

Do Sex Offender Registration and Notification Laws Affect Criminal Behavior?

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Abstract

Sex offenders have been the targets of some of the most far reaching and innovative crime legislation in the U.S. over the last twenty years. Unlike most criminal laws, which attempt to reduce illegal activity by explicitly increasing expected punishment levels for *all* potential offenders, recent sex offender legislation focuses on reducing “same crime” recidivism of those already convicted of sex offenses. Two primary examples are registration and notification laws. Registration laws require that convicted sex offenders provide valid contact information to law enforcement authorities, while notification laws require that sex offender information be released to members of the public who are likely to be targeted if a sex offender recidivates (e.g., neighbors and former victims). Using detailed information on the variable timing and scope of state law, we study how this type of legislation has affected the overall frequency of sex offenses, the incidence or mix of sex offenses across victims, and response of police to reported crimes. In line with a simple model of criminal behavior, we find evidence that registration laws reduce crime frequency by providing local law enforcement with information on local sex offenders, and that active community notification laws deter crime, most likely by raising the expected punishment to individuals not currently registered. Importantly, we also find some evidence that notification laws may *increase* offenses committed by registered offenders, perhaps by making non-criminal activity relatively less attractive.

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1. Introduction

Criminal recidivism poses a serious risk to public safety. Approximately 40 percent of all criminals sent to prison in the U.S. over the last twenty years had already been convicted of a prior felony (authors' calculations, National Corrections Reporting Program 1983-2002). Recently, victims' advocates and others have argued that persons convicted of rape and sexual assault, or sex offenders more generally, are highly likely to "same crime" recidivate (Langan et al. (2003)). Although criminal behavior declines steeply with age after the early twenties for most types of crime, the decline for sex offenses appears to be more gradual (Hanson (2002)). Partly for these reasons, and because of a number of high profile crimes in the late 1980s and early 1990s, sex offenders have been the target of considerable legislation and public spending intended to reduce their recidivism over the last twenty years.

In the 1990s, two sets of laws targeting sex offenders emerged across the United States. A federal mandate in 1994 (the Jacob Wetterling Act, named after the victim of a crime in Minnesota) required states created registries of sex offenders for use by law enforcement. Another federal mandate in 1996 (Megan's Law, named after a New Jersey victim, Megan Kanka) required that states provide public notification on the location of sex offenders to local residents or other "at risk" groups. The basic motivations for registration and notification were, respectively, to aid law enforcement in monitoring and apprehending sex offenders who may recidivate and to help local households protect themselves through monitoring and, ostensibly, avoidance of offenders in their neighborhoods.

Despite the proliferation of sex offender registration and notification laws, it is unclear whether they have been successful in reducing crime by sex offenders, or whether they have achieved other goals (e.g., increased speed or probability of capture). It is also unknown whether sex offenders responded (or are able to respond) to these laws in other ways (e.g., adjusting how they select their victims). The answers to these questions are important not only for evaluating the costs and benefits of registration and notification laws, but also for understanding how an important group of convicted criminals responds to changes in incentives. Empirical work provides some support for the claim that criminals in general respond significantly to changes in expected punishment (e.g., Levitt (1998), Kessler and Levitt (1999); Nagin (1998)). However, it is unclear whether this is true for all individuals (see McCrary and Lee (2005) on juvenile

offenders), and whether these results extend to sex offenders in particular is unknown (see Bachman, Paternoster, and Ward (1992)).

For a number of years, only two credible studies sought to measure the impact of registration and notification on recidivism. Schram and Milloy (1995) and Adkins et al. (2000) examined recidivism of offenders released just before and just after registration and notification laws became effective in, respectively, Iowa and Washington State.¹ Neither study found a statistically significant difference in future arrests for sex offenses between these two otherwise comparable groups. Recently, several new studies have used data on registration laws passed in all states to examine the relationship between the timing of laws' passage and changes in the frequency of sex offenses using national data from the FBI's Uniform Crime Reports (Walker et al. (2005), Shao and Li (2006), and Agan (2007)).² Only Shao and Li (2006) report any evidence that offender registration laws caused a statistically significant reduction in sex offenses, and their findings are sensitive to empirical specification and rely on treating registration and notification laws as a single treatment. Agan (2007) offers some evidence that posting sex offender information on the internet reduced the number of arrests for sex offenses, but her results are similarly sensitive and open to alternative interpretations. These more recent papers all examine data aggregated by state and year.

In this paper, we build upon existing work by studying the impact of laws targeting criminal behavior by convicted sex offenders along a number of dimensions. Using data from the National Incident Based Reporting System (NIBRS), we examine effects on 1) the overall frequency of reported sex offenses, 2) the relationship mix between offenders and victims, and 3) the probability and speed of arrests.

We focus separately on registration laws and notification laws. While registration requirements were intended solely to help law enforcement track and apprehend offenders, notification laws aimed both at reducing crime through greater public awareness and increasing

¹ Unlike many other states, there was no lag between the passage of registration and notification laws in Washington and Iowa.

² Agan also examines recidivism rates among released sex offenders in a special data set that followed offenders released in 1994. She finds no evidence that recidivism rates were lower for prisoners released in states with registration laws.

the likelihood of capture conditional on the commission of a crime (Prentky (1996), Pawson (2002), Levenson and D'Amora (2007)). Neither approach was intended to affect the “incidence” of sex offenses, but some observers have suspected that notification laws might simply displace crime (see Prentky (1996), Filler (2001)) by changing the population of victims targeted by sex offenders. For example, if notification laws cause offenders to seek victims outside of their neighborhoods, one might expect little or no overall reduction in crime, but merely a change in the typical relationship between a victim and an offender.

This study required that we conduct extensive research into the sex offender legal regimes of the states in our sample. Although registration and notification were required by the federal government, states varied in the speed with which they passed laws, and were given considerable discretion concerning many important details of this legislation.³ An important limitation in estimating the impact of notification is the difficulty in understanding the timing and scope of this body of law. We describe the history of sex offender registration and notification laws in detail in Section 2 and provide information on these enactments in the Appendix. Importantly, sex offender laws have changed over time due to legislative amendments and judicial decisions, all of which we have painstakingly recorded.

Our paper is an advance over previous studies in that our analysis distinguishes between sex offender registration and notification laws and distinguishes among the various features of different notification laws (e.g., access to paper registry, internet access to information, or proactive community notification). Notification laws require the dissemination of information about sex offenders (e.g., criminal history, physical description, home address, and other information). Registration laws, in contrast, require that a sex offender register their residential location with a public authority (usually local police), but this information is otherwise kept confidential. With the exception of Agan (2007), who examines registration and the availability of information via internet, the work on sex offenders cited above does not make this important distinction.

³ For example, some states (e.g., Rhode Island) limit registration to individuals convicted of more serious crimes such as Rape, Sexual Assault and Child Molestation, while others (e.g., North Carolina) include crimes such as Incest, Child Pornography, and Indecent Exposure. Some states (e.g., Massachusetts) required registration and notification of convicted sex offenders who committed offenses for many years prior to the advent of the law, while others (e.g., Michigan) limited their attention to offenders convicted or released from prison after their laws were created.

We also make a methodological contribution to the sex offender literature by using variation in the number of offenders actually registered with authorities to separate the various ways in which registration and notification may influence criminal behavior and police responsiveness. For example, notification laws are aimed at protecting the public against recidivists, but may also have a separate deterrent effect for potential sex offenders who have never been convicted. The institution of a notification law raises the expected punishment to potential first-time sex offenders because their crimes and personal information will be made public upon release if they are caught and convicted. This effect should be invariant to the number of offenders actually registered, while any effect on recidivism should be stronger when the registry contains information on a large number of sex offenders.

We find some evidence that sex offender registration and notification laws decreased the total frequency of sex offenses in the states we examine. Specifically, the creation of an offender registry is associated with a significant decrease in the frequency of crime, particularly when a large number of offenders are registered. This is in line with predictions from a simple model of criminal behavior, in which the registry itself serves to increase expected punishment for individuals other than registered offenders, and the provision of information on registered offenders to local authorities increases monitoring and therefore the expected punishment of recidivism. In addition, we find that active community notification provisions reduced the overall frequency of crime. One potential explanation for this effect, again consistent with our model, is that notification raises the expected punishment for future offenders.

