The Deterrent Effect of Death Penalty Eligibility: Evidence from the Adoption of Child Murder Eligibility Factors

Michael Frakes, *Petrie-Flom Center for Health Law Policy, Biotechnology and Bioethics, Harvard Law School,* and Matthew Harding, *Stanford University*

We draw on variations in the reach of capital punishment statutes between 1977 and 2004 to identify the deterrent effects associated with capital eligibility. Focusing on the most prevalent eligibility expansion, we estimate that the adoption of a child murder factor is associated with an approximately 20% reduction in the child murder rate. Eligibility expansions may enhance deterrence by (i) paving the way for more executions and (ii) providing prosecutors with greater leverage to secure enhanced noncapital sentences. While executions themselves are rare, this latter channel may be triggered fairly regularly, providing a reasonable basis for a general deterrent response. (*JEL* K14, K42)

1. Introduction

Capital punishment has long been one of the most controversial topics in the political and moral discourse in the United States. The death

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Send correspondence to: Michael Frakes, Harvard Law School, Petrie-Flom Center, 23 Everett St., 3rd floor, Cambridge, MA 02138, USA; Telephone: 617-384-5475; Fax: 617-384-5001; E-mail: mfrakes@law.harvard.edu.

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penalty debate persists on numerous fronts today, as legislatures and courts continue to evaluate the general propriety of capital punishment, along with the propriety of its specific components (e.g., execution methods). In 2007, the New Jersey legislature even went so far as to repeal its death penalty statute entirely. New Mexico has recently followed suit. Those who continue to support the death penalty identify its potential to deter future homicides as a principal justification for its application. The existence of a deterrent effect itself, however, remains a controversial subject. An extensive empirical literature has attempted to estimate the association between homicide rates and the use of capital punishment. The literature to date, however, has presented a set of largely mixed and uncertain results.

In this paper, we take up this empirical task and estimate certain deterrent forces associated with capital punishment using a novel source of variation in death penalty legislation: the within-state expansion of capital-eligibility factors over time. In accordance with relevant Supreme Court doctrine, states emerged from the death penalty moratorium of the 1970s by enacting statutes that restricted the application of capital punishment to homicides that meet certain delineated characteristics. Since the post-moratorium reinstatements, virtually every state has periodically added to its list of eligibility criteria. Studies that use variation in death penalty laws to test for deterrence generally focus on the extensive margin: does the relevant state have a death penalty statute in effect? We are aware of no study that has explored variations along the intensive margin attributable to within-state eligibility expansions.

We focus our deterrence analysis on a relatively targeted investigation into the relationship between child murder eligibility provisions and child murder rates, derived using incident-level homicide data from the Federal Bureau of Investigation's (FBI) Supplementary Homicide Reports (SHR). This child murder analysis offers the richest level of legislative variation among the possible individual eligibility factors and thus facilitates the estimation of a specification that may be well suited to address the limitations of a difference-in-difference research design. In our preferred specification, we identify 16 states that amended their capital punishment eligibility statutes between 1985 and 2001 to include a specific provision for child murders. While some amount of child murders would have been eligible for capital punishment prior to these amendments under the remaining eligibility characteristics, most of the relevant treatment states lacked alternative provisions flexible enough to cover the entirety of this ground.

In each child murder specification, we include a general eligibility measure to control for the scope of the remaining eligibility factors prevailing in the relevant state and year. This general measure also facilitates a falsification exercise in which we explore the relationship between child murder rates and the addition of eligibility factors that do not specifically target child homicides. To parameterize these multifaceted eligibility expansions, we embrace the incident-level nature of the SHR data and simulate the proportion of national homicides that would be eligible for the death penalty in each state–year cell based on (i) the death penalty laws prevailing in the relevant state and year and (ii) the observed characteristics of the individual homicides. This simulation approach is inspired by studies in public finance and labor economics that identify certain economic relationships using within-state variations in delineated sets of eligibility criteria—for instance, variations in the eligibility criteria for Medicaid coverage (Currie and Gruber, 1996).

Much of the existing death penalty literature has focused on estimating the deterrent effects associated with the application of the death penalty—that is, death sentences and executions. However, the death penalty is so rarely employed in the United States and there is often such a large gap between the time of sentencing and the time of execution, that any association between homicide rates and these application measures is likely to be small in magnitude (if it exists at all). An expansion of capital punishment eligibility, such as that considered in this paper, may lead to enhanced deterrence under two channels: (i) by leading to an increase in the number of capital sentences imposed and the number of executions performed and (ii) by providing the state with greater leverage to prosecute murderers and thus to secure stronger punishments, even those noncapital in nature (Kuziemko, 2006). While the first channel may be triggered in rare instances only, the effects arising from an enhanced prosecutorial bargaining position should be felt with much greater frequency given the proportion of homicides that meet the necessary eligibility requirements. Accordingly, it may be plausible to expect a stronger deterrent response resulting from a strengthening of a prosecutorial bargaining position.

We find evidence of a negative association between child murder rates and the addition of child murder provisions to capital eligibility statutes.

Specifically, we estimate that the addition of a specific eligibility factor for child murder is associated with an approximately 20% reduction in the homicide rate of youth victims. The association between homicide rates and child murder expansions appears to be isolated to the case of child homicides, as we find no evidence of a comparable relationship between child murder provisions and adult homicide rates. We also find no relationship between child murder rates and general eligibility expansions that are not specific to child murders. Moreover, these results do not appear to be driven by state-specific trends that predate the adoption of child murder eligibility laws.

We estimate results of similar magnitude when we turn to the estimation of a general eligibility specification that explores the relationship between a more general homicide rate and a simulated eligibility measure that draws on the full range of eligibility criteria (including child murders). However, these results are relatively noisy and do not hold up well to the exclusion of the child murder factor from the general simulation calculation.

The paper proceeds as follows. In Section 2, we review the existing literature and discuss the nature of our contributions. In Section 3, we discuss the nature of the expansions in capital punishment eligibility criteria that have taken place over the last several decades. In Section 4, we discuss the data and illustrate the two approaches that we take in the parameterization of capital eligibility laws. In Sections 5 and 6, we present the empirical methodology and discuss the estimation results. Finally, in Section 7, we conclude.

2. Death Penalty and Deterrence

Theories on criminal behavior provide an ambiguous prediction regarding the impact of capital punishment. On the one hand, the threat of the death penalty may operate to increase the expected costs of murder and thus reduce incentives to engage in homicidal behavior. On the other hand, executions may stimulate more homicides by validating the social acceptability of retributive actions (Shepherd, 2005). Moreover, even ruling out the possibility of this latter "brutalization" effect, the deterrent effect of capital punishment relies on the existence of certain preconditions. For instance, criminals must understand and acknowledge death penalty probabilities and must find this form of punishment sufficiently more severe than the alternatives.[1] Given this underlying theoretical ambiguity, further analysis becomes critical to understanding whether this principal justification for capital punishment holds empirical merit. Accordingly, a long line of empirical studies has endeavored to estimate the deterrent impact of the death penalty.

While earlier sociology studies did exist, the deterrence literature largely took off with Ehrlich's (1975) analyses of 1933–1969 national time-series data. Ehrlich's findings suggested that each execution leads to eight fewer homicides.Ehrlich's analysis received significant attention from policymakers and academics alike and his findings inspired a slew of follow-up studies, many of which subjected these results to a range of specification checks.[2] The results of these studies varied markedly, throwing substantial uncertainty on the question of whether capital punishment deters criminal behavior.

The past decade has seen a resurgence of this literature, with a number of studies taking advantage of jurisdiction-level panel data on homicide rates during the post-moratorium period. Many of these recent studies present evidence of significant and far-reaching deterrence effects.[3] Donohue and Wolfers (2006) take an intensive look at this recent wave of papers and attempt to replicate many of the key findings. Their analysis demonstrates the sensitivity of these recent findings to a host of specification checks and other modifications, including: (i) the use of alternative sample periods, (ii) the addition of certain control groups, and (iii) the treatment of within-group autocorrelation in estimating standard errors. We defer to Donohue and Wolfers' paper for a full discussion of the fragility of recent deterrence studies; however, we identify a couple of their more general observations about the limitations of the existing literature.

Donohue and Wolfers' primary insight is to cast doubt on the ability to estimate an association between homicide rates and measures of the intensity with which capital punishment is implemented. Given that any such relationship may be limited in magnitude and given the rarity of death penalty sentences and executions, it may simply be too difficult for the

1. See Hjalmarsson (forthcoming) for a more complete discussion on the conditions that are likely required for capital punishment to deter criminal behavior.

2. See Cameron (1994) for a review of many of the post-Ehrlich studies.

3. Fagan et al. (2006) provides a list of recent studies claiming such effects. Examples include Mocan and Gittings (2003), Shepherd (2004), Dezhbakhsh et al. (2003), Dezhbakhsh and Shepherd (2006), and Zimmerman (2004).

econometrician to separate the impact of these rare occurrences from the effects of other factors driving the large fluctuations in homicide rates.[4] This difficulty may help to explain the substantial sensitivity of much of the results generated by the literature to date.

In addition, Donohue and Wolfers identify a second major concern generally confronted by the deterrence literature: omitted-variables bias. This concern is particularly pronounced in those studies that identify deterrence using variations in the intensity with which capital punishment is applied. Prosecutors, after all, may be subject to political and other influences in deciding whether to pursue the death penalty, where those influences may themselves be correlated in some fashion with observed homicide rates (e.g., "get tough on crime" philosophies). Similar confounding factors may also shape the decisions of the juries that ultimately impose the death penalty, along with the incentives of the state to push for executions in the post-sentencing period.

In the empirical analysis below, we explore the deterrent effect of capital punishment with these two empirical concerns in mind. Donohue and Wolfers' first concern regarding the rarity of the application of the death penalty is largely statistical in nature; however, partially underlying this concern is the idea that the true relationship between these application measures and homicide rates may itself be small in magnitude. From a theoretical perspective, the limited scope of this relationship may also follow from the infrequency of executions and from the large delays between sentences and executions (Katz et al., 2003). In the present study, we avoid estimating a specification that considers only the deterrent effects ensuing from those rare instances in which the death penalty is actually applied (i.e., sentences and executions). By exploiting variation in the existence or extent of capital punishment legislation, we draw on an additional source of criminal deterrence that is less prone to this theoretical concern: prosecutorial leverage. That is, regardless of how often capital punishment is actually employed, as long as the threat of its use remains viable in the face of an alleged murderer, the possibility of capital punishment may provide prosecutors with greater leverage to negotiate pleas with alleged murderers. These heightened negotiations may lead to stronger overall punishments, which may, in turn, deter further criminal behavior. Given the large proportion of murders that

4. See Katz et al. (2003) for further discussion regarding this empirical dilemma.

are at least eligible for capital punishment, these prosecutorial forces may be expected to operate quite frequently, providing an arguably more plausible basis for a sizeable deterrent effect. With the possibility of a stronger deterrent channel, the resulting estimates may be robust even in the face of widely varying homicide rates and may be less sensitive to specification error than those estimates based on an evaluation of the intensity by which capital punishment is applied.

