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The Diminishing Returns of Incarceration: Evidence from California's Substance Abuse and Crime Prevention Act (SACPA)

THESIS

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by

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DEDICATION

To my parents, John and Sue Bartos, for the all of the love, support, and sacrifices they have made to provide me with the best education available.

To my mentor Dr. Richard McCleary, who has provided me with an unending supply of insights, enrichment, and attention that have been invaluable to my intellectual development.

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

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ABSTRACT OF THE THESIS

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Professor Richard McCleary, Chair

An inefficient reliance on incarceration as a means to reduce crime has led to massive costs and widespread demand for reform. However, criminal justice policymakers since the "Tough on Crime" era have been reluctant to support decarceration reforms out of fear that the reforms could be perceived to threaten public safety, and by extension, their chances at re-election. The current study examines a precursor to the national decarceration movement sparked by the 2008 Financial Crisis, California's Substance Abuse and Crime Prevention Act (SACPA). Using synthetic control group methods, this study evaluates whether SACPA threatened public safety or cost more than it saved, as critics predicted. The results suggest UCR Part 1 property crimes increased and aggravated assaults decreased in the years following SACPAs implementation. Although motor vehicle thefts and other property crimes increased following the intervention, almost no support was found for the claim that SACPA caused violent crime to increase and threaten public safety. The estimated increases in motor vehicle theft, burglary, larceny-theft, and robbery are translated into costs using both a conservative and a generous "cost-per-offense" metric. The combined cost of the estimated property crime increases from 2001 to 2006 amounts to less than a quarter of California's SACPA-related savings over the same time period. Thus, SACPA, a decarceration measure opposed by criminal justice policymakers because it would endanger public safety and cost more than it saved, resulted in substantial savings and no net effect on violent crime in the five years following its implementation.

INTRODUCTION

From 1980 to 2010, the total population of adults under correctional supervision in the United States increased sharply from 1.84 million to 7.09 million (Glaze & Kaeble, 2013). Following the 2008 financial crisis, the ballooning cost of incarceration became a focal point for budget-focused politicians and civil rights advocates alike. The emergence of the "Right on Crime" movement among congressional republicans, and the bipartisan support that emerged for the current national decarceration movement indicate just how important incarceration costs have become in post-recession American politics (Goode, 2013). This focus on correctional costs is at odds, however, with policymakers' responsibility for protecting public safety. Thus, policymakers are particularly interested in decarceration reforms which reduce correctional costs without threatening public safety.

Research on incarceration rates and crime suggest that high incarceration rates do, in fact, reduce overall crime occurrence (Liedka, Piehl, & Useem, 2006). However, James Q. Wilson famously stated in 1995 that we have hit the point of "diminishing returns" with incarceration (Wilson, 1983). In other words, Wilson argued that we have reached a level of incarceration where each additional incarcerated person produces a smaller crime-reducing effect than the previous one (Liedka, Piehl, & Useem, 2006). This concept is known formally as *marginal efficiency of investment* among economists.[1] John Maynard Keynes is credited with coining the term in arguing for the importance of profit expectations. If Wilson is correct, then continuing to expand America's net of incarceration would reduce crime rates, albeit at a diminishing rate of

[1] As the quantity of investment increases, the rates of return from it may be expected to decrease because the most profitable projects are undertaken first. Additions to investment will consist of projects with progressively lower rates of return. Logically, investment would be undertaken as long as the marginal efficiency of each additional investment exceeded the interest rate (Keynes, 1937).

return. This inefficiency has led to massive incarceration costs and the demand for reform nationwide (Brown, 2012; Gottschalk, 2010, 2011a, 2011b).

The demand for reform is met, however, with reluctance from many criminal justice policymakers. Since the Willie Horton scandal which is thought to have cost Michael Dukakis the 1988 presidential election (Edsall & Edsall, 1991; Tonry, 1994,1995), policymakers have faced the real possibility that they will not be re-elected if they support reforms that are perceived to threaten public safety (Greene & Mauer, 2010; Gottschalk, 2011; Brown, 2012). This fear has paralyzed many would-be reformers, as the potential for political blowback appears too great. This paradox is the unfortunate reality of criminal justice reform in the era of diminishing returns. Policymakers are aware that we are past the point of diminishing returns, but very few are willing to accept that these inefficient returns are worth relinquishing.

The 2008 financial crisis was a turning point in the public and political discourse surrounding mass incarceration (Greene & Mauer, 2010; Gottschalk, 2011; Brown, 2012), but this turning point, and the emergence of the contemporary decarceration movement, came long after the point of diminishing returns on incarceration. Following the 2008 financial crisis, reform efforts aimed at curbing and reversing the growth of America's incarceration system have been accepted more readily, as the public seems to acknowledge that continuing to increase our reliance on incarceration would be unsustainably costly.[2] However, decarceration efforts prior to the financial crisis were harshly scrutinized for their potential to endanger public safety (Garland, 2001).

[2] See California Proposition 36(2011), Proposition 47(2014), and New York's dismantling of "Stop and Frisk" (2013).

This article focuses on a precursor to the contemporary decarceration movement that illustrates the diminishing returns paradox, California's Proposition 36 (2000), or the Substance Abuse and Crime Prevention Act (SACPA). If America is long past the point of diminishing returns of incarceration, then one solution would be to scale back incarceration among the lowest levels of offenders. California's SACPA attempted to do this through the mandatory diversion of non-violent drug offenders to treatment programs instead of incarceration. The ballot measure was popular among voters, but viciously opposed by many politicians and criminal justice stakeholders that claimed it would cause crime increases, weaken drug courts, and threaten public safety. The analysis that follows addresses two issues of practical importance. First, did SACPA's implementation threaten public safety by increasing levels of violent crime in California? Second, did the state of California save more through reducing its reliance on incarceration than it cost to implement SACPA?

As state and local governments explore ways to reduce the cost of incarceration, an evaluation of SACPA will enable policymakers to make evidence-based decisions about drug reforms and recidivism without compromising public safety. In response to chronic prison overcrowding, California legislators enacted "Public Safety Realignment Act" which shifted responsibility for incarcerating, monitoring, and tracking low-level felons from state prisons to county jails starting on October 1st, 2011. Realignment delegated a great deal of discretion to county governments to decarcerate those previously sent to state prisons (Schlanger, 2013). Counties were not discouraged from relying entirely on local jails to absorb the "realigned" population, but they were also free to explore alternatives to incarceration (e.g. electronic monitoring, house arrest, etc.). Research suggests "Realignment" resulted in a net decrease in prison and jail admissions state-wide in the year following its implementation; however, many

counties simply absorbed this displaced population into county jails (Lofstrom & Raphael, 2013a). Although it has been suggested that motor vehicle thefts increased over this decarceration period, violent crimes and other property crimes did not (Lofstrom & Raphael 2013b). Verma's (2016) findings suggest that historical imprisonment trajectories, jail capacities, and local demographic factors affected counties' likelihood to expand their use of alternatives to incarceration or simply absorb the displaced population into local jails. As policymakers in California's 58 counties explore avenues for decarceration that do not threaten public safety, SACPA's net savings and impact on public safety at the state-level will enable county policymakers to make evidence-based decisions about decarceration, public safety, and finances.

Using a state-level panel dataset, a synthetic control group is constructed to approximate a "Counterfactual[3]" California.[4] The difference between California's observed and counterfactual trend can, under a set of assumptions discussed below, be interpreted as the causal effect of the intervention of interest on the dependent variable (UCR Index 1 offense categories) in California. Taking an agnostic stance on the types of crime that would be impacted by SACPA as well as the mechanism causing crime(s) to increase, the analytic procedure is applied to each Index 1 UCR offense category.

