THREE EMPIRICAL ESSAYS ON CRIME

by

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A Dissertation
Submitted to the
Graduate Faculty
of
George Mason University
in Partial Fulfillment of
The Requirements for the Degree
of
Doctor of Philosophy
Economics

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ABSTRACT

THREE EMPIRICAL ESSAYS ON CRIME

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US Federal and State laws aimed at combating sex crimes have evolved significantly and become increasingly severe over the past 30 years. These include federal laws mandating that states create online registries for tracking and notifying the public of the addresses and identities of sex offenders and state laws imposing mandatory minimum sentences for sex crimes. In this dissertation I analyze these laws and their effects from an economic perspective.

Chapter One uses updated data and new statistical methods to analyze the effectiveness of sex offender registration and notification in controlling sex crimes, focusing on the impact of new policies, including the 2006 Adam Walsh Act (AWA). I find evidence that sex offender registration and notification have a lagged negative effect on sex crime rates. However, I also find pitfalls of sex offender registration and notification, including evidence that registrants substitute strangers for victims known to them and the

conviction-based approach to determining inclusion in registries mandated by the AWA has the perverse effect of making registries less effective at controlling sex crimes.

Chapter Two analyzes the effectiveness of Jessica's Law, a state law establishing mandatory minimum sentences for child sexual assault and electronic monitoring of sex offenders enacted in different states at different times. I find no support for the law's effectiveness in controlling sex crimes but find evidence that the age thresholds for the imposition of the mandatory minimum sentences caused offenders to substitute adults for child victims.

Chapter Three provides evidence that the severity of sex crime laws is negatively correlated with the level of treatment provided to sex offenders across states and examines the econometric implications of this negative relationship and the economic and political factors causing policymakers to prefer either treatment or more severe crime laws in combating crime.

CHAPTER 1 A REEXAMINATION OF THE EFFECTIVENESS OF SEX OFFENDER REGISTRATION AND NOTIFICATION LAWS: THE IMPACT OF NEW POLICIES AND REGISTRY EXPANSION

Federal and State sex crime laws have become increasingly stringent over the past 30 years. The Jacob Wetterling Act of 1994 required states to implement sex offender registries to track convicted sex offenders and information on them.[1] Megan's Law, a 1996 amendment to the Wetterling Act, required states to make information on registered sex offenders available to the public. While states originally had discretion in how they met this notification requirement and could use flyers, newspapers,[2] and/or community meetings to do so (Agan 2011), the 2003 PROTECT Act amendments to the Wetterling Act mandated that states develop internet sex offender registries (Levenson and D'Amora 2007, p.8).[3]

The 2006 federal Adam Walsh Act (AWA) required states to impose more stringent registration and notification requirements, resulting in a significant increase in the size of sex offender registries in compliant states. Although it was enacted in 2006, most states are still not compliant with the AWA and those that are became compliant at different times between 2009 and 2017. I exploit this variation in when and if states

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[1] The Wetterling Act imposed a financial penalty in the form of reduced federal criminal justice funding for non-compliance (Filler 2001, p.316).
[2] Texas, for example, used newspapers to publish information on local sex offenders before implementation of its internet registry in 1998 (Dittrick 1996).
[3] PROTECT stands for Prosecutorial Remedies and Other Tools to End the Exploitation of Children Today (Levenson and D'Amora 2007, p.8-9).

became AWA-compliant to examine the effect of the AWA and the expansion of registry size in the states that implemented it on sex crime incidence.

Given recent criticism of using two-way fixed effect (TWFE) models to test the impact of staggered interventions (Goodman-Bacon 2021), I utilize new statistical methods, including the Callaway and Sant'Anna (CS) estimator (Callaway and Sant'Anna 2021) to assess the impact of the AWA. I also analyze the initial registration and notification requirements required by the Wetterling Act and Megan's Law using updated data and the CS estimator. Part 4 of this chapter focuses on this analysis of the effectiveness of the Wetterling Act and Megan's Law and Part 5 focuses specifically on the impacts of the AWA, while Parts 1,2, and 3 provide background information, a literature review, and my research design.

Part 1 Background

The stated purpose of the AWA is to "to protect children from sexual exploitation and violent crime" and "prevent child abuse and child pornography" (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006]). The act includes 7 titles, and Title 1, the Sex Offender Registration and Notification Act (SORNA), contains the sex offender registration and notification requirements that states are required to implement and is the focus of this study. As such, SORNA will be used to designate these requirements for the remainder of this chapter. Title II of the AWA strengthens laws against and imposes mandatory minimum sentences for federal sex crime offenses,[4] Title III authorizes the civil commitment by the federal government of offenders considered to be "sexually dangerous" after they have completed their prison sentence,[5] while the remaining titles include studies, grants, and other measures aimed at protecting children from sex offenders and combating child pornography.

SORNA strengthened sex offender registration and notification requirements in multiple ways. It established 3 Tiers to categorize different types of sex offenders based on the crime that they commit and designated registration and notification requirements associated with each tier.[6] As such, SORNA mandated a conviction-based approach to determining sex offender registration and notification requirements, whereas states previously had more discretion with regard to which sex offenders they included on their registries and many used a risk- assessment based approach that considered "factors such as treatment completion" (Stenehjem 2012), prior criminal convictions, age, number of prior sex offenses, "indicators of psychopathy and deviant sexual arousal" (Harris, Lobanov-Rostovsky, and Levenson 2010, p.505), prison discipline history, offense-related sexual interests, access to victims, relationship with past victims, drug and alcohol abstinence, and employment stability.[7] This transition resulted in an increase in registry

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[4] Most sex crimes are handled within the jurisdiction in which they occur and not charged as federal crimes. They can be charged as federal crimes if they involve the transport of a victim across state lines. Sex trafficking, child pornography, and, in some cases, kidnapping are often charged as federal crimes.

[5] 20 states, starting in 1990, have enacted similar laws. These laws are broadly known as "Sexually Violent Predator" laws and discussed in more detail in Chapter 3. See Barker (2009, p. 143) and Krauss et al. (2015).

[6] For example, forcible fondling of a child under 13 and forcible rape are Tier III sex offenses requiring lifetime sex offender registration, while non-consensual fondling of a minor at least 13 years old is a Tier II sex offense requiring registering as a sex offender for 25 years.

[7] As of 2008, almost all states conducted some form of actuarial risk assessment on sex offenders to determine their risk of re-offending (Daly 2008, p.14). As of 2003, slightly over half (Janus 2006, p.66; Logan 2003, p.340) and, as of 2007, approximately half of states (Levenson and D'Amora 2007, p.8; Freeman and Sandler 2010, p.34) used risk assessment to determine registration and notification

size in SORNA-compliant states with the deputy director of Wyoming's Criminal Justice Information Services attributing to it a 1,060% in the size of the state's registry (Grinberg 2011). SORNA further contributed to this expansion by mandating that states keep registrants on their registries for, in case of Tier II offenders, 25 years, and, in case of Tier III offenders, their entire lives. In Ohio and Oklahoma, registrants categorized as low risk were shifted to higher tier levels previously reserved for only high-risk offenders to comply with SORNA's conviction-based approach, resulting in an "upward realignment of the registered population from lower into higher tiers" (Harris, Lobanov-Rostovsky, and Levenson 2010, p.514). States opposing SORNA, including North Dakota, argued that their risk-based approach to determining registration requirements was superior to the SORNA Tier-based approach and refused to comply on that basis (Stenehjem 2012).

SORNA also expanded registration requirements and registry size by controversially mandating sex offender registration for juveniles[8] and those convicted of sex offenses before registration requirements existed.[9] The retroactive registration

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requirements. Prior to SORNA and in many states that have not implemented SORNA's conviction- based approach, offenders' risk assessment score helps determine registration and/or notification requirements. For example, North Dakota bases registration requirements on the offender's risk assessment score. Other states, like Oregon, use risk assessments to determine notification requirements with only high-risk offenders appearing on the state's public registry. In California, an offender's risk assessment score helps determine whether s/he can apply to be excluded from the state's online registry.
See also McGrath, Lasher, and Cumming (2011), and Janus (2006, p. 56).

[8] Specifically, SORNA requires that juveniles at least 14 years old register if they were convicted of an offense equally or more severe than aggravated sexual assault. SORNA does provide some leniency for juvenile offenders in that, in case of a Tier III offense, the length of their registration requirement can be reduced from lifetime to 25 years if they maintain a clean record. Originally, SORNA required that States include juveniles on online sex offender registries but later guidance in 2010 removed this requirement (Supplemental Guidelines for Sex Offender Registration and Notification, 75 Fed. Reg. [May 14, 2010]).

[9] Some states already required retroactive sex offender registration prior to SORNA though it was not mandated by the federal government. The retroactive registration requirement in SORNA applies to convicted sex offenders who are in prison but committed a sex crime prior to the enactment of the

requirement in SORNA has been challenged in court for violating the ex post facto clause in the constitution, which prohibits laws changing the legal consequences of crimes of crimes committed before the law was enacted.[10] The lifetime registration requirement for juveniles adjudicated delinquent for a Tier III sex offense has also been challenged in courts in Ohio and Pennsylvania for violating due process by assuming that juveniles "will commit some sex crime in the future without giving" them "the opportunity to challenge that assumption" and constituting cruel and unusual punishment (Kelley 2018). These legal challenges resulted in delays to SORNA implementation in some states. For example, a federal court enjoined Nevada from enacting a law designed to implement SORNA mandating retroactive registration of juvenile sex offenders.[11] Additional ways in which SORNA expanded registration requirements include establishing a 1 year minimum sentence for sex offenders failing to comply with registration requirements and specifying both the information that needs to be collected during registration, which includes a DNA sample of the sex offender, and the frequency with which sex offenders need to appear in person to verify the information in the registry that they are included in.

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registration requirement or those who have been released from prison but reenter the legal system by committing a subsequent crime even if that crime is not a sex crime. The retroactive registration requirement was later modified in 2010 to apply only to those who reenter the legal system by committing a felony as DOJ determined it would be a burden for states to have to track the criminal history of everyone convicted of a misdemeanor to determine if they ever committed a sex crime (Supplemental Guidelines for Sex Offender Registration and Notification, 75 Fed. Reg. [May 14, 2010]).

[10] Court rulings on whether retroactive sex offender registration violates the ex post facto clause have differed. In 2003 the Supreme Court ruled in Smith vs. Doe that it did not because sex offender registration is not "punitive" in nature. Subsequent court rulings have found that retroactive registration is unconstitutional but the US Supreme Court has not yet reexamined the issue and it has not fully been settled.
See Sex Offender Registration and Notification in the United States Current Case Law and Issues (March 2018).

[11] However, this injunction was lifted in 2018.

SORNA also broadened the definition of sex offense, thereby expanding the types of offenses requiring sex offender registration. It defines a "sex offense" as "a criminal offense that has an element involving a sexual act or sexual contact with another" or "an attempt or conspiracy to commit" such an offense (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006]). This definition covers crimes, such as prostitution, that were not subject to sex offender registration in the past but now are in some states. SORNA further broadened the definition of sex offense to include certain specified offenses against minors that are nonsexual in nature, including nonparental kidnapping even if the kidnapping is not for sexual purposes and false imprisonment by anyone other than a parent or guardian.

SORNA expanded sex offender notification as well as registration requirements. It established an online National Sex Offender Registry including every sex offender on each state's online registry.[12] While many states have included only a subset of registrants or "high risk" offenders on their public registries,[13] SORNA mandates that all registrants be included on the online registries with the exception of juveniles and Tier I offenders whose offense was not against a minor. Given these requirements, states face significant costs in implementing SORNA and many have chosen not to comply because the cost of implementation exceeds the financial penalty that they face for failing to

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[12] The National Sex Offender Public Website (NSOPW), available at www.NSOPW.gov, had already been required by the 2003 PROTECT Act and initially established in 2005 but was renamed the Dru Sjodin National Sex Offender Public Website by the AWA. See "S.151 - 108th Congress (2003-2004): PROTECT Act" at Congress.gov and "Legislative History of Federal Sex Offender Registration and Notification" at smart.ojp.gov.

[13] States have determined which registrants to include on the public registry based on risk assessment, date of conviction, whether their victim was an adult or minor, whether they were convicted of a felony or misdemeanor, and other factors. See Count Analysis of US Registries (2008).

implement SORNA.[14] By imposing these federal requirements on states, SORNA contributes to the increasing federalization of sex crime laws in a trend that started with the passage of the Wetterling Act and Megan's Law (Wright 2008).

Originally, states were required to implement SORNA by July 27, 2009 but that deadline was extended when states did not meet it. A new office within the Department of Justice (DOJ), the Office of Sex Offender Sentencing, Monitoring, Apprehending, Registering, and Tracking (SMART), was created to administer implementation. SMART has been monitoring the progress of US states, territories, and federally recognized Indian tribes[15] in implementing SORNA and, based on a review of information provided by the states, determining when and if states have substantially implemented SORNA. Ohio was the first state that SMART determined had substantially implemented SORNA in September 2009 and, as of December 2021, SMART has determined that 18 states are SORNA-compliant. Significant resources have been spent to make more states and territories compliant with SORNA. The Office of Justice Program (OJP) awarding $16 million in grants to improve compliance in December 2021 (SMART 2021). Figure 1 shows when and which states have implemented SORNA.[16]

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[14] Specifically, federal crime control assistance provided as a grant to the states is reduced for states that fail to implement SORNA. As an example, studies in Texas estimated that compliance with SORNA would cost $38.7 Million while failure to comply would result in losing $1.4 Million in this federal assistance. See Gunnarsson (2011).

[15] SORNA expands sex offender registration requirements to federally recognized Indian tribes but allows the tribes to decide whether to create their own registry or have sex offenders in their tribe included in the registry of the state in which the tribe resides.

[16] Due to a lag in the review process and other reasons discussed in Part 3, the year when states implemented SORNA in some cases preceded the year when SMART determined that they had implemented SORNA.

As shown in Figure 2, a preliminary review of the data on sex crime rates and the size of sex offender registries across states shows a steady and significant increase in state registry size since 2003 and no consistent trend in sex crime rates. Specifically, as of 2016,[17] average state registry size increased by 206% since 1998 and 64% since 2003 while sex crime rates increased by 9.7% since 2003 and have fluctuated significantly.[18] The relationship between registry size and sex crime rates is bidirectional in that registries could expand because more sex crimes are being committed but their expansion could also cause sex crime rates to decrease since expanding registries enable more monitoring of sex offenders by the police. Figure 2 shows neither of these effects, but it is possible both are operating at once and this chapter focuses on better understanding the relationship between sex offender registries and sex crime rates.

[17] The latest date for which the NIBRS data used to calculate sex crime rates was available.

[18] These calculations are based on data only for those states that reported to NIBRS. The crimes included as sex crimes in the chart are rape, sodomy, sexual assault with an object, fondling, incest, statutory rape, and kidnapping of a minor unless that kidnapping was by a family member. Registry size is interpolated for 2003 and 2004 using linear interpolation due to non-availability of data for 2002-2004.

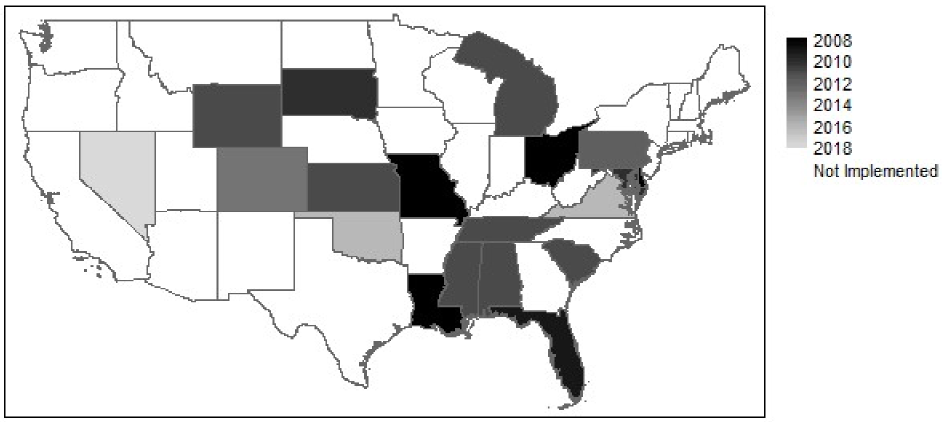

Figure 1 SORNA Implementation Status of US States

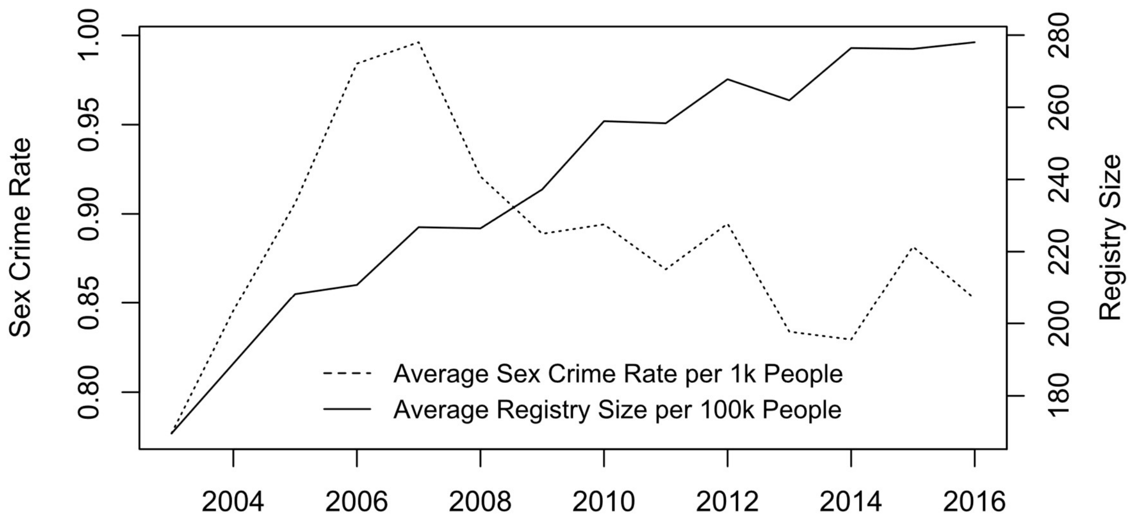

Figure 2 Sex Crime Rates and Registry Size from 2003 to 2016

Part 2 Literature Review

The two most comprehensive studies of the impact of sex offender registration and notification laws on sex crime rates are Agan (2011) and Prescott and Rockoff (2011). These papers focus on the effects of the registration and notification requirements established in the Wetterling Act and Megan's Law on sex crime incidence.

Agan exploits the fact that states implemented their sex offender registries at different times following and, in some cases, before enactment of the Wetterling Act, to use a TWFE regression model to evaluate the impact of sex offender registration on sex crime rates. She uses data from the FBI's Uniform Crime Reporting (UCR) Summary Reporting System (SRS), which all states report to and includes data on sex offense arrests but, except in case of forcible rape, not sex crime incidents. She also exploits timing differences in when states posted their registries on the internet to evaluate the impact of publicly available registries on sex crime rates. She does not find convincing evidence for the effectiveness of sex offender registration and notification laws. Her only statistically significant result is that the availability of sex offender registries on the internet is associated with a 17% decrease in sex crime arrest rates, but this result is only significant at the .1 level. She also uses Bureau of Justice Statistics (BJS) data on prisoners released from state prisons in 1994 to examine the effect of sex offender registration on recidivism and finds that registration does not reduce recidivism.

Prescott and Rockoff find more evidence for the effectiveness of sex offender registration, though they also find that notification laws are potentially counterproductive. They also exploit timing differences in when states implemented their registries to use a

TWFE model to evaluate their effectiveness but use different crime data than Agan. The National Incident Based Reporting System (NIBRS) data that Prescott and Rockoff use includes more detailed information about specific crime incidents and, unlike the UCR SRS, incidence data for sex offenses other than rape but only includes data from a subset of states since, as of 2016, only 39 states report to NIBRS and reporting from many of those states only includes data from a subset of law enforcement agencies.

Prescott and Rockoff separate the effects of registration and notification on deterrence and recidivism by including interaction terms between state registry size and dummy variables indicating a state's implementation of sex offender registration and notification. They hypothesize that registration and notification could affect sex crime rates by both deterring prospective offenders facing the possibility of being placed on a registry from committing sex crimes and reducing recidivism through increased monitoring of registrants, and the magnitude and even direction of these effects might not be the same. They argue that the deterrent effect of registration will be invariant to registry size, while the effect of registration and notification on recidivism, in contrast, will only be realized once sex offenders are on the registry and thereby subject to increased monitoring and will have more of an impact as more sex offenders register. The effect on recidivism is therefore captured by the interaction terms between registry size and registration and notification, while the deterrent effect is captured by the coefficients on the standalone registration and notification dummy variables.

Based on this methodology, they find that registration reduces recidivism with a resulting decrease in reported sex offenses by 1.07% for each additional offender registered per 10,000 people in the population but has no effect on deterrence (Prescott and Rockoff 2011, p.181). As such, they argue that the effectiveness of sex offender registries depends on their size. States with larger registries are likely to realize reductions in recidivism as a result of enhanced monitoring while sex offender registration is likely to have little impact in states with small registries where few offenders are subject to that monitoring. Notification, on the other hand, deters unregistered sex offenders, reducing crime frequency by 12.8% through deterrence but has the perverse effect of increasing recidivism and will be most effective in states with small publicly available registries. Their intuitive explanation is that the enhanced monitoring enabled by registries is effective for controlling recidivism but has no deterrent effect, while the social ostracism resulting from notification has the perverse consequence of reducing the incentive of sex offenders to restrain their impulses and become law-abiding citizens in a society where they have already become outcasts. In light of these results, Prescott and Rockoff's policy recommendation is that states have comprehensive sex offender registries but only subject a small subset of registrants to public notification (Prescott and Rockoff 2011, p.182). Based on these findings, the expansion of registry size resulting from SORNA expansion should reduce sex offender recidivism. SORNA's expansion of notification requirements is, however, not consistent with their recommendation and could counteract the predicted negative effect of registry expansion.

Prescott and Rockoff further analyze whether registration and notification laws are more effective at reducing sex crimes against family members, acquaintances, and neighbors than against strangers. In case of notification laws, this result is plausible because potential local victims who know or live near registrants are more likely to be notified of their presence on the registry than strangers or potential victims in other neighborhoods.[19] In an article criticizing Megan's Law, Prentky (1996, p.295) further suggests that notification laws could increase sex crimes against strangers through displacement of victims by causing registrants to substitute strangers for local victims since they will be more easily able to target strangers in adjacent communities who are not aware of their status as a sex offender than neighbors or acquaintances. If accurate, this hypothesis could help explain why sex offender recidivists are statistically more likely to target strangers than first time offenders (Duwe, Donnay, & Tewksbury 2008, p.492).[20] However, Prentky provides no empirical evidence for his argument and Prescott and Rockoff's results do not support it. They find that notification laws increase recidivism against neighbors and acquaintances as well as strangers, while registration, in contrast, is more effective at decreasing recidivism against neighbors and acquaintances than strangers but does not cause offenders to substitute strangers for local victims. Their intuitive explanation for this latter finding is that police will be better able to monitor registrants around their families and neighbors than around strangers and have an easier time locating them "when a nearby crime occurs" (Prescott and Rockoff 2011, p.184).

Other studies on the effects of notification laws do not support Prescott and Rockoff's finding that they increase recidivism. Zevitz (2006) and Schram and Milloy

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[19] This is true for multiple reasons further discussed in Part 5.

[20] Another likely explanation is that repeat offenders are more likely to be pathological offenders, which would make them more likely to seek out victims who they do not already have a relationship with.

(1995) use matching to compare the recidivism rates of offenders who were subject to community notification to those of similar offenders who were not[21] in Wisconsin and Washington State, respectively. Neither study finds that notification laws have a statistically significant effect on sex offender recidivism. In contrast, Duwe and Donnay (2008), who use a similar matching design, and Barnoski (2005) find evidence that notification laws reduce sex offender recidivism using data for Washington State and Minnesota, respectively.

The fact that these two studies found notification laws to be effective and focused on states that used an actuarial risk assessment[22] to determine the level of notification to subject offenders to[23] points to the importance of the criteria for determining notification requirements (Lasher and McGrath 2012, p.20). Community notification could both increase recidivism by making it harder for offenders to reintegrate into society, as reflected in Prescott and Rockoff's study, and decrease recidivism by making it harder for offenders to recidivate as a result of the community being more informed about the risk that they pose. We would expect the former effect to dominate in the case of low-risk offenders who are not likely to recidivate if they successfully reintegrate and the latter effect to dominate for high-risk offenders who may be unable to control their propensity

[21] Due to being released from prison before the notification law was implemented in case of Schram and Milloy (1995) and because of discretion by law enforcement in who to subject to notification in case of Zevitz (2006).
[22] Specifically, both states used the Minnesota Sex Offender Screening Tool (MnSOST), a sex offender risk assessment tool developed in Minnesota that has been used in other states as well (Daly 2008, p. 10), at the time of Barnoski and Duwe and Donnay's studies.
[23] Although both Barnoski (2005) and Schram and Milloy (1995) use data for Washington State, Barnoski uses more recent data from the period following Washington State's 1997 modification of its notification law, which revised the criteria for determining the level of notification that offenders are subject to by basing it on an actuarial risk assessment score.

for crime (Lasher and McGrath 2012, p.20; Andrews and Borta 2006). Notification requirements are therefore likely to be most effective when they are based on an accurate assessment of recidivism risk, making the criteria for determining them important.

The change in that criteria imposed by SORNA is therefore potentially consequential. Two studies evaluate whether the conviction-based criteria imposed by SORNA accurately predicts whether offenders will recidivate. Zgoba, Miner, Levenson, Knight, Letourneau, and Thornton (2016) determine the SORNA tier that offenders released in four states would have been placed in had SORNA been implemented in those states at the time of their release. They then use the most widely used risk assessment instrument for sex offenders[24] to calculate the actuarial risk assessment score for each released offender and determine whether SORNA classification tier, actuarial risk assessment score, or the tier systems existing in the four states when the offenders were released best predicts the actual recidivism of the previously released offenders. They find that actuarial risk assessment scores and the existing classification tiers[25] outperformed SORNA classification tier with SORNA Tier 2 offenders more likely to recidivate than Tier 3 offenders. Freeman and Sandler (2010) use a similar research design and obtain consistent results using data on sex offenders released in New York State.[26] The SORNA classification system's poor results in predicting the risks posed by

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[24] The Static-99R, a revised version of the STATIC-99. See Reeves et. al. (2018, p. 890) for more on the Static-99 and Static-99R.