Kuziemko (2006) provides evidence in support of the contention that capital punishment eligibility leads to greater bargaining power in the hands of prosecutors. Estimating differences-in-differences-in-differences models using the 1995 reinstatement of the death penalty in New York, in connection with variations in the propensity of county prosecutors to pursue the death penalty, Kuziemko estimates that the death penalty leads defendants to accept plea bargains with harsher terms, while finding no impact on a defendant's propensity to plead guilty itself. The harsher punishments ensuing from capital punishment under this bargaining story may, in turn, lead to a general deterrent effect on criminal behavior. Any such deterrent effect does not require that potential offenders be aware of the actual extensions in the death penalty laws. Rather, potential offenders only need to respond to an observation of harsher sentences being imposed on their criminal counterparts, where those harsher sentences may follow from the prosecutorial forces associated with capital eligibility.

By drawing on a source of legislative variation that may lead to deterrence through both of the channels identified above, our results should be interpreted appropriately. We are estimating neither the separate relationship between homicide rates and the application of the death penalty (as measured by execution and sentencing rates), nor the separate relationship between homicide rates and some metric of prosecutorial leverage. Rather, the results generated from our specification can be interpreted as the relationship between homicide rates and capital eligibility itself. That is, eligibility takes on a distinctive presence in this analysis.

With respect to Donohue and Wolfers' second major concern, we address a potential omitted variables problem by estimating difference-in-difference specifications that exploit within-state variation in capital punishment eligibility statutes. Drawing on state-specific eligibility expansions allows us to address unobserved factors by controlling for fixed differences across states, fixed differences across time periods, and state-specific linear time trends.

The variation in statutory eligibility laws is arguably less sensitive, though still not immune, to confounding influences.

Of course, other deterrence studies have explored "natural-experiment" methodologies based on variations in state capital punishment laws. For instance, Dezhbakhsh and Shepherd (2006) use state abolitions of the death penalty, along with their subsequent reinstatements, during the period surrounding the death penalty moratorium of the 1970s to estimate a fixed-effects deterrence specification. While Donohue and Wolfers (2006) demonstrate the sensitivity of Dezhbakhsh and Shepherd's (2006) deterrence findings to certain specification checks (e.g., the inclusion of year fixed effects, as opposed to decade fixed effects), the abolitions and reinstatements of the death penalty considered by Dezhbakhsh and Shepherd nonetheless present an interesting set of variations by which to test the deterrent impact of capital punishment. Moreover, on–off policy changes of this nature will also pick up general deterrent effects arising from enhanced prosecutorial bargaining power.

However, while a large number of states experienced policy changes in the time period surrounding the national death penalty moratorium of the 1970s, most of this variation occurred at identical moments of time over a large number of states. In modeling abolitions of death penalty statutes, Dezhbakhsh and Shepherd consider three abolitions in the pre-1972 period (including New York in 1965), along with thirty-four abolitions in 1972, thirty-two of which occurred as a result of the Supreme Court's decision in *Furman v. Georgia*. This variation can thus be seen as attributable to a relatively limited number of actual policy changes. As such, with an arguably small number of effective treatment groups, the results of this abolition model implicate concerns over the consistency of the estimated coefficients along with the appropriateness of the standard methods of inference performed (Conley and Taber, 2005). Of course, a difference-in-difference model exploiting the on-off variation associated with the national moratorium of the 1970s, as estimated by Dezhbakhsh and Shepherd, also draws on the subsequent reinstatements of the death penalty statutes. The vast majority of these reinstatements effectively occurred in the 1-year period surrounding the 1976 *Gregg* decision by the Supreme Court. However, seven states did reenact their death penalty statutes in subsequent years (between 1978 and 1995).

In the eligibility-expansion model that we estimate below, we draw on a far more staggered set of policy changes than that possible by an exploration of the abolitions of the early 1970s. Moreover, while our primary specifications focus solely on expansions of existing death penalty statutes, we also estimate specifications that parameterize general eligibility laws in such a fashion that we necessarily draw on the same set of post-*Gregg* statutory reinstatements considered by Dezhbakhsh and Shepherd (2006). In light of the rich level of policy variation available from a model that embraces post-moratorium eligibility expansions, our analysis may serve as a novel contribution to an empirical literature plagued with limitations in the amount of information available to identify deterrent effects.

Moreover, in addition to drawing on an extensive set of eligibility expansions, by taking advantage of incident-level homicide data made available in the post-moratorium period, we are able to target the deterrence analysis on the set of homicides that are generally implicated by capital punishment statutes—that is, those homicides that are potentially eligible for capital punishment. With this approach, we may derive more precise deterrence estimates by removing any noise arising from variations in the rates of non-capital-eligible homicides. This approach is in the spirit of Fagan et al. (2006) who estimate the association between the application of capital punishment (e.g., capital sentence and execution rates) and the rates of potentially-death-eligiblehomicides (and between the general incidence of capital punishment statutes and potentially death-eligible homicides). However, we are aware of no study that has used variations in the underlying eligibility factors themselves as a source of exogenous variation to identify the deterrent effect of capital punishment.

3. Capital Punishment Eligibility

The Supreme Court effectively voided the capital punishment statutes of all death penalty states with its 1972 decision in *Furman v. Georgia* (and companion cases), 408 U.S. 238 (1972), expressing concern over the unbridled discretion granted to juries in imposing death sentences. This decision suspended capital punishment in the United States until the Supreme Court's 1976 decision in *Gregg v. Georgia*, 428 U.S. 153 (1976), in which the Court upheld newly enacted death penalty statutes that provided juries with guided discretion in capital cases. The Court in *Gregg* specifically upheld a Georgia statute that bifurcated capital trials into guilt and sentencing stages, where juries in the latter stage were required to determine the existence of certain aggravating circumstances and then weigh those factors against other mitigating considerations. This process serves the function of both (i) providing juries with clear and objective guidance and (ii) narrowing the class of crimes eligible for capital punishment (Kirchmeier, 2006).

In the aftermath of these decisions, new death penalty statutes set the scope of capital eligibility either by restricting the definition of capital murder itself or by delineating a set of aggravating circumstances for juries to consider during sentencing stages (Kirchmeier, 2006). Reviewing various statutory materials, we track the evolution of each state's list of eligibility factors/aggravating circumstances from the mid-1970s to the present.[5] From the beginning of the post-moratorium period, states did vary somewhat in the set of eligibility factors that they selected. Nonetheless, certain factors appeared rather consistently across these initial statutes, including: (i) murders of police officers or public officials; (ii) murders committed by those with previous felony convictions; (iii) murders by those who knowingly created a great risk of death to more than one person by means of a destructive device; (iv) felony murders (usually robberies, rapes, burglaries and arsons); (v) murders committed for pecuniary gain; and (vi) murders committed to avoid arrest. Various other factors were found across some initial death penalty statutes, including murders committed while under incarceration, murders of witnesses in legal proceedings, and murders involving especially heinous or atrocious behavior (e.g., torture).

Several states did expand their eligibility statutes to add some of the above factors (e.g., arson-related homicides) in the years following their initial reinstatements. However, the bulk of the post-reinstatement expansions involved the following identifiable factors: child murder (sixteen states), multiple-victim murders (nine states), murders committed in connection with infractions of narcotics laws (nine states), murders associated with gang-related activities (four states), elderly murders (four states), and

5. We codify those aggravating circumstances that can be identified by the SHR data. Table 1 indicates the year in which specific child murder provisions became effective. The code used to assign capital eligibility status for each homicide in the SHR sample based on the prevailing eligibility laws of each state–year cell can be obtained from the authors upon request.

Table 1. Expansions of Death Penalty Statutes to Include Murders of Youth Victims

State	Year of adoption	Operable age cutoff[a]	State	Year of adoption	Operable age cutoff[a]
Mississippi[b]	1983	"child"	Delaware	1995	15
Arizona	1985	15	New Jersey	1995	14
Louisiana	1986	12	Connecticut[b]	1996	16
South Carolina	1986	12	Florida[c]	1996	12
Indiana	1987	12	Nevada	1996	14
Wyoming	1989	17	South Dakota	1996	13
Pennsylvania	1990	12	Ohio	1998	13
Alabama[b]	1992	14	Oregon	1998	14
Colorado	1994	12	Virginia	1998	14
Texas	1994	6	Arkansas	2001	13

[a]Homicides of victims below the indicated age are eligible for capital punishment in the relevant state (post-reform). We assume a cutoff of 16 for those specifications that include Mississippi, which extends eligibility to instances of deaths resulting from the abuse of a child and where subsequent case law has verified that this provision is triggered by the killing of a child by any means. See, for example, *Stevens v. State*, 806 So. 2d 1031 (Miss, 2001).
[b]These states effectively place different sets of restrictions on capital eligibility in both the definition of capital murder and the list of aggravating circumstances to consider during sentencing. We exclude these states from our preferred specifications given the difficulty in determining how these dual sets of restrictions interact with each other.
[c]Florida is only represented in the SHR prior to 1996.

murders committed during carjackings (five states).[6] Moreover, in the case of many of these eligibility factors, the statutory expansions occurred in a relatively staggered manner over the sample period. For instance, as illustrated in Table 1, five of the child murder policy changes occurred during the 1980s, five during the early 1990s, five during the late 1990s, and one following the turn of the century.Additions of narcotics-related homicides, on the other hand, largely occurred over a very narrow time period (1989–1990).

We ultimately attempt to combine all of this variation in some rational fashion into a single specification and evaluate whether eligibility extensions are generally associated with an average reduction in homicide rates.

6. In summarizing the number of states that modify eligibility laws to add the identified provisions, we exclude those states that effectively impose a two-tiered eligibility process, as discussed below. These figures also exclude Florida, which is absent from the homicide sample after 1996 (we omit Florida from the specifications for this reason; however, we estimate virtually identical results when we include Florida's pre-1996 years). With respect to the number of states that add multiple-murder eligibility provisions, we exclude New York, New Jersey, and Kansas, which add multiple-murder eligibility provisions contemporaneously with the reinstatement of their capital punishment laws.

To facilitate this exercise in the face of vastly different eligibility categories and in the face of multiple expansions within the same state over time, we turn to a parameterization methodology that effectively simulates a state's propensity to extend capital eligibility to a given homicide. As discussed further below, however, this general eligibility investigation may be limited by measurement error involved in the simulation process and by other concerns stemming from the necessary use of a broadly defined and widely varying homicide rate. For these reasons, we focus the analysis on the estimation of a difference-in-difference specification that uses a more limited homicide rate and that draws on expansions of eligibility laws covering a single homicide type: the murder of youth victims.

With sixteen statutory amendments (as many as twenty in some specifications), child murder eligibility expansions represent the most common of the relevant policy changes over the sample period. Eligibility categories for multiple-victim homicides and narcotics-related homicides were also added by nine states over the same period; accordingly, we do present difference-in-difference results for these additional expansions in Section 6.4 below. The multiple-victim homicide specification generates results of a similar nature and magnitude to those generated by the child murder specification. The narcotics-related adoptions, however, largely occurred together in the 1989–1990 period, leaving few effective treatment groups. Similarly, each of the additional eligibility categories (e.g., homicides of elderly victims) only varies over a small number of states throughout the sample period. With few effective treatment groups, estimating separate difference-in-difference specifications for each of these eligibility types would raise concerns regarding the consistency of the estimated eligibility coefficients and of the resulting standard errors (Conley and Taber, 2005).[7] The child murder specification is perhaps best suited to address the limitations of a difference-in-difference approach. In any event, considering the possibility of estimating eligibility specifications of a similar nature for the remaining eligibility types, we do address inference on the child murder results with a consideration of possible family-wise error.

