Sex Offender Registries: Fear without Function?

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Abstract

I use three separate data sets and designs to determine whether sex offender registries are effective. First, I use state-level panel data to determine whether sex offender registries and public access to them decrease the rate of rape and other sexual abuse. Second, I use a data set that contains information on the subsequent arrests of sex offenders released from prison in 1994 in 15 states to determine whether registries reduce the recidivism rate of offenders required to register compared with the recidivism of those who are not. Finally, I combine data on locations of crimes in Washington, D.C., with data on locations of registered sex offenders to determine whether knowing the locations of sex offenders in a region helps predict the locations of sexual abuse. The results from all three data sets do not support the hypothesis that sex offender registries are effective tools for increasing public safety.

1. Introduction

Sex offender registries that contain the names, addresses, and photographs of sex offenders are now easily accessible on the Internet. People appear to value this information and pay to avoid living near a registered sex offender; homes close to a registered offender sell for about $5,500 less than comparable homes farther away (Linden and Rockoff 2008). The major justification for the existence of registries is to protect the public. Are sex offender registries effective at serving this purpose, or do they create fear without increasing public safety? Two channels exist through which registries could plausibly be effective. Registration implies a larger penalty for sex offenders, which could deter new offenders and recidivists not yet on a registry. For those offenders who are on a registry, registration could

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reduce recidivism through target hardening (potential victims' taking increased safety precautions) or through increased police monitoring.

This paper evaluates the effectiveness of such registries from two angles. First, I use variation across states in sex offender registration laws to explore whether the introduction of sex offender registration or community notification changes sex offense rates and whether offenders who are released into states with registration laws are less likely to recidivate. Second, I use variation across census blocks in Washington, D.C., to evaluate whether the presence of a registered sex offender in a block leads to higher rates of sex offense there. The first analysis addresses directly the effectiveness of sex offender registries. The second addresses the potential effectiveness of registries by considering whether where offenders live is predictive of where they offend.

I find little evidence to support the effectiveness of sex offender registries, either in practice or in potential. Rates of sex offense do not decline after the introduction of a registry or public access to a registry via the Internet, nor do sex offenders appear to recidivate less when released into states with registries. The data from Washington, D.C., indicate that census blocks with more offenders do not experience statistically significantly higher rates of sexual abuse, which implies that there is little information one can infer from knowing that a sex offender lives on one's block.

I use three different data sets and designs to test these different aspects of registry effectiveness. First, I answer whether the existence of a sex offender registry or public access to this registry via the Internet decreases the rate of rape and other sex offenses. I use state panel data from 1985 to 2003 that includes rape incidents and sex offense arrests reported to the Federal Bureau of Investigation (FBI) Uniform Crime Reporting (UCR) program. Demographic control variables and variables representing the general crime environment are included. I then run a fixed-effects regression to examine whether these measures declined after the introduction of a registry or public access via the Internet.

Second, I determine whether sex offender registries reduce the recidivism rate of offenders required to register compared with those who are not. I use a data set that tracks arrests and convictions of individual sex offenders for 3 years after their release from prison in 1994. I compare subsequent criminal records for sex offenders who should have been registered in the state sex offender registry upon release with criminal records for sex offenders who would not have been required to register upon release. The variation arises from the fact that six of the 14 states in the study had registration laws in effect by 1994, three enacted registration laws in 1994, and five had no registries by the end of 1994. I compare the two groups on two measures of recidivism: arrest and conviction.

Third, I test whether sex offender registries are potentially effective by determining whether locations with more registered sex offenders have higher rates of sexual abuse crimes. Or, to put it another way, I examine whether knowing the locations of sex offenders in a region helps predict the locations of sexual abuse crimes. I combine data on sex offenses in Washington, D.C., that include crime locations with the addresses of registered offenders from the Washington, D.C., sex offender registry. These locational data are then used to determine whether blocks with registered offenders are subject to increased rates of sexual assault. These data are useful for understanding whether releasing information on the location of a sex offender is valuable or helpful to the public.

Despite the importance of and controversy surrounding sex offender registries, there has been little empirical research on their effects. Adkins, Huff, and Stageberg (2000) and Schram and Milloy (1995) compare the behaviors of offenders who were required to register or were subject to community notification with those of offenders who were not in two specific states, Iowa and Washington, respectively. Both studies find no statistically significant difference in the recidivism rate between the two groups. These studies imply that neither registration nor community notification helps to reduce sex offender recidivism. However, both studies suffer from small sample sizes ($N = 435$ and 139, respectively) and from restriction to effects in just one state.

More recent work by Prescott and Rockoff (2009) and Walker et al. (2005) uses data across states and exploits the variation in timing of the start of registration to attempt to determine the effectiveness of sex offender registries. Prescott and Rockoff (2009) use data from the National Incident-Based Reporting System (NIBRS), a subset of the UCR, which allows them to use incident-level data rather than aggregate data, and they find mixed results for the effects of registration.[1] They attempt to separate out the effects of registration and notification on the relationship between offenders and victims, on deterrence, and on recidivism. They find that the number of sex offenses committed against known victims (relatives or others with a prior relationship with the offender) was reduced after registries were implemented. They find no effect on sex offenses committed against strangers. In terms of recidivism, the authors find that registered offenders may actually be more likely to recidivate when subject to community notification. They find no evidence that the registration requirement deters first-time offenders but do find evidence that registration itself (separate from community notification) reduces the recidivism of registered sex offenders—likely because of the increased information the registries provide to the police. When looking at arrests rather than incidents, they find no significant effect of registration or notification.

Walker et al. (2005) find similarly mixed results. They use monthly UCR counts of rape incidents to analyze registry effectiveness. Looking across states, they find that only three states experienced a significant decrease in rape incidents after the implementation of registration laws. They find that some states even had increases in rape incidents, though these increases were often not significant.[2]

[1] National Incident-Based Reporting System (NIBRS) data cover a small subset of states and within a state represent only cities with fewer than 1 million occupants.

[2] In addition to the few empirical papers on sex offender registries, some surveys have been conducted to extract opinions from sex offenders and professionals who work with sex offenders about the registries and community notification. A survey of the opinions of mental health professionals who work with sexual abusers found that most believed that community notification would

The paper continues as follows. Section 2 provides background on sex offender registries. Section 3 presents data and results from the state-level panel data. Section 4 presents data and results from the recidivism analysis. Section 5 presents data and results from the analysis of locational data in Washington, D.C. Section 6 concludes.

2. Background

A sex offender is anyone who is convicted of a sex crime. The crimes that qualify as sex crimes vary by state, but most states include rape (forced and statutory), sexual assault or battery, child molestation or any sexual conduct with a minor, production or possession of child pornography, and attempts to commit any of these crimes. Even though California began registering sex offenders as early as 1947, sex offender registries did not become widespread until the 1990s,[3] after state registries became federal law with the passing of the Jacob Wetterling Act in 1994.[4] Over the next few years, more states enacted sex offender registries to comply with the provisions of the Wetterling Act, and by the end of the 1990s every state was registering sex offenders. These registries contain information about convicted sex offenders including name, address, physical characteristics, and sex crime committed. Sex offenders must periodically report to a local authority, usually a local police department, and verify their current address. Although these registries were originally meant for use by police, community notification about registered sex offenders was pioneered in Washington in 1990. Community notification subsequently became part of federal legislation in 1996 with the addition of Megan's Law, an amendment to the Wetterling Act.[5] This notification can be carried out through a variety of both passive and active means, including community meetings and flyers.[6] In 1997, some states began putting registry information on the Internet. By the summer of 2006 all states had some subset of their registry available for public search on the Internet. In many states the entire contents of the registry are available for search online (Alabama, Florida, Idaho, North Carolina, and Texas, among others); others

be ineffective in preventing future sex crimes (Malesky and Keim 2001). Surveys of sex offenders themselves reveal that a majority experience some type of negative consequence from registries, such as harassment, loss of employment, or feelings of isolation. These surveys reveal mixed feelings about the effectiveness of registration and community notification (Zevitz and Farkas 2000; Levenson and Cotter 2005).

[3] See California Office of the Attorney General, Megan's Law—Sex Offender Registration and Exclusion Information (http://www.meganslaw.ca.gov/sexreg.htm).

[4] In October 1989 in St. Joseph, Minnesota, Jacob Wetterling, age 11, was abducted by an armed, masked man. To this day it is unknown what happened to Jacob, but his parents began the Jacob Wetterling Foundation and have been advocating for child safety and protection laws ever since.

[5] Megan's Law is named for Megan Kanka, who was murdered at age 7 by a prior sex offender in her neighborhood, although since community notification was not in effect his neighbors did not know he had committed offenses in the past.

[6] For a discussion of the possible effects of non-Internet community notification, see Prescott and Rockoff (2009), who differentiate various forms of notification in addition to the provision of registries' information on the Internet.

include only sex offenders in a certain tier or category that indicates the severity of the crime, such as Arizona, which only lists level 2 and 3 offenders, or Minnesota, which only lists level 3 offenders.

States also differ as to whether the registration or public notification requirement is retroactive, as well as what event in the criminal process triggers the effective date of the requirement. For example, in Connecticut the registry became effective on October 1, 1998, and it applies to anyone released from state custody on or after October 1, 1988. However, in Delaware the law applies only to those convicted on or after the date the registry became effective, June 27, 1994. Registration and retroactivity are not without controversy. Several lawsuits have been filed on behalf of sex offenders, and two went to the Supreme Court of the United States. In both cases the Court upheld the constitutionality of registration. The Missouri Supreme Court found similarly with one key exception—the registration requirement could not be applied retroactively to sex offenders convicted before the law's inception on January 1, 1995.[7]

State governments cite public safety and protection as the reasons for having sex offender registration and notification laws. These justifications for registries and public access typically relate to the sex offenders who are required to register, in terms of specific deterrence or target hardening, rather than to general deterrence among those who have yet to commit a crime. The Maryland Department of Public Safety and Correctional Services notes that the information on its Web site is "provided as part of the State's effort to protect children and others from those with histories of crimes against children and other sex offenses."[8] Several states also refer to sex offenders' high rates of recidivism. For example, Mississippi's legal code states that "[t]he Legislature finds that the danger of recidivism posed by criminal sex offenders and the protection of the public from these offenders is of paramount concern and interest to government" (Miss. Code Ann. sec. 45-33-21 [2001]). Most studies, however, show that sex offenders are less likely to reoffend than many other types of criminals (Langan, Schmitt, and Durose 2003; Langan and Levin 2002; ODRC 2001; Arizona Department of Corrections 1998).

