Justice at What Cost? Penalties, Rape Deterrence, and the Incentive to Murder*

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Abstract

We present a theoretical model to examine how increasing the penalty for one crime may lead the perpetrator to commit a collateral crime, thereby reducing the probability of conviction for the first crime. We then consider the treatments of spousal rape laws and the mandatory minimum 25-year sentence for sex crimes against a child. Both treatments were introduced in the United States in a staggered fashion. Abolishing spousal rape exemptions increased wife homicides by approximately 5 victims per state-year (27 percent), while mandatory 25-year minimum sentences for child sexual abuse increased child homicides by approximately 11 victims (80 percent).

JEL codes: K14, K42, J12, J16.

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1 Introduction

Can increasing the penalties for criminal sexual assault cost lives? Rape remains one of the most traumatic violent crimes, with profound consequences for victims (Finkelhor and Yllo , 1985). Studies in criminology, psychology, and economics consistently find that sexual assault has long-lasting social, emotional, and economic costs, such as trauma, loss of employment, and reliance on public support and costly health interventions (Koss and Harvey , 1991, Campbell et al. , 2009, Peterson et al. , 2017). Recognizing its severity, policymakers have, over the past several decades, significantly strengthened criminal sanctions against sexual violence. Their efforts have included the abolition of the marital rape exemption, which historically prevented men from being prosecuted for rape within marriage, as well as the introduction of mandatory minimum sentencing laws for sexual crimes involving minors (Berger, Neuman and Searles , 1991). These reforms reflect a broad societal commitment to protecting victims and deterring crime.

Despite the importance of these efforts, little is known about the broader behavioral consequences of harsher penalties for non-lethal crimes such as rape. In particular, there has been limited empirical or theoretical attention given to the possibility that stricter enforcement may have the unintended consequence of incentivizing escalation—such as witness intimidation or even murder. This study addresses this gap by providing a formal framework and empirical evidence regarding how increasing the severity of rape penalties can alter the strategic calculus of rational offenders, potentially increasing the likelihood of murder to reduce the chance of conviction.

A key institutional feature underlying our mechanism is the centrality of victim testimony in the prosecution of sexual offenses. Unlike most violent crimes, sexual assaults typically occur in a private setting, rarely involve third-party eyewitnesses, and physical evidence is often limited or ambiguous. As a result, criminal liability frequently turns on the credibil-

ity, availability, and persistence of the victim's testimony. This dependence on testimony is explicitly recognized in criminal law doctrine and prosecutorial practice: courts and leading treatises describe cases of sexual assault as paradigmatic "credibility contests," in which, absent contemporaneous forensic documentation or corroborating witnesses, the complainant's testimony is often the primary, and sometimes only, source of probative evidence (LaFave , 2003).

This evidentiary reality is reflected in persistently low clearance and conviction rates for sexual offenses relative to other violent crimes, even conditional on reporting. The pattern is emphasized in federal prosecutorial guidance and empirical legal scholarship (U.S. Department of Justice , 2013, Cassell , 2008). In this context, reforms that substantially increase the expected sanction upon conviction can raise the strategic value of preventing the witness from giving testimony. When conviction hinges on a single indispensable witness, rather than increasing deterrence, a longer potential sentence may unintentionally strengthen incentives to eliminate the witness.

Murders carried out to eliminate victims who might otherwise report or testify in sexual assault cases are a legally recognized phenomenon. In State v. Gillies (1984), the Arizona Supreme Court upheld a finding of witness elimination where the defendant admitted killing a rape victim to prevent her from testifying. More recently, a Kansas appellate court in State v. Nesbitt (2018) found that the defendant's rape of a teenage girl was inextricably linked to her subsequent murder, underscoring the risk that victims of sexual violence may be silenced through lethal means. Outside of the courtroom, a 2023 case in Florida drew national attention after a teenage girl who reported being raped was subsequently murdered. Prosecutors successfully argued that the killing was motivated by a desire to prevent her from testifying, and the defendant was convicted of murder-for-hire in federal court.1 These cases illustrate the legal and factual plausibility of a behavioral response by individuals who, when faced with increased legal sanctions for sexual assault, may try to eliminate witnesses

to avoid prosecution[1] . These cases illustrate the legal and factual plausibility of a behavioral response wherein individuals, when faced with increased legal sanctions for sexual assault, may seek to eliminate witnesses to avoid prosecution.[2]

We first provide a formal conceptual model for determining the optimal penalty for rape, taking into account the possibility that increasing the punishment for rape could incentivize criminals to eliminate witnesses through murder. The model starts with the criminal's decision problem and derives the conditions under which murder becomes optimal. Finally, we provide comparative statics with respect to the death penalty. In particular, the model has precise predictions that the incentive to "kill the witness" should be driven by situations where homicide is not punished harshly, i.e. in states without the death penalty.

We consider two critical settings for evaluating the link between rape and sexually motivated homicides: the abolition of the marital-rape exemption in the definition of rape, and the introduction of a mandatory minimum 25-year prison sentence for crimes of adults against children who are younger than 12 years old. In the United States, these treatments were staggered. The first spanned several decades, and the second was a reaction of several states to the brutal kidnapping, rape, and murder of Jessica Lunsford, in Florida. These crimes were committed by a previously convicted sex offender and caused national outrage.

Rape within marriage had not been recognized as a criminal offense in the United States. According to the legal doctrine of marital exemption, by marrying, a woman gave irrevocable consent to sexual intercourse with her husband, rendering the concept of rape legally incoherent. Spousal rape refers specifically to non-consensual sexual intercourse between legally married partners—a violation of bodily autonomy irrespective of marital status. Until the

[1] `https://people.com/isabella-scavelli-florida-sexual-assault-murder-lenard-white-11847450`, article written on Novembeer 11, 2025, url accessed on December 6, 2025 (Wright, 2023).

[2] These legal principles are echoed in cinema, where the elimination or intimidation of witnesses to silence testimony often drives plot and character decisions. For example, in *Witness (1985),* a child becomes the target of corrupt police officers after witnessing a homicide; and in *A Time to Kill (1996),* the threat of retaliation against the defense attorney, his family, and associates plays a central role in the courtroom drama. These portrayals reflect a widely held—and often legally substantiated—concern that individuals who report or testify about sexual or violent crimes may face lethal consequences.

1970s, marital rape was exempted from criminal relevance. Beginning in the 1970s, there were efforts to remove this longstanding exemption. Nebraska was the first state to fully criminalize spousal rape in 1976, followed closely by Oregon in 1977. Other states gradually followed suit over the next several decades.

In the mid-2000s, the public outcry over violent crimes against children led to public outrage, prompted by the kidnapping, rape, and murder of Jessica Lunsford, a 9-year-old girl assaulted and buried alive by a repeat sex offender living near her home. Jessica's case captured national attention. The fact that the offender had a criminal history involving sex crimes but had nevertheless been free to offend again increased the public's demand for harsher sentences. As a result, Florida enacted legislation that dramatically increased penalties for sexual offenses against minors, with one of the most notable provisions being a mandatory minimum sentence of imprisonment for 25 years to life for child molestation involving victims under the age of 12.

The core rationale for this minimum sentencing was deterrence and incapacitation. First, the high media visibility of minimum-sentence provisions was expected to deter criminals from sexually attacking children. Second, the public assumed that crime would be reduced by incapacitating those who had already demonstrated a willingness to harm children by ensuring that they could not re-offend while incarcerated. The economic literature confirms that incapacitation reduces recidivism (Drago, Galbiati, and Vertova , 2009).The policy was so politically popular that within a few years, many other states had, in the name of Jessica Lunsford, also increased the minimum sentence for child molestation to 25 years. While these legal reforms were an important advancement in the recognition of women's rights, they also raise complex policy questions regarding enforcement and deterrence. Increasing the legal penalties (by extending prison sentences) for spousal rape or child molestation increases the expected cost for perpetrators, which is generally assumed to act as a deterrent. However, as the theoretical model discussed above highlights, this effect may be complicated

by offenders' strategic behavior. Specifically, when the penalties for rape rise substantially, rational offenders may become more willing to commit an additional crime - such as murder - to reduce the probability of being convicted. If, for example, killing their spouse (who is often the sole witness) or a child sharply reduces the likelihood of being convicted for rape, then harsher penalties for that crime may inadvertently incentivize more violent forms of concealment. Knowing the elasticity of witness elimination to rape penalties can inform the fundamental trade-off in designing penalties when crimes are sequential, and evidence is tied to the victim's survival.