[25] In the case of 2 of the 4 states (Minnesota and New Jersey), the existing tier classification system was based at least partially on sex offender risk assessment factors and/or actuarial risk assessment score (Zgoba, Miner, Levenson, Knight, Letourneau, and Thornton 2016, p. 724-725).

[26] They find that previously released offenders who would have been classified as Tier 1 based on the SORNA classification system had higher rates of recidivism than those who would have been classified as Tier 2 or 3 (Freeman and Sandler, 2010).

released sex offenders[27] suggests that SORNA implementation could impact the effectiveness of sex offender registries, which is one of the hypotheses that I test in this chapter.

Part 3 Methodology

Part 4 of this chapter analyzes the effects of the creation of sex offender registries and the posting of those registries on the internet using updated data and new statistical methods. The hypothesis that I test is that 1) The creation of State Sex Offender Registries and the inclusion of those registries on the internet decreased sex crime incidence. Part 5 of this chapter evaluates the effects of SORNA. The hypotheses that I test are that 1) SORNA implementation leads to an expansion of registry size, 2) SORNA implementation decreases the frequency of sex crimes as a result of this expansion and the sex offender registration and notification requirements that it imposes, and 3) the effect of registry expansion on sex crime incidence will be different in SORNA-compliant and non-compliant states as a result of the difference in their criteria for who is included on their registries. I make no prediction as to whether the conviction or risk-based criteria is a better indicator of the threat posed by prospective registrants. In addition to providing enhanced monitoring, sex offender registration and notification and registry size expansion could reduce sex crime incidence by alerting prospective victims or their parents of offenders in their neighborhood so they can take proper precautions and alert police if they see suspicious behavior (Bierie 2015, p.4).

[27] One potential reason for its unreliability is the fact that plea bargains are often used in sex crime cases, meaning that the crime that an offender is convicted of may not be reflective of or as serious as the crime s/he committed (Lasher and McGrath 2012, p.21).

Registry size expansion may have differing effects in SORNA-compliant and non-compliant states because SORNA mandates a conviction-based criteria for determining inclusion on the registry while many states use a risk-based criteria. If risk assessment scores are a better indicator of the danger posed by an offender than the crime that the offender was convicted of, as suggested by Sandler and Freeman's (2010) and Zgoba, Miner, Levenson, Knight, Letourneau, and Thornton's (2016) studies, registry expansion may be more efficacious in non-compliant than SORNA-compliant states. If the conviction-based criteria is a better indicator, we would expect the reverse. The criteria that states use to determine inclusion on registries is likely to be as if not more important than registry size. SORNA implementation across states enables me to analyze the impact of a change in that criteria.

I incorporate new statistical methods into my analysis because the use of TWFE regressions to evaluate the effects of sex offender registration and notification requirements is problematic. While the canonical "two group/two period" difference in difference model that controls for changes over time affecting both groups equally and differences between groups that are not impacted by time is sound, extensions of that model to interventions implemented at different times across multiple groups, as in the case of the implementation of federal sex offender registration and notification requirements across states, present methodological problems. Andrew Goodman-Bacon demonstrates that the estimate of the effect of an intervention derived from a TWFE difference in difference (DD) estimator is a weighted average of "all possible two group/two period" (2x2) difference in difference "estimators in the data" (Goodman-

Bacon 2021, p. 2). Problematically, in case of staggered interventions, this includes estimators in which previously treated units whose treatment status does not change in the periods following initial treatment are used as controls. In case of time-varying or lagged treatment effects, the inclusion of such estimators can cause researchers to reach an incorrect conclusion about the effects of an intervention. This problem is most severe when all units are eventually treated, in which case already or not-yet treated units comprise the control group in all of the 2x2 DD estimators that the overall treatment effect is a weighted average of. Consequently, the weight of DD estimators in which previously treated units are controls will tend to be higher than in cases where there is a large number of never-treated units. An analysis of the effectiveness of sex offender registration and notification laws involves a staggered intervention in which all units were eventually treated. To comply with federal requirements, all states eventually created sex offender registries.

Goodman-Bacon also demonstrates that 2x2 DD estimators derived using groups whose treatment status exhibits more variance will be more heavily weighted in calculations of the overall treatment effect. As a result, 2x2 DD estimators derived using groups treated towards the middle of the period under study will arbitrarily be weighted more heavily than those derived using groups treated towards its beginning or end (Goodman-Bacon 2021, p.4).

In response to these findings, Goodman-Bacon provides a methodology to perform a decomposition of TWFE estimates that I incorporate into my analysis. The Bacon Decomposition provides more insight into the 2x2 DD estimates that the overall estimated treatment effect in TWFE models is comprised of by decomposing the overall estimated treatment effect into estimates in which not-yet treated units, already treated, and never treated units comprise the control group[28] and providing the weight and value of each of these three estimates. A high weight on 2x2 DD estimates in which already treated units comprise the control group is problematic in case of time-varying treatment effects. The effect of sex offender registration on sex crime incidence is likely to be time-varying. As Prescott and Rockoff emphasize, their effectiveness is likely to be limited immediately after implementation when they contain few registrants. However, they could become useful as a monitoring tool as more offenders register over time. Given these methodological problems with using TWFE models to analyze the effectiveness of Sex Offender Registration and Notification, I analyze the effects of both SORNA and the initial creation of State Sex Offender Registries and the posting of those registries on the internet using updated data, the Bacon Decomposition, and the Callaway and Sant'Anna (CS) estimator.

The CS estimator is a new method to measure treatment effects in case of staggered interventions created in response to the problems with TWFE regression models. It calculates a separate "group-time average treatment effect" for each group for each post-treatment period with group defined based on when the units in that group first undergo treatment. The group-time-average treatment effect treatment effect is

[28] To implement the Bacon Decomposition, I use the Bacondecomp package available in Stata (2019) and R (2018).

calculated by comparing pre and post-treatment values of the dependent variable for each group of treated units for each post-treatment period according to the specification:

Equation 1

$$ATT(g,t) = E\left(Y_t - Y_{g-1} \middle| G_g = 1\right) - E(Y_t - Y_{g-1}|C = 1)$$

where t is year, Y is the dependent variable, G_g is a dummy variable that equals one for units first treated in year g, and C is dummy variable equaling one for control group units (Callaway and Sant'Anna 2021, p.11). Control group units include never treated units or, in cases where all or almost all units are eventually treated, not-yet treated units. By only including units in these categories as controls, the CS estimator avoids the problematic use of already treated units as controls in TWFE regressions. To address potential pre-treatment differences between treatment and control group units that could cause the parallel trends assumption to be violated, the CS estimator includes three estimation methods for incorporating the pre-treatment value of covariates into the calculation of group-time average treatment effects. For CS models that include pre-treatment levels of covariates, I use outcome regression, but cross-validate my results using the other two methods. Outcome regression and the other two estimation methods are described in more detail in Appendix 1 Part A. As the group-time average treatment effects are of limited interest on their own, Callaway and Sant'Anna also provide methods for aggregating them to form an overall estimate of the average treatment effect on the treated (ATT). I use simple aggregation but cross-validate my results against the other two aggregation methods provided by Callaway and Sant'Anna. These aggregation methods are described in more detail in Appendix 1 Part A.

I control for the effects of state sex crime laws in evaluating the federally-imposed registration and notification requirements. As demonstrated by the correlation matrix in Figure 3, which incorporates data from each of the 50 US states across a range of variables measuring the stringency of the state's sex crime laws and how proactive it has been in implementing federal sex crime laws, states that implemented SORNA are more likely to have adopted stringent measures to combat sex crimes at the state level that, if not controlled for, could bias regression results by affecting state specific trends. Specifically, SORNA-compliant states are more likely to have enacted Jessica's Law, a sex crime law enacted in different states at different times between 2005 and 2014 further discussed in Chapter 2, and have lifetime electronic monitoring (LEM) requirements for sex offenders than non-compliant states. They also are likely to have higher mandatory minimum sentences for sex crimes[29] than non-compliant states. More broadly, states show consistency in the extent to which they are likely to enact stringent sex crime laws and be proactive in adopting federal sex crime legislation.

[29] Minimum sentence refers to the minimum sentence in a respective state for 1st time sexual assault not involving the use of a weapon or deadly force. Due to Jessica's Law, in most states this minimum sentence is higher if the sexual assault victim is under a certain age threshold. As such, except in the case of states that have not enacted Jessica's Law and in which the minimum sentence imposed does not depend on the victim's age, the minimum sentence referred to here is the minimum sentence for a sex crime against a child. In states without mandatory minimum sentences for sex crimes, presumptive sentence guidelines were used to derive the states' minimum sentence. The mandatory minimum sentence is listed as 0 only if no record of a mandatory minimum or presumptive sentence for sexual assault was found.

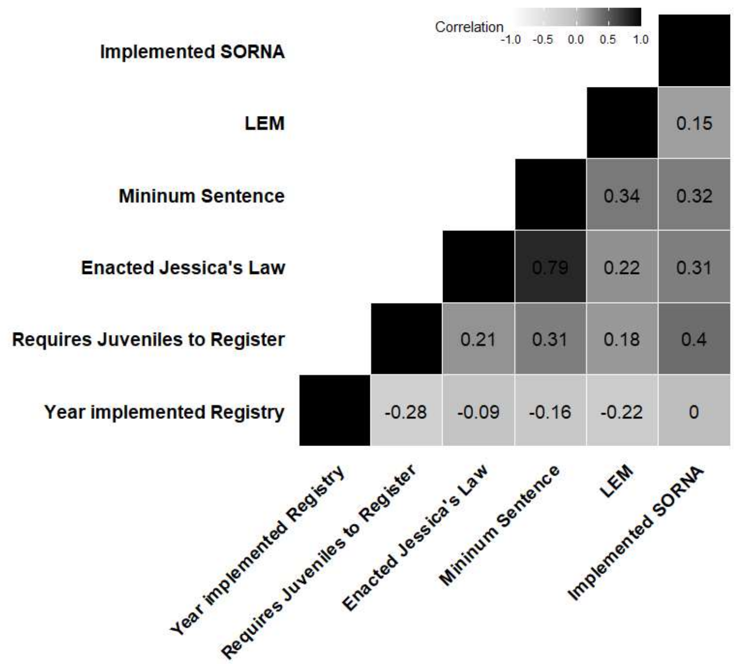

Figure 3 Correlation Matrix of Different Measures of the Stringency of Sex Crime Laws Across States[30]

To test my hypotheses, I use 1985-2018 crime data from the Unified Crime Reporting (UCR) Summary Reporting System (SRS) and 2003-2016 yearly crime data from the National Incident Based Reporting System (NIBRS).

For both the UCR and NIBRS data, I conduct my analysis at the reporting agency level using the Originating Agency Identifier Code (ORI) included in the data. Conducting the analysis at the ORI instead of State level is important because the number of agencies and, thus counties and cities within states, reporting to NIBRS states expanded between 2003 and 2016. The inclusion of additional reporting agencies could

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[30] Data sources for Figure 3 include Prescott and Rockoff (2011), Agan (2011), Davis et al. (2013), Logan (2021), Love (2020), FindLaw.com, the Rape, Abuse and Incest National Network, and state statutes from the sources below.
Virginia's Legislative Information System, Arizona State Legislature, Hawaii State Legislature, Alaska Legal Resource Center, Minnesota State Legislature, Illinois General Assembly, Delaware General Assembly, New York State Assembly, and Pennsylvania General Assembly.

affect state crime rates but my inclusion of ORI fixed effects in the TWFE models controls for any factors unique to a specific reporting agency and the county(ies) or city(ies) that it covers. As fixed effects cannot be used in the CS model, there is no way to control for changes in reporting agencies over time. Therefore, I use the CS estimator on both my full unbalanced UCR and NIBRS panels and on balanced panel subsets of my data and note any differences in results.[31] To calculate rates, I divide the total number of sex crime incidents[32] or, in case of the UCR data, forcible rapes and sex crime arrests in the area covered by a respective ORI in a given year by the population of that area and multiply by 1000. My NIBRS dataset contains 2003-2016 data for 7,650 reporting agencies across 40 states while my UCR dataset on forcible rape contains 1985-2018 data for 24,103 reporting agencies across 50 states and DC and my UCR dataset on sex offense arrests contains 1985-2018 data for 23,829 reporting agencies across 50 states and DC.[33]

My data on the size of state sex offender registries is from a combination of the Bureau of Justice Statistics, which provided this data only for 1998 and 2001 (Adams 2002), and the nonprofit organizations the National Center for Missing and Exploited

[31] I also do not include any observations based on less than 12 months of reporting when using the CS method. In some cases, agencies fail to report their crime statistics to NIBRS or UCR every month. In my TWFE model I control for reporting frequency but time-varying covariates cannot be used with the CS method.

[32] More specifically, I use the total number of sex crime offenses to calculate rates since an "incident," as defined by NIBRS, can involve multiple offenses as long as they were "committed by the same offender, or group of offenders acting in concert, at the same time and place" (National Incident Based Reporting System Resource Guide 2021).

[33] I excluded agencies whose jurisdiction does not have a population associated with it from my analysis due to the impossibility of calculating rates. This is the case, for example, for "national parks, colleges and universities, tall bridges and tunnels, and most state police departments" (National Incident Based Reporting System Resource Guide 2021).

Children (NCMEC), and Parents for Megan's Law. Data on when and if Jessica's Law was enacted across states is from a review of a combination of news articles, press releases, and state statutes,[34] while data on when and if states implemented SORNA is primarily from substantial implementation reports issued by the Office of Sex Offender Sentencing, Monitoring, Apprehending, Registering, and Tracking (SMART). In some cases, I find that the year in which a state implemented SORNA was not the same as the year in which SMART determined that the state had substantially implemented SORNA. This discrepancy was, in some cases, due to the lag in SMART's review process and, in case of Nevada, because SMART approved the state's implementation of SORNA on the basis of state legislation that had not yet been enacted.[35] In these cases, I use the date that the state implemented SORNA or enacted SORNA-implementing legislation[36] instead of the date that SMART determined that the state substantially implemented SORNA.[37] Data on the year in which states implemented sex offender registries and made their registries available online is from Prescott and Rockoff (2011), Agan (2011), and Logan (2021). Table 12 includes the year of registry implementation, SORNA implementation, Jessica's Law enactment across states, as well as the year that states first made their registries available online. Table 13 includes data on registry size and baseline data for

[34] See Footnote 30 for specific sources.
[35] This was because Nevada was enjoined from enacting its SORNA implementing legislation by a federal court, as described in Part 1. SMART determined that this was an exceptional circumstance.
[36] Or the date that legislation became effective, if different from the date when it was enacted.
[37] Based on this methodology, I include Pennsylvania as a state that implemented SORNA since the state enacted SORNA implementing legislation in 2011 and that legislation became effective in 2012 and led to an increase in the size of Pennsylvania's registry. SMART determined that Pennsylvania had substantially implemented SORNA in 2012 but later revoked that determination. Due to this revocation, my dataset includes 19 states that have implemented SORNA whereas SMART currently lists 18 states as SORNA-compliant (Pennsylvania State Police Megan's Law Section Annual Report 2018).

Part 5 of this chapter on the incidence of sex crimes and other violent crimes, which include murder, robbery, and aggravated assault, across states. Rates refer to the number of annual incidents or arrests per 1000 people.

I expand on prior research in numerous ways. By using new statistical methods, I avoid the previously discussed methodological problems raised by Agan (2011) and Prescott and Rockoff's (2011) use of TWFE models. By exploiting variation across states in whether and when they implemented SORNA, I examine the effects of registry size expansion on sex crime rates using an exogenous explanatory variable. Specifically, SORNA led to an expansion of registry size but, as I will demonstrate, was not more likely to be implemented in states with high sex crimes rates. In contrast, the danger of using registry size as an explanatory variable, as Prescott and Rockoff do, is the relationship between registry size and sex crime rates is bidirectional. Registry expansion could reduce sex crime rates but registries will also grow more rapidly when more people are committing sex crimes. As such, SORNA is an effective instrument for evaluating the effect of registry size expansion on sex crime rates.[38]

Given the different data sources and results reached by Agan and Prescott and Rockoff, I also expand on their analysis by using both NIBRS and UCR data and comparing the results that I derive from each data source. This approach enables me to examine the extent to which the differences in Agan and Prescott and Rockoff's results were due to the difference in their data sources as opposed to their methodology.

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[38] I tried using SORNA as an instrumental variable for registry size but the results were not significantly different from the model included in this chapter in which I include SORNA as an explanatory variable. I therefore do not include the instrumental variable model in this chapter.

Part 4 Analysis of the Effectiveness of the Jacob Wetterling Act and Megan's Law

4.1 Empirical Strategy

I analyze the effectiveness of sex offender registries and the posting of those registries on the internet. Borrowing from Agan (2011), my TWFE model is:

Equation 2

$$SexCrimeRate_{i,t} = \beta_1 Registry_{s,t} + \beta_2 Internet_{s,t} + X_{i,t} + \rho_t + \tau_i + \epsilon$$

where t represents the year, from 1985 to 2018, s represents the state, i represents reporting area within state s, τ represents ORI fixed effects, ρ represents time fixed effects, and X represents a vector of controls. Controls include the number of months included in ORI i's crime reporting in year t[39] and the number of other violent crimes per 1,000 people in reporting area i at time t. The dependent variable takes the form of both the rape rate and arrests for sex offenses other than rape or prostitution.

I use a TWFE event study regression to check for lagged effects of registry implementation and the posting of registries to the internet. Lagged effects are probable given that, as shown in Figure 2, registries have grown significantly over time. As Prescott and Rockoff (2011) argue, this growth could impact their effectiveness. Their usefulness in monitoring and notifying the public of the whereabouts of dangerous offenders will be greater when they include more potentially dangerous registrants. My event study regression specification is:

[39] See Footnote 31.

Equation 3

$$Y_{i,t} = \sum_{k=T_0}^{-2} \gamma_k^{lead} D_{i,t}^k + \sum_{k=0}^{T_1} \gamma_k^{lag} D_{i,t}^k + X_{i,t} + \rho_t + \tau_i + \epsilon$$

where k represents the number of years between time t and registry implementation or the posting of registries to the internet, D is a dummy variable equaling one if the number of years between time t and registry implementation or the posting of registries to the internet in the state that reporting area i is within equals k, T_0 and T_1 are the minimum and maximum number of leads and lags included in the model, γ represents the coefficients on the lead and lag indicators, τ represents ORI fixed effects, ρ represents time fixed effects, and X represents a vector of the same controls used in Equation 2.

I also test the effect of registry creation and the posting of registries on the internet with the CS estimator. I use the outcome regression (OR) model specified in Equation 14 with X as the pre-treatment levels of other violent crime rates, and I aggregate group-time average treatment effects to derive an overall treatment effect using the simple aggregation described in Appendix 1 Part A.

4.2 Results

Using updated data and new statistical methods, I find evidence that sex offender registration and notification have a lagged negative effect on sex crime incidence. I start by replicating Agan's analysis using the 1985-2003 UCR data that she used[40] and the TWFE model borrowed from her and specified in Equation 2 above. Although my model is not exactly the same as hers,[41] my regression results are consistent with hers. As shown in Table 1, neither registries nor the posting of those registries to the internet has a statistically significant effect on the rape rate. Registries do not have a statistically significant effect on the sex offense arrest rate but, like Agan, my results using the 1985-2003 data do show a negative effect of posting registries to the internet on the sex offense arrest rate that is statistically significant at the .1 level.

Updated UCR data provides significantly more support for the effectiveness of registries and internet notification. As shown in Table 1, in my results using the same model and updated 1985-2018 data, both registries and the posting of registries to the internet have a statistically significant effect on the sex crime rates. Registries reduce the rape rate by approximately 10.3% in a result that is statistically significant at the .01 level,[42] while posting registries to the internet reduces the sex offense arrest rate by approximately 8.5% in a result that is also statistically significant at the .01 level.

However, a Bacon Decomposition of these estimates created using a balanced panel subset of my UCR data[43] and the results of which are included in Table 14, does

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[40] The data is not exactly the same since she uses UCR data aggregated by State.

[41] We do not use exactly the same controls and Agan uses the natural logarithm of the rape and sex offense rate as her dependent variables (Agan 2011, p. 215).

[42] One concern with incorporating the updated data is that the definition of rape used in UCR reporting was broadened in 2013, as is described in more detail at the bottom of Table 1. As such, pre and post-2013 rape data is not entirely comparable. For this reason, I also used the TWFE model specified in Equation 2 with 1985-2011 UCR data. While the estimated negative effect of registries on rape is not as strong in my results based on the 1985-2011 data, it is still statistically significant at the .1 level.

[43] The Bacon Decomposition can only be used with a balanced panel. I also did not include controls when using the Bacon Decomposition since, while it can be performed for TWFE models with controls, it provides a less granular decomposition. Due to the absence of reporting frequency as a control, I excluded

show potential problems with the TWFE effects model used to obtain these results. As discussed in Part 3, TWFE regression models like Equation 2 are problematic in case of staggered inventions and most problematic when all units are eventually treated, as is the case in the implementation of sex offender registries. In these cases, a larger percent of the estimated treatment effect is based on 2x2 DD estimates in which the control group is comprised of already treated units. The use of sex offender registration as an independent variable is especially problematic because four states created sex offender registries before 1985, which is the first year included in the datasets that Agan and I use and ten states created sex offender registries before 1991, which is the first year included in the NIBRS data that Prescott and Rockoff use. As a result, a large percent and, in the case of the model in which the sex offense arrest rate is the dependent variable, most of the overall treatment effect is from 2x2 DD estimates in which the control group is comprised of these always treated units.

The results of the Bacon Decomposition are less problematic for the conclusion that the posting of registries to the internet decreases sex offense arrest rates. Using 1985-2003 data, based on which Agan and I find a negative effect of internet registries on sex offense arrest rates that is significant at the .1 level, the Bacon Decomposition of my results shows that most of the estimated effect of posting registries on the internet is based on 2x2 DD estimates in which never treated units comprise the control group.

observations based on less than 12 months of reporting. Finally, while Equation 2 has sex offender registration and the posting of registries to the internet as two separate independent variables, the Bacon Decomposition can only be performed on the results of models with a single binary treatment variable so I performed it on the results of models in which the effects of sex offender registration and the posting of registries to the internet were tested separately.

Using data through 2018, by which time all states had posted their registries to the internet, this is no longer the case, but the results of 2x2 DD estimates in which not yet treated units comprise the control group are significantly negative and comprise approximately half of the overall treatment effect. The results of the Bacon Decomposition show that Agan's estimates of the effectiveness of registry implementation, which is where she did not find any statistically significant effects, were problematic due to the absence of never treated units and the inclusion of always treated units. In contrast, her estimates of the effectiveness of posting registries to the internet, which is where she did find a statistically significant negative effect on sex offense arrest rates, were more reliable due to a large number of never treated units or states that had not yet implemented internet registries by 2003. This result suggests that both use of a methodologically flawed model could have caused her to underestimate the effects of sex offender registries and models other than Equation 2 are needed to test the effectiveness of sex offender registration.

I therefore further analyze the effectiveness of sex offender registration and notification using the event study regression model specified in Equation 3 and the CS estimator. The event study regression results based on updated UCR data and depicted in Figure 4 and Figure 5 are consistent with the results in Table 1 described above and provide evidence of a negative effect of sex offender registration and posting registries to the internet on sex crime rates. A comparison of the event study plots in Figure 4 shows that, especially at later event times, the updated 1985-2018 UCR data provides more evidence of a negative effect of sex offender registration on rape rates than the 1985-2003 data. Similarly, the event study plot in Figure 5 created using updated UCR data provides evidence of a negative effect of posting registries to the internet on the sex offense arrest rate that becomes stronger over time.[44]

As of 2003, not all states had yet posted their registries to the internet so this lagged effect may not have been evident in the 1985-2003 data. While all states had created sex offender registries by 2003, the last state to implement sex offender registration did so in 2000 so the lagged effect of sex offender registration on crime rates would not have been fully captured in 1985-2003 data. As is further discussed in Part 5, data on registry size across states suggests that registries grew significantly over many years after initial implementation before approaching a steady state value. As Prescott and Rockoff demonstrate, the effectiveness of registries is likely to depend on their size since a registry with few registrants is not useful for monitoring dangerous offenders. As such, their full effectiveness would have been likely to take many years to realize and may not have been evident from the earlier data or effectively estimated by a TWFE model using states that already implemented registries as controls. The lagged effect of sex offender registration and internet notification demonstrated in Figure 4 and Figure 5 using updated UCR data is thereby consistent with Prescott and Rockoff's result that increases in registry size lead to decreased sex crime rates and helps bridge the apparent gap between the UCR data and the NIBRS data that Prescott and Rockoff used that

[44] An alternative interpretation of this result also discussed in Agan (2011, p. 221) is that internet registries lead to a decrease in the proportion of sex crimes leading to arrest or the arrest ratio rather than sex offense incidence. In this case, the negative effect of internet registries on the sex offense arrest rate is not evidence for their effectiveness at controlling crime. The relationship between the stringency of sex crime laws and the arrest ratio for sex crimes is further discussed in Chapter 3.

caused Agan and Prescott and Rockoff to reach differing conclusions about the effectiveness of registries for reducing recidivism (Agan and Prescott 2021, p.120), despite using similar models and unsuccessfully trying to replicate each other's results using different data sources.[45]

The results of the CS model provide more evidence for the effectiveness of sex offender registration and the posting of registries to the internet in reducing rape rates than sex offense arrest rates. Internet notification reduces the rape rate by approximately 9% in a result that is significant at the .05 level and robust to the Doubly Robust Difference in Differences (DRDID) estimation method and all three of the aggregation methods described in Appendix A. Sex offender registration reduces the rape rate by 27% in a result that is robust to all three aggregation methods but not robust across estimation methods.[46] In contrast, unlike the TWFE and Event Study models above, the results of the CS model do not provide evidence for a negative effect of sex offender registration on the sex offense arrest rate. I also use the CS method with a balanced panel subset of my UCR data. The results of this robustness check are included in Table 14 and provide less support for the effect of sex offender registration and notification. While the estimated treatment effects of sex offender registration and notification are, in all but one of the models, negative, they are not statistically significant. While the results of the CS

[45] Agan (2011, p.237) replicates Prescott and Rockoff's model using the UCR data and restricting her dataset to only those states included in Prescott and Rockoff's NIBRS data. Nevertheless, unlike them, she does not find statistically significant evidence that sex offender registration reduces recidivism, which she attributes to differences in sample size and the differences in the populations covered by their data sets (Agan 2011, p.224-225).