A public registry could increase public safety through several means, including reducing the incentive to commit sex offenses, reducing recidivism, spurring target hardening, and increasing police monitoring of known sex offenders. Registration and public access increase the cost of committing a sex offense for an offender not on the registry. Once released from jail, the offender would have to report to a local authority at least once a year (if not more often) in order to verify his location for anywhere from 10 years to life. His identity might be

[7] The two Supreme Court cases are *Smith v. Doe,* 538 U.S. 84 (2003), regarding the law in Alaska, and *Connecticut Department of Public Safety v. Doe,* 538 U.S. 1 (2003). The Missouri Supreme Court case is *Jane Doe I v. Thomas Phillips,* 194 S.W.3d 833 (June 30, 2006), in which Missouri sex offenders filed to challenge the constitutionality of Missouri's version of Megan's Law.

[8] See Maryland Department of Public Safety and Correctional Services, Sex Offender Registry: Downloadable Registry Listings (http://www.dpscs.state.md.us/onlineservs/sor/download.shtml).

revealed to the public via the Internet or through community meetings and flyers. Levenson and Cotter (2005) find that one-third of the sex offenders they surveyed in Florida experienced some sort of adverse consequences from having to register, including harassment and job loss, and a majority suffered negative emotions such as stress or hopelessness. This increased cost of offending could help deter unregistered offenders from committing a sex offense and thus increase public safety.

A registry could also reduce recidivism through its effects on the offenders already on the registry. Since offenders are required to register with a local authority, the local police or sheriff's department knows them and is aware of their current location. This increased monitoring may raise the probability of an offender being caught if he chooses to recidivate and thus may increase the expected cost of reoffending conditional on being on the registry. Having sex offenders' information on the Web for public access could also decrease further crime through target hardening. The increased awareness of registered offenders among members of their neighborhoods could increase the cost of finding a victim and thus cause offenders to change their behavior. In their survey of sex offenders, Levenson and Cotter (2005) do find that some sex offenders (22 percent) thought that registration and community notification helped to prevent them from reoffending.

The size of a registry may affect the probability of being caught for both registered and unregistered sex offenders. As the number of registered offenders increases, more sex offenders are being monitored and more information is being given to the police, which could increase the probability of catching offenders. However, as this number increases there could also be a crowding effect—there are more and more offenders to monitor, likely by a relatively fixed number of public safety officials, and this may decrease the probability that any given registered offender is considered as a suspect or arrested in a reported sex crime. Assuming that police first turn to registered offenders in the area when a sex crime occurs, having more registered offenders could decrease the probability of arrest for an unregistered sex offender. Registry size could also affect actions by other individuals in the community—if I see on the Internet that there is one registered offender on my block I may be more cautious near that house, but if I see that there are 20 then I may be more cautious in general.

It is not clear that the only effect of sex offender registries will be positive. For one thing, registration laws may compel offenders to commit a different though similar crime that would not result in their being placed on a registry. Moreover, as mentioned above, reintegration costs for offenders are high since the public is aware of their status as sex offenders. Lower property values near offenders, instances of vigilante justice, and new laws restricting where sex offenders can live and work show that the public tends to react with fear and suspicion to registered offenders. Some sex offenders have been fired from their jobs or expelled from their homes because of their status as registered offenders (Levenson and Cotter 2005; Zevitz and Farkas 2000). In a sense, registered sex offenders are stigmatized and shunned by society. This implies that being a registered sex offender may reduce the offender's outside opportunities (in terms of jobs and social life) and thus lower the opportunity cost of choosing crime over legal activities. Braithewaite (1989) contends that this labeling of criminals is a form of "disintegrative shaming" that drives them to continue their criminal behavior and may make them more likely to recidivate.

Further, for registered offenders, already being on a registry eliminates the possible deterrent effect of having to register for committing a sex crime. So again the opportunity cost of committing another sex offense is lowered because the offender is already experiencing the costs of reintegration and will continue to do so whether or not he reoffends. Prentsky, Knight, and Lee (1997, p. 9) reflect the opinion that community notification may increase recidivism through increased stress caused to offenders by "threats of bodily harm, termination of employment, on-the-job harassment, and forced instability of residence," all of which were experienced by at least some offenders in the surveys of Levenson and Cotter (2005) and Zevitz and Farkas (2000). A similar conclusion is reached by Prescott and Rockoff (2009), who find evidence that recidivism rates of registered offenders may be higher rather than lower after community notification is instituted.

3. Panel Data

3.1. Data Description

In order to test the effectiveness of sex offender registries, I use state panel data that include information on when each state enacted its registry, when it first allowed public access to the registry via the Internet, the size of the registry, crime rates, and controls. No comprehensive repository of information on each state's sex offender registry existed at the time of initial analysis, so for each state I collected data on the date the registry was enacted, the date the registry was placed on the Internet for search by the public, and whether the registration requirement was retroactive (and if so, to what date). I collected the data from a variety of sources, including state Web sites, personal contact with relevant representatives over phone or e-mail, information collected by the Klaas Kids Foundation,[9] and state legal codes. Data from Massachusetts and Arizona are excluded from the analysis.[10] Wherever feasible I have cross-checked my dates to ensure that they are as accurate as possible. The dates of enactment of both

[9] The parents of Polly Klaas, a 12-year-old girl who was kidnapped and murdered in 1994, founded the Klaas Kids Foundation to help stop crimes against children.

[10] Determining when Arizona began its sex offender registry proved very difficult. Mixed results were found from different sources, including conversations with different people within the Arizona Department of Public Safety; because no date could be cross-checked, I chose to not use Arizona in the analysis. Massachusetts went through a series of court cases that alternately suspended and reenacted sex offender registration through the late 1990s, which thus makes any analysis of the effectiveness of the registry difficult.

the registries and public access via the Internet and retroactive dates are presented in Table 1. Prescott and Rockoff (2009) use only the 13 NIBRS states in their data, and we disagree on dates of enactment for three states: Connecticut, Kentucky, and North Dakota. Appendix A includes more thorough justifications for my effective dates for those three states.

Most states enacted their registry laws after Congress passed the Wetterling Act in 1994. Some states, such as California, Washington, and North Dakota, had registries before the 1994 law. The earliest a state put any portion of its registry on the Internet was 1997, and Oregon and South Dakota were the last to do so, in the summer of 2006. Figures 1 and 2 show cumulative distribution functions that illustrate the rapid growth in the percentages of states with registries and public Internet access to the registries after their initial introductions.

States vary as to whether or not their registration requirement is retroactive; that is, not all states require registration for convictions that took place prior to registry enactment. For the purpose of this study I define a state's registration requirement as not retroactive if the state's registry was theoretically empty on the day it began. For example, Alabama's sex offender registry was enacted on September 6, 1967, and requires sex offenders released on or after September 6, 1967, to register; therefore, it was theoretically empty on the day it began, even though it applied to crimes that were committed before the enactment date. Eighteen states have registry requirements that are retroactive, while 33 do not (Washington, D.C., is included in the total). Of those that are retroactive, nine are completely retroactive—that is, the law applies both retrospectively and prospectively to any sex offender ever convicted of a sex offense.

I combined the registry date information I collected with data on forcible rape incidents (as opposed to statutory rape) and arrests for forcible rape or sex offenses per state from 1985 to 2003 from the FBI's UCR, data on registry size from various sources, and demographic characteristics from the Census Bureau.[11] The FBI breaks up the UCR into two categories, parts 1 and 2. Data on all known offenses are reported for part 1 crimes, whereas only data on offenses for which arrests are made are reported for part 2 crimes. Forcible rape is considered a part 1 offense; thus, states report data on all known forcible rapes. This means that if a rape is reported to a police department but an arrest is never made, that rape is still reported in the UCR as an incident. Sex offenses other than rape and commercial prostitution are considered part 2 offenses. Thus, for sex offenses other than forcible rape, only arrest rates are available.

I also collected data on the size of registries. Finding historical data on registry sizes proved difficult. The information that exists is spotty, with individual data points coming from many, varying sources. I was able to collect data on the size

[11] The Uniform Crime Reporting (UCR) arrest data were provided by the Bureau of Justice Statistics on a CD-ROM. Some data points for sex offense arrest rates are missing from the CD-ROM. The missing information was filled in from data directly from the state whenever possible; linear interpolation was used to estimate remaining missing data. Sources of state data are available from the author upon request.

of registries from various sources representing the years 1996, 1998, 2001, 2005, and 2008.[12] For states with nonretroactive registries, I coded the registry size as 0 in the year that it began. In the spirit of Prescott and Rockoff (2009), in order to interpolate the registry size in the years for which data were missing, I assumed a quadratic state-specific trend and regressed size on time and time squared, allowing state-specific slopes. I then filled in the missing years with the predicted values from the regression. In order to account for the fact that size is predicted and not known, when estimating I implement a multiple imputation model, explained in more detail below.

