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Estimating the effect of death penalty moratoriums on homicide rates using the synthetic control method

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Abstract

Research Summary: Research examining death penalty deterrence has been characterized as inconclusive and uninformative. The present analysis heeds a recommendation from prior research to examine single-state changes in death penalty policy using the synthetic control method. Data from the years 1979–2019 were used to construct synthetic controls and estimate the effects of death penalty moratoriums on homicide rates in Illinois, New Jersey, Washington, and Pennsylvania. Moratoriums on capital punishment resulted in nonsignificant homicide reductions in all four states.

Policy Implications: Inconsistent with a deterrence hypothesis, no evidence of a deterrent effect attributable to death penalty statutes was found. Given the gravity and finality of state-sanctioned execution, it is important that policy makers consider the weight of evidence of the death penalty's capacity to deter, as well as issues of equity, justice, and fairness, in their decision making about death penalty policy.

KEYWORDS
death penalty, deterrence, homicide, synthetic control method

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Perhaps no other idea has influenced criminal justice policy in the United States more than deterrence. Predicated on a fundamental assumption that humans are rational actors who seek to avoid pain, deterrence theory posits that punishment, or the threat of punishment, discourages individuals from committing crime. As is the case with other punitive measures imposed by the judicial system (e.g., incarceration), the death penalty is hypothesized to represent a punishment of such consequence that it deters homicide in society at-large, in addition to incapacitating the individual convicted of the offense. A potential death penalty deterrent effect rests not only on the assumption that humans are rational actors who calculate the costs and benefits of their actions, but also that the statutes dictating the possible consequences of such actions are known to the public. Of the three deterrence elements—certainty, severity, and celerity—any deterrent effect stemming from the death penalty is expected to operate primarily through the severity of the punishment given that it is infrequently imposed and not sentenced in a celeritous fashion.

Since capital punishment was reinstated in 1976, 1550 people have been executed in the United States (Death Penalty Information Center, n.d.-a). More executions have been carried out by the state governments of Texas, Oklahoma, and Florida than all other states combined (with Texas accounting for 37% of all executions) (Death Penalty Information Center, 2021a). Other states have not carried out an execution since capital punishment was reinstated and/or have moved to abolish or temporarily prohibit the death penalty in the form of moratoriums. In the past 40 years, 16 states have implemented death penalty moratoriums or abolished the death penalty altogether (Death Penalty Information Center, n.d.-b). Following the abolition of the death penalty in Virginia in March of 2021, 24 states have retained the death penalty as a formal sanction (Death Penalty Information Center, 2021a).

Given the nature of capital punishment and the core elements of deterrence, few, if any, state-sanctioned punishments are predicated so heavily on the principle of deterrence. Likewise, no other punishment exacts its penalty with such finality, thereby making judgments of its effectiveness a theoretical and empirical undertaking worth pursuing. Much of the existing research examining the death penalty's capacity to deter can be characterized as highly sensitive. Findings from influential studies such as Dezhbakhsh et al. (2003) suggest that executions have a substantial deterrent effect (e.g., as many as 52 lives saved per execution in some analyses [Donohue & Wolfers, 2005]). Other studies, often replications or critical reviews reanalyzing prior work, have noted null effects or deterrent effects that are highly sensitive to econometric corrections or modest specification changes. Similarly, changes in operationalization of execution risk based on strong assumptions can alter the estimated effect of the death penalty from a sanction that deters to one that brutalizes.

The extent to which a potential deterrent effect applies to death penalty statutes is unclear. Therefore, state-level policy analyses are needed to better understand the impact of death penalty policies on homicide rates. The present analysis uses the synthetic control method (SCM)—a data-driven technique that does not involve unverifiable assumptions about execution risk—to assess whether the effects of state changes in death penalty policy on homicide rates are consistent with a hypothesized deterrent effect. Synthetic control estimation is an intuitive approach that uses a weighted combination of potential control states to approximate what would have happened had a policy not changed. The effects of moratoriums are estimated by comparing the postintervention homicide rates of the state that implemented a moratorium and the "synthetic" counterfactual (composed of states that retained the death penalty).

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1 | LITERATURE REVIEW

1.1 | Deterrence theory

A fundamental premise of deterrence theory is that the threat of sanctions can effectively discourage criminal offending. Sanctions that are perceived as being more severe, certain, and administered swiftly are theorized to have a greater deterrent capacity than those which are less severe, less likely, and slowly imposed. Although a punishment of death is no more certain or swift than the imposition of a lengthy prison sentence, the possibility of receiving a more severe punishment could theoretically have a general deterrent effect on homicide. Likewise, the "removal" of capital punishment as a possible sanction, whether via moratorium or abolition, could result in an "increase" in homicide.

There are two distinct processes by which the death penalty may deter homicide: (1) the application or frequency of executions may significantly influence the perceived risk of execution, thereby deterring an individual from committing homicide; or (2) the mere existence of an active death penalty statute, regardless of the frequency of imposition, may pose a sufficient threat in and of itself to increase the perceived costs of offending. Indeed, operationalizations of execution risk in the death penalty literature involve either the frequency of execution or the existence of an active death penalty policy (Kovandzic et al., 2009).

A general deterrent effect is contingent upon potential offenders carefully weighing the costs and benefits of crime. However, it is unclear what proportion of homicides may be deterrable by a threat of sanctions that requires a rational calculus of risks. As noted by Kovandzic et al. (2009), it is unlikely that perpetrators of homicide experiencing emotional distress or those under the influence of drugs first carefully weigh the potential costs of their offenses, let alone the differential impact that capital punishment may carry throughout a lengthy prison sentence. The assumption that potential offenders could have an "accurate" perception of the possible sanction risks stemming from homicide is also questionable (Nagin, 2013a).

Notwithstanding the criticisms of accurate perceptions described above, it remains plausible that the prospect of the death penalty stemming from a state's capital punishment status may hold some deterrent capacity.

1.2 | Death penalty research

Research throughout the past several decades has outlined persistent discrimination and arbitrariness in prosecutorial decisions to seek the death penalty. Such practices were, in part, the basis of the Supreme Court's original decision to strike down the death penalty as unconstitutional in *Furman v. Georgia*, with Justice Stewart writing that the Eighth and Fourteenth Amendments could not tolerate death sentences "under legal systems that permit this unique penalty to be so wantonly and so freakishly imposed" (Furman v. Georgia*, 408 U.S. 238* [1972], p. 310). Nonetheless, following the reinstatement of capital punishment in Gregg v. Georgia*, 428 U.S. 153 (1976)*, studies have found that extra-legal factors significantly influence prosecutors' decisions to seek the death penalty. Research suggests that prosecutors are more likely to seek capital murder charges against defendants in cases involving White victims than Black victims (Baldus et al., 1990; Jacoby & Paternoster, 1982; Paternoster, 1983; Songer & Unah, 2006). Further, disparities in prosecutors' decisions to seek the death penalty are more pronounced in cases with Black defendants and White victims (Baldus et al., 1990; Jacoby & Paternoster, 1982; Keil & Vito, 2006; Radelet & Pierce,

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1985; Sorensen & Wallace, 1999; Unah, 2009). In addition to the arbitrary and discriminatory use of capital punishment, there is a long-standing debate as to whether the death penalty actually deters.

For many years, researchers have estimated the effect of the death penalty on homicide rates using econometric methods. With few exceptions, the most influential studies have used instrumental variables estimation or some form of least squares regression to analyze panel data in the post-*Gregg* era (i.e., after 1976). Following a decade of renewed interest in death penalty research, the National Research Council for the National Academies concluded in a 2012 report that "research to date on the effect of capital punishment on homicide is not informative about whether capital punishment decreases, increases, or has no effect on homicide rates" (National Research Council et al., 2012, p. 2). As the following section will illustrate, much of the evidence of a death penalty deterrent effect is sensitive to changes in model specification, operationalizations of execution risk, study periods, the exclusion of highly influential states, the handling of standard errors that is consistent with current standard practices, or other methodological decisions (e.g., including control states in analyses). Taken as a whole, the existing body of research purporting to find a deterrent effect associated with the death penalty is far from conclusive (see also Chalfin et al., 2013; Chalfin & McCrary, 2017; Donohue & Wolfers, 2005, 2009; Kovandzic et al., 2009).

The differences that account for discordant findings among researchers originated in the 1970s. In his rebuke of Thorsten Sellin's study that found no evidence of a death penalty deterrent effect using matching techniques, Ehrlich's (1975) national time-series analysis covering 1933–1969 noted a significant negative relationship between execution risk and homicide (Baldus & Cole, 1975). Ehrlich's analysis played a central role in the decision to reinstate capital punishment in *Gregg v. Georgia*; the Solicitor General cited Ehrlich's findings in an amicus brief while criticizing the approach used by Sellin (Baldus & Cole, 1975). However, as Justice Marshall wrote in his dissent in Gregg v. Georgia*, 428 U.S. 153 (1976)*, Ehrlich's findings suffer from an aggregation problem—the nation was the observational unit as opposed to abolitionist and retentionist states—and his conclusions "are extremely sensitive to the choice of the time period" (p. 236). Indeed, Passell and Taylor (1977) failed to detect a deterrent effect upon subjecting Ehrlich's data to sensitivity tests, including restricting the study period to 1935–1963 and using untransformed data.

More recently, Dezhbakhsh and Shepherd (2006) examined changes in homicide rates by state using 1-, 2-, and 3-year windows before and after states' abolitions and reinstatements of the death penalty during 1960–2000. The researchers noted a significant deterrent effect for all windows except the 3-year reinstatement window. Notably, Dezhbakhsh and Shepherd only examined "treated" states, ultimately attributing a deterrent effect to death penalty statutes without analyzing trends in "untreated" states during the same windows. Given that significant deterrent effects of comparable magnitude were also observed in the subsample of control states that did not change their policy, Donohue and Wolfers (2005) concluded that the analysis did not provide evidence that death penalty laws influence homicide rates. They also illustrated two other instances in which the findings are sensitive to corrections that are common practices in econometrics. Clustering standard errors at the state level to address autocorrelation and controlling for year fixed effects resulted in nonsignificant effects of the death penalty coefficient (Donohue & Wolfers, 2005).