We find that abolishing spousal rape exemptions increased wife homicides by approximately 5 victims per state-year (27 percent), while mandatory 25-year minimum sentences for child sexual abuse increased child homicides by approximately 11 victims per state-year (80 percent), by estimating a traditional difference-in-differences model.

Because the TWFE estimators may be biased in the presence of staggered policy adoption and heterogeneous treatment effects, we show that the results are robust when estimated with methods up to date with the difference-in-differences literature: we turn to the doubly robust estimator for the spousal sample, and the stacked difference-in-differences specification for the children sample. We show that the results are robust to these methods.

These results complement an extensive empirical literature documenting behavioral responses to changes in criminal sanctions (e.g., Becker , 1968, Levitt , 1998) and align with evidence that domestic violence is highly responsive to changes in legal and economic constraints (Iyengar , 2009). An important distinction in the economics of crime and criminal law is between policies that operate *ex ante*, by increasing the probability of arrest or immediate intervention, and policies that operate *ex post*, by increasing the severity of punishment conditional on conviction. A person can be more likely to be arrested, without an arrest necessarily leading to a conviction and, hence, a penalty. Mandatory arrest laws and related enforcement reforms primarily affect policing behavior at the time of the offense, altering

the likelihood that an incident results in arrest. By contrast, statutory changes such as the ones we study, the abolition of marital rape exemptions or the introduction of mandatory minimum sentences, operate at the adjudication and sentencing stages, sharply increasing expected penalties only if a conviction is secured. Our theory is focused at the sentencing stage. This distinction is central in settings where conviction hinges on victim testimony: while ex ante enforcement may deter some offenses through increased detection, ex post penalty enhancements can also raise incentives to prevent testimony altogether when the expected cost of conviction rises sufficiently. Existing empirical evidence on the unintended consequences of enforcement-based domestic violence policies has yielded mixed conclusions, and subsequent reanalysis has urged caution in interpreting some earlier findings (Iyengar , 2009, 2016).[3] Our analysis focuses instead on ex post penalty regimes and identifies a distinct mechanism linking punishment severity, witness dependence, and incentives for lethal escalation.

A central implication of the model is that penalty enhancements should have larger effects on homicide in jurisdictions without the death penalty. When the maximum sanction is life imprisonment, raising the penalty for sexual violence increases the "distance" between rape and murder, increasing incentives for witness elimination. In contrast, where capital punishment is already available as a potential sentence, the marginal change in expected punishment is limited. We show that the heterogeneity results are consistent with the model.

An important assumption of the difference-in-differences method is that the pre-trends are parallel across the treatment and the control groups. We verify that the assumption holds.

Finally, we estimate two sets of placebo estimations. We estimate our specifications on sets of victims plausibly unrelated to our main channel. The placebo sample includes

[3]In particular Iyengar (2016) writes: ""Nevertheless, the replication results granted me sufficient pause over the original findings to write an erratum and suggest explicit caution should be taken when interpreting the original findings from my 2009 paper."

siblings or strangers victims for the spousal treatment, and older victims for the children sample. Across both reforms, the estimated effects are concentrated as the mechanism predicts, among victims who are *both* (i) directly protected by the policy and (ii) capable of serving as witnesses. The absence of effects on non-targeted groups strengthens the causal interpretation of the findings and rules out spurious explanations, such as contemporaneous shocks to violence, enforcement, or reporting.

This study contributes to several strands of the literature. First, it contributes to the literature on the consequences of the penalties for rapes (Agan , 2011, Berger, Neuman and Searles , 1991, Berger, Searles and Neuman , 1988, Gao and Petrova , 2022). The paper contributes to this literature by showing that rapes can cause "rationally motivated" (in light of the past rape) homicides.

Second, it contributes to the literature of the economic approach to crime, that establishes that the perpetrators of criminals respond to incentives and are less likely to commit a crime if enforcement for that crime increase (Becker , 1968, Drago, Galbiati, and Vertova , 2009, Hansen , 2015, Katz, Levitt, and Shustorovich , 2003). The paper contributes to this literature by showing that the increases the penalties for one crime can increases the probability of committing another crime when the probability of conviction for the former crime reduces as a direct result of perpetrating the latter crime.

Third, it contributes to the small literature relating rapes and homicides (DeLisi , 2014, Connery , 2013). DeLisi (2014) examines how a history of rape predicts repeat or multiple homicides. Connery (2013), importantly for this paper, establishes that the evidence from rapes has been used to solve murders, for instance with DNA traces extracted from rape kits (Connery , 2013). This paper contributes to this literature establishing a precise link, both theoretical and empirical, through which rape penalties and sexually motivated murders are related: the elimination of an uncomfortable witness useful for rape conviction, the victim.

The paper proceeds as follows. Section 2 presents the conceptual framework. Section 3

presents the data and the institutional setting. Section 4 present the econometric strategy. In Section 5 we present the results. We conclude in Section 6

2 Conceptual Framework

In this model we provide a simple theoretical model rationalizing our main question: whether an exogenous increase in rape penalties causes an increase in the number of murders. We follow the Beckerian's economic approach to crime, which sees crime depending on the probability of convinction and on the economic cost of the penalty (Becker , 1968). A rational criminal considers committing two possible crimes: Crime A (rape), which generates a private benefit B_A; and crime B (murder, i.e., eliminating the witness), which generates a private benefit B_B (e.g., preventing conviction for rape).

The expected utility function of the criminal is given by:

$$U = B_A d_A + B_B d_B - p_A(w) F_A d_A - p_B F_B d_B, \tag{1}$$

where: $d_A \in \{0, 1\}$ is the decision to commit rape; $d_B \in \{0, 1\}$ is the decision to commit murder (eliminating the witness); B_A, B_B are the private benefits of rape and murder, respectively; $p_A(w)$ is the probability of conviction for rape, which depends on the presence of a witness ($p_A(1)$ if the witness is alive; $p_A(0)$ if the witness is dead); p_B is the probability of conviction for murder; F_A and F_B are the punishments for rape and murder, respectively. The Beckerian's view of crime received considerable empirical support. For instance, Levitt (1998) shows how an increase in police activity reduces crime, and Drago, Galbiati, and Vertova (2009), importantly for this paper, show how a longer prison sentences reduces crime.

The criminal is subject to a risk constraint:

$$p_A(w)d_A + p_B d_B \leq R, \tag{2}$$

where R represents the criminal's risk tolerance (how much legal risk they are willing to take). This budget constraint reflects the idea that engaging in multiple offenses raises the cumulative risk of punishment and that offenders account for this joint risk when deciding whether to commit each crime. There is a limited risk of punishment that the prospective perpetrator allows, and therefore the possibility of committing two related crimes generates a tradeoff: committing one crime consumes part of the offender's risk "budget," making it more costly, in expected punishment terms, to commit an additional crime. This setup focuses on how expected sanctions jointly constrain behavior, allowing us to isolate substitution patterns between crimes, rather than modeling the utility or private returns from each offense.

2.1 When Does the Criminal Commit Murder?

A criminal commits murder ($d_B = 1$) if and only if the expected reduction in punishment from eliminating the witness outweighs the cost of being convicted for murder. Mathematically, this occurs when:

$$F_A \left(p_A(1) - p_A(0)\right) > p_B F_B. \tag{3}$$

Rearranging:

$$p_A(1) - p_A(0) > \frac{p_B F_B}{F_A}. \tag{4}$$

The left-hand side is the extent to which killing the witness reduces the probability of conviction for rape, and the right-hand side is the relative deterrence effect of murder penalties.

Proposition 1. *In the model where a rational offender chooses whether to commit rape ($d_A \in \{0,1\}$) and/or murder ($d_B \in \{0,1\}$), and where the probability of rape conviction depends on whether the witness is alive ($p_A(w)$, with $w = 1$ if alive, $w = 0$ if eliminated), then:*

$$\frac{\partial d_B^*}{\partial F_A} > 0$$

That is, the incentive to commit murder increases with the severity of the rape penalty F_A.

Proof. The offender chooses $d_A, d_B \in \{0,1\}$ to maximize utility:

$$U = B_A d_A + B_B d_B - p_A(w) F_A d_A - p_B F_B d_B$$

subject to the constraint:

$$p_A(w) d_A + p_B d_B \leq R$$

Assume $d_A = 1$ (rape is committed), and that $w = 1$ if $d_B = 0$ (i.e., witness survives), and $w = 0$ if $d_B = 1$ (i.e., murder eliminates witness).

The criminal compares the utility from $(d_A = 1, d_B = 0)$ and $(d_A = 1, d_B = 1)$.