Importantly, we find no evidence that notification laws (as opposed to registration laws) reduced crime by lowering recidivism. While community notification is associated with a decrease in crime, this effect is actually *weaker* when a large number of offenders are on the registry. This finding may be indicative of a brutalization effect, in which convicted sex offenders become more likely to commit crime when their information is made public because the shame and/or difficulties that arise from notification make crime more attractive.

We also find that the drop in the frequency of reported crime occurred primarily by reducing those attacks in which the victim was close to an offender, either a family member, friend, acquaintance, or a neighbor, and not by reducing sex offenses committed against strangers. Because we find nothing to indicate that this reduction occurred by way of crime

displacement (i.e., convicted sex offenders changing their targets to strangers), we hypothesize that registration and notification laws, particularly active community notification, deterred “non-stranger” crimes by increasing the expected punishment for those offenses, even for first-time offenders.

The rest of the paper proceeds as follows. In Section 2 we provide a description of the variation in the timing and scope of states’ registration and notification laws. In Section 3 we lay out the potential impacts of registration and notification using a simple model of criminal behavior and present our basic empirical methodology. In Section 4 we describe the data used in our analysis and we present our results in Section 5. Section 6 concludes.

2. The Evolution of Sex Offender Registration and Notification Laws

To characterize the sex offender registration and notification laws properly for the empirical work below, we conducted painstaking legal research into the evolution of these laws in states covered by NIBRS in the 1990s. We relied principally on paper legislative sources, legal databases containing statutory language and judicial opinions, news releases and stories, and conversations and email communications with state employees to construct a detailed legal timeline for each state. We catalogued enactment dates, effective dates, and compliance dates for each legal change, and verified, where possible, that such changes took place in reality, as opposed to simply on paper. We cross-checked our research with other sources containing compilations of sex offender laws and resolved all conflicts. Finally, we recorded the precise content of these legal changes, which is particularly necessary with sex offender notification laws because their substance differs across states on various dimensions.

Determining the timing with which sex offender registration and notification laws became effective proved to be a difficult task. Table 1 illustrates this by showing the dates used by Shao and Li (2006), Agan (2007), and Walker et al. (2005) in their analyses of the impact of

registries on crime rates, in addition to our own legal analysis.⁴ Comparisons of these dates across research studies show a fairly low rate of agreement. There are only 15 states for which all studies agreed on the exact date, and only 16 for which all dates fell within the same calendar year.⁵ For example, consider the state of Utah, for which none of the four studies agree with one another on the effective registry date. We place this moment on March 30, 1983, when Utah's first generally applicable sex offender registry became effective. Shao and Li use May 19, 1987, a date we cannot locate in legislative history, but which is quite close to the enactment (as opposed to effective) date of a 1987 law that re-codified and amended the registration law then in place. Agan uses July 1, 1984, which likely refers to a 1984 law that also amended the original 1983 enactment, but the effective date for that law was February 16, 1984. Walker et al. use the year 1996, which was the year that Utah passed a law granting public access to the registry information.

We divide the legal changes we study into four categories: registration, public access, internet availability, and active notification. Registration laws are invariably the first strategy states employ to protect against sex offender recidivism. These laws require that sex offenders (always at least the violent and habitual ones) provide state authorities with information on each offender's demographics (e.g., age, race, distinguishing features) and location (e.g., home, work, or school address), as well as criminal history, upon release from custody or probation. Until notification laws were enacted, this information was confidential. In theory, registration laws make future sex offenses easier to solve because 1) a set of likely offenders will have already been identified, and 2) authorities will know where to locate (and apprehend) that set of offenders. Registration may also lower sex offense rates through increased police surveillance or by reducing the expected payoff of committing a new sex offense.

⁴ Shao and Li (2007), Walker et al. (2005) are more liberal in how they define a "registration" law, and, as practical matter, appear to treat registration and notification as the same thing. Shao and Li refer to laws that include notification provisions as well registration laws. Walker et al. admit that they are examining both "registration and notification" laws, but do not distinguish between the two in their empirical work. Agan (2007), on the other hand, recognizes the distinction with respect to internet availability of registry information, but nevertheless does not distinguish between other forms of public access, active notification, and registration. This can lead to questionable coding as well as interpretation problems. For example, Iowa enacted registration and a limited form of public access at the same time, which means that Agan's work attributes any change entirely to the registration law.

⁵ None of the previous studies use incident level data, as we do, and so variation in the precise day or month (as opposed to year) is unlikely to affect their results significantly.

The remaining three categories of laws – public access, internet availability, and active notification – are designed to make information about offenders (identity and location) available to the public, rather than to assist police directly. As we explain in more detail below, the public can, in theory, reduce sex offender recidivism by avoiding convicted offenders (reducing the number of potential target victims) and by reporting suspicious behavior. Most states began this process by providing public access to their registration databases, but varied in the restrictions they placed on access to this information. Some states (e.g., Idaho) only allowed the public to make information requests in writing or about specific suspected persons. Others made information available about all sex offenders in the area and allowed them to be openly inspected at police departments or other government agencies (e.g., Michigan). Both approaches to public access assume that potential victims or witnesses will make use of these opportunities despite their nontrivial travel and time costs.

Over time, restrictive states loosened their access restrictions, and all states eventually moved registration information onto the internet to minimize transactions costs and maximize information dissemination. Sex offender “Web” registries allow the public to search for offenders using a suspected individual’s information (e.g., a name or alias) or by entering a specific address to determine whether registered offenders live nearby. Most states also implemented some form of “active notification” of individuals likely to be victimized. Active notification laws require that state officials do more than simply release information to someone who inquires. Examples include announcing the release or residential move of a sex offender through a notice placed in a newspaper, by personal visits or letters to neighbors, former victims, or others likely to have direct contact with the offender, and opt-in provisions, which allow former victims or members of the public to request notification if a certain sex offender or one satisfying certain conditions is released or moves. Both of these developments were designed to reduce the information costs for potential victims.⁶

Figure 1 shows the timing of adoption of registration, public access, internet availability, and active notification for each NIBRS state (see also Appendix Table 1), as well as the year in

⁶ Michigan provides an example of a fairly typical sex offender law “timeline.” Michigan passed its first sex offender registration law in July 1994 (effective October 1995), enacted its first public access law in January 1997 (effective April 1997), went online with its sex offender information in February 1999, and finally enacted an active notification requirement in March 2006 (effective January 2007).

which agencies from each state began reporting to NIBRS. While a similar evolution of sex offender laws from confidential registries to searchable internet sites and active notification occurred across all states, there is significant variation in the timing of the passage of these laws. For example, Idaho began registration and (limited) public access simultaneously in 1993, but did not have an internet registry live until 2001 and did not have community notification until 2003. Texas, in contrast, began registration in 1991, started both public access and community notification in 1995, and launched an internet site in 1999. This type of variation provides the basis for our identification strategy.

Although typically a concern in studies that use variation in state laws to identify a causal effect, endogeneity is unlikely to be a problem in this context for two reasons. First, unlike criminal law in general, where rising crime rates might lead to increases in penalties or police spending, many state sex offender laws were passed quickly, in response to one or two well-publicized and usually gruesome incidents (which occasionally took place in other states) and not to a rising trend in sex offenses.⁷ Legislative debates almost invariably did not consider aggregate trends in sex offenses and, in fact, sex offense rates declined throughout the period in which these laws were passed. Second, two federal laws passed in 1994 and 1996 (at least in part in response to specific crimes against individual children in Minnesota and New Jersey) mandated that states pass registration and notification laws. These federal laws left states with discretion as to substance and timing, but had minimum requirements and did impose deadlines. Finally, the timing of passage in states in our sample, like North Dakota, which has a legislature that meets only once every two years, was partly dictated by the pre-existing legislative schedule rather than by changing sex offense trends.

In addition to recording the effective dates and the substantive scope of state sex offender laws in our legal research, we also collected information on the retroactivity of the registration and notification laws of the states in our sample. Retroactivity provisions specify which offenders are covered by the laws in light of the *timing* of their conviction or their release from custody. Massachusetts, for example, made its sex offender laws retroactive. Massachusetts' first registration law was not effective until October 1, 1996, but anyone convicted on or after

⁷ In fact, many sex offender laws are named after victim in the case that sparked the legislative effort. Megan Kanka's case, for example, prompted the passage of many of the first notification laws even though the crime occurred in New Jersey.