Other studies have adopted different measures to model the mechanism by which the death penalty may deter, such as the number of executions or probability of execution (measured in various ways using the ratio of executions to prisoners, prisoners on death row, offenders sentenced to death, or the general population) (Kovandzic et al., 2009). In their analysis of panel data

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from 1950–1990, Katz et al. (2003) found that the effect of the execution rate (measured as the number of executions per 1000 prisoners) on homicide was highly sensitive to model specification. A replication of their work also obtained identical and highly sensitive estimates with large standard errors (Kovandzic et al., 2009). Further, analyses that extended the time period to 1950–2000 and 1934–2000 did not find a significant relationship between executions per 1000 prisoners and homicide rates (Donohue & Wolfers, 2009).

A more complex measure of execution risk was used in two studies published in 2003 (Dezhbakhsh et al., 2003; Mocan & Gittings, 2003). Both analyses used the following three measures to comprise the expected cost of homicide (i.e., deterrence): probability of arrest (measured as the number of homicide arrests divided by homicides), probability of a death sentence (measured as the number of death sentences divided by homicide arrests 2 years prior), and probability of execution (measured as executions divided by the number of death sentences imposed 6 years prior). The execution risk measure incorporated a 6-year lag to capture the average time between death sentences and execution (Dezhbakhsh et al., 2003). Execution risk was significantly negatively associated with homicide rates in both studies (Dezhbakhsh et al., 2003; Mocan & Gittings, 2003). Using a 1-year lagged measure (Donohue & Wolfers, 2005) and lags of 8–10 years to reflect the longer average time between sentence and execution from 1984 to 1997 (Chalfin et al., 2013), similar analyses of Mocan and Gittings' (2003) work failed to find a significant association between execution risk and homicide rates. Donohue and Wolfers (2005) also noted that the findings reported by Dezhbakhsh et al. (2003) were highly sensitive to specification changes in OLS regression (although the original study used an instrumental-variables approach).

A 2004 study incorporated a similar set of deterrence variables as those mentioned above but used contemporaneous and 1-year lag measures of the three variables as opposed to 2- and 6-year lags used for the probabilities of death sentence and execution, respectively. As was the case in Donohue and Wolfers' (2005) analysis of Dezhbakhsh et al. (2003), significant deterrent effects were observed for the probability of execution variable in two-stage least squares (instrumental variables) regressions—an estimated 14 fewer homicides per execution—but not in ordinary least squares models (Zimmerman, 2004). Zimmerman (2004) stated that the significant results were highly sensitive to functional form (e.g., logarithmic instead of linear specification) and other researchers found insignificant effects after correcting for autocorrelation (Chalfin et al., 2013; Donohue & Wolfers, 2005).

In addition to differences in model specification, functional form, and other analytic techniques described above, variation in the use of the death penalty across states has also been a source of inconsistent findings. Berk (2005) used panel data from Mocan and Gittings' (2003) study to illustrate how a small group of states, or even a single state, can drive the relationship between executions and homicide. Of the 1000 observations in the analysis spanning 50 states and 20 years, 859 state-years did not have any executions, 78 had one execution, and 11 had five or more executions (Berk, 2005). By simply restricting the sample to the 99% of state-year observations with fewer than five executions, Berk (2005) did not observe a significant relationship between lagged executions and homicide rates. In a supplementary test, he excluded Texas from the sample (which accounted for eight of the 11 observations with five or more executions) and randomized pairings of lagged executions with homicide rates. Given that values were intentionally randomly reassigned, there was no relationship between executions and homicide rates. However, reintroducing Texas without randomly reassigning its values resulted in a highly significant negative relationship (Berk, 2005). In other words, 5% of observations drove the significant relationship between executions and homicide which was otherwise random by design. Berk (2005) concluded that "for the vast majority of states for the vast majority of years, there is no evidence for deterrence in these

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analyses" (p. 328). Donohue and Wolfers (2005) also noted the disproportionate influence of Texas, as excluding the state from their analysis of Dezhbakhsh and Sheperd's (2006) data resulted in an insignificant association between executions and homicide rates.

One of the most recent and comprehensive analyses covered the years 1977–2006 and incorporated variations of the three main operationalizations of execution risk: (1) death penalty law, (2) probability of execution, and (3) frequency of executions. Kovandzic et al. (2009) tested numerous model specifications but did not find a significant deterrent effect associated with execution risk. In an extension of their 2003 analysis that used a similar time period and variables as Kovandzic et al. (2009), Mocan and Gittings (2010) criticized the measures used by other researchers and concluded that the deterrent effects of the death penalty are insensitive to numerous specification variations. Among other critiques of Mocan and Gittings (2010), Chalfin et al. (2013) noted that the results of a longer time period in Kovandzic et al. (2009) are consistently insignificant despite similar specifications and variables.

Finally, in a 2012 report concluding that the extant research is not informative about the effect of the death penalty on homicide rates, the National Research Council (2012) wrote that "the existing studies use strong and unverifiable assumptions to identify the effects of capital punishment on homicides" (p. 4). Specifically, the report cited the operationalizations of perceptions held by potential murderers as a major issue. Given the lack of data to suggest how offenders perceive sanction risks, researchers have used several different measures of execution risk. The National Research Council (2012) wrote that researchers use publicly accessible data "to construct statistics that purport to measure the objective risk of execution" (p. 106). Under an assumption of rationality, it follows that potential offenders assess the risk of sanctions and act according to such rational expectations. However, as addressed in the report, "What 'rational expectations' actually means in practice is that researchers construct their own measures of execution risk and assume that potential murderers perceive the risk in the same way" (National Research Council et al., 2012, p. 106). In addition to the use of "implausible" models of perceptions (p. 3), the report also noted other major limitations of existing research, such as the absence of analyses examining the differential effect of capital punishment relative to other punishments (e.g., incarceration) and highly sensitive findings pertaining to execution risk specifications for which there is no clear resolution (National Research Council et al., 2012). Finally, and most relevant to the present analysis, the report raised what it considers to be a fundamental problem pertaining to policy analysis: "the outcomes of counterfactual sanction policies are unobservable" (National Research Council et al., 2012, p. 7). It is not possible to know what the homicide rate would be in states had they not abolished the death penalty. However, a recently developed technique offers a framework for estimating such unobservable outcomes using "synthetic" control units.

1.3 | Synthetic control method

As noted by Chalfin et al. (2013), techniques that incorporate counterfactual comparison groups may avoid some of the challenges that plague other death penalty analyses (e.g., unverifiable assumptions). The SCM is a relatively new statistical technique that is used to estimate the effects of policy changes or other large-scale interventions. The approach involves comparing the outcomes of a treated state with that of a weighted combination of control states. Unlike the difference-in-differences approach, SCM allows for unobserved confounders to vary over time and does not rely on an assumption of parallel trends (Abadie, 2021; Bouttell et al., 2018). The technique was first used by Abadie and Gardeazabal (2003) to estimate the effect of terrorism

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on gross domestic product in Basque Country and has since been used to evaluate the effects of prison and jail downsizing (Bartos & Kubrin, 2018), changes in firearm policies (Crifasi et al., 2015), tobacco laws (Abadie et al., 2010), a minimum wage increase at the city level (Mitre-Becerril & Chalfin, 2021), and the economic impact of Germany reunification in 1990 (Abadie et al., 2015) among other state-wide and national changes.

2 | PRESENT STUDY

In a review of capital punishment research, Chalfin et al. (2013) recommended that future studies focus their evaluative efforts on analyzing state-specific policy changes. Therefore, the present analysis seeks to contribute to the empirical research on death penalty deterrence by adopting a technique that Chalfin et al. (2013) argued "may prove fruitful in estimating the overall deterrent effects associated with single-state changes in policy" (p. 41). Given the potentially heterogeneous impact of moratoriums across states, and the possibility of individual states driving results, it is important to examine single-state death penalty policy changes as individual cases. I use a synthetic control approach to examine four states' experiences establishing death penalty moratoriums and estimate the effect such policy changes have on homicide rates. Adopting a synthetic control approach seeks to address what the National Research Council et al. (2012) noted as a fundamental problem—"that is, there is no way to determine what would have occurred if a given state had a different sanction regime" (p. 7).

To date, only two studies have utilized the SCM to estimate the impact of the death penalty on homicides. Gius (2020) first analyzed the impact of New Jersey's death penalty moratorium using a poorly fit synthetic control (among other flaws addressed in the Discussion). In addition to investigating the effect of the death penalty moratorium in Illinois, I analyze homicide rates in New Jersey using longer pre- and postintervention periods, more predictor variables, in-time placebo tests, permutation tests, and a synthetic control that more closely approximates homicide rates than Gius (2020). Further, I use homicide rates that are not log-transformed and longer pre- and postintervention periods than Parker (2021) to estimate treatment effects, in addition to examining moratoriums in Washington and Pennsylvania that were not included in either study.

3 | DEATH PENALTY MORATORIUMS

Governors can effectively halt capital punishment in their state through executive action or by signing legislation into law. Following more than a decade of what Warden (2012) described as a "cavalcade" of exonerations (p. 247), Illinois governor George Ryan announced a moratorium on executions in January 2000 that remained in effect until the state abolished the death penalty in 2011. Citing the number of wrongful convictions in his decision to appoint a panel to examine the state's capital punishment system, Governor Ryan stated that "[t]here is a flaw in the system, without question, and it needs to be studied" (Asim, 2004, para. 2). Between the *Furman* decision and the clearing of death row in 2003, Illinois exonerated more people (13) than it executed (12) (Warden, 2004). Modest reforms were also implemented around the time of the moratorium, such as the establishment of a fund to provide more money for defense in capital cases and a requirement that, beginning in 2005, police record interrogations of homicide suspects (Warden, 2004).