[46] This result is not included in Table 1 since it was obtained using the DRDID estimation method described in Appendix A and is not robust to the outcome regression method used in this chapter, which does not provide evidence of a statistically significant effect of sex offender registration on the Rape Rate.

model, which has limited ability to capture lagged effects when all units are eventually treated,[47] do not provide overwhelming support for the effectiveness of sex offender registration and notification, updated UCR data, the event study plots, and the results of the Bacon Decomposition suggest that Agan underestimated their effectiveness due partly to her use of a methodologically flawed TWFE model for testing the effectiveness of sex offender registration.

Table 1 Effect of Registry Implementation and the Posting of Registries on the Internet on Sex Crime Rates

TWFE Models

	Rape Rate	Sex Offense Arrest Rate	Rape Rate	Sex Offense Arrest Rate
Registry	-.006(.004)	-.007(.007)	-.019(.004)***	-.003(.006)
Internet	.004(.003)	-.023(.012)*	-.0001(.003)	-.014(.005)***
Other Violent Crime Rates	Yes	Yes	Yes	Yes
ORI Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Dependent Variable Mean	.169	.19	.185	.164
#Obs	279,293	279,280	523,267	519,246
#States	50	50	50	50
#ORI Reporting Areas	15,849	15,849	17,098	17,095
Years	1985-2003	1985-2003	1985-2018	1985-2018

CS Estimator

Effect of Registry Creation		
ATT	.002(.008)	.007(.012)
Other Violent Crime Rates	Yes	Yes
Dependent Variable Mean	.216	.306
#Obs	123,212	86,495
Years	1985-1997	1985-1997
Posting to Internet		

[47] This is due to the absence of a control group once all units are treated. As such, I was only able to use 1985-1997 data for testing the effectiveness of registry implementation using the CS model and 1985-2005 data for testing the effectiveness of posting registries to the internet using the CS model.

ATT	-.021(.01)**	.012(.017)
Other Violent Crime Rates	Yes	Yes
Dependent Variable Mean	.233	.308
#Obs	220,970	153,761
Years	1985-2005	1985-2005

Note. Numbers in parentheses are standard errors clustered by ORI. Rape and Sex Offense Arrest rates are calculated as rates per 1000 people. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. Prior to 2013, forcible rape was defined in the UCR as the forcible carnal knowledge of a female against her will. Rapes include forcible and attempted forcible rapes. In 2013, the definition of rape expanded to include any nonconsensual penetration of the anus or vagina by a body part, object, or sex organ (Crime in the United States 2013). Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

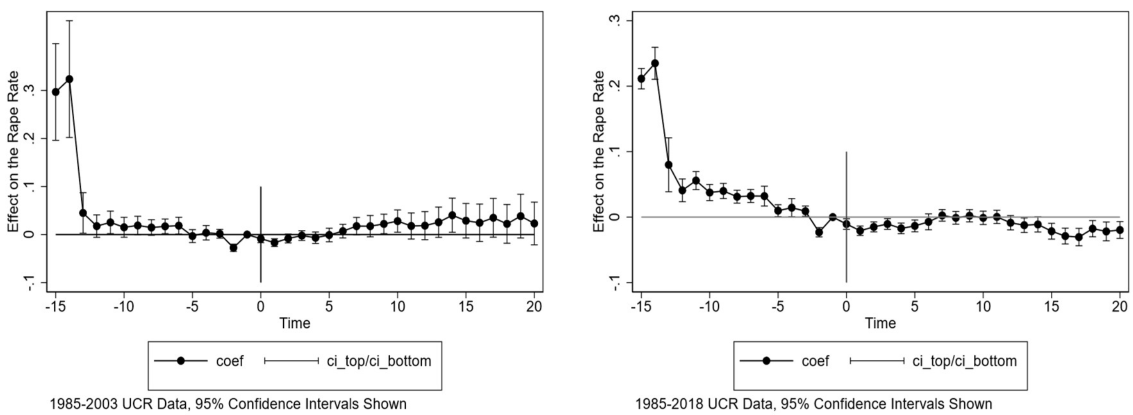

Figure 4 Effect of Registries on the Rape Rate Using 2003 and 2018 Data

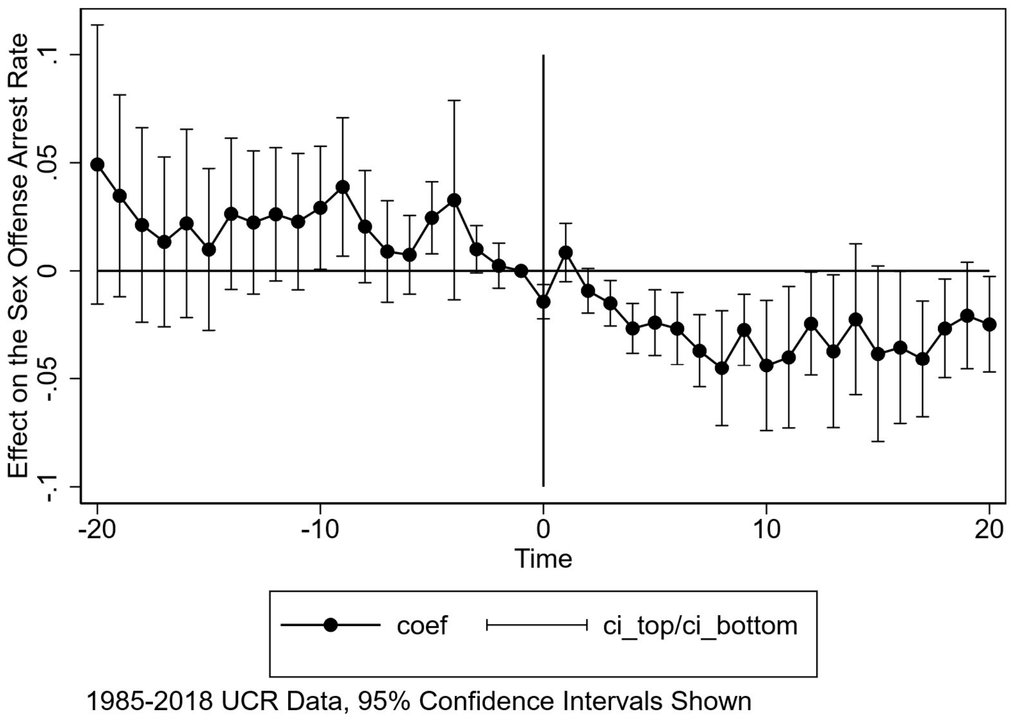

Figure 5 Effect of Posting Sex Offender Registries to the Internet on Sex Offense Arrest Rates

Part 5 Evaluation of SORNA

5.1 Empirical Strategy

I evaluate the impact of SORNA on state registry size and sex crime rates. This includes the rates of crimes, including kidnapping of minors[48] and sex crimes committed by juveniles, newly designated as sex crimes as a result or SORNA or for which SORNA mandated that perpetrators must register. My third model includes an interaction term between registry size and SORNA to evaluate whether registry size affects sex crime rates differently in SORNA-compliant and non-compliant states. I incorporate NIBRS

[48] Unless committed by a parent or guardian.

data into my evaluation of SORNA due to the usefulness for this analysis of the additional detail provided in NIBRS, the fact that more states and agencies reported to NIBRS during the more recent timeframe covered in this analysis, and because, unlike NIBRS, the UCR SRS data does not contain data on kidnapping. My TWFE regression specifications are:

Equation 4

$$Rg\ Size_{s,t} = \beta_1 SORNA_{s,t} + \rho_t + \tau_s + \epsilon$$

Equation 5

$$SexCrimeRate_{i,t} = \beta_1 SORNA_{s,t} + \beta_2 Jessica'sLaw_{s,t} + X_{i,t} + \rho_t + \tau_i + \epsilon$$

Equation 6

$$SexCrimeRate_{i,t} = \beta_1 Rg\ Size_{s,t} + \beta_2 Rg\ Size\ x\ SORNA_{s,t} + X_{i,t} + \rho_t + \tau_i + \epsilon$$

where t represents the year, from 2003 to 2018, s represents the state, i represents reporting area within state s, τ represents ORI fixed effects or, in case of Equation 4, state fixed effects, ρ represents time fixed effects, and X represents a vector of controls. *Rg Size* is the size of the sex offender registry of state s per 100k people in year t, $SORNA$ is a dummy variable equal to 1 if state s implemented SORNA by or in year t, and $Jessica'sLaw$ is a dummy variable equal to 1 if state s enacted Jessica's Law by or in year t. Controls include the number of months included in ORI i's crime reporting in year t and the number of other violent crimes per 1,000 people in reporting area i at time

t. In regressions using the UCR data, the rape rate and sex offender arrest rate are used as the dependent variable in the models specified in Equation 5 and Equation 6.

I again use a Bacon Decomposition to evaluate the results of my TWFE models and use the event study model specified below to check for lagged effects of SORNA on registry size and sex crime rates:

Equation 7

$$Y_{i,t} = \sum_{k=T_0}^{-1} \gamma_k^{lead} D_{i,t}^k + \sum_{k=1}^{T_1} \gamma_k^{lag} D_{i,t}^k + X_{i,t} + \rho_t + \tau_i + \epsilon$$

where k represents the number of years between time t and SORNA implementation, D is a dummy variable equaling one if the number of years between time t and SORNA implementation in the state that reporting area i is within equals k, T_0 and T_1 are the minimum and maximum number of leads and lags included in the model, γ represents the coefficients on the lead and lag indicators, τ represents ORI fixed effects, ρ represents time fixed effects, and, in my evaluation of the effect of SORNA on sex crime rates, X represents a vector of the same controls used in Equation 5. For reporting areas in states that are non-compliant with SORNA, k is set to 0.

I also use the CS estimator to analyze the effect of SORNA on registry size and sex crime rates. To evaluate SORNA's impact on sex crime rates, I use the outcome regression model specified in Equation 14 with X as the pre-treatment level of other violent crime rates. To test its effect on registry size, I do not include a control variable but otherwise use the same specifications.

If states with higher sex crime rates were more likely to implement SORNA either earlier or at all, SORNA implementation would be endogenous to sex crime incidence and estimates of its effectiveness at reducing sex crimes would be biased. However, baseline sex crime rates are not higher in SORNA-compliant states. Based on NIBRS data, the average sex crime rate in SORNA-compliant states in 2006, the year SORNA was enacted, was lower by .026 than the average sex crime rate in states that have not implemented SORNA. Generally, new sex crime laws have been enacted in response to specific egregious cases attracting media attention instead of high sex crime incidence, making endogeneity less of a concern in evaluating the effectiveness of sex crime laws than other crime laws (Agan 2011, p.217-218). The abductions of Adam Walsh and Jessica Lunsford, who are the namesakes of the laws examined in this chapter, are examples of such cases.

5.2 Results

SORNA implementation leads to an increase in the size of state sex offender registries. Registry size in states that are currently SORNA-compliant grew more than twice as much as in non-compliant states in the 10 years following enactment of the AWA.[49] Based on the TWFE model specified in Equation 4 and reported in Table 2, SORNA implementation leads to a 15% increase in registry size in a result that is

[49] Specifically, registry size per 100k people grew, on average, by 50% and 22% between 2007 and 2017 in SORNA-compliant and non-compliant states, respectively. While this difference seems high relative to the results of the model specified in Equation 4, it, unlike the TWFE model, does not account for when states became SORNA-compliant but merely compares overall growth between the two groups from 2007-2017.

significant at the .01 level.[50] A review of the Bacon Decomposition included in Table 15 shows that this positive effect is reliable. Many states never implemented SORNA, as a result of which there are a large number of never treated units in the data, and 84% of the TWFE estimate is based on 2x2 DD estimates in which those never treated states comprise the control group. The CS estimator provides less strong support for the positive impact of SORNA on registry size but does still estimate a statistically significant[51] positive effect.

The Bacon Decomposition in Table 15 also suggests that this positive effect of SORNA on registry size is greatest at a lag. The estimated effect of SORNA implementation in 2x2 DD estimates in which states that implemented SORNA earlier comprise the control group is negative, suggesting that registries grew faster in states that implemented SORNA earlier than in those where it had just been implemented. This result is not surprising given that the immediate effect of SORNA implementation on registry size is likely to be limited, but, as sex offenders are newly released from prison and new sex crimes are committed, the broader criteria for registry inclusion will lead to significant growth in registries. Given the time varying effect of SORNA implementation, 17.5%, which is based only on 2x2 DD estimates in which never treated units comprise the control group as reflected in Table 15, is a slightly higher and

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[50] 15% is not as high as Part 1 would suggest partly because registry size in some non-compliant states, including Nebraska, West Virginia, and Oregon, grew considerably between 2005 and 2018. In case of Nebraska, this growth may be partly due to the fact that the state is compliant with SORNA in terms of which adult sex offenders it registers but was deemed non-compliant by SMART due to its failure to register juveniles and meet SORNA-mandated notification requirements (SORNA Substantial Implementation Review: State of Nebraska 2010).

[51] at the .1 level.

potentially more reliable estimate of the increase in registry size as a result of SORNA implementation. The event study plot in Figure 7 further suggests that the effect of SORNA on registry size is greatest at a lag. The immediate estimated positive effect is small but increases in the third year following implementation and remains statistically significant before eventually decreasing.

Figure 6, which compares the average size of state sex offender registries between 1998 and 2018 in SORNA-compliant and non-compliant states provides further insights into the dynamics of registry size growth. Registry size growth in both groups of states followed roughly parallel trends prior to the 2006 enactment of the AWA. This growth was high following enactment of the Jacob Wetterling Act and Megan's Law in the 1990s but begins to decline in the late 2000s in non-compliant states. We would expect such a decline in growth once registries have grown significantly since larger registries will experience more attrition, and the number of people leaving the registry each year will approach the number of new registrants. Eventually registry size should gravitate towards a steady state, as is suggested by Figure 6, that will only be disrupted by changes in the amount of crime being committed or in the criteria for registry inclusion. Consistent with this hypothesis, in the late 2000s and early 2010s, registry size continues to exhibit high growth in SORNA-compliant states where the criteria for registry inclusion was broadened but declines significantly in non-compliant states. The chart also shows that the period of rapid growth in both groups of states following enactment of the Jacob Wetterling Act and Megan's Law in the 1990s lasted many years. Given such a long period of growth, the effectiveness of

Table 2 Impact of SORNA on State Registry Size per 100k People

TWFE Models

	Registry Size	Log Registry Size
SORNA	37.419(13.867)***	.151(.055)***
State Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes
#Obs	714	714
#States	51	51
Years	2005-2018	2005-2018

CS Estimator

	Registry Size	Log Registry Size
ATT	23.973(14.248)*	.0873(.05)*
#Obs	714	714
Years	2005-2018	2005-2018

Note. Numbers in parentheses are standard errors clustered by state. Registry Size refers to state registry size per 100,000 people in the population.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

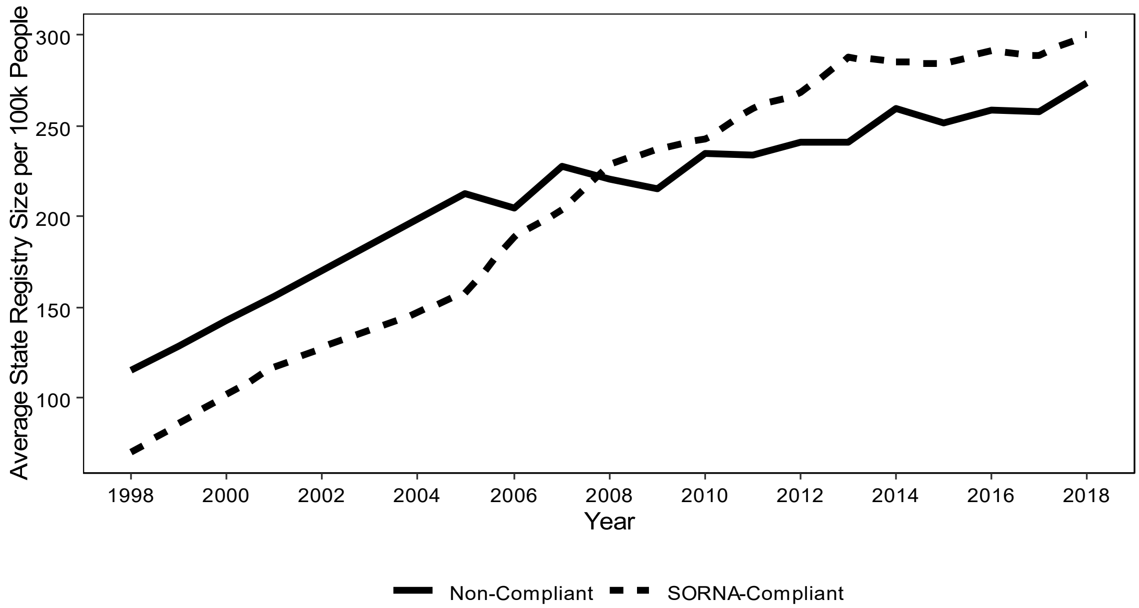

Figure 6 State Registry Size Growth in SORNA-Compliant and Non-Compliant States[52]

registries is likely to be greatest at a significant lag since their value for monitoring dangerous criminals will be greatest when their size is larger, as is discussed and demonstrated in Part 4.

Evidence for the effectiveness of SORNA in reducing sex crime rates is not as strong as for that of initial registry creation and internet notification. Based on the results of the CS Estimator in Table 3, SORNA leads to an 8% reduction in sex crime rates and a 7% reduction in the Rape Rate. However, these results are only significant at the .1 level and not robust across aggregation methods, and the results of the TWFE model in Table 16 provide no evidence of a negative effect. Results provided in Table 17 from using the CS estimator with a balanced subset of the UCR and NIBRS data also provide limited

[52] Registry size data is unavailable for 1999-2000 and 2002-2004 and is interpolated using linear interpolation in this chart. Any state that ever implemented SORNA is included as SORNA-compliant.

evidence of a negative effect but are not robust across estimation methods. The event study plot in Figure 8 of the effect of SORNA on the rape rate provides further evidence of a negative effect of SORNA on sex crime incidence. The estimated negative effect is greatest four or more years following implementation, suggesting that it is a lagged effect and the result of the increase in registry size caused by SORNA, which is similarly at its apex four years after implementation, as shown in Figure 7.

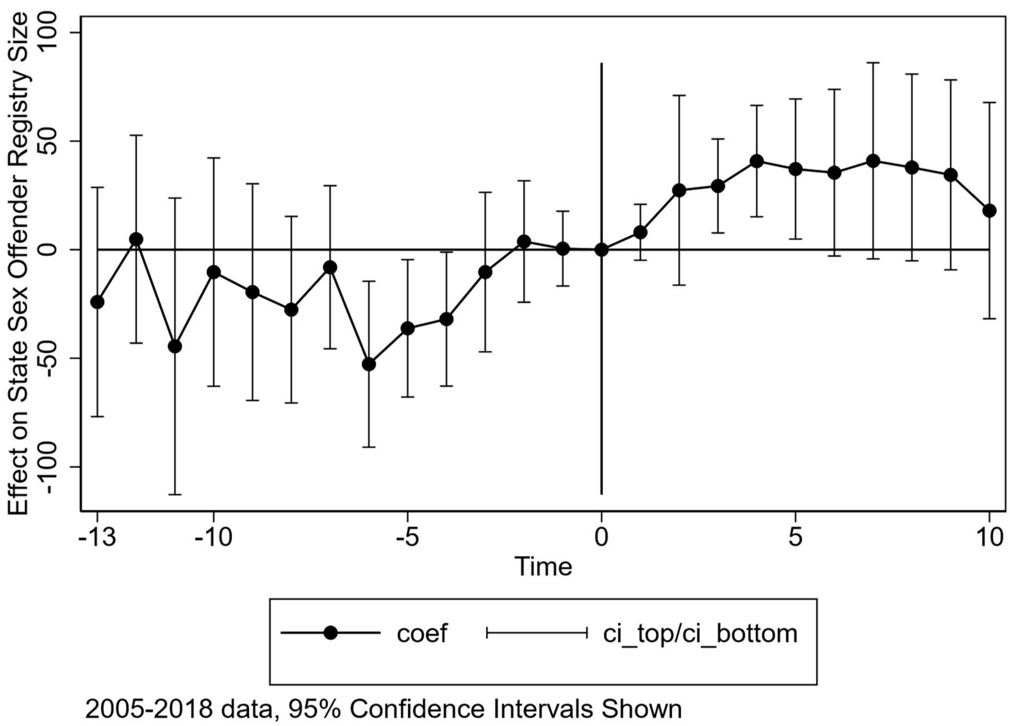

Figure 7 Effect of SORNA on State Registry Size per 100k People

Table 3 Effect of SORNA on Sex Crime Incidence

	Sex Crime Rate	Rape Rate	Sex Offense Arrest Rate
ATT(Simple Aggregation)	-.029(.052)	-.005(.012)	.014(.016)

ATT(Dynamic Aggregation)	-.061(.035)*	-.017(.01)*	.006(.016)
Other Violent Crime Rates	Yes	Yes	Yes
Dependent Variable Mean	.789	.261	.222
#Obs	56,644	197,399	132,586
Years	2003-2016	2003-2018	2003-2018

Note. Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. Sex crimes are defined as forcible rape, sodomy, sexual assault with an object, fondling, incest, statutory rape, and kidnapping of a minor unless that kidnapping was by a family member. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

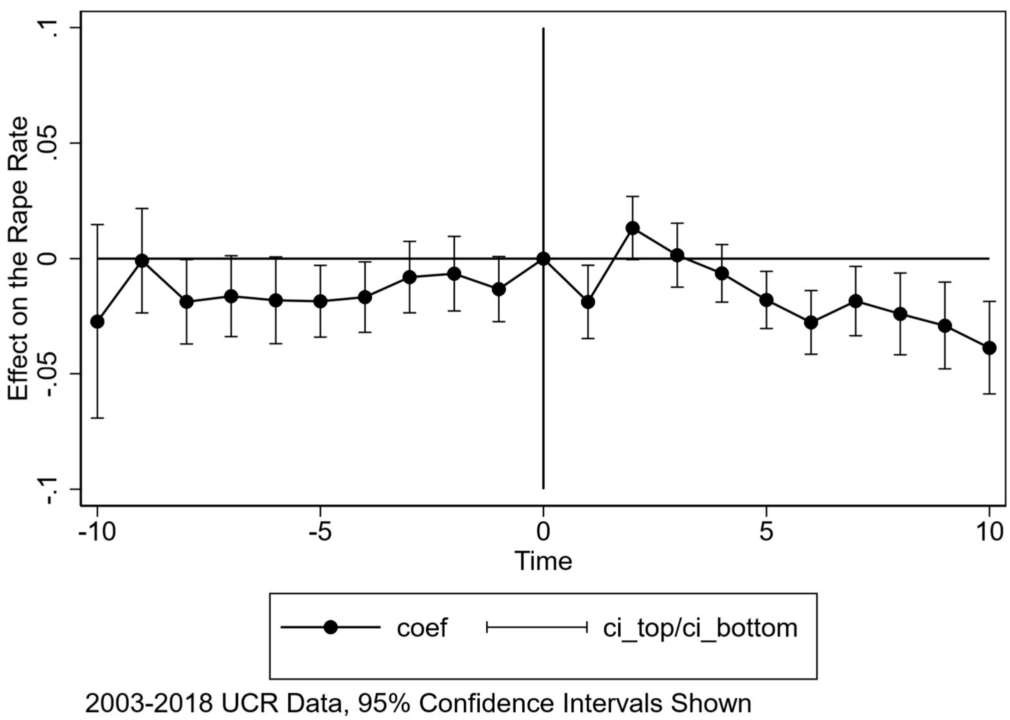

Figure 8 Effect of SORNA on the Rape Rate

I also test the effect of SORNA Implementation on the incidence of specific offenses for which the AWA mandates more severe consequences. As explained in Part

1, SORNA broadens the definition of "sex crime" and requires registration for crimes that had not previously been registerable offenses. These crimes include non-parental kidnapping of minors, which SORNA designates as a Tier 3 sex offense even if it is not for sexual purposes, and sex crimes committed by juvenile offenders. Given that the AWA prioritizes the protection of children based on its stated goals[53] and uses 13 as an age threshold by mandating more severe consequences for sexual abuse of victims under than over 13, I also test the effectiveness of SORNA in reducing sex crimes against children under the age of 13.[54] As shown in Table 4, SORNA implementation reduces the incidence of neither juvenile sex crimes, non-parental kidnapping of minors, nor sex crime against victims under 13.

Table 4 Effect of SORNA on Crimes Made Registerable or More Severe Offenses by SORNA

	Juvenile Sex Crime Rate	Sex Crimes with Victims Under 13	Kidnapping of Minors
ATT	.012(.013)	-.002(.013)	-.006(.013)
Other Violent Crime Rates	Yes	Yes	Yes
Dependent Variable Mean	.112	.237	.015
#Obs	56,644	56,644	56,644
Years	2003-2016	2003-2016	2003-2016

Note. Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. Juvenile sex crime rates include sex crimes committed by minors between 14 and 17. Sex crimes committed by children under 14 are not included because registration is only mandated by SORNA for juveniles at least 14 years old. Kidnapping of minors excludes kidnapping by a family member because kidnapping committed by a parent or guardian is not a sex crime according to SORNA. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level

[53] The AWA's stated goals are "to protect children from sexual exploitation" and "prevent child abuse" (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006]).
[54] Specifically, "abusive sexual contact" against a minor under 13 is a Tier III sex offense while "abusive sexual contact" against a child at least 13 years old is a Tier II offense. Also, according to the AWA, consensual sexual conduct between children or between a child and a young adult who is not more than 4 years older than that child does not constitute a sex offense if the victim is at least 13 years old. (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006])

* denotes statistical significance at the .1 level

I use the model specified in Equation 6 to evaluate the effectiveness of SORNA by comparing the effects of registry expansion in SORNA-compliant and non-compliant states. The value of registry expansion in controlling sex crimes should be highest when the offenders added to registries are those who are most likely to commit crime and therefore most in need of monitoring. If, as Sandler and Freeman (2010) and Zgoba, Miner, Levenson, Knight, Letourneau, and Thornton (2016) argue, SORNA's conviction-based criteria is a poor indicator of the risks posed by an offender, registry expansion is therefore likely to be less effective in controlling sex crimes in SORNA-compliant states. Due to the problems described in Part 3 associated with using TWFE models in cases of staggered interventions, I restrict the states that I include to those that never implemented SORNA and those that implemented SORNA in 2011, the year when the largest number of states became SORNA-compliant.[55] In Table 18, I provide the results that I obtain when I remove this restriction and include all states for which data is available, which are consistent and differ only in the level of statistical significance of certain coefficients.