August 1, 1981, of a qualifying sex offense was nonetheless required to register. As a result, in October 1996, Massachusetts already had fifteen years' worth of released sex offenders who were required to be registered.⁸ Michigan, on the other hand, made its sex offender laws prospective. Michigan's first registration law became effective on October 1, 1995, but it only required that those people "convicted or released" on or after October 1, 1995, register. As a result, on the first day the law was effective, Michigan's registry was effectively empty.⁹ We use the different retroactivity approaches taken by states as an additional source of variation by which to identify the causal effect of sex offender registration laws, and, with respect to notification laws, to separately identify deterrence of all potential offenders and "incapacitation" (by public awareness) of recidivists, as we explain more fully below.¹⁰

3. Conceptual Model and Empirical Framework

We consider the potential impacts of registration and notification on crime through a simple model of behavior wherein individuals weigh the benefits and costs of crime commission. Following Becker (1968), criminal offenses committed by individual i (O_i) are governed principally by his probability of punishment (p_i), the punishment he faces if convicted (f_i), and the utility he receives from committing crime, relative to other legal behaviors (u_i).¹¹ We add to Becker's notation a subscript j for each potential victim of offenses committed by individual i ,

⁸ Indeed, close to 8,000 offenders were already registered when the Massachusetts registry became effective in October, 1996 (Boston Globe (1996a, 1996b)). The total number of offenders estimated by the Massachusetts Department of Public Safety to be required to register was 10,000.

⁹ Although we do not have data from the start of the Michigan registry, we have good historical data on registrations in North Carolina and Kentucky since the inception of their laws. Neither of these states' laws applied retroactively, and, as we would expect, their registries started from almost nothing and grew gradually (and roughly linearly) over time. See Appendix Figure 1.

¹⁰ The choice whether to make a sex offender law retroactive is also unlikely to be endogenous to crime rates. Under certain conditions, criminal laws with retroactive features can violate the U.S. and state constitutions. The decision of whether to make a law retroactive in any particular state turned in significant part on governing judicial opinions in the state in question.

¹¹ This utility term should be considered an analog to Becker's concept of the individual's "willingness to commit an illegal act." Typically the term u_i also encompasses important variables such as wages in the legal sector. However, unlike theft and burglary, sex offenses themselves do not produce income and are therefore unlikely to substitute for market work. Less than 2 percent of incidents in the NIBRS data that include a sex offense also include a reported burglary or robbery.

and a term c_j that reflects the cost to offender i of targeting victim j . Sex offenders need victims, and some of the laws we consider were specifically intended to make it difficult for offenders to victimize people in their vicinity – neighbors, acquaintances, and friends. By assumption, offenses are increasing in the relative utility of commission, and decreasing in the cost of targeting a victim, the probability of punishment, and the severity of punishment. For simplicity, we also assume that the level of punishment and the relative utility of criminal behavior are invariant across victims.

$$O_{ij} = O_{ij}(c_{ij}, p_{ij}, f_i, u_i) \quad (1)$$

Sex offender registration and notification laws may affect offenses through changes in all four of the elements in Equation 1. However, there are several particular effects which we believe are most likely. First, registration may increase the ability of police to monitor and apprehend registered sex offenders (RSOs). In terms of the model, p_{ij} would rise for RSOs, and particularly for victims local to the area where the police track the offender. Indirectly, this feature of registration may also affect forward-looking, unregistered individuals, for whom the punishment for detection (f_i) now includes a higher future probability of detection. However, so long as registry information is held confidential by police, it seems unlikely that it would alter the cost to targeting victims or the utility of crime commission.

Notification—either via public access to registry information or active community notification—may further affect criminal behavior. First, the punishment for sex offenses now includes public airing of personal information and one's criminal history. This publicity has been shown to have negative consequences for RSOs along several dimensions, including loss of employment, housing or social ties, harassment from neighbors, and psychological costs such as increased stress, loneliness, and depression (see Zevitz and Farkas (2000a), Tewksbury (2005), and Levenson and Cotter (2005)). Thus, for individuals other than RSOs, f_i would be higher.¹²

In contrast, RSOs would already face the costs associated with notification, so committing another offense only has the effect of prolonging their presence on the registry.

¹² We believe this is, in all likelihood, correct. However, we note a clever argument made by Teichman (2005) showing how the imposition of non-legal punishments for sex offenses could lead to lower expected punishment levels. Non-legal punishments cause fewer offenders to be willing to plea guilty to sex offenses and allow them to commit credibly to go to trial. Prosecutors with limited resources—who previously won pleas to sex offenses—may optimally respond by taking a few cases to trial and accepting many pleas for other, less serious offenses.

Given that most RSOs face an extended registration period (the federal requirement is 10 years but a number of states have lifetime registration for some or all types of offenses), this may exert a relatively small influence on their behavior. Moreover, some researchers have proposed that the negative consequences of notification may cause RSOs to commit more crime (Freeman-Longo (1996), Prentky (1996), Winick (1998), Presser and Gunnison (1999), Edwards and Hensley (2001)). In the context of our model, punishment (f_i) would stay constant for RSOs (or perhaps rise slightly), while the relative utility of criminal behavior (u_i) would rise.¹³

Second, by allowing local residents, friends, and acquaintances to identify and avoid registered offenders, notification may increase the costs of targeting this subset of potential victims. Indeed, a major motivation for the passage of Megan's Law was the presumption that Megan Kanka would have avoided her fate had her parents been notified of her eventual attacker's presence in the neighborhood.¹⁴ However, even if local residents can avoid victimization, it is unclear to what degree this will mitigate sex offenses overall. Prentky (1996) makes this point succinctly:

“Although the immediate neighbors will be able to warn their children to stay away from an offender, there is nothing to prevent the offender from going to the adjacent community, or getting into his car and driving to an even more distant community. In other words, we will accomplish nothing more than changing the neighborhood in which the offender looks for victims. For those with a rudimentary appreciation of the forces that motivate repetitive sex offenders, it is all too obvious that notifying the neighbors will serve no purpose if the man is intent on finding a victim.”

If offenders can easily target victims outside of their neighborhood who are unaware of their presence, then notification may change the relationships of offenders and victims but have a

¹³ Although we conceive these burdens on offenders as raising the relative utility of criminal behavior, one could also think of them as lowering punishment levels because they make life in prison seem relatively more attractive than life on the outside. Both of these effects would increase offenses committed by registered sex offenders. We thank David Autor for this observation.

¹⁴ Though no legislators disputed this claim, other neighbors claimed that local households, including her parents, did know that the house where Megan was killed contained a sex offender. See Filler (2001).

negligible impact on overall crime rates. In other words, the response of criminals to notification may result in crime displacement, rather than crime reduction.¹⁵

In addition to raising the cost of targeting local victims, there may also be a “community policing” effect of notification (Lieb (1996)) that increases the likelihood that an offender is apprehended if they attack a local victim (e.g., by increasing vigilance and knowledge of an offender’s actions within the neighborhood). Again, if the likelihood of punishment only rises for crimes committed against local victims, the offender may go other neighborhoods to commit sex offenses.

The ideas laid out above can help distinguish ways in which the impacts of these laws can be measured with aggregate crime statistics. The impact of registration on crime via increased probability of punishment (due to improved police surveillance and apprehension) should reduce crime, increase the probability of arrest, and increase the speed of arrests. However, the effects should be small when relatively few offenders are registered, and should grow in proportion to the relative size of the registry.¹⁶ Thus, the timing of this effect will depend on how long the registry has been in existence, the degree to which the registration law applied retroactively to previously released offenders, the inclusion or exclusion of offenders convicted of less serious crimes, and overall compliance with the registration law. In contrast, any impact of registration on (f_i) for forward looking, unregistered individuals would not depend on the size of the registry, and should have no impact on the probability and speed of an arrest via police behavior.

Notification may raise the expected punishment (f_i) for individuals other than RSOs, and this would have a negative impact on aggregate crime, irrespective of the size of the sex offender registry. It may have an impact on the probability/speed of arrest to the extent that individuals can shift towards targeting offenses (i.e. victims) where the probability of punishment is lower. This response may in turn affect aggregate offender-victim relationships if offenders’ probability

¹⁵ Crime displacement has been an important consideration in other empirical research on criminals’ responses to changes in their environments. For example, Jacob et al. (2004) consider displacement of crime along a temporal dimension due to weather shocks, and Di Tella and Schargrotsky (2004) test for geographic crime displacement in their study of the effect of police on crime.