Prior to abolishing capital punishment in 2007, a bill establishing a moratorium on executions in New Jersey (S.709, 211th Leg., Reg. Sess [2004] [https://www.njleg.state.nj.us/bill-search/2004/S709]) was signed into law in January 2006. A notable difference between the moratoriums in

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Illinois and New Jersey (other than the involvement of the state legislature) was the consideration given to cost. Although the Governor's Commission on Capital Punishment in Illinois purposely avoided cost-related considerations (Commission on Capital Punishment, 2002), the New Jersey Death Penalty Study Commission was specifically tasked with studying "whether there is a significant difference between the cost of the death penalty from indictment to execution and the cost of life in prison without parole" (New Jersey Death Penalty Study Commission, 2007, p. 4). In addition to citing a lack of penological purpose and the risk of irreversible mistakes in its recommendation to abolish the death penalty, the Commission cited estimates from the Department of Corrections that abolishing the death penalty would result in cost savings of around $1 million during the life of each prisoner (New Jersey Death Penalty Study Commission, 2007).

Cost was similarly cited by Washington Governor Jay Inslee in a 2014 announcement of a capital punishment moratorium in Washington. Inslee also echoed concerns raised by Governor George Ryan in 2000, stating that there are "too many flaws in the system" and "too much at stake to accept an imperfect system" (Governor Jay Inslee, 2014, para. 7). The death penalty was subsequently abolished in Washington in 2018.

Although Pennsylvania has not abolished the death penalty, a moratorium was announced by Governor Tom Wolf in February 2015 pending the report of the Pennsylvania Task Force and Advisory Commission on Capital Punishment. In a statement outlining his decision, Governor Wolf underscored racial disparities in sentencing as a "moral crisis" and characterized Pennsylvania's capital sentencing system as "riddled with flaws, making it error prone, expensive, and anything but infallible" (Governor Tom Wolf, 2015, pp. 2, 3).

By all indications, the moratoriums in Illinois, New Jersey, Washington, and Pennsylvania were narrowly focused reforms intended to address issues with capital punishment and sentencing rather than elements of sweeping changes to criminal justice policy more broadly.

4 | METHODS

4.1 | Data and measures

Mortality data for the 1979–2019 period were obtained from the Centers for Disease Control and Prevention (CDC) WONDER database (CDC, 2003, 2020). The outcome variable—homicide rate—is measured as the total number of assaultive deaths (ICD-9 codes E960–E969 for years 1979–1998 and ICD-10 codes U01, U02, X85-Y09, and Y87.1 for years 1999–2019) divided by the state's population and multiplied by 100,000. Homicide rates were smoothed by analyzing 3-year moving average rates to address year-to-year volatility. Given the uniquely tragic loss of life from the September 11th terrorist attacks that resulted in a drastic increase in the homicide rate in New Jersey but not in potential control states, deaths coded as U01 (Terrorism—Assault [homicide]) or U02 (Sequelae of terrorism) were excluded from New Jersey's 2001 homicide total.

Eighteen predictor variables were included in the study based on prior inclusion in death penalty studies (Kovandzic et al., 2009) and/or utility in constructing counterfactual homicide rates in other synthetic control studies (Kagawa et al., 2018; McCourt et al., 2020). The following state-level predictor variables were used in synthetic control analyses: population, population density, proportion of the population residing in a metropolitan statistical area, proportion of the population age 15–24, proportion Black, proportion Hispanic, proportion White, unemployment rate, proportion of the population 25 and older with a high school degree or equivalent, per capita ethanol consumption, ratio of firearm suicides to overall suicides, poverty rate, mean individual

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income, Gini index measuring inequality, law enforcement population per 100,000, incarceration rate, robbery rate, and executions per 100,000. Variable descriptions and data sources are provided in Appendix A of the Supporting Information.

4.2 | Sample

Although several states implemented moratoriums or abolished capital punishment altogether during the study period, synthetic controls that closely approximated pretreatment homicide rates could only be constructed in four states. Synthetic controls for other moratorium/abolition states (Oregon, Connecticut, Maryland, New Mexico) exhibited large preintervention prediction errors and/or visually poor preintervention fits (see Appendix C in the Supporting Information for plots, donor state weights, and comparisons of mean preintervention characteristics for excluded states).

The SCM was applied independently to Illinois, New Jersey, Washington, and Pennsylvania. The beginning of each state's postintervention period was operationalized as the year in which a moratorium on capital punishment was established. Therefore, the posttreatment periods for each state are as follows: Illinois (2000–2018); New Jersey (2006–2018); Washington (2014–2018); Pennsylvania (2015–2018). The pretreatment periods for Illinois, Washington, and Pennsylvania begin in 1980, whereas the pretreatment period in New Jersey begins in 1983 following the state's reinstatement of the death penalty. Analyses of each state allow for a sizable number of pretreatment years in which to establish a synthetic control (Abadie et al., 2015). The donor pool is limited to 24 states that had a death penalty statute for the entire 1979–2019 period without periods of moratorium or abolition.

Wyoming was excluded due to several years of low homicide counts that were suppressed in WONDER.

4.3 | Analytical strategy

As described in Abadie et al. (2010), a single unit does not often provide an appropriate counterfactual for a treated unit. Instead, pooling from several potential controls to create a "synthetic" control can provide a better match to the treated unit on preintervention outcomes and other characteristics. The goal of the SCM is to construct a weighted combination of untreated units that closely resembles the treated unit prior to the intervention.

Two important components in synthetic control construction are W^* and V^*. As described in Ferman et al. (2020), "Intuitively, [W^*] is a weighting vector that measures the relative importance of each unit in the synthetic control… while [V^*] measures the relative importance of each one of the… predictors" (p. 514). States in the donor pool are assigned nonnegative weights that form W and collectively sum to one. W^* is a vector of optimized donor unit weights representing the synthetic control that minimizes the difference—measured as $||X_1 - X_0W||$—between the preintervention characteristics of the treated and synthetic control units. Abadie et al. (2010) formulate the difference between preintervention characteristics of the treated unit (X_1) and the donor pool units (X_0) using the following metric:

$$\sqrt{(X_1 - X_0W)'V(X_1 - X_0W)},$$

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where V is a matrix of weights assigned to the predictor variables based on their ability to predict the outcome in the preintervention period. The donor weights indicate the relative contribution of each donor unit to the synthetic control (W^*) and are therefore conditional on V. The matrix V captures the relative predictive importance of the predictor variables and is chosen to minimize the mean squared prediction error (MSPE) of the outcome variable in the preintervention period (Ferman et al., 2020). The MSPE is measured as

$$\sum_{t=1}^{T_0}\left(Y_{1t}-\sum_{j=2}^{J+1} w_j^*(V)\,Y_{jt}\right)^2,$$

where $t = 1$ represents the first year of the preintervention period, T_0 is the number of preintervention years, Y_{1t} is the outcome of the treated unit at time t, $J + 1$ represents the donor pool, w^* is a vector of optimized donor weights, V is a matrix of predictor weights, and Y_{jt} is the outcome of donor units at time t (Cunningham, 2021).

A synthetic control that has preintervention outcomes mirroring those of the treated unit is intended to represent the counterfactual in the postintervention period, such that deviation in the outcomes of the treated and control units following the intervention is attributed to the policy change. The estimated treatment effect $\hat{\alpha}$ in the postintervention period ($t > t_0$) is measured as

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt},$$

where Y_{1t} is the outcome of the treated unit, $J + 1$ represents the donor pool, w^* is an optimized vector of j weights, and Y_{jt} is the outcome of unit j at time t (Abadie et al., 2010). More simply, the treatment effect is estimated by taking the average difference between outcomes of the treated and synthetic control units following the intervention.

The transparent nature of synthetic control construction (i.e., explicitly stating the relative contribution of each donor state and predictor variable) is a primary strength of SCM. Given that all donor unit weights are known, it is possible to quantitatively and qualitatively assess the similarity between the treated unit and its unobserved counterpart. For example, in the Gius (2020) study, readers can assess whether it is reasonable for the least densely populated state in the contiguous U.S. (Wyoming) to comprise 54.5% of the synthetic control for the most densely populated state (New Jersey). Quantitatively, the mean values of preintervention predictor variables can be compared across the treated and synthetic control units to assess their similarity. Although there is no standard by which to assess "balance" between a treated and synthetic control unit, Abadie et al. (2015) recommend limiting the donor pool to units that are similar to the treated unit.

In the present study, measures of the outcome variable in the preintervention period were also included as predictor variables. Consistent with other analyses, lagged outcome measures for the final preintervention year and one or more prior years were included. Abadie (2021) has suggested experimenting with lagged measures in preintervention years to achieve an optimal synthetic control, stating that "the researcher has some flexibility in the way preintervention outcomes are incorporated in X_1 and X_0" if a good preintervention fit between the treated and synthetic control unit outcomes is achieved (p. 16). Synthetic controls for Illinois, New Jersey, and Washington were constructed that minimized the MSPE during the full preintervention period, whereas optimization for synthetic Pennsylvania was limited to the years 1985–2014 to achieve a better fit between

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TABLE 1 Weights of donor states comprising synthetic controls of Illinois, New Jersey, Washington, and Pennsylvania

Donor state	Synthetic IL	Synthetic NJ	Synthetic WA	Synthetic PA
Alabama	0	0	0	0
Arizona	0	0	0	0.055
Arkansas	0	0	0	0
Colorado	0.046	0.313	0.152	0.008
Florida	0.029	0	0	0.001
Georgia	0	0	0	0
Idaho	0	0	0	0
Indiana	0	0	0	0.111
Kentucky	0	0	0	0
Louisiana	0.255	0	0	0.001
Mississippi	0	0	0	0
Missouri	0	0	0	0.004
Montana	0	0	0.165	0
Nebraska	0	0	0	0.068
Nevada	0.126	0	0	0.039
North Carolina	0	0	0	0
Ohio	0.206	0.687	0	0.6
Oklahoma	0	0	0	0
South Carolina	0	0	0	0
South Dakota	0	0	0	0
Tennessee	0.337	0	0	0.081
Texas	0	0	0	0
Utah	0	0	0.44	0
Virginia	0	0	0.243	0.031
Mean squared prediction errors (MSPEs)	0.038	0.064	0.098	0.066

treated and synthetic homicide rates in the years preceding the death penalty moratorium. Data from predictor variables dating back to 1980 were still used to construct synthetic Pennsylvania; only the optimization period over which the MSPE was minimized was limited to 1985–2014 to avoid closely fitting outcomes in the 1980–1984 period at the expense of a better fit between 2010 and 2014. The weights of donor states comprising each synthetic control are presented in Table 1. Predictor variable weights are included in Table A2 in the Supporting Information.