My results provide strong evidence that registries are indeed less effective in SORNA-compliant states. Borrowing from Prescott and Rockoff, I use the level of detail provided by NIBRS[56] to analyze separately the effect of registries on incidence of sex

[55] The Bacon Decomposition and CS Method require a binary treatment variable so are not compatible with this model, which is testing the effect of an intervention on an interaction.
[56] NIBRS provides information about the relationship between the offender(s) and victim(s) of included crime incidents.

crimes against victims who are known to or neighbors of the offender or, as they are referred to in Table 5, local victims and their effect on the incidence of sex crimes against strangers. Like Prescott and Rockoff, I find that registry expansion is most effective in controlling sex crimes against local victims with an increase in registry size of 100 per 100,000 people in the population leading to an estimated 3% decrease in the rate of sex crimes against local victims in a result that is statistically significant at the .05 level. However, in SORNA compliant states this effectiveness is significantly reduced, as demonstrated by the positive and statistically significant coefficient on the interaction term between SORNA and registry size.[57] As shown in Table 5, the coefficient on registry size is also negative when I use the overall sex crime rate or sex offense arrest rate as my dependent variable but no longer statistically significant, while the coefficient on the interaction term between registry size and SORNA also remains positive in these alternative specifications and is statistically significant at the .01 level.

I also find evidence that registries cause offenders to substitute strangers for local victims. As Prescott and Rockoff argue, police are more easily able to monitor registered

[57] An alternative interpretation of this result is that it is due to reverse causality. More stringent sex offender registration requirements in SORNA-compliant states could cause more sex crimes to result in perpetrators having to register, leading to a positive correlation between sex crime rates and registry size that might not be as strong in non-compliant states. I do find evidence that registry size and sex crime rates are more strongly correlated in SORNA-compliant than non-SORNA-compliant states. Specifically, based on 2005-2016 NIBRS data, the correlation coefficient is .38 in SORNA-compliant states and .09 in non-SORNA compliant states. However, this interpretation does not account for the difference in sign on the coefficient on registry size and the coefficient on the interaction term between SORNA and registry size. Also, as Prescott and Rockoff argue, sex offenders do not register until after imprisonment so there is a significant lag between when offenders commit a sex crime and when they register. Less than 3% of offenders who committed rape, sexual assault, or child molestation in 2002 were imprisoned for one year or less. As such, short-term changes in sex crime rates are unlikely to be a significant cause of short-term changes in registry size, making reverse causation unlikely in this model (Prescott and Rockoff 2011, p. 171).

sex offenders around family members and acquaintances (Prescott and Rockoff 2011, p. 184). This increased monitoring capability explains why registries are most effective at controlling sex crimes against local victims and could prompt sex offenders to instead target strangers. Neighbors, family, and acquaintances will also be most aware of the potential danger posed by a registered sex offender. Online registries allow people to search for sex offenders who live in their residential area, people receive alerts when sex offenders move into their neighborhood, and some states mandate door-to-door dissemination of information on neighboring sex offenders. In some states, community meetings with law enforcement are held for those living in the same neighborhood as a sex offender. While such enhanced scrutiny could be effective for controlling sex crimes in neighborhoods where offenders live and work, it could also, as Prentky (1996, p. 295) argues, cause offenders to seek out victims in adjacent communities who are unknown to them and unaware they are a sex offender. Consistent with this hypothesis, I find that registry expansion has a positive effect on the rate of sex crimes against strangers with an increase in registry size of 100 per 100,000 people in the population leading to an estimated 14% increase in the rate of sex crimes against strangers in a result that is statistically significant but only at the .1 level. While not highly statistically significant, this result does call into question the overall value of registries, despite evidence in Part 4 for their effectiveness and their demonstrated negative effect on sex crimes against local victims, which constitute the vast majority sex crime incidents.[58] In contrast, Prescott

[58] Based on 2003-2016 NIBRS data, the victims of 91.6% of sex crimes were neighbors, related to, or otherwise known to the offender.

and Rockoff, using less recent NIBRS data,[59] also find that the coefficient on registry size switches signs when they use sex crimes against strangers instead of family members and acquaintances as their dependent variable but reject the substitution hypothesis due to statistical insignificance.[60]

Table 5 Comparison of the Effect of Registry Expansion in SORNA-Compliant and Non-Compliant States

	Sex Offense Arrest Rate	Sex Crime Rate	Sex Crimes against Local Victims	Sex Crimes Against Strangers
Registry Size	-.00002(.00003)	-.00009(.00008)	-.0002(.00007)**	.00005(.00003)*
SORNA X Registry Size	.00008(.00002)***	.0002(.00006)***	.0003(.00005)***	-
Other Violent Crime	Yes	Yes	Yes	Yes
ORI Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Dependent Variable Mean	.134	.751	.586	.036
#Obs	160,734	42,521	42,521	57,091
#States	39	31	31	39
#ORI Reporting Areas	12,040	4,432	4,432	6,148
Years	2005-2018	2005-2016	2005-2016	2005-2016

Note. Numbers in parentheses are p-values. Sex Crime and Sex Offense Arrest rates are calculated as rates per 1000 people. See the note below Table 3 for how sex crimes are defined. Sex Crimes Against Local Victims include sex crimes where the victim was related to, an acquaintance of, a neighbor of, or otherwise known to the offender. Sex Crimes Against Strangers include sex crimes where the victim and the offender were strangers. Registry Size refers to state registry size per 100,000 people.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Conclusion

Using updated data and new statistical methods, I find evidence of a lagged negative effect of sex offender registration and internet notification on sex crime

[59] Prescott and Rockoff use 1991-2005 data.
[60] The positive coefficient on registry size that they estimate when they use the rate of sex crimes against strangers as their dependent variable is not statistically significant at any conventional level (Prescott and Rockoff 2011, p. 183).

incidence. Once registries have had time to grow, they will provide more comprehensive monitoring of potentially dangerous offenders. I also find that registries are most effective at controlling sex crimes against victims who are neighbors, acquaintances, or family members of or otherwise known to offenders. However, I also find pitfalls of sex offender registration and notification. I find evidence that registries cause offenders to substitute strangers for local victims and that the offense-based criteria for registry inclusion established by SORNA makes registries less effective at controlling sex crimes in SORNA-compliant states. From a policy perspective, this finding suggests that sex offender registration and notification policies should be determined locally instead of on the basis of sweeping federal mandates.

CHAPTER 2 THE IMPACT OF JESSICA'S LAW ON SEX CRIMES

This chapter examines the effectiveness of Jessica's Law in reducing sex crime rates. Jessica's Law was enacted in different states at different times between 2005 and 2014 and established minimum mandatory prison sentences for child sexual assault and GPS surveillance for all or certain types of sex offenders. I exploit the timing difference in when Jessica's Law was enacted in different states to test its effectiveness in reducing sex crime incidence. I use a Two-Way Fixed Effects (TWFE) model to compare the changes in sex crime rates in states enacting the law in a given year to changes in those rates in other states. Given criticism of the use of TWFE models in case of staggered interventions, I also use the Callaway and Sant'Anna (CS) estimator to test the law's effectiveness (Callaway and Sant'Anna 2021). I use the same techniques to test the effect of the law on the proportion of sex crimes committed against children to test my hypothesis that sexual predators may substitute adults for child victims when the punishment for abusing children becomes more severe.

Part 1 Background

In contrast to the federal laws examined in Chapter One, Jessica's Law was enacted at the state level.[61] Florida enacted the law first following the adduction, sexual abuse, and murder of 9-year old Jessica Lunsford. The Florida law imposed a minimum mandatory sentence of 25 years for "lewd or lascivious molestation against a victim less than 12 years of age" (Davis 2013, p.10) and lifetime electronic monitoring (LEM) for

[61] A federal version of Jessica's Law was introduced in congress but failed to pass.

sex offenders whose victims were under 15 years old (Dierenfeldt and Carson 2017, p.91). Between 2005 and 2014, most states copied Florida's model by adopting their own versions of Jessica's Law.

While versions of Jessica's Law differed slightly from state to state, most included the minimum mandatory sentence of 25 years for child sexual assault introduced by Florida. Differences between the versions of the law in different states included the age that the victim had to be under for minimum mandatory sentence to apply, the sex crime that had to be committed against the child for the law to apply,[62] and whether the law required lifetime electronic monitoring of all or a subset of sex offenders. California's version of Jessica's Law, also known as Proposition 83 and arguably the most severe versions of the law,[63] also included residency restrictions for Sex Offenders.

Some states, including Massachusetts and New Hampshire, passed "partial" versions of Jessica's Law that included either a mandatory minimum sentence of less than 25 years or allowed judges to decide whether to enforce the 25-year sentence. Partial versions of the law typically represented political compromises satisfying Democratic politicians weary of the severity of the laws while still significantly increasing the punishment for child sex abuse. The law also had to be modified in response to opposition in Texas prior to enactment. The 25-year mandatory minimum sentence in Texas' Jessica's Law originally applied to a broader range of sex crimes against children

[62] For instance, in some states the minimum mandatory sentence applied to 1st degree sexual assault against a child, while in others it could include a broader range of sex crimes against children, including statutory rape.
[63] California was the only state for which Jessica's Law required lifetime electronic monitoring for all felony sex offenders.

but only applies to cases where the victim is under 6 or the victim is under 14 and either bodily injury or the use of a weapon or repeated sexual abuse is involved in the final version of the law (Bradley 2007).[64] Political gridlock also affected the timing of the law's enactment. Colorado did not enact its version of Jessica's Law until 2014 as a result of the difficulty that Democratic and Republican politicians had in finding a version of the law supported by both sides of the political spectrum. Figure 9 shows the timing of Jessica's Law enactment across states.

Since enactment, the number of people charged under and affected by Jessica's Law has been substantial. In Massachusetts, 657 cases between 2008 and 2015 involved "one or more of the charges covered under Jessica's Law" and, as of 2016, 127 abusers had received the minimum sentence mandated by Jessica's Law (Redmond 2016). In California, as of 2007, the residency restrictions and GPS monitoring imposed by Jessica's Law applied to 3,500 parolees (Simerman 2007). Jessica's Law did have unintended consequences resulting in subsequent modifications to the law in certain states. The severe residency restriction imposed by California's version of Jessica's Law was relaxed after it led to high rates of vagrancy among sex offenders. In some states, young adults faced the high mandatory minimum sentences imposed by Jessica's Law after having sex with underage peers who were not much younger than them. In response, states have recently passed so-called "Romeo and Juliet Laws" relaxing but not

[64] Texas' version of Jessica's also allowed for the death penalty for repeat sexual assault of a child but this provision of the law was invalidated by the Supreme Court's 2008 decision that the death penalty could only be used in case of murder.

necessarily eliminating the punishments faced by young adults who have a relationship with a child who is not far outside their age range.

Although released sex offenders are tracked far more assiduously than other released criminals as a result of these laws, data does not suggest that they are more likely to recidivate. The Bureau of Justice Statistics tracked recidivism of prisoners, including 9,691 sex offenders from 15 different states, released in 1994 and recidivism of prisoners, including 20,195 sex offenders from 30 different states, released in 2005 (Alper and Durose 2019). As illustrated in Figure 10, the percentage of sex offenders rearrested within 3 years after release was in both cases lower than the percentage of all prisoners rearrested within 3 years after release.[65] Moreover, based on the limited evidence provided by these studies, the federal sex crime laws of the 1990s did not reduce sex offender recidivism, which increased from 43% among sex offenders released in 1994 to 48.9% among sex offenders released in 2005.[66] Such statistics call into question both the necessity and effectiveness of the sex crime laws of the last 30 years but, given the number of variables that could have impacted sex offender recidivism between 1994 and 2005, do not provide conclusive evidence for or against the efficacy of the laws.

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[65] The percentage of sex offenders who were rearrested for a sex crime within 3 years after release was, however, higher than the percentage of all prisoners rearrested for a sex crime within 3 years after release in both cases. For example, among the prisoners released in 1994, 5.3% of the sex offenders were rearrested for a sex crime within 3 years while 1.3% of the non sex offenders were rearrested for a sex crime (Langan, Schmitt, and Durose 2003).

[66] Measured as the % rearrested within 3 years after release. See Langan, Schmitt, and Durose (2003) and Alper and Durose (2019).

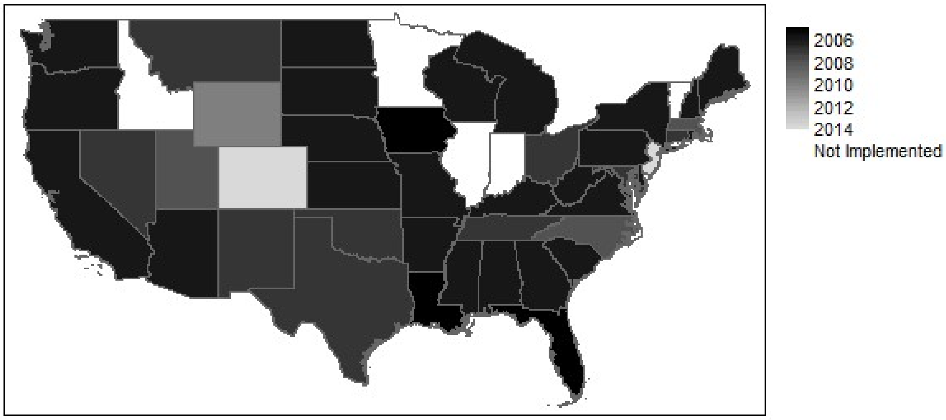

Figure 9 Enactment of Jessica's Law by State

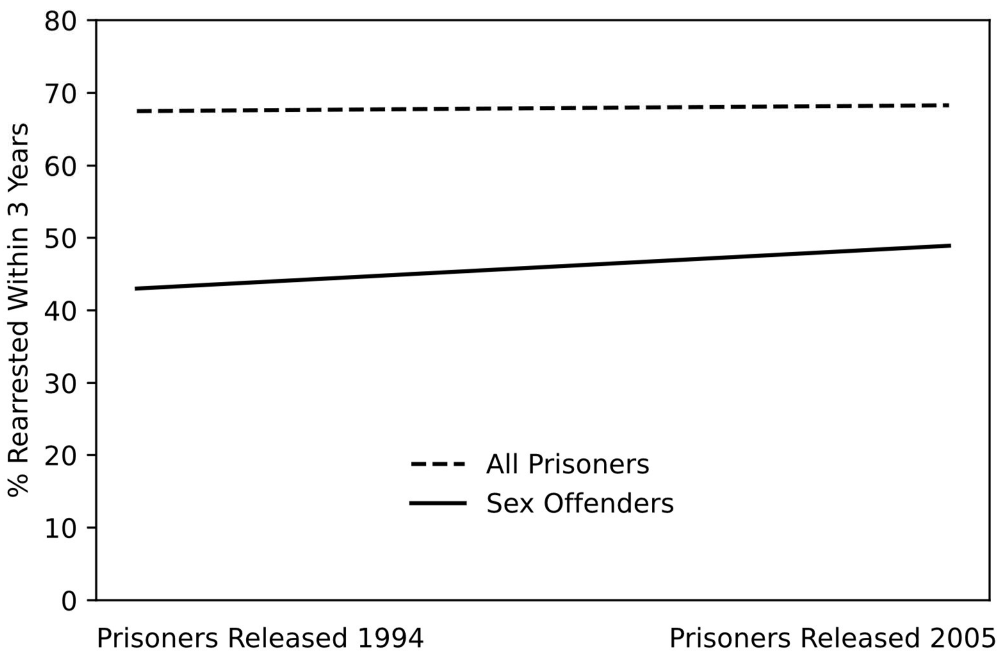

Figure 10 Recidivism Rates of Sex Offenders vs. All Prisoners[67]

[67] The total number of prisoners released in 1994 included in the study was 300,000 (Langan, Schmitt, and Durose 2003), and the total number of prisoners released in 2005 included in the study was 401,288 (Alper and Durose 2019).

Part 2 Literature Review

The only publication that I am aware of focusing on the effects of Jessica's Law does not find evidence for the law's efficacy. Using data from the UCR, Rick Dierenfeldt and Jennifer Varriale Carson (2017) examine the effect of the law on the frequency of forcible rape in 10 states with versions of Jessica's Law that include lifetime electronic monitoring and find no significant effect.

However, their study suffers from significant limitations. UCR data provides no information on the age of the victims of offenses, and the mandatory minimum sentences in Jessica's Law only apply to sex crimes committed against victims under a specified age threshold. Even if Jessica's Law did not reduce the overall frequency of rape, it may still have reduced the frequency of child rape. Secondly, Jessica's Law applies to a broader range of sex crimes than forcible rape in most states and could have affected the overall incidence of sex crimes even if it had no effect on the frequency of forcible rape. Thirdly, I use a different research design than Dierenfeldt and Varriale that I argue will be more effective for evaluating the effects of Jessica's Law. By using both a TWFE model and the CS Estimator, I control for other changes over time potentially affecting sex crime rates, including the imposition of more stringent Federal sex crime laws. Using time series analysis, in contrast, the effect of the enactment of Jessica's Law cannot easily be separated from changes over time affecting all states.

Part 3 Research Design

The hypotheses that I test are that 1) Jessica's Law led to a decrease in sex crime rates, 2) This decrease was strongest for sex crimes against children under 18, and 3)

Jessica's Law led to a decrease in the proportion of sex crimes committed against victims under 18. Jessica's Law could reduce sex crime rates by deterring prospective offenders through the severity of the increased prison sentences it imposes and the social humiliation and loss of privacy resulting from having to wear a GPS anklet. It could further impact sex crime rates by reducing recidivism through the increased surveillance of offenders provided by the electronic monitoring required by the law. Finally, lengthier prison sentences are expected to reduce sex crime rates since offenders will not be able to commit crimes from behind bars, although this incapacitative effect will not be fully captured by the data that I use for my analysis, which only includes 11 years of data following enactment of the law in Florida. I expect these effects to be strongest in case of offenses against victims under 18 because the minimum mandatory prison sentences and, in some cases, lifetime electronic monitoring requirements, applied only to offenses against children. Given increased punishments for offenses against children, I expect sexual predators to substitute adult victims for child victims, leading to a decrease in the proportion of sex crimes committed against children under 18.

To test these hypotheses, I use the same NIBRS dataset as in Chapter 1, which includes 2003-2016 data. Although more states and law enforcement agencies report to the UCR, only NIBRS data identifies the age of the victim of each reported offense. The number of states and law enforcement agencies reporting to NIBRS increased between 2003 and 2016, which I control for in my TWFE model by using agency fixed effects. As the CS method does not allow for fixed effects and non-random changes in what law enforcement agencies report to NIBRS could affect sex crime rates, I use it on a balanced panel subset of my data but also include the results of using the method on my full unbalanced panel in Appendix 2. My NIBRS data includes a total of 40 states, including states enacting Jessica's Law as early as 2005 and as late as 2014. It includes six states that did not enact the law. However, only two of these six states reported to NIBRS consistently and comprehensively between 2003-2016.[68] Table 6 includes information on when each of the 40 states enacted the law, the length of the minimum mandatory sentence imposed by the law, the age that the victim must be under for that sentence to apply, the type of sex crime that the minimum mandatory sentence applied to, and the surveillance requirement imposed by the law, if any.[69]

I count as sex crimes forcible rape, forcible sodomy, sexual assault with an object, forcible fondling, incest, and statutory rape and calculate sex crime rates by dividing the total number of sex crime incidents reported by a law enforcement agency in a given year by the total population covered by that agency and multiplying by 1000. Table 7 includes baseline sex crime rates for each of the states in my dataset for which 2003 data is available.[70]

Table 7 also includes the percentage of sex crimes in each of these states in 2003 against victims under 18. These percentages are higher than might be expected primarily

[68] Washington DC, Idaho, Illinois, Indiana, Minnesota, and Vermont are all included in my NIBRS dataset but, of these six states, only Idaho and Vermont reported consistently and comprehensively to NIBRS.
[69] Data sources for Table 6 include Davis et at. (2013), FindLaw.com, the Rape, Abuse, and Incest National Network, and state statutes from the sources below:
Virginia's Legislative Information System, Arizona State Legislature, Minnesota State Legislature, Illinois General Assembly, Delaware General Assembly, Pennsylvania General Assembly, and Texas District and County Attorneys Association.
[70] Due to the increase in the number of states reporting to NIBRS between 2003 and 2016, not all States in my dataset reported to NIBRS in 2003. Although Maine reported to NIBRS in 2003, it is also excluded from the Baseline Comparison since only one law enforcement agency in the state reported to NIBRS that year, making it impossible to calculate meaningful baseline sex crime rates for the state.

because the majority of victims in cases of forcible fondling, forcible sodomy, sexual assault with an object, and incest are children and all victims of statutory rape are under 18. These percentages are also in line with prior estimates of the age distribution of victims of sexual assault with the Bureau of Justice Statistics finding that "over two-thirds of all victims of sexual assault reported to law enforcement agencies" from 1991 through 1996 were under 18.[71]

Table 6 Jessica's Law in the 40 States in the Dataset

State	Year Enacted	Mandatory Minimum Sentence (years)	Victim Age	Crime	Surveillance
Alabama	2006	20[72]	12	Forcible Rape	Required for Sexually Violent Predators (SVPs)
Arizona	2006	30[73]	13	Sexual Conduct	-
Arkansas	2006	25	14	Rape	10 years for SVPs
Colorado	2014	24*	13	Class 2 Felony Assault	-
Connecticut	2007	25	13	Sexual Assault	-
Delaware	2006	25	14	Sexual Assault	-
DC	-	-	-	-	-
Georgia	2006	25	14	Sexual Assault	Required for Sexually Dangerous Predators for Life
Idaho	-	1	-	Rape	Throughout Probation/Parole for Sexually Violent Offenders
Illinois	-	6	13	Sexual Contact	Throughout Probation/Parole for SVPs
Indiana	-	3*	-	Rape	Required for SVPs
Iowa	2005	17.5	12	Sexual Assault	5 years for Certain Sex Offenders (SOs) as a Condition of Probation/Parole

[71] Specifically, from 1991-1996, the victims of 67% of all sexual assault offenses were under 18. Howard N. Snyder. "Sexual Assault of Young Children as Reported to Law Enforcement: Victim, Incident, and Offender Characteristics." *Bureau of Justice Statistics*. July 2000, 2.

[72] If the victim is under 7, the minimum sentence increases to life imprisonment without the possibility of parole. Rape, Abuse, and Incest National Network (RAINN). http://apps.rainn.org/policy/policy-crime-definitions-export.cfm?state=Alabama&group=3

[73] According to Arizona's version of Jessica's Law, anyone committing sexual assault against or engaging in sexual conduct with a child 12 years old or younger must be confined for at least 35 years. However, this minimum mandatory sentence is conditional on the imposition of a life sentence. If a life sentence is not imposed, a presumptive sentence of 20 years is instead required. Given this complexity, I designate the mandatory minimum sentence as a value between 20 and 35 years in the table above. See Arizona State Legislature. http://www.azleg.gov/legtext/48leg/2r/laws/0097.htm

Kansas	2006	25**	14	Certain Sex Offenses	LEM for Certain Sex Offenses
Kentucky	2006	20*	12	Certain Sex Crimes	Conditional on Court Authorization
Louisiana	2005	25	13	Sex Crimes	LEM for Certain Sex Offenders
Maine	2006	20	12	Gross Sexual Assault	During Supervised Release in Cases of Gross Sexual Assault
Massachusetts	2008	10	16	Rape	Throughout Probation for Certain SOs
Michigan	2006	25	13	1st Degree Sexual Assault	LEM for Sexual Conduct with a Child Under 13
Minnesota	-	12**	13	Sexual Contact	Allowable on Certain SOs
Mississippi	2006	20	14	Sexual Battery	Allowable on Certain SOs
Missouri	2006	30	12	Forcible Rape or Sodomy	LEM Required for Certain SOs
Montana	2007	25	13	Sex-Related Crimes	Required for Certain SOs
Nebraska	2006	15	12	1st Degree Sexual Assault	Authorized for Certain SOs
New Hampshire	2006	25**	13	Sexual Assault	-
North Dakota	2006	20**	15	Gross Sexual Imposition	Required for Certain SOs
Ohio	2007	25	13	Rape	Authorized for Certain SOs
Oklahoma	2007	25	12	Sex-related Crimes	Required for Certain SOs
Oregon	2006	25	12	1st Degree Sex Crimes	LEM Required for Certain SOs
Pennsylvania	2006	10	16	Rape	Authorized
Rhode Island	2006	25	14	1st Degree Sexual Assault	LEM Required for Child Molesters, High-Risk Offenders
South Carolina	2006	25	11	Sexual Conduct	Required for Certain SOs
South Dakota	2006	15	13	Rape	Authorized as Condition for Parole/Probation, not Specific to SOs
Tennessee	2007	25	13	Rape	LEM for Rape of a Child Under 13
Texas	2007	25	6	Rape	Authorized
Utah	2008	25	14	Sex Crimes	-
Vermont	-	10*	13	Sexual Assault	-
Virginia	2006	25	13	Sex Crimes	Required for Certain SOs
Washington	2006	25	15	Rape, Child Molestation	Authorized
West Virginia	2006	25	12	Sexual Assault	Required for SVPs
Wisconsin	2006	25	16	Sexual Assault	LEM for Certain SOs

Note. In some states, the mandatory minimum sentence differs depending on whether the crime involved use of deadly force or a weapon or whether it was a first-time or repeat offense. The mandatory minimums in this table are for 1st time offenses that did not involve use of a weapon or deadly force.
*The listed sentence is a presumptive sentence instead of a mandatory minimum due to no record of a mandatory minimum sentence.
**It is possible to deviate from this mandatory minimum sentence under exceptional circumstances.