3.2. Empirical Strategy

I use a fixed-effects regression model to determine how crime rates changed after implementation of a sex offender registry or public access to it via the Internet. Two different dependent variables are explored: the natural log of the rape incidence rate and the natural log of the sex offense arrest rate.[13] The regression specification is as follows:

$$\ln \text{Crime}_{s,t} = \text{Registry}_{s,t}\beta + \text{Internet}_{s,t} + \delta_t + \gamma_s + \boldsymbol{X}_{s,t}\varphi + \varepsilon, \qquad (1)$$

where s represents the state and t represents the year, from 1985 to 2003. The variable Registry is a dummy set equal to one if the state had a registry in that year; similarly, Internet is a dummy equal to one if the state allowed access to the registry via the Internet in that year. Time fixed effects are represented by δ_t and state fixed effects by γ_s. The variable $\boldsymbol{X}$ is the vector of controls. Controls include the arrest ratio for rape (arrests reported divided by incidents reported), the natural log of the rate of other violent crimes not including rape,[14] and demographic information.[15] The dependent variable takes on either of two definitions of the sex offense rates, as defined above.

To use size information in my analysis I need to account for the fact that the size of registries for some years is estimated (Murphy and Topel 2002). I implement a multiple imputation analysis to estimate the coefficients and standard errors (Rubin 1987). Essentially, instead of just filling in each missing value with a single estimate, I create a vector of estimates by taking draws from the predictive distribution of the initial regression. This creates 10 different imputed data sets, each with one candidate estimate of the size of the registry for the years in which

[12] The 1996 data are from Matson (1996); 1998 and 2001 data are from Adams (2002); 2005 data are from Parents for Megan's Law, Number of Registrants Reported by State/Territory (http://www.parentsformeganslaw.org/public/meganReportCard.html); 2008 data are from National Center for Missing and Exploited Children, Map of Registered Sex Offenders in the United States (http://www.solresearch.org/~SOLR/cache/date/20080717-NCMEC-SOmap.pdf).

[13] Natural logs are used so that coefficients can be read as percentage changes in the crime rate.

[14] The Federal Bureau of Investigation (FBI) defines violent crimes as murder, rape, aggravated assault, and robbery; property crimes are arson, auto theft, burglary, and larceny.

[15] Demographic controls include population density, real per capita income, real per capita unemployment payments, and percentages in various age, race, and sex categories (Ayres and Donohue 2003; Kuziemko and Levitt 2004).

Table 1
Sex Offender Registries: Significant Dates

State	Registry Begins	Retroactive	Qualifying Event	On Internet
Alabama	September 6, 1967	No	Release	August 1, 1998
Alaska	August 10, 1994	No	Conviction	June 12, 1997
Arizona	June 1, 1996	Yes	All	1998
Arkansas	August 1, 1987 (h) August 1, 1997 (o)	No	Conviction	January 1, 2004
California	1954	No	Conviction	December 15, 2004
Colorado	July 1, 1991 (c) October 1, 1998 (o)	No No	Release (c) Conviction (o)	July 1, 2001
Connecticut	October 1, 1998	October 1, 1988 (v) No (o)	Release	January 1, 1999
Delaware	June 27, 1994	No		November 1, 1998
District of Columbia	June 1, 2000	No		March 1, 2001
Florida	October 1, 1993 (v) October 1, 1997 (o)	No	Commission (v) Release (o)	October 1, 1997
Georgia	July 1, 1996	No	Conviction	1998
Hawaii	January 1, 1996 (v) July 1, 1997 (o)	Yes	All	May 1, 2005
Idaho	July 1, 1993	No	Conviction	2002
Illinois	August 15, 1986	No	Conviction	July 1, 2002
Indiana	July 1, 1994	Yes	All	January 1, 2003
Iowa	July 1, 1995	No		July 1, 1998
Kansas	July 1, 1993	No	Conviction	April 24, 1997
Kentucky	July 15, 1994	No		April 1, 2000
Louisiana	June 18, 1992	No	Custody	May 1, 2000
Maine	September 1, 1996	January 1, 1982	Sentencing	October 1, 2003
Maryland	October 1, 1995 (c) July 1, 1997 (o)	No	Commission	2002
Massachusetts		August 1, 1981	Release	August 1, 2004
Michigan	October 1, 1995	No	Conviction	1999
Minnesota	July 1, 1991	No		January 1, 1997
Mississippi	1994	Yes	All	1997
Missouri	July 1, 1979	No	Conviction	June 18, 2004
Montana	1989	No		2001
Nebraska	January 1, 1997	No	Conviction	2000
Nevada	January 1, 1998	July 1, 1956	Conviction	May 1, 2004
New Hampshire	1993	January 1, 1988		2001
New Jersey	October 31, 1994	No		February 21, 2002
New Mexico	July 1, 1995	No		July 1, 2000
New York	January 21, 1996	No		May 11, 2000
North Carolina	January 1, 1996	No		May 11, 2000
North Dakota	1991	July 31, 1985	Conviction	November 1, 2001
Ohio	July 1, 1997	No		January 1, 2001
Oklahoma	November 1, 1989	No	Commission	January 29, 2005
Oregon	October 3, 1989	No		June 29, 2006
Pennsylvania	April 21, 1996	Yes	All	November 24, 2004
Rhode Island	1992	Yes	All	April 13, 2005
South Carolina	July 1, 1994	No	Conviction	September 1, 1998
South Dakota	1994	Yes	All	2006
Tennessee	January 1, 1995	No		July 1, 1997

Table 1 (*Continued*)

State	Registry Begins	Retroactive	Qualifying Event	On Internet
Texas	September 1, 1991	September 1, 1970	Conviction	January 1, 1998
Utah	March 30, 1983	No	Release	July 1, 1998
Vermont	September 1, 1996	No		October 1, 2004
Virginia	July 1, 1994	No	Conviction	July 1, 1999
Washington	February 28, 1990	Yes	All	March 1, 2005
West Virginia	1993	Yes	All	September 1, 1998
Wisconsin	December 25, 1993	No	Conviction	June 1, 2001
Wyoming	1994	January 1, 1985	Sentencing	

Note. Single-letter abbreviations in parentheses indicate the type of sex offender (c = child; h = habitual; v = violent; o = other). For states with retroactive registry laws, the past date to which the law extends is indicated, if known, and "yes" indicates that the law applies both prospectively and retroactively to all offenders ever convicted of a sex offense. The qualifying event is, when known, what event in the criminal process must have occurred on or before the effective date of the registry for an offender to be required to register. "All" indicates that all convicted sex offenders—regardless of date of conviction, release, or sentencing—must register. When only a month and year are available, the date is listed as the first of that month. When only a year is available, it is listed alone. Information on when Massachusetts began its sex offender registry is excluded. In Utah, the registry was made available to the public via the Internet in July 1998, but in September 1998 a federal appeals case was filed (*Femedeer vs. Haun*, 227 F.3d 1244 [10th Cir. 2000]) and the Web site was frozen, with no new data added or old data removed, until December 2000, when the case was decided in the state's favor and the Web site was once again operational. I use the July 1998 date because I assume that offenders would not be as informed about the court case as about the Web site and that they would assume they would be named on the Web site. Information on when Wyoming first put its registry on the Internet was unavailable, even after several phone calls and e-mails with the Wyoming Sex Offender Registration Program.

size information is missing. Standard analysis is performed on each data set, and results are combined following Rubin (1987) to estimate the reported coefficients and standard errors via Rubin's rules.[16] The resulting estimates incorporate the uncertainty caused by the missing size data. Note that I also have run all the analyses in this section using a bootstrap method to deal with the standard errors for the imputed data rather than a multiple imputation method, and I obtained the same results.[17]

I also consider that the effects on crime rates may be different in states with retroactive registries than in states with nonretroactive registries. One might expect this difference because with an empty registry there is not yet any effect of police monitoring or increased probability of arrest—there is only the deterrent effect on new offenders. On the other hand, a state whose registry is full would experience both effects. I address this difference by adding an interaction between measures of the registry beginning and the registry being retroactive.

Endogeneity is often a problem when dealing with crime policy like this. If the introduction of a registry were endogenous to the rate of sex offenses, then states with higher sex offense rates would have implemented registries earlier, thus distorting the results. However, the history of sex offender registries suggests

[16] This analysis was made easy in Stata via the programs mim and ice written by Patrick Royston, to whom I am indebted.

[17] The bootstrap estimates used 200 replications and were performed on a singly imputed data set using the imputation method specified previously.

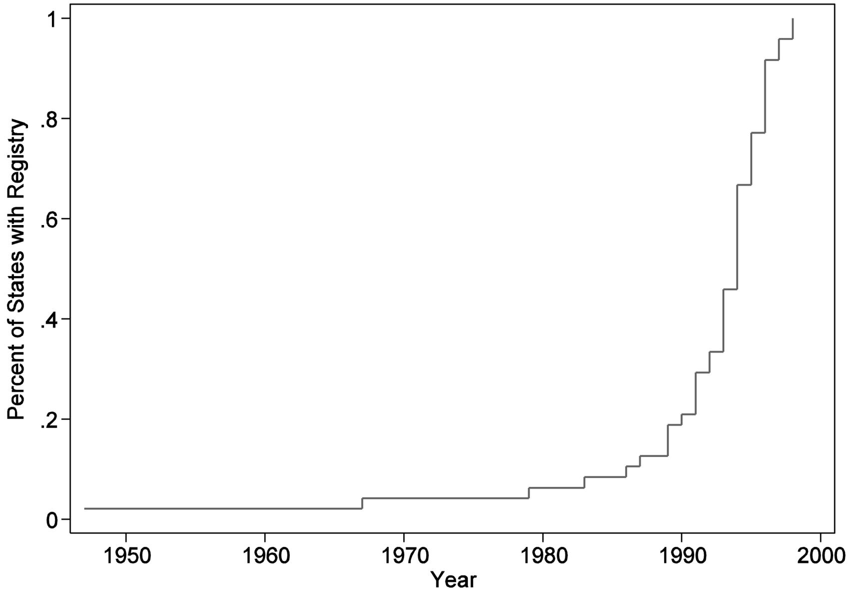

Figure 1. States with sex offender registries

that the date of creation is largely exogenous and due to random shocks. It is revealing that all major laws pertaining to sex offender registration were enacted on the basis of high-profile cases, not because of a multitude of cases in general. New Jersey pioneered Megan's Law after the brutal rape and murder of Megan Kanka by a convicted sex offender living in her town, not because New Jersey was experiencing a high incidence of these events. Similarly, the Wetterling Act, Jessie's Law, and the Adam Walsh Act are named after children who were victims of highly publicized sexual crimes. These singular events can be considered random shocks and not representative of the amount of crime in each state. Further, the passing of the Wetterling Act in 1994 removed the decision as to whether to have a registry from the states' hands. Those states that enacted their registries before 1994—California, Idaho, Kansas, Louisiana, Minnesota, Missouri, Montana, New Hampshire, North Dakota, Oregon, Texas, and Washington—are on the whole not considered to have high rates of sex offenses. In fact, if you rank the states by their 1995 rape incidence rate, these states tend to fall in the middle of the distribution. The endogeneity issue is explored further in Section 3.3.