Following synthetic control construction using the *Synth* package (Abadie et al., 2011), placebo tests were conducted to evaluate the size of the effect relative to "placebo interventions" in other states. In-time placebo tests, which reassign the intervention to a prior year, were also used as falsification tests to determine whether estimated changes in homicide rates are sensitive to a shortened preintervention period (and thus, should not be attributed to the intervention itself). All statistical analyses were conducted using R version 4.2.1.

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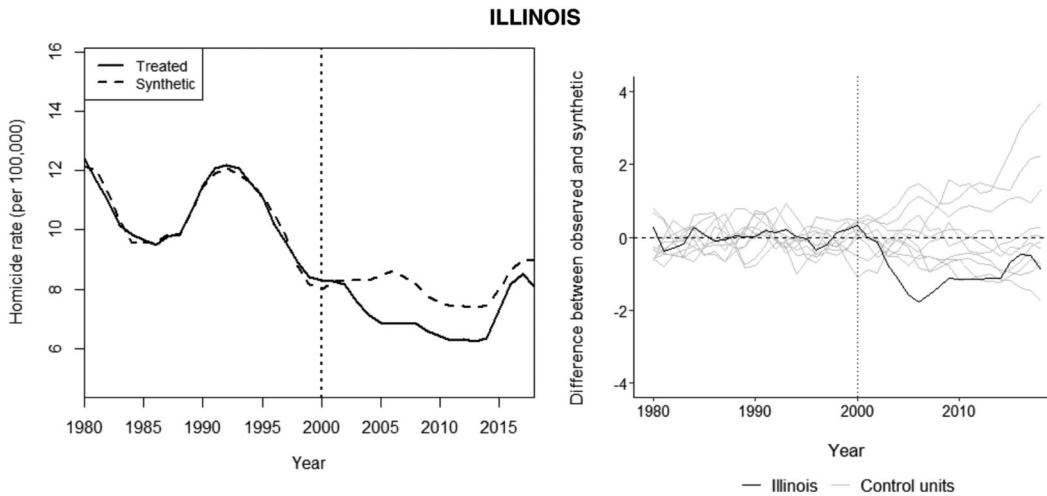

FIGURE 1 Three-year moving average homicide rates in Illinois and Synthetic Illinois during the 1980–2018 study period (left); gaps in the observed and synthetic homicide rates of Illinois and placebo states (right)

5 | RESULTS

5.1 | Illinois

A synthetic control was constructed that closely approximated homicide rates in Illinois from 1980–1999. The small preintervention MSPE (0.038) is indicative of a synthetic control that has a highly similar trend in preintervention homicide rates. Synthetic Illinois is composed of the following states and respective weights: Tennessee (0.337), Louisiana (0.255), Ohio (0.206), Nevada (0.126), Florida (0.029), and Colorado (0.046). Although Synthetic Illinois is less populated and less densely populated than Illinois, similarities in other sociodemographic variables are evident in Table 2.

Following the moratorium, the homicide rate in Illinois declined each year during the first 8 years and remained lower than the homicide rate of Synthetic Illinois between 2002 and 2018 (Figure 1). The estimated treatment effect, taken by subtracting the average postintervention homicide rate of Synthetic Illinois from the observed homicide rate in Illinois, is –0.91 deaths per 100,000. Restricting the postintervention period to the 5 years immediately following the implementation of the moratorium results in a treatment effect of –0.34, or a 4.07% reduction in the homicide rate (48 fewer homicide deaths per year).

An "in-time" placebo test was conducted to determine whether artificially reassigning the intervention date 5 years earlier would similarly result in a decrease in homicide rates (thus undermining the case that the observed decrease in homicide is attributable to the moratorium). Although the synthetic and observed homicide rates start to diverge slightly near 2000, the trend of the placebo Synthetic Illinois largely mirrors that of the original synthetic control. The lack of a drastic change following the 1995 placebo moratorium offers reassurance that the estimated treatment effect is not simply an artifact of an arbitrary intervention. Despite backdating the moratorium, the trajectories of the treated and control units hold.

To further assess the decline in homicide rates in Illinois, placebo interventions were modeled in other states in the same year as the moratorium. Synthetic controls were constructed for each

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TABLE 2 Mean preintervention characteristics of homicide predictor variables in the treated states, synthetic controls, and donor pool

Variable	Treated	Synthetic control	Donor pool
Illinois 1980–1999			
Population	11,680,200	5,749,897	4,790,454
Population density	210.382	131.87	84.586
Population 15–24	0.155	0.156	0.158
Proportion Black	0.151	0.17	0.131
Proportion White	0.822	0.813	0.842
Unemployment rate	7.28	7.11	6.227
Alcohol consumption	2.042	1.988	1.831
Ratio of FS:S	0.457	0.692	0.681
Poverty rate	13.015	16.111	15.082
Mean income	20,334	17,228	16,785
Law enforcement pop.	247.132	222.389	193.162
Incarceration rate	229.576	321.172	293.308
Robbery rate (1985–)	329.773	212.975	140.492
Executions per 100,000	0.005	0.011	0.019
1999 homicide rate	8.399	8.184	6.579
New Jersey 1983–2005			
Population	8,052,739	8,554,053	5,103,661
Population density	1,095.015	190.678	89.334
Proportion pop. MSA (1995–)	1	0.82	0.673
Population 15–24	0.135	0.15	0.152
Proportion Black	0.142	0.088	0.133
Proportion Hispanic (1999–)	0.142	0.076	0.081
Proportion White	0.808	0.894	0.836
Unemployment rate	5.55	6.276	5.698
Educational attainment (1999–)	86.843	87.926	84.494
Alcohol consumption	1.956	1.828	1.789
Ratio of FS:S	0.336	0.564	0.663
Poverty rate	8.687	11.458	14.35
Mean income	29,581	21,819	20,872
Gini index (1999–)	0.453	0.435	0.444
Law enforcement pop.	350.749	198.289	207.754
Incarceration rate	279.877	293.476	360.714
Robbery rate (1985–)	227.39	140.154	132.53
Executions per 100,000	0	0.005	0.025
1983 homicide rate	6.12	5.7	8.565
2001 homicide rate	3.975	4.286	6.45
2005 homicide rate	4.936	5.023	6.375

(Continues)

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TABLE 2 (Continued)

Variable	Treated	Synthetic control	Donor pool
Washington 1980–2013			
Population	5,532,393	3,334,461	5,308,798
Population density	83.249	59.999	92.396
Proportion pop. MSA (1995–)	0.859	0.756	0.697
Population 15–24	0.145	0.162	0.152
Proportion Black	0.037	0.06	0.135
Proportion Hispanic (1999–)	0.097	0.1	0.093
Proportion White	0.886	0.896	0.831
Unemployment rate	6.954	5.22	6.139
Educational attainment (1999–)	90.107	89.555	85.052
Alcohol consumption	1.925	1.569	1.818
Ratio of FS:S	0.537	0.597	0.647
Poverty rate	10.829	10.765	14.463
Mean income	28,193	24,324	23,687
Gini index (1999–)	0.441	0.43	0.451
Law enforcement pop.	153.294	198.081	208.606
Incarceration rate	212.521	256.958	379.405
Robbery rate (1985–)	113.066	66.076	124.392
Executions per 100,000	0.003	0.016	0.023
1983 homicide rate	4.781	5.132	8.565
2000 homicide rate	3.301	3.949	6.489
2010 homicide rate	2.736	3.027	5.688
2013 homicide rate	3.069	2.812	5.673
Pennsylvania 1980–2014			
Population	12,229,140	8,550,949	5,345,055
Population density	273.32	204.294	92.933
Proportion pop. MSA (1995–)	0.85	0.785	0.699
Population 15–24	0.144	0.148	0.152
Proportion Black	0.103	0.108	0.135
Proportion Hispanic (1999–)	0.048	0.059	0.094
Proportion White	0.876	0.87	0.83
Unemployment rate	6.481	6.507	6.127
Educational attainment (1999–)	87.219	86.981	85.19
Alcohol consumption	1.757	1.747	1.819
Ratio of FS:S	0.532	0.577	0.645
Poverty rate	11.429	12.769	14.646
Mean income	27,455	25,329	24,206
Gini index (1999–)	0.455	0.446	0.452
Law enforcement pop.	193.523	191.561	209.089
Incarceration rate	259.557	338.728	382.79
Robbery rate (1985–)	153.27	147.74	123.06

(Continues)

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TABLE 2 (Continued)

Variable	Treated	Synthetic control	Donor pool
Executions per 100,000	0.001	0.013	0.023
1990 homicide rate	6.669	6.463	8.99
1999 homicide rate	5.215	5.073	6.579
2006 homicide rate	6.044	5.949	6.499
2008 homicide rate	5.542	5.459	6.235
2014 homicide rate	5.023	5.442	5.859

Note: Homicide rates are presented as 3-year moving averages. The proportion of the population residing in metropolitan statistical areas (MSAs) (obtained for 1995–2018) was missing in Illinois for preintervention years 1995–1999 and therefore not included as a predictor variable.

of the 24 donor states during the same preintervention period to estimate the impact of a placebo intervention in each state. The probability of obtaining an equal or greater change in homicide rates than the estimated effect in Illinois due to chance was calculated by comparing post-/pre-MSPE ratios within the permutation distribution of the pooled effects (i.e., the treatment effect in Illinois and placebo effects in the donor pool). Consistent with the approach in Abadie et al. (2010), placebo states with poorly fitting synthetic controls (i.e., preintervention MSPEs greater than seven times that of Illinois) were excluded. Of the remaining 10 placebo states, no state had a more extreme change in its mean squared prediction error from the pre- to postintervention period than Illinois. The probability of observing a treatment effect at least as extreme as Illinois is therefore 1/11, or 0.091 (Table 3). Although not a "significant" decrease by the traditional standard of $\alpha = 0.05$, none of the 10 placebo states with adequate synthetic control fits had a larger reduction in the homicide rate following the intervention in 2000 than Illinois (see Figure A1 in the Supporting Information).