Table 7 Comparison of Sample States at Baseline: 2003

State	Sex Crime Rate	Sex Crime Rate Under 18	Sex Crime Rate Under 14	Sex Crime Rate Under 13	% Victims Under 18

Arkansas	.42	.26	.16	.14	62.41
Colorado	.93	.55	.31	.24	58.75
Connecticut	.38	.24	.13	.11	63.77
Delaware	.64	.44	.22	.17	69.17
Idaho	1.29	1.01	.56	.47	78.31
Iowa	.68	.44	.27	.22	64.33
Kansas	1.05	.69	.37	.3	65.58
Kentucky	.95	.58	.38	.33	61.72
Louisiana	.56	.34	.23	.19	61.06
Massachusetts	.52	.29	.16	.12	55.43
Michigan	1.23	.89	.55	.46	72.18
Nebraska	.82	.58	.37	.30	70.55
New Hampshire	1.2	.86	.5	.43	71.59
North Dakota	.77	.49	.25	.2	63.68
Ohio	.62	.37	.22	.18	60.91
Oregon	.14	.1	.05	.04	73.4
South Carolina	1.12	.75	.43	.34	66.73
South Dakota	.53	.38	.17	.14	71.17
Tennessee	.95	.61	.35	.28	64.05
Texas	.8	.57	.33	.27	71.16
Utah	1.57	1.14	.72	.62	72.88
Vermont	.47	.35	.18	.15	74.9
Virginia	.75	.5	.32	.26	66.16
West Virginia	.56	.37	.23	.19	66.03

Note. Baseline data is not available for all of the states in my NIBRS dataset since some did not start reporting to NIBRS until after 2003.

My regression specifications for my TWFE models are:

Equation 8

$$1)\ SexCrimeRate_{i,t} = \beta_1 Jessica'sLaw_{s,t} + \beta_1 Jessica'sLaw_{s,t} \text{ x } Severity_s + \rho_t + \tau_i + X_{i,t} + \epsilon$$

Equation 9

$$2)\ SexCrimeRateUnder18_{i,t} = \beta_1 Jessica'sLaw_{s,t} + \rho_t + \tau_i + X_{i,t} + \epsilon$$

Equation 10

$$3)\ ProportionofVictimsUnder18_{i,t} = \beta_1 Jessica'sLaw_{s,t} + \tau_i + \epsilon$$

where t represents the year, from 2003 to 2016, s represents the state, i represents reporting area within state s, τ represents agency fixed effects, ρ represents time fixed effects, and X represents a vector of controls. Time fixed effects are omitted in the regression specified in Equation 10 based on the expectation that the proportion of sex crime victims under 18 would not change over time for reasons other than the enactment of Jessica's Law. However, this assumption is relaxed in my robustness checks in Part 5. Controls include the number of months included in ORI i's crime reporting in year t and the number of other violent crimes, including murder, robbery, and aggravated assault, per 1,000 people in reporting area i at time t. *Jessica'sLaw* is a dummy variable equal to one if the state had enacted Jessica's Law by or in year t, while *Severity* is a dummy variable equal to one if the state enacted a version of Jessica's Law that included a mandatory minimum sentence of 25 years or more and/or an LEM requirement.[74] I use the interaction term between *Jessica'sLaw* and *Severity* to test whether stringent versions of Jessica's Law were more effective for combating crime than the "partial" versions enacted in states like Massachusetts. A Bacon Decomposition of my TWFE model results created using a balanced panel subset of my NIBRS data is also included in Appendix 2.

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[74] Although Arizona and New Hampshire enacted versions of Jessica's Law with minimum sentences of 25 years or more, I do not categorize their versions of the law as "severe" because they allow for some degree of judicial discretion, and it is possible to deviate from the minimum sentence. I also do not categorize Texas' version of Jessica's Law as severe since the 25 year mandatory minimum sentence only applies to first-time offenders if their victim is under 6, which is a low age threshold relative to other states.

The CS Model is specified in Equation 1 where t is year, Y is the sex crime rate, G_g is a dummy variable that equals one for reporting areas in a state first adopting Jessica's Law in year g, and C is dummy variable equaling one for control group units. Control group units include reporting areas in states that never enacted Jessica's Law. I use the simple aggregation method in my results but cross-validate it with the other two methods and note any differences. For estimating the effect of Jessica's Law on sex crime rates using the CS method, I use the outcome regression model specified in Equation 14 with X as the pre-treatment level of other violent crime rates. I also use the event study model specified in Equation 7 to check for lagged effects of Jessica's Law enactment on sex crime rates where k represents the number of years between time t and Jessica's Law enactment, D is a dummy variable equaling one if the number of years between time t and Jessica's Law enactment in the state that reporting area i is within equals k, T_0 and T_1 are the minimum and maximum number of leads and lags included in the model, γ represents the coefficients on the lead and lag indicators, τ represents reporting area fixed effects, ρ represents time fixed effects, and X represents vectors of the same controls used in Equation 8. For reporting areas in states that did not enact Jessica's Law, k is set to 0.

Even though the research design I use in this paper controls for changes over time affecting my dependent variables and differences between states and reporting areas within states, my results could still be biased if my explanatory variable is endogenous. If states enacted Jessica's Law because they had high sex crime rates, sex crime rates in those states could decrease more rapidly than in other states for reasons other than the enactment of Jessica's Law. States with high sex crime rates may also adopt other measures, including treatment for sex offenders and increased prosecution of sex crime, leading to a decrease in sex crime rates.

The history behind the law suggests that neither enactment of the law nor the timing of its enactment in different states was a response to high sex crime rates. As Agan argues, sex crime laws have usually been "enacted on the basis of high-profile cases" and "not because of a multitude of cases in general" (Agan 2011, p.218). Jessica's Law was no exception. The namesake of the law, Jessica Lunsford, was raped and buried alive in a particularly heinous crime that attracted heightened attention partly because her killer was a convicted sex offender. Media pressure also played a role in enactment of the law. Bill O'Reilly, formerly the host of the O'Reilly Factor on the Fox News Channel, became a staunch advocate of the law and used his show to highlight which states had not yet enacted it and publicly criticize the politicians in those states who he believed were responsible for the delay. Finally, lobbying plays a significant role in the enactment of sex crime laws, including Jessica's Law. The lobbying efforts of John Walsh, father of the murder victim Adam Walsh and formerly the host of America's Most Wanted, helped lead to the enactment of Megan's Law and the Adam Walsh Act. Mark Lunsford, the father of Jessica Lunsford, lobbied for Jessica's Law after his daughter's murder.

Baseline values of the dependent variables in Table 7 also do not suggest that states enacted Jessica's Law in response to high sex crime rates. Specifically, Idaho, which did not enact Jessica's Law, had the second highest sex crime rate in 2003 of the states that reported to NIBRS that year while Colorado, which did not enact the law until 2014, also had a relatively high sex crime rate in 2003.

Part 4 Results

Figure 11 shows the sex crime rate between 2003 and 2016 based on data from all states that reported to NIBRS during those years. Although it shows a downward trend in sex crime rates following 2005, the first year that Jessica's Law was enacted, a review of Figure 12, which shows sex crime rates plotted separately in each state, shows that this downward trend was not most pronounced in states that enacted Jessica's Law. Instead, two of the states with the steepest downward trends are Idaho and Vermont, neither of which enacted Jessica's Law. The sex crime rate decreased by 31% in Idaho between 2006 and 2016. During the same timespan, it increased by 17% in Rhode Island, which enacted a version of Jessica's Law that included a lifetime electronic monitoring requirement.

The regression results in Table 8 provide further evidence for the ineffectiveness of Jessica's Law in controlling sex crimes. Based on the TWFE models, Jessica's Law has a statistically significant effect on neither the overall sex crime rate nor the rate of sex crimes with victims under 18 years old. The coefficient on the interaction term between *Jessica's Law* and *Severity* in the second column of Table 8 is statistically insignificant and positive, suggesting that even the more stringent versions of Jessica's Law are ineffective. A Bacon Decomposition of these estimates is provided in Table 20.[75] The

[75] The results of the Bacon Decomposition in Table 20 suggest that the TWFE estimates are not entirely reliable. Given the small number of states that never enacted Jessica's Law in the dataset, the overall estimated treatment effect is based mostly on 2x2 Difference in Difference (DD) estimates in which already treated units comprise the control group.

positive and statistically significant treatment effects estimated using the CS Method in Table 8 suggest that Jessica's Law increased sex crime rates. This result is likely due to the fact that the control group when using this method consists entirely of states that never enacted Jessica's Law,[76] of which only Idaho and Vermont[77] reported consistently and comprehensively to NIBRS between 2003 and 2016. As previously discussed and shown in Figure 12, sex crime rates in these two states shows a clear downward trend relative to other states. Based on a 2008 survey of sex offender treatment policies across states by the Vera Institute of Justice (Daly 2008), Idaho and Vermont invest heavily in treatment and risk assessment of sex offenders, suggesting that these methods of combating sex crimes may be at least, if not more, efficacious than harsher punishment.[78] Similarly, the event study plot in Figure 13 shows evidence of a positive effect of Jessica's Law on sex crime rates.[79] In my robustness checks in Part 5, I test the effect of

[76] It is also possible to use units that have not yet been treated as the control group when using the CS method in cases where all or almost all units are eventually treated. Use of not yet treated units as the control group does not significantly change the results shown in Table 8 in this case. However, use of not yet treated units as the control group combined with the removal of Idaho from the dataset does change the results, causing the effect of Jessica's Law on the sex crime rate estimated using the CS method to be statistically insignificant. This change suggests that Idaho could be an outlier, in response to which I use UCR data that includes a larger number of states that never implemented Jessica's Law in Part 5.
[77] However, Vermont is not included as a control in the balanced panel subset of the NIBRS data used to estimate the results of the CS method in Table 8 due to missing data for Vermont in 2015. It is included in the full unbalanced panel used to estimate the results of the CS method in Table 19.
[78] This idea is further explored in Chapter 3. Specifically, as of 2008, Vermont was one of only five states that developed a customized actuarial risk assessment tool for predicting sex offender recidivism, the Vermont Assessment of Sex Offender Risk (VASOR), and the only state to create an actuarial tool to develop individualized treatment plans for sex offenders and assess treatment progress (Daly 2008, p.10). Idaho and Vermont also provide treatment for imprisoned sex offenders and sex offenders released on probation or parole and reentry services for sex offenders released from prison (Daly 2008).
[79] One possible explanation for this estimated positive effect is that enactment of Jessica's Law incentivized police to more assiduously investigate sex crimes. News coverage of the law, in particular, could have focused their attention on sex crimes, and a resulting increase in investigatory activity could have led to more records of sex crimes in NIBRS. However, the negative relationship between Jessica's Law enactment and rape clearance rates discussed and demonstrated in Part 5 makes this interpretation doubtful.

Jessica’s Law using UCR data, which allows for a larger number of control states since more states report to UCR than NIBRS.

The results provide more support for my third hypothesis. As shown in Table 8, Jessica’s Law is estimated to lead to a 2.4 percentage point decrease in the proportion of sex crime victims under 18 in a result that is statistically significant at the .01 level, suggesting that the harsher sentences imposed by Jessica’s Law for crimes against children may cause offenders to substitute adults for child victims even if the law does not reduce sex crimes overall. Figure 14 shows a steady and steep drop in the proportion of sex crimes victims under 18 between 2005, the year that Jessica’s Law was first enacted in Florida, and 2016. This drop is notable partly because NIBRS data suggests that the proportion of sex crime victims under 18 stayed relatively stable between the 1990s and 2003, equaling 67% for both the period between 1991 and 1996 (Snyder 2000, p.2) and 2003.

As shown in Table 9, unlike Jessica’s Law, higher clearance rates and increases in the percent of crimes resulting in arrest are associated with lower sex crime rates. This finding is in accordance with theories that higher probabilities of arrest deter criminals more than more severe punishments. While highly statistically significant, this effect is not very strong in magnitude with a 10-percentage point increase in the percent of sex crime incidents resulting in arrest associated with a .9% decrease in sex crime rates. I cross-validate this finding using 2003-2018 UCR data by testing the effect of clearance rates for Rape on the Rape Rate and find that this result is consistent between the two data sets, as the estimated effect of higher clearance rates on the rape rate shown in Table 9 is similar in magnitude. Both results are also robust to a non-logarithmic specification.

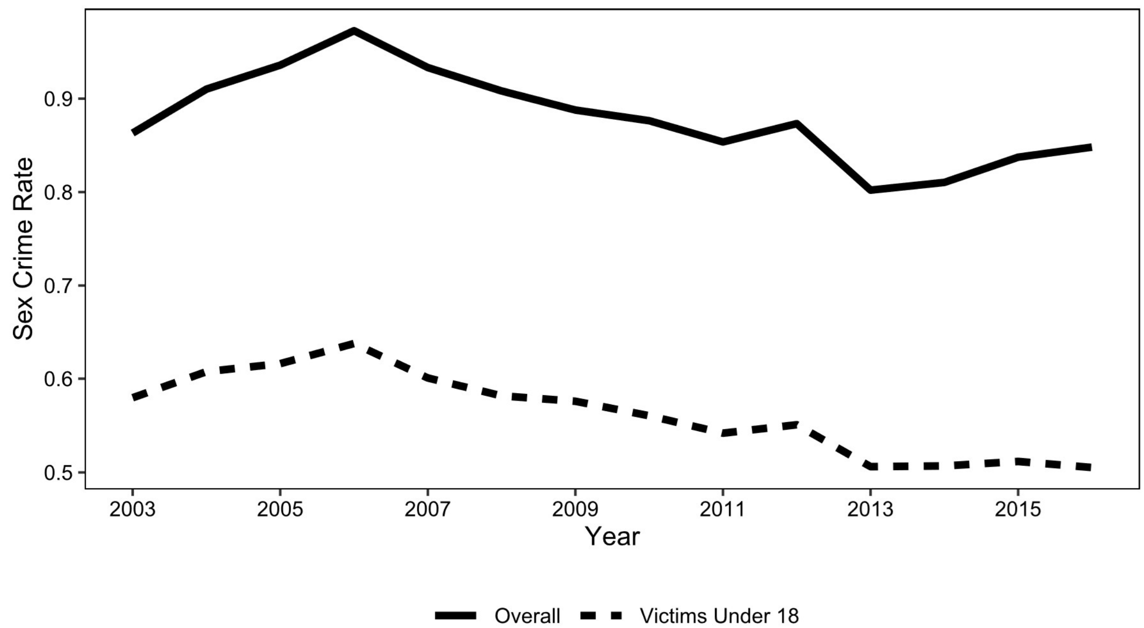

Figure 11 Sex Crime Rate Between 2003 and 2016 Based on NIBRS Reporting[80]

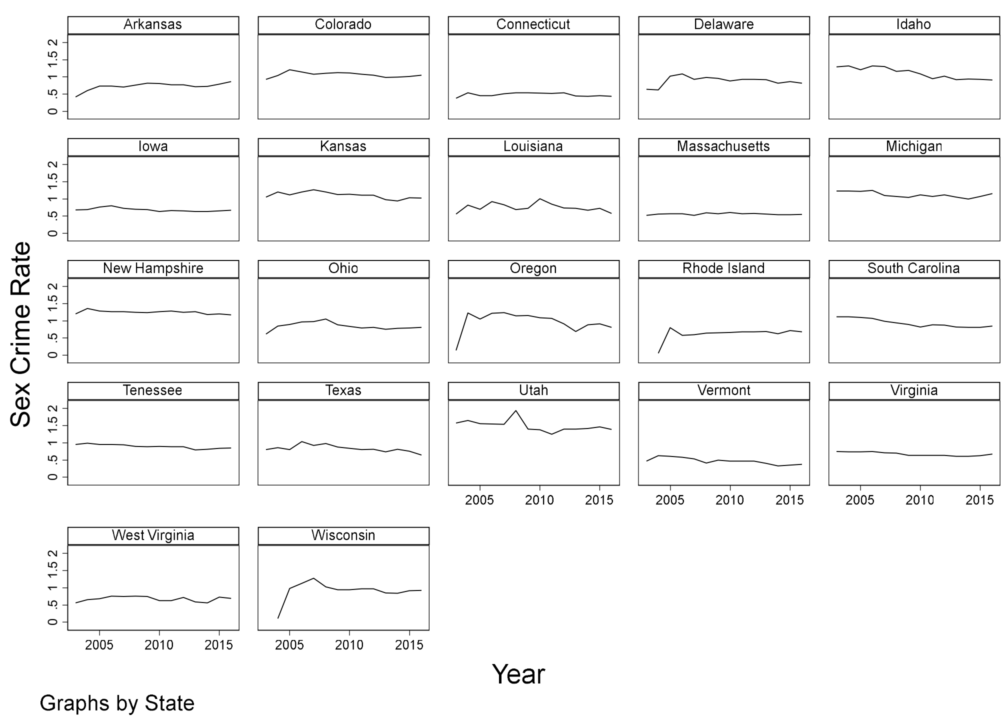

Figure 12 Sex Crime Rates Across 22 States[81]

Table 8 Impact of Jessica's Law

FE Models

	Sex Crime Rate	Sex Crime Rate	Sex Crime Rate Under 18	Proportion of Victims Under 18
Jessica's Law	.001(.049)	-.021(.066)	.014(.013)	-.024(.003)***
Jessica's Law*Severity		.037(.034)		
Other Violent Crime Rates	Yes	Yes	Yes	No
ORI Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	No
Dependent Variable Mean	.715	.715	.481	.651
#Obs	64,243	64,243	64,243	53,641
#States	39	39	39	40
#ORI Reporting Areas	6,222	6,222	6,222	6,438
Years	2003-2016	2003-2016	2003-2016	2003-2016

CS Estimator

	Sex Crime Rate	Sex Crime Rate	Sex Crime Rate Under 18	Proportion of Victims Under 18
ATT	.161(.043)***		.168(.039)***	
Other Violent Crime Rates	Yes		Yes	
Dependent Variable Mean	.858		.584	
#Obs	26,404		26,404	
Years	2003-2016		2003-2016	

Note. Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. Sex crimes are defined as forcible rape, sodomy, sexual assault with an object, fondling, incest, and statutory rape. Sex Crime Rate Under 18 refers to the rate of sex crimes with victims under 18 years old. Proportion of Victims Under 18 refers to the proportion of the victims of these sex crimes that were under 18 years old. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 9 Effect of Clearance Rates on Sex Crime Incidence

	Log Sex Crime Rate	Log Rape Rate
%Resulting in Arrest	-.0009(.0001)***	
%Cleared		-.0007(.0003)**
Other Violent Crime Rates	Yes	Yes
ORI Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes
#Obs	48,531	119,622

[80] Based on sex crime data from all states that reported to NIBRS between 2003 and 2016.
[81] The 22 states included in this figure were selected based on the comprehensiveness and consistency of their reporting to NIBRS between 2003 and 2016.

#States	39	51
#ORI Reporting Areas	5,646	13,119
Years	2003-2016	2003-2018

Note. Numbers in parentheses are standard errors clustered by ORI. %Resulting in Arrest is the percent of sex crime incidents in NIBRS data resulting in one or more arrest(s). An incident is defined as one or more offenses committed by one or more offenders at the same time and place in which each offender commits each offense. See the note below Table 8 for how sex crimes are defined. %Cleared is the percent of rapes cleared by arrest based on UCR data. See the note below Table 1 for how rape is defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

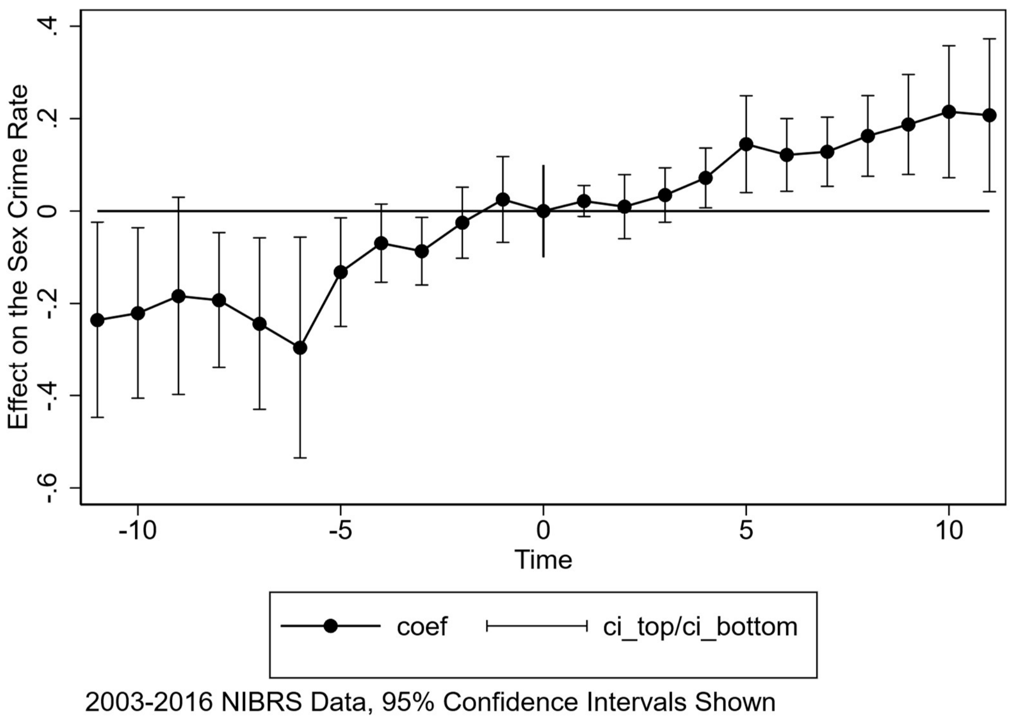

Figure 13 Effect of Jessica's Law on the Sex Crime Rate

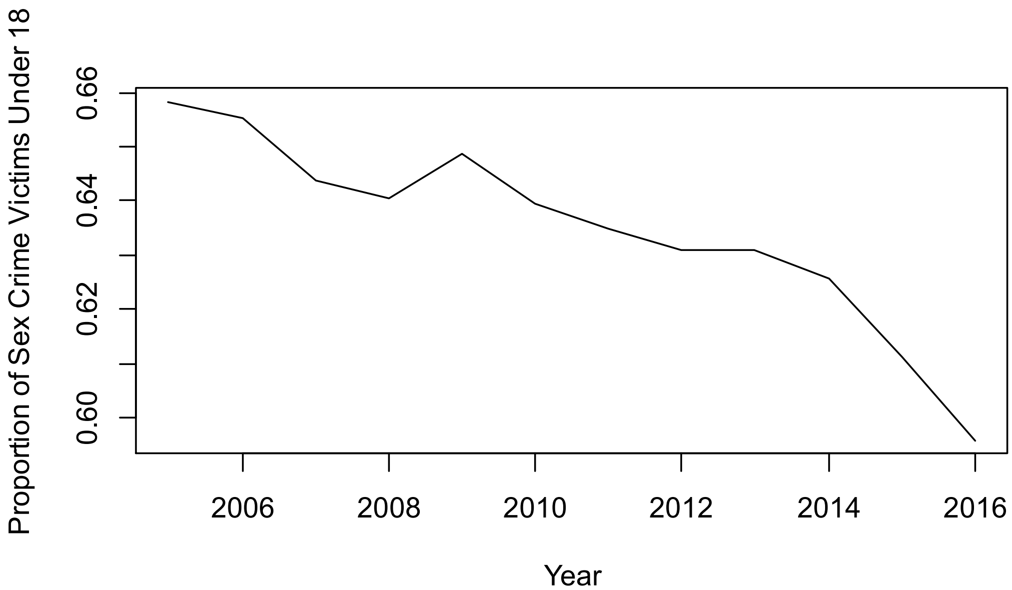

Figure 14 Proportion of Sex Crime Victims Under 18

Part 5 Robustness Checks

Given that Vermont and Idaho were the only states that did not enact a version of Jessica's Law and reported consistently and comprehensively to NIBRS between 2003-2016, I reexamine the effectiveness of Jessica's Law for controlling sex crimes using UCR data. Although, as discussed in Part 2, UCR data provides less information about each crime incident and does not include incidence data for sex crimes other than rape, it includes reporting from a larger number of states. Sex crime rates showed a downward trend in both Vermont and Idaho between 2003 and 2016, as shown in Figure 9, which could have caused me to underestimate the effectiveness of Jessica's Law in Part 4 if these states represent outliers. Especially in case of Idaho, this possibility is not unfounded since, as shown in Table 7, the state had the second highest sex crime rate at baseline in my NIBRS data set, making a steep decline potentially more likely than in states with lower baseline sex crime rates.

UCR data, in contrast, includes consistent reporting by six states[82] that did not enact a version of Jessica's Law. Nevertheless, it does not provide evidence for Jessica's Law's effectiveness. My results in Table 10 below from using the CS method with balanced panel subset of the UCR data also indicate that the law has a statistically significant effect on neither the Rape Rate nor the Sex Offense Arrest Rate. The results of my TWFE regressions in Table 22 also indicate that the law has a statistically significant effect on neither the Rape Rate, nor the Sex Offense Arrest Rate.

Unlike in Part 4, I also do not find evidence that Jessica's Law increases sex crime incidence based on UCR data and conclude that the law has a null effect. The estimated effect of Jessica's Law on the rape rate is positive but statistically insignificant, while the estimated effect of Jessica's Law on the Sex Offense Arrest Rate is negative and, as shown in Table 21, statistically significant if I use the CS Method with the full unbalanced panel of UCR data. However, given that this result is not robust to use of the balanced panel subset of the UCR data and Table 10 below shows evidence of a negative relationship between Jessica's Law enactment and the percent of rapes cleared by arrest,[83] it does not provide convincing evidence for the effectiveness of Jessica's Law. In fact, the negative relationship between clearance rates and Jessica's Law enactment casts

[82] The six states are Minnesota, Vermont, Idaho, Illinois, Indiana, and Hawaii.

[83] The Sex Offense Arrest Rate could decrease as a result of either a decrease in sex crimes or a decrease in the percent of sex crimes resulting in arrest. As such, a negative relationship between Jessica's Law enactment and the percent of rapes cleared by arrest suggests that an estimated negative effect of Jessica's Law on sex crime arrest rates could be due to a decrease in the percent of sex crimes resulting in arrest instead of a decrease in sex crime incidence.

further doubt on the law's effectiveness since part of the purpose of the increased surveillance required by Jessica's Law is to make it easier to track and clear sex crimes.

Table 10 Effect of Jessica's Law on Rape and Sex Offense Arrest Rates

	Rape Rate	Sex Offense Arrest Rate	Percent of Rapes Cleared
ATT	.01(.015)	-.038(.031)	-6.512(1.777)***
Other Violent Crime Rates	Yes	Yes	No
Dependent Variable Mean	.279	.226	41.11
#Obs	115,664	55,072	45,232
#States	50	45	50
Years	2003-2018	2003-2018	2003-2018

Note. Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. Percent of Rapes Cleared is the percent of rapes cleared by arrest. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

For my final robustness check, I relax the assumption that the proportion of sex crime victims under 18 would not have changed for reasons other than the enactment of Jessica's Law and include time, as well as reporting agency, fixed effects in my model. As shown in Table 11 below, my finding that Jessica's Law led to a decrease in the proportion of sex crimes against victims under 18 is not robust to the inclusion of time fixed effects or the CS estimator even though the coefficient on Jessica's Law remains negative in my TWFE model.