7. State–year shocks unrelated to the legislative variables may nonetheless be spuriously correlated with such variables. Spurious correlations of this nature should dissipate, on average, with a greater number of policy variations. With few treatment groups, the resulting estimates may not be reflective of an actual policy response.

Moreover, adoptions of child murder factors represent a considerable expansion in death penalty eligibility considering that roughly 5% of the homicides in our sample were committed against victims under the age of 15 and that the average state in our sample only extends capital eligibility to 16% of total homicides (based on the observed characteristics of the homicides in our sample). Of course, some child murders would have been eligible for capital punishment prior to these statutory amendments based onthe remaining eligibility factors, in which case these figures may overstate the extent of the expansion. For instance, certain states would have captured some range of child murders under the "especially heinous, atrocious or cruel" (HAC) aggravating factor. While all murders arguably meet the definitions of these words, courts are not meant to use this factor as a catch-all category and are generally required to restrict its application to extraordinary situations involving, for instance, wanton and depraved infliction of serious physical pain (e.g., torture).[8] However, many courts have nonetheless taken a flexible approach with this aggravating factor and some have allowed consideration of the helplessness of the victim in determining whether this condition has been met.[9]

In any case, it is reasonable to expect that the addition of a specific eligibility factor for child murder will indeed lead to the extension of capital eligibility to a large number of child murders. First of all, out of the sixteen treatment states that adopted child murder eligibility provisions over the sample period, only four provide for an alternative eligibility factor concerning murders of an HAC-like nature.[10] The remaining treatment states either include no such factor (e.g., Ohio) or avoid the use of this vague terminology and specifically limit capital eligibility to instances of torture (e.g., Pennsylvania).[11] Out of those control states that did not amend their

8. See, for example, *Godfrey v. Georgia*, 446 U.S. 420 (1980).

9. See, for example, *Arizona v. Gretzler*, 135 Ariz. 42 (1983). For a more complete discussion on the flexibility taken by courts in applying HAC-like aggravating factors, see Rosen (1986) and Kirchmeier (1998).

10. These states include Arizona, Colorado, South Dakota, and New Jersey. Delaware also includes such a provision in its statute; however, the Delaware Supreme Court invalidated this provision in a 1981 decision. *In re Petition of State for Writ of Mandamus*, 433 A.2d 325.

11. These latter states take two different approaches to limiting eligibility to instances of torture. Most of the relevant states avoid the use of any HAC-like language and only extend eligibility to instances of torture. Wyoming and Arkansas, on the other

eligibility statutes over the sample period to add child murder provisions, a larger proportion of them provide for an HAC-like alternative factor. Thus, it is possible that the treatment states, in enacting specific child murder provisions, were responding to the perceived inability of the remaining factors to extend capital eligibility to child homicides.

Second, when applicable, the helplessness of the victim is but one factor to consider in determining whether murders are of an especially HAC-like nature. Even when courts consider the helplessness of the victims in an HAC-like analysis, they nonetheless continue to stress other circumstances of the homicides, including the seriousness of the pain inflicted or the depraved state of mind of the offender.[12] Thus, there is little evidence to suggest that such states would extend capital eligibility to all instances of child homicide under an HAC factor. Moreover, given the ill-defined and controversial nature of this eligibility factor (Rosen, 1986), it is quite reasonable to believe that the separate delineation of a child murder aggravating factor will strengthen the state's case for capital punishment and provide for an additional, clearly defined aggravating factor that may be used in outweighing any determined mitigating circumstances.[13]

hand, include eligibility factors regarding murders that are especially atrocious or cruel (Wyoming) or especially cruel or depraved (Arkansas), while specifying (in the statutory language itself) that murders meet these conditions when they involve the infliction of torture (or serious physical abuse or mental anguish, in the case of Arkansas).

12. See, for example, *Gretzler*, 135 Ariz. at 52–3 ("[t]he mere existence of senselessness or helplessness of the victim, in isolation, need not always lead to a holding that the crime is heinous or depraved, however").

13. In 1983, Illinois amended its capital punishment statute to extend eligibility to situations in which "the murdered individual was under 12 years of age and the death resulted from exceptionally brutal or heinous behavior indicative of wanton cruelty." Section 720 ILL. COMP. STAT. 5/9–1 (2009). Given the above treatment of HAC-like factors, it would be inconsistent to treat Illinois's statute as a pure child murder eligibility factor. As such, we exclude Illinois from the child murder specifications. However, the results presented below remain virtually unchanged when we include Illinois and treat this provision as representing a child murder eligibility provision of the same nature as that adopted by the remaining treatment states. In the general deterrence specifications, which draw on additional eligibility expansions, we include Illinois in the regression analysis but drop those years prior to 1983. We drop observations from Illinois in the 2000–2004 period given the uncertainty in its capital punishment environment following the moratorium on executions announced by Governor Ryan.

4. Data and Parameterizations of Eligibility Laws

Homicide data from 1977 to 2004 come from the FBI's SHR. The SHR is an incident-level database containing information on various individual characteristics of reported homicides, including (i) the time and location of the offense; (ii) certain victim characteristics (e.g., age, race, etc.); (iv) certain offender characteristics; (v) the weapon used; and (vi) the circumstances of the homicide (e.g., during robbery). This information is provided each month to the FBI by local law enforcement agencies participating in the FBI's Uniform Crime Reporting Program. While not completely inclusive, the SHR sample contains information on just over 90% of the homicides that occurred over the sample period. The SHR provides individual weights to allow state–year SHR homicide counts to match the more complete state–year homicide rates reported under the FBI's Uniform Crime Reports. We use unweighted observations for the primary analysis below. However, as indicated in Section 6, we estimate nearly identical results when we incorporate the provided weights.

To form the dependent variables used in the specifications estimated below, we aggregate the SHR homicide records into state–year cells and calculate various state–year homicide rates.[14] We use different homicide rates for the different regression specifications estimated below, for example, murder rates of youth victims for the child murder eligibility models. Thus, while the specifications are of an aggregate nature, we draw on the provided individual homicide characteristics to tailor the state–year cells to particular classes of homicides. We discuss these calculations in further detail in Section 5 below. Table 2 provides descriptive statistics for the homicide rates and relevant eligibility law variables. We match data on certain covariates to each of these state–year cells. We control for the following state–year measures: unemployment rate, incarceration rate, policeemployment rate, police expenditure rate, judicial/prosecutorial expenditure rate, percentage of population 15–19 years old, percentage of population 20–24 years old, percentage of black population,

14. Consistent with the relevant literature (see, for example, Fagan et al., 2006), we calculate homicide rates by counting records from the SHR category for murders and non-negligent manslaughter. We use the terms "homicide" and "murder" interchangeably throughout to refer to killings of this nature.

Table 2. Means and Standard Deviations of Selected Variables

Variable	Mean (standard deviation)
Child murder eligibility law dummy	0.23 (0.42)
Simulated percentage of child murders (under 17 years of age) eligible for death penalty	0.13 (0.24)
Simulated percentage of "potentially death-eligible" murders eligible for death penalty	0.47 (0.25)
Simulated percentage of potentially death-eligible murders eligible for death penalty (excluding child murder factor from simulation and murder-rate calculation)	0.50 (0.27)
Homicide rate: victim age <15 years (per 100,000 people <15 years old)	1.63 (0.63)
Homicide rate: victim age <5 years (per 100,000 people <5 years old)	3.13 (1.26)
Homicide rate: victim age <10 years (per 100,000 people <10 years old)	1.93 (0.74)
Homicide rate: victim age ≥20 years (per 100,000 people ≥ 20 years old)	8.46 (4.08)
Homicide rate: potentially death-eligible homicides (per 100,000 people)	2.43 (1.28)
Homicide rate: potentially death-eligible homicides (excluding child murder factor, per 100,000 people)	2.19 (1.23)

The 1977–2004 homicide data are from the Federal Bureau of Investigation's Supplementary Homicide Reports (SHR). Homicide rates are calculated at the state–year level and are derived from a national sample of homicides, with an average annual sample size of roughly 17,000. Homicides are excluded from the calculation where the offender is under 16 years of age. Reported statistics are then presented for a sample of 1154 state–year cells, weighted by the total population of the relevant state and year. The sample excludes states that effectively impose a two-tiered eligibility process. The sample excludes state–year cells during which the relevant portions of the death penalty statutes were deemed unconstitutional or during which the constitutionality of such provisions was uncertain. Denominators used for the age-specific homicide rates are based on the population within the relevant age group. Denominators used for potentially death-eligible homicide rates are based on total population counts for the relevant state and year.

percentage of population living in urban areas, and median household income.[15]

To each state–year cell containing homicide and covariate information, we also merge measures indicating the status of death penalty eligibility laws in effect in the relevant state and year. We parameterize eligibility laws in two basic manners. First, we consider the amendment of capital punishment statutes to extend eligibility to certain specific types of homicides, primarily

15. State–year unemployment rates are from the U.S. Bureau of Labor Statistics. Demographic measures and percent urbanization are from decennial Census files (1969–1999) and American Community Surveys (ACS) (2000–2006). Population measures are from the Census population estimates. Incarceration rates are from the Bureau of Justice Statistics. Police employment data are from the Uniform Crime Reports, while expenditure data on police operations and on judicial and prosecutorial operations are from the Criminal Justice Expenditure and Employment (CJEE) Extracts (1982–2004) and the CJEE Surveys (1977–1981).

child murder. That is, we match to each state–year cell a binary indicator variable that equals 0 during those years in which a state does not specifically include child murder as an eligibility factor/aggravating circumstance and 1 during those years in which it does provide for this factor. In the specifications estimated below, we drop the state–year cell corresponding to the year of the law change itself in order to avoid any difficulty in assigning indicator variables to mid-year adoptions.[16]

Second, instead of confining the analysis to the effect of specific eligibility expansions (i.e., child murder), we attempt to draw on the full range of eligibility expansions that occur over the sample period. Rather than including a set of individual indicator variables for each such factor, we parameterize eligibility statutes along these more general lines using a single measure of the propensity of a given state to provide capital eligibility for a given murder. Having documented the evolution of each state's eligibility statutes over time, we apply the operable statutes of each state–year cell to a sample of individual homicides in order to simulate the likelihood that a given homicide will be subject to capital eligibility. More specifically, we do the following calculation for each state–year cell: (1) determine whether each individual homicide from a national sample of homicides (for the relevant year) is eligible for capital punishment based on the laws of the state–year cell under investigation and the reported characteristics of the individual homicide[17] and (2) calculate the proportion of the national annual sample of homicides that is eligible for capital punishment based on the individual simulations from Step (1).[18] Repeating this procedure for each state–year cell gives a full set of simulated eligibility percentages. In forming these

16. The estimation results presented below are nearly identical when we instead set the child murder indicator equal to 1 in the year of adoption if the effective date of the law change occurs in the first half of the year and equal to 1 in the following year if the effective date falls in the latter half of the adoption year.

17. The national sample of homicides used to simulate an eligibility percentage for a given state excludes the homicides associated with that state. We take separate national samples for each year to form the simulated measure for each state in that year. However, we generate nearly identical results (not shown) when we take the full national sample over all years (or from just one given year, e.g., 1996) and apply that fixed national sample to the laws of each state and year.