Counterfactual estimates are then compared to multiple error terms that vary the risk of Type 2 error. Additionally, the findings are evaluated for spuriousness using in-sample placebo tests and robustness to changes in the composition of synthetic or "Counterfactual" California. The estimated effects of SACPA on Index 1 offense categories are then compared to the savings associated with its enactment estimated by Urada et al; (2008). A cost-benefit analysis follows

[3] In the context of SACPA, "counterfactual" California represents what would have occurred in the absence of the policy enactment (SACPA).

[4] See Abadie et al. (2003, 2004, 2010, 2011, 2015)

from the counterfactual inquiry. The article concludes with a brief discussion about diminishing returns and the politically intransigent nature of criminal justice reform.

PROPOSITION 36: SUBSTANCE ABUSE AND CRIME PREVENTION ACT (SACPA)

In November 2000, California voters passed the Substance Abuse and Crime Prevention Act (SACPA), otherwise known as Proposition 36. Under SACPA, adults convicted of nonviolent drug offenses in California who meet eligibility criteria would be diverted to probation with substance abuse treatment instead of probation without treatment or incarceration.[5] Levels of care may include drug education, regular and intensive outpatient drug-free treatment, short- and long-term residential treatment, and narcotic replacement therapy (typically methadone for clients whose primary drug is heroin).[6] Previous diversion programs focused on first-time offenders; however, SACPA made diversion to drug treatment compulsory for all non-violent drug offenders. SACPA's provisions provide local governments with $120 million per year to fund the expansion of community treatment programs. This cost is offset by the savings from restricting the use incarceration (Ehlers & Ziedenberg, 2006; Urada et al., 2008).

SACPA was popular among voters, largely because of its expected fiscal impact. In the Official Voter Information Guide, SACPA was estimated to result in "annual savings of $100 million to $150 million to the state and about $40 million to local governments" (California Secretary of State Elections Division, 2000, pg.25). The measure passed in November, 2000, with 61% of voters approving.

[5] Offenders on probation or parole who commit nonviolent drug offenses or who violate drug related conditions of their release may also obtain treatment under Proposition 36. Offenders who commit non-drug violations on probation/parole may face termination from Proposition 36.

[6] Consequences of drug violations depend on the severity and number of such violations. The offender may be assigned to more intensive treatment, or probation/parole may be revoked.

Although SACPA's fiscal impact made the measure popular among voters, it met predictable resistance. Prior to voter approval, criminal justice practitioners and stakeholders launched an opposition campaign claiming that "Proposition 36 prohibits jail for persons convicted of using heroin, crack, PCP and other illegal drugs, or for possessing 'date rape' drugs—even those with prior convictions for rape, child molesting and other violent crimes. Proposition 36 has no regulatory safeguards, cripples legitimate treatment, invites fraud and endangers public safety" (California Secretary of State Elections Division, 2000, pg. 27). Calling upon the usual suspects for opposition to criminal justice reform, the coalition of politicians, practitioners, and stakeholders opposing SACPA claimed that it would have many unintended consequences, threaten public safety, and cost, rather than save, money.[7]

In the years following SACPA's passage, various cost-benefit analyses, evaluations of its impact on crime, and audits of the treatment process have been conducted (Orange, 2003; Ehlers & Ziedenberg, 2006; Urada et al., 2008); however, these studies have either been limited to the effect of SACPA on crime rates in certain counties (Orange County Grand Jury, 2003) or descriptive analysis of state-wide trends (Urada et al., 2008). Evaluations conducted by Urada et al (2008), suggest that offenders who successfully complete drug treatment under SACPA were less likely to be arrested in the future. However, a significant proportion of offenders diverted under SACPA failed to complete the mandated drug treatment programs. Therefore, Urada's

[7] Proposition 36 was also opposed by Attorney General Bill Lockyer, U.S. Senator Dianne Feinstein(D-CA), LA County Sheriff Lee Baca, LA County District Attorney Gil Garcetti, The Los Angeles Times, and Phoenix House - the nation's largest treatment provider. Senator Feinstein was joined by Congressman Cal Dooley (D-Kings County), and public safety professionals and crime victim's advocates from throughout California - including President and members of the California Sheriffs Association, President and members of the California District Attorneys Association, members of Crime Victims United of California, members of the California Association of Drug Court Professionals.

(2008) finding does not speak to the state-wide net-effect of SACPA on non-drug crimes. The current study utilizes state-wide panel data and synthetic control methods to evaluate the causal impact of SACPA's implementation on public safety in California via changes in Index 1 UCR offense incidence.

METHODS

To estimate the effect of an exogenous intervention on a treated unit, a control unit is necessary. A comparison between California and the national time-series of crime rates post-intervention would not yield interpretable findings because it is unknown whether the difference in crime rates was caused by the intervention[8] or some other factor. Propensity score matching can be utilized to select the optimal comparison unit for difference-in-difference designs based on pre-intervention characteristics. However, the optimally similar control unit available in a donor pool of non-treated controls may not be sufficiently similar to the treated unit across the pre-intervention period for causal inferences about the treatment effect to be drawn. To overcome this limitation, California's crime rates are compared to a weighted combination of "donor pool" states chosen to optimally match California's pre-intervention crime trends.

The donor pool is composed of states that did not enact diversion policies for drug offenders in either the pre-intervention matching or post-intervention estimation period. Comparison units are meant to approximate the counterfactual of California had the intervention

[8] The treatment (SACPA) was experienced by both California and a portion of "the nation". If California were the only state to receive the treatment this would still violate the Stable Unit Treatment Value Assumption (SUTVA). SUTVA requires that the treatment applied to the unit of interest does not impact the outcome variable in untreated states. See Appendix A for more on SUTVA.

(i.e., SACPA) not occurred,[9] therefore, the donor pool must be limited to states that have not adopted similar diversion policies. Eleven other states enacted compulsory diversion to treatment programs for drug offenders between 1970 and 2007[10] (VanderWaal et al., 2006; Gardiner *et al.*, 2012), leaving a 38 state donor pool from which a Counterfactual California can be constructed. By limiting the donor pool to states that have not enacted compulsory diversion programs for drug offenders, the synthetic control unit constructed from the donor pool represents the absence of the treatment across contributing units and in-aggregate.

Valid causal inference assumes, nevertheless, that the actual California and "Counterfactual California" time series have identical trends prior to SACPA, and that the trends would have continued absent SACPA. Two time series are co-integrated if they share a set of causal forces that keep them on a common path. An *ideal* control time series is one that is co-integrated with the treated time series prior to the intervention (McCleary, McDowall, & Bartos, *in press*). The intervention changes the set of causal forces determining the path of the treated time series and sends it off in a new direction. Thus, the intervention destroys the co-integrated relationship, and the distance between the diverging time series throughout the post-intervention period is interpreted as the causal effect of the intervention. In most instances, an ideal control time series is not available in nature[11]. When an ideal control time series cannot be found, a synthetic control group can be constructed to *approximate* an ideal control time series using a weighted combination of the donor pool states (McCleary, McDowall, & Bartos, *in press*).

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[9] For more detail on the Synthetic Control Group method and its Stable Unit Treatment Value assumption, see Appendix A.

[10] AL, AZ, CT, FL, HI, ID, IN, KS, MD, MA, MO, TX, WA were dropped.

[11] There is no individual state or country that perfectly mimics California's pre-intervention crime trends.

Abadie et al. (2010) employ a similar strategy to examine Prop 99, a tobacco control program implemented in California during 1988. Applied to SACPA, a synthetic California control time series is constructed from a set of weights that minimize the distance between the two time series on pre-SACPA levels of the outcome of interest. The algorithm is described in Abadie, Diamond and Hainmueller (2010; 2011). Here we describe the larger process which consists of four steps: devising a donor pool of possible controls; constructing the synthetic control time series; estimating the causal effect of SACPA; and performing post-estimation tests for spuriousness and rigorousness to changes in the synthetic unit's donor pool weights. A more complete econometric description of the method can be found in Appendix A.