This finding suggests a need to look for other potential causes of the steep downward trend in the proportion of sex crime victims under 18 shown in Figure 14. Figure 15 appends Figure 14 by including pre-2005 data from NIBRS on the proportion of sex crime victims under 18. While this proportion remained relatively stable in the 1990s and very early 2000s, a downward trend is evident prior to when Jessica's Law was first enacted in Florida in 2005.

As shown in Figure 15, the onset of the downward trend aligns more closely with the creation of state internet sex offender registries, which first appeared in 6 states in 1997 and had been created in 18 states by the end of the 1990s. Internet registries could cause the proportion of sex crime victims under 18 to decrease if they are more effective at controlling sex crimes with child than adult victims. This hypothesis is plausible since parents of minors are far more likely to check online sex offender registries than the general public,[84] sex offender registration and notification requirements are more stringent for offenders whose victims were minors,[85] data from sex offender registries is frequently publicized by nonprofit groups focused on combating child abuse including the National Center for Missing and Exploited Children (NCMEC) and Parents for Megan's Law (PML), and residency restrictions are used to keep registered sex offenders away from areas frequented by children, including schools.

As shown in Table 11 below, the TWFE model provides evidence that internet registries lead to a one percentage point decrease in the proportion of sex crime victims under 18 in a result that is statistically significant at the 10% level but not robust to the CS method. Other factors, including increasing awareness and coverage of the problem of

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[84] Based on a 2005 poll, 36% of parents of children under 18 have checked online state sex offender registries versus 23% of Americans overall (Saad 2005).
[85] Based on the 2006 Federal Adam Walsh Act, offenders whose victims were minors have to register as a sex offender for a longer duration than those whose victims were adults. Those whose victims were under 13 face the most stringent registration and notification requirements (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006]).

child sexual abuse in the news media and elsewhere, could have also contributed to the relatively steep and consistent downward trend depicted in Figure 15, for which other potential causes are a topic for further research.

Table 11 Effect of Jessica's Law and Internet Registries on the Proportion of Victims Under 18

			TWFE Models
	Proportion Under 18	Proportion Under 18	Proportion Under 18
Jessica's Law	-.004(.007)	.003(.008)	-
Jessica's Law*Severity	-	-.011(.007)	-
Internet	-	-	-.013(.007)*
ORI Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Dependent Variable Mean	.651	.651	.656
#Obs	53,641	53,641	67,218
#States	40	40	40
#ORI Reporting Areas	6,438	6,438	6,569
Years	2003-2016	2003-2016	1995-2016
			CS Estimator
ATT (Jessica's Law)	.022(.014)		-
ATT (Internet)	-		.005(.02)
Dependent Variable Mean	.67		.705
#Obs	21,938		4,904
Years	2003-2016		1996-2003

Note. Numbers in parentheses are standard errors clustered by ORI. Proportion of Victims Under 18 refers to the proportion of the victims of sex crimes that were under 18 years old. See the note below Table 8 for how sex crimes are defined.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

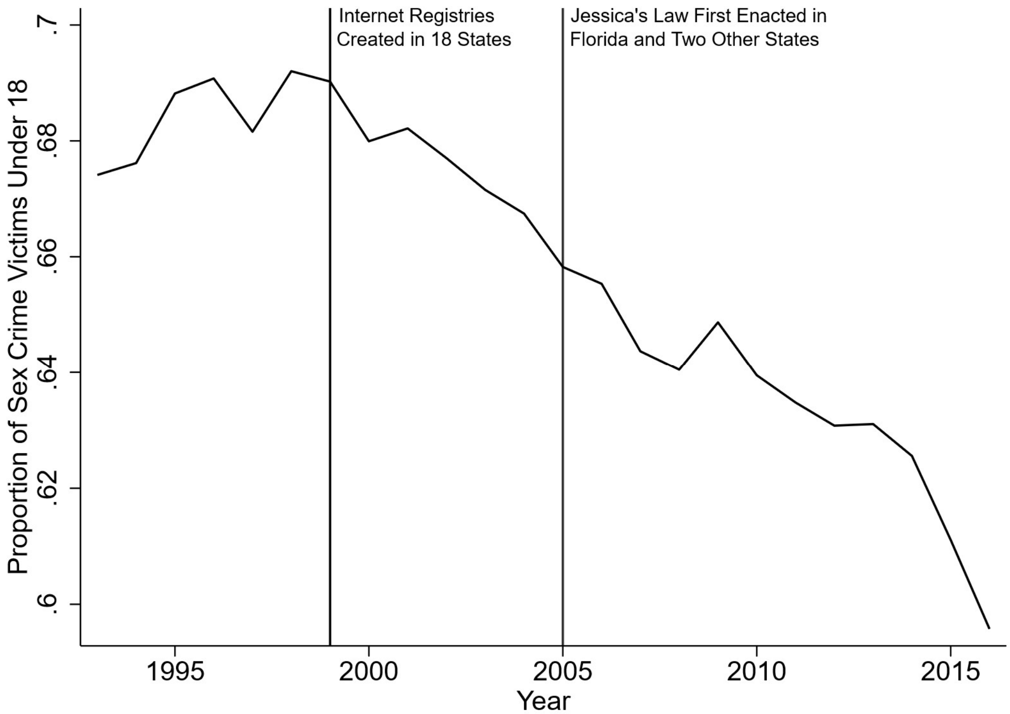

Figure 15 Proportion of Sex Crime Victims Under 18 From 1993 to 2016

Conclusion

I do not find evidence for the effectiveness of Jessica's Law in controlling sex crimes. As I find strong evidence that higher clearance rates for sex crimes reduce sex crime incidence, my results are in accordance with theory that increasing the probability of arrest controls crime more effectively than more stringent punishments for offenders. From a policy perspective, Jessica's Law is hard to justify given its high costs and the lack of evidence for its effectiveness. The satellite tracking equipment required by Jessica's Law alone costs "$15 to $20 per person per day." [86] Considering the enormous

[86] There has been suspicion that GPS and tracking companies have helped encourage enactment of Jessica's Law since they have a lot to gain from it financially given these high costs. The association of Mark

additional costs involved in keeping sex offenders in prison for longer, the law should offer a more demonstrable positive social benefit in order to justify the costs that it imposes.

I find evidence that the mandatory minimum sentences imposed by Jessica's Law cause offenders to substitute adult for child victims by making the punishment for child sexual assault more severe, contributing to a decrease in the proportion of sex crime victims under 18. However, the results of my robustness check suggest that other factors, including the creation of online State Sex Offender Registries, which parents of minors are far more likely to utilize than the general public, may have also contributed to the steep and steady downward trend in the 2000s and 2010s in the proportion of sex crimes with child victims.

My research design suffers from limitations. The effect of the lengthier prison sentences mandated by Jessica's Law on sex crime rates may not be fully realized for years to come. With data covering only 11 years[87] following initial enactment of the law in Florida, I am able to estimate the deterrent effect of the law but not fully account for its incapacitative effect on offenders who may not be able to commit a crime in the future as a result of being incarcerated for a longer period of time than they otherwise would have been.

Lunsford, father of the namesake of Jessica's Law and lobbyist for the law, with these companies has helped give rise to this suspicion (Sheppard 2011).

[87] Or 13 years, in case of the UCR data used in Part 5, which is through 2018.

CHAPTER 3 TREATMENT AS AN ALTERNATIVE TO MORE STRINGENT CRIME LAWS: AN EXAMINATION OF SUBSTITUTE APPROACHES TO COMBATING CRIME

Research in Law and Economics has produced a rich body of work evaluating the effectiveness of crime laws, including the effect of capital punishment on the murder rate (Ehrlich 1975), the deterrent effect of the three strikes laws introduced in the 1990s (Tabarrok and Helland 2007), and the effect of sex offender registration and notification laws on sex crime incidence and sex offender recidivism (Agan 2011; Prescott and Rockoff 2011). Less attention has been paid to the effectiveness of treatment for offenders in controlling crime.

This paper uses data on policies aimed at combatting sex crimes across states to demonstrate that the stringency of sex crime laws and the level of treatment provided to sex offenders are negatively correlated, suggesting that the two methods for controlling crime are substitutes. This finding has econometric implications for studies examining the effectiveness of crime laws. If the severity of punishments and the level of treatment provided for offenders are negatively correlated, studies that do not control for treatment provision could underestimate the effectiveness of increasing the punishments for offenders. While past studies have controlled for other variables impacting crime rates, including the probability that a crime will result in arrest,[88] no study to my knowledge has included treatment provision for offenders as an explanatory variable.

[88] For example, Ehrlich (1975) includes clearance rates as an explanatory variable.

This paper then examines the economic and political factors causing policymakers to favor either treatment or stricter crime laws. Facing a fixed budget constraint, policymakers choose to allocate funding between these alternative methods of combating crime. Political party, high profile crime events attracting media attention, and the outcome of past policy experiments across states are all examined as factors influencing this choice.

Part 1 Background

The tradeoff faced by policymakers between increasing the severity of punishments for offenders and the likelihood of their arrest has been frequently discussed in scholarship in Law and Economics (Becker 1968; Friedman 2000; Miceli 2017; and Ulen and Cooter 2012). Apprehension and punishment are both necessary to deter crime but are also costly (Friedman 2000, p. 225), making finding the optimal level of punishment and probability that a crime will be apprehended a complex policy question. One cost effective solution is to make punishments more severe while reducing the probability of apprehension, as was practiced in eighteenth and nineteenth century Anglo-Saxon countries (Becker 1968, p.184). Appendix 3 Part A suggests that sex crime laws in the US, the focus of this paper, may also have become more severe partly over the past 30 years to compensate for a relatively low and decreasing probability that perpetrators will be punished.

Less attention has been paid to treatment for offenders as an alternative method for combatting crimes. The influence of the rational choice theory (RCT) helps account for this lack of emphasis on treatment. As articulated by Becker (1968, p.170), RCT assumes that, instead of being the result of "psychological inadequacies," crime is in the rational self-interest of those who commit it. Offenders commit crime because their expected utility from committing an offense is higher than the utility they could obtain from an alternative use of their resources (Becker 1968, p.176). Based on this theory, increasing the severity of punishments and probability of arrest for offenses decrease the expected utility of committing those offenses and could therefore be effective ways of deterring crime. In contrast, treatment is unlikely to be effective if criminals are rational and not driven by psychological or moral deficiencies.

Recent research in Law and Economics has paid more attention to treatment as an alternative approach to reducing crime. Blattman et al. (2017) provides experimental evidence showing that cognitive behavioral therapy was effective for reducing criminal behavior among high-risk young men in Liberia, though this effectiveness only persisted if the therapy was accompanied with a cash grant. In contrast to RCT, this result suggests that deficiencies in noncognitive skills, including self-control and the regulation of emotions, help explain criminal behavior and remediating these deficiencies through treatment can reduce crime (Blattman et al. 2017, p.1166). Similarly, Vogler (2020) and He and Barkowski (2020) exploit the fact that only a subset of states chose to expand Medicaid coverage under the Affordable Care Act (ACA) to use difference in difference analysis to demonstrate that this expansion reduced crime. Vogler (2020, p.1171) hypothesizes that this effect could be due to increased access to treatment for prospective or past offenders, though He and Barkowski (2020, p.265) also point out that Medicaid expansion also increases the opportunity cost of crime since imprisonment will lead to the loss of Medicaid benefits.

In research on approaches to combating sex crimes, both RTC and deficiencies in noncognitive skills have been used to explain sexual offending. Borrowing directly from Becker (1968), Prescott and Rockoff (2011, p.168) model sex offenders as rational utility maximizers whose likelihood of offending will decrease with more severe punitive consequence or a higher probability of arrest. Other research emphasizes explanations for sexual offending that can be addressed through treatment, including psychological deficiencies and lack of emotional control (Ward et al. 2005, p.15). Consistently, evidence has been mixed as to whether more punitive measures or treatment are more effective at combating sex crimes. The effectiveness of the former is examined in the first two chapters of this dissertation while that of the latter is reviewed briefly in Appendix 3 Part B.

Given these different models of criminal behavior and their different implications about what methods of crime control will be most effective, the choice by policymakers between these methods and how they make that choice is a fruitful area of research and the focus of this paper.

Part 2 Empirical Strategy

This paper uses data on policies across states for combating sex crimes to provide evidence that the provision of treatment for offenders and increasing the probability of arrest are substitute methods for combating crime and negatively correlated across states. To examine the relationship between the stringency of sex crime laws and the level of treatment provision for sex offenders, I use the Vera Institute of Justice's 2008 survey on sex offender treatment provision across states (Daly 2008) to create multiple measures of treatment provision. The value of these measures across states is summarized in Table 26. States differ in the level of treatment that they provide for both imprisoned sex offenders and sex offenders on probation or parole and whether they provide reentry services for sex offenders reentering society after imprisonment. They also differ in whether funding[89] is available for the entire or partial cost of this treatment. State treatment for SOs is a variable that I created to combine the other measures of treatment provision that takes on a value between 0 and 3 depending on whether funding for community-based treatment for sex offenders on parole or probation, reentry services specifically for sex offenders,[90] and/or treatment for imprisoned sex offenders are available in the respective state.

I also create multiple measures of sex crime law stringency across states. States differ in the severity of the state sex crime laws that they have enacted. Most states enacted a version of Jessica's Law, a law first enacted in Florida in 2005 establishing minimum mandatory sentences for sex crimes against children and surveillance requirements for sex offenders. However, states that enacted Jessica's Law differ in the length of the minimum mandatory sentence[91] imposed by the law and whether it includes lifetime electronic monitoring (LEM) for sex offenders.

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[89] Funding can include state, federal, insurance, provider or grant funding. In the absence of funding, sex offenders are responsible for paying for treatment on their own.
[90] Some states offer reentry services for all prisoners upon their release. The measure used in the correlation matrix refers instead to reentry programming specifically for sex offenders.
[91] See Footnote 29 for how I define minimum sentence.

States also differ in whether and how proactively they have implemented federal laws aimed at combating sex crimes, including when they created sex offender registries, as required by the 1994 Jacob Wetterling Act, made those available to the public, as required by the 1996 Megan's Law, and posted those registries on the internet, as required by the 2003 PROTECT Act. They also differ in whether they have implemented the Sex Offender Registration and Notification Act (SORNA), Title 1 of the 2006 Adam Walsh Act, which broadened and standardized between states the criteria for inclusion on sex offender registries. Data on sex crime policies across states is from a combination of sources, including state statutes, reports on States' compliance with sex offender registration and notification laws issued by the DOJ Office of Sex Offender Sentencing, Monitoring, Apprehending, Registering, and Tracking (SMART).[92] The values of my measures of sex crime law stringency across states are summarized in Table 12 and Table 25.

Part 4 Results

As shown in the correlation matrix depicted in Figure 16, which incorporates data for each of these measures across all 50 states and DC,[93] states with more stringent sex crime laws are less likely to invest heavily in treatment for sex offenders. States with higher minimum mandatory sentences for sex crimes are less likely to have both funding

[92] Data sources include Prescott and Rockoff (2011), Agan (2011), Davis et al. (2013), Krauss et al. (2015), FindLaw.com, the Rape, Abuse and Incest National Network, and state statutes from the sources below: Virginia's Legislative Information System, Arizona State Legislature, Hawaii State Legislature, Alaska Legal Resource Center, Minnesota State Legislature, Illinois General Assembly, Delaware General Assembly, New York State Assembly, and Pennsylvania General Assembly.

[93] Data for some of the treatment measures was unavailable for certain states, as is clear from Table 26. In these cases, correlations are based on all data for all states for which data was available.

available for community-based treatment for sex offenders and reentry services specifically for sex offenders after their release from prison. They also are likely to have a relatively smaller percent of both imprisoned sex offenders and sex offenders released on parole or probation in treatment. The same trend is evident for states that have enacted Jessica's Law and, to lesser extent, implemented SORNA, which is less punitive in nature than Jessica's Law. States that proactively implemented their registries relatively earlier are also likely to have fewer imprisoned sex offenders or sex offenders in probation or parole in treatment and less likely to have reentry services specifically for sex offenders. As a specific example, Florida was the first state to enact Jessica's Law and one of the first to implement SORNA but, as of 2008, provided neither treatment to any of the sex offenders in its prisons, funding for community-based treatment, nor reentry services specifically for sex offenders (Daly 2008, p.56-57). Similarly, California enacted the most severe version of Jessica's Law but, as of 2008, provided no treatment for sex offenders in any of its prisons (Daly 2008, p.38).

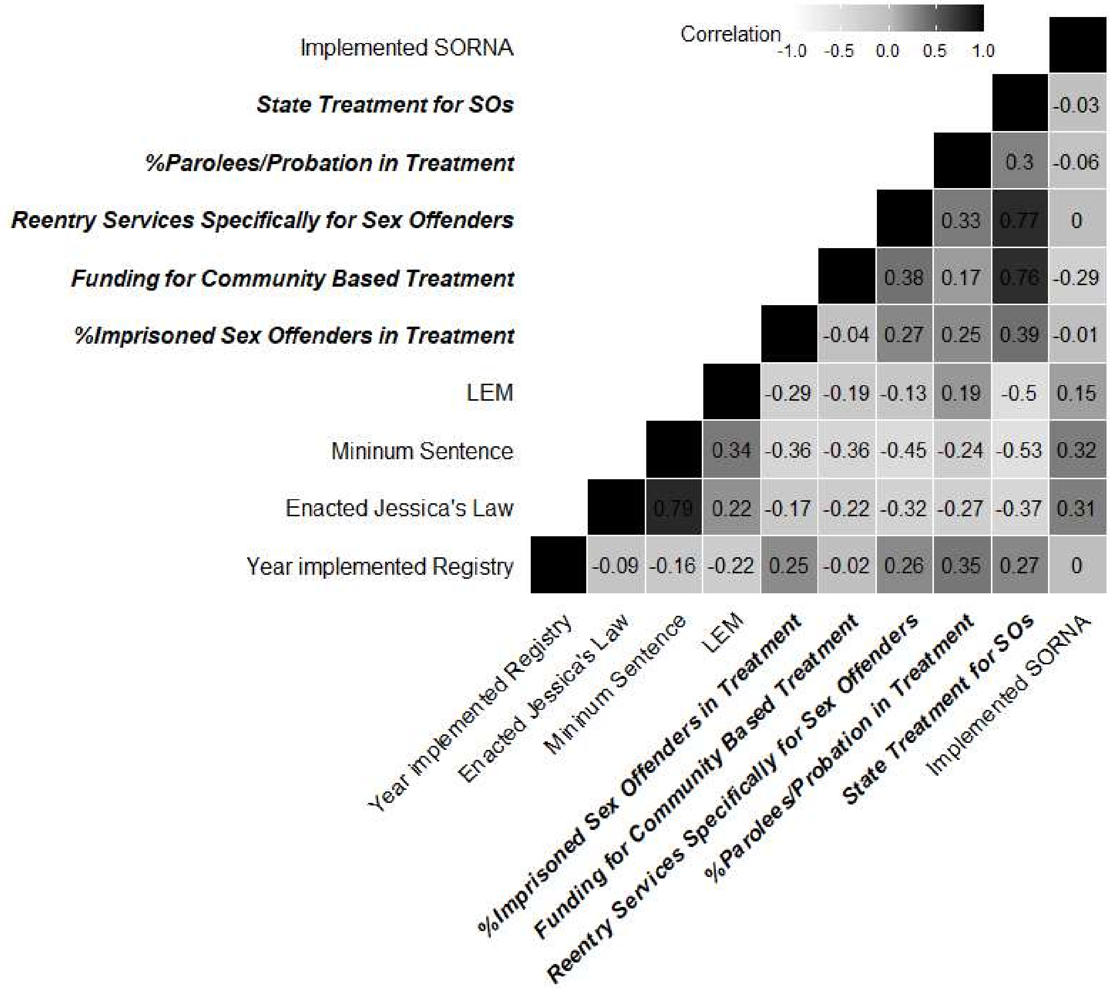

Figure 16 Correlations Between Different Measures of SO Treatment Provision and the Severity of Sex Crime Laws Across States

States also show consistency in the stringency of their sex crime laws and their reliance on treatment. As shown in the correlation matrix, states that implemented SORNA have higher minimum sentences for sex offenders and are more likely to have LEM requirements for sex offenders and have enacted a version of Jessica’s Law. States that implemented their registries earlier have higher minimum sentences for sex offenders and are more likely to have enacted Jessica’s Law and have LEM requirements for sex offenders. Similarly, states providing treatment for imprisoned sex offenders are more likely to provide treatment for sex offenders on probation or parole and reentry services for sex offenders reentering society after imprisonment. These correlations suggest that some states are consistently relatively more likely to rely on punishment in combating sex crimes, while others show a greater reliance on treatment.

This negative correlation between the provision of treatment for sex offenders and the stringency of sex crime laws across states has econometric implications. No past study on the effects of crime laws or other punitive crime control measures to my knowledge has included the level of treatment provision as an independent variable. Given this negative correlation, such an omission could cause the effectiveness of crime laws to be underestimated if, in a subject examined in more detail in Appendix 3 Part B, treatment is effective. Similarly, the effectiveness of treatment could be underestimated if punitive measures are not controlled for and, in a subject examined in more detail in the first two chapters of this dissertation, those measures are effective.

More broadly, these results suggest that modeling the imposition of crime laws as an exogenous change is problematic. More stringent crime laws represent only one of the arsenal of tools available to policymakers to control crime. The decision about what crime laws to impose is instead part of a larger optimization problem in which policymakers facing a fixed budget constraint choose between alternative measures to reduce crime, which include more stringent crime laws, increasing the probability of arrest, and treatment. In this way tougher crime laws are endogenous to the policymaker's preferred method of crime reduction. The remainder of this paper focuses on the political and economic factors determining this preference.

Preferred methods for combating sex crime have changed over time. As discussed in more detail in Appendix 3 Part B, sexual psychopath laws were enacted in most states between the 1930s and 1960s after first being enacted in Michigan in 1935 (Lave 2009, p.549). These laws confined sex offenders to treatment facilities as an alternative to facing criminal trial though in some states they did face criminal trial once their treatment was complete (Lave 2009, p.578). The 1980s saw both the rise of RCT (Gough 2013, p. 313) and "a conservative turn in the country" and a consequent deemphasis on combating crime through rehabilitation and greater reliance on punitive measures (Janus 2006, p.17). Most of the sexual psychopath laws were repealed in the 1980s (Pazzani and Maddan 2017, p.245), and determinate prison sentencing preventing the early release of sex offenders on parole replaced indeterminate sentencing (Janus 2006, p.17). As previously discussed, more severe measures were adopted in the 1990s and 2000s, the nature and extent of which differed significantly across states. The factors determining these differences are examined in the remainder of this paper.

These factors include the results and impact of a state's past policies. Blackshaw et al. (1989) argues that states that were originally "pioneers in providing mental health treatment" for sex offenders, including Florida, Washington, and California, which sponsored the Sex Offender Treatment and Evaluation Program (SOTEP) described in more detail in Appendix 3 Part B, adopted a more "correctional orientation" by 1989 in response to the disappointing results of programs like SOTEP. The perceived failure of the previously referenced sexual psychopath laws, which were repealed in Washington State in 1984, also contributed to this shift (McSherry and Keyzer 2009, p. 4). Other states, in contrast, started to rely more on treatment and adopted a more rehabilitative approach out of concern that imprisonment was not an effective deterrent for sex offenders (Blackshaw et al. 1993, p. 3). More recent trends suggest that this "correctional orientation" has been long-lasting and the results of past policy experiments by states can have long-term effects on their approach to combating crime. Washington state enacted a severe version of Jessica's Law relatively early[94] and, as previously referenced, both Florida and California have continued to favor relatively severe sex crime laws in the more recent past.

Political party orientation also affects a state's approach to combating sex crimes. Stringent sex crime laws conform to Republicans' historical emphasis on law and order and became especially popular among conservative Republicans in the 1990s partly as a defensive response to criticisms by liberal feminists of the role of patriarchy in sexual violence and the 1994 Violence Against Women Act, which was sponsored primarily by Democrats (Janus 2006, p.87). The history of Jessica's Law shows that Republican policymakers tend to favor harsher sex crime laws. As discussed in Chapter 2, in many states that had difficulty enacting Jessica's Law or enacted "compromise" versions of the law with mandatory minimum sentences less than 25 years, democrats provided opposition to the harshest proposed measures. The correlation matrix in Figure 17, which

[94] It enacted a version of Jessica's Law with a 25-year mandatory minimum sentence for sex crimes against a child under 15 in 2006 (Davis et al. 2013).

also includes three additional measures of crime law stringency not captured in Figure 16 due to space constraints, shows that states that I categorize as Republican based on electoral history[95] posted registries to the internet earlier, have longer mandatory minimum sentences for sex crimes, and are more likely to have enacted a version of Jessica's Law, be SORNA-compliant, require lifetime electronic monitoring (LEM) for all or a subset of SOs, and require juvenile SOs to register.

The occurrence of high-profile crime incidents attracting media attention also influences a state's approach to combating sex crimes. The Jacob Wetterling Act, Megan's Law, the Adam Walsh Act, and Jessica's Law were all named after the victims of high-profile crimes involving sexual assault or abduction and murder. State laws implementing federal sex offender registration and notification requirements, including Zachary's Law in Indiana, Ashley's Law in Texas, and the Amy Jackson Law in North Carolina,[96] (Filler 2001) were also named after high-profile crime victims. Two high-profile sex crimes in Washington State in the late 1980's helped influence the state's previously referenced shift to a more "correctional orientation" (Blackshaw et al. 1993, p. 3) and had the more immediate effects of prompting the state to establish a sex offender registry before it was federally required to do so and enact the country's first sex offender notification, Sexual Violent Predator and three strikes laws.[97]

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[95] I differentiate between "Republican" and "Democrat" based on state electoral college results in presidential elections between 2000 and 2016 with more weight placed on more recent elections

[96] In some cases, including 1995's Ashley's Law in Texas, state laws implementing sex offender registration or notification requirements were enacted before these requirements were federally mandated.

[97] The incidents in Washington State referred to here are the rape and murder of Diane Ballasiotes in 1988 and the rape and mutilation of Ryan Hade in 1989, both of which were committed by sex offenders who had recently been released from prison. These incidents helped lead to the Community Protection Act of 1990 in Washington State, which included the country's first Sexually Violent Predator law and established

Economist Daniel Kahneman discusses how events depicted in the media triggering strong emotional reaction can cause people to overestimate the likelihood that similar events could occur and the associated risk to themselves and others and thereby lead to "public panic" and affect public policy in a mechanism that he refers to as "availability cascade"[98] (Kahneman 2011, p.142). Given the relatively low incidence of sex crimes, as depicted in Figure 18, their frequent coverage in the media (Janus 2006, p.154-155), the emotional reaction and panic that they trigger, and the large number of laws named after and, in some cases, created in response to fear and anger-inducing sex crime incidents, Kahneman's analysis applies to sex crime policy.