The criminal compares the utilities in the two scenarios

$$U_0 = B_A - p_A(1) F_A$$

and

$$U_1 = B_A + B_B - p_A(0) F_A - p_B F_B$$

The criminal chooses $d_B = 1$ (murder) if and only if:

$$U_1 \geq U_0$$

$$B_B - (p_A(0) - p_A(1)) F_A - p_B F_B \geq 0$$

We can define the incentive to murder (threshold inequality):

$$\underbrace{B_B - p_B F_B}_{\text{net benefit of murder}} \quad \geq \quad \underbrace{(p_A(0) - p_A(1))F_A}_{\text{benefit from silencing witness}}$$

We can solve for the critical value of F_A at which equality holds:

$$F_A^* = \frac{B_B - p_B F_B}{p_A(1) - p_A(0)}$$

Note: $p_A(1) > p_A(0)$ implies denominator > 0.

Comparative statics.

We examine how the optimal choice of d_B^* responds to a marginal increase in F_A.

From the inequality:

$$d_B^* = 1 \iff F_A \geq F_A^* = \frac{B_B - p_B F_B}{p_A(1) - p_A(0)}$$

Therefore, as F_A increases, it becomes more likely that:

$$F_A \geq F_A^* \Rightarrow d_B^* = 1$$

Hence,

$$\frac{\partial d_B^*}{\partial F_A} > 0$$

A higher rape penalty F_A increases the incentive to commit murder $d_B = 1$, because it makes the benefit of reducing the rape conviction probability (via silencing the witness) more valuable.

□

2.2 Heterogeneity with Severity of Murder Penalties

The baseline model establishes that increasing the rape penalty F_A raises the incentive to commit murder. However, the model as presented does not directly address how this relationship varies with the severity of the murder penalty F_B. Intuitively, we expect the marginal effect of raising F_A to be stronger when F_B is relatively low: if murder is already punished severely (e.g., with the death penalty), further increases in the rape penalty should have limited impact on the murder decision, since the criminal already faces extreme consequences. By contrast, when murder penalties are modest, raising the rape penalty widens the gap between the two offenses and should more strongly incentivize witness elimination.

To formalize this intuition, we extend the model to incorporate two features: (i) decreasing marginal disutility of punishment, and (ii) heterogeneity in the private benefits of murder across potential offenders. The first feature captures the idea that criminals exhibit diminishing sensitivity to punishment as expected penalties rise - a natural assumption if utility is concave in wealth or if psychological costs of imprisonment exhibit satiation. The second feature allows us to characterize the fraction of offenders who choose murder as a function of policy parameters.

Assume that the disutility of expected punishment is given by:

$$D(E[\text{Penalty}]) = (E[\text{Penalty}])^{\alpha},$$

where $0 < \alpha < 1$. The concavity of $D(\cdot)$ reflects decreasing marginal disutility: each additional unit of expected punishment imposes a smaller incremental psychological or monetary cost than the previous unit. This formulation is consistent with standard assumptions of risk aversion or diminishing marginal utility of consumption, and it captures the intuition that criminals who already face severe punishment are less sensitive to further increases.

Under this specification, the expected utility without murder is:

$$U_0 = B_A - [p_A(1) \cdot F_A]^{\alpha},$$

and with murder it is:

$$U_1 = B_A + B_B - [p_A(0) \cdot F_A + p_B \cdot F_B]^{\alpha}.$$

The criminal chooses murder when $U_1 > U_0$, which simplifies to:

$$B_B > [p_A(0) \cdot F_A + p_B \cdot F_B]^{\alpha} - [p_A(1) \cdot F_A]^{\alpha} \equiv \Psi(F_A, F_B).$$

The function $\Psi(F_A, F_B)$ represents the threshold benefit required to make murder optimal. We are interested in how this threshold responds to changes in F_A and F_B, and in particular, in the cross-partial derivative.

Taking the partial derivative with respect to F_A:

$$\frac{\partial \Psi}{\partial F_A} = \alpha \left[p_A(0) \cdot F_A + p_B \cdot F_B\right]^{\alpha-1} p_A(0) - \alpha \left[p_A(1) \cdot F_A\right]^{\alpha-1} p_A(1).$$

To sign this derivative, note that witness elimination substantially reduces the conviction probability for rape: $p_A(1) \gg p_A(0)$. In the limiting case where witness elimination is fully effective ($p_A(0) \approx 0$) the first term vanishes, and we obtain:

$$\frac{\partial \Psi}{\partial F_A} \approx -\alpha \left[p_A(1) \cdot F_A\right]^{\alpha-1} p_A(1) < 0.$$

This result confirms that increasing the rape penalty F_A lowers the threshold Ψ, making murder more attractive. Intuitively, a higher rape penalty increases the value of eliminating the witness, thereby reducing the private benefit B_B required to justify murder.

Similarly, the partial derivative with respect to F_B is:

$$\frac{\partial \Psi}{\partial F_B} = \alpha \left[p_A(0) \cdot F_A + p_B \cdot F_B \right]^{\alpha - 1} p_B > 0.$$

Higher murder penalties raise the threshold, making murder less attractive. This is the standard deterrence effect.

The key result concerns the cross-partial derivative:

$$\frac{\partial^2 \Psi}{\partial F_A \partial F_B} = \alpha(\alpha - 1) \left[p_A(0) \cdot F_A + p_B \cdot F_B \right]^{\alpha - 2} p_A(0) p_B < 0,$$

where the inequality follows from $\alpha - 1 < 0$ and the fact that all other terms are positive.

This negative cross-partial derivative implies that the marginal effect of increasing F_A on the murder decision is *smaller in absolute value* when F_B is high. To see this more clearly, note that:

$$\frac{\partial}{\partial F_B} \left(\frac{\partial \Psi}{\partial F_A} \right) < 0.$$

In words: as the murder penalty rises, the marginal impact of the rape penalty on the attractiveness of murder diminishes. The mechanism operates through the decreasing marginal disutility of punishment. When F_B is already high, the total expected punishment $p_A(0) \cdot F_A + p_B \cdot F_B$ is large, placing the criminal on a relatively flat portion of the disutility function. In this region, incremental changes in F_A have limited impact on total disutility. By contrast, when F_B is low, incremental changes in F_A have a larger effect, since the criminal is on a steeper portion of the curve.

To translate this threshold analysis into predictions about aggregate behavior, assume there is a continuum of potential offenders, indexed by i, each with a heterogeneous private benefit from murder $B_{B,i}$ drawn from a cumulative distribution function $G(\cdot)$ with associated density $g(\cdot)$. The fraction of offenders (among those who commit rape) who also commit

murder is then:

$$\mu(F_A, F_B) = \Pr\left(B_{B,i} > \Psi(F_A, F_B)\right) = 1 - G\left(\Psi(F_A, F_B)\right).$$

The first-order effect of increasing F_A on the murder rate is:

$$\frac{\partial \mu}{\partial F_A} = -g\left(\Psi\right)\frac{\partial \Psi}{\partial F_A} > 0,$$

since $\partial\Psi/\partial F_A < 0$ and $g(\cdot) > 0$. This reaffirms that higher rape penalties increase the prevalence of murder.

The cross-partial derivative is:

$$\frac{\partial^2 \mu}{\partial F_A \partial F_B} = -g'\left(\Psi\right)\frac{\partial \Psi}{\partial F_A}\frac{\partial \Psi}{\partial F_B} - g\left(\Psi\right)\frac{\partial^2 \Psi}{\partial F_A \partial F_B}.$$

The second term is unambiguously positive, since $\partial^2\Psi/\partial F_A \partial F_B < 0$ and $g(\Psi) > 0$. The first term is also positive under the plausible assumption that the density $g(\cdot)$ is decreasing in the relevant range—that is, that fewer offenders have very high private benefits from murder. Specifically, if $g'(\Psi) < 0$, then:

$$-g'\left(\Psi\right)\frac{\partial \Psi}{\partial F_A}\frac{\partial \Psi}{\partial F_B} > 0,$$

since $\partial\Psi/\partial F_A < 0$ and $\partial\Psi/\partial F_B > 0$.

Combining both terms, we obtain:

$$\frac{\partial^2 \mu}{\partial F_A \partial F_B} < 0$$

under the maintained assumptions of decreasing marginal disutility ($\alpha < 1$) and a non-increasing density of private benefits in the upper tail ($g'(\Psi) \leq 0$).