18. We form eligibility percentages based on the SHR victim file, which provides one record for each homicide victim. The SHR data also include an alternative organization of homicide records that provide one record for each offender. We estimate nearly identical results when we use the SHR offender file to generate the simulated eligibility measures.

simulated measures, we confine the underlying national sample of homicides to the universe of "potentially death-eligible" homicides—that is, the set of homicides with characteristics that would trigger capital eligibility in at least one state.[19]

The resulting variable can be thought of as a measure of the extent to which any given state draws from the universal list of eligibility characteristics. Additions of eligibility factors in a state translate into higher simulated percentages. The addition of a specific factor will contribute to this simulated percentage according to the joint likelihood of observing that specific factor, together with accompanying factors, in a given homicide.

We apply this simulation approach in two ways in the death penalty analysis presented below. First, in the primary child murder specification, we include a general simulation measure in order to control for the extent of the remaining eligibility provisions in effect for the relevant state and year. In calculating this general covariate measure, we exclude the child murder eligibility factor from the simulation exercise. This control is important in light of the reasonable likelihood that child murder eligibility provisions are correlated with the presence of additional eligibility factors. As discussed further in Section 6 below, this general covariate measure also facilitates a falsification exercise in which we estimate the effect of general eligibility provisions (other than child murder) on child murder rates. Second, we apply this simulation methodology in alternative specifications that explore a more general relationship between eligibility and homicide rates. In such specifications, we calculate a general simulation measure based on all eligibility factors, including child murder.

By using a national sample of homicides to simulate the above eligibility percentages, rather than a state-specific sample, we abstract from state-specific factors (other than eligibility laws) that may confound the empirical analysis below by contributing both to state-specific homicide rates and to observed measures of a state's likelihood of extending capital eligibility to given homicides. That is, this simulated eligibility measure is designed to capture variations in eligibility laws themselves and not variations in the state-specific applications of these laws. This simulation methodology is

19. As discussed further in Section 5 below, in calculating these measures, we also exclude from the underlying national sample all homicides committed by offenders under the age of 16.

motivated by a number of studies in public finance and labor economics. Representing one of the pioneering applications of this approach, Currie and Gruber (1996) draw on within-state changes in Medicaid eligibility rules over time to estimate the effect of Medicaid eligibility on healthcare utilization and outcomes. To abstract from individual- and state-specific factors that may be correlated with both utilization and eligibility propensities, they instrument individual eligibility with simulated measures of the percentage of children in randomly drawn national samples (within age groups) that are eligible for Medicaid based on the prevailing eligibility rules for the relevant state–year–age group.

On average, there are roughly 6200 potentially death-eligible homicides in the annual national samples from which these simulated eligibility percentages are derived. The SHR does not contain enough information to allow for a perfect eligibility calculation. For instance, it would not be possible to determine whether the homicide was committed against a potential witness to a crime or whether the murder involved torture. Nonetheless, the SHR does provide numerous homicide characteristics that implicate eligibility provisions, including the ages of the victims and offenders, the weapon used (e.g., explosives), and the following circumstances of the homicide: (i) robbery; (ii) rape; (iii) burglary; (iv) arson; (v) certain other felonies (e.g., auto theft); (vi) institution killing (e.g., prison homicide); (vii) narcotics-related; and (viii) gang-related. Moreover, the SHR files also allow for identification of those instances in which more than one victim were killed during this incident, another common eligibility factor. While incomplete, the information provided by the SHR data allows for the identification of a substantial majority of death-eligible homicides. Fagan et al. (2006) use the first 100 executions that occurred following March 1, 2006 as a benchmark to test the accuracy of a capital-eligibility assignment process based on SHR data. Using court records, they identify the aggravating circumstances established during the course of the proceedings of those benchmark cases. Based on the characteristics of the homicides associated with these executions, they then found that the categories available in the SHR files would have identified capital eligibility in all but five of these one hundred cases.[20]

20. The proceedings in two of those five unidentified cases, for instance, based the death sentence on the heinousness or atrociousness of the killing, without also basing it on another factor that would have been identified by the SHR data. Of course, Fagan et al.'s

5. Methodology

We draw on within-state variations in the scope of capital punishment laws to identify the deterrent effect associated with capital eligibility, as distinct from the more specific deterrent effect associated with the application of capital punishment, that is, executions and death penalty sentences. Specifically, we estimate the following specification:

$$H_{s,t} = \alpha + \gamma_s + \lambda_t + \varphi_{s,t} + \beta_1 X_{s,t} + \beta_2 ELIG_{s,t} + \varepsilon_{s,t}, \qquad (1)$$

where s indexes state, t indexes year, and $X_{s,t}$ represents various state–year covariates (e.g., unemployment rate). State fixed effects, γ_s, and year fixed effects, λ_t, control for fixed differences across states and across years, respectively. We include a set of state-specific linear time trends, $\varphi_{s,t}$, to control for slowly moving correlations between state homicide rates and expansions of capital punishment eligibility criteria.

The relevant eligibility variables are included in $ELIG_{s,t}$. The construction of these variables is discussed in greater detail in Section 4 above. We estimate two essential types of specifications. In our primary specifications, we focus on the factor that contributes most to the within-state variation in eligibility laws over time: child murder. In such specifications, *ELIG* includes an indicator variable for the presence of a law making the murder of a youth victim specifically eligible for capital punishment. In each child murder specification, we also control for the scope of the remaining eligibility provisions by including a single variable that simulates the percentage of potentially death-eligible homicides that is eligible for capital punishment based on the eligibility provisions in place for the relevant state–year cell, excluding child murder eligibility as a factor in this simulation exercise. In an alternative set of specifications, we investigate the relationship between capital eligibility and more general homicide rates (as opposed to child murder rates). In such specifications, we parameterize eligibility expansions using an even more general simulation approach that considers all possible eligibility factors, including child murder.

(2006) accuracy analysis assumes that a given jurisdiction's eligibility laws include all of those factors available in the SHR files. Thus, if one attempts to parameterize the expansion of eligibility factors, Fagan et al.'s reported analysis does not identify the accuracy of this parameterization. For instance, in the case of the addition of a child murder factor, we do not know how many child homicides would have been deemed eligible for capital punishment prior to the addition of this new factor based on the alternative heinous, atrocious, or cruel factor (if applicable).

The coefficient of interest in the above specification is represented by β_2, which captures the association between homicide rates and capital punishment eligibility. Negative values of β_2 are consistent with a deterrent response. Generally, reductions in rates of criminal behavior arising from stronger forms of punishment may be attributable to either a deterrent effect or an incapacitation effect (i.e., putting criminals in a position where they can commit no further crimes). In the present study, we examine the impact of enhancing the strongest form of punishment for murder. The alternatives to capital punishment, however, are already expected to result in significant prison time. Thus, it is reasonable to interpret the marginal impact of capital eligibility as arising from deterrence of criminal behavior and not from the incapacitation of convicted murderers (Abrams, 2007). Of course, if capital eligibility does provide prosecutors with a greater ability to reach plea agreements in the first place, as distinct from reaching stronger pleas, then the results may be picking up some level of incapacitation.

We tailor the dependent variable, H, to the particular specification. The primary specification focuses on the effect of child murder eligibility expansions. In these binary child murder models, we specify the dependent variable as the log of the child murder rate, where this rate equals the number of murders of youth victims divided by the youth population. Thus, we do not attempt to explain variations in those homicides that are not directly implicated by a child murder eligibility provision. In the general percentage-eligibility models, we specify the dependent variable using a broader range of homicides and using total state–year populations as the denominator. However, as we do in the case of the child murder models, we avoid any noise arising from variations in homicides that are not directly deterrable by capital punishment by focusing the homicide rate calculation on the set of potentially death-eligible homicides. That is, we compile a list of all of the aggravating circumstances that exist across the various death penalty statutes (and that can be identified in the SHR data) and then calculate state–year homicide rates out of the universe of homicides that contain at least one such characteristic (regardless of the eligibility provisions of the given state).[21]

21. In other words, we calculate general homicide rates excluding those homicides that would fail to trigger capital eligibility under the death penalty laws of every state. For instance, homicides committed by means of an explosive device trigger capital eligibility in certain states. Thus, to form the relevant dependent variable, we include all

A small number of state–year cells have no child homicides. Before log-transforming the dependent variable, we set the homicide rates for these zero-valued cells at 0.1 (Malani, 2002). The pattern and magnitude of results presented below remain virtually identical when we estimate alternative specifications that log-transform unadjusted homicide rates (and thereby drop zero-valued cells) and when we specify the dependent variable as actual homicide rates (i.e., non-log-transformed).

Over most of the sample period (from 1988 to 2004), states were subject to Supreme Court doctrine specifically prohibiting them from imposing capital punishment on offenders under the age of 16.[22] Prior to 1988, as discussed in the Supreme Court's 1989 *Stanford* decision, the majority of death penalty states also declined to impose the death penalty on offenders under the age of 16 (fifteen states) or 17 (twelve states). While we do not parameterize any slight variations in eligibility percentages that may exist due to variation in offender age limits, we nonetheless attempt to focus the empirical analysis on the set of homicides that are generally implicated by capital punishment laws. Accordingly, in calculating both the state–year homicide rates and the simulated eligibility measures, we exclude from the underlying sample all homicides committed by offenders under the age of 16.[23]

Finally, we restrict the regression sample to those states that effectively narrow the scope of capital eligibility using only one set of eligibility restrictions. Most death penalty states meet this condition either by limiting the definition of capital murder or by requiring the finding of delineated aggravating circumstances during capital sentencing stages. A small number of states, however, apply a two-tiered narrowing process that effectively imposes different (i.e., nonoverlapping) eligibility restrictions in both the capital murder definition and the sentencing proceedings.[24] Wedrop these states in order to avoid concerns over the unknown nature of the interaction

explosive-related homicides in the homicide-rate calculation for each state, even for those states that do not provide for capital eligibility in these instances.

22. *Thompson v. Oklahoma*, 487 U.S. 815 (1988); *Stanford v. Kentucky*, 492 U.S. 361 (1989). The Supreme Court later raised this minimum age to 18 in 2005. *Roper v. Simmons*, 543 U.S. 551 (2005).

23. We do not draw on any variations in this offender-age margin as we did not research and compile the full history of relevant laws.

24. These states include Connecticut, Alabama, Mississippi, New Hampshire, and Kansas. Certain other states technically contain a death penalty evaluation process at both

between these two tiers (e.g., where states may place greater emphasis on one set of restrictions).Alabama, for instance, added child murder as an eligibility factor in its capital homicide definition in 1992 but excludes a specific child murder provision from the list of aggravating circumstances to be determined during sentencing. In this instance, it is possible that child murders will still meet one of the aggravating circumstances required at sentencing (e.g., Alabama's HAC circumstance), even if we cannot identify that circumstance using SHR records. However, the reach of these alternative factors/circumstances is unknown, confounding our ability to properly specify both the binary child murder model and the general simulated eligibility model. Nonetheless, we explore alternative specifications below that relax these sample restrictions and include capital eligibility provisions from the relevant two-tiered states.