The influence of high-profile incidents and the panic and anger that they induce could help explain why punitive measures for combating sex crimes are often preferred by policymakers to treatment provision. Janus (2006, p.115) argues that treatment is underutilized by policymakers relative to punitive measures in combating sex crimes and cites the more than $78 million that California spent to incarcerate sex offenders in 2004 while offering no treatment for imprisoned sex offenders as an example. He also argues that resources spent on incarcerating sex offenders should be diverted to expanding police forces and the supervision of released sex offenders (Janus 2006, p.115). More broadly, Donahue and Siegelman (1998) argue use data from existing and pilot social programs, including early childhood intervention programs, family-based therapy for children with disciplinary problems, treatment for juvenile delinquents, and labor market intervention

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sex offender registration and notification requirements, and Washington State's Three Strikes Law of 1993. See Janus (2006, p. 14-15) and Schiraldi et al. (2004, p. 3) for more on these incidents and their response.

[98] He attributes the invention of this mechanism and its name to legal scholar Cass Sunstein and jurist Timur Kuran (Kahneman 2011, p. 142).

programs for young adults, to argue that shifting resources spent on imprisonment to social programs would reduce crime.[99]

To quantify the impact of high-profile sex crime incidents, I calculate the number of such incidents by state. To limit the number of incidents to those attracting the most attention and having the most impact on policy, I include high-profile incidents of sexual abuse, assault, and/or abduction involving a single incident or victim, most of which also involved murder, that are referenced in, resulted in, or are the namesake of a U.S. state or federal crime law enacted or proposed during the last 30 years. SORNA lists seventeen incidents that helped inspire it (Adam Walsh Child Protection and Safety Act of 2006, Pub L. No. 109-248, 120 Stat. 587 [2006]), all of which I include. I also include eighteen other high-profile incidents involving sexual abuse, assault, and/or abduction that I found through keyword searches, review of news articles and other sources[100] that led to or were the namesake of crime laws. Table 27 includes summary information on these incidents across states.

As shown in Figure 17, the number of these high-profile incidents in a state is positively correlated with sex crime law stringency. States with more of these high-profile incidents created SO registries and posted them to the internet earlier and are more likely to have enacted a Sexually Violent Predator (SVP) law,[101] require juvenile SOs to

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[99] This conclusion is based on many assumptions, including the accuracy of their estimate of "the elasticity of crime with respect to incarceration" and the extent to which the social programs would be able to target at-risk youth (Donahue and Siegelman 1998).

[100] Sources include Filler (2001), Feeley and Simon (2013), Janus (2006), Schiraldi et al. (2004), McSherry (2009), Stuart and Sykora (2011), Johnson (1996), Logan (2021), Bayles (1994), and Russakoff (1998).

[101] These laws enable states to involuntarily civilly commit sex offenders determined to be dangerous after their release from imprisonment and have been passed in 20 states since the first was enacted in Washington State in 1990 (Krauss et al. 2015, p.245). They are also referred to as "Sexually Dangerous

register, and require lifetime electronic monitoring for all or a subset of SOs. As a particular example, Florida had, at six, the most high-profile sex crime incidents and has adopted stringent sex crime laws. It requires juveniles SOs to register and was the first state to implement a version of Jessica's Law and the fifth state to implement SORNA. It also was one of only six states allowing the death penalty for child rape when the Supreme Court determined in 2008 that only murderers could be executed (Mears 2008).

On the other hand, the number of these high-profile incidents in a state is not as highly correlated with sex crime law stringency as political party affiliation. Unlike political party affiliation, the number of high-profile incidents is uncorrelated with the mandatory minimum sentence for sex crimes and slightly negatively correlated with whether a state enacted Jessica's Law or implemented SORNA. In Idaho, for example, a grisly 2005 crime incident involving abduction and murder by a registered sex offender that attracted media attention did not lead the passage of a version of Jessica's Law in the state despite a petition for harsher laws from the only surviving victim.[102] Similarly, a version of Jessica's Law has not been enacted in Minnesota despite seven[103] high-profile

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Persons' Laws." States vary in their definition of who qualifies as "sexually dangerous" or "sexual violent" and therefore needs to be institutionalized after their release from prison. Minnesota's 1994 Sexually Dangerous Persons' law defines a "sexually dangerous person" as someone who "has engaged in a course of harmful sexual conduct," "manifested a sexual, personality, or other mental disorder or dysfunction," and "as a result, is likely to engage in acts of harmful sexual conduct." Washington State's SVP law uses a similar definition. See Janus (2006), Johnson (1996, p.1174), Krauss et al. (2015, p.246), and Minnesota Legislature: Office of the Revisor of Statutes. http://www.revisor.mn.gov/laws/1994/1/Session+Law/Chapter/1/1994-08-31%2000:00:00+00:00/pdf

[102] The abduction and murder of members of the Groene family by Joseph Edward Duncan. The only surviving victim petitioned for Slade and Dylan's Law, a "one strike" rule for violent sex offenders named after her murdered brothers, that would send them to jail for life after their first offense. See Janus (2006, p.2) and http://www.change.org/p/idaho-slade-and-dylans-law

[103] Only five of which are included in the correlation matrix in Figure 17 and Table 27 since the laws that two of the incidents resulted in were enacted more than 30 years ago.

sex crime incidents in the 1980's and 90s and pressure for harsher sex crime laws in the state following the 2003 murder of Dru Sjodin, though the incidents did lead to Minnesota's 1994 SVP Law (Johnson 1996, p.1174; 1994 Minn. Laws 5-9)[104] and affected sex crime law stringency in ways not captured in the correlation matrix by leading to a higher minimum sentence for repeat sex offenders in 1989,[105] higher maximum sentences for other sex crimes in 1989 and 1992 (Johnson 1996, p. 1149-1151; 1989 Minn. Laws 1622, 1593; 1992 Minn. Laws 1992-1993, 1994-1995),[106] and a 2005 law imposing mandatory life sentences on only "those convicted of the most disturbed and heinous" sex crimes (Janus 2006, p.159).[107]

Conclusion

This paper demonstrates a negative correlation between the stringency of sex crimes laws and the provision of treatment for sex offenders across. The econometric implications of this negative correlation are that the failure to include treatment provision as an independent variable in studies of the effects of crime laws could cause the effectiveness of those laws to be underestimated. This paper then reviews the political

[104] Though this law was later declared unconstitutional (Krauss et al. 2015, p.245)

[105] The legislature established a minimum sentence of 37 years for a third time sex offender. See Minnesota Legislature: Office of the Revisor of Statutes http://www.revisor.mn.gov/laws/1989/0/Session+Law/Chapter/290/1989-05-22%2000:00:00+00:00/pdf

[106] In 1989, following the 1988 rape and murders of Mary Foley and Carrie Coonrod in Minnesota by sex offenders who had recently been released from prison, the legislature increased the maximum sentence for first degree sex offenses from 20 to 25 years and the maximum sentence for second degree sex offenses from 15 to 20 years. After the rapes and murders of Melissa Johnson, Jamie Cooksey, and Carin Streufert in Minnesota from 1991-1992, the legislature again increased the maximum sentence for first degree sex offenses from 25 to 30 years and the maximum sentence for second degree sex offenses from 20 to 25 years. See Minnesota Legislature: Office of the Revisor of Statutes. http://www.revisor.mn.gov/laws/1989/0/Session+Law/Chapter/290/1989-05-22%2000:00:00+00:00/pdf. http://www.revisor.mn.gov/laws/1992/0/Session+Law/Chapter/571/1992-04-16%2000:00:00+00:00/pdf

[107] In its definition of "heinous," the law includes sex crimes involving torture, mutilation, the intentional infliction of "great bodily harm," and "extreme inhumane conditions." See Minnesota Legislature: Office of the Revisor of Statutes. http://www.revisor.mn.gov/statutes/2005/cite/609.3455

and economic factors determining a budget-constrained policymaker's preference for relying on either treatment or more punitive measures in order to combat crime, which include the outcome of past policy experiments, political party orientation, and the occurrence of high-profile crime incidents.

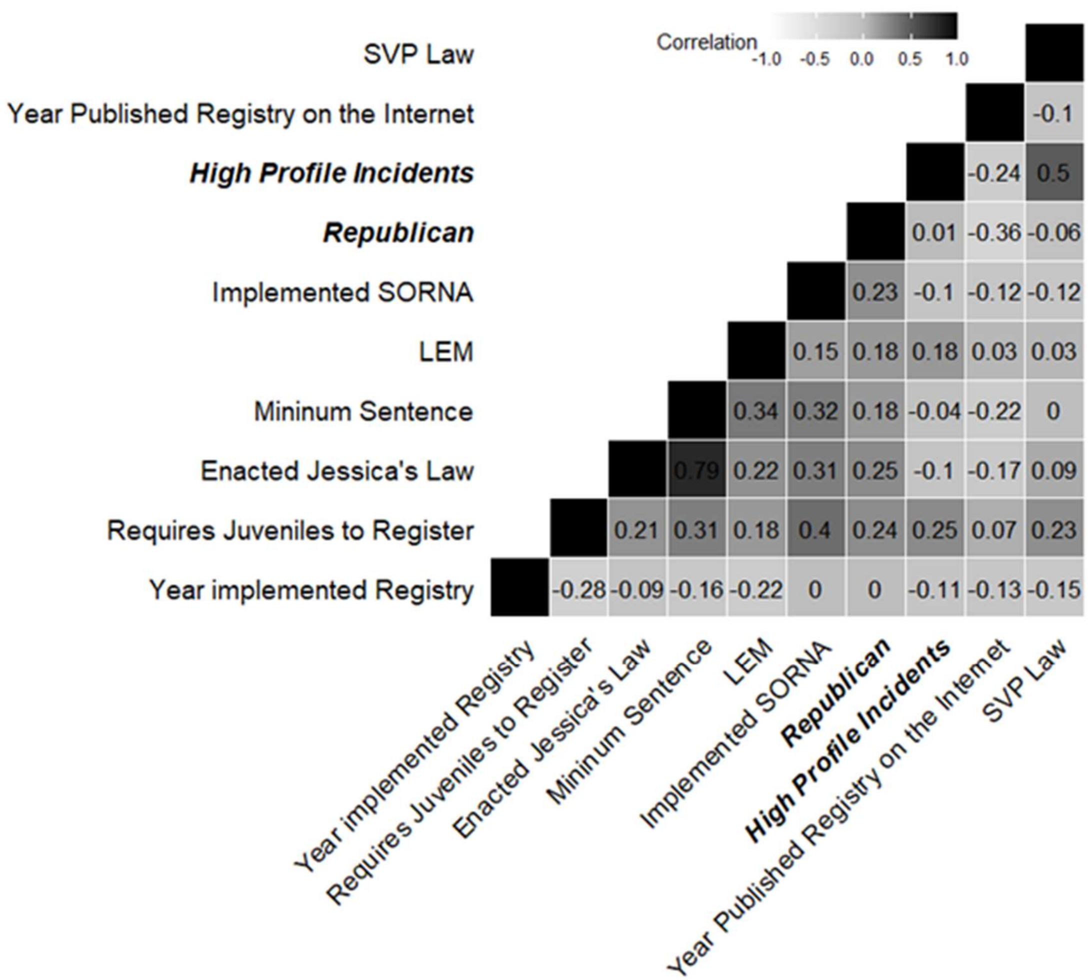

Figure 17 Correlation Between Crime Law Stringency, Political Party, and High Profile Incidents Impacting Stringency

APPENDIX 1

Part A Mathematical

As referenced in Part 3 of Chapter 1, Callaway and Sant'Anna provide methods for aggregating group-time average treatment effects to form 1) an overall estimate of the average treatment effect on the treated (ATT), 2) estimates of the ATT for each group, 3) and estimates of the ATT by event time. The overall estimate of the ATT can be calculated either as 1) an average of all of the group-time average treatment effects weighted by group size in what they describe as a simple aggregation, 2) an average of the ATT for each group weighted by group size, or 3) an average of the ATT for each event time in what they describe as a dynamic aggregation (Callaway and Sant'Anna 2021 p.18-19). The weights used to compute these aggregations are determined only by the number of units in each group. In this way, the CS estimator avoids the previously referenced problems with the weights used to average 2x2 DD estimators into an overall estimate of the treatment effect in TWFE estimates. In this paper, I use the simple aggregation but cross-validate my results against the other two methods.

Callaway and Sant'Anna specify three different methodologies for controlling for differences in pre-treatment values of covariates between the treatment and control groups in calculating group time average treatment effects (Callaway and Sant'Anna 2021, p.10). These include weighting control group observations based on propensity score matching (Abadie 2005), using regression to model the outcome path of control group units as a function of the pre-treatment value of selected covariates (Heckman et al.

1997), and the Doubly Robust Difference in Differences (DRDID) estimator (Sant'Anna and Zhao 2020), which combines both of the other methods. For CS models in this paper that include pre-treatment levels of covariates, I use the second of these methods, outcome regression (Heckman et. Al 1997), but cross-validate my results using the other two methods. The first step in the outcome regression method is to use regression to model the outcome evolution of control group units as a function of the pre-treatment level of covariate according to the specifications:

Equation 11

$$\hat{\mu}_{0,0}(X) = \beta_0 + \beta_1 X$$

Equation 12

$$\hat{\mu}_{0,1}(X) = \beta_0 + \beta_1 X$$

where $\hat{\mu}_{0,0}$ represents the pre-treatment level of the dependent variable for control group observations, $\hat{\mu}_{0,1}$ represents the post-treatment level of the dependent variable for control observations, and X represents the pre-treatment level of one or more covariates for control group observations.[108] These regressions can be used to derive the expected outcome evolution of control group observations conditional on the pre-treatment level of the covariates:

[108] This derivation draws on Cunningham (2021) and Sant'Anna and Zhao (2020, p. 104).

Equation 13

$$m_{g,t}(X) = E\left[Y_t - Y_{g-1} | X, C = 1\right]$$

where t represents year, g represents the year when the treatment group whose group-time average treatment effect is being calculated first received treatment, and C is a dummy variable equaling one for control group units. $m_{g,t}(X)$ can then be used to calculate group-time average treatment effects in year t for units first treated in year g by subtracting the expected outcome evolution of the control group conditional on the actual levels of pre-treatment covariates for treatment group units from the actual outcome evolution of those treatment group units:

Equation 14

$$ATT(g,t) = E\left[\frac{G_g}{E|G_g|}\left(Y_t - Y_{g-1} - m_{g,t}(X)\right)\right]$$

where Y is the dependent variable and G_g is a dummy variable that equals one for units first treated in year g. In this paper, I use the outcome regression approach in my models testing the effect of registry implementation, the posting of registries to the internet, and SORNA on sex crime rates with X as the pre-treatment level of the other violent crime rate.

Part B Additional Tables

Table 12 Year of Registry Creation, Implementation of Internet Registries, SORNA Implementation, and Jessica's Law Enactment Across States

State	Year Implemented Registry	Year Registry Available Online	Year Implemented SORNA	Year Enacted Jessica's Law
Alabama	1967	1998	2011	2006
Alaska	1994	1997	-	2006
Arizona	1996	1998	-	2006
Arkansas	1987	2004	-	2006
California	1947	2004	-	2006
Colorado	1991	2001	2013	2014
Connecticut	1998	1999	-	2007
Delaware	1994	1998	2008	2006
DC	2000	2001	-	-
Florida	1993	1997	2009	2005
Georgia	1996	1998	-	2006
Hawaii	1996	2005	-	-
Idaho	1993	2002	-	-
Illinois	1986	2002	-	-
Indiana	1994	2003	-	-
Iowa	1995	1998	-	2005
Kansas	1993	1997	2011	2006
Kentucky	1994	2000	-	2006
Louisiana	1992	2000	2008	2005
Maine	1996	2003	-	2006
Maryland	1995	2002	2010	2007
Massachusetts	1996	2004	-	2008
Michigan	1995	1999	2011	2006
Minnesota	1991	1997	-	-
Mississippi	1994	1997	2011	2006
Missouri	1979	2004	2008	2006
Montana	1989	2001	-	2007
Nebraska	1997	2000	-	2006
Nevada	1998	2004	2018	2007
New Hampshire	1993	2001	-	2006
New Jersey	1994	2002	-	2014
New Mexico	1995	2000	-	2007
New York	1996	2000	-	2006
North Carolina	1996	2000	-	2008
North Dakota	1991	2001	-	2006
Ohio	1997	2001	2008	2007
Oklahoma	1989	2005	2016	2007
Oregon	1989	2006	-	2006
Pennsylvania	1996	2004	2012	2006
Rhode Island	1992	2005	-	2006
South Carolina	1994	1998	2011	2006
South Dakota	1994	2006	2010	2006
Tennessee	1995	1997	2011	2007
Texas	1991	1998	-	2007

Utah	1983	1998	-	2008
Vermont	1996	2004	-	-
Virginia	1994	1999	2016	2006
Washington	1990	2005	-	2006
West Virginia	1993	1998	-	2006
Wisconsin	1993	2001	-	2006
Wyoming	1994	N.A.*	2011	2010

*The year that Wyoming first placed its sex offender registry on the internet is unavailable

Table 13 Baseline Crime Statistics Across the 50 States and DC

State	2005 Sex Crime Rate	2005 Registry Size (per 100k people)	2003 Rape Rate	2003 Sex Offense Arrest Rate	2003 Other Violent Crime Rate
Alabama	N.A.*	124.68	.3445	.096	3.71
Alaska	N.A.*	443.89	.9107	.4407	4.92
Arizona	.7478	278.24	.3257	.3238	4.71
Arkansas	.7491	214.51	.291	.1258	3.72
California	N.A.*	289.08	.2787	.4393	5.47
Colorado	1.2644	182.80	.3909	.2788	2.81
Connecticut	.4789	108.91	.1756	.1652	2.56
Delaware	1.0613	200.56	.4397	.0951	6.13
DC	N.A.*	107.16	.4863	N.A.*	15.6
Florida	N.A.*	199.44	.3932	N.A.*	6.87
Georgia	1.4315	108.66	.242	.4709	3.99
Hawaii	N.A.*	159.8	.2689	.2799	2.22
Idaho	1.2385	192.11	.3781	.2786	1.99
Illinois	N.A.*	135.01	N.A.*	.1665	3.33
Indiana	N.A.*	128.16	.2454	.2169	2.9
Iowa	.7672	220.81	.2607	.0938	2.41
Kansas	1.1513	114.83	.3128	.0927	2.51
Kentucky	.9465	118.44	.1076	.0644	1.45
Louisiana	.7103	147.31	.3258	.304	5.51
Maine	.7851	118.74	.2633	.1875	.79
Maryland	N.A.*	76.78	.2394	.2287	4.66
Massachusetts	.5811	256.31	.2798	.0827	4.51
Michigan	1.2338	307.67	.5181	.1333	4.39
Minnesota	N.A.*	324.93	.3896	.1893	2.1
Mississippi	N.A.*	115.41	.2867	.1274	2.1
Missouri	N.A.*	187.27	.2439	.5683	4.65
Montana	1.1303	452.5	.158	.0658	2.05
Nebraska	1.0521	117.26	.2695	.3482	2.51
Nevada	N.A.*	204	.3875	.6278	5.76
New Hampshire	N.A.*	239.93	.243	.103	.67
New Jersey	N.A.*	121.1	.1478	.2183	3.47
New Mexico	N.A.*	95.38	.4417	.0793	5.5
New York	N.A.*	108.91	.1745	.2292	4.03
North Carolina	N.A.*	127.46	.2399	.2214	4.08
North Dakota	.7982	144.61	.2275	.1353	.49
Ohio	.9487	117.7	.3531	.1285	2.62

Oklahoma	N.A.*	38.64	.4209	.2204	4.56
Oregon	1.086	448.96	.3316	.3696	2.54
Pennsylvania	N.A.*	56.75	.2561	.2509	3.3
Rhode Island	.6561	140.04	.4665	.1192	2.37
South Carolina	1.1274	193.15	.4681	.2056	7.55
South Dakota	1.2295**	221.84	.4349	.2439	1.19
Tennessee	.9723	134.37	.363	.1298	6.53
Texas	.8108	172.25	.3613	.2084	5.14
Utah	1.581	327.89	.3568	.3515	1.94
Vermont	.6326	360.43	.1971	.0525	.91
Virginia	.773	177.61	.2383	.1459	2.47
Washington	N.A.*	300.29	.4594	.1987	2.97
West Virginia	.6961	122	.1492	.0752	2.08
Wisconsin	9816	314.91	.2127	.7059	1.94
Wyoming	N.A.*	185	.2644	.2289	2.26

*Data missing or unavailable. For sex crime rates, this will be the case for any states that did not report to NIBRS in 2005.
**2006 rate reported in lieu of 2005 rate because more ORIs within SD reported to NIBRS in 2006 so the 2006 rate is more reflective of the state as a whole.

Table 14 Effect of Registry Implementation and the Posting of Registries on the Internet on Sex Crime Rates: Results from Bacon Decomposition and CS Method Using a Balanced Panel Subset of the UCR Data

Bacon Decomposition

	Rape Rate	Sex Offense Arrest Rate	Rape Rate	Sex Offense Arrest Rate
	Beta/Weight	Beta/Weight	Beta/Weight	Beta/Weight
Effect of Registry Creation				
Earlier vs Later	.029/.245	.012/.175	.04/.106	-.003/.057
Later vs Earlier	-.014/.377	-.013/.277	-.011/.447	.009/.228
Later vs Always	.06/.378	.046/.548	.083/.447	.051/.715
#Obs	90,174	31,996	131,988	38,012
Years	1985-2003	1985-2003	1985-2018	1985-2018
Posting to Internet				
Earlier vs Later	.043/.318	-.005/.264	.032/.47	-.05/.475
Later vs Earlier	.008/.062	-.001/.048	.004/.53	-.062/.525
Treated vs Untreated	.026/.62	-.07/.689		
#Obs	89,186	31,787	130,526	37,740
Years	1985-2003	1985-2003	1985-2018	1985-2018

CS Estimator

Effect of Registry Creation		
ATT	.017(.01)	-.016(.014)
Other Violent Crime Rates	Yes	Yes

Dependent Variable Mean	.238	.324
#Obs	66,157	26,143
Years	1985-1997	1985-1997
Posting to Internet		
ATT	-.006(.009)	-.011(.025)
Other Violent Crime Rates	Yes	Yes
Dependent Variable Mean	.268	.343
#Obs	95,214	32,802
Years	1985-2005	1985-2005

Note. Numbers in parentheses are standard errors clustered by ORI. Rape and Sex Offense Arrest rates are calculated as rates per 1000 people. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 15 Effect of SORNA on Registry Size: Bacon Decomposition of TWFE Results

	Registry Size	Log Registry Size
	Beta/Weight	Beta/Weight
Earlier vs Later	17.995/.079	.035/.079
Later vs Earlier	-6.274/.077	.003/.077
Treated vs Untreated	43.228/.844	.175/.844
#Obs	714	714
Years	2005-2018	2005-2018

Table 16 Effect of SORNA on Sex Crime Incidence: TWFE Model Results

	Sex Crime Rate	Rape Rate	Sex Offense Arrest Rate
SORNA	.06(.031)*	.005(.005)	.027(.006)***
Jessica's Law	-.005(.049)	.003(.006)	-.038(.024)
Other Violent Crime Rates	Yes	Yes	Yes
ORI Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Dependent Variable Mean	.729	.201	.136
#Obs	64,243	260,805	256,773
#States	39	51	51
#ORI Reporting Areas	6,222	16,947	16,944
Years	2003-2016	2003-2018	2003-2018

Note. Numbers in parentheses are standard errors clustered by ORI. Rape, Sex Offense Arrest, and Sex Crime rates are calculated as rates per 1000 people. See the note below Table 3 for how sex crimes are defined. The Sex Offense Arrest Rate includes arrests for

sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 17 Effect of SORNA on Sex Crime Incidence: Results from the CS Method Using Balanced Panel Subsets of the NIBRS and UCR Data

	Sex Crime Rate	Sex Crime Rate	Rape Rate	Rape Rate	Sex Offense Arrest Rate
ATT	-.018(.026)	-.064(.035)*	-.006(.014)	-.019(.011)*	.011(.012)
Other Violent Crime Rates	Yes	Yes	Yes	Yes	Yes
Estimation Method	OR	DRDID	OR	DRDID	OR
#Obs	25,960	25,960	102,588	102,588	52,008
Dependent Variable Mean	.845	.845	.279	.279	.208
Years	2007-2016	2007-2016	2007-2018	2007-2018	2007-2018

Note. Numbers in parentheses are standard errors clustered by ORI. Rape, Sex Offense Arrest, and Sex Crime rates are calculated as rates per 1000 people. See the note below Table 3 for how sex crimes are defined. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 18 Comparison of the Effect of Registry Expansion in SORNA Compliant and Non-compliant States: Results from Including Data for All Available States

	Sex Offense Arrest Rate	Sex Crime Rate	Sex Crimes against Family or Acquaintance
Registry Size	-.00003(.00004)	-.00004(.00008)	-.0001(.00007)*
SORNA X Registry Size	.00006(.00003)**	.0002(.00007)**	.0002(.00007)**
Other Violent Crime	Yes	Yes	Yes
ORI Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Dependent Variable Mean	.132	.727	.566
#Obs	225,289	57,091	57,091
#States	51	39	39
#ORI Reporting Areas	16,931	6,148	6,148
Years	2005-2018	2005-2016	2005-2016

Note. Numbers in parentheses are standard errors clustered by ORI. Sex Offense Arrest and Sex Crime rates are calculated as rates per 1000 people. See the note below Table 3 for how sex crimes are defined. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

APPENDIX 2

Part A Additional Tables

Table 19 The Impact of Jessica's Law on Sex Crime Rates: Results from the CS Method Using the Full Unbalanced Panel of NIBRS Data

	Sex Crime Rate	Sex Crime Rate Under 18
ATT	.122(.064)*	.141(.061)**
Other Violent Crime Rates	Yes	Yes
Dependent Variable Mean	.776	.521
#Obs	54,907	54,907
Years	2003-2016	2013-2016

Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. See the note below Table 8 for how sex crimes are defined. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 20 The Impact of Jessica's Law on Sex Crime Rates: Bacon Decomposition of TWFE Results Using a Balanced Panel Subset of the NIBRS Data