DATA

The study employs an annual state-level dataset containing frequencies of all FBI Uniform Crime Report Index I offense categories from 1970 through 2014. Proposition 36 was passed in November of 2000 and took effect on July 1, 2001. Since the list of SACPA-like reforms nationwide reported by VanderWaal *et al.* (2006) and Gardiner *et al.* (2012) ends in 2007, the effect estimates are limited to the 2001 through 2007 time-period. 13 states enacted similar compulsory diversion programs for drug offenders between 1970 and 2007. These states are excluded from the donor pool of possible control states. (See footnote 11 above).

DEPENDENT VARIABLES

To empirically evaluate the effect of SACPA's enactment on crime and public safety, synthetic control group models are constructed for each Index 1 FBI Uniform Crime Reports offense category [12]. These offenses include: homicide, rape[13], aggravated assault, robbery,

[12] The present analysis is limited to Index 1 offense categories -- a convention in the field of criminology -- because Index 1 offenses are thought to occur with relative regularity across the county, are more likely to be known/reported to the police, and are more serious crimes compared to the Index 2 offenses.

burglary, larceny-theft, and motor vehicle-theft. These observed crime totals are log transformed and "Rate of Change" growth/decay factors are calculated by dividing the natural log of yearly crime frequencies ($\ln(Y_t)$) by the natural log of each state's 1970 frequency ($\ln(Y_{70})$). These transformations allow states of different sizes and levels of crime to be compared in terms of change from a historical baseline, or a growth factor.

Standardizing annual crime frequencies into per capita rates controls for population differences but adds year-to-year unwanted variation (i.e. year-to-year census population estimates[14]). California is the most populous state in the U.S. and leads the nation annually in most offense categories. Unless a method of standardizing by state size is employed, no suitable synthetic control can be constructed because no state (or combination of states for that matter) can approximate the state with the highest crime frequency in the U.S.

In the event that our estimates suggest that SACPA had a positive effect on one or more violent crimes (homicide, rape, aggravated assaults, and robberies), it would suggest that SACPA indeed threatened public safety in the years following its enactment. This would be a surprising result, as there is no reason to suspect that a sentencing policy change from which violent

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[13] For UCR rape, the optimal weighting scheme for synthetic California did not reproduce treated California's pre-intervention trend sufficiently to allow for counterfactual comparison. Therefore, it has been omitted from the results. However, the analysis is available upon request to the author at bbartos@uci.edu.

[14] Annual population estimates at the state level are subject to a host of biases such as homelessness, undocumented immigration, and (monthly/weekly/daily) transience. These factors are also known to affect crime rates in complex ways (such as reduced community cohesiveness, diminished guardianship, or scarcity of conventional support resources) to an unknown magnitude. The error in census estimates is therefore related to the variance in crime rates in an unknown, but non-zero manner. To avoid conflating the known biases of UCR crimes known to the police with errors in state population estimates, a growth factor with states' 1970 levels of the offense category as a baseline can be used as an alternative.

offenders are exempt would affect levels of violent crime in California. It would be much less surprising, however, if the results indicate that one or more categories of property crime (burglary, larceny-thefts, and motor-vehicle thefts) increased following SACPA's enactment. In the event our results suggest one or more property crimes increased following SACPA, the size of these estimated increases will be compared to the savings generated by reduced drug offender admissions (Urada et al., 2008; Orange, 2003; Ehlers & Ziedenberg, 2006). If the analysis shows that one or more of these crimes decreased following SACPA, it would indicate that the policy had unintended, salutatory consequences.

PREDICTOR VARIABLES

There are two conventional approaches to the choice of predictor variables in synthetic control models. The first approach includes proxy variables for characteristics and factors thought to causally influence the outcome of interest. This requires (1) perfect knowledge of the causal nexus for the outcome of interest, and (2) the assumption that the relationship between the predictors and the outcome of interest is uniform across states, (3) years, and (4) offense categories. Alternatively, Abadie et al. (2010) describe a "data-driven" approach using pre-intervention levels of the outcome of interest as predictors. The data-driven approach does not assume a static relationship between predictors and the outcome of interest across states, time or offense categories. Further, it does not require perfect knowledge of the causal nexus of factors affecting the outcome of interest.

This study adopts a data-driven approach using each pre-intervention level of the outcome of interest as a predictor (1970-2000). In addition, first-difference scores between the outcome of interest and the previous year's lagged observation (1971-2000) are included as predictors (McCleary, McDowall, & Bartos, 2017). The inclusion of first-difference scores essentially fits a quadratic function to each observation, adding information without sacrificing

the flexibility gained from the data-driven approach. Our data-driven approach to selecting donor pool weights ($W_1+W_2+\ldots Wn=1$)[15] produces synthetic controls with less pre-intervention error than the conventional method under most circumstances (McCleary, McDowall, & Bartos, *in press*). The amount of pre-intervention error for a synthetic control produced using the data-driven approach is directly related to the number of inflection points and the magnitude of variance in the dependent variable time-series prior to the intervention.

To determine whether an estimated effect can reasonably be attributed to something other than our failure to adequately represent California throughout the pre-intervention period, the most basic approach is to compare the estimated effect to the model's Root Mean Squared Prediction Error (RMSPE). RMSPE measures the magnitude of the gap in the outcome variable between each state and its synthetic counterpart. A large post-intervention gap is not indicative of a causal effect if the synthetic control does not closely reproduce the outcome of interest prior to the intervention. That is, a post-intervention gap between the treated unit and its synthetic counterpart cannot be attributed to anything beyond matching error unless the estimated effect is larger than the pre-intervention RMSPE.

RESULTS

For each offense category, synthetic California's optimal weighting scheme can be found in Table 1 of Appendix B[16]. Figure 1 displays the offense trajectories of California(solid line) and its synthetic counterpart(dashed line) for the 1970-2007 period. For each offense category, except homicide, the treated and synthetic trends do not diverge substantially during the pre-intervention period (i.e. 1970-2000), they diverge markedly following SACPA's July 2001

[15] Where n is the identifier for donor pool states and W is the weight applied to the donor pool unit contributing to Synthetic California.

[16] Note that for each offense the donor pool weights sum to one.

implementation, and the synthetic trends do not exhibit jagged spikes or trend shifts in the post-intervention period (i.e. 2001-2006) that would indicate an overfitting problem[17]. For homicide, however, the pre-intervention gaps between the treated and synthetic trends are greater in magnitude than the post-intervention gap (individually and on average), and the synthetic trend exhibits jagged sign shifts in the post-intervention period signalling the potential for overfitting.

PROPERTY CRIMES

Motor Vehicle Thefts

Following SACPA's implementation in July 2001, the treated and synthetic lines begin to diverge markedly. Synthetic California's motor vehicle theft trend continued to decline, while California's actual number of motor vehicle thefts increased in each year between 2001 and 2006 before resuming its decline. The post-intervention gap between synthetic and actual California suggests that SACPA's implementation led to a moderate increase in motor vehicle thefts.