This formal result delivers a sharp empirical prediction: the effect of increasing rape penalties on murder should be stronger in jurisdictions without the death penalty (low F_B) than in jurisdictions with the death penalty (high F_B). When murder is already punished with the maximum available sanction, further increases in the rape penalty generate limited additional incentive for witness elimination, because the marginal disutility of punishment is small. By contrast, in the absence of capital punishment, raising rape penalties meaningfully widens the punishment gap between rape and murder, creating stronger incentives to eliminate the witness.

We test this prediction directly in the empirical analysis by estimating heterogeneous treatment effects across states with and without the death penalty. The model predicts that the estimated coefficients on the rape penalty reforms should be significantly larger - in absolute magnitude - in non-death-penalty states, consistent with $\partial^2\mu/\partial F_A \partial F_B < 0$.

3 Data and Institutional Setting

In this section, we outline two empirical settings used to bring the core predictions of our model to the data. First, we examine the abolition of the marital exemption in the legal definition of rape. Second, we investigate the adoption of "Jessica's Law" across multiple states in the United States. At the state level, such laws were aimed at increasing the severity of punishments for child molestation. The law was initially adopted in Florida in 2005, and other states implemented it between 2005 and 2008. While the specific provisions of Jessica's Law vary by state, a central and relatively consistent component was a mandatory minimum sentence of 25 years for first-time offenders convicted of molesting young children. We define the victim as a child if they are under the age of 12 (18 U.S.C. 2241 and 18 U.S.C. 2243), ensuring consistency in definition across the sample.

We find support for our main question—whether eliminating the marital exemption for

rape or increasing the penalties for sexual violence against children could unintentionally increase the risk of spousal or child homicide—in longstanding legal concerns about witness elimination as a motive for murder. Courts have repeatedly recognized that individuals may kill the victims of sexual assault to prevent their testimony. Arizona is an interesting anecdotal case study. The Arizona Supreme Court has upheld witness elimination as an aggravating factor under A.R.S. §13-751(F)(10), particularly when a murder is committed with the intent to silence a victim or witness to another crime, including rape (State v. Gillies, 1984; State v. Stokley, 1995; State v. Speer, 2009). In State v. Gillies (1984), the defendant admitted to an acquaintance that he and an accomplice killed the victim to prevent her from testifying against them for rape. Similarly, in State v. Stokley (1995), the defendant confessed to a detective that his motive for killing victims after sexually assaulting them was fear that they would report the crimes. Finally, in State v. Speer (2009), where evidence showed the defendant arranged to have the victim killed to prevent testimony in his pending burglary trial, the court upheld that this was an "especially heinous or depraved" aggravating factor. These cases illustrate how Arizona courts have consistently treated witness elimination as evidence of particularly culpable conduct deserving enhanced punishment.

3.1 Spousal Rape and Murder

Until the late twentieth century, most U.S. states adhered to the common-law principle that men were exempt from prosecution for raping their wives. This so-called "marital rape exemption" was rooted in centuries-old English common law, most notably articulated in 1736 by Sir Matthew Hale, who claimed that a husband could not be guilty of raping his wife because by marriage she had given irrevocable consent to sexual intercourse (Hale , 1736). The doctrine persisted in American legal systems well into the 1970s, based on historical notions that women were the property of their husbands and that a husband and his wife were a single legal entity. Starting in the mid-1970s, states began to dismantle

this exemption. Over time, legal and social shifts, including those brought about by the women's rights movement, increased recognition of intimate partner violence, and changes in constitutional interpretations of privacy and bodily autonomy, pressured legislatures to fully criminalize spousal rape. The movement culminated in a wave of statutory reforms in the 1980s and 1990s. As detailed by McMahon-Howard, Clay-Warner and Renzulli (2009), these reforms reflected a more profound change in how the law viewed consent, marriage, and the autonomy of women in intimate relationships.

Table 1 reports the year in which each U.S. state removed the spousal exemption from its rape statutes, allowing non-consensual sex within marriage to be prosecuted as rape. These legal reforms represent a sharp change in how the criminal justice system treats intimate-partner violence. Before the treatment, the common-law principle of implied marital consent made non-consensual spousal sex a legitimate act. The primary source for this coding is the legal analysis conducted by McMahon-Howard, Clay-Warner and Renzulli (2009), who provide a detailed review of statutory and case law developments in the majority of states. We verify their coding through independent review of the relevant statutory and judicial sources and augment the dataset where information was missing or ambiguous. In particular, we update the entries for New York, South Dakota, and Washington based on direct legal citations: New York's exemption was struck down by the Court of Appeals in *People v. Liberta*, 64 N.Y.2d 152 (1984); South Dakota's statutory reform is found in Chapter 22-22 of the South Dakota Codified Laws; and Washington eliminated the exemption via Substitute Senate Bill 3007 in 1983. These dates structure our empirical analysis of the downstream consequences of the legal recognition of spousal rape, allowing us to implement a staggered policy-adoption framework that captures the timing and geographic variation in reform.

The data on spousal and intimate murders are drawn from the Supplementary Homicide Reports (SHR, henceforth) issued by the Federal Bureau of Investigation (United States DOJ , 2015). The SHR is an annual data-collection program compiling detailed information on

homicides occurring across the United States. The SHR data contain information about the degree of relationship between the victim and the perpetrator. We aggregate the SHR dataset to the state-year level and restrict attention to observations with valid state identifiers and known victim–offender relationships. The summary statistics in Table 3 describe the distribution of homicide incidents by relationship type across states and years.

Our primary outcome of interest is spousal homicide, which we define as the number of victims murdered by their legal spouse (husband or wife). We examine broader patterns of domestic violence by constructing a composite category of intimate partner homicides that includes victims killed by their current or former spouses, cohabiting partners (common-law spouse, boyfriend, girlfriend), and ex-partners. This broader category captures a wider spectrum of domestic violence outside of marital ties.

We use three variables to test robustness: murders that are not intimate, homicides of siblings, and homicides of strangers. All three should not be directly affected by changes in marital rape laws. These serve as falsification outcomes to assess the robustness of the treatment effect. Non-intimate murders are defined as the total murders minus those involving intimate partners.

The sample includes 1,880 state-year observations, with an average of 19.3 spousal homicides and 36.2 intimate homicides per state-year. We use the data from 1976 to 2015.

3.2 Child Homicide

The crime of child rape is among the most serious offenses prosecuted under state criminal codes, reflecting a broad social consensus about the need to protect children from sexual violence. Legal definitions of child rape typically refer to sexual acts involving a minor below a specified age threshold—often under 12 or 16 years old—with consent being legally irrelevant due to the age of the victim. These statutes recognize the particular vulnerability of children and the long-term psychological and developmental harm caused by sexual abuse

(Beitchman et al., 1992, Currie and Tekin, 2012). Over time, the public and legislative responses to child rape have become increasingly punitive, particularly in cases involving repeat offenders. High-profile incidents in the early 2000s sparked widespread concern about gaps in the ability of the criminal justice system to prevent recidivism, leading to a push for stricter mandatory minimum sentences. These legislative efforts were driven by a deterrence rationale and a desire to impose uniform, severe punishment for crimes perceived as uniquely heinous.

Table 2 summarizes the adoption of state-level legislation imposing mandatory minimum sentences of at least 25 years for the crime of child rape. These statutes were enacted across several states in the mid-2000s in response to public concern over repeat sex offenders and the perceived leniency of sentences. While sharing a deterrence objective, the specific statutes vary in their wording and scope and were passed under different bill numbers and legislative vehicles (House Bills, Senate Bills, Assembly Bills, and Public Laws). Table 2 presents the state, year of enactment, and the legal reference of the relevant law. The coding is based on an original review and verification of legislative texts and statutory records by the authors. The systematic coding of mandatory sentencing for child rape of "at least 25" years is one of the contributions of the study. This variation in policy timing allows us to identify the treatment effects in a staggered difference-in-differences design, discussed below.

Our data on child homicides are sourced from the National Incident-Based Reporting System (NIBRS), which is part of the Uniform Crime Reporting Program managed by the Federal Bureau of Investigation. Compared with the SHR dataset, NIBRS provides incident-level data with greater detail, including information on the age of the victim, the location and timing of the incident, and all associated offense types. This level of detail is critical for our analysis. We use the NIBRS extract files of each year, as these use the original segments of NIBRS data. We reorganize these into four levels—incident, victim, offender, and arrestee—for ease of use. We specifically use the victim-level files because these allow

us to identify the age of all victims associated with an incident; if an incident had two child victims, we consider them as two separate homicides.

