	.098	0	0	.208	0	0	0
ME	Maine	0	0	0	0	0	0	0
MD	Maryland	0	0	0	.172	0	0	0
MA	Massachusetts	0	0	0	0	0	.019	.27
MI	Michigan	0	0	0	0	0	0	0
MN	Minnesota	0	0	0	0	0	0	0
MS	Mississippi	0	0	0	0	0	0	0
MO	Missouri	0	0	0	0	0	0	0
MT	Montana	0	0	0	0	0	0	0
NE	Nebraska	0	0	0	0	0	0	0
NV	Nevada	.156	.428	0	.067	.363	.479	.101
NH	New Hampshire	0	0	0	0	0	0	0
NJ	New Jersey	0	0	0	0	0	0	0
NM	New Mexico	0	0	0	0	0	0	0
NY	New York	.462	.572	.349	.225	.335	.406	0
NC	North Carolina	0	0	0	0	0	0	0

(Continued)

Continued

State	State Name	Homicide	Rape	Aggravated Assault	Robbery	Burglary	Larceny	Motor Vehicle Theft
ND	North Dakota	0	0	0	0	0	0	0
OH	Ohio	0	0	0	0	0	0	0
OK	Oklahoma	.039	0	0	0	0	0	0
OR	Oregon	0	0	0	0	0	0	0
PA	Pennsylvania	0	0	0	0	0	0	0
RI	Rhode Island	0	0	0	0	0	0	0
SC	South Carolina	0	0	0	0	0	0	0
SD	South Dakota	0	0	0	0	0	0	0
TN	Tennessee	0	0	0	0	0	0	0
TX	Texas	.067	0	0	.075	0	0	.295
UT	Utah	0	0	0	0	0	0	0
VT	Vermont	0	0	0	0	0	0	0
VA	Virginia	0	0	0	0	0	0	0
WA	Washington	0	0	0	0	0	0	0
WV	West Virginia	0	0	0	0	0	0	0
WI	Wisconsin	0	0	0	0	0	0	0
WY	Wyoming	0	0	0	0	0	0	0

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