Unobservable state–year factors are represented by the random error term $\varepsilon_{s,t}$. The estimated coefficient of the eligibility variable, *ELIG*, is identified under an assumption of conditional mean independence ($E[\varepsilon \mid ELIG, X, \gamma, \lambda, \varphi] = E[\varepsilon \mid X, \gamma, \lambda, \varphi]$), that is, an assumption that the eligibility variable is uncorrelated with unobservable state–year shocks, taking as given the state effects, year effects, state-specific linear trends, and observable covariates. This condition may not hold if states expand capital eligibility statutes to include child homicides contemporaneously with other policy changes that may impact child homicide rates. We partially address this concern below by testing for evidence indicative of contemporaneous policy changes targeting homicide rates more broadly. More generally, the identification condition may fail to hold if states adopt child murder expansions in response to unobservable factors that contribute to a differential trend in homicide rates between treatment and control states. We partially explore these concerns by modifying the primary specification to include leads of the child murder

the capital-definition stage and the sentencing stage, but effectively impose delineated eligibility limitations at only one stage. In the other stage, such states often require juries to make more subjective determinations. For instance, at the sentencing stage in Texas, juries are asked to determine "whether there is a probability that the defendant would commit criminal acts of violence that would constitute a continuing threat to society." TEX. CODE CRIM. PROC. art. 37.071. For the purposes of this empirical analysis, we focus solely on those stages that impose delineated eligibility criteria. We also include states with two-stage eligibility processes in which the capital-definition stage and the sentencing stage effectively impose parallel criteria that vary together over time (at least with respect to the factors identifiable in the SHR records), for example, Louisiana.

eligibility provision. The coefficients of the lead indicators allow us to test for differential child homicide trends during the period leading up to eligibility expansions. The presence of any such lead effects may implicate the confounding influence of unobservable factors. The estimated lead coefficients also allow us to evaluate whether the primary difference-in-difference result is, in part, a reflection of a spike in child homicide rates in the period surrounding the relevant reforms, where this spike may have contributed to the expansion decision itself.[25]

6. Results

6.1. Child Murders

We begin by presenting difference-in-difference estimates of the effect of child murder eligibility expansions on child murder rates. Each observation in the specifications estimated throughout Section 6 is weighted by the state–year population count used to form the denominator in the relevant dependent variable (e.g., the number of children under the age of 15 for those models using a dependent variable based on the under-15 homicide rate). Moreover, in the models estimated throughout this section, all standard errors are clustered at the state level to allow for arbitrary within-state correlations of the error structure.

In our primary child murder specification, we specify the dependent variable as the log of the rate of homicides of victims under the age of 15. We present results from these primary specifications in Table 3. As presented in column 1, we estimate a statistically significant coefficient of –0.20 for the child murder eligibility indicator, representing an approximately 20% decrease in the under-15 homicide rate following the adoption of a capital punishment law that establishes a specific eligibility category/aggravating circumstance for child murder. With an average annual homicide rate of

25. Of course, even if states do enact reforms in response to recent state-specific events, spikes of this nature may not occur if states act in response to a small number of high-profile incidents (or a single incident). Legislative endogeneity may also contribute to a biased estimate if states add child murder eligibility provisions in anticipation of an uptrend in child homicide rates. This could lead to the estimation of a positive relationship between child murder adoptions and child murder rates when no such causal relationship exists. However, in the face of this conceivable positive bias, we nonetheless estimate a negative relationship consistent with a deterrent effect of capital eligibility.

Table 3. The Relationship between Child Murder Eligibility Provisions and Murder Rates of Child Victims

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient of child murder eligibility law dummy							
2-Year lag	–	–	0.029 (0.084)	0.053 (0.069)	0.104 (0.074)	–	–
Contemporaneous	−0.200**(0.047)	−0.212**(0.053)	−0.243**(0.084)	−0.234**(0.073)	−0.205*(0.082)	−0.183**(0.051)	−0.170**(0.055)
2-Year lead	–	0.019 (0.065)	0.039 (0.092)	0.032 (0.080)	0.042 (0.079)	–	–
3-Year lead	–	–	0.030 (0.109)	0.036 (0.110)	0.035 (0.108)	–	–
4-Year lead	–	–	0.030 (0.084)	0.043 (0.080)	0.039 (0.083)	–	–
5-Year lead	–	–	−0.134 (0.096)	−0.134 (0.094)	−0.147 (0.094)	–	–
6-Year lead	–	–	−0.012 (0.082)	−0.023 (0.079)	−0.013 (0.086)	–	–
Relevant homicide rate (logged)	Under 15 years of age	Under 15 years of age	Under 15 years of age	Under 15 years of age	Under 15 years of age	Under 5 years of age	Under 10 years of age
State—Year controls?	Yes	Yes	Yes	Yes	No	Yes	Yes
State-specific linear trends?	Yes	Yes	Yes	No	No	Yes	Yes
R^2	0.54	0.54	0.55	0.50	0.49	0.45	0.47
N	1,062	1,062	1,010	1,010	1,010	1,062	1,035

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the indicated state–year homicide rate on a dummy variable for the presence of a law extending capital eligibility to child murders. Each regression also includes various state–year controls (except column 5), state-specific linear trends (except columns 4 and 5), and a measure capturing the scope of the remaining eligibility provisions: the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws (coefficients omitted above). Columns 2–5 include certain leads and lags of the child murder law dummy, where an x-year lead variable indicates at time t the status of a state's child murder eligibility law at time $t+x$ (capturing an effect that predates adoption) and where an x-year lag indicates the status of a state's child murder eligibility law at time $t-x$. Column 7 excludes Texas (the one state with an age cutoff under 10 years of age) from the specification. Regressions are weighted by the relevant population count used in the homicide-rate calculation. Homicide data are from the SHR.

1.6 per 100,000 children under the age of 15 (weighted by the under-15 population) and an average under-15 state population of 1.2 million (for the estimated sample), this estimate corresponds to an annual per-state reduction of roughly four child homicides.

In column 2 of Table 3, we modify the primary difference-in-difference specification to include a 2-year lead indicator variable, which switches from 0 to 1 two years prior to the adoption of the relevant child murder provision. We specify the 2-year lead indicator based on whether the initial adoption occurred during the first or second half of the calendar year (e.g., the 2-year lead switches from 0 to 1 in 1988 for an amendment that occurs in December, 1989). However, we continue to drop the state–year cells representing the actual year of adoption from the specification to address mid-year adoption concerns regarding the primary coefficients (as such, we begin with a 2-year lead indicator, as opposed to a 1-year lead indicator, to ensure that we do not drop any cells that would otherwise identify the separate lead coefficient).[26] We now estimate a statistically significant coefficient of –0.21 for the contemporaneous child murder indicator and a smaller, statistically insignificant coefficient of 0.02 for the lead indicator variable. Accordingly, we find no evidence of differential trends between treatment and control states in child murder rates that predate the adoption of child murder eligibility laws. This finding provides greater confidence that the estimated contemporaneous results are reflective of a policy response to the amendment of capital punishment statutes, as opposed to other factors that may contribute to differential trends. These findings also suggest that child murder expansions were not adopted in the aftermath of large spikes in child murder rates, which would otherwise confound the primary difference-in-difference analysis.

We expand on this pre-adoption analysis in column 3 of Table 3 by including an even greater number of lead indicator variables (from 2 to 6 years prior to adoption). This specification allows for a richer view of any differential pre-adoption trend between treatment and control states. We estimate statistically insignificant coefficients for each of the lead indicator variables.

26. However, we estimate a nearly identical result using an alternative approach that also drops the actual year of law change but that includes a lead indicator variable that switches from 0 to 1 in the period represented by the calendar year prior to the year of adoption (e.g., a 1-year lead period represented by the entirety of 1995 for a law change that occurs in November, 1996).

The 2-, 3-, and 4-year lead coefficients are positive in sign and small in magnitude (relative to the contemporaneous effects), again suggesting little difference between treatment and control states in the period leading up to child murder eligibility adoptions. We estimate a larger negative differential in the period between 4 and 5 years prior to adoption, though this estimate is still smaller in magnitude than the contemporaneous effects. Finally, the estimated coefficient of the 6-year lead is nearly 0 in magnitude. The dynamic results presented in column 3 emphasize the extent of the drop in child murder rates that occur upon the enactment of a child murder eligibility provision and provide even greater confidence in an interpretation of the primary deterrence estimates as not simply reflecting trends that began in the pre-adoption period.

It may take time for deterrent responses to materialize following the expansion of capital punishment statutes. Any increase in capital sentences or in executions that arises from an expansion in the number of capital-eligible crimes may be slow to emerge considering the long delays that exist in the adjudication of capital cases and in the carrying out of capital sentences. On the other hand, increases in non-capital sentence lengths arising from death penalty eligibility may emerge with less of a delay depending on how quickly prosecutors begin to take advantage of their enhanced bargaining positions. In column 3 of Table 3, we also test for delayed effects of child murder eligibility adoptions by including a 2-year lag indicator variable that switches from 0 to 1 two years following the adoption of the relevant eligibility provision. While we estimate an approximately 24% reduction in the under-15 homicide rate in the 2-year period following a child murder adoption, we estimate a small, statistically insignificant positive differential between this immediate period and the subsequent years, suggesting both a persistence in the estimated relationship and a relatively immediate impact. The immediacy of this relationship suggests that it is more likely to occur through a prosecutorial-leverage channel than a subsequent-death-sentence/execution channel.

Column 4 of Table 3 estimates the same dynamic specification of column 3, but no longer includes the set of state-specific linear time trends. Column 5 subsequently drops the additional state–year covariates (e.g., unemployment rate). We estimate a nearly identical pattern of results in each case, suggesting that the estimated coefficients presented in column 1–3 are robust to any assumption regarding the linearity of omitted state–year factors and are otherwise not the spurious result of large changes in observed factors that happen to coincide with the adoption of child murder provisions (Gruber and Hungerman, 2008).

States vary in the age cutoffs that they use in their child murder eligibility statutes, as presented in Table 1. While most statutes set cutoffs ranging from 12 to 16 years, thestate with the lowest limit, Texas, provides capital eligibility for murders of victims below the age of 6. We estimate nearly identical results when we specify the dependent variable as the log of the rate of homicides of victims under the age of 5 and thus confine the analysis to a set of homicides that falls entirely within thescope of each of the child murder eligibility laws (column 6 of Table 3).[27] All of the treatment states other than Texas have a cutoff that is above 10 years. Thus, in the same spirit, we also estimate a specification that excludes Texas and bases the dependent variable on an under-10 homicide rate. As presented in column 7 of Table 3, we continue to estimate substantially similar results.

In Table 4, we consider an alternative parameterization of the child murder eligibility variable that accounts for the variations across states in the operable victim age cutoffs. In this approach, we take a national sample of all homicides of children under the age of 17 and simulate, for each state, the percentage of those child homicides that would be eligible for capital punishment under the relevant state's child murder eligibility provisions. Wyoming, for instance, amended its statute in 1989 to provide capital eligibility for murders of victims under the age of 17 (the highest age cutoff among the states) and thus generates a simulated eligibility value of 1 in the post-1989 period. As presented in column 1 of Table 4, we estimate that as a state makes 100% of homicides of children under the age of 17 eligible for capital punishment, the child homicide rate falls by approximately 31% (significant at the 1% level). Moreover, as suggested by column 2, this negative effect does not appear to be reflective of a trend that began in the preadoption period.