We define child homicide as any criminal incident categorized in the NIBRS dataset as "murder" and "nonnegligent manslaughter," in cases where the victim falls below certain statutory age thresholds. Specifically, we construct two alternative definitions of child homicide based on victim age: under 12 and under 16. These thresholds reflect two relevant legal concepts in the context of child victimization: the age of general legal minority and the statutory age of sexual consent.[4] We restrict the incident-level data to homicide offenses involving victims below these respective age cutoffs and aggregate the filtered observations to the state-year level to construct our key outcome variables.

It is important to note that NIBRS is a voluntary reporting system, so not all states participate. Consequently, our analysis draws on data from 32 participating states.[5] There are twelve states that are not included in our analysis.[6] There is also substantial variation in the timing and consistency of reporting among participating states. Indiana and Pennsylvania did not begin submitting NIBRS data until 2013 and 2014, respectively, and Mississippi only began reporting in 2009. Because there are insufficient observations for these states, we exclude them from the analysis. Similarly, although Missouri, Montana, and Oklahoma provided data beginning in 2006, 2005, and 2008, respectively, these states do not have a valid pre-treatment period, as their treatment years coincide with or precede the start of reporting—2005 for Missouri and Montana and 2007 for Oklahoma. These states are

[4]The age thresholds used-under 12 and under 16-are consistent with federal legal definitions of child sexual abuse and statutory rape. Under U.S. federal law (18 U.S.C. § 2243), sexual acts with individuals between ages 12 and 16 are criminalized when the perpetrator is at least four years older, while more severe penalties apply for victims under age 12 (18 U.S.C. § 2241(c)). These age limits also reflect prevailing standards in state-level statutory rape statutes.

[5]These states are Alabama, Arizona, Arkansas, Colorado, Connecticut, Delaware, Georgia, Idaho, Illinois, Iowa, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Nebraska, New Hampshire, North Dakota, Ohio, Oregon, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, and Wisconsin.

[6]California, Florida, Alaska, Hawaii, Maryland, Minnesota, North Carolina, New Jersey, New Mexico, Nevada, New York, and Wyoming.

therefore excluded. We address these data limitations by constructing a panel that omits any state without at least one observation in both the pre- and post-treatment periods. For never-treated states, we define the pre-treatment period as any year prior to 2008, and the post-treatment period as any year after 2005, reflecting the broader window during which the policy intervention took effect in treated states. Lastly, we also exclude Washington, D.C. from our analysis. While NIBRS began publishing data in 1991, we restrict our sample to the years 1992 through 2015 to ensure consistent coverage across the treatment window. Hence, our sample covers 32 states, of which 15 are never treated, and 17 are treated. We summarize the varaibles in Table 4.

4 Econometric Strategy

In this section, we outline the approach used to estimate the response of spousal and intimate murders to the abolition of the marital rape exemption. We implement a difference-in-differences strategy, leveraging the staggered introduction of the abolition of the marital rape exemption to analyze their impact on the decision to kill a potentially undesirable witness for an important rape conviction.

The first specification is, therefore:

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \phi_i + \phi_t + \epsilon_{ist} \tag{5}$$

The main dependent variables are spousal killings and intimate killings in state i and year t, in logarithms. The dummy Post is equal to one after the abolition of the marital rape exemption in year s, which differs across states. The state and year fixed effects, ϕ_i and ϕ_t, control respectively for state and time invariant factors. The standard errors, ϵ_{ist}, are robust to heteroskedasticity and clustered at the state level. The coefficient of interest β_1 captures the effect of introducing marital rape as a crime on the incecntive to kill, under

plausible assumptions. This methodology has two main assumptions. The first is that treated and control states have to be on parallel trends for the main outcomes of interest. If they were not on parallel trends, the coefficient β_1 may capture pre-existing differences in the evolution of the timeseries. We verify this assumption when discussing the results. The second assumption is more challenging to test: the absence of contemporaneous policy events that are systematically correlated with the treatment and affect differentially the treatment group and the control group. The rationale is clear: if there were such events, one would not be able to clearly ascribe the effect to the policy of interest.

For what matters the second setting, the minimum sentencing for child molestation, we estimate as a baseline a very similar equation:

$$Z_{it} = \beta_0 + \beta_1 Post_{is}^{CM} + \phi_i + \phi_t + \epsilon_{ist} \tag{6}$$

The main dependent variables are children killings in state i and year t, in logarithms. The dummy Post is equal to one after the minimum 25 years sentencing for child molestation. ϕ_i are state fixed effects and ϕ_t are time fixed-effects. ϵ_{ist} are clustered standard errors at the state level. It has to be noted that the number of clusters for this sample is smaller than 42. With a limited number of clusters, conventional cluster-robust standard errors may produce biased inference, particularly downward-biased standard errors that over-reject the null hypothesis. To address this concern, we rely on the wild-bootstrap clustering, following Cameron et al. (2008) and Angrist and Pischke (2009).

4.1 Robustness Tests

Then, we look at the doubly robust difference-in-differences (DiD) estimator. This method is particularly suited for settings with staggered treatment adoption, as is the case here. We follow closely the approach of Callaway and Sant'Anna (2020), focusing on the not yet treated

units as a proper comparison group. We consider a case with $\mathcal{T}$ periods, still denoting with t the individual period of interest.

Consider a random sample:

$$\{(Y_{i,1}, Y_{i,2}, ..., Y_{i,\mathcal{T}}, D_{i,1}, D_{i,2}, ..., D_{i,\mathcal{T}})\}_{i=1}^{n}$$

where Y_{it} is the outcome for unit i in period t, $D_{it} = 1$ if unit i is treated in period t, and 0 otherwise. In this setting the treatment is staggered and once abolished the marital rape exemption the state typically does not introduce it again , therefore

$$D_{i,t} = 1 \Longrightarrow D_{i,t+1} = 1, \text{ for } t = 1, 2, ..., \mathcal{T}$$

Following Callaway and Sant'Anna (2020), we define the treatment start date for state i as $G_{i,g} = 1$ if unit i is the first treated at time g and zero otherwise, and $Y_{it}(0)$ as unit i's untreated potential outcome at time if they remain untreated through time period $\mathcal{T}$. For $g = 2, ..., \mathcal{T}$, let $Y_{it}(g)$ denote the potential outcome that unit i would experience at time t if the treatment begins at period g. The parameter of interest is the following:

$$ATT(g, t) = E[Y_t(g) - Y_t(0)|G_g = 1] \text{ for } t \geq g$$

which is the average effect for the group of units first treated at time period g, in calendar time t. We make an assumption which is clearly related to the parallel trends in traditional difference-in-differences. Callaway and Sant'Anna (2020) define the assumption a "parallel trends assumption based on the "not-yet treated groups,"

For each $(s, t) \in \{2, ..., \mathcal{T}\} \times \{2, ..., \mathcal{T}\}$, $g \in \mathcal{G}$ such that $t \geq g$, $s \geq t$

:

$$E[Y_t(0) - Y_{t-1}(0)|G_g = 1] = E[Y_t(0) - Y_{t-1}|D_s = 0, G_g = 0] \; a.s.$$

With this assumption in mind, the estimand of interest becomes:

$$ATT^{ny}_{unc}(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|D_t = 0, G_g = 0] \tag{7}$$

where *unc* means “unconditionally" from controls and *ny* stands for “not-yet treated" (the comparison group).[7]

In our baseline analysis, we estimate the impact of introducing the mandatory minimum penalties of 25 years for child rape using a staggered difference-in-differences (DiD) specification. For our second setting we note that the treatment was adopted fast, within less than 5 years, by all of the treated states. This compressed treatment timing poses serious challenges for conventional cross-sectional DiD (CSDID) models like the one modeled for the first setting (spousal rape), where states treated at different times are implicitly used as controls for each other. The key limitation is not that the compressed treatment adoption makes hard to test the parallel trends assumption. First, when most states are treated within a couple of years, there is very little untreated variation left for meaningful pre- and post-treatment comparisons. In many post-treatment periods, all comparison units are already treated, undermining the interpretability of dynamic effects. Second, when units are treated in quick succession, the overlap in pre-treatment periods between early and late treated units shrinks. As a result, testing for pre-trends becomes more difficult. Because the doubly-robust estimator that we discussed for the case of spousal rape relies on treated units as controls for others (e.g., early-treated vs. late-treated), the absence of truly untreated units leads to comparisons between groups that are all affected to varying degrees.

[7]The robustness to controls will be tested in the last part of the empirical results.