3.3. Results

Figures 3 and 4 give a preliminary idea of whether registration or Internet access to registries had any effect on crime rates. The figures show average crime

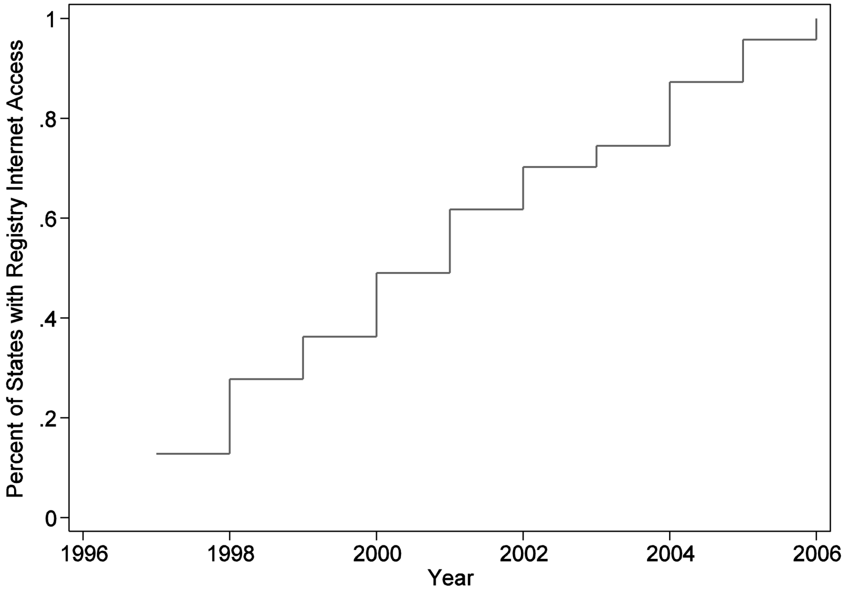

Figure 2. States with public Internet access to sex offender registries

rates across states over time, with time 0 being the point at which registration or public Internet access, respectively, began. The *x*-axis shows time before or since the registration (or Internet notification) began, and the *y*-axis represents the average crime rate in states *x* years before or since registration. The crime variables (sex offense arrests and incidents of rape, violent crimes not including rape, and property crimes) are measured per 100,00 in the population. In Figure 3, there is no real change in trend for any of the four categories of crime after a registry is implemented. For sex offense arrest rates, there is a general downward trend that continues after the implementation of a registry, though just before a registry is introduced there appears to be a very slight upward movement that is reversed after the registry. Figure 4 shows the results of the beginning of Internet access to a registry. Again, there is no break in trend for any of the four crime specifications. Overall, the figures indicate little change in rates of sex offense crimes (or other crimes, as we would expect) after registration or public notification via the Internet.

The results from the fixed-effects regression using the state panel data are presented in Table 2. The first specification uses the natural log of the rape incidence rate as the dependent variable. The coefficients of interest are on the dummy variables Registry and Internet. The results suggest, if anything, a positive impact of registration and notification on aggregate crime rates, though both coefficients are small and not statistically significant. I can reject decreases of

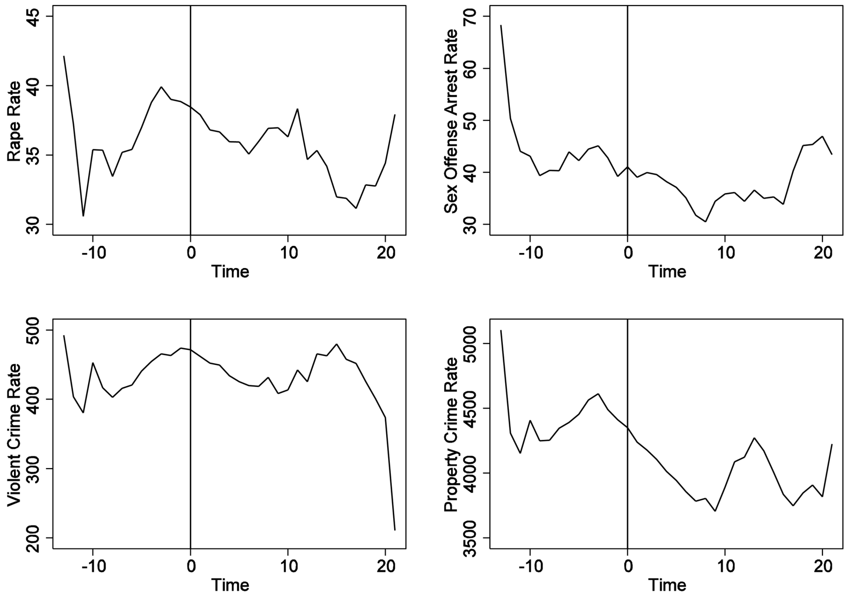

Figure 3. Effect of sex offender registries on crime over time

more than 5 percent in rape incidents after registry implementation and of more than 6 percent after Internet notification. These clearly are socially important decreases; however, I also cannot reject increases of as much as 5 percent after the registry and of almost 10 percent after Internet notification. Not surprisingly, states with higher rates of violent crime also tend to have statistically significantly higher rates of rape incidents. And finally, a higher arrest ratio for rape is associated with a significant decrease in rape incidents—the expected result (see Levitt [1998] for comparisons of own-crime arrest ratios to incidence rates).

Although no measure of crime is perfect, incident reports are one of the better proxies available. Since incident reports were available only for rapes and not for other sex offenses, the second specification in Table 2 uses arrests for sex offenses as the dependent variable to try to determine the effect on a broader range of sex crimes. In this specification there is, again, a positive coefficient on Registry; I can reject a decrease in arrests for sex offenses of more than 4 percent due to the existence of a registry in the state but cannot reject an increase of up to 17 percent. However, putting the registry on the Internet has a significant negative effect, resulting in a significant decrease in arrests for sex offenses of approximately 17 percent.[18] In regressions run using Prescott and Rockoff's

[18] Per a referee's suggestion, I also ran each regression without control variables to address possible concern about unobservables, and I found that the coefficients were all similar in sign and magnitude.

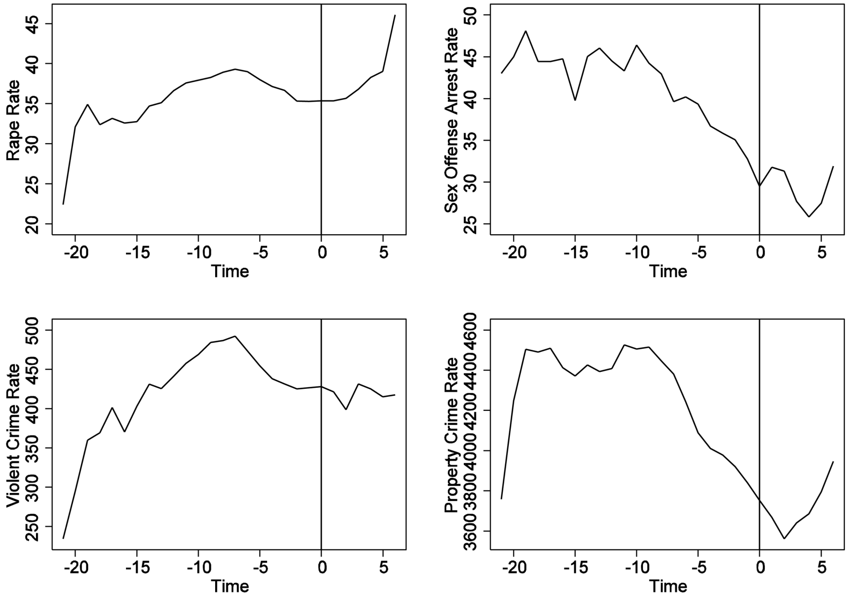

Figure 4. Effect of public Internet access to sex offender registries over time

(2009) effective dates for registries for the states over which we disagree, the results remain similar.

Though the interpretation of the coefficients on rape incidents is rather clear, it is not obvious how to interpret the coefficients on arrests nor, thus, what that significant decline truly means. The interpretation depends on the percentage of crime incidents for which there is an arrest, or the arrest ratio (arrests over incidents). If arrests decrease and the number of actual incidents stays the same, then a decline in arrests does not imply that registries are effective. If arrests decrease and incidents decrease, then registering could be seen as effective. A clearer picture could be seen if there were data on incidents of sexual assault, but unfortunately the UCR reports arrests only for part 2 offenses. If we assume that arrests are a good proxy for incidents, however, the results indicate that sex offenses decreased significantly after registry contents became searchable on the Internet.

Clearly we might expect there to be varying trends between states in sex offense arrest rates or rape incidents that could affect the results. As noted in Prescott and Rockoff (2009), having both the size of the registry and state-specific trend variables in the regression reduces the amount of information being used to identify the coefficients, since size closely mimics the state trend. However, as a robustness check I added a state-specific trend to the regressions in columns 1 and 2 of Table 2, and the results did not change in size or magnitude.