5.2 | New Jersey

New Jersey's synthetic control generated appropriately similar homicide trends in the pretreatment period as New Jersey (MSPE = 0.064). Two states make up Synthetic New Jersey: Ohio (0.687) and Colorado (0.313) (Table 1). Contrary to the results in Gius (2020), the homicide rate remained lower in New Jersey than in Synthetic New Jersey for the entire postintervention period (Figure 2). The treatment effect of the moratorium and subsequent abolition (measured as the average difference between postmoratorium homicide rates of New Jersey and Synthetic New Jersey) is –0.68, or a 13.58% decrease. The immediate 5-year treatment effect of –0.28 translates to roughly 24 fewer homicide deaths per year.

A comparison of the treatment effect in New Jersey with placebo states was limited to states with preintervention MSPEs less than five times that of Synthetic New Jersey. A more restricted error threshold (i.e., MSPEs less than five times instead of seven times) was chosen due to Synthetic New Jersey's larger MSPE relative to Synthetic Illinois. The post-/pre-MSPE ratio in New Jersey is the second most extreme result of the 14 states (Table 3). The probability of randomly observing at least as large of a change in the homicide rate in 2006–2018 as New Jersey is therefore 2/14, or 0.143. Similar to Illinois, the estimated treatment effect in New Jersey is robust to an in-time placebo test backdating the moratorium 5 years.

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TABLE 3 Synthetic control estimates for 3-year moving average homicide rates in Illinois, New Jersey, Washington, and Pennsylvania following death penalty moratoriums

State (Moratorium year)	MSPE	Donor states	Donor weights	Treatment effect per 100,000	Percentage change	Post-/Pre-MSPE placebo test
Illinois (2000)	0.038	Colorado	0.046	−0.91	−11.2%	1/11 $p = 0.091$
		Florida	0.029			
		Louisiana	0.255			
		Nevada	0.126			
		Ohio	0.206			
		Tennessee	0.337			
New Jersey(2006)	0.064	Colorado	0.313	−0.73	−14.4%	2/14 $p = 0.143$
		Ohio	0.687			
Washington (2014)	0.098	Colorado	0.152	−0.15	−4.6%	10/11 $p = 0.909$
		Montana	0.165			
		Utah	0.440			
		Virginia	0.243			
Pennsylvania (2015)	0.066	Arizona	0.055	−0.50	−8.0%	5/13 $p = 0.385$
		Colorado	0.008			
		Florida	0.001			
		Indiana	0.111			
		Louisiana	0.001			
		Missouri	0.004			
		Nebraska	0.068			
		Nevada	0.039			
		Ohio	0.600			
		Tennessee	0.081			
		Virginia	0.031			

Note: Donor pool states with poor preintervention fits (measured by their respective mean squared prediction errors [MSPEs]) were excluded from placebo tests. The following values indicate the thresholds at which donor pool states with preintervention MSPEs greater than (#) times that of the treated unit were excluded: Illinois = 7; New Jersey = 5; Washington = 3; Pennsylvania = 5.

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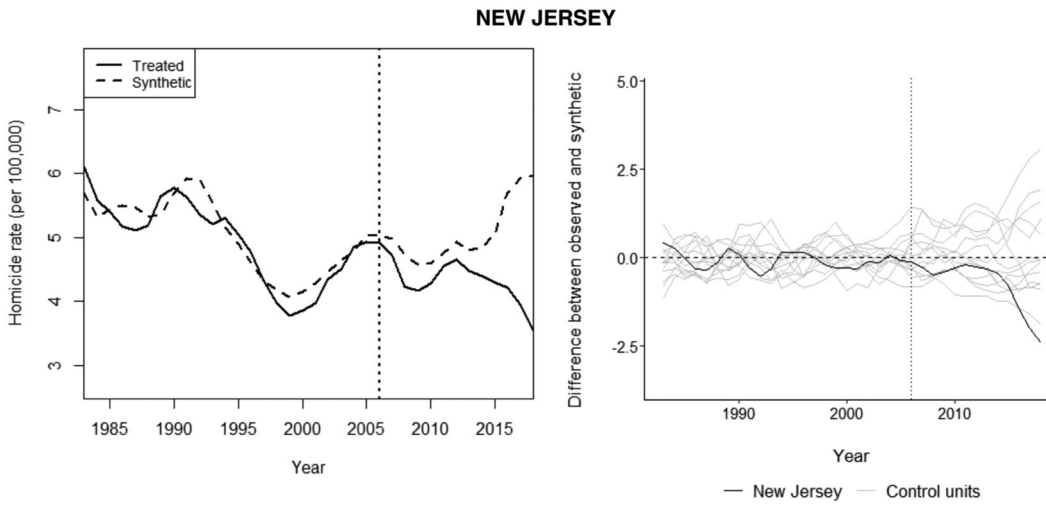

FIGURE 2 Three-year moving average homicide rates in New Jersey and Synthetic New Jersey during the 1983–2018 study period (left); gaps in the observed and synthetic homicide rates of New Jersey and placebo states (right)

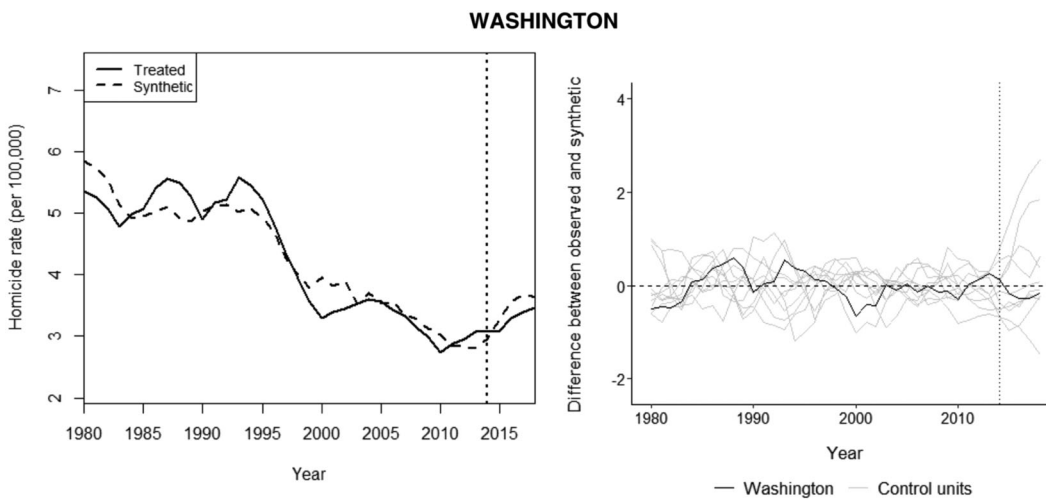

FIGURE 3 Three-year moving average homicide rates in Washington and Synthetic Washington during the 1980–2018 study period (left); gaps in the observed and synthetic homicide rates of Washington and placebo states (right)

5.3 | Washington

Similar to Illinois and New Jersey, homicide rates dropped precipitously in Washington state between 1993 and 2000. The outcome trend in Synthetic Washington closely approximates actual homicide rates in the state, particularly in the 10 years preceding the moratorium in 2014 (Figure 3). Synthetic Washington is composed of Utah (0.440), Virginia (0.243), Montana (0.165), and Colorado (0.152) (Table 1). Relative to its synthetic counterfactual, homicide rates fell by 0.15 per 100,000 in the postmoratorium period, translating to a 4.6% reduction (Table 3).