Table 1. Synthetic Control Group Results and Effect Estimates for the Impact of SACPA's Implementation on levels of UCR Offenses in California

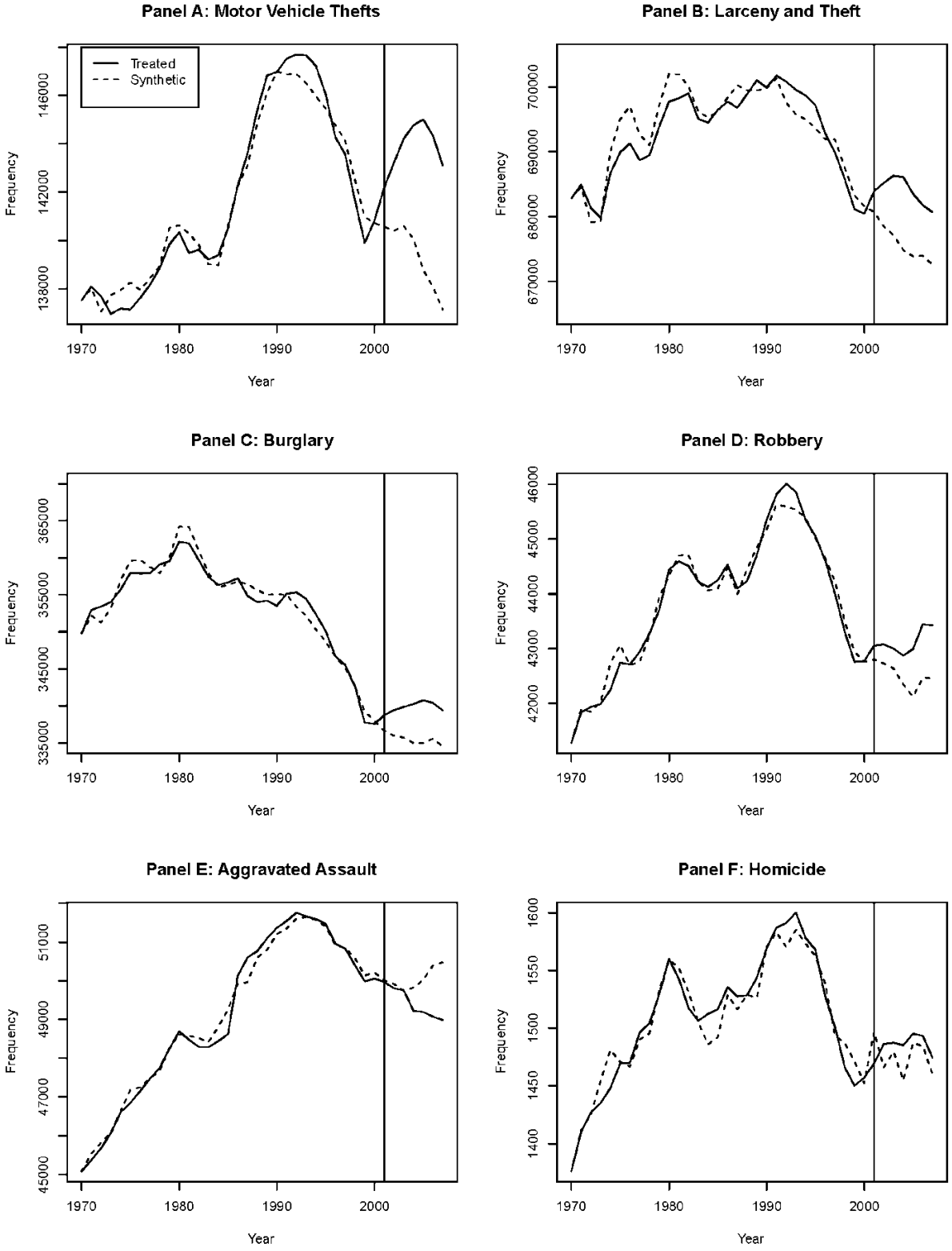
Year	Root Mean Squared Prediction Error (RMSPE)	Post-SACPA difference: total (mean)	Post-SACPA difference beyond RMSPE: total (mean)	Greatest preintervention residual	Post-intervention effect beyond greatest residual: total (mean)
Motor Vehicle Thefts	630.6	31238 (4463)	+26821 (+3832)	1263	+22397 (+3200)
Larceny-thefts	2660.4	55843 (7978)	+37220 (+5318)	5733	+18291 (+2613)
Burglary	1308.5	30383 (4340)	+21223 (+3032)	2277	+14665 (+2095)
Robbery	187	4298 (614)	+2989 (+427)	492	+1367 (+195)
Aggravated Assaults	222.7	4443 (635)	-3400 (-486)	636	-1784 (-255)
Homicides	13.4	61 (8)*	+25 (+3.5)*	33	No ± effect beyond error

*Note: *within pre-intervention error range*

As Table 1 shows, the average post-SACPA effect (+4,463) is greater than the model's pre-SACPA RMSPE (±630.6). The gap between treated and synthetic trends is larger than the pre-SACPA RMSPE in each year post-intervention year as well as on average. The largest pre-intervention error was 1,263 in 1998 and the average post-intervention increase was 4,463 motor vehicle thefts, or 3.53x the size of the greatest error. SACPA's implementation is associated with an annual increase in CA motor vehicle thefts between 3,832 and 5,094 from 2001 to 2007.

[17] "Overfitting" arises when the characteristics of the unit affected by the intervention or event of interest are artificially matched by combining idiosyncratic variations in a large sample of unaffected units.

Figure 1. Synthetic control group plots for Index 1 UCR offenses



Note: The vertical line in 2001 represents SACPA's implementation year.

Larceny

Following SACPA's implementation in July 2001, the treated and synthetic lines begin to notably diverge. Synthetic California's larceny trend continued to decline, while California's actual number of larceny-thefts increased from 2001 to 2004 before resuming its decline. The post-intervention gap between synthetic California and actual California suggests that SACPA's implementation led to a minor and temporary increase in larceny-thefts. In fact, by 2006 California's frequency of larceny-thefts fell below its 1970 frequency. As Table 1 shows, the average post-SACPA effect (+7,978) is greater than the model's pre-SACPA RMSPE (±2660.4). SACPA's implementation is associated with an increase in California larceny-thefts between 5,318 and 10,638 annually from 2001 to2007.

Burglary

Following SACPA's implementation in July 2001, the treated and synthetic lines begin to notably diverge. Synthetic California's burglary trend continued to decline, while California's actual number of burglaries increased from 2000 to 2006 before resuming its decline. The post-intervention gap between synthetic California and actual California suggests that SACPA's implementation led to a temporary increase in burglaries. As Table 1 shows, the average post-SACPA effect (+4,340) is greater than the model's pre-SACPA RMSPE (±1308.5). From 2001 to 2007 the gap between treated and synthetic trends is larger than the pre-SACPA RMSPE in each year as well as on average. SACPA's implementation is associated with an increase in California burglaries between 2,095 and 3,032 annually from 2001 to 2007.

VIOLENT CRIMES[19]

Robbery

Following SACPA's implementation in July 2001, the treated and synthetic lines begin to notably diverge. Synthetic California's robbery trend continued to decline through 2005, while California's actual number of robberies increased from 1999 to 2002 and 2004 to 2006. The post-intervention gap between synthetic California and actual California suggests that SACPA's implementation led to a minor and temporary increase in robberies. The actual and synthetic trends both spike in 2006, possibly reflecting the temporary impact of SACPA on robbery levels. As Table 1 shows, the estimated effect averaged across the post-intervention period (+614) is greater than the model's pre-intervention RMSPE (±187). From 2001 to 2007 the gap between treated and synthetic trends is larger than the pre-SACPA RMSPE in each year as well as on average. Overall, SACPA's implementation appears to have caused an increase in California robberies between 427 and 801 annually from 2001 to 2007.

Aggravated Assault

Following SACPA's implementation in July 2001, the treated and synthetic lines continue to track closely and do not diverge until 2004. The magnitude and stability of the post-2004 divergence may signal a delayed impact of SACPA's implementation on UCR aggravated assaults. Both treated and synthetic California's aggravated assault trend continued their pre-intervention decline through 2003. From 2004 to 2006, however, California's actual level of aggravated assaults declined while its synthetic counterpart sharply increased. The post-intervention gap between synthetic California and actual California suggests that SACPA's

[19] For UCR rape, the optimal weighting scheme for synthetic California did not reproduce treated California's pre-intervention trend sufficiently to allow for counterfactual comparison. Therefore, it has been omitted from the results. However, the analysis is available upon request to the author at bbartos@uci.edu.

implementation led to a delayed but substantial decrease in UCR aggravated assaults. As Table 1 shows, the estimated effect averaged across the post-intervention period (+635) is greater than the model's pre-intervention RMSPE (±222.7). SACPA's implementation is associated with a decrease in California aggravated assaults between -485 and -858 annually from 2001 to 2007.