Table 2

Changes in Rape and Sex Offense Arrest Rates after Registry and Public Notification

	Rape (1)	Sex Offense (2)	Rape (3)	Sex Offense (4)	Rape (5)
Registry	.005	.068			.008
	(.027)	(.052)			(.028)
Internet	.017	−.167+			.017
	(.040)	(.087)			(.040)
Registry × Retroactive					−.025
					(.029)
Size of Registry	−.000	−.011	−.000	−.011	−.000
	(.004)	(.023)	(.004)	(.022)	(.004)
ln(Violent Crime Rate)	.204*	.103	.207*	.075	.204*
	(.078)	(.152)	(.079)	(.154)	(.079)
ln(Arrest Ratio for Rape)	−.065*	.648**	−.066*	.658**	−.066*
	(.024)	(.099)	(.024)	(.099)	(.024)
Endog			−.017	−.046	
			(.015)	(.047)	
Webendog			.011	.025	
			(.021)	(.035)	
Constant	3.330	4.623	3.290	5.111	3.329
	(2.143)	(3.487)	(2.093)	(3.450)	(2.143)

Note. All dependent variables are natural logs of rates (Rape is the natural log of the rape rate), so coefficients are interpreted as percentage change in the crime rate. Numbers in parentheses are robust standard errors clustered by state and are calculated through multiple imputation. The variable Registry (or Internet) equals one if a state had a registry (or Internet access to the registry) in that year and zero otherwise. The variable Endog (or Webendog) equals one in the year before the registry (or Internet access to the registry) went into effect. The size of the registry is a predicted value and is measured as offenders per 1,000 in the population. Controls included but not listed are state and year fixed effects and demographic characteristics. Violent crimes are murder, robbery, and aggravated assault. $N = 912$.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

Although I believe that the history of sex offender registries points to the lack of an endogeneity issue, the panel data also can be used to address the possibility of endogeneity empirically. In columns 3 and 4 of Table 2 I add dummies for the year before the registry or public access via the Internet began (Endog and Webendog). If anything, the coefficient indicates that in the year before a given state enacted a registry, numbers of rape incidents were slightly lower. A similar result is found for sexual assault arrest rates, which suggests that the creation of the registry was not driven by higher rape or sexual assault rates and thus may be treated as plausibly exogenous.[19] The fact that registry laws have often been enacted after a single high-profile incident rather than in response to a multitude of incidents over time is probably a stronger argument, but at the very least the empirical results do not dispute the assumption of nonendogeneity.

[19] Admittedly, the cause could be that sex offenders knew a registry was going to be enacted and changed their behavior accordingly; however, the previous results do not show offenders changing behavior much in response to actual registries, so it seems unlikely that there was a lot of behavioral change in anticipation of registries.

Table 3
Changes in Rates of Other Crimes

	Burglary (1)	Auto Theft (2)	Violent Crime (3)
Registry	−.014	.033	.039
	(.024)	(.030)	(.033)
Internet	−.023	−.040	.077^{+}
	(.018)	(.044)	(.042)
ln(Arrest Ratio for Rape)	−.033^{+}	−.034	.011
	(.018)	(.030)	(.027)
ln(Violent Crime Rate)	.292**	.489*	
	(.096)	(.143)	
Size of Registry	.000	.000	−.000
	(.000)	(.001)	(.001)
Constant	5.648**	3.550	7.429**
	(1.085)	(2.045)	(1.334)

Note. All dependent variables are natural logs of rates (Burglary is the natural log of the burglary rate), so coefficients are interpreted as percentage change in the crime rate. Numbers in parentheses are robust standard errors clustered by state and are calculated through multiple imputation. The variable Registry (or Internet) equals one if a state had a registry (or Internet access to the registry) in that year and zero otherwise. The size of the registry is a predicted value and is measured as offenders per 1,000 in the population. Controls included but not listed are state and year fixed effects and demographic characteristics. Violent crimes are murder, robbery, and aggravated assault. $N = 912$.
$^{+}$ $p < .10$.
* $p < .05$.
** $p < .01$.

In column 5 of Table 2 I rerun the specifications in columns 1 and 2 but add an interaction between the measure of having a registry and the measure of whether the registry is retroactive to see if there are differing effects on retroactive and nonretroactive states. The results show that retroactive states experienced slight declines in rape incidents as compared with nonretroactive states, but the difference is not statistically significant—I can reject a decrease of more than 6 percent but cannot reject an increase of up to 23 percent over nonretroactive states. Taken at face value, this result would mean that states that started with theoretically empty registries did not experience as much of a decline in rape incidents as states with offenders on their registries when they began.

Though in general I do not find strong evidence that states with sex offender registries had lower sex offense rates, I look at the effect of registries on other crimes as a falsification test. I first look at two crimes that might be expected to be unrelated to sex crimes and thus unaffected by sex offender registries: burglary and auto theft. Table 3 reports results from the regression in Table 2 but with burglary and auto theft as the dependent variables. Both coefficients of interest, those on Registry and Internet, are small and statistically insignificant, as expected.[20] In the regression that uses violent crimes (not including rape) as the dependent variable, violent crime incidents rise statistically significantly after

[20] If I run the regression with all property crimes together, the results are similar.

a registry is put on the Internet. While at first this may call into question the specification, since I am getting a result in what is essentially a falsification test, it seems highly plausible that violent crimes are substitutes for sex offenses, such that when the cost of committing a violent sex crime increases because of Web notification, some criminals choose to commit other violent crimes (assault, murder, or robbery). (Levitt [1998] posits that all violent crimes are substitutes.)[21]

On the whole, the panel data do not present strong evidence that registries are effective in reducing rape or sex offenses, though they do offer limited evidence of reducing sex offense rates.

3.4. Comparison to Prescott and Rockoff

Prescott and Rockoff (2009) add interactions between the measures of size and registry effective date as well as size and Internet notification effective date. Their interpretation is that the coefficients on the interaction terms represent deterrence of registered offenders and the coefficients on the uninteracted terms represent deterrence of new offenders. They find that theoretically empty registries are associated with higher incidence of sex offenses, but once size increases from 0 this effect begins to decrease, and it becomes negative after approximately three to four offenders per 100,000 in the population have been put on the list (their interpretation being that registration does not deter unregistered offenders but does reduce recidivism). The results in Table 2 appear to indicate a stronger negative effect for those states whose registries were not theoretically empty when they began, though they do not differentiate between unregistered and registered offenders.

In Table B1, column 1, I add interactions between the measures of size and registry effective date as well as size and Internet notification effective date to mimic Prescott and Rockoff's (2009) specification in my data, using rape incidents as the dependent variable. Column 3 copies results from their analysis, which uses sex offense incidents from NIBRS as the dependent variable, for comparison (Prescott and Rockoff 2009, table 3, col. 4). I obtain coefficients that are similar in sign, except for Internet notification, on which the coefficient is smaller in magnitude and not statistically significant. Clearly, differences in significance could be an artifact of the size of our data sets; given that Prescott and Rockoff have incident-level data available, they have many more observations than the state-by-year observations that I have. Other differences could come from the fact that NIBRS represents a different population than UCR, as discussed in Section 1, or that for three states in NIBRS we disagree on registry effective dates. To account for some of these potential influences, I reran the specification from Table B1, column 1, using just NIBRS states and Prescott and Rockoff's dates. The results are roughly similar to those in column 1. However, NIBRS

[21] This proposition may be further bolstered by the fact that, in regressions run separately for aggravated assault, murder, and robbery, the statistically significant increase remains only for aggravated assault—the crime most intuitively substitutable for sexual assault.

does not cover cities with more than 1 million people—and since my data set only covers states and not cities, it is impossible to eliminate cities with more than 1 million people. Thus, I cannot reject the possibility that the differing results arise from the different populations being covered.[22]

Note that applying their interpretation to these results, ignoring problems of significance, would imply that "community notification deters first-time sex offenders, but may increase recidivism by registered offenders" (Prescott and Rockoff 2009, abstract). This interpretation rests on the assumption that "the potential impact of registration on the punishment level of forward-looking, unregistered individuals . . . would not depend (or depend very little) on the size of the registry" (p. 12). However, it is plausible that registry size could affect unregistered offenders through the arrest probability or the relative utility of offending even if it does not affect punishment levels, which makes this interpretation of the coefficients a little murkier.[23]

4. Recidivism Analysis

4.1. Data Description

With the aggregate crime data it is difficult to separate effects on recidivism and deterrence. I use a data set that tracks the behavior of individual offenders in order to test whether individual recidivism is different for offenders who had to register versus those who did not. The Bureau of Justice Statistics (BJS) collected data on a subset of prisoners released from prison in 1994 in 15 states (Arizona, California, Delaware, Florida, Illinois, Maryland, Michigan, Minnesota, New Jersey, New York, North Carolina, Ohio, Oregon, Texas, and Virginia), which included all sex offenders released into those states (Langan, Schmitt, and Durose 2003).[24] Arizona, however, is not used in my analysis (see note 10). The information in the data set includes demographic characteristics of the released prisoners, such as race, date of birth, and gender, as well as criminal history, including past arrests and convictions. The BJS tracked these prisoners for 3

[22] This critique of the coverage of NIBRS data is not meant to invalidate the analysis of Prescott and Rockoff (2009). The NIBRS has many advantages in addition to disadvantages. Rather, this critique seeks to help explain the disparate results. Understanding what is going on both at the aggregate national level and for the subpopulations represented by NIBRS is important to understanding the effectiveness of sex offender registries.

[23] If having more registered offenders leads to increased arrest probability for those on the registry because of increased surveillance, this seems to imply that police are likely looking at registered offenders first—and in turn this would directly imply that unregistered offenders would have a lower probability of arrest, or at least a longer time to arrest. Target hardening could also change commensurably with size and could affect the cost of finding a victim (and thus the relative utility of offending) for both registered and unregistered offenders. Registry size may also be a function of prosecutorial zeal; this could affect punishment severity and probability of punishment for both registered and unregistered offenders.