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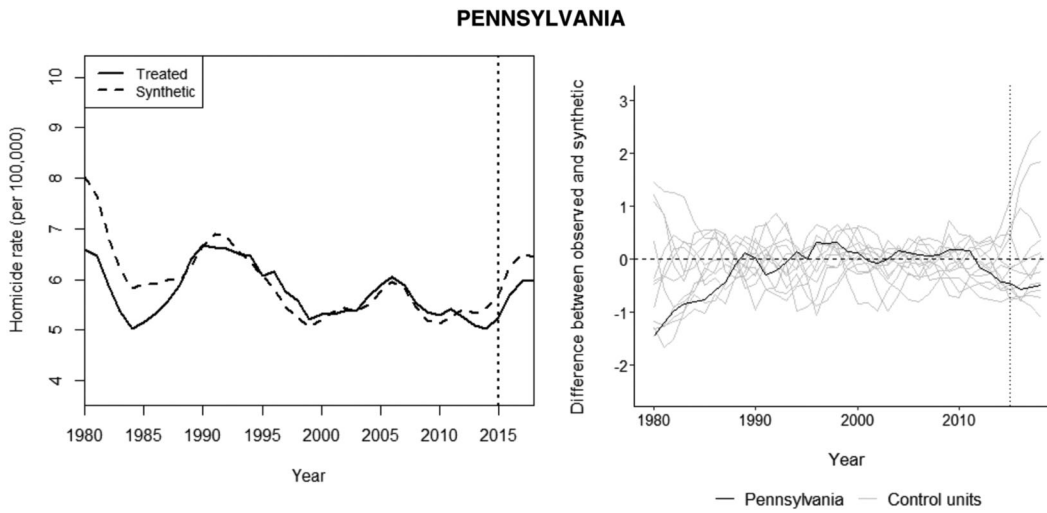

FIGURE 4 Three-year moving average homicide rates in Pennsylvania and Synthetic Pennsylvania during the 1980–2018 study period (left); gaps in the observed and synthetic homicide rates of Pennsylvania and placebo states (right)

To avoid comparing the treatment effect of the moratorium in Washington to placebo interventions in other states with poorly fit synthetic controls, the threshold for inclusion in placebo tests was limited to states with preintervention MSPEs less than three times that of Synthetic Washington. Unlike the estimated effects in Illinois and New Jersey, the ratio of post- to- pre-moratorium prediction error in Washington is not unusually large. For example, nine states have post-/preintervention MSPE ratios that are larger than Washington. The probability of observing a larger magnitude change than Washington in 2014–2018 is therefore 10/11, or 0.909. As was the case in Illinois and New Jersey, an in-time placebo test backdating the intervention did not result in a substantially different synthetic control trend.

5.4 | Pennsylvania

The homicide trend in Pennsylvania has several valleys and peaks between 1980 and 2015 when the state's moratorium was announced (Figure 4). As previously mentioned, a closer approximation of homicide rates in 1980–1984 was sacrificed for a better fit in the years immediately preceding the 2015 moratorium. Synthetic Pennsylvania (MSPE = 0.066) consists of the following states and weights: Ohio (0.600), Indiana (0.111), Tennessee (0.081), Nebraska (0.068), Arizona (0.055), Nevada (0.039), Virginia (0.031), Colorado (0.008), Missouri (0.004), Florida (0.001), and Louisiana (0.001) (Table 1). Despite a heavy reliance on lagged homicide rates as predictors—the five homicide rates combined for a weight of 0.7 in Synthetic Pennsylvania—the mean preintervention characteristics of Pennsylvania and its synthetic control are notably similar (Table 2).

The homicide rate in Pennsylvania increased following the 2015 moratorium. However, the increase was more pronounced in Synthetic Pennsylvania, suggesting that homicide rates would have been higher had no policy change occurred. The average postintervention homicide rate in Pennsylvania is 0.50 per 100,000 less than its synthetic control (Table 3).

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Four of the 12 placebo states with preintervention MSPEs less than five times larger than Synthetic Pennsylvania experienced changes in the homicide rate at least as large as Pennsylvania in 2015–2018 (Figure A4 in the Supporting Information). In other words, it is relatively likely that an equal or greater magnitude change would have been observed by chance alone (5/13 = 0.385). Indeed, the postintervention homicide rates of other states exhibit more extreme deviations from their respective synthetic controls in 2015–2018 (Figure 4). An in-time placebo test did not result in a meaningfully different synthetic control trend in Pennsylvania.

6 | SUPPLEMENTAL ANALYSES

In addition to the placebo tests described above, other checks were performed to examine the robustness of results and offer support for the assumption that the timing of the moratoriums was not confounded by other concurrent changes that would explain variations in homicide rates.

A primary challenge of identifying a causal effect using SCM is ruling out the potential impacts of other changes that may coincide with the intervention (Mitre-Becerril & Chalfin, 2021). First, state incarceration rates and law enforcement population levels were plotted during the study period to assess whether broader changes in incarceration or law enforcement staffing were evident (see Figure E1 in the Supporting Information). Following decades of increases in incarceration, postmoratorium incarceration rates remained near or below the levels at the time of intervention. Law enforcement staffing levels remained similarly unchanged in the years immediately following the moratoriums, suggesting that there were not dramatic changes in law enforcement investment that would have confounded the effect of moratoriums.

Second, state and local expenditure data for police and public welfare in Illinois, New Jersey, and Pennsylvania were obtained from the Urban Institute's State and Local Finance Data tool (U.S. Census Bureau, n.d.). From 1980 to 2018, per capita direct police expenditures (in 2019 inflation-adjusted dollars) aggregated to the state level increased steadily in each state without any large-scale changes (see Figure E2 in the Supporting Information). Similarly, no substantial changes in public welfare spending coincided with state moratoriums in Illinois and Washington. In New Jersey, expenditures decreased in 1997 prior to a sharp increase between 2002 and 2006. Pennsylvania is the only state in which a moratorium preceded a rise in public welfare expenditures (see Figure E3 in the Supporting Information).

Next, the RAND State Firearm Law Database was reviewed to determine whether any firearm policy changes that could impact homicide rates occurred around the time of each state's moratorium (Cherney et al., 2020). Aside from a child access prevention law in 2000 intended to limit firearm access for minors under the age of 14, no state-level firearm policies were implemented in Illinois within 3 years of its moratorium. In New Jersey, laws expanding domestic violence restraining order prohibitions, mandating the seizure of weapons at scenes of domestic violence, mandating reporting of lost or stolen firearms, and limiting handgun purchases to one per month were implemented between 2004 and 2009. Policies related to the confiscation of firearms at scenes of domestic violence have not been found to reduce rates of intimate partner homicide (Díez et al., 2017; Vigdor & Mercy, 2006; Zeoli et al., 2018; Zeoli & Webster, 2010). It is unclear whether the other policy changes implemented in New Jersey would have significantly impacted homicide rates.

Background check requirements in Washington were expanded in December of 2014 to include all firearm sales. Given the slight increase in the proportion of homicides committed with a firearm in Washington from 2009–2013 to 2014–2018 (CDC, 2020), it is unclear how or if the policy

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change impacted homicide. It is similarly unclear whether a 2014 law that requires domestic violence restraining order respondents to surrender firearms had a meaningful impact on overall homicide rates. In Pennsylvania, no firearm policy changes were noted within 3 years of the state's moratorium.

It was also important to determine whether synthetic models would estimate treatment effects for death-ineligible offenses that should theoretically remain unaffected by changes in capital punishment policy. Significant treatment effects on other forms of crime would underscore the possibility that concurrent policy changes may be confounding the estimated moratorium effect on homicide rates. Robustness tests were conducted using a set of nonequivalent dependent variables as outcome measures—property crime, robbery, and aggravated assault (FBI, 2021). Notwithstanding the poor preintervention fits for some outcomes, the results do not suggest that property crime, robbery, or aggravated assault rates responded to the moratoriums in Illinois, New Jersey, Washington, and Pennsylvania more than in donor states following placebo interventions (see Table D1 and Figures D1–D4 in the Supporting Information).

Broader methodological changes were also applied in a series of robustness checks. First, the main synthetic control analyses were rerun using untransformed rates to ensure that estimated treatment effects were not driven by the measurement of the outcome. Results from analyses using untransformed homicide rates are highly similar to those discussed above (see Appendix B in the Supporting Information for comparisons of analyses using 3-year averages and untransformed homicide rates).

As Ohio contributed considerable weight to the synthetic controls of Illinois, New Jersey, and Pennsylvania, additional analyses were conducted to assess the sensitivity of estimated effects to the exclusion of Ohio from the donor pool. The effect of the moratorium in Illinois is robust to the modification; however, the exclusion resulted in Colorado consuming Ohio's lost contribution to Synthetic New Jersey and poor preintervention fits in New Jersey and Pennsylvania (see Table C1 and Figure C1 in the Supporting Information). Therefore, given the lack of a suitable control upon the exclusion, the estimates described above for New Jersey and Pennsylvania are sensitive to the removal of Ohio as a donor state.

Similarly, given that there were notable differences in the characteristics of Illinois and Synthetic Illinois, the postmoratorium period was restricted to 2000–2005 to allow for the synthetic control to pull from an expanded pool of donor states that may be more similar to Illinois on important characteristics. The expanded donor pool added the following states that changed their death penalty policy after 2005 (and therefore were excluded from the primary analysis): California, Connecticut, Delaware, New Jersey, New Mexico, Maryland, Pennsylvania, Oregon, and Washington. Using an expanded donor pool resulted in improved balance among covariates and estimated treatment effects that are nearly identical to those previously estimated for the 5-year postmoratorium period (see Tables C2 and C3 and Figure C2 in the Supporting Information).

6.1 | Augmented synthetic control method

Finally, augmented synthetic controls were used to assess whether the main results are sensitive to overfitting in the preintervention period. The augmented synthetic control method (ASCM) is an extension of SCM that incorporates ridge regression and negative donor weights into the synthetic control construction process to debias original SCM estimates (Ben-Michael et al., 2021). ASCM models with fixed effects and jackknife standard errors were estimated using the *augsynth* package in R (Ben-Michael et al., 2021). To protect against overfitting, the leave-one-out cross-validation

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selection process proposed by Ben-Michael et al. (2021) was used to identify the optimized value for λ in constructing augmented synthetic controls (Doucette et al., 2022). Cross-validation mean squared error plots, donor weights, and tables presenting predictor variable balance between treated and synthetic units from ASCM models are presented in Appendix F in the Supporting Information. Augmented synthetic controls resulted in improved balance among several predictor variables that lacked balance in the original synthetic controls. Compared to the original SCM estimates, the average treatment effect using ASCM moved toward the null in Pennsylvania, remained essentially unchanged at –0.62 homicides per 100,000 in New Jersey, increased to –0.44 in Washington, and increased to –2.18 in Illinois. As was the case in the original SCM analyses, ASCM models using untransformed homicide rates generated nearly identical results as those using 3-year moving averages (Figure 5).