Homicide

Following SACPA's implementation in July 2001, the treated and synthetic lines continue the pre-intervention pattern of year-to-year swings from positive to negative difference. The magnitude and sign- instability of the post-intervention differences do not suggest a stable of fixed effect of SACPA's implementation on UCR homicides. Although California's level of UCR homicides increased in 1999, ending a precipitous decline beginning in 1993, the synthetic trend does not consistently fall above or below California's actual homicide level post-intervention. As Table 1 shows, the estimated effect averaged across the post-intervention period (+8.7) is not greater than the model's pre-intervention RMSPE (±13.38). The post-SACPA gap was larger than the RMSPE in 2001 and 2004, but these gaps are opposite in sign. Therefore, any conclusions that can be drawn about the effect of SACPA's implementation on levels of UCR homicide in California appear to be limited by our inability to sufficiently mimic California in the pre-intervention period.

In the following section, SACPA's estimated impacts on levels of UCR offenses are tested for spuriousness (likelihood of estimating an effect of equal or greater magnitude if treatment condition is assigned at random) and "overfitting" (robustness of the estimated effect to changes in donor pool weights).

PLACEBO TESTS AND ROBUSTNESS CHECK

To evaluate the likelihood than a synthetic control group estimate reflects the impact of the intervention of interest or merely a spurious correlation, Abadie *et al.* (2003, 2010, 2015)

employ "in-sample placebo tests" to determine the probability of finding an effect with an equal or greater ratio of post-intervention effect to pre-intervention error in non-treated donor pool states. An in-sample placebo test is performed by iteratively reassigning the treatment condition to each donor pool[20]. Abadie *et al.* (2003, 2010, 2015) use a ratio of post-intervention effect over pre-intervention error to express the magnitude of the estimated effect per unit of pre-intervention error[21].

The in-sample placebo test ranks are listed in Table 2 of Appendix B. SACPAs estimated effect on motor vehicle thefts per unit of pre-intervention error was greater than any state in the donor pool. For motor vehicle thefts, California's post-intervention gap is about 8 times larger than the pre-intervention gap. If a state was picked at random from the sample, the probability of obtaining a ratio as high as California's would be 1/37≈0.027. Robbery (2/37≈0.054), Burglary (5/37≈0.135), Aggravated Assaults (6/37≈0.162), and Larceny-thefts (9/37≈0.243) also ranked highly. For homicide, however, California's ratio of post-intervention to pre-intervention gap did not rank highly and should be interpreted with extreme caution, if at all.

[20] It is worth noting that these "in-sample" placebo tests can be incredibly computationally intensive, particularly when a large number of predictor variables are included and/or a sizeable donor pool is available. For each offense in this study, 59 predictor variables and 38 donor pool states were included in the optimization algorithm. The in-sample placebo test for each offense demanded ~100 Gigabytes of physical memory and took up to a week to complete, barring a crash due to a lack of RAM. In order to work around these computational requirements, the University of California's High-Performance Computing Cluster was employed. A special thanks to Harry Mangalam for his technical support. For more on the computational and technical demands of these tests, contact the author.

[21] There are cases, however, where a visually weak synthetic control group model appears strong by this metric. Effects of varying sign and large magnitude are not penalized by this metric.

LEAVE ONE OUT TESTS

Whereas the in-sample placebo test in the section above examined the evaluated the likelihood that an effect of equal or greater size could be found spuriously (via random assignment), the "Leave One Out" test below is used to evaluate the robustness of the estimated effects to changes in W donor pool weights. The contributing donor pool units are iteratively removed (from greatest weight to smallest) to determine if the estimated effect is dependent on the contribution of a certain state (or group of states).

By iteratively removing a state in the donor pool with the greatest weight, the model sacrifices goodness-of-fit; but if the effect estimated with the original contributing states excluded from the donor pool is same in sign and similar in magnitude to the original model then it is reasonable to eliminate "overfitting" as an alternative explanation for the estimated effect.

Figure 2 contains the "leave one out" test results for aggravated assault, robbery, burglary, larceny-thefts, and motor vehicle thefts[22]. The new donor pool weights for these offenses with the original matching donor states excluded are listed in Table 3 of Appendix B. In each time-series, California's actual level of the offense is represented by a solid black line, the counterfactual constructed prior to restricting the donor pool is the dashed black line, and the five grey dashed lines represent the constructed counterfactual with the one through four of the greatest contributing donor pool states as well as all original contributing states. In other words, the grey lines represent the range of possible values that counterfactual California would fall between if the donor pool were restricted differently. If the difference between the grey and solid black lines has the same sign and similar magnitude to the difference between the dashed and

[22] Leave one out robustness tests were performed but not reported for homicide and rape due to the weakness of the synthetic controls prior to the exclusion of any donor pool units. Thus, the leave one out test would only demonstrate how a bad model gets worse as the donor pool is restricted.

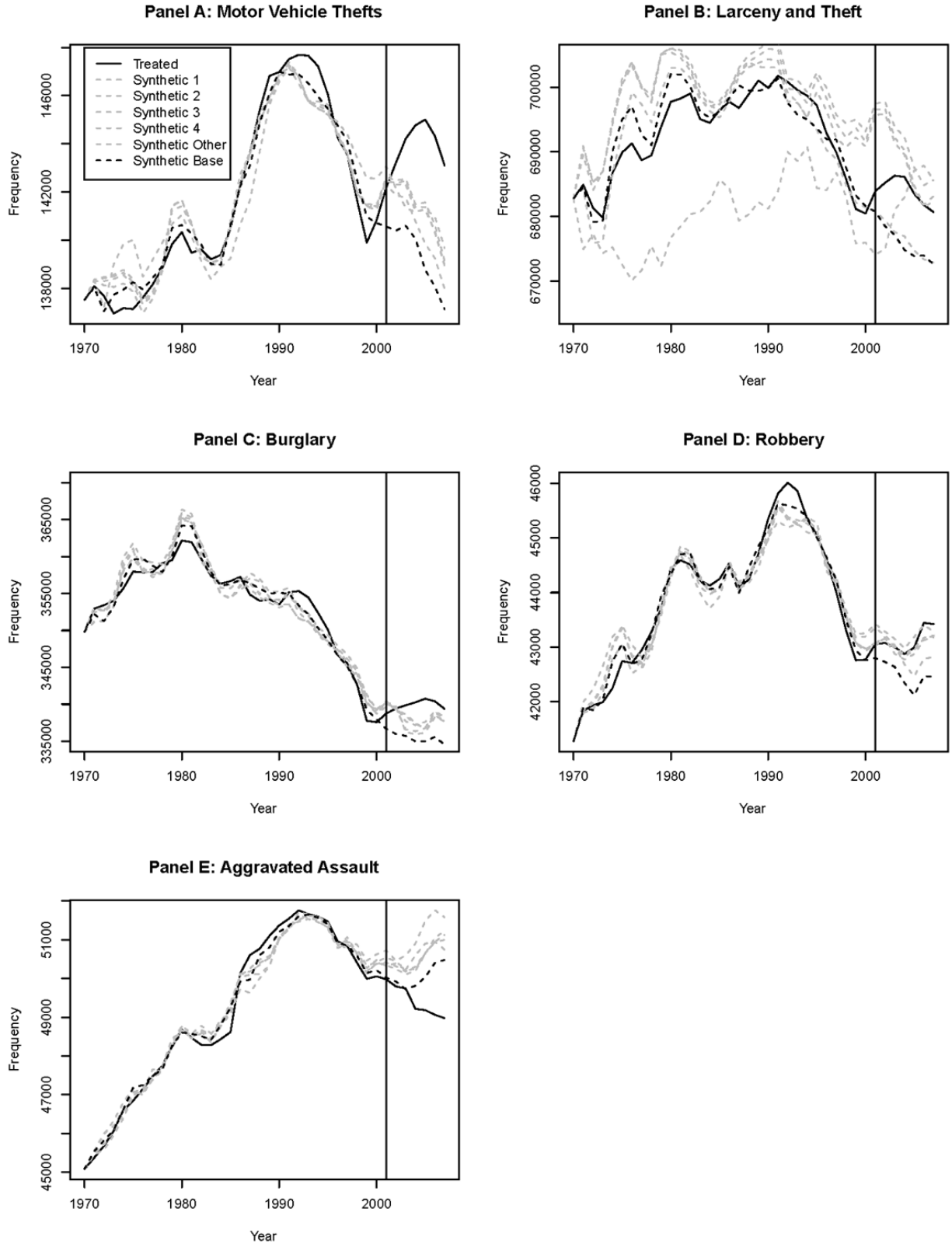
solid black lines, it would suggest that the estimate is not being driven by the contribution of a particular donor pool state (or group of states) to California's synthetic control.