[24] For an application of these data to three-strikes laws in California, see Helland and Tabarrok (2006).

years after their release, adding information on all subsequent arrests, convictions, and sentences.

The BJS data set is useful because I can compare the behavior of offenders who would have had to register upon release in 1994 with the behavior of those who would not. California, Illinois, Minnesota, Oregon, Texas, and (partially) Florida enacted registries before 1994, the beginning of the study period.[25] Virginia, New Jersey, and Delaware enacted registries during 1994, the year of initial release in the BJS data set. The law in Delaware, however, applies only to offenders convicted on or after June 27, 1994, and thus does not apply to any prisoners in this study. Virginia's and New Jersey's laws include anyone in state custody on the date the law was enacted; thus, any offender released in Virginia after July 1, 1994, and any offender released in New Jersey after October 31, 1994, would have been required to register.[26]

The BJS data contain information on 10,510 prisoners released in 1994 whose offense was a sex offense. Because registration laws vary across the states, I further restricted the data to those offenders who appear to have remained in the state that released them. I did so by excluding offenders who were arrested out of state at any point during the 3-year follow-up period ($N = 620$).[27] Also, most states do not require registration of and/or public access to information about juvenile offenders, and juvenile offenders are often subject to different sentence lengths and penalty requirements. Because of the lack of information about whether these offenders were tried as juveniles or as adults, and to avoid ambiguity, I also excluded offenders who were juveniles at the time of their admission ($N = 108$). Finally, the 138 offenders released into Arizona were excluded. After these adjustments, 9,623 sex offenders released in 1994 are included in the analysis.[28]

By examining the crime the offenders committed, the date the registry law was enacted in the state that released them, and the relevant laws regarding registration in that state, I determined that 5,195 (53.99 percent) of the offenders should have been required to register upon their release in 1994. All offenders released in Minnesota, California, Oregon, and Illinois would have had to register, because the laws in those states went into effect before 1994 and required offenders released after the effective date to register. Offenders released in Virginia

[25] Florida has two tiers for categorizing those who have committed sex crimes: sexual predator and sexual offender. Sexual predator is the more serious of the two labels. Sexual predators were required by the state to register starting in 1994, but sexual offenders were not required to register until 1997; hence, Florida had enacted registries for only part of the sex offender population by 1994.

[26] Virginia and New Jersey could have served as ideal states within which to test the effect of registration by comparing the populations of offenders who were registered with those who were unregistered but released in the same year and same state. However, this analysis would suffer from an extremely small sample size. Of the 247 sex offenders released into Virginia, only 12 were rearrested for a sex offense, five who would have had to register and seven who would not. It would be difficult to conclude anything from this small sample.

[27] Running the regressions with these prisoners included does not significantly change the results.

[28] I also excluded the 21 prisoners who died during the 3-year follow-up period.

and New Jersey would have had to register if they were released after the dates when those states' registries went into effect; thus, 130 of Virginia's 247 released offenders and 56 of New Jersey's 404 would have had to register. No offender released in Delaware would have had to register, because the law in that state applied only to those offenders convicted on or after June 27, 1994. Similarly, Texas law in 1994 required only those convicted after September 1, 1991, to register. I used admission date as a proxy for conviction date and considered only those admitted to prison after September 1, 1991, and then released in 1994 as having to register; according to this analysis, 366 of Texas's 665 offenders should have had to register. Florida began registering sexual predators (as distinct from sexual offenders; see note 25) who committed offenses on or after October 1, 1993. Sixty offenders released in Florida in 1994 were admitted after October 1, 1993; however, for a criminal to qualify as a sexual predator, the offense must have been serious, and given that these offenders had only served for approximately 1 year, I assumed that none would qualify as predators and therefore that none would have been registered.

4.2. Empirical Strategy

I first compare recidivism rates of offenders in each group (registered and unregistered) using three definitions of recidivism: subsequent arrest for rape, subsequent arrest for a sex offense other than rape, and subsequent conviction for a sex offense for which the arrest took place after release.[29] I separate rape and other sex offenses for easier comparison to the UCR data used in Section 3. After this straightforward comparison of rates across registered and unregistered offenders, I use a Cox proportional hazard model to compare the recidivism of the two groups so that controls may be included for differences across the groups and across states.

For any time t, the Cox proportional hazard model reports the hazard of an individual proportional to the baseline. The hazard is defined as

$$h_i(t) = \exp(\mathrm{ROR}\,\beta + \boldsymbol{I}\theta + \boldsymbol{S}\phi)h_o(t), \tag{2}$$

where $h_o(t)$ is the baseline hazard at time t, $h_i(t)$ is the hazard of the ith individual at time t, ROR (registered on release) is a dummy variable set equal to one if the offender should have had to register, $\boldsymbol{I}$ is a vector of individual-level controls, and $\boldsymbol{S}$ is a vector of state-level controls. The estimated coefficient β can then be used to determine the ratio of the hazards for those on the registry and those not on the registry at any given time:

[29] There are some prisoners in the data set who were convicted after their 1994 release of a crime for which they had been arrested before they entered prison. These types of situations are not included in the comparison.

$$\frac{h(t|\mathrm{ROR}=1)}{h(t|\mathrm{ROR}=0)}=\frac{\exp(\mathrm{ROR}\,\beta+\boldsymbol{I}\theta+\boldsymbol{S}\phi)h_o(t)}{h_o(t)} \\ =\exp(\mathrm{ROR}\,\beta+\boldsymbol{I}\theta+\boldsymbol{S}\phi). \tag{3}$$

In this case, the hazard rate I am measuring is for recidivism, defined in one of the three ways previously mentioned.

Because of the nature of the variable that indicates registration, which is equal to zero or one for entire states in some cases, I cannot utilize state fixed effects to account for differences across states. Therefore, I use state-level controls in addition to individual-level controls to try to account for some of those differences. Individual-level controls include age at first release; race; gender; sentence length; whether the individual went to jail for rape, sexual abuse, child molestation, or sodomy; and the number of rape arrests and other sex offense arrests before the individual's release in 1994. State-level controls are included for the state releasing the offender. These include the number of police per 1,000 in the state's population, the 1993 arrest ratio for rape (from the UCR data), the percentage of the state population residing in urban areas, punishment severity (the average sentence length for the rapists in the sample), past rape incidence rates and sex offense arrest rates in the state, a dummy that indicates whether the state elects rather than appoints its judges, and variables that help determine the general social climate of the state.

4.3. *Results*

Summary statistics for demographic information and criminal records are presented in Table 4, broken down by registered and unregistered offenders. For the most part the two groups are similar. The major differences between them lie in sentence length and demographic variables; those differences are likely attributable to the states that make up the two groups.[30]

Table 5 uses the BJS data and the three definitions of recidivism as well as arrests for other crimes to compare sex offenders released in 1994 who recidivated in terms of the percentages of those who had to register and those who did not. The differences are small and not statistically significant across the three main definitions of recidivism, but they do show that offenders who had to register were more likely to be arrested for a nonsexual offense than those who did not. This preliminary analysis does not fully support the hypothesis that registration reduces recidivism among sex offenders, as registered offenders do not appear to have lower rates of recidivism. However, the summary statistics in Table 4

[30] For example, the large difference between the groups in the percentage of sex offenders who are Hispanic is most likely explained by the fact that California and Texas both enacted their registry laws prior to 1994; thus, all offenders released in those two states in 1994 would have had to register, and both states have a relatively high percentage of Hispanic releasees (30.9 percent and 21.8 percent, respectively).

Table 4
Summary Statistics for Bureau of Justice Statistics Data, by Registration Status

	Not Registered on Release	Registered on Release	Difference
N	4,428	5,195	
Average Age	36.90	37.12	−.21
Average Age at First Release	25.41	25.53	−.12
Average Sentence Length (months)	96.10	67.20	28.90*
Average Rape Arrests prior to 1994 Release	.41	.43	−.02
Average Sex Offense Arrests prior to 1994 Release	.85	.84	.01
% Black	37.38	25.70	11.68*
% Hispanic	6.71	23.31	−16.60*
% Male	98.64	99.21	−.57*

Note. The variable Average Age at First Release is the average age of an offender when he was released from jail for his first conviction, which is not necessarily the release in 1994; Average Age pertains to his release in 1994. Significance for difference of means is calculated using a two-sample t-test. Significance for difference of percentages is calculated using a two-sample test of proportion.
* $p < .01$.

Table 5
Percentage of Offenders Who Recidivated, by Registration Status

	Arrested for Sex Offense	Arrested for Rape	Convicted for Sex Offense	Arrested for Other Offense
Not registered on release	3.59	1.22	2.15	30.58
Registered on release	3.25	1.60	2.39	33.44
Difference	.34	−.38	−.24	−2.86*

Note. Any discrepancies in sums are caused by rounding. Significance for difference of percentages is calculated using a two-sample test of proportion.
* $p < .01$.

showed that there are some fundamental differences between the registered and unregistered groups that I was unable to control for in Table 5.

Table 6 presents results from the Cox proportional hazard model using the same three definitions of recidivism as well as various control variables at the individual and state levels. The variable of interest is the dummy Registered on Release, which represents whether the offender should have had to register upon his release in 1994. Subsequent arrests and convictions for sex offenses or for other crimes are not significantly different for registered and unregistered offenders. But offenders on the registry are significantly more likely to be subsequently arrested for rape. The older an offender is, the less likely he is to recidivate, and those with more past convictions are more likely to recidivate. States with harsher sentences for rape tend to have less recidivism for sex offenses but more recidivism for other crimes.