In light of the recent literature on difference-in-differences (Goodman-Bacon , 2021, Baker, Larcker, and Wang , 2022), we provide, for our second setting, a robustness check using a *stacked event-study approach.* We estimate a *stacked event study model* following the approach of Cengiz et al. (2019), Borusyak, Jaravel, and Spiess (2024). We estimate:

$$Y_{st} = \sum_{e=-K,\, e\neq -1}^{K} \beta_e \cdot \mathbf{1}[t - T_s = e] + \alpha_s + \lambda_t + \varepsilon_{st} \tag{8}$$

where: Y_{st} is the measure of child homicides for state s in year t; T_s is the year when the the minimum 25 years penalty is first implemented in state s; $\mathbf{1}[t - T_s = e]$ is an indicator for being e years away from the treatment year ; β_e captures the event-time-specific effect of treatment at relative year e (with β_{-1} normalized to zero); α_s are state fixed effects; λ_t are year fixed effects; ε_{st} is the error term.

This stacked event-study design pools units treated in different years into a single dataset, including only control states that are untreated within the symmetric event window $[-K, K]$ around each treatment year. The specification assumes homogeneous treatment effects across states and time.

5 Results

This section presents the empirical results for the effects of the two major criminal-justice reforms discussed above: (i) the abolition of spousal rape exemptions, and (ii) the introduction of mandatory-minimum 25–year sentences for child sexual abuse. We first estimate the reduced-form effects on homicides using difference-in-differences specifications with state and year fixed effects. Then, following recent developments in the difference-in-differences literature, we present, for the spousal sample, the doubly robust estimator of Callaway and Sant'Anna (2020), while for child homicide, we rely on the stacked difference-in-differences design.

5.1 Spousal Rape Exemption Abolition

We begin by estimating conventional two-way fixed effects (TWFE) difference-in-differences models. Table 5 shows that the abolition of the marital rape exemption is associated with a sizable increase in domestic homicide. Across specifications that include state- and year-fixed effects, the estimated coefficients for spousal homicides and intimate homicides range from 0.18 to 0.194. These estimates imply semi-elasticities between *19.7%* and *20%*.

Because the TWFE estimators may be biased in the presence of staggered policy adoption and heterogeneous treatment effects, we turn to the doubly robust estimator of Callaway and Sant'Anna (2020), which combines inverse-probability weighting and OLS regression to recover consistent group-time average treatment effects. Table 6 reports the corresponding results. The preferred specification (Column 1) yields an ATT estimate of 0.235 (s.e. 0.103) for spousal homicides, implying a *26.5%* increase in these following the reform. For intimate partner homicides (Column 2), the estimate is 0.308 (s.e. 0.110), corresponding to a *36.0%* increase. The point estimates are remarkably similar to the TWFE coefficients, confirming the robustness of the findings and avoiding the known aggregation biases associated with TWFE under staggered reform timing.

These results complement an extensive empirical literature documenting behavioral responses to changes in criminal sanctions (e.g., Becker , 1968, Levitt , 1998) and align with evidence that domestic violence is highly responsive to changes in legal and economic constraints (Iyengar , 2009). Taken together, the estimates suggest that converting spousal rape from a non-prosecutable offense into a felony carrying sentences of multi-year imprisonment increases the incentive for perpetrators to eliminate witnesses, resulting in a substantial rise in intimate partner homicide.

5.2 Mandatory Minimum Sentences for Child Sexual Abuse

We begin with conventional TWFE difference-in-differences estimates. Table 7 reports the effects of introducing a mandatory minimum 25–year sentence for child sexual abuse on child homicide. For children under age 12, the TWFE coefficient of 0.598 (s.e. 0.252) implies an *81.8%* increase in child homicide following the reform. For victims under age 16, the corresponding coefficient of 0.529 (s.e. 0.250) translates into a *69.7%* increase. These estimates are large and statistically significant at the 5% level. They provide a clear first indication that sharply increasing the statutory penalty for child sexual abuse is associated with a sizable increase in child homicide.

Because TWFE estimators may be biased in settings with staggered reforms and heterogeneous treatment effects, Table 8 sets out the stacked difference-in-differences specification, which is now the preferred approach. The stacked design yields coefficients of 0.586 (s.e. 0.199) for children under 12 and 0.515 (s.e. 0.197) for victims under 16, corresponding to increases of *79.7%* and *67.4%*, respectively. The similarity between the TWFE and stacked estimates confirms the robustness of the results and mitigates concerns about bias from the staggered reforms.

The magnitude of the effects is economically meaningful. In an average state-year, the reforms imply between a half and two-thirds additional child homicides per 100,000 children. This may appear small in absolute terms, but large relative to baseline levels of this extremely rare yet extremely severe form of violence. Consistent with the theoretical expectations, the sharp increase in the legal sanction for child sexual abuse raises the incentive for perpetrators to eliminate witnesses when they retain access to the victim. The patterns mirror those observed for spousal rape, underscoring how steepening the sanction gradient between sexual violence and homicide can generate perverse and unintended behavioral responses.

5.3 Heterogeneity by Death Penalty Status

A central implication of the model is that penalty enhancements should have larger effects on homicide in jurisdictions without the death penalty. When the maximum sanction is life imprisonment, raising the penalty for sexual violence increases the "distance" between rape and murder, increasing incentives for witness elimination. In contrast, where capital punishment is already available as a potential sentence, the marginal change in expected punishment is limited.

Tables 9 and 10 set out the results of direct tests. For the spousal-rape reform, the estimated effect is modest and statistically insignificant in death-penalty states ($\hat{\beta} = 0.100$; exact semi-elasticity = 10.5%), but roughly three times larger in states without the death penalty ($\hat{\beta} = 0.259$; semi-elasticity = 29.6%). This difference aligns with the mechanism of perpetrators responding more strongly when the policy meaningfully alters the relative sanction for rape versus murder.

For mandatory minimum sentences, the heterogeneity is even sharper. In states with the death penalty, the effects on child homicide are small and statistically imprecise. In non–death-penalty states, however, the reforms generate extremely large increases: for children under 12, $\hat{\beta} = 1.161$ implies a *219.3%* increase in incidents of homicide, and for children under 16, $\hat{\beta} = 0.988$ implies a *168.6%* increase. These patterns provide strong support for the notion that the behavioral response is governed by changes in the marginal, not the absolute, sanction, which is a core prediction of the economic model of crime.

5.4 Pre-Trends and Placebo Tests

An important assumption of the difference-in-differences method is that the pre-trends are parallel across the treatment and the control groups. In Figures 1 and 2, we verify the parallel trends assumption and note the absence of statistically significant results in the pre-treatment

period. Statistical significance arises only after the treatment.

Finally, we assess whether the reforms plausibly operate through channels unrelated to witness elimination. Table 11 shows that the elimination of exemptions for spousal rape has no detectable effect on homicides of siblings or strangers; all point estimates are small and statistically indistinguishable from zero. Table 12 shows that introducing the mandatory minimum for child sexual abuse has no effect on homicides of victims aged 40 or above, a population that could not be mistaken for children and thus does not include potential victim witnesses in cases of child sexual abuse.

Across both reforms, the estimated effects are concentrated as the mechanism predicts, among victims who are *both* (i) directly protected by the policy and (ii) capable of serving as witnesses. The absence of effects on non-targeted groups strengthens the causal interpretation of the findings and rules out spurious explanations, such as contemporaneous shocks to violence, enforcement, or reporting.

6 Conclusion

In this study, we have shown that criminal-justice reforms intended to strengthen the prosecution of sexual violence can have substantial effects on homicide. Using two natural experiments in U.S. criminal law - the abolition of the marital rape exemption and the introduction of mandatory-minimum 25–year sentences for child sexual abuse - we document substantial increases in the murders of those that the reforms are most directly intended to protect. The results are consistent across estimation strategies and concentrated among victims who are both likely to be witnesses to the underlying offense and situated in jurisdictions where the reform meaningfully increased the sanction gap between sexual violence and homicide. Taken together, the evidence suggests that on the margin, raising penalties for sexual assault can alter offender incentives in ways that endanger the very individuals the law seeks to protect.

Our findings raise several questions for further research. First, although our analysis focuses on the connection between sexual offenses and intimate or familial homicide, the logic applies more broadly to any setting in which a witness's testimony can meaningfully increase the probability of conviction. Organized crime provides a useful illustration: individuals who cooperate with law enforcement often face a quantitatively higher risk of reprisal, a phenomenon memorably portrayed in popular culture and well documented in criminology.[8] Similar dynamics may arise in financial fraud, human trafficking, environmental crimes, or public corruption, where key witnesses play a pivotal role in the legal process. Extending the empirical framework to these contexts would deepen our understanding of how testimony, information, and credibility interact with the structure of criminal penalties.