Aggravated assault, burglary, and motor-vehicle theft did not change in sign and remained largely stable in magnitude as the donor pool states contributing to the original counterfactual were removed. For these offenses, counterfactual California can be constructed without using any of the original donor pool states and the interpretation of SACPA's effect remains the same. The sign and magnitude of these estimates are robust to changes in counterfactual California's composition.

Panel D of Figure 2 demonstrates how changes in the composition of synthetic California produce results with varying interpretations. Originally, the synthetic control group plot for robbery indicated a moderate and stable increase following SACPA. As the states that compose the original synthetic control are removed from the donor pool one-by-one, the resulting synthetic trends produce effect estimates that vary in both sign and magnitude. The estimated increase in robbery due to SACPA should therefore be interpreted with caution, as the effect is not robust to changes in synthetic California's composition.

The leave one out plot for larceny-thefts suggests that the effect is robust to the exclusion of the greatest contributing donor state as well as the top four contributing donor states. However, when all of the original contributing states are excluded from the donor pool, the estimate changes both sign and magnitude. Although the effect on larceny-thefts is robust to the exclusion of some (even many) of the originally contributing donor states, it does appear to be dependent on the contributions of particular donor pool states.

Figure 2. Leave one through originals out results plots



Note: The vertical line in 2001 represents SACPA's implementation year.

COST EFFECTIVENESS

The question remains, however, whether SACPA was cost-effective. In the years following SACPA's passage, various cost-benefit analyses, evaluations of its impact on crime, and audits of the treatment process have been conducted (Urada et al., 2008; Orange, 2003; Ehlers & Ziedenberg, 2006). A cost-benefit analysis conducted by Ehlers and Ziedenberg (2006) at the Justice Policy Institute, estimated that SACPA saved the state of California approximately $926,006,362 from 2001-2006. Table 2 breaks down the savings estimated by Ehlers and Ziedenberg (2006), into four categories: prison beds made available through reductions in drug offender admissions, savings on jail beds due to compulsory diversion to treatment and probation for drug offenders, savings from construction projects that were delayed or abandoned due to reduced admissions, and savings from prison closures made possible by declining drug offender populations.

Table 2. SACPA's Estimated Savings from Ehlers & Ziedenberge (2006)

Category	Savings
Prison Beds Saved	$ 357,406,362
Jail Beds Saved	$ 62,708,736
Planned Construction Projects	$ 475,000,000
Prison Closings	$ 31,600,000
Subtotal: Estimated savings due to SACPA, 2001-2006	$ 926,715,098

Ehlers and Ziedenberg (2006) estimate that SACPA's implementation saved the State of California $357,406,362 in prison beds and $62,708,736 in jail beds net of its implementation costs between 2001 and 2006 via reductions in drug offender admissions. Falling drug offender admissions further enabled the state to delay construction on five new prison facilities (saving the at least $450,000,000[23]) and close the Northern California Women's Facility (saving the state

[23] Legislative analyst's review of the Substance Abuse and Crime Prevention Act of 2000, File No. SA 1999 RF 0040, Amendment No. 2-NS, December 6, 1999, p. 5.

$31,600,000). In total, the Justice Policy Institute estimates that SACPA saved the state of California approximately $926,715,098 net of its implementation costs from 2001-2006.

Although it appears that SACPA did, as proponents claimed, save the state of California close to a billion dollars from 2001-2006 via reduced incarceration costs; the estimated property crime increases associated with SACPA's implementation incurred costs that Ehlers and Ziedenberg did not include in their analysis. To determine if the property crime increases associated with SACPA from 2001-2006 cost the state more than it saved over this period, the average dollar value of property taken during motor vehicle thefts, burglaries, robberies, and larceny thefts (as reported in the FBI's Uniform Crime Reports 2001 through 2006) are used as a conservative estimate of the per-offense cost of each crime (footnote about range of estimates). The per-offense cost in each year is multiplied by SACPA's estimated effect in that year to get an estimate of the costs incurred by SACPA's impact on levels of crime in California. These calculations can be found in Table B4 of the appendix. Although this measure serves as a useful heuristic for comparing the magnitude of SACPA's costs and savings, it does not account for the costs of crime prevention programs, the fear and suffering of the victim, and many other potential costs to society (see Cohen, 2004; Cohen *et al.*, 2004; McCollister *et al.*, 2009). Economists and criminologists have produced a number of studies attempting to estimate the economic impact of criminal activity, but the estimates reported in studies of this type vary wildly as new factors are included and excluded.

To capture the range of existing cost-per-offense estimates in the literature, the estimated property crime increase in each post-intervention year is multiplied by both the "value of damaged/stolen property per offense" estimate reported annually in the FBI's Uniform Crime Report from 2001 to 2006, and the combined "tangible" and "intangible" costs per offense

estimated by McCollister *et al.* (2009). Although neither perfectly represents the innumerable costs crime incurs to both victims and society, they illustrate the range of existing scholarly estimates at the conservative low end(Uniform Crime Report 2001, 2002, 2003, 2004, 2005, 2006) and generous high end (McCollister *et al*,. 2009).

Table 3. Cost Estimates for post-SACPA Crime Increases

Offense Category		Uniform Crime Reports 2001-2006 (low)	McCollister et al., 2009 (high)
Motor Vehicle Thefts	Effect beyond RMSPE 2001-2006	24,892	24,892
	Cost-per-offense	6,512.3 (avg)[a]	10,772
	Cost of estimated effect 2001-2006	130,272,730	268,136,624
Larceny-Thefts	Effect beyond RMSPE 2001-2006	31,810	31,810
	Cost-per-offense	745.5 (avg)[a]	3,532
	Cost estimate 2001-2006	92,160,049	112,352,920
Burglary	Effect beyond RMSPE 2001-2006	17,680	17,680
	Cost-per-offense	1,653.5 (avg)[a]	6,462
	Cost estimate 2001-2006	38,985,845	114,248,160
Robbery	Effect beyond RMSPE 2001-2006	2,216	2,216
	Cost-per-offense	1,264.8 (avg)[a]	42,310
	Cost estimate 2001-2006	2,465,637	93,758,960
Aggravated Assault	Effect beyond RMSPE 2001-2006	-2,116	-2,116
	Cost-per-offense	107,020[b]	107,020
	Cost estimate 2001-2006	-226,454,320[b]	-226,454,320
Subtotal: SACPA's impact on crime	Property Crime cost	263,884,261	588,496,664
	Violent Crime Savings	-226,454,320[b]	-226,454,320
	Net cost of SACPA-related crime increases	37,429,941	362,042,344
Subtotal: SACPA's Estimated Savings 2001-06	Total savings: Subtotal from Table 2 (Ehlers & Ziedenberge, 2006)	926,715,098	
Total	Net savings due to SACPA 2001-2006	$889,285,157	$564,672,754

[a] cost-per-offense values are displayed as a 2001-2006 average, however, the cost-per-offense in each year 2001-2006 are used to calculate the total cost of SACPA's effect over this period. The values used to calculate the cost of SACPA's effect can be found in Table B4 of Appendix B.