The results show that an offender who should have had to register appears to behave no differently, or possibly worse, than one who did not have to register. If anything, registered offenders have higher rates of recidivism. Again, however, a caveat is warranted. The definitions used for recidivism all involve arrests or

Table 6
Effect of Registration on Offender Recidivism: Cox Proportional Hazard Model

	Sex Offense Arrest (1)	Sex Offense Conviction (2)	Rape Arrest (3)	Other Crime Arrest (4)
Registered on Release	1.068	.969	1.995^{+}	.865
	(.404)	(.371)	(.834)	(.080)
ln (1993 Arrest Ratio)	1.765	.755	4.918	.312**
	(1.539)	(.721)	(5.711)	(.065)
Age at First Release	.987*	.977**	.973*	.955**
	(.006)	(.009)	(.011)	(.002)
Previous Rape Arrests	1.177^{+}	1.650**	2.385**	1.036
	(.113)	(.146)	(.175)	(.024)
Previous Sex Offense Arrests	2.214**	2.537**	1.520**	1.034^{+}
	(.081)	(.122)	(.129)	(.019)
Previous Violent Crime Arrests	.943	.928	1.081	1.153**
	(.108)	(.107)	(.104)	(.014)
Previous Property Crime Arrests	1.063	.929	.767^{+}	1.261**
	(.098)	(.113)	(.118)	(.020)
Black	.896	.995	1.285	1.803**
	(.123)	(.171)	(.261)	(.059)
Hispanic	.907	.897	1.544^{+}	1.136**
	(.160)	(.203)	(.363)	(.055)
Male	3.055	3.265E+15	8.238E+14	.776^{+}
	(3.064)	(2.191E+23)	(2.257E+22)	(.108)
Rapist	1.214	2.835	1.539	
	(.672)	(2.954)	(1.619)	
Sexual Abuser	1.169	2.806	2.165	
	(.633)	(2.889)	(2.258)	
Statutory Rapist	.654	2.581	1.157	
	(.491)	(2.928)	(1.344)	
Child Molester	1.244	3.649	1.291	
	(.677)	(3.775)	(1.352)	
Sodomizer	1.585	4.813	1.789	
	(.947)	(5.148)	(1.945)	
Police per 1,000 (1997)	.449*	2.496*	.179*	.646**
	(.150)	(1.025)	(.138)	(.055)
Rape Incidents per 1,000 (1993)	.661	3.316*	.461	.753**
	(.256)	(1.641)	(.385)	(.081)
Sex Offense Arrests per 1,000 (1993)	.154^{+}	.282	2.295	1.900**
	(.153)	(.292)	(2.969)	(.438)
% Urban	1.015	1.144**	.971	1.025**
	(.022)	(.041)	(.038)	(.006)
% Christian (2000)	.555	6.104^{+}	2.558	.091**
	(.484)	(5.850)	(2.679)	(.019)
Elected Judges	.751	2.841*	.950	1.076
	(.261)	(1.289)	(.472)	(.091)
% Republican Votes (1992)	1.047	1.351**	.852^{+}	1.029*
	(.047)	(.082)	(.076)	(.012)

Table 6 (*Continued*)

	Sex Offense Arrest (1)	Sex Offense Conviction (2)	Rape Arrest (3)	Other Crime Arrest (4)
Average Rape Sentence (years)	.936**	.873**	1.060	1.010*
	(.020)	(.022)	(.039)	(.005)
Prisoners per 1,000 (1994)	1.413	.312*	2.415	.475**
	(.655)	(.178)	(1.746)	(.059)

Sources. The measure % Christian is the percentage of Christian church adherents in 2000 (U.S. Census Bureau 2001). Data for Elected Judges are from Tabarrok and Helland (1999). The measure % Republican votes is the percentage of the state that voted for the Republican candidate in the 1992 presidential election (Dave Leip, Atlas of U.S. Presidential Elections[http://www.uselectionatlas.org]).
Note. Reported coefficients are exponentiated for ease of interpretation. A given coefficient can be read as the proportional change in the hazard rate with respect to that variable. Other crime arrests are arrests for an offense other than a sex offense or rape. The variable Registered on Release equals one if the sex offender's characteristics imply that he would have had to register on his release in 1994. No females in the sample were later convicted of a sex offense or arrested for rape; thus, Male is dropped from the second and third specifications. The dummy variables Rapist, Sexual Abuser, Statutory Rapist, Child Molester, and Sodomizer indicate the crime the sex offender committed for which he was released in 1994. The variable Police per 1,000 is a state-level control. The dummy Elected Judges equals one if a state elects its judges rather than appoints them. The measure Average Rape Sentence is based on the length of the prison stay from which the offender was released in 1994. $N = 12{,}427$.
$^{+}$ $p < .10$.
* $p < .05$.
** $p < .01$.

convictions, not actual rates of offense. Obviously, it is impossible to measure the true number of offenses that a sex offender commits after release. In the case of these BJS data, arrests are the closest proxy I have to a measure of offenses committed. It is possible that offenders on the registry are more likely to be arrested for a crime because of the fact that they are on the registry and thus under suspicion for any sex offense that arises, so even if their recidivism rate were not higher, their arrest rate would be. This could imply that, in fact, registries have the positive effect of giving police more information about registered sex offenders and thus allowing the police to catch them. It is interesting, however, that the two groups have no statistically significant difference in conviction rates for arrests after release.

5. Locational Analysis

5.1. Data Description

To understand whether registries are potentially effective, I combine data on locations of crime in Washington, D.C., with data on locations of registered sex offenders. I have data on locations of crime from January 1, 1997, to July 30, 2003, from the Washington, D.C., Metropolitan Police Department (MPD).[31, 32] These data contain the type of offense committed (sexual abuse, arson, burglary, theft, theft from auto, stolen auto, homicide, assault with a deadly weapon, or robbery), the date and hour when the crime took place, and the block where the crime occurred.

The MPD's sex offender registry Web site has information about all class A and B sex offenders.[33] I obtained information on each offender's home and work addresses as well as date of initial registration from the Web site. Unfortunately, I only have information on offenders' currently registered addresses; I do not have information about changes in the registry information over time. Thus, I assume that offenders have not moved, that is, that the address listed on the Web site has been their address since their initial registration. This is, admittedly, a big assumption. Because the time period is short, it is not entirely unreasonable, but the results should be thought of as a first attempt at this sort of analysis, which could be greatly improved by better data. Figure 5 presents a map of where registered sex offenders in Washington, D.C., live. While there are some parts of Washington with few or no sex offenders, they appear to be fairly spread out over the city.

5.2. Empirical Strategy

I broke up the data into year-month groups and counted the number of offenders living in a particular census block group in any given year-month. This can change over time as new offenders register. I similarly counted the number of sexual abuse incidents in a given year-month–census block group and used this as the dependent variable. First, I ran regressions with different crime rates per 1,000 in the population as the dependent variables, including sexual abuse incidents and violent and nonviolent crime incidents. The regressions include a variable for the number of sex offenders living in a year-month–census block. Also included in the regressions are demographic controls at the block level, such as the percentages of the population that are black, Hispanic, female, or in a certain age group, as well as the percentage of housing that is occupied by renters. I then ran the same regressions with census block fixed effects instead of the controls.

Second, I compared the effects on sex crime rates when there was only a registry and when there was public access to the registry via the Internet. I did

[31] The Metropolitan Police Department (MPD) requires the following disclaimer: "These data reflect preliminary crime reports made by individual police districts to the MPD's Central Crime Analysis Unit. These data DO NOT reflect official index crime totals as reported to the FBI's Uniform Crime Reporting Program. These data are subject to change for a variety of reasons, including late reporting, reclassification of some offenses, and the discovery that some offenses were unfounded."

[32] See Klick and Tabarrok (2005) for another application of these data.

[33] Washington, D.C., uses a three-tiered classification system. The highest tier is class A, and the lowest is class C. Examples of class A crimes are forcible rape and first-degree child abuse; examples of class C crimes are kidnapping with the intent to commit a sex offense and threatening to commit a sex offense.

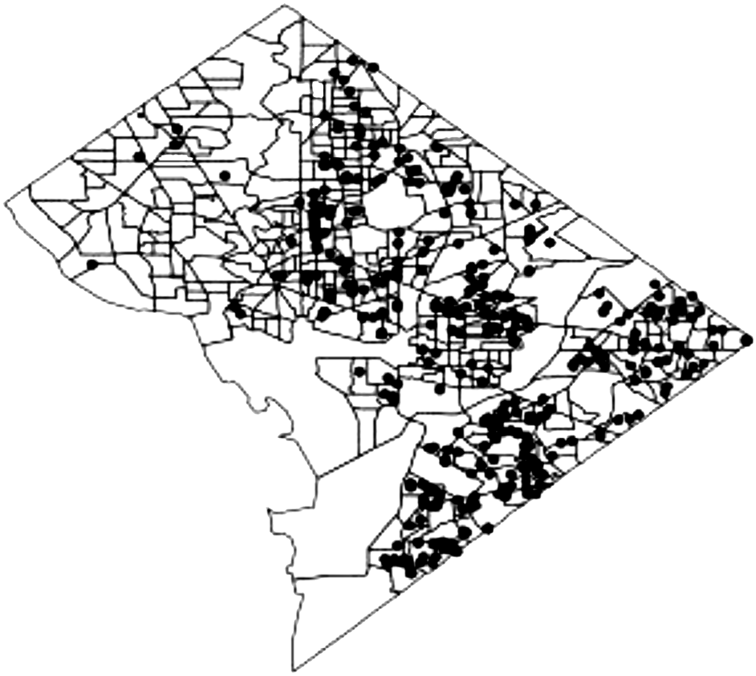

Figure 5. Registered sex offenders in Washington, D.C.

so by creating a variable that represents the number of registered offenders before public Internet access to the registry existed (Before Internet) and another that represents the number of registered offenders that are both registered and listed on the Internet (After Internet). I then used a specification similar to previous regressions with census block fixed effects. The results provide insight into whether knowing the location of sex offenders in Washington, D.C., helps to predict the location of sexual abuse incidents.[34]

5.3. Results

Table 7 presents the results from the first set of regressions. The variable of interest is offenders per 1,000 living in the block group. The results for the first specification indicate a slight decrease (not statistically significant) in sexual abuse crimes near where sex offenders live. Note that the average block has approximately .92 sexual abuse incidents per 1,000 in the population. The second spec-

[34] I also imitated the analysis from Section 3.3 by regressing sexual abuse on the measure of whether a registry existed and time and season dummies and found that, similar to the results using the national data, the existence of a registry appeared to have no statistically significant effect on sexual abuse rates across the city.