If potential offenders do respond to perceived changes in the punishment of child murderers (whether by observing plea bargains reached or sentences imposed at trial), it may be too demanding to assume that they could make

27. We use an under-5 homicide rate, as opposed to an under-6 homicide rate, given that our data on population estimates by age (which form the relevant homicide-rate denominators) are collected only in 5-year age increments.

Table 4. The Relationship between Murder Rates of Victims under 15 Years of Age and the Simulated Percentage of Child Murders Eligible for the Death Penalty

	(1)	(2)
Coefficient of simulated child murder eligibility percentage		
Contemporaneous	−0.309**(0.091)	−0.338**(0.098)
2-Year lead	–	0.048 (0.114)
Coefficient of general simulated eligibility percentage (excluding child murders)	−0.035 (0.285)	−0.034 (0.285)
R^2	0.54	0.54
N	1062	1062

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year homicide rate of victims under 15 years of age on a variable capturing the percentage of national homicides of victims under the age of 17 that are eligible for capital punishment based on the eligibility laws for the relevant state–year cell. The estimated specifications also include a measure of the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws for the remaining eligibility factors. Each specification includes various state–year controls along with a set of state-specific linear time trends. Column 2 includes a 2-year lead dummy variable for the adoption of child murder eligibility provisions. Regressions are weighted by the under-15 population count used in the homicide-rate calculation. Homicide data are from the SHR.

such observations with an awareness of any differential treatment in the ages of the relevant victims (and thus deem it less risky to harm a child that is, for example, 15 years old instead of 14 years old). Thus, it is arguably valid to estimate specifications that include a general, uniform child homicide rate as the dependent variable (such as those using under-15 homicide rates in Tables 3 and 4), even in the face of eligibility laws that vary across age groups.[28] While potential offenders may not acknowledge the existence of differential punishments across specific age groups, they may be responsive to the overall level of punishment observed against youth offenders. Moreover, the operable age cutoffs may indeed place bounds on prosecutorial behavior and may nonetheless have a real effect on such overall punishment levels. Thus, it may nonetheless be informative to estimate, as we do in Table 4, the relationship between child homicide rates and a

28. For this reason, plausibility concerns may be too severe with an alternative approach that estimates a triple-differences specification that draws on differential age cutoffs and variations in the homicide responses among different childhood age groups. Also confounding any such approach is the fact that the age cutoffs are all predominantly clustered above 12 years of age, with only one state having a below-10 cutoff (Texas).

propensity measure that proxies for a state's general aggressiveness (at least on a statutory basis) in prosecuting child homicides.

The final row in Table 4 presents estimated coefficients for the covariate measure represented by the simulated percentage of potentially death-eligible homicides that are eligible for capital punishment based on the relevant state–year eligibility rules for all remaining eligibility factors (excluding the child murder factor from the calculation). This variable controls for the scope of the remaining eligibility provisions prevailing in the relevant state and year, while at the same time allows for a falsification test in which we estimate the effect of expansions in other eligibility factors (e.g., narcotics-related) on the rate of child homicides. There may of course be some relationship between such expansions and child murder given that youths are the victims of homicides that may attain capital eligibility under some other factor. However, it is reasonable to expect that any such relationship is weaker than that resulting from an expansion of eligibility statutes that specifically reaches all child homicides. Consistent with these expectations, we find no statistically significant relationship between child murders and general eligibility expansions. While the estimated coefficients are negative, they are significantly smaller in magnitude than the primary child murder eligibility coefficients.

In the above specifications, we focus solely on eligibility expansions, as distinct from initial adoptions of capital punishment statutes. Accordingly, for those several states that enact general statutes over the sample period (e.g., New York), we exclude those years in which no death penalty statute was in effect.[29] In Table 5, we present results from alternative specifications that include all sample years, along with an indicator variable for the presence of a general death penalty statute.[30] We estimate nearly identical coefficients for the relevant child murder eligibility dummies and continue to estimate a small, statistically insignificant coefficient for the general simulated eligibility percentage (for the remaining eligibility factors). Furthermore, we estimate a coefficient of −0.33 (P-value of 0.06) for

29. In each specification, however, we do include as controls those states that never reinstated the death penalty following the national moratorium.

30. In no instance does the date of an initial statutory enactment coincide with the date of a child murder eligibility adoption. That is, for those states that adopt general statutes over the sample period, child murder eligibility factors were not included among the relevant initial statutes.

Table 5. The Relationship between Murder Rates of Victims Under 15 Years of Age and the Enactment of General Death Penalty Statutes and Child Murder Eligibility Provisions

	(1)	(2)
Coefficient of general death penalty enactment dummy		
Contemporaneous	−0.328 (0.169)	−0.457*(0.184)
2-Year lead	–	0.190**(0.058)
Coefficient of child murder eligibility law dummy		
Contemporaneous	−0.195**(0.047)	−0.204**(0.053)
2-Year lead	–	0.020 (0.064)
Coefficient of simulated eligibility percentage (excluding child murders)	0.003 (0.247)	−0.002 (0.243)
R^2	0.55	0.55
N	1088	1088

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year homicide rate of victims under 15 years of age on dummy variables for the presence of a general death penalty law and a law specifically extending capital eligibility to child murders, along with a measure of the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws for the remaining eligibility factors. Each specification includes various state–year controls along with a set of state-specific linear time trends. Column 2 includes 2-year lead dummy variables for general death penalty laws and child murder eligibility provisions. Regressions are weighted by the under-15 population count used in the homicide-rate calculation. Homicide data are from the SHR.

the general death penalty indicator, suggesting that the general presence of a death penalty statute, aside from the scope of its eligibility, may be associated with a reduction in child homicide rates. In column 2, we include 2-year lead indicators for both the child murder provision and the general death penalty statute. We estimate a spike in child homicide rates in the 2-year period prior to initial statutory enactments (implicating possible legislative endogeneity concerns), followed by an even larger decline in the period thereafter.

Table 6 presents the results from a falsification test in which we estimate the association between the adoption of child murder eligibility laws and the rate of homicides of victims over 20 years old. Given that these particular expansions are expected to result in enhanced punishments of child murderers only, one may not expect to observe a reduction in homicide rates of older victims. Consistent with these expectations, we estimate a smaller, statistically insignificant relationship between child murder eligibility adoptions and adult homicide rates. While the estimated coefficient of the child murder dummy, −0.04, is still negative in sign, it is considerably smaller in magnitude relative to the estimated coefficient from the primary child

Table 6. The Relationship Between Child Murder Eligibility Provisions and Homicide Rates of Victims over 20 Years of Age (Falsification Test)

	(1)	(2)
Coefficient of child murder eligibility law dummy		
Contemporaneous	−0.043 (0.042)	0.007 (0.044)
2-Year Lead	–	−0.080*(0.031)
P-value of significance test of difference in estimated coefficients of child murder dummies between under-15 specification and over-20 specification:		
Contemporaneous	0.001**	0.001**
2-Year Lead	–	0.131
R^2	0.93	0.93
N	1062	1062

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year homicide rate of victims over 20 years of age on a dummy variable for the presence of a law specifically extending capital eligibility to child murders, along with a measure of the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws for the remaining eligibility factors. Each specification includes various state–year controls along with a set of state-specific linear time trends. Column 2 includes a 2-year lead dummy variable for the child murder eligibility law. Also presented are P-values of significance tests comparing the respective coefficients from the over-20 specifications to those from the analogous under-15 specifications (columns 1 and 2 of Table 3), where the relevant F-tests are performed following the estimation of seemingly unrelated regressions. Regressions are weighted by the over-20 population count used in the homicide-rate calculation. Homicide data are from the SHR.

murder specification. As presented in Table 6, we can reject at 1% confidence the hypothesis that the child murder coefficients from the respective under-15 and over-20 specifications are equal. Moreover, the results from column 2 suggest that any negative differential in adult homicide rates between treatment and control states may have begun in the period prior to the relevant eligibility amendment. Thus, unlike the primary specifications estimated above, it does not appear that the adoption of a child murder eligibility law is associated with a corresponding reduction in adult homicide rates. While it is possible that potential murderers may be deterred by stronger punishments of homicides generally (e.g., they may learn of the imposition of stronger punishments for murders but be unaware of the particulars of the crimes), we find that the response of child murder eligibility laws is targeted at child murder rates.[31]

31. In a similar analysis (not shown), we add the general death penalty enactment variable to this falsification exercise. While the child murder eligibility provision may be expected to have a different relationship with under-15 homicides and over-20 homicide rates, the general enactment variable may be expected to have similar relationships with each such homicide rate. Consistent with these expectations and similar to the findings

To the extent that child murder eligibility adoptions may be correlated with a general "get-tough-on-crime" movement within a state, the estimated reduction in child homicide rates may be due to these latter unobserved efforts and not due to the eligibility expansions themselves. However, if such a correlation were to exist, one might expect to observe a relationship between child murder eligibility adoptions and rates of other types of homicides. Moreover, if eligibility expansions in general move contemporaneously with other crime-fighting developments, one might also expect to find a relationship between child murder rates and the adoption of other eligibility expansions (i.e., those not specific to child murder). Thus, the falsification exercises considered above, which find no evidence to support any of these additional relationships, may appease concerns of this nature. Of course, they do not eliminate such concerns and one should still be cautious in attributing causation to these estimates. The child murder findings, after all, may still be reflective of contemporaneous movements within states of a more specific "get-tough-on-*youth*-crime" nature.

6.2. Additional Child Murder Specification Checks

The child murder results presented above are robust to a number of additional sensitivity tests. For instance, we find that the sign, magnitude, and statistical significance of the estimated coefficient of the child murder indicator presented in column 1 of Table 3 are robust to the systematic, one-by-one exclusion of each treatment state, including Texas, from the estimation model (not shown). Each separate panel in Table 7 further subjects the primary specification estimated in column 1 of Table 3 to certain modifications. First, we estimate a substantially similar coefficient when dropping the state-specific linear time trends from the primary specification (panel A)[32] and when estimating a specification that includes both linear and quadratic state-specific trends (panel B). Likewise, we estimate a nearly identical coefficient when excluding the control variable meant to capture the scope of the remaining eligibility provisions (panel C). Considering that some child murders may attain capital eligibility under an aggravating factor

of the child murder specifications estimated in Table 5, we estimate a large negative relationship (with a statistically significant coefficient of −0.50) between the general enactment variable and the homicide rate of victims older than 20 years.