[b] The FBI's Uniform Crime Reports do not provide cost-per-offense estimates for violent crimes such as aggravated assault. Therefore, the cost-per-aggravated assault from McCollister et al., 2009 is applied to both columns. If the savings from the SACPA-related reduction in aggravated assaults were ignored, the net savings due to SACPA 2001-2006 according to UCR cost-per-offense estimates would be $662,830,837.

As Table 3 reports, the SACPA-associated increase in California property crime from 2001-2006[24] cost between $227.94(2006 Uniform Crime Report) and $588.5 million (McColister *et al.*, 2009). However, when the reduction in aggravated assaults is included in this estimate

[24] Note that "Effect beyond RMSPE 2001-2006" values differ from those provided in Table 1. This is due to the estimates in Table 1 including the effect estimated in 2007. The effects are estimated the same way, but 2007 is ignored in Table 3 to allow for a direct comparison with the savings estimated by Ehlers and Ziedenberg (2006) from 2001 to 2006.

(~$226 million in savings) the $588.5 million cost shrinks to $362 million[25]. The property crime increases in California associated with SACPA's implementation do not appear to have cost the state more than was saved through reduced reliance on incarceration. SACPA's savings from reduced reliance on incarceration appears 2.6 to 4.1 times greater than the cost incurred by property crime increases. In total, SACPA appears to have saved California between $564,672,754 and $899,285,157 from 2001 to 2006.

CONCLUSION

Prior to its enactment, many criminal justice stakeholders argued that SACPA would threaten public safety and cost rather than save money[26]. The current study finds little support for these claims. The synthetic control group analysis suggests that UCR motor vehicle thefts, larceny-thefts, robberies, and burglaries did increase, at least temporarily, following SACPA's implementation. However, aggravated assaults decreased substantially over this period, and the combined cost of the estimated increases amounts to less than a quarter of the savings generated from California's reduced reliance on incarceration. The increases in property crime and robbery did not, as many critics prophesized, cost the state more than it saved. Thus, the present analysis provides evidence that the savings promised through decarceration reforms can be realized, rather than wiped away by crime rate increases or the cost of expanding alternatives to incarceration, as opponents to decarceration reforms commonly claim.

Robbery (a violent offense) does appear to have increased moderately following SACPA's implementation, however, aggravated assaults surprisingly decreased over the same period. Homicide does not appear to have been affected by SACPA in either direction. Thus, the

[25] The FBI does not report property values per offense for Aggravated Assaults.

[26] See "Arguments Against" in (California Secretary of State Elections Division, 2000)

adoption of compulsory diversion programs for drug offenders in California appears to have had a net non-effect on violent crime in California.

While the synthetic control group models suggest, as critics warned, that motor vehicle thefts, larceny-thefts, burglaries, and robberies increased following SACPA's implementation, these increases did not incur costs beyond the policy's savings or threaten public safety through a net increase of violent crime. This is the unfortunate reality of criminal justice reform in the era of diminishing returns. Given the knowledge that returns on incarceration have diminished, it is nonetheless difficult for policymakers and stakeholders to accept marginal crime rate increases as a by-product of decarceration and criminal justice reform generally. The present analysis finds no support for the claim that SACPA caused more violent crime than it eliminated, however, the ballot initiative's potential impact on violent crime was enough to generate an opposition movement among politicians and criminal justice practitioners.

California has been past the point of diminishing returns on incarceration for many years, however, diminishing returns does not mean no returns at all. Therefore, decarceration effect, even among lower level offenders, should be expected to cause crime rates to increase, at least marginally. The conflict criminal justice stakeholders and policymakers must navigate is whether the diminishing returns of incarceration warrant continued investment. If keeping crime rates at or below current levels is prioritized over all else, then criminal justice costs will continue climbing to all-time highs. The present findings should, however, serve as evidence that reforms aimed at decarceration can, and have, resulted in net savings without threatening public safety.

Appendix A

For a state-level intervention with a dependent variable such as crime, the design assumes that crime rates are observed in each of N states for T years.

$$n = 1, \ldots, N \text{ states} \qquad t = 1, \ldots, T, T+1, \ldots, T \text{ years}$$

If n=1 represents California, or the "treated unit[27]", then states n=2 to n=N+1 constitute a "donor pool" of potential comparison units. Breaking the T years into pre- and post-SACPA segments of t_0 and t_1 years respectively, the crude effect of SACPA in California is given by the difference score

$$\Delta\text{california} = \sum_{t_1+1}^{T} \text{crime}_T - \sum_{1}^{t_1} \text{crime}_T$$

Difference scores like $\Delta_{\text{California}}$ are often used to control the effects of confounder variables when regression adjustments are unfeasible. If the analogous difference score for a "best" Control state is Δ_{Control}, then under ideal circumstances the difference-in-differences equation(1),

$$\Delta = \Delta\text{california} - \Delta\text{control} \qquad (1)$$

can be interpreted as the *causal* effect of SACPA on crime. The framework for a causal interpretation begins by coding the binary variable X_i to indicate the implementation status of SACPA in the n^{th} state and t^{th} year. That is,

$X_i = 1$ if SACPA is implemented in the n^{th} state and t^{th} year

$X_i = 0$ otherwise

Then for Y_i, a crime rate in the n^{th} state and t^{th} year, the effect of SACPA is estimated by the regression

$$Y_i = \alpha + \beta X_i$$

[27] The unit exposed to the intervention of interest.

These estimates are not necessarily causal effects. But if we assume that SACPA can be implemented in any of the n states and t years, prior to implementation of SACPA, the i^{th} state will have two "potential outcomes," Y_{i0} or Y_{i1}.

$$Y_{i0} = E(Y_i \mid X_i = 0) \tag{2a}$$

$$Y_{i1} = E(Y_i \mid X_i = 1) \tag{2b}$$

After implementation, of course, only one of the potential outcomes can be observed. But the potential outcomes are related to Y_i as

$$Y_i = X_i Y_{i1} + (1 - X_i) Y_{i0} \tag{3}$$

which Rubin (1978, 1980) calls the "stable unit treatment value assumption" (SUTVA) [28].

SUTVA requires that the treatment applied to the unit of interest does not impact the outcome variable in untreated states. SUTVA would be violated, for example, if the treatment in state *n* has an effect on the outcome variable in states *n*+1. When SUTVA is violated, neither of the potential outcomes in (3) can be observed. Causal effects are not identified as a consequence and effect estimates based on Y_i are biased.

Lechner (2010) demonstrates that, subject to SUTVA, the causal effects in (1) are identified.

[28] See, *e.g.*, Manski (2010): "Social interactions are common within households, schools, workplaces, and communities. Yet research on treatment response has mainly assumed that a person's outcome may vary only with his own treatment, not with those of other members of the population. Some researchers have called this 'no interference between units' or the Stable Unit Treatment Value Assumption. I have called it individualistic treatment response (ITR), to mark it as an assumption that restricts the form of treatment response functions."