Table 7
Effect of Registered Sex Offenders Living in a Census Block Group on Crime Rates

	Sexual Abuse Incidents (1)	Nonviolent Crimes (2)	Violent Crimes (3)	Sexual Abuse Incidents (4)
Offenders per 1,000 living in block group	−.011	−159.809	−15.277	−.046
	(.019)	(141.072)	(13.302)	(.052)
Population	−.000	−.036	−.008	
	(.000)	(.219)	(.020)	
Black	.000	−5.666	−.561	
	(.001)	(4.555)	(.415)	
Hispanic	.001	−44.706	−4.193	
	(.002)	(32.241)	(2.947)	
Female	.000	−187.384	−17.080	
	(.009)	(135.443)	(12.350)	
Ages 18–39	−.001	−40.227	−3.617	
	(.002)	(29.859)	(2.719)	
Renter-occupied housing	.000	.641	.062	
	(.000)	(.654)	(.060)	
Violent crime	.009*			.008*
	(.001)			(.001)
Nonviolent crime	.001*			.000*
	(.000)			(.000)
Constant	−.060	11,981.755	1,096.468	−.030
	(.391)	(8,571.084)	(775.875)	(.090)
R^2	.06	.27	.21	.06

Note. Block group effects are included in column 4; time fixed effects are included in all models. Numbers in parentheses are robust standard errors, clustered on block group. Nonviolent crimes are stolen auto, theft, theft from auto, arson, and burglary. Violent crimes are homicide, assault, and robbery. Demographic data are measured per 100 in the population. Crime variables are measured per 1,000 in the population. Although using the natural log of the crime rate would have made interpretation easier, this was not possible because many block groups had no crimes. $N = 15{,}280$.
* $p < .01$.

ification shows that blocks with more sex offenders also tend to have lower rates of nonviolent and violent crime, though again this result is not statistically significant. The mean number of nonviolent crimes per 1,000 in a given census block group–month is 135, with a standard deviation of 2,500, and for violent crimes the mean is 15, with a standard deviation of 261, so I can reject changes in the number of violent or nonviolent crimes per capita of well within a standard deviation in either direction. In essence, there is little difference in overall crime rates for block groups with more sex offenders. The results show that knowing where a sex offender lives does not reveal much about where sex crimes, or other crimes, will take place. The last specification in Table 7 presents the results when fixed effects instead of observables are used to control for differences across blocks. These results are similar to those of the first specification.

These data offer a unique opportunity to study the effects on sex crime rates when there was only a registry and when there was also public access to it via the Web. There were 9 months between June 2000 and March 2001 when the registry in Washington, D.C., existed but the contents were not available online.

Table 8
Effect of Public Internet Access to Registry on Sex Crime Rates

	Sexual Abuse Incidents
Offenders per 1,000 living in block group after Internet	.011
	(.009)
Offenders per 1,000 living in block group before Internet	.003
	(.022)
Constant	.039^{+}
	(.021)
R^2	.07

Note. Block group fixed effects and time fixed effects are included. Numbers in parentheses are standard errors. Sexual abuse incidents are measured per 1,000 in the population. N = 15,280.
$^{+}$ $p < .10$.

Table 8 attempts to differentiate between these two periods, with census-block fixed effects used to control for differences across blocks. The results show that there is little difference in the effect of having a registered sex offender in a census block when this information is not public versus when it is public.

The results from this analysis indicate that knowing that a sex offender lives on your block does not give you information about rates of sexual abuse there. However, again, the analysis suffers from limitations, and further work needs to be done to verify the results.

6. Conclusion

The data in these three data sets do not strongly support the effectiveness of sex offender registries. The national panel data do not show a significant decrease in the rate of rape or the arrest rate for sexual abuse after implementation of a registry or access to the registry via the Internet. The BJS data that tracked individual sex offenders after their release in 1994 do not show that registration had a significantly negative effect on recidivism. And the D.C. crime data do not show that knowing the locations of sex offenders by census block can help predict the locations of sexual abuse. This pattern of noneffectiveness across the data sets does not support the conclusion that sex offender registries are successful in meeting their objectives of increasing public safety and lowering recidivism rates.

Aside from the direct policy interest in meeting those objectives, understanding whether sex offender registries work is potentially important because they serve as a precedent for other types of registries. Tennessee, along with Illinois, Montana, and Minnesota, recently began a public registry for methamphetamine offenders. Not only can different types of criminals be registered, but technological advances, such as global positioning systems (GPS), can allow the physical location of individual criminals to be tracked on a 24-hour basis. Several states have passed laws that require high-risk offenders to be monitored by GPS so police know their exact location at any given moment.[35] In addition to these technological changes to sex offender management, in 2006 Congress passed the Adam Walsh Child Protection and Safety Act. This law requires states to enact stricter registration requirements, including an increase in the penalty for not registering and more frequent verification of sex offenders' locations. With these developments in registration efforts, sex offender registries should be evaluated before states commit to further extensions, and we should look critically at any attempts to extend registration to other criminals.

Appendix A

Registry Enactment Dates

Prescott and Rockoff (2009) and I disagree on three dates of registry enactment. Below are justifications for the dates I use in my paper.

Kentucky: July 15, 1994

Registration System for Adults Who Have Committed Sex Crimes or Crimes against Minors—Persons Required to Register—Manner of Registration—Penalties—Notifications of Violations Required (Ky. Rev. Stat. Ann. sec. 17.510 [LexisNexis 1994]) contains the following information: "History: Amended 2008 Ky. Acts ch. 158, sec. 13, effective July 1, 2008.—Amended 2007 Ky. Acts ch. 85, sec. 100, effective June 26, 2007.—Amended 2006 Ky. Acts ch. 182, sec. 6, effective July 12, 2006.—Amended 2000 Ky. Acts ch. 401, sec. 16, effective April 11, 2000.—Amended 1998 Ky. Acts ch. 606, sec. 138, effective July 15, 1998.—Created 1994 Ky. Acts ch. 392, sec. 2, *effective July 15, 1994.*"[36]

Connecticut: October 1, 1998

According to Connecticut Public Act 98-111, "an act concerning the registration of sexual offenders" was approved on May 17, 1998, and, according to the Smith Law Firm Web site, was effective October 1, 1998: "As of October 1, 1998, Public Act 98-111, An Act Concerning the Registration of Sexual Offenders, went into effect." See also Registration of Sexual Offenders (Conn. Gen. Stat., title 54, ch. 969); Pub. Act No. 98-111 is listed first in the history for each section of the chapter, and, as stated above, Pub. Act No. 98-111 became effective October 1, 1998.[37]

[35] Examples include Florida, Missouri, Ohio, and Oklahoma.

[36] From the Kentucky legislature Web site (http://www.lrc.ky.gov/KRS/017-00/510.PDF); the emphasis is mine.

[37] The text of Pub. Act No. 98-111 is available on the Connecticut General Assembly Web site (http://www.cga.ct.gov/ps98/Act/pa/1998PA-00111-R00SB-00065-PA.htm); for the text from the Smith Law Firm Web site, see Beverly Brakeman Colbath, Sex Offender Registration/Community Notification—It's Not That Simple (http://www.smith-lawfirm.com/Connsacs_Beverly_opinion.htm); see also Conn. Gen. Stat., title 54, ch. 969 (http://www.cga.ct.gov/2005/pub/Chap969.htm).

North Dakota: 1991

The Frequently Asked Questions page on North Dakota's Sex Offender Web site includes the following text: "When did this sex offender law begin? The first sex offender registration statute in North Dakota was passed in the 1991 legislative session. Since then, there have been changes or additions made to the original statute in every legislative session."[38]

Appendix B

Comparisons with Prescott and Rockoff

Table B1

Adding Interactions of Registry and Size of Registry

	Rape		Sex Offense
	(1)	(2)	(3)
Registry	.012	.003	.020
	(.029)	(.054)	(.035)
Registry × Size of Registry	−.000	−.000	−.007*
	(.001)	(.002)	(.003)
Internet	.009	.056	−.072*
	(.044)	(.069)	(.027)
Registry × Internet	.001	−.001	.006*
	(.002)	(.003)	(.002)
N	913	267	210,209

Note. All dependent variables are natural logs of rates (Rape is the natural log of the rape rate), so coefficients are interpreted as percentage change in the crime rate. Numbers in parentheses are robust standard errors clustered by state. The variable Registry (or Internet) equals one if a state had a registry (or Internet access to the registry) in that year and zero otherwise. The size of the registry is a predicted value and is measured per 1,000 in the population. Standard errors are calculated through multiple imputation. Controls included but not listed are state and year fixed effects and demographic characteristics. Column 1 uses all Uniform Crime Reports data available. Column 2 restricts the data to those states also available in National Incident-Based Reporting System (NIBRS) data and uses the Prescott and Rockoff (2009) dates for the three states where our registry effective dates differ. Column 3 presents the results from Prescott and Rockoff (2009, table 3, col. 4) for comparison, who use sex offense incidents from NIBRS as the dependent variable. See their table notes for other variables included in that regression.

* $p < .05$.

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[38] See State of North Dakota Office of Attorney General, Sex Offender Web Site (http://www.sexoffender.nd.gov/FAQ/faq.shtml).

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