32. The dynamic specifications estimated in columns 4 and 5 of Table 3 also drop the state-specific linear trends.

Table 7. Various Sensitivity Tests. The Relationship between Child Murder Eligibility Provisions and Homicide Rates of Victims Under 15 Years of Age

	Coefficient of child murder eligibility law dummy
Panel A: Excluding state-specific linear time trends	−0.174**(0.039)
Panel B: Including state-specific linear and quadratic time trends	−0.168*(0.076)
Panel C: Excluding control variable for the scope of the remaining eligibility provisions	−0.199**(0.487)
Panel D: Excluding treatment states with existing eligibility provisions covering actions of a heinous, atrocious or cruel nature (or similar provision)	−0.202**(0.054)
Panel E: Using weights provided by the SHR to ensure that the SHR homicide counts match those of the FBI's Uniform Crime Reports	−0.201**(0.047)
Panel F: Including state–year robbery rates and over-20 homicide rates to control for prevailing violent-crime levels	−0.168**(0.046)
Panel G: Excluding treatment states that have not executed anyone in the post-moratorium period	−0.191**(0.049)
Panel H: Including states that impose two-tiered eligibility criteria (basing eligibility only on restrictions imposed by relevant definition of capital murder)	−0.148*(0.056)
Panel I: Including state–year legal abortion rates per 1,000 women aged 15–44 (by state of residence)	−0.198**(0.048)
Panel J: Excluding controls for police and judicial–legal expenses	−0.202**(0.047)

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year homicide rate of victims under 15 years of age on a dummy variable for the presence of a law extending capital eligibility to child murders. Each regression also includes various state–year controls, state-specific linear trends (except panel A), and a measure capturing the scope of the remaining eligibility provisions: the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws (except for panel C). Each panel modifies the basic child murder specification as indicated. Regressions are weighted by the relevant population count used in the homicide-rate calculation. Homicide data are from the SHR.

that targets homicides of an HAC-like nature, we also estimate a specification that excludes those four treatment states that provide for these alternative eligibility provisions (or similarly worded provisions).[33] As presented in

33. As discussed in Section 3 above, certain states (e.g., Pennsylvania) specify eligibility factors that are similar in spirit but that avoid the use of vague terminology. Instead, such statutes specifically limit capital eligibility to instances of torture. We continue to include these states in the estimation for the purposes of this specification check. There

panel D, we find that the adoption of child murder eligibility laws by this more limited set of states continues to be associated with an approximately 20% reduction in the rate of homicides with victims under the age of 15.

We also estimate substantially similar or virtually identical results when we calculate child homicide rates using the weights provided by the SHR to ensure that the SHR homicide counts match those of the FBI's Uniform Crime Reports (panel E), include state–year robbery rates and state–year general homicide rates (e.g., adult homicide rates) as measures to control for prevailing violent-crime levels (panel F), and estimate the child murder specification using only those states that have executed at least one person in the post-moratorium period (panel G). Moreover, in panel H, we demonstrate the robustness of the findings to the inclusion of those several states that impose two-tiered eligibility criteria, where we calculate the relevant eligibility variables based solely on the restrictions imposed by the respective definition of capital murder (as opposed to the separate delineation of aggravating circumstances). To address the potential relationship between infant homicides and prevailing abortion rates (Kalist and Molinari, 2006), we likewise demonstrate the robustness of the primary results to the inclusion of such rates (panel I).[34]

The primary specifications include controls for expenditures on police operations and judicial and prosecutorial operations. These data are derived from the Criminal Justice Expenditure and Employment (CJEE) Extracts for the post-1981 period and from the CJEE Surveys for the pre-1981 period.[35] To address potential comparability issues between these two data sources, we estimate a modified specification that simply excludes these measures

are two additional states, Arkansas and Wyoming, that include the vague terms in their statutes (e.g., specially cruel or depraved), but that subsequently define those terms (in the statute) to pertain to instances of torture or serious physical abuse. The estimation results remain largely unchanged when we also exclude these two additional states.

34. Abortion data are obtained from the Guttmacher Institute and represent state–year legal abortion rates per 1000 women aged 15–44 (by state of residence). The presented results use contemporaneous abortion rates. However, the estimated coefficient of the child murder dummy is nearly identical when we include up to 4-year lags in the abortion rates.

35. The CJEE Extract data are only available in the period after 1981. The CJEE Survey data were collected between 1971 and 1979 and in 1985. To estimate the missing 1980 and 1981 measures, we linearly interpolate data from the 1979 and 1985 CJEE Surveys. Imperfect comparability between these data sources may contribute noise to the estimates.

altogether (panel J), generating nearly identical results. The estimates also remain unchanged when we focus only on the post-1981 period (not shown).[36]

6.3. Potentially Death-Eligible Murders

We next evaluate whether the child murder results extend to the relationship between general expansions in death penalty eligibility factors and potentially death-eligible homicide rates. Described in greater detail in Section 5 above, potentially death-eligible homicides are those that exhibit characteristics that would garner capital eligibility in at least one state. The resulting homicide rate is thus constructed so as to avoid any noise associated with variations in the rate of homicides that are not implicated by the capital eligibility statute of any state and that are consequently of an arguably nondeterrable nature.

Panel A of Table 8 presents results of specifications that parameterize variations in capital eligibility using an incident-level database of homicides to simulate the percentage of national potentially death-eligible homicides that would be eligible for capital punishment based on the death penalty statutes in operation for each state–year cell. In columns 1 and 2of panel A, we perform these simulations using all possible eligibility factors, including the murder of youth victims. We estimate that an increase from 0% to

36. Difference-in-difference specifications may still reflect a tendency to over-reject the null hypothesis of no-effect when the number of analytical groups is fixed, even with standard-error adjustments for within-group autocorrelation (Bertrand et al., 2004; Conley and Taber, 2005). While the number of groups (i.e., states) considered in the above analysis is large, we nonetheless perform hypothesis tests on the child murder eligibility coefficient using a randomization inference approach (Duflo et al., 2005), which allows for valid inference using any number of groups. Using only the set of states that did not amend their statutes to add child murder eligibility over the sample period, we randomly generate (and assign) 5000 sets of placebo laws and then estimate the specification used in column 1 of Table 3 on each of these simulated sets of laws. We simulate the placebo set so that the expected distribution of placebo law changes over time matches the distribution of the child murder law changes that actually took place (Gruber and Hungerman, 2008). We find that the child murder coefficient from the primary difference-in-difference specification estimated above (using actual variation in eligibility laws) is in the second percentile of the empirical distribution of the 5,000 estimated coefficient means from the above simulations. This placement corresponds to a P-value of roughly 0.04 and is thus consistent with a 5% significance level. This exercise provides additional confidence in the conclusion that the estimated deterrent effect of child murder eligibility expansions would likely not be observed if the true effect were zero.

Table 8. The Relationship between Potentially Death-Eligible Homicide Rates and Simulated Eligibility Percentages

	(1)	(2)	(3)	(4)
Panel A: Excluding expansions associated with general death penalty reinstatements				
Coefficient of simulated eligibility percentage:				
Contemporaneous	−0.261 (0.222)	−0.316 (0.229)	−0.007 (0.248)	−0.132 (0.275)
2-Year lead	–	0.139 (0.333)	–	0.350 (0.412)
N	1,123	1,123	1,123	1,123
Panel B: Including expansions associated with general death penalty reinstatements				
Coefficient of simulated eligibility percentage:				
Contemporaneous	−0.536**(0.130)	−0.595**(0.149)	−0.416**(0.113)	−0.518**(0.122)
2-Year lead	–	0.150 (0.210)	–	0.227 (0.174)
N	1,153	1,153	1,153	1,153
Eligibility factors considered in eligibility simulations and in potentially death-eligible homicide rate calculation?	All eligibility factors		All eligibility factors other than child murder	

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year potentially death-eligible homicide rate on a measure representing the percentage of national potentially death-eligible homicides that are in fact eligible for capital punishment based on the relevant state–year eligibility laws. Potentially death-eligible homicide rates represent the rate of homicides with characteristics that trigger eligibility in at least one state. Each specification includes various state–year controls along with a set of state-specific linear time trends. Columns 3 and 4 exclude the child murder factor from both the calculation of potentially death-eligible homicide rates and from the calculation of the simulated eligibility percentage. Columns 2 and 4 include a 2-year lead eligibility percentage variable that equals at time t the state's simulated eligibility percentage at time $t+2$ (capturing a change in eligibility laws prior to their occurrence). Regressions are weighted by the total population count used in the homicide-rate calculation. Homicide data are from the SHR.

100% in the percent of potentially-death-eligible homicides that are in fact eligible for capital punishment in a given state is associated with a roughly 26% decline in the potentially death-eligible homicide rate (or an average annual reduction of roughly 33 such homicides). While this estimate is rather imprecise and statistically insignificant, it corresponds rather closely with the magnitude of the deterrence estimates derived in the above child murder specifications, where the most direct comparison is arguably with the 31% decline in child murder rates identified in Table 4, which parameterizes variations in child murder eligibility laws through an analogous simulation exercise. In column 2 of panel A, we test for pre-adoption trends and estimate a positive, statistically insignificant coefficient for the 2-year lead indicator variable, suggesting that the negative homicide-rate differential between treatment and control states did not begin in the period prior to the expansion of eligibility statutes (while a positive differential may have preceded the statutory expansions).

Having already considered the relationship between child murder eligibility provisions and child murder rates, we remove the child murder eligibility factor from the simulation analysis in columns 3 and 4 of panel A. Focusing solely on the relationship between homicide rates and the scope of the remaining factors, we now estimate a coefficient for the simulated eligibility variable that is nearly 0 in magnitude. These findings thus do not appear to offer evidence in support of an extension of the above child murder results to general eligibility factors. However, by removing the child murder eligibility factor, we have removed a substantial portion of the variation in eligibility laws, leaving results that are quite noisy and limiting the ability to make inferences regarding a more general effect. Moreover, this exercise explores the general impact of eligibility expansions, essentially treating all factors (other than the child murder factor) alike. However, it remains possible that certain specific expansions have independent deterrent effects. While we may have few treatment states with which to test for any independent effects of the remaining factors, we do consider certain additional difference-in-difference specifications in Section 6.4 below, taking care to address any family-wise-error concerns.

Over the length of the sample period, the vast bulk of the variation in capital punishment statutes is represented by expansions of preexisting statutes. However, several states, including New York, did wait well into the post-moratorium period and thus well into the sample period before

Table 9. The Relationship between Potentially Death-Eligible Homicide Rates and the Enactment of General Death Penalty Statutes

	(1)	(2)
Coefficient of general death penalty enactment dummy		
Contemporaneous	−0.349** (0.056)	−0.492** (0.079)
2-Year Lead	–	0.203* (0.097)
R^2	0.83	0.83
N	1260	1260

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions of the log of the state–year potentially death-eligible homicide rate on a dummy variable for the presence of a general death penalty statute. Potentially death-eligible homicide rates represent the rate of homicides with characteristics that trigger eligibility in at least one state. Each specification includes various state–year controls along with a set of state-specific linear time trends. Column 2 includes a 2-year lead indicator variable for a state's general death penalty laws. Regressions are weighted by the total population count used in the homicide-rate calculation. Homicide data are from the SHR.

reinstating their general death penalty statutes. While statutory reinstatements of the death penalty do represent an increase in the number of homicides that are eligible for capital punishment, there is a reasonable possibility that the presence of a capital punishment statute has a deterrent impact independent of the reach of such a statute. For this reason, we separate these two elements to the variation in capital punishment statutes. To allow for a clean analysis of eligibility expansions, we focus in panel A of Table 8 solely on expansions of existing statutes and thus drop those state–year cells in which the relevant death penalty state had yet to enact a post-moratorium death penalty statute.