Appendix B

Table B1. Optimal Combination of Donor Pool Weights for Synthetic Control Group Models

State	Motor Vehicle Theft	Larceny	Burglary	Robbery	Aggravated Assault	Homicide
Alaska	0	0	0.049	0.027	0	0
Arkansas	0	0	0.036	0.043	0.053	0
California	-	-	-	-	-	-
Colorado	0	0	0	0	0	0
Delaware	0	0	0	0	0	0
Georgia	0.116	0	0	0	0.335	0
Illinois	0	0	0	0	0	0
Iowa	0	0	0	0	0.01	0
Kentucky	0	0	0	0	0.107	0
Louisiana	0.214	0	0.044	0.298	0	0
Maine	0	0	0	0	0	0
Michigan	0	0.571	0.081	0	0	0
Minnesota	0	0	0.051	0	0.09	0
Mississippi	0	0	0	0.057	0	0
Montana	0	0	0.031	0	0	0.003
Nebraska	0	0	0	0	0	0.002
Nevada	0.046	0	0	0.003	0.05	0.155
New Hampshire	0	0	0	0.009	0	0
New Jersey	0.065	0	0.074	0	0	0
New Mexico	0	0	0.116	0	0	0
New York	0.326	0.315	0.472	0.377	0.171	0.4
North Carolina	0	0	0.046	0.007	0	0
North Dakota	0	0	0	0	0	0
Ohio	0	0	0	0	0.125	0
Oklahoma	0.04	0	0	0.095	0	0.051
Oregon	0.076	0	0	0.066	0	0.072
Pennsylvania	0	0	0	0	0	0
Rhode Island	0	0	0	0.006	0.016	0.074
South Carolina	0	0	0	0	0	0
South Dakota	0	0.048	0	0	0	0
Tennessee	0.082	0	0	0	0	0
Utah	0	0.057	0	0	0	0.105
Vermont	0	0	0	0	0.011	0.006
Virginia	0	0.009	0	0	0	0
West Virginia	0	0	0	0	0	0
Wisconsin	0.033	0	0	0.012	0.031	0.132
Wyoming	0	0	0	0	0	0

Table B2. In-sample Placebo Test Ranks

State	Motor Vehicle Theft	Larceny	Burglary	Robbery	Agg. Assault	Homicide
Alaska	32	37	34	15	32	33
Arkansas	20	17	2	30	10	15
California	1	9	5	2	6	22
Colorado	2	7	3	5	3	13
Delaware	25	28	12	21	21	31
Georgia	30	36	32	35	16	18
Illinois	8	16	4	25	1	2
Iowa	24	24	28	36	34	6
Kentucky	34	32	18	7	18	7
Louisiana	4	12	29	4	28	14
Maine	14	19	13	22	15	30
Michigan	13	22	11	37	24	34
Minnesota	26	27	23	8	26	35
Mississippi	37	26	25	33	7	11
Montana	17	30	10	19	8	26
Nebraska	21	3	37	16	14	28
Nevada	3	10	7	12	11	16
New Hampshire	23	35	36	9	20	24
New Jersey	6	4	22	32	17	4
New Mexico	28	6	35	27	22	19
New York	5	21	6	3	12	3
North Carolina	12	23	26	17	25	25
North Dakota	33	18	14	24	4	37
Ohio	16	15	1	1	31	8
Oklahoma	27	20	33	28	30	21
Oregon	22	13	16	10	2	5
Pennsylvania	7	11	19	14	36	1
Rhode Island	35	25	21	13	19	23
South Carolina	18	1	20	6	9	10
South Dakota	36	5	15	29	27	36
Tennessee	11	8	8	26	5	17
Utah	9	31	27	23	13	20
Vermont	15	34	17	34	29	29
Virginia	29	29	30	31	35	9
West Virginia	10	14	31	11	33	12
Wisconsin	19	2	9	18	23	32
Wyoming	31	33	24	20	37	27

Table B3. Leave Out Originals Test: New Donor Pool Weights

State	Motor Vehicle Theft	Larceny	Burglary	Robbery	Aggravated Assault	Homicide
Alaska	0.001	0	-	-	0	0
Arkansas	0.283	0	-	-	-	0
California	-	-	-	-	-	-
Colorado	0	0	0.014	0	0.369	0
Delaware	0	0	0	0	0	0
Georgia	-	0	0	0.073	-	0
Illinois	0	0.033	0	0.232	0.212	0
Iowa	0	0	0	0	-	0
Kentucky	0	0	0	0	-	0
Louisiana	-	0	-	-	0.041	0.626
Maine	0	0	0	0	0	0.061
Michigan	0.011	-	-	0	0	0
Minnesota	0	0	-	0	-	0.042
Mississippi	0	0	0	-	0	0
Montana	0	0	-	0	0	-
Nebraska	0	0	0	0	0.046	-
Nevada	-	0	0	-	-	-
New Hampshire	0	0	0	-	0.01	0.051
New Jersey	-	0	-	0.445	0.069	0
New Mexico	0.005	0	-	0	0	0.181
New York	-	-	-	-	-	-
North Carolina	0	0	-	-	0.038	0
North Dakota	0	0	0	0	0	0.015
Ohio	0	0	0	0	-	0
Oklahoma	-	0	0	-	0	-
Oregon	-	0	0.048	-	0	-
Pennsylvania	0.377	0	0	0	0.004	0
Rhode Island	0.093	0.967	0.225	-	-	-
South Carolina	0	0	0	0.019	0	0
South Dakota	0	-	0.007	0	0	0
Tennessee	-	0	0	0.223	0.211	0
Utah	0	-	0	0	0	-
Vermont	0	0	0	0	-	-
Virginia	0.23	-	0.705	0	0	0
West Virginia	0	0	0	0.007	0	0
Wisconsin	-	0	0	-	-	-
Wyoming	0	0	0	0	0	0

Table B4. Cost Estimates for post-SACPA Crime Increases: Cost per offense estimates from Uniform Crime Reports

Year	Measure	Motor Vehicle Thefts	Larceny-Thefts	Burglary	Robbery	Total
2001	Cost per offense	$ 6,646	$ 730	$ 1,545	$ 1,258	
	Effect estimate beyond RMSPE	991	496	747	66	
	Cost associated with estimated effect	$ 6,586,173	$ 361,835	$ 1,154,718	$ 83,314	$ 8,186,041
2002	Cost per offense	$ 6,701	$ 699	$ 1,549	$ 1,281	
	Effect estimate beyond RMSPE	2,192	4,025	2,157	161	
	Cost associated with estimated effect	$ 14,686,633	$ 2,813,258	$ 3,341,794	$ 206,415	$ 21,048,100
2003	Cost per offense	$ 6,797	$ 698	$ 1,626	$ 1,244	
	Effect estimate beyond RMSPE	2,985	6,502	2,836	175	
	Cost associated with estimated effect	$ 20,287,587	$ 4,538,469	$ 4,611,047	$ 218,236	$ 29,655,340
2004	Cost per offense	$ 6,108	$ 727	$ 1,642	$ 1,308	
	Effect estimate beyond RMSPE	4,081	8,643	3,967	340	
	Cost associated with estimated effect	$ 16,653,674	$ 74,700,880	$ 15,739,776	$ 115,556	$107,209,886
2005	Cost per offense	$ 6,173	$ 764	$ 1,725	$ 1,230	
	Effect estimate beyond RMSPE	5,602	6,983	4,470	683	
	Cost associated with estimated effect	$ 34,578,618	$ 5,334,661	$ 7,710,340	$ 840,396	$ 48,464,016
2006	Cost per offense	$ 6,649	$ 855	$ 1,834	$ 1,268	
	Effect estimate beyond RMSPE	5,637	5,159	3,505	790	
	Cost associated with estimated effect	$ 37,480,044	$ 4,410,945	$ 6,428,170	$ 1,001,720	$ 49,320,879
Total	Cost associated with estimated effect	$ 130,272,730	$ 92,160,049	$ 38,985,845	$ 2,465,637	$263,884,261

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