Second, the study is positive rather than normative: we estimate behavioral responses but do not characterize optimal sentencing policy. Developing a formal framework in which legislators choose penalties in the presence of witness-dependent enforcement would be an important step forward. Such a model would need to balance the benefits of deterrence against the risk of escalating violence and would likely generate non-monotonic prescriptions for penalty design. The interaction between statutory minimums, prosecutorial discretion, and negotiated pleas provides especially fertile ground for theoretical and empirical work.

Finally, our results speak to a policy environment like that of the United States, where public knowledge of criminal sanctions is relatively high and legal information is widely accessible. In many developing economies, individuals are less familiar with statutory penalties, and the relationship between awareness, expectations, and behavior may differ substantially from the U.S. context. Studying the relationship between legal knowledge and offender responses in settings of low state capacity would help clarify the external validity of our findings and contribute to ongoing debates in the fields of development economics, law and economics, and behavioral public policy.

[8]For example, Gambetta (1996) discusses the strategic use of violence by organized crime to suppress potential informants, a pattern echoed in classic depictions such as the movie *The Godfather II*.

Tables and Figures

Table 1: Year of Removal of Spousal Rape Exception by State

State	Year of Removal
Nebraska	1976
Oregon	1977
New Jersey	1978
Massachusetts	1981
New Hampshire	1981
Wisconsin	1981
Washington	1983
New York	1984
Vermont	1985
North Dakota	1987
Colorado	1988
Indiana	1989
Maine	1989
South Dakota	1990
Missouri	1991
Montana	1991
New Mexico	1991
Utah	1991
North Carolina	1993
Texas	1993
Pennsylvania	1995
Georgia	1996
Delaware	1998
Kentucky	2000
West Virginia	2000
Arkansas	2001

Notes: *Year of Removal* stands for the year in which the spousal rape exception was repealed from the definition of rape.The sources are, first, McMahon-Howard, Clay-Warner and Renzulli (2009) for the majority of states, plus authors' coding: New York (People v. Liberta, 64 N.Y.2d 152, 474 N.E.2d 567, 485 N.Y.S.2d 207 (1984)), South Dakota (South Dakota Codified Laws, Chapter 22-22 (Sex Offenses)) and Washington (Substitute Senate Bill 3007, Sess. Laws Wash. 1983, ch. 118 (Wash. Rev. Code 9A.44.040, 9A.44.050)).

Table 2: State Mandatory Sentencing - Child Rape (25+ Years)

State	**Year**	**Law**
Arkansas	2006	HB 1005
Connecticut	2007	SB 1458
Delaware	2006	HB 404
Florida	2005	HB 1877
Georgia	2006	HB 1059
Kansas	2006	HB 2576
Louisiana	2006	HB 4
Michigan	2006	HB 5421 & 5422
Missouri	2005	HB 353
Montana	2005	SB 207
Oklahoma	2007	HB 1816
Oregon	2006	HB 3511A
Rhode Island	2006	P.L. 2006, ch. 206
South Carolina	2006	SB 1138
Tennessee	2007	HB 2314
Texas	2007	HB 008
Utah	2008	HB 0256
Virginia	2006	HB 846 (Chapter 853)
Washington	2006	HB 3277
West Virginia	2006	HB 101A
Wisconsin	2006	AB 784

Notes: This table reports the year and legislative reference for the adoption of mandatory minimum sentencing laws requiring at least 25 years of imprisonment for child rape offenses. "HB" and "SB" denote House Bill and Senate Bill, respectively. "PL" stands for Public Law. "AB" stands for Assembly Bill. The coding is based on authors' original review of state statutes and legislative records.

Table 3: Summary Statistics

	Mean	SD	Min	Max	N
Spousal Murders	19.34149	25.6872	0	196	1880
Intimate Murders	36.21649	45.24949	0	309	1880
Spousal Homicides (Log.)	2.410062	1.14446	0	5.283204	1880
logintmurder	2.999229	1.187819	0	5.736572	1880
Post Full Abolition Marital Rape Exempt.	.3723404	.483557	0	1	1880
Death Penalty State	.5558511	.4970031	0	1	1880
Non-Intimate Murders	318.4606	472.7084	0	3890	1880
Murder of Strangers	56.3	109.4005	0	1079	1880
Murder of Siblings	3.895213	5.123819	0	44	1880

Notes: *Spousal Homicides* stands for number of victims murdered by a legal spouse. *Intimate Homicides* stands for number of victims murdered by a legal spouse, former spouse, cohabiting partner and ex-partner. *Post Full Abolition Marital rape Exempt.* is a time dummy which equals to 1 after the full abolition of the marital rape exemption. *Death Penalty State* is a space dummy which equals to 1 in state that have the death penalty. *Non-Intimate Murders* refers to the total number of murders where the relationship with the perpetrator is known, and excluding those committed by intimate partners as defined above. *Murder of Stranger* captures the number of victims murdered by an offender identified as a stranger to the victim. *Murder of Sibling* counts the number of cases in which the victim was the brother or sister of the offender.

Table 4: Summary Statistics

	Mean	SD	Min	Max	N
Child Homicides below 12	13.64	19.52	0.00	113.00	546
Child Homicides below age 12(log)	1.90	1.30	0.00	4.74	546
Child Homicides below 16	16.76	23.13	0.00	133.00	546
Child Homicides below age 16(log)	2.08	1.34	0.00	4.90	546
Incidents: 1 victim 40+(log)	3.05	1.53	0.00	5.80	546
Incidents: all victims 40+(log)	3.20	1.52	0.00	5.93	546
Incidents: any victim 40+ (log)	4.09	1.46	0.00	6.47	546
Post 25-yr Min Imprisonment Law	0.29	0.45	0.00	1.00	546

Notes: *Child Homicides below age 12* refers to total homicide incidents where the victim is below age 12. *Child Homicides below age 16* refers to total homicide incidents where the victim is below age 16. *Post 25-year Minimum Imprisonment Law* is a dummy equal to 1 after the implementation of the minimum 25-year sentence for child sexual assault. *Incidents: 1 victim 40+* counts the number of incidents with at least one victim above age 40. *Incidents: all victims 40+* counts the number of incidents with all victims above age 40. *Incidents: any victim 40+ (log)* refers to incidents with any victim above age 40.

Table 5: Spousal and Intimate Homicide Responses of Spousal Rape Introduction

	(1) Spousal Homicides	(2) Intimate Homicides
Post Full Abolition Marital Rape Exempt.	0.188*	0.194**
	(0.0952)	(0.0921)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	1880	1880
R^2	0.854	0.894

Notes: *Spousal Homicides* stands for the number of victims murdered by a legal spouse (in logarithm). *Intimate Homicides* stands for the number of victims murdered by a legal spouse, former spouse, cohabiting partner, or ex-partner (in logarithm). *Post Full Abolition Marital Rape Exempt.* is a dummy equal to 1 after the full removal of the marital rape exemption.

Standard errors are clustered at the state level. The specification is a traditional DiD TWFE. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Doubly-Robust DiD Impact of Spousal Rape Introduction

	(1) Spousal Homicides	(2) Intimate Homicides
ATT	0.235**	0.308***
	(0.1033)	(0.1108)
N	616	616

Notes: *Spousal Homicides* stands for the number of victims murdered by a legal spouse (in logarithm). *Intimate Homicides* stands for the number of victims murdered by a legal spouse, former spouse, cohabiting partner, or ex-partner (in logarithm).

ATT denotes the Average Treatment Effect on the Treated, estimated using the doubly-robust IPW estimator combining inverse probability weighting with OLS (dripw).

Standard errors are clustered at the state level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 7: DID Estimator on Child Homicide

	(1)	(2)
	Below 12	Below 16
Min 25 years imprisonment	0.598**	0.529**
	(0.252)	(0.250)
Observations	546	546
R-squared	0.765	0.768

Notes: Standard errors are clustered at the state level with wild bootstrap corrections applied following the small-sample procedure used in the child-sample analysis. *Child Homicides* stands for homicides of children (in logarithm), defined as murder or non-negligent manslaughter where the victim falls below the relevant statutory age threshold (under 12 or under 16). The specification is a traditional DiD TWFE.

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 8: DID estimator on Child Homicide (Stacked Design)

	(1)	(2)
	Below 12	Below 16
Min 25 years imprisonment	0.586***	0.515**
	(0.199)	(0.197)
Observations	1084	1084
R-squared	0.733	0.739

Notes: Standard errors are clustered at the state level with wild bootstrap corrections applied following the small-sample procedure used in the child-sample analysis.