A meta-analysis using inverse variance weighting was conducted to estimate the average effect of moratoriums across the four states. Given that the effect of suspending the death penalty may vary by state, a random effects model was specified instead of a fixed effects model (Borenstein et al., 2010). As shown in Figure 6, the average effect of moratoriums across Illinois, New Jersey, and Pennsylvania is –0.35 homicides per 100,000. The nonsignificant reduction in homicides remains unchanged when untransformed homicide rates are used.

7 | DISCUSSION

Studies using panel data regression to assess the death penalty's potential deterrent effect on homicide have been characterized as "inconclusive as a whole and in many cases uninformative" (Chalfin et al., 2013, p. 41). The SCM was used in the preset study to address a methodological gap in the death penalty literature and estimate the treatment effect of death penalty moratoriums. Synthetic controls were constructed using at least 20 years of preintervention data from 24 donor states to assess the effect of death penalty moratoriums on homicide rates in Illinois, New Jersey, Washington, and Pennsylvania. Results indicate that the observed postmoratorium homicide rates were lower than those of the synthetic control in each state. Estimates were robust to sensitivity analyses involving placebo tests, changes in outcome measurement, and the use of augmented synthetic controls. Taken together, the results do not suggest that death penalty moratoriums resulted in increased homicide rates in any of the four states.

Deterrent effects associated with the death penalty in prior research are highly sensitive to model specification, which led Donohue and Wolfers (2005) to characterize the evidence of deterrence as "surprisingly fragile" (p. 2), and the National Research Council to conclude in 2012 that the existing research was "not informative about whether capital punishment decreases, increases, or has no effect on homicide rates" (p. 2). The current study adds to the literature on death penalty deterrence by adopting a rigorous and transparent method to estimate the impact of state-level death penalty policy changes on homicide. Although neither a panacea to address issues raised in the National Research Council's report nor a definitive assessment of the death penalty's effect on homicide, SCM avoids pitfalls of other approaches used to analyze panel data. Whereas other studies make unverifiable assumptions about perceived execution risk and a homogenous deterrent effect across states (National Research Council et al., 2012), the present analysis used an intuitive, data-driven approach to estimate the effect of death penalty policy changes in individual states. Given that the key variable of interest is an intervention, strong assumptions pertaining to execution risk and potential offenders' perceptions are

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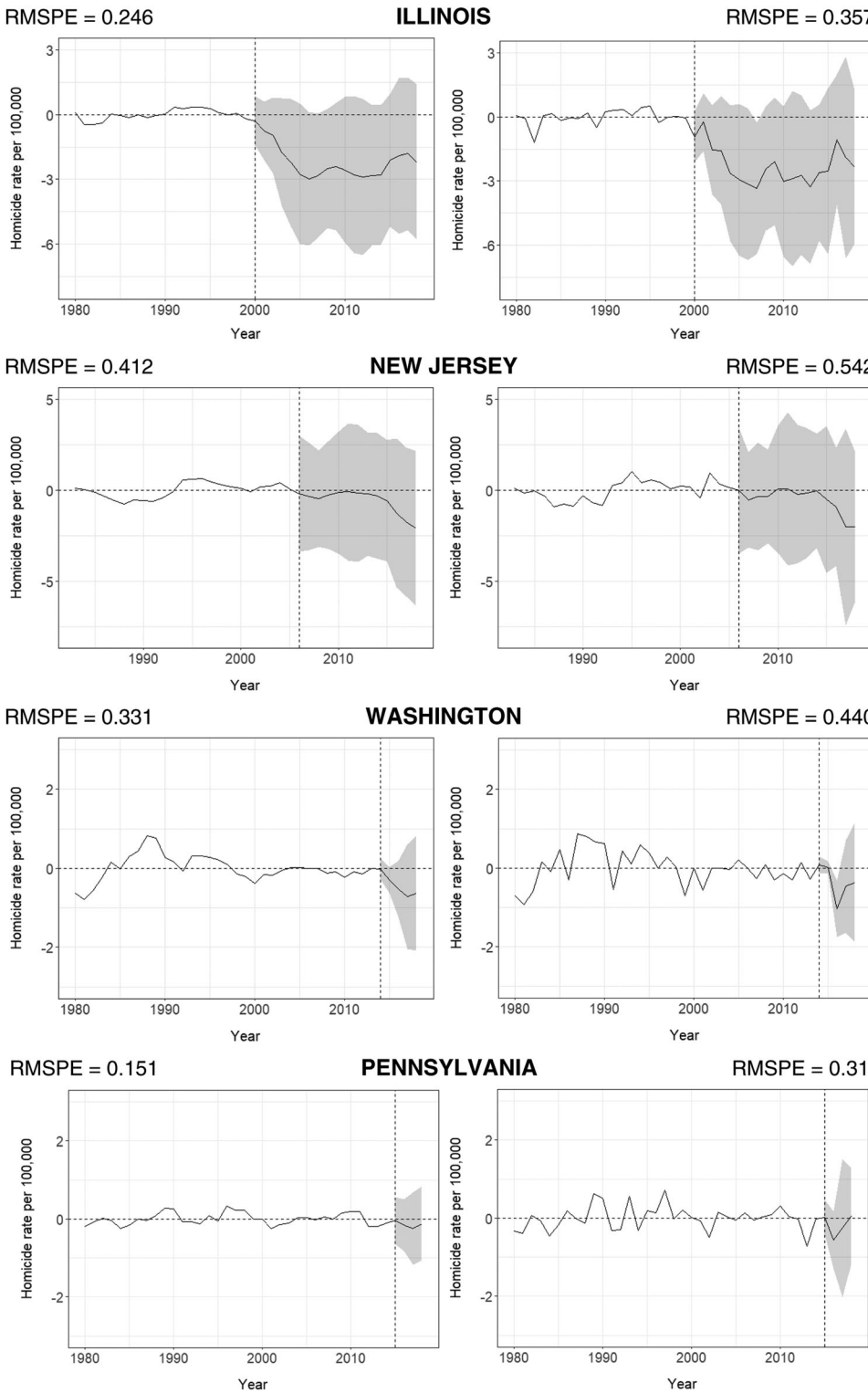

FIGURE 5 Augmented synthetic control analyses with fixed effects and jackknife standard errors for 3-year moving average homicide rates (left) and untransformed homicide rates (right)

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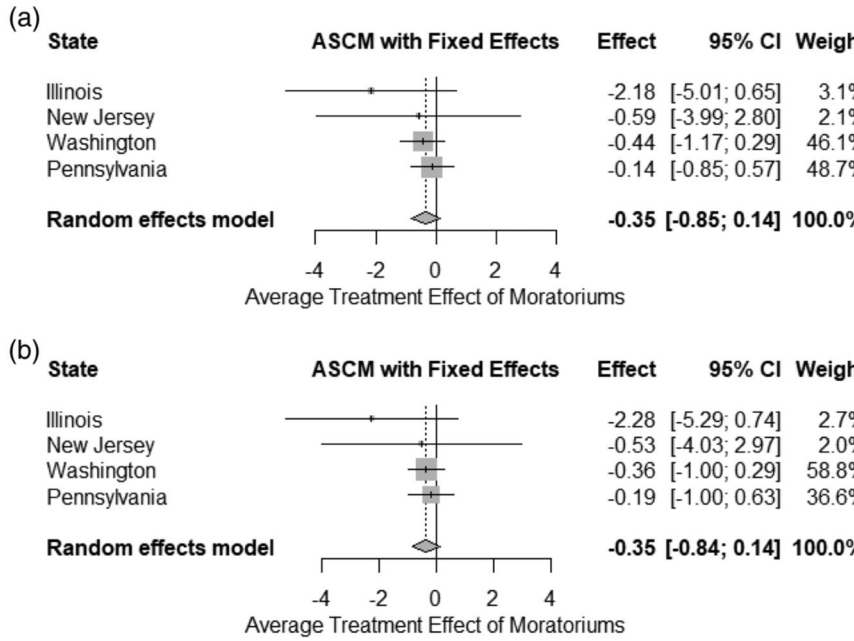

FIGURE 6 Meta-analyses estimating the average treatment effect of moratoriums from augmented synthetic controls using (a) 3-year moving average homicide rates and (b) untransformed homicide rates

necessarily avoided. To date, only two studies have used the SCM to assess a potential deterrent effect of death penalty policies at the state level.

There are several differences between the present analysis and Gius (2020) that warrant discussion. First, the present study approximated preintervention homicide rates in New Jersey for a longer time period and with less error than the synthetic control in Gius (2020). Gius (2020) presents a nonsignificant *t*-test statistic as evidence that the synthetic control's preintervention homicide rates closely match those of New Jersey. However, such a test is largely meaningless, given that a synthetic control that underpredicts rates in the first half of the preintervention period and overestimates rates in the second half by a similar margin would result in the researcher failing to reject the null hypothesis that there is no difference in the mean preintervention rates. Second, the validity of a synthetic control approach is dependent on constructing a control that can empirically and reasonably represent the counterfactual had the policy change not taken place. In the present study, the preintervention homicide rate in Synthetic New Jersey closely tracks that of New Jersey (particularly in the years immediately preceding the moratorium). The homicide rate of the synthetic control in Gius (2020) diverges from the observed rate in the 6 years preceding the moratorium. Additionally, Gius (2020) does not provide a comparison of the mean values for the preintervention predictor variables. Ideally, there should be balance between New Jersey and its synthetic control (i.e., similar levels of unemployment, income, and other predictor variables) and it is commonplace for researchers to provide such information. Third, Gius does not comment on the magnitude of the post-/pre-MSPE ratio of New Jersey compared to that of other states in the placebo tests he described. Readers have no way of assessing how large the homicide rate "increase" was relative to the distribution of placebo effects. Instead, Gius (2020) states that a significant test statistic between the postmoratorium homicide rates of New Jersey and synthetic New Jersey is evidence of a significant increase (although the difference does not visually appear

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to be much larger than the difference in the 4 years preceding the moratorium). In light of these differences and the present findings, there is no evidence to suggest that the death penalty moratorium in New Jersey resulted in an increase in the homicide rate, let alone a significant increase as suggested by Gius (2020). The results instead indicate that there was a nonsignificant decrease in the homicide rate following the policy change.