¹⁶ As noted above, it is possible that registration can lower crime even with an empty registry. Forward looking offenders may account for the increased probability of punishment for future crimes and be less inclined to offend in the present. However, this was not the mechanism for crime reduction put forth by supporters of this legislation.

of punishment is correlated with their relationship to victims. For example, the reporting rate to police, and hence the probability of punishment, may be significantly lower for crimes committed against children within families (see Filler (2001)).

Notification may also have several, offsetting effects on RSOs: increasing the cost to targeting local victims, increasing the probability of punishment for local crimes, slightly increasing expected punishments, and increasing offenders' relative utility of crime commission. The impact of these combined effects on overall crime frequency is indeterminate, though any impact is likely to grow with the size of the registry. The local nature of notification may cause offenders to shift towards targeting strangers, rather than neighbors and acquaintances in their communities.¹⁷ The probability/speed of arrests may also increase for local crimes, though the impact on the overall probability/speed of arrests is unclear, given the potential shift towards crimes committed outside of the community.

Our simple model and the discussion above give rise to the empirical specification we use in this paper. To examine the frequency of crimes, we estimate the following type of reduced form equation:

$$C_{jt} = \alpha_j + \gamma_t + \lambda X_{jt} + \sum_s D_j^s (\beta_0 Rg_t^s + \beta_1 Nt_t^s + (\beta_2 Rg_t^s + \beta_3 Nt_t^s) * RgSize_{st}) + \varepsilon_{jt} \quad (2)$$

C_{it} is a measure of crime frequency (e.g. offenses per 10,000 people) for reporting area i in time period t . (Our analysis can be done at various levels of geographic and time aggregation, so we use the phrases “reporting area” and “time period” for generality here.) α_i is a reporting area fixed effect to capture any persistent heterogeneity in crime across areas, γ_t is a time effect to capture any secular changes in crime over time, and X_{it} are time varying reporting area characteristics that are likely to impact crime (e.g., demographics).¹⁸ The variables “ Rg_t ” and “ Nt_t ” are vectors of dummy variables indicating which states (if any) had a sex offender registry or a notification law in place during time period t , and “ $RgSize_t$ ” is a vector measuring the size of

¹⁷ Though this is the effect implied by our model, it is also possible that notification may cause victims to be more likely to recognize their assailants as community members. This could lead towards a shift away from reports of crime by strangers towards more reports of crime by neighbors. However, this would merely reflect a change in reporting, not a real change in victimization.

¹⁸ We will test the robustness of our results to the inclusion of additional controls such as reporting area trends and/or state-year fixed effects. We omit these from the basic specification for simplicity.

the offender registries of each state in time period t .¹⁹ D_i is a dummy variable indicating the state in which reporting area i is located.

The coefficient β_0 indicates the impact of having a sex offender registry on individuals other than RSOs. If the registry increased the expected punishment for unregistered individuals (due to future registration) then this coefficient should be negative. A stronger prediction of our model is that the coefficient β_2 should be negative, reflecting the increase in the probability of punishment for RSOs.

Likewise, the coefficient β_1 indicates the effect of a sex offender notification law on individuals other than RSOs. We hypothesize that this coefficient should be negative, reflecting the increase in the expected punishment for unregistered individuals from notification. In contrast, we do not have a clear prediction for the coefficient on β_3 due to the offsetting effects that notification might have on RSO behaviors. A finding that β_3 is negative would suggest the most important effect is to reduce the availability of victims. Such a finding would considerably bolster the claims made by proponents of notification, as protecting the public was the law's intended effect. However, the other factors mentioned above (i.e., shifting of victims and increased recidivism due to "brutalization" caused by notification) could lead to an insignificant or even positive estimate for β_3 .

We can also use the specification in Equation 2 to examine the impact of registration and notification on the relationship between victims and offenders. However, instead of examining relationship change across all sex offenses, we separately examine offenses where the victim and offender have a particular type of relationship. If having a sex offender registry or notification law increases expected punishment for non-RSOs (β_0 and β_1), this should have similar effects on crime reduction across victims. However, the impact of the registry on RSOs should be greatest (β_2 most negative) for offenses against local victims. For incidents with a distant offender-victim relationship (e.g., crimes against strangers) we would expect to find a much smaller effect or, potentially, a positive effect from offenses being displaced from local victims to distant

¹⁹ We present a simple specification with a general notification law for simplicity. In reality, there are different types of notification, including access to a paper registry at a local police department, public internet access, and proactive "community notification." To the extent possible given our data, we explore variation in the impacts of these different types of laws.

victims. How the impact of notification on RSOs (β_3) should vary across victims is unclear. If the increased cost of targeting local victims is a dominant effect, then we should find a negative effect for local victims and a zero or positive effect for more distant victims. However, if the increased relative utility of crime commission (i.e., the “brutalization effect”) is dominant, then we might expect to see a positive effect across all victims.

When considering the impact of registration and notification on the probability/speed of an arrest given a reported sex offense, we adjust our specification to account for the fact that we use incident level data. We add a subscript i to denote an incident within reporting area j , and use A_{it} to denote the outcome of interest (Equation 3). Other variables follow the notation from Equation 2 described above.

$$A_{it} = \sum_j D_i^j \alpha_j + \gamma_t + \lambda X_{it} + \sum_s D_i^s (\delta_0 Rg_t^s + \delta_1 Nt_t^s + (\delta_2 Rg_t^s + \delta_3 Nt_t^s) * RgSize_{st}) + \varepsilon_{it} \quad (3)$$

The impact of having a sex offender registry or a notification law on the likelihood or speed of arrests of individuals *other* than RSOs (δ_0 and δ_1) via changes in police behavior is expected to be zero, since police have no additional information on these individuals. However, these laws could have an effect on aggregate arrest statistics via changes in criminal behavior. In Equation 1, individuals choose the number of offenses committed against each victim. If the punishment level (f) rises, then individuals will likely decrease their number of offenses by substituting away from “marginal” victims where the probability of punishment (p) is relatively high. Thus, we may see a negative effect of registries and notification laws on the likelihood and speed of arrests (δ_0 and δ_1), but we should only see this if these policies also affect the frequency of offenses (β_0 and β_1).

The impact of these laws on RSOs is also unclear. The first order effect of the registry (δ_2) should be to increase arrest probability/speed, reflecting increased ability by law enforcement to monitor, locate, and apprehend RSOs. However, any response of RSOs to the laws should work against finding this in the aggregate arrest data—RSOs should realize they are being monitored and decrease offenses committed against victims where the probability of arrest is high. The impact of notification (δ_3) is also unclear. The first order impact of “community policing” may be an increase in the probability/speed of arrests, but the shifting of offenses across victims will go in the opposite direction. In addition, any overall increase in offenses due

to a rise in the relative utility of crime commission (u) should lead to increases in arrest probability, as offenders victimize those for whom the probability of punishment (or the cost of commission) was previously too great. Thus, the impact of these policies on arrest probability and speed must be interpreted in light of their impact on overall crime frequency.

The predicted relationships between the registration and notification variables in Equations 2 and 3 and the model parameters and outcomes of interest are given in Table 2.

4. Data

The primary source of data we use in our analysis is the National Incident Based Reporting System (NIBRS). NIBRS is a part of the FBI's Uniform Crime Reporting Program (UCR), but presents several opportunities for research that are unavailable with standard UCR crime data. These stem from the fact that NIBRS links information on victims, offenders, and arrestees for each incident in the dataset. Thus, in addition to examining the impact of registration and notification on reported crime frequency, which previous studies have sought to do using UCR data, we are able to examine effects on the relationship mix between offenders and victims (or the "incidence" of sex offense), on the ability of police to secure an arrest (and how quickly) given that a crime has been reported. NIBRS also provides detailed geographic identifiers on each incident and the exact date on which each incident occurred.²⁰ We are therefore able to control for relevant fixed and time varying local characteristics, and we can exploit within-year variation in the timing of sex offender laws.