Child Homicides stands for homicides of children (in logarithm), defined as murder or non-negligent manslaughter with victims below the age cutoff. Estimates rely on the stacked event-study design following Cengiz et al. (2019) and Borusyak, Jaravel, and Spiess (2024).

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 9: log Effects of Introduction of Spousal Rape - Death Penalty Heterogeneity

	(1) Spousal Hom.- Death (D)	(2) Spousal Hom. - No D	(3) Int. Hom.- Death (D)	(4) Int. Hom. - No D
Treatment	0.100	0.259*	0.151	0.223
	(0.1318)	(0.1474)	(0.1332)	(0.1407)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	1045	835	1045	835
R^2	0.864	0.818	0.890	0.889

Notes: Standard errors are clustered at the state level.

Child Homicides stands for homicides of children (in logarithm), defined as murder or non-negligent manslaughter with victims below age 12 or 16. The specification is a traditional DiD TWFE in two sub-samples. D stands for death penalty.

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 10: log Effects of Introduction of Min. 25 Yrs Sentencing - Death Penalty Heterogeneity

	Death Penalty		No Death Penalty	
	Below 12	Below 16	Below 12	Below 16
Min 25 years imprisonment	0.231	0.161	1.047***	0.988***
	(0.4094)	(0.4218)	(0.2958)	(0.2451)
R^2	0.776	0.779	0.789	0.794
Observations	264	264	282	282

Notes: Standard errors are clustered at the state level, with wild bootstrap corrections applied following the small-sample procedure used in the child-sample analysis.

Child Homicides stands for homicides of children (in logarithm), defined as murder or non-negligent manslaughter with victims below age 12 or 16. The specification is a traditional DiD TWFE in two subsamples.

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 11: Placebo Spousal Sample - Other Murder Victims

	(1) Non-Intimate	(2) Siblings	(3) Strangers
Post Full Abolition Marital Rape Exempt.	0.0991	-0.0391	0.147
	(0.0735)	(0.0776)	(0.1162)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	1880	1880	1880
R^2	0.941	0.753	0.893

Notes: Standard errors are clustered at the state level.

Spousal Homicides stands for murders committed by a legal spouse (in logarithm). *Intimate Homicides* stands for murders committed by a legal spouse, former spouse, cohabiting partner, or ex-partner (in logarithm). *Post Full Abolition Marital Rape Exempt.* is a dummy equal to 1 after the exemption is abolished.

Outcomes include *Non-Intimate Murders*, *Sibling Murders*, and *Stranger Murders*, which should not be affected by the reform.

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 12: Effects of Introduction of Min. 25 Yrs Sentencing on Adult Homicide

	1 victim above 40	All victims above 40	Any victim above 40
Min 25 years imprisonment	0.1888	0.2812	0.0745
	(0.3290)	(0.3128)	(0.2953)
Observations	546	546	546
R-squared	0.757	0.759	0.747

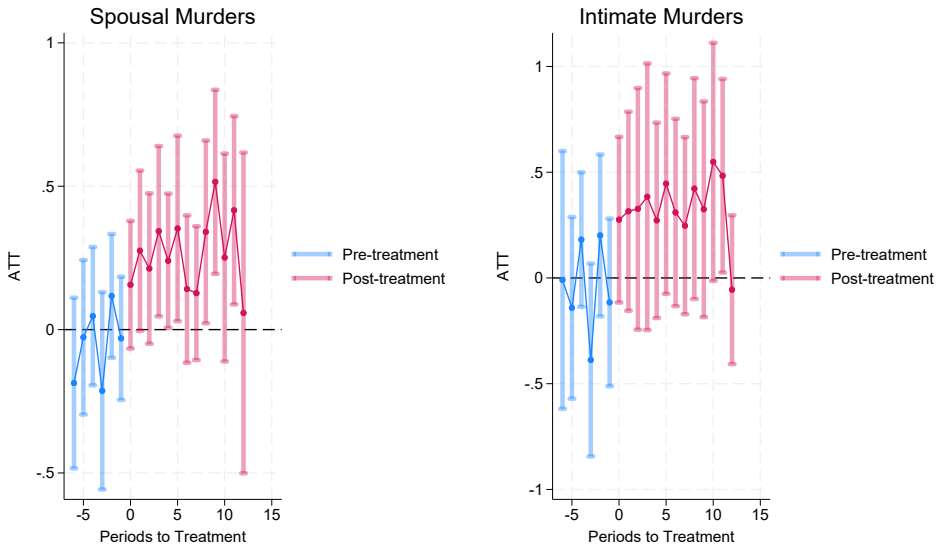
Notes: Standard errors are clustered at the state level with wild bootstrap corrections applied following the small-sample procedure used in the child-sample analysis.

Placebo outcomes include *Incidents: 1 victim 40+*, *Incidents: all victims 40+*, and *Incidents: any victim 40+*, all in logarithm. These outcomes should not be affected by the minimum 25-year sentencing for child sexual assault.

Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Figures

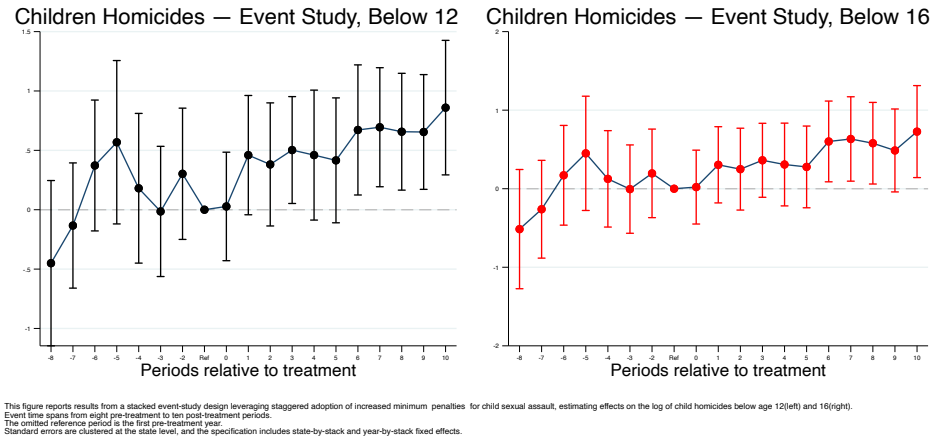
Figure 1: Pre-trends



Notes: *Spousal Homicides* stands for murders committed by a legal spouse (in logarithm). *Intimate Homicides* stands for murders committed by a legal spouse, former spouse, cohabiting partner, or ex-partner (in logarithm). *Post Full Abolition Marital Rape Exempt.* is a dummy equal to 1 after the complete removal of the marital rape exemption.

Each point represents the yearly event-study estimate of the *ATT*, obtained using the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2020). Standard errors are clustered at the state level.

Figure 2: Pre-trends



Notes: *Child Homicides* stands for murders of children (in logarithm), defined as homicide or non-negligent manslaughter where the victim is below the relevant statutory age cutoff.

Each point represents the yearly event-study estimate of the ATT, obtained using the stacked difference-in-differences design following Cengiz et al. (2019) and Borusyak, Jaravel, and Spiess (2024). Standard errors are clustered at the state level, with wild bootstrap corrections applied following the small-sample procedure used in the child-sample analysis.

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Online Appendix: Data Sources

Table A.1: Data Sources

Variable Name	**Source**
Spousal Homicides	FBI Supplementary Homicide Reports (SHR)
Intimate Partner Homicides	FBI Supplementary Homicide Reports (SHR)
Child Homicides (Under 12, Under 16)	FBI National Incident-Based Reporting System (NIBRS)
Victim–Offender Relationship	SHR (adult sample); NIBRS (child sample)
Placebo Outcomes (Adult Sample, SHR)	
Sibling Homicides	FBI Supplementary Homicide Reports (SHR)
Stranger Homicides	FBI Supplementary Homicide Reports (SHR)
Non-Intimate, Non-Family Homicides	FBI Supplementary Homicide Reports (SHR)
Placebo Outcomes (Child Sample, NIBRS)	
Adult Homicides (1 victim 40+)	FBI National Incident-Based Reporting System (NIBRS)
Adult Homicides (All victims above 40)	FBI National Incident-Based Reporting System (NIBRS)
Adult Homicides (Any victim above 40)	FBI National Incident-Based Reporting System (NIBRS)
Marital Rape Exemption Repeal	State statutes; McMahon-Howard, Clay-Warner and Renzulli (2009)
Mandatory Min. Penalties for Child Abuse	State statutes
Death Penalty States	State statutes