Fewer differences exist between the present study and Parker (2021). The current study includes more predictor variables, longer pre- and postintervention periods, homicide rates that are not log-transformed to better illustrate the changes over time, and estimates of the effects of moratoriums on homicide rates in Washington and Pennsylvania. Using a different data source for state homicide rates (the National Center for Health Statistics instead of FBI data), the current findings are largely consistent with those of Parker (2021) in that no death penalty deterrent effect was observed.

7.1 | Implications

Although not significant, the estimated reductions in homicide following death penalty moratoriums warrant additional discussion and future research. One explanation for the negative sign of the estimands may be that implementing moratoriums had a legitimizing effect. Cross-nationally, political legitimacy has been found to have a strong, protective effect against homicide independent of other important structural factors (Nivette & Eisner, 2013). Nivette and Eisner (2013) hypothesize that individuals in states that lack legitimacy may be reluctant to use the criminal justice system to resolve conflicts and instead take matters into their own hands in the form of "self-help" (Black, 1983, p. 36) or "private justice." Indeed, among a sample of Black male residents in high-crime neighborhoods, many respondents adopted self-help behaviors such as retaliatory violence as a result of what the study's authors described as "widespread distrust of the police and deeply rooted legal cynicism regarding the overall criminal justice system" (Brunson & Wade, 2019, p. 634). Particularly in Illinois, where the death penalty moratorium was supported by 70% of Illinois residents (Warden, 2012) following a series of exonerations and wrongful convictions, it is plausible that the policy change restored some level of trust in the criminal justice system and reduced the perceived need for self-help. To the extent that halting capital punishment helped legitimize a state's criminal justice system as one that "evokes confidence in its ability to provide fair and equal rights and protections" (Nivette & Eisner, 2013, p. 15), individuals may have relied more on the criminal justice system to resolve conflicts that otherwise would have been addressed with "private justice."

Although more research is needed, the current finding that moratoriums on one of the most severe forms of punishment did not adversely impact homicide rates has implications for deterrence theory and policy. Increased certainty of sanctions, often considered the most important deterrence element (Nagin, 2013b), may be a more effective deterrent than the existence of a policy permitting a more severe punishment. Additionally, focused deterrence, which leverages the collective threat of swiftness, severity, and certainty in sanctioning through direct communication of law enforcement strategy to chronic offenders (Braga et al., 2018), has been identified as an effective approach for addressing firearm-related homicide (Braga, 2008), youth homicide (Braga et al., 2001), and overall homicide (McGarrell et al., 2006; Papachristos et al., 2007) at the city level. Capital punishment statutes likely do not alter offenders' perceived risk of sanctions in the same manner as would a more direct and proximate warning by law enforcement.

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Although the current findings may not be generalizable to other states, policy makers considering death penalty moratoriums should be aware that an analysis of four recent death penalty moratoriums found no evidence to suggest that they led to increases in homicide. Given the gravity of capital punishment and the inability to right wrongful executions, it is critically important that policy makers weigh evidence of the death penalty's capacity to deter with considerations of equity, justice, and fairness. Arbitrariness and discrimination in prosecutorial discretion for death-eligible cases has been widely documented, with studies consistently finding that extralegal considerations such as race (Baldus et al., 1990; Jacoby & Paternoster, 1982; Keil & Vito, 2006; Paternoster, 1983; Radelet & Pierce, 1985; Songer & Unah, 2006; Sorensen & Wallace, 1999; Unah, 2009) and sex (Berk et al., 1993; Keil & Vito, 2006; Paternoster, 1983; Songer & Unah, 2006; Unah, 2009) significantly influence the likelihood that a prosecutor will seek the death penalty. Further, given that one wrongfully convicted death row prisoner is exonerated for every 8.3 executions carried out in the United States (Death Penalty Information Center, 2021b), the possibility that an innocent person has been or will be executed is a significant concern. In a discussion of retribution and the possibility of wrongful execution, Unnever and Cullen (2005) posed the question: "How is a system 'just' and principled if its process inevitably executes people who have done nothing wrong?" (p. 31). Given the issues raised above, as well as the present findings and the sensitivity of prior research, one might also ask: What justification exists for a grave punishment policy that does not appear to achieve one of its primary penological goals?

7.2 | Limitations

Despite the strength of the synthetic control approach, there are notable limitations associated with the method itself and the extent to which the analysis illustrates the death penalty's effect on homicide. First, discrepancies exist between preintervention characteristics of treated and synthetic control units. However, these discrepancies were reduced when augmented synthetic controls were used. A second limitation relates to the assumption that the postintervention outcomes of synthetic controls represent the counterfactual outcomes of the treated unit. As is the nature of counterfactuals, and as outlined in the National Research Council's (2012) report, "the data alone cannot reveal what the homicide rate in a state without (with) a capital punishment regime would have been had the state (not) had such a regime" (p. 7). Although counterfactual homicide rates cannot be measured, the data-driven approach of synthetic controls represents one of the most rigorous and systematic methods for estimating the effects of a policy change. Athey and Imbens (2017) described the synthetic control approach as "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey & Imbens, 2017, p. 9). The established similarity between homicide trends during at least 20 years predating each moratorium, as well as the robustness of results to various tests, should bolster confidence in the estimated effects.

Third, although the analysis estimated the treatment effect of each state's death penalty policy change, it did not examine how execution risk or frequency influences homicide rates. Therefore, a strength of the study—analyzing the effect of death penalty policies—also represents an important limitation regarding the mechanism of deterrence. It may be more appropriate to model the hypothesized deterrent mechanism of capital punishment as a measure of execution risk as opposed to a policy status. Indeed, prior panel studies have examined how a state's death penalty status impacts homicide rates, and a call for future research to examine policy changes was the basis for this analysis. Yet, it is unclear whether simply having a death penalty statute represents

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a mechanism, let alone the primary mechanism, through which capital punishment could deter homicide. How aware are potential offenders of their state's status as a retentionist or abolitionist state? The question is of central importance to analyzing whether policies in and of themselves deter, but analogous questions related to other operationalizations of potential death penalty deterrence mechanisms are equally as limiting. For example, what is the proper measure of execution risk? Should execution risk be modeled using a 6-year lag to reflect the time from sentencing to execution (Dezhbakhsh et al., 2003; Mocan & Gittings, 2003), a 1-year lag (Donohue & Wolfers, 2005; Zimmerman, 2004), a ratio of contemporaneous executions and death sentences (Zimmerman, 2004), the number of executions per 1000 prisoners (Katz, Levitt, & Shustorovich, 2003), or some other measure? As discussed in Chalfin et al. (2013) and Kovandzic et al. (2009), the assumption that homicide offenders rationally balance the costs and benefits of their actions is not consistent with criminological research findings that many people who commit homicide are under the influence of drugs/alcohol and often have histories of violent crimes (i.e., they have not been deterred). Regardless, strictly in terms of policy, the findings do not support the hypothesis that death penalty policies had a deterrent effect on homicide in four states.

There are also limitations related to the analyses of moratoriums in Illinois and Pennsylvania. The moratorium on executions announced by Governor Ryan in 2000 remained in place until the death penalty was formally abolished in Illinois in 2011. Despite the moratorium and mass sentence commutations granted in 2000, prosecutors throughout the state could continue to seek the death penalty (although death sentences were historically rare during the 11-year period) (Death Penalty Information Center, n.d.-c). With regard to Pennsylvania, there is a mean discrepancy in homicide rates of 0.27 deaths per 100,000 between Pennsylvania and its synthetic control in the 3 years preceding the moratorium. Nonetheless, subtracting the 2012–2014 difference from the postintervention treatment effect of –0.50 still results in approximately 0.23 fewer annual homicide deaths per 100,000 (an effect that is consistent with the estimate generated using ASCM).

Alternative explanations for the decreased homicide rates following moratoriums should also be considered. Despite robustness checks examining firearm policy changes, state expenditures, and impacts on other forms of crime, it is possible that concurrent changes—justice-related or otherwise—confound the reduction in homicides attributed to each state's moratorium. Perhaps the most notable criminal justice policy change that occurred near a state's moratorium is the "Truth in Sentencing" (TIS) law that took effect in Illinois in August 1995. However, the potential threat to the identification of the moratorium's treatment effect posed by the TIS law is diminished given that most of the states that comprise the main synthetic controls for Illinois passed similar laws in the 1990s to meet the federal requirement that individuals convicted of violent offenses serve at least 85% of their sentence (Ditton & Wilson, 1999).

Lastly, consistent with prior research on the death penalty, the analysis examined overall homicide rates and did not investigate whether specific forms of homicide may be deterrable by death penalty statutes. Additional research that disaggregates homicides by situational factors is needed to determine whether the death penalty deters specific types of homicide.

8 | CONCLUSIONS

Using synthetic control estimation, no evidence of a deterrent effect attributable to death penalty statutes was found in four states that implemented moratoriums. The present study addressed the shortcomings of Gius (2020) by including more preintervention years, examining the experiences

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of three additional states that adopted death penalty moratoriums, constructing synthetic controls with less prediction error, and conducting placebo tests to assess moratorium treatment effects. Synthetic and augmented synthetic control estimators indicated that the moratoriums resulted in nonsignificant reductions in homicide rates. Robustness tests and supplementary analyses were conducted to rule out other out changes that could confound the effect of the moratoriums. The precise mechanism by which state-level moratoriums might reduce homicide rates warrants further exploration. Future research should continue exploring the effects of death penalty moratoriums through synthetic control studies that utilize recent methodological innovations, as well as qualitative analyses that assess decision making in homicide cases.

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CONFLICT OF INTEREST

The author declares no conflict of interest.

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