While several features of the NIBRS are useful for our analysis, it does suffer from significant limitations. First, like most data on crime, NIBRS only contains information on incidents recorded by police. Changes in reported crime may be driven by true changes in

²⁰ For roughly 20 percent of sex offenses and 13 percent of assaults, we have the date the incident was reported to the police, rather than the date on which the incident occurred. Unfortunately, the NIBRS does not report both the date of the incident and the date of the report to police. However, we can try to get a rough sense of the gap between incident and report among the incidents for which we do have an incident date by exploiting the fact that a subset of crimes reported in the NIBRS took place in a prior year. If we examine sex offenses, of the incidents that took place in the year prior to the NIBRS year in which they were reported, 23 percent took place in December, 16 percent in November, and 11 percent in October, while only 10 percent took place between January and March. Thus, it is likely that most crimes reported to the police take place within a few months of their reports. However, a non-trivial fraction may be reported with a considerable lag.

victimization or by changes in reporting by victims.²¹ We return to this issue when interpreting our findings. Another limitation is that the first year for which NIBRS data are available is 1995—one year after the federal government required that states create sex offender registries. Thus, the amount of data available prior to the advent of registries for most states is less than ideal. In addition, only a subset of states participates in the NIBRS program. In 1995, there were just nine states; by 1998, eight more states joined, and 30 states were included as of 2004. Our legal research and data analysis focus on 15 states that were in the NIBRS by 1998: Colorado, Connecticut, Idaho, Iowa, Kentucky, Massachusetts, Michigan, Nebraska, North Dakota, Ohio, South Carolina, Texas, Utah, Vermont, and Virginia.²² These states are geographically spread across the U.S. (see Figure 2), but they do not include any states from the far west (e.g., California) and the “deep” South (e.g., Mississippi).

In addition to the complexity of new states joining NIBRS over time, the participation of law enforcement agencies can vary within a state. Agencies are identified in NIBRS by an “Originating Agency Identifier” (ORI) code, and, within a state, the number of reporting ORIs increases over time. For example, the number of reporting ORIs from Nebraska more than quintupled between 1998 (the first reporting year) and 2005. We include ORI fixed effects in all of our regressions. Thus, in addition to taking account of the growth in reporting agencies over time, we also control for persistent heterogeneity in ORI characteristics.

Because NIBRS provides incident level data, the files are quite large. To simplify our analysis, we limit our focus to incidents (and arrests) with a reported sex offense. Sex offenses can be categorized into three groups: rape and sexual assault, sexual molestation (called “forcible fondling” in NIBRS), and other non-violent sex offenses (i.e., Incest and Statutory Rape). Multiple offenses can be reported in a single incident, and we classify an incident as a sex offense if any of the reported offenses fell under one of the sex offense categories.

²¹ This issue runs throughout most empirical research on crime. There does exist a large, publicly available data set on crime as reported by victims—The National Crime Victimization Survey (NCVS)—but it does not contain geographic identifiers that would allow us to link registration and notification laws to crime incidents. Although we cannot examine reporting issues directly, it is encouraging that national crime rates reported in the NCVS have tracked UCR crime rates fairly well since the early 1990s.

²² Tennessee and West Virginia also joined the NIBRS in 1998. However, early in our legal research we found that they had passed both its registration and notification laws by that time. Given that we rely on a “difference-in-differences” identification strategy, we did not pursue further the collection of detailed legal data on these states.

Table 3 shows summary statistics on the sample of incidents we examine. For purposes of comparison, we include information on assault as well. Assault is a more common crime than sex offense; there are roughly 14 assaults for every sex offense in our sample. Average annual victimizations per 10,000 averaged for 122.3 for assaults and 8.5 for sex offenses. Reporting of incident dates, arrest rates and time until arrest are quite different for the two types of crime. The frequency with which incident dates are not reported (and only a report date is available) is higher for sex offenses (20 vs. 14 percent). Arrests are less common for sex offenses (25 vs. 36 percent) and the time to arrest is considerably longer (58 vs. 10 days). A small part of this last difference is driven by arrests made on the same day as the incident; this occurs for just over 10 percent of assaults but for less than 2 percent of sex offenses.

The relationship between offenders and victims is similar for sex offenses and assaults, with family members and acquaintances as the two most common categories of offenders. The overall fraction of incidents with an acquaintance is somewhat higher for sex offenses (30 vs. 23 percent) but (reported) incidents of sex offense are less common between family members (23 vs. 26 percent) and significant others (8 vs. 16 percent). For both sex offenses and ordinary assaults, in 20 percent of incidents the victim claimed that the offender was a stranger or did not know his/her relationship to the offender.

Assaults and sex offenses differ substantially in the demographic characteristics of victims. While 47 percent of sex offense victims were aged 15 or below, the corresponding figure for assault was only 18 percent. Sex offense victims are also more likely to be female (88 vs. 58 percent) and white (78 vs. 69 percent). Offender characteristics between the two crimes also differ. The age distribution of sex offenders is wider than for assault, with more mass in both the youngest and oldest age groups. Reported sex offenders are much more likely to be male (95 vs. 76 percent) and White (68 vs. 61 percent).

In addition to the information on victims, offenders, and arrestees from NIBRS, we add annual, county level demographic data from the U.S. census on the fraction of the population in 18 age categories and five ethnicities. We also add annual state level data on income per capita, poverty rates, and unemployment. These demographic variables serve as controls in our regressions. While ORIs tend to be smaller than counties, we believe these controls should reflect most demographic shifts that may have occurred in ORIs over our sample period. For the

2 percent of ORIs that are located in multiple counties, we assign them to the county with the majority of its covered population.²³

Last, but not least, we use our legal research to classify each incident based on the laws in effect and the size of the offender registry at the time of the incident. We mark each incident with a set of dummy variables for the state of the registration and notification provisions in effect in the state. For example, an incident that occurred on July 1, 1995 in Michigan would have a registration law enacted, but no registration or notification laws in effect at the time, while another incident occurring on July 1, 1999 in Michigan would have registration, public access, and internet access in effect.

We also mark each incident with a value for the size of the registry at the time the incident took place. Unfortunately, historical data on the size of registries across states is very hard to find, particularly for the early years of registries' existence. However, we were able to find a limited amount of information on the number of registered offenders in each state.²⁴ These cross sectional reports allows us to make some progress in determining the historical size of the registries for the NIBRS states. Simply put, we run a state-specific least squares regression of registry size on quadratic function of date, using all data points available for each state, and then use the predicted values from this regression as measures of registry size. In addition, we know based on our legal research that a number of states did not apply their laws retroactively, and, for any such state, we include a zero at the start of the registry when making these empirical estimates. The results of these regressions are depicted in Figure 3a.

An alternative method for gauging the size of sex offender registries is to rely only on timing and retroactivity provisions of each registry. For example, one could estimate registry size using the following equation:

²³ In a future revision, we plan to add weighted averages of county characteristics for ORIs that cover multiple counties. We also intend to add county level controls for income per capita, poverty, and unemployment, instead of state level controls.

²⁴ Two reports from the National Institute of Justice provide us with states' registry sizes at the end of 1998 and 2001 (Bureau of Justice Statistics (2002)). In addition, we have been able to gather documents posted on-line by the National Center for Missing and Exploited Children that provide counts of offenders registered in each state at several points in time from 2003 through 2007. The exact dates when the information was gathered varied by state, but, in general, this gives us a snapshot of registry sizes in 2003, 2005, 2006 and 2007. We also add a few additional data points from articles and reports for specific states.

$$RgSize_{ist} = \frac{t - t_{retro}}{365.25}$$

The size of the registry when incident i is committed on day t in state s ($RgSize_{ist}$) is measured by the difference (in years) between the date of the incident and the date to which the registration law is retroactive (t_{retro}). This relies on the intuition that registries will start very small in states whose laws were not retroactive but states with retroactive provisions will have larger registries, all else equal, from the start. From this point, all registries should grow steadily over time as more offenders fall under the law's requirements.²⁵ This measure of $RgSize$ roughly approximates the number of “cohorts” of sex offenders required to register by law.

Both measures of registry size are plotted in Figure 3b.²⁶ It is clear from this figure that the two series are similar, but do not always coincide. For example, both measures of registry size show that Michigan's registry started quite small and grew steadily over time, and both measures show that Massachusetts' registry started out with a sizeable number of registrants and grew only slightly. However, the legal formula predictions for some states diverge considerably from the empirical predictions. In particular, it appears that states that instituted significant retroactivity clauses after their registry began (e.g., Connecticut, North Dakota, and Texas) did not see a sharp rise in the number of offenders registered, as the legal formula would imply. Therefore, in our analysis, we rely on the empirical predictions as a measure of registry size. It is our hope to be able to uncover more data and improve these estimates in a future revision.

5. Empirical Methodology, Results, and Discussion

²⁵ At some point, some offenders may qualify to be removed. However, under federal law, violent offenders and offenders who commit crimes against minors (a large percentage of the NIBRS incidents) must register for a minimum of ten years. In any event, in most states, even many non-violent offenders must register for a minimum of ten years and violent and repeat offenders typically must register for life. Also, the actual size of the registries will depend on the inclusion or exclusion of offenders convicted of less serious crimes and compliance with the registration law. We do not incorporate these factors into this registry size measure.

²⁶ For the legal formula estimates, we take the earlier of the two dates in cases where the retroactivity date differs for conviction and release. We also cap the measure at 10 in order to avoid unusually large registry sizes in states with long retroactivity clauses. For example, South Carolina and Utah both had full retroactivity—i.e., registration of anyone who was *ever* convicted of a sex offense—for at least part of the period we examine. To normalize across states, we divide the estimated registry counts by annual population estimates taken from the U.S. Census.

We estimate least squares regression coefficients of the effect of registration and notification laws using the specifications outlined in Section 3. Regressions includes ORI fixed effects, year and month fixed effects, and control for annual per-capita income, unemployment, and poverty rates (at the state level) and the fraction of the population in five ethnicity categories and five-year age categories (at the county level). In addition, we run regressions that include the number of assaults committed per 10,000 persons as a control variable. Though we do not report the coefficients, assaults are always positively related to sex offenses, highly statistically significant, and serve as a proxy for other ORI specific time-varying factors that influence crime rates and may be correlated with the legal variables. While including assaults as a control may be problematic (assaults might also be affected by sex offender registration and notification), their inclusion turns out to have little or no influence on our results.

The registry indicator signifies that the state has an active offender registry, and registry size is measured using our empirical estimates, as explained in Section 4. For notification, we have a number of measures from which we can choose, since the details of these laws varied considerably by state. As discussed above, there are three major types of notification: public access, internet access, and community notification. Within these categories, we focus on states whose statutes implied widely available or “full” access to information on sex offenders by the general public. In particular, we define full public access to mean a law in which access is not subject to the discretion of local authorities and where members of the public can inquire about local offenders in general, as opposed to making a person specific inquiry. We define full internet access to mean that the internet registry is on-line and generally complete.²⁷ We define full community notification to mean a law that makes notification mandatory and includes neighbors or the media as a group to which the information is provided.

The results of our analysis of crime frequency generally support the predictions from our model, although there are some notable departures. Our unit of observation is an ORI-by-month cell, and the dependent variable is measured as incidents per 10,000 persons covered by the

²⁷ We located articles in a number of states suggesting that the internet registry, when launched, was incomplete. For some of these states, we additionally found notice that the registry had been completed. For states where we have an indication of incompleteness but do not have any notice of completion, we consider the internet to be fully available three months after the site was launched.

ORI.²⁸ The regressions are weighted by ORI population coverage so that the coefficients reflect average changes in crime risk faced by a typical person covered by the NIBRS sample, and to take account of likely heteroskedasticity.²⁹ Our coefficient estimates are similar if we remove these weights, though they are less precise.

Our results for overall frequency of sex offenses are shown in Columns 1 and 2 of Table 4, where Column 2 includes a control for assault frequency. The impact of the (empty) sex offender registry is negative both specifications, though only marginally statistically significant when assaults are included as control variable (p-value .11). The point estimates suggest that the registry is estimated to decrease sex offenses by 0.06 to 0.07 per 10,000 persons per month. Multiplying by twelve, this implies a reduction of about 0.8 crimes per 10,000 per year, or about nine percent of the sample average. The interaction of the registry indicator with the size of the registry is negative and statistically significant in both specifications, as predicted. At the median registry size (about 13 offenders per 10,000 persons), we estimate that the registry lowers crimes by an additional 1.5 per 10,000 persons per year, or 18 percent of the sample average. These estimates give support to the idea that placing information on offenders in the hands of local law enforcement helps reduce the frequency of sex offenses.

The estimates of the impact of notification laws on crime are mixed. We find no significant impacts of either public access or internet access statutes on overall crime frequency. However, the estimated coefficient on community notification is large, negative, and statistically significant in both specifications. This estimate would suggest that active community notification laws—probably the most aggressive and visible form of notification—reduce crime frequency by 2.1 to 3.2 crimes per 10,000 persons per year via a deterrent effect on individuals not currently registered as sex offenders. While this is the sign we predicted from our model, the size of coefficient is quite large and should be interpreted with caution.

²⁸ For the purposes of analyzing data aggregated to the ORI-month level, our legal variables reflect the law as of the 15th day of the calendar month, even though our legal variables can vary within calendar months in the incident level data.

²⁹ To illustrate the heteroskedasticity issue, suppose we have two ORIs, each with ten sex offenses per 10,000 persons in a given month, but one ORI has 1000 persons and another has 100,000. These two values correspond roughly to the 5th and 95th percentile of covered population among ORIs in our sample. The smaller ORI in this example had only one sex offense, and would drop to zero per 10,000 persons if there are no crimes the following month (which is quite likely to happen given sampling variation). In contrast, the large ORI had 100 sex offenses during the month, and is much less likely to drop to zero per 10,000 due to sampling error.

Last, but not least, the interaction of notification with registry size is positive and marginally statistically significant (p-values .092 and .104 with and without assaults, respectively). Although these coefficients are not precisely estimated, they would suggest that any beneficial impact of notification on crimes committed by *registered* sex offenders (via increased costs of victimization and/or community policing) may be dampened by the punitive aspects of notification, as discussed in Section 3. Notification appears to deter potential sex offenders from committing crime, but convicted sex offenders may be brutalized by notification laws, causing them to offend more often, all else even.³⁰

Next we investigate the extent to which registration and notification laws may have affected the relationship mix of offenders and victims. We divide victims into three groups based on the intimacy of their relationship with the offender: “close,” “near,” and “stranger.” The close group includes family members, significant others, and friends; the near group includes neighbors, acquaintances, or victims who “otherwise know” the offender, and the stranger group includes incidents where the victim claimed the offender was a stranger or where the offender-victim relationship was unknown to the victim.³¹ As we described above, notification laws are designed specifically to protect individuals who know offenders or come into contact with them in their local area by helping these potential targets avoid situations in which they or their friends and relatives could be victimized. We examine whether (as lawmakers hoped) the frequency of victims who were close or near to the offender drops, and whether (as lawmakers had not hoped) the frequency of “stranger” sex offenses increases due to crime displacement.

The results of this analysis lend some support to the notion that any effects of these laws were concentrated on crimes against individuals with a significant personal relationship with the offender (Table 4 Columns 3 to 8). The estimated coefficients on the indicator for having a registry are all negative, but none are statistically significant in any specification. In contrast, the interaction between this indicator and the size of the registry are significant and negative for both

³⁰ We also have run regressions that examine Rape and Sexual Assault, Sexual Molestation, and other non-violent sex offenses separately. Though point estimates and statistical significance vary across these specifications, there are no major qualitative differences in the results across these crime categories.

³¹ Note this is distinct from instances where the relationship variable is missing in NIBRS.

the close and near victim groups, but insignificant (and quite close to zero) for the stranger group. This lines up closely with the idea that the benefits of registration may help reduce crime by local offenders against local victims, but would not reduce crimes committed against more distant individuals. The fact that we do not see a significantly positive effect on the number of crimes committed against strangers suggests that victim displacement may not be occurring as a result of the passage of registration laws. Lack of displacement may indicate that the costs, broadly interpreted, of targeting a stranger are quite high, or that the model outlined in Section 3 does not characterize sex offender behavior.³²

The estimated coefficients for notification laws are more similar across the three groups. The only statistically significant relationships between notification provisions and crimes are for the close group of victims. Specifically, both internet availability and active community notification have negative coefficients, and there are positive interactions between registry size and both internet availability and community notification. However, the estimated coefficients for near and stranger groups are similar in magnitude, only less precisely estimated. This pattern of results is consistent with the earlier finding that notification in and of itself may serve as a deterrent to crime, but also that it is less effective at reducing crime among offenders on the registry by allowing “local” victims to protect themselves.

There are, of course, other ways in which these findings can be interpreted. It is important to recall that we examine the frequency of *reported* crime, and it is possible that the statistically significant relationships we find are driven by changes in victim reporting behavior. For example, victims who have close personal relationships with their offenders may decide that the sanction of notification is too harsh a punishment and decide to report less frequently. Another possibility may be that these laws cause offenders to move away from family, friends, and acquaintances due to a “shaming” effect. Thus, our results could be explained by decreased frequency with which offenders have *any* contact with closely or “nearly” related individuals.

³² Put differently, the fact that sex offenders choose not to offend rather than select an alternative target is consistent with sex offenders being “impulse” criminals on the margin. Whether sex offenders are impulse or rational criminal actors has obvious significance to designing policies to deter and incapacitate them.

Nevertheless, this type of explanation would still imply an overall reduction in crime risk for states passing these laws.³³

When turning to the analysis for time until an arrest was made, we make several changes to our regression specification. First, we examine incident level data instead of ORI-month aggregates. Second, we drop the control for assaults (which is an aggregate statistic) and include several incident specific variables in addition to the controls mentioned earlier: victim age indicators (in five-year categories), victim sex and ethnicity indicators, indicators for the type of offender-victim relationship (i.e., close, near, stranger), and indicator for the type of sex offense (i.e., rape and sexual assault, sexual molestation, other non-violent sex offense). The motivation for this added set of controls is to control (as best we can) for the information available to law enforcement authorities and to examine law enforcement performance conditional on this information.

The estimated effects of registration and notification laws on the probability of arrest are shown in Column 1 of Table 5. Neither of the registration related variables are statistically significant. Thus, the negative impacts of the registry laws on crime frequency are not mirrored in the likelihood of arrest, as we predicted. However, the impacts of notification laws on the likelihood of arrest are indeed in line with our predictions. The main (interaction) effect of community notification is statistically significant and negative (positive), just like the impact on overall crime frequency. Interestingly, the same pattern is true of the coefficients for public access laws, though these variables did not significantly influence crime frequency in Table 4. These results provide some support for the notion that notification causes individuals other than registered offenders to shift away from offenses against victims where there is a relatively high likelihood of punishment, and that notification has the opposite effect on criminal behavior by registered sex offenders.

When examining the speed with which arrests are made, it is important to recall that the NIBRS reports either the date an incident took place or the date on which police received a report. The latter group of incidents is really ideal for our analysis of speed of arrests, since the time from the incident to the arrest will be affected by the timeliness of reporting by victims. We

³³ Whether it implies an overall reduction in crime risk for society is unclear. States that pass extremely harsh laws against sex offenders might cause them to move across state lines and offend elsewhere.

therefore restrict the sample of incidents to those where we have a report date.³⁴ In addition, it is important to recognize that the distribution of the time until arrests are made is highly skewed, with a large number of arrests taking place within one day of the report and then a long tail of values extending out over several months. We believe this is indicative of a dichotomy in arrest patterns. Some arrests are made on the spot, either at the scene of a crime or immediately after a credible report is given regarding an offender who is easily identified and apprehended, while others require investigative work. In order to take account of this “split” in the time until arrest measure, we split our analysis into two pieces. First, we run a linear probability regression on whether an arrest is made within one day of a report date. Second, we run a regression on the log of the days it takes to make an arrest, conditional on the arrest happening at least two days after the crime report.

The results of these two regressions are shown in Columns 2 and 3 of Table 5. We find a positive and statistically significant relationship between the probability that arrests are “quick” and the interaction of registry with registry size. This is in line with the idea that police may be more likely to monitor registered sex offenders closely and quickly apprehend them in the event of a reported incident. However, it goes against our prediction that offenders would be less likely to commit offenses where they would be easily caught and punished. In addition, we also find a positive main effect of internet availability and a negative interaction of internet availability with registry size. These estimates also go against our earlier predictions for the impact of notification. The results on time-to-arrest for incidents where arrests take longer than one day are quite imprecise. There is a marginally significant main effect of active community notification. This is in line with the results of Column 1; arrests are not only less likely, they also take longer to materialize when they occur. However, given the lack of precision of the estimates, it is difficult to draw firm conclusions from these results. It is also worth stressing that our sample selection criteria considerably reduce the pool of incidents from which we can make these estimates. Of almost 300,000 incidents in our data, the regressions shown in Columns 2 and 3 of Table 5 are estimated with only about 10,000 observations each.

³⁴ Quick inspection of the NIBRS data reveals that whether a report date or incident date is given is nonrandom. It appears that some ORIs are much more likely to report one type of date than others, and that ORIs are fairly consistent in the portion of incidents reported with each type of date. However, when we examine the summary statistics reported in Table 3 by whether an incident has an incident versus a report date, the characteristics of the crimes are very similar.

6. Conclusion

Using a detailed panel of state legal data and incident-level crime data from NIBRS, we examined the effect of registration and notification laws on the total frequency of crime, the incidence of that crime on various victim groups, and on police performance, conditional on a crime occurring. We find evidence suggesting that registration laws reduce the frequency of sex offenses, particularly for local victims, both by deterrence of potential sex offenders and by “incapacitation” of convicted sex offenders. We also find evidence that notification laws reduce crime, but do so by deterring potential criminals, not necessarily recidivists. In fact, our results suggest that recidivist offenders might be more likely to commit crime in a state that imposes a set of notification requirements, perhaps because of the difficulties that are associated with the public release of their information.

Our results also suggest that the reduction in crime was somewhat “locally” concentrated, in line with policymakers’ intentions, with reductions generally greater among victims with a personal connection to offenders. We did not find evidence that crime displacement increased victimizations among more “distant” groups of victims.

Though researchers are still in the process of measuring the benefits of registration and notification laws, the costs have been well documented. A number of researchers have documented financial, physical, and psychological damage done to registered sex offenders and their families (e.g., Zevitz and Farkas (2000a), Tewksbury (2005), and Levenson and Cotter (2005)). The labor and capital costs to law enforcement agencies who are required to monitor offenders can also be substantial (Zevitz and Farkas (2000b)). Moreover, there is evidence that these laws have created financial and psychological costs for neighbors of registered sex offenders. Linden and Rockoff (2006) and Pope (2006) document declines in property value for households living close to registered offenders, and Zevitz and Farkas (2000b) find little evidence that community notification alleviates concerns among community members who have been notified of an offender’s presence.

The lack of empirical evidence on the benefits of registration and notification has not stopped politicians and policymakers from further regulation of sex offenders. Registration and notification laws are, in some sense, old technology. Today, states are in the midst of imposing

ever more draconian laws, such as residency restrictions and civil commitment, as a means to reduce recidivism. These more restrictive policies clearly impose higher costs on sex offenders and their families than registration and notification laws, and we hope that future research will explore their impact on criminal behavior.

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Figure 1: Timing and Score of Registration and Notification Laws
Among States in the NIBRS Sample

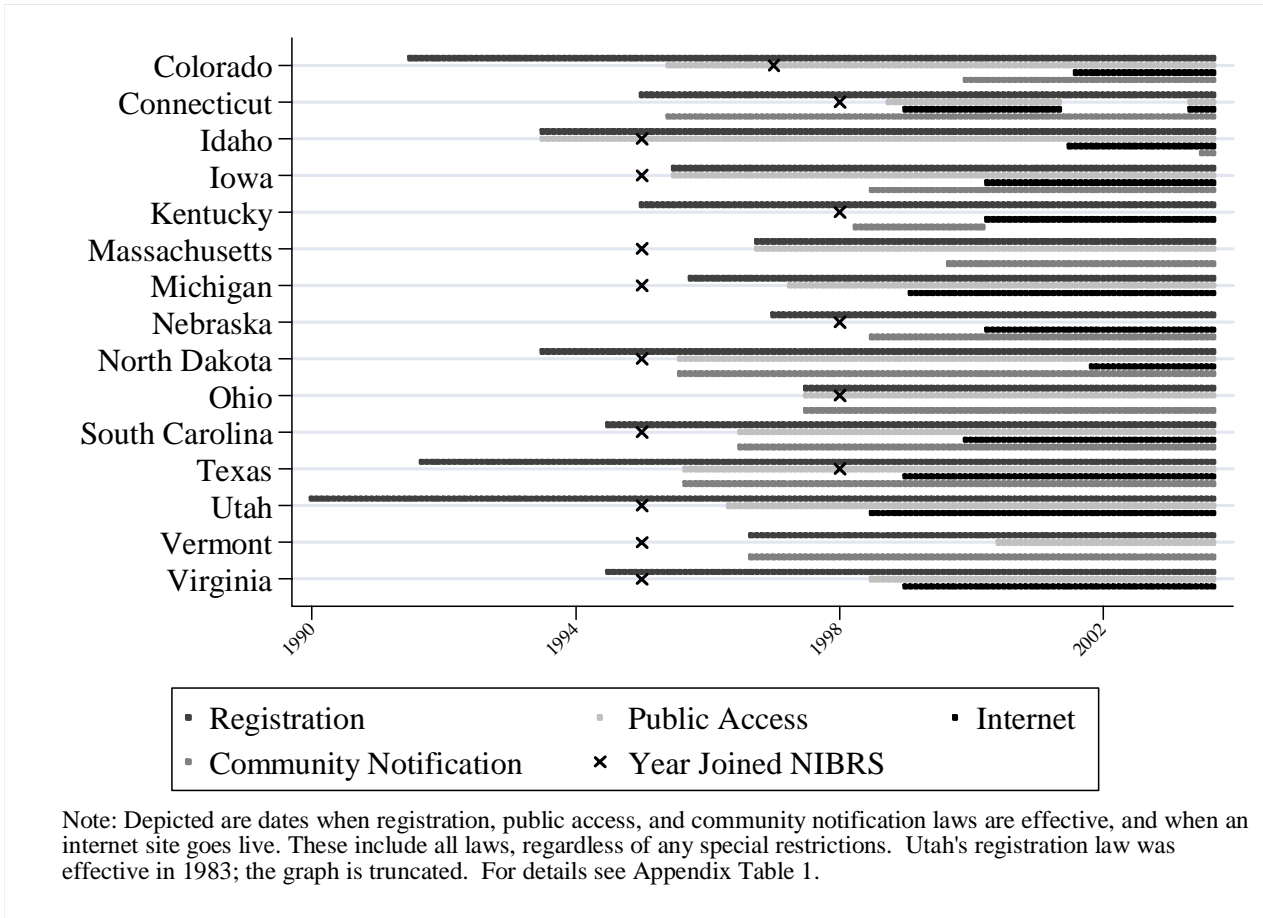
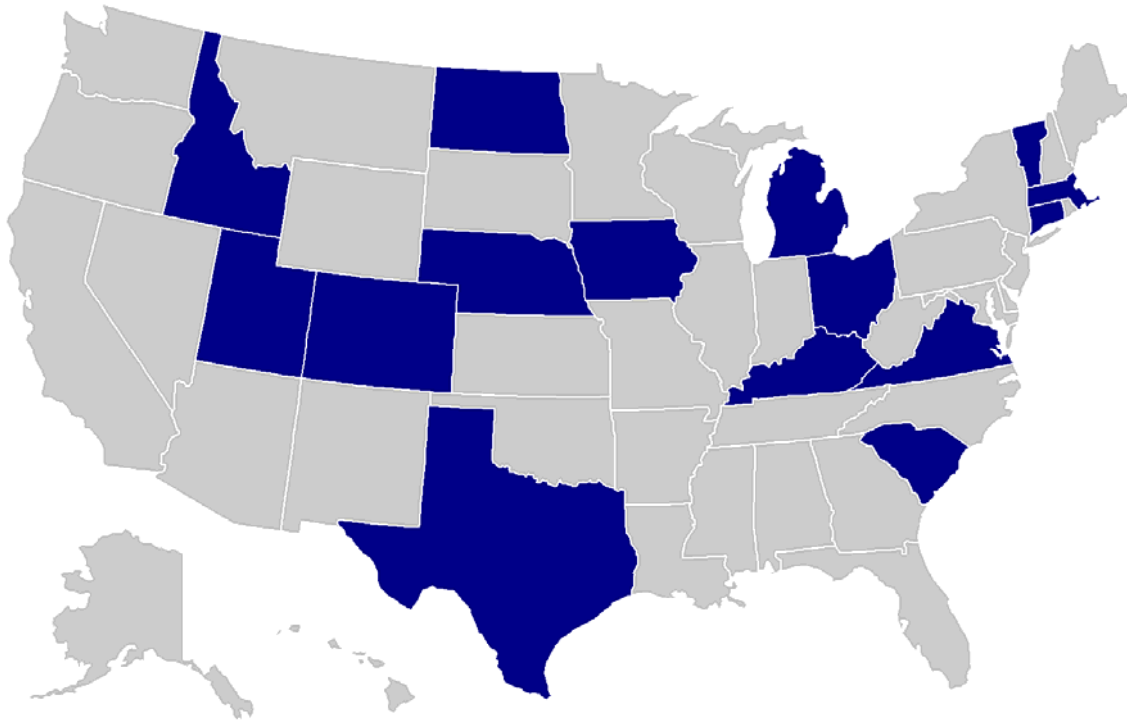


Figure 2: States Included in NIBRS Data Analysis



Note: Shaded states are those included in our analysis. They include: Colorado, Connecticut, Idaho, Iowa, Kentucky, Massachusetts, Michigan, Nebraska, North, Dakota, Ohio, South, Carolina, Texas, Utah, Vermont, and Virginia.

Figure 3a: Observations of Registry Size and Empirical Estimates

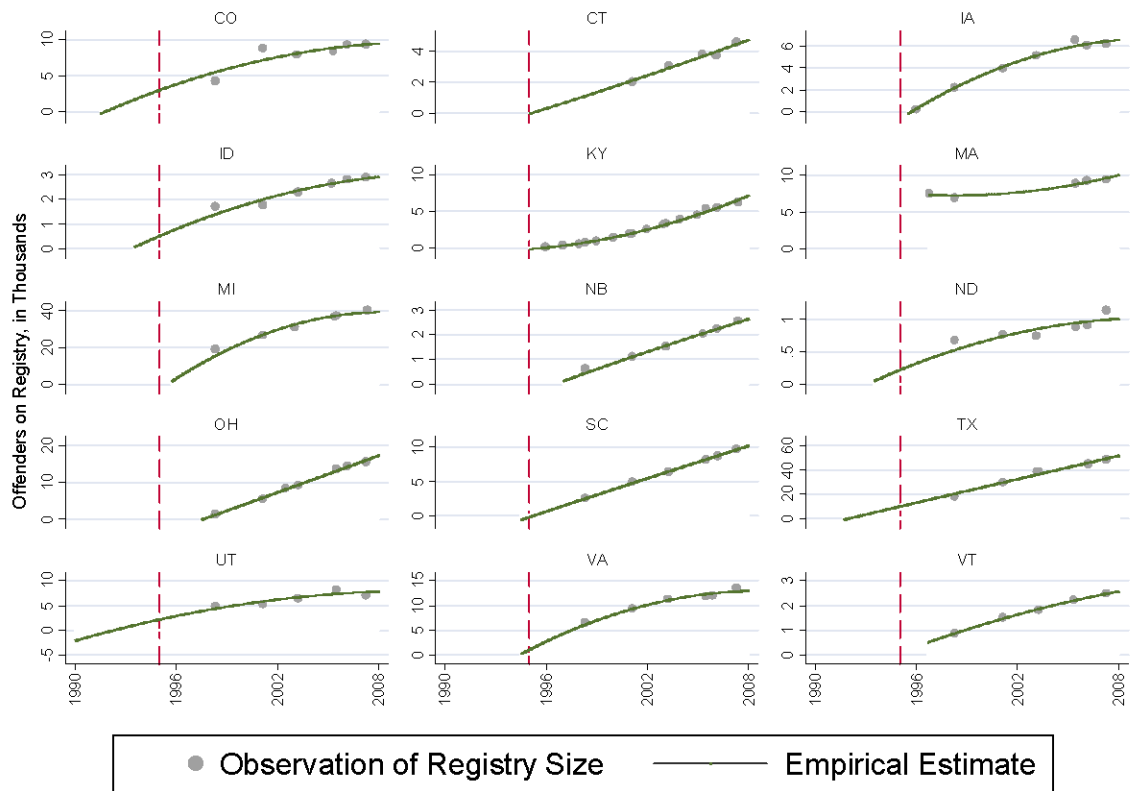