We do not attempt to estimate the separate effect of statutory reinstatements and subsequent statutory expansions in the same specification given that reinstatements necessarily occur at the same time that the affected states expand the number of homicides that are eligible for the death penalty. Nonetheless, in Table 9, we estimate specifications that focus solely on the coefficient of the general death penalty statute variable. Given that such specifications no longer include the simulated eligibility percentage measure, we now include those several states that were dropped from the above analysis due to the two-tiered nature of their eligibility process (though the results are similar excluding such states).[37] In column 1, we estimate a large,

37. Nonetheless, we continue to exclude Kansas from the specification. While Kansas did reinstate the death penalty in 1994, Kansas homicide records are missing from the

statistically significant relationship between homicide rates and the reinstatement of a death penalty statute, with a coefficient of −0.35 (implying an annual reduction of approximately forty-five lives). The extent of this estimate suggests that the act of reinstating a death penalty statute may have a large saliency effect that is independent of the scope of that statute. However, the positive coefficient estimated for the lead indicator variable in column 2 is large enough to suggest that the estimated magnitude of this reinstatement effect may be confounded by legislative endogeneity concerns.

Of course, the substantial reduction in homicide rates estimated for those several reinstatement states may actually arise from a pure expansion effect (and not from any such independent reinstatement effect) that happens to be of significant magnitude for this small set of states. With this possibility in mind, in panel B of Table 8, we estimate the same specifications considered in panel A, but include the pre-reinstatement years for the relevant set of reinstatement states (e.g., New York). In such states, the simulated eligibility percentage jumps from a value of 0 to the proper percentage upon reinstatement. In each such specification, we estimate a large negative association between homicide rates and expansions in capital punishment eligibility.

The mixed findings of the general eligibility-expansion specifications stand in contrast with the robust deterrence findings of the child murder specifications. This discrepancy may be attributable to several factors. First, by using total homicide rates and a general parameterization of eligibility factors, the general simulation specification inherently puts different types of homicides and different eligibility criteria on equal footing. However, the effect of adding eligibility for child murder, for instance, may differ from the effect of adding eligibility for narcotics-related homicides. Child murderers may simply perceive and respond to punitive risks in an entirely different manner. Moreover, prosecutors may elect to embrace the bargaining potential of capital eligibility in the case of child murderers to a different extent than they would in the case of other eligible murderers.

Second, the divergencebetween the findings from the child murder eligibility model and the general simulated eligibility model may be due to certain other methodological limitations of the latter model. The inability of

SHR files between 1993 and 1999. However, when we include Kansas and rely only on its post-reinstatement homicide records from 2000–2004, we estimate substantially similar coefficients for the general death penalty variable.

the SHR records to identify all instances in which homicides meet eligibility standards may lead to measurement error in the simulated eligibility measures.Moreover, each of the statutory amendments that drive the variation in the simulated eligibility probabilities is itself only targeted at a specific type of crime. For instance, an amendment adding narcotics-related homicides to the list of eligibility factors may only lead to deterrence in homicidal behavior of this nature, depending on the channels by which these rules affect the criminal mind. However, our model necessarily estimates the impact of such an expansion on a more broadly defined homicide rate. While the tractability of the general simulation specification requires the use of a broad homicide definition, the fact that identification in this model results from staggered eligibility amendments of a distinct, limited nature may make it difficult to isolate the true deterrent forces in play. The child murder model, on the other hand, may simply provide for a more powerful statistical approach by targeting both the legislative variation and the dependent variable on the same subset of homicidal behavior.

6.4. Multiple-Outcome Tests

The above analysis focuses largely on an examination of one specific type of eligibility expansion, i.e., child murders. The analysis, however, does draw (albeit in a more generalized manner) on expansions of additional eligibility types, possibly implicating family-wise error concerns in that we emphasize standard hypothesis tests on the child murder regression coefficients without correcting the relevant standard errors for the possibility that the child murder outcome is indeed part of a family of hypotheses, that is, a family of separate examinations of the impact of each eligibility type.[38] To address possible family-wise error concerns, researchers often adjust the critical values for each separate hypothesis test in such a fashion that there is aless than 0.05 probability that at least one of the individual tests in the family would exceed the adjusted critical value (Duflo et al., 2005).

Standard error corrections accounting for family-wise error, however, may not be appropriate in the present context, in that we are not estimating the impact of the same natural experiment on a multitude of outcomes.

38. The essence of this concern is that, if we test a policy reform on numerous outcomes and consider separate hypothesis tests on each outcome, there may reasonably be a greater than 0.05 probability that we reject at least one of those individual tests (based on an individual 95% confidence interval).

Rather, with each different eligibility type comes an entirely different set of legislative variation. We emphasize the child murder specification in that it presents the richest set of variations, with at least 16 treatment groups (depending on the specification) and with adoption years staggered over a long horizon. Most of the remaining expansion types occur across 1–4 treatment groups only. As indicated above, difference-in-difference specifications with few treatment groups may implicate concerns regarding the consistency of the estimated coefficients and the appropriateness of the estimated standard errors (Conley and Taber, 2005). Focusing on the child murder specification for these reasons, we elect to perform our primary inference on the child murder results under standard one-outcome methods. Moreover, this individual-outcome approach may be appropriate to those who are solely interested in evaluating the impact of child murder eligibility (e.g., policymakers who, for other reasons, desire only to extend capital eligibility to this class).

In any event, we also demonstrate the robustness of the child murder results to a standard, conservative adjustment in the estimated P-values for possible family-wise error. In Table 10, we expand the individual child murder investigation to consider the separate impact of several of the next most relevant expansions and present the results of four separate difference-in-difference regressions. Each regression evaluates the impact of a particular type of eligibility provision (child murder, multiple-victim murder, narcotics-related murder, and elderly murder) on the appropriate homicide rate.[39] The final row in each panel of Table 10 presents a Bonferroni-adjusted P-value for the coefficient of the relevant eligibility dummy. With a Bonferroni adjustment (Savin, 1984), the P-value is simply multiplied by the number of tests in the family (in this instance four). With a stand-alone P-value of less than 1/1,000, the child murder estimates continue to be significant at 1% after correcting for any family-wise error in this four-outcome

39. For the multiple-victim specifications, we use a general homicide rate (potentially death-eligible homicide rate) as the dependent variable, capturing the possibility that such provisions also impact the incentives to commit homicides in the first instance (acknowledging that individuals may have a stronger incentive to commit additional murders to cover up an initial murder). However, we estimate similar relationships using alternative variables that capture the rate of multiple-victim homicides themselves. In the case of the elderly murder specifications, we base the dependent variable on the rate of homicide of victims older than 70.

Table 10. The Relationship between Various Homicide Rates and the Enactment of Specific Death Penalty Expansions

Eligibility factor	Homicide rate	Coefficient of relevant eligibility law dummy	
Panel A: Child murder (replicating Table 7, panel C)	Under-15 (logged)	Child murder eligibility dummy	−0.199** (0.049)
		Bonferroni-adjusted P-value	0.00**
Panel B: Multiple-victim murder	Potentially death-eligible (logged)	Multiple-victim eligibility dummy	−0.152* (0.068)
		Bonferroni-adjusted P-value	0.12
Panel C: Narcotics-related murder	Narcotics-related (logged)	Narcotics-related eligibility dummy	0.286 (0.567)
		Bonferroni-adjusted P-value	1.00
Panel D: Elderly murder	Over-70 (logged)	Elderly murder eligibility dummy	0.158 (0.106)
		Bonferroni-adjusted P-value	0.56

* significant at 5%; ** significant at 1%. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Bonferroni-adjusted P-values are indicated in each panel, representing the product of the P-value for the relevant eligibility law estimate and the number of outcomes considered. Reported coefficients are from separate difference-in-difference regressions of the log of the indicated homicide rate on a dummy variable for the capital eligibility factor indicated in the first column. Each specification includes various state–year controls along with a set of state-specific linear time trends. The specifications do not include controls for the scope of the remaining eligibility provisions. Regressions are weighted by the total population count used in the homicide-rate calculation. Homicide data are from the SHR.

investigation. With this initial P-value, the same would likely hold even if it were possible to estimate reliable results for each of the remaining expansion types.

The results presented in Table 10 also demonstrate a negative association between homicide rates and multiple-victim eligibility adoptions. This specific result, however, is not robust to the Bonferroni adjustment. We also estimate a positive, insignificant relationship between narcotics-related homicides and narcotics-related eligibility laws and between homicides of elderly victims and associated elderly homicide rates.[40]

40. In unreported regressions, we include lead indicator variables in these specifications. The results from that modification suggest that the positive coefficient estimated for the narcotics specification appears to be reflective of a positive trend that emerged prior to the adoption of the narcotics-related eligibility factor.

This multiple-outcomes framework allows us to perform more conservative inference on the specific child murder estimates. However, since the Bonferroni-adjusted critical values are set so that there is a less than 0.05 probability of estimating a significant coefficient in at least one of the outcomes, this framework, at the same time, allows us to evaluate a more general hypothesis, that is, a joint null hypothesis of zero effects across all outcomes. Thus, the robustness of the child murder coefficients to the Bonferroni adjustment suggests that we can reject this joint null hypothesis and infer that eligibility expansions may be associated with reduced homicide rates on at least some level.[41]

There are of, course, alternative general hypotheses that may be of interest. For instance, one may want to test the null hypothesis of a zero average treatment effect across the different outcome specifications (as distinct from testing a null hypothesis of zero effects in every instance). Researchers interested in testing for more general effects of this nature sometimes consider the following approach: (i) estimate the treatment effects from each separate outcomes specification, (ii) calculate a weighted average of these separate point estimates, and (iii) conduct inference on this average treatment effect measure (Duflo et al., 2005). The general simulated eligibility exercise considered above is very much in this spirit, although it operates by combining different types of legislative variation into one specification rather than by combining separately estimated treatment effects. This simulation exercise nonetheless achieves the same function of confronting family-wise error concerns while allowing for a more general evaluation of the relationship between homicide rates and the propensity of a state to extend capital eligibility to a given homicide.

7. Conclusion

Drawing on a rich set of legislative variation, we have found evidence to suggest that extending eligibility for capital punishment to the murder of youth victims is associated with an approximately 20% reduction in the homicide rate of youth victims, corresponding to close to four fewer child

41. We also consider a similar hypothesis test in which we estimate these 4 specifications in a seemingly unrelated regression and then consider an F-test of zero effects across all 4 eligibility coefficients. We reject this hypothesis at 1%.

homicides annually in a state of average size. We have considered certain falsification exercises that ease (without eliminating) concerns that these results are merely reflective of contemporaneous crime-fighting movements of another nature. The analysis is limited, however, by an inability to ease concerns regarding spurious correlations resulting from contemporaneous "get-tough-on-crime" movements that target child victims.

Eligibility expansions may induce a deterrence response either by paving the way for ultimate executions or by providing prosecutors with enhanced bargaining leverage. While the former channel may only materialize on rare occasions, capital eligibility itself is triggered quite frequently, in which event the threat of its application may impact a relatively large number of plea-bargaining outcomes. The plausibility of these sizeable findings may rest on the strength by which prosecutors embrace this bargaining tool and on the degree to which potential offenders are, in fact, responsive to observed variations in punishment levels.

While confining the estimation to the case of child murder leads to certain methodological advantages, one should be cautious in viewing the child murder model as representative of a general deterrent effect. When we turn to the estimation of a model that draws on a broader range of eligibility expansions, we continue to estimate regression coefficients of the same sign and of comparable magnitude. These findings, however, are sensitive to the exclusion of the child murder factor from the general eligibility analysis. Thus, with relatively noisy and mixed findings, the results provide little indication that the more targeted findings of the child murder specification will extend to expansions in eligibility criteria of other kinds.

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