	Sex Crime Rate	Sex Crime Rate Under 18
	Beta/Weight	Beta/Weight
Earlier vs Later	.086/.239	.045/.239
Later vs Earlier	.07/.56	.029/.56
Treated vs Untreated	.122/.2	.087/.2
#Obs	26,404	26,404
Years	2003-2016	2003-2016

Table 21 Effect of Jessica's Law on Rape and Sex Offense Arrest Rates: Results from the CS Method Using the Full Unbalanced Panel of UCR Data

	Rape Rate	Sex Offense Arrest Rate	Percent Cleared
ATT	-.009(.03)	-.071(.018)***	8.368(1.313)***

Other Violent Crime Rates	Yes	Yes	Yes
Dependent Variable Mean	.261	.222	39.18
#Obs	197,399	132,586	131,421
Years	2003-2018	2003-2018	2003-2018

Note. Numbers in parentheses are standard errors clustered by ORI. All dependent variables are calculated as rates per 1000 people. The Sex Offense Arrest Rate includes arrests for sex offenses other than rape and prostitution. See the note below Table 1 for how rape is defined. %Cleared is the percent of rapes cleared by arrest. Other Violent Crime Rates indicates that Other Violent Crime Rates were controlled.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 22 Effect of Jessica's Law on Rape and Sex Offense Arrest Rates: Results from the TWFE Model

	Rape Rate	Sex Offense Arrest Rate	Percent of Rapes Cleared
Jessica's Law	.003(.006)	-.035(.024)	-1.684(.63)***
Other Violent Crime Rates	Yes	Yes	No
ORI Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Dependent Variable Mean	.201	.136	39.178
#Obs	260,805	256,773	131,421
#States	51	51	51
#ORI Reporting Areas	16,947	16,944	14,880
Years	2003-2018	2003-2018	2003-2018

Bacon Decomposition Using a Balanced Panel Subset of the UCR Data

	Beta/Weight	Beta/Weight	Beta/Weight
Earlier vs Later	.017/.241	-.006/.267	1.676/.213
Later vs Earlier	-.018/.534	.018/.548	-2.516/.65
Treated vs Untreated	.03/.224	-.032/.184	-5.603/.136
#Obs	115,696	55,088	45,232

Table 23 The Effect of Jessica's Law and Internet Registries on the Proportion of Victims Under 18: Results from the CS Method Using the Full Unbalanced Panel of NIBRS Data

	Proportion Under 18	Proportion Under 18
ATT (Jessica's Law)	.021(.014)	
ATT (Internet Registries)		.009(.015)
Dependent Variable Mean	.65	.677

#Obs	52,159	18,020
Years	2003-2016	1995-2005

Numbers in parentheses are standard errors clustered by ORI. Proportion of Victims Under 18 refers to the proportion of the victims of sex crimes that were under 18 years old. See the note below Table 8 for how sex crimes are defined.
*** denotes statistical significance at the .01 level
** denotes statistical significance at the .05 level
* denotes statistical significance at the .1 level

Table 24 The Effect of Jessica's Law and Internet Registries on the Proportion of Victims Under 18: Bacon Decomposition of TWFE Results Using a Balanced Panel Subset of the NIBRS Data

	Proportion Under 18	Proportion Under 18
	Beta/Weight	Beta/Weight
Effect of Jessica's Law		
Earlier vs Later	-.004/.245	
Later vs Earlier	-.009/.57	
Treated vs Untreated	.026/.185	
Effect of Internet Registries		
Earlier vs Later		-.001/.186
Later vs Earlier		-.019/.814
#Obs	21,938	8,338
Years	2003-2016	1995-2016

APPENDIX 3

Part A An Analysis of Sex Crime Clearance Rates and Sex Crime Law Stringency Over the Past 30 Years

This appendix includes a brief analysis of the relationship between sex crime clearance rates and sex crime law stringency based on current and historical trends. The consequences for committing sex crimes have become increasingly more severe over the past 30 years with the passage of the Jacob Wetterling Act in 1994, Megan's Law in 1996, and the Adam Walsh Act (AWA) in 2006 and the enactment of different versions of Jessica's Law in different states at different times between 2005 and 2014. Sexually violent person (SVP) laws have also been passed in 20 states (Krauss et al. 2015, p.245). Sex crime laws have been criticized as being too harsh given the inclusion of sex offenders on publicly available online registries, the requirement in some states that sex offenders be tracked electronically via GPS, residency restrictions faced by sex offenders, and the relatively long length of prison sentences imposed for sex crimes (Daly 2008, p.1), which has been increasing since the middle of the 1980s.[109]

However, as shown in Figure 18, clearance rates for sex crimes are relatively low. Based on National Incident-Based Reporting System (NIBRS) data, less than 20% of sex crime incidents led to arrest in 2016 versus 76.2% for drug crimes, 39.1% for assaults, and 42.9% of homicides. The severity of punishments for sex offenders could therefore

[109] For example, based on Bureau of Justice Statistics (BJS) data, the amount of prison time served by rapists increased from approximately 41 months in 1985 to approximately 61 months in 1996 (Janus 2006, p. 82).

be justified by the low probability that they face of being arrested given the tradeoff analyzed in Chapter 3 Part 1. Guilt is notoriously difficult to prove in case of sex crimes as a result of lack of witnesses or incriminating evidence,[110] which could help explain the low clearance rates and reliance on severe punishments for sex offenders.

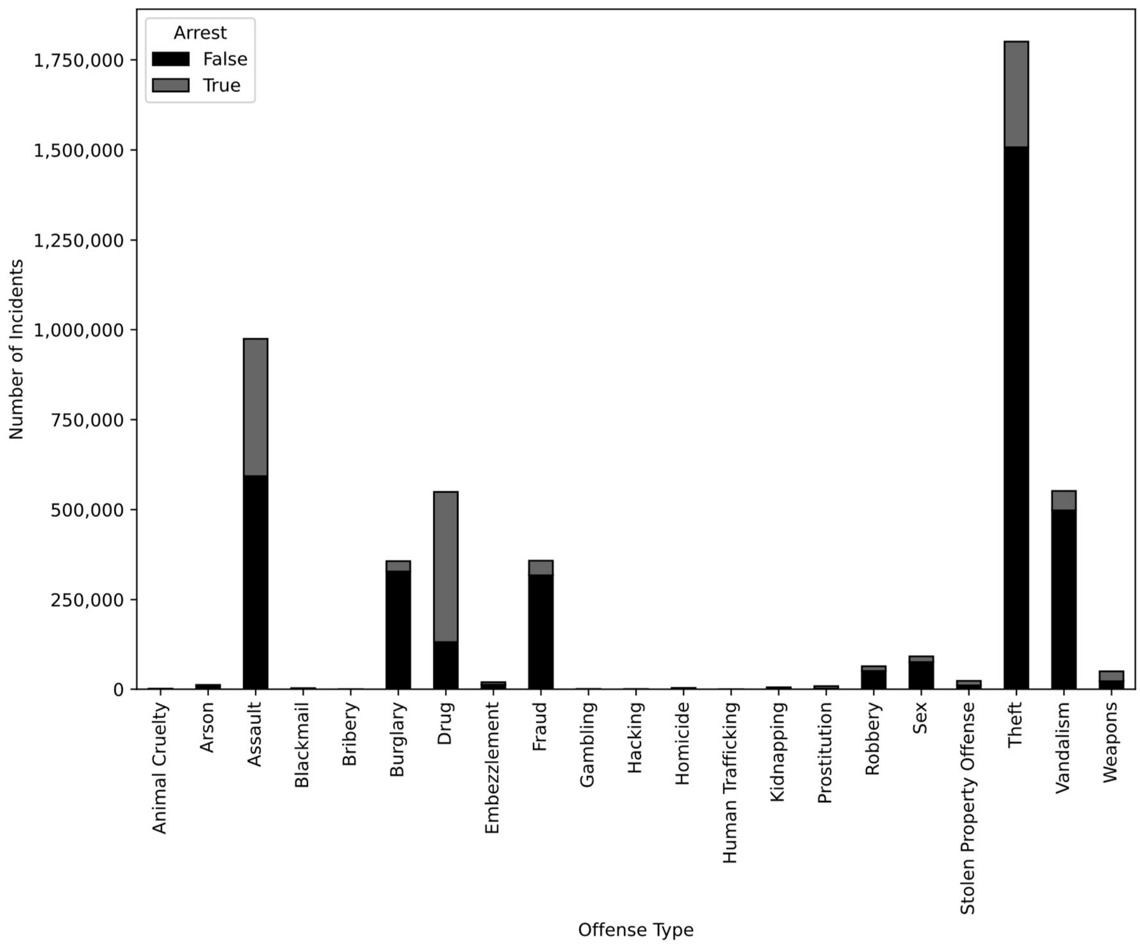

Figure 18 Frequencies of Incidents and Arrests in 2016 NIBRS Reporting
Sex crimes include rape, sodomy, sexual assault with an object, forcible fondling, incest, statutory rape, and pornography. This figure only includes crime incidents reported in NIBRS that involved one of the offense types listed above, which was the case for 92% of the total number of crime incidents included in NIBRS 2016 reporting. In rare cases, NIBRS crime incidents involve multiple offense types.

[110] However, DNA evidence can sometimes be used to identify the perpetrator in case of rape or sexual assault.

As the punishments for sex offenders have increased over the last 30 years, clearance rates for sex crimes have decreased. On the basis of 1975-2018 Unified Crime Reporting (UCR) data and as shown in Figure 19, while clearance rates for other violent crimes remained stable, clearance rates for rape trended consistently downwards starting in the 1990s. This downward trend coincides with the previously referenced increase in the severity of the punishments for sex crimes starting in the 1990s. As analysis of rape clearance rates is complicated somewhat by changes in the definition of rape,[111] I also use the more detailed NIBRS data to create a consistent definition of sex crimes[112] and analyze changes in the percent of sex crimes leading to arrest during the same timeframe. As shown in Figure 20, it also decreased significantly between the early 1990s and 2016, and this downward trend does not appear to be due to an increase in sex crime incidence, which shows no clear trend between 1993 and 2016, as shown in Figure 20. Increased crime law stringency could be associated with lower clearance rates both because increasing the severity of punishments and the probability of arrest represent substitute methods of controlling crime, as discussed in Chapter 3 Part 1, and because more expensive punishments, such as the mandatory minimum sentences imposed by Jessica's Law, make police less likely to arrest suspected offenders. More draconian punishments

[111] The UCR definition of rape broadened in 2013, as described in the note below Table 1. On a cultural level, society's conception of rape has also broadened since the 1970s, which could affect crime reporting (Janus 2006, p.81).
[112] This definition is provided in the note below Table 3.

may also make victims, especially if they are related to or a close acquaintance of an offender, less likely to cooperate, thereby reducing the probability of arrest.

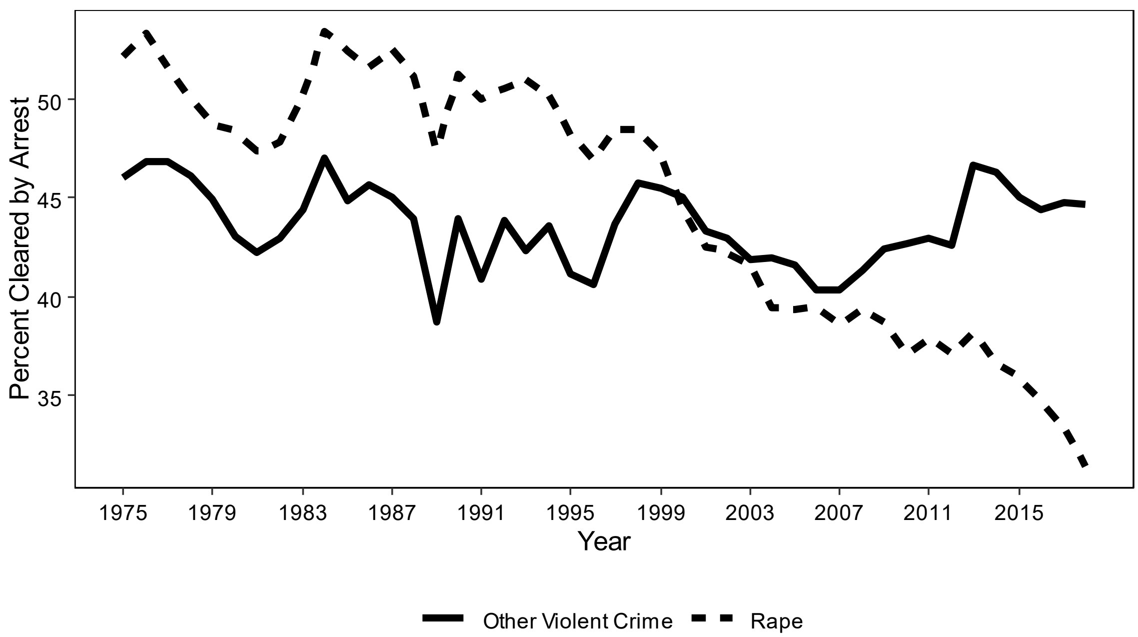

Figure 19 Clearance Rates for Rape and Other Violent Crimes in UCR Data
Other violent crimes include murder, robbery, and aggravated assault. See the note below Table 1 for the definition of Rape.

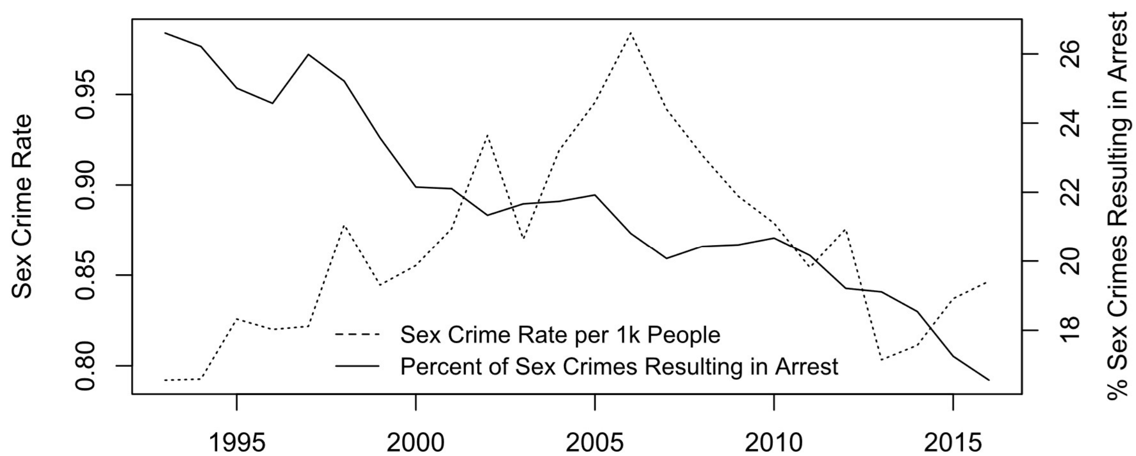

Figure 20 Percent of Sex Crimes Resulting in Arrest in NIBRS Reporting
See the note below Table 3 for how sex crime is defined.

Part B A Review of Evidence on the Effectiveness of Treatment for Sex Offenders

The different forms of treatment that exist for sex offenders include cognitive behavioral therapy, arousal control, empathy building, antidepressants, and, in extreme cases and usually with the consent of the offender, chemical or surgical castration.

As previously discussed, the sexual psychopath laws first enacted in the 1930s confined sex offenders to treatment as an alternative to punishment in combating sex crimes. However, these laws were broadly perceived to be a failure. In many cases, states lacked treatment programs and sex offenders were institutionalized without actively being treated (Lave 2009, p.587). Doubt in the mental health field about the efficacy of treatment for sex offenders (Johnson 1996, p.1141) also contributed to the repeal of most of these laws in the 1980s (Pazzani and Maddan 2017, p.245). Early studies of the effect of sex offender treatment on recidivism did not find much evidence for treatment efficacy. One of the most thorough studies was the state-sponsored[113] Sex Offender Treatment and Evaluation Project (SOTEP) in California from the 1980s,[114] which randomly assigned imprisoned sex offenders who volunteered to participate to treatment or control groups to assess treatment impact. Members of the treatment group participated in a two-year treatment program at a state hospital where they were treated using cognitive-behavioral therapy. Recidivism rates for the treatment and control groups were tracked during a 5-year period following treatment. SOTEP did not find strong evidence for the efficacy of treatment. Offenders in the treatment group were not less likely to recidivate than those in the control group (Marques et al. 2005, p.98). However, among high-risk offenders in the treatment group, those who successfully completed treatment were significantly less likely to recidivate than those who did not (Marques et al. 2005, p.98).[115] Similarly, in a meta-analysis on sex offender recidivism, Blackshaw et al. (1989) do not find evidence that treatment reduces recidivism.

However, this meta-analysis has been criticized on the grounds that the studies it included suffered from methodological problems[116] and the specific treatments used in those studies may have been "inappropriate" (Andrews and Borta, 2017, p.328). More recent research has found stronger evidence for the efficacy of treatment. The methods used to treat sex offenders have also "evolved rapidly" since the early studies were

[113] It was funded by the California State Legislature.
[114] Selected volunteers continued to receive treatment as part of this project from 1985-1995 and evaluation of treatment outcomes continued for six years following closure of the treatment unit at the state hospital (Marques et al. 2005, p.81).
[115] Successful completion was measured on the basis of a 9-point scale assessing whether members of the treatment group reached program goals (Marques, et al. 2005, p.97).
[116] Including follow-up periods that were too short to meaningfully measure recidivism (Blackshaw et al. 1989, p.27).

conducted (Janus 2006, p.53). In a meta-analysis incorporating data from 43 studies, Hanson et al. (2002) find recidivism rates to be significantly lower for treated than non-treated sex offenders and cognitive-behavioral treatment to be most effective form of treatment (Hanson et al. 2002, p.189). Similarly, Losel and Schmucker (2005) find that treated sex offenders are 37% less likely to recidivate that non-treated sex offenders in a meta-analysis based on 69 studies and that physical castration, chemical castration using hormonal medications, and cognitive behavioral therapy are the most effective forms of treatment. Overall, recent research provides a "growing consensus" that treatment is effective for reducing sex offender recidivism (Marques et al. 2005, p.80).

Part C Additional Tables

Table 25 Sex Crime Law Stringency Across States

State	Minimum Sentence (years)	LEM Requirement for SOs	Requires Juvenile SOs to Register	Enacted SVP Law
Alabama	20	No	Yes	No
Alaska	25*	No	No	No
Arizona	30[117]	No	Yes	Yes
Arkansas	25	No	Yes	No
California	25	Yes	Yes	Yes
Colorado	24*	No	Yes	No
Connecticut	25	No	No	No
Delaware	25	No	Yes	No
DC	0	No	No	No
Florida	25	Yes	Yes	Yes
Georgia	25	Yes	No	No
Hawaii	0	No	No	No
Idaho	1	No	Yes	No
Illinois	6	No	Yes	Yes
Indiana	3*	No	Yes	No
Iowa	17.5	No	Yes	Yes
Kansas	25**	Yes	Yes	Yes
Kentucky	20*	No	Yes	No

[117] See Footnote 73 for more details on the mandatory minimum sentence imposed by Arizona's version of Jessica's Law and how I derived the value used in this chart.

Louisiana	25	Yes	Yes	No
Maine	20	No	No	No
Maryland	25	Yes	Yes	No
Massachusetts	10	No	Yes	Yes
Michigan	25	Yes	Yes	No
Minnesota	12**[118]	No	Yes	Yes
Mississippi	20	No	Yes	No
Missouri	30	Yes	Yes	Yes
Montana	25	No	Yes	No
Nebraska	15	No	No	Yes
Nevada	35	No	Yes	No
New Hampshire	25**	No	Yes	Yes
New Jersey	25	No	Yes	Yes
New Mexico	18*	No	No	No
New York	10	No	No	Yes
North Carolina	25	Yes	Yes	No
North Dakota	20**	No	Yes	Yes
Ohio	25	Yes	Yes	No
Oklahoma	25	No	Yes	No
Oregon	25	Yes	Yes	No
Pennsylvania	10	No	Yes	Yes
Rhode Island	25	Yes	Yes	No
South Carolina	25	No	Yes	Yes
South Dakota	15	No	Yes	No
Tennessee	25	No	Yes	No
Texas	25	No	Yes	Yes
Utah	25	No	Yes	No
Vermont	10*	No	No	No
Virginia	25	No	Yes	Yes
Washington	25	No	Yes	Yes
West Virginia	25	No	No	No
Wisconsin	25	Yes	Yes	Yes
Wyoming	25	No	Yes	No

*The listed sentence is a presumptive sentence instead of a mandatory minimum due to no record of a mandatory minimum sentence.
**It is possible to deviate from this mandatory minimum sentence under exceptional circumstances.

Table 26 Sex Offender Treatment Provision Across States Based on Daly (2008)

State	% SOs in Probation or Parole in Treatment	Reentry Services Specifically for SOs	Funding for Community Based Treatment	% Imprisoned SOs in Treatment
Alaska	27.5*	Yes	State Funding	0
Arizona	-	-	-	8.6

[118] Based on a 2005 Minnesota law, a mandatory life sentence is imposed on perpetrators of the most "heinous" sex crimes, which include those involving torture or mutilation. However, as described in footnote 29, I define minimum sentence as the mandatory minimum sentence in a state for 1st time sexual assault not involving use of a weapon or deadly force.

Arkansas	48.8	-	State Funding	7
California	4.7*	No	State Funding	0
Colorado	-	No	State Funding	13.4[119]
Connecticut	87.5*	Yes	State Funding	1
Delaware	28	Yes	No	No
DC	67.5*	-	Federal Funding	-
Florida	-	No	No	0
Georgia	72.7	No	No	0
Idaho	94	Yes	Grant Funding Available	8
Illinois	85	-	State and Private Funding	3
Indiana	98	Yes	State Funding	28
Iowa	71	Yes	State Funding	30
Kansas	75	No	State Funding	11
Kentucky	35	No	State Funding	20
Maine	96.5*	-	Some Federal Funding	16
Maryland	20	-	No	-
Massachusetts	-	Yes	-	-
Michigan	100	Yes	State Funding	-
Missouri	95	No	No	-
Montana	-	No	No	-
New Hampshire	-	No	No	15
New Mexico	-	No	State Funding	16
North Carolina	-	-	-	1.1
North Dakota	50	-	State Funding	-
Ohio	-	Yes	State Funding	5
Oklahoma	-	Yes	State Funding	3
Oregon	99	Yes	State Funding	-
Pennsylvania	-	Yes	State Funding	20
Rhode Island	-	No	-	20.4
South Carolina	-	-	-	1.7
South Dakota	56	Yes	State Funding	13
Texas	-	No	State Funding	2
Utah	-	Yes	Funding Available from State Programs	-
Vermont	54	Yes	State and Insurance Funding	20
Virginia	-	No	State Funding	5
Washington	30	No	State Funding	6.5
West Virginia	100	No	State Funding	-
Wisconsin	-	Yes	-	12
Wyoming	61	Yes	No	33

-Data is not available.
*Daly (2008) provides an estimated range but not exact value. Approximate value derived based on midpoint of estimated range.

[119] Refers to % of lifetime imprisoned SOs in case of Colorado.

Table 27 High Profile Sex Crime Incidents Across States

State	High Profile Sex Crime, Assault or Abduction Incident (by Year and name of victim)	Proposed or Enacted Law(s) Named After or Created in Response
Arizona	Christy Ann Fornoff (1984)	Referenced in AWA (2006)
California	Polly Klaas (1993)	California Three Strikes Law (1994), Referenced in AWA
California	Samantha Runnion (2002)	Referenced in AWA
Florida	Jessica Lunsford (2005)	Jessica's Law (2005), Referenced in AWA
Florida	Adam Walsh (1981)	Adam Walsh Act
Florida	Sarah Lunde (2005)	Referenced in AWA
Florida	Jimmy Ryce (1995)	Jimmy Ryce Act (1998), Referenced in AWA
Florida	Carlie Brucia (2004)	Carlie's Law (2004)*, Referenced in AWA
Florida	Amanda Brown (1998)	Referenced in AWA
Idaho	Slade, Dylan, and Shasta Groene (2005)	Slade and Dylan's Law (2016)*
Indiana	Zachary Snider (1993)	Zachary's Law (2003)
Iowa	Jetseta Gage (2005)	Referenced in AWA
Kansas	Stephanie Schmitt (1992)	Stephanie's Law (1994)
Louisiana	Jeremy Guillory (1992)	Louisiana Sex Offender Registration Law, LA.Rev.Stat.15:540 (1992)
Massachusetts	Ally Zapp (2002)	Massachusetts' Sexually Dangerous Persons Law (Ally Zapp Law) (2004), Referenced in AWA
Massachusetts	Molly Bish (2000)	Referenced in AWA
Michigan	Michelle and Melissa Urbin (1991)	Michigan Sex Offender Registration Act (1995)
Minnesota	Jacob Wetterling (1989)	Jacob Wetterling Act (1994)
Minnesota	Katie Poiriet (1999)	Katie's Law (2000)
Minnesota	Jamie Cooksey (1990)	1992 Minn. Laws 1992-1993 and 1994-1995
Minnesota	Melissa Johnson (1991)	1992 Minn. Laws 1992-1993 and 1994-1995
Minnesota	Carin Streufert (1991)	1992 Minn. Laws 1992-1993 and 1994-1995
New Jersey	Amanda Wengert (1994)	Megan's Law in NJ (1994)
New Jersey	Megan Kanka (1994)	Megan's Law in NJ (1994) and Federal (1996)
New Jersey	Joan d'Allesandro (1973)	Joan's Law (1997)
New York	Suzanne Lyall (1998)[120]	Suzanne's Law, Section of 2003 PROTECT Act
North Carolina	Amy Jackson (1995)	Amy Jackson Law (1995)
North Dakota	Dru Sjodin (2003)	Referenced in AWA
Pennsylvania	Masha Allen (1998)	Masha's Law (2006), Section 707 of AWA
Texas	Pam Lychner (1990)	Pam Lychner Act (1996)
Texas	Ashley Estell (1993)	Ashley's Laws (1995)
Texas	Amber Rene Hagerman (1996)	Amber Hagerman Child Protection Act (1996)
Utah	Elizabeth Smart (2002)	Elizabeth Smart Law (2018)*, Referenced in AWA
Washington	Diane Ballasiotes (1988)	Washington State's Three Strikes Law (1993)
Wisconsin	Amie Zyla (1996)	Referenced in AWA

[120] Abduction is suspected but the case has never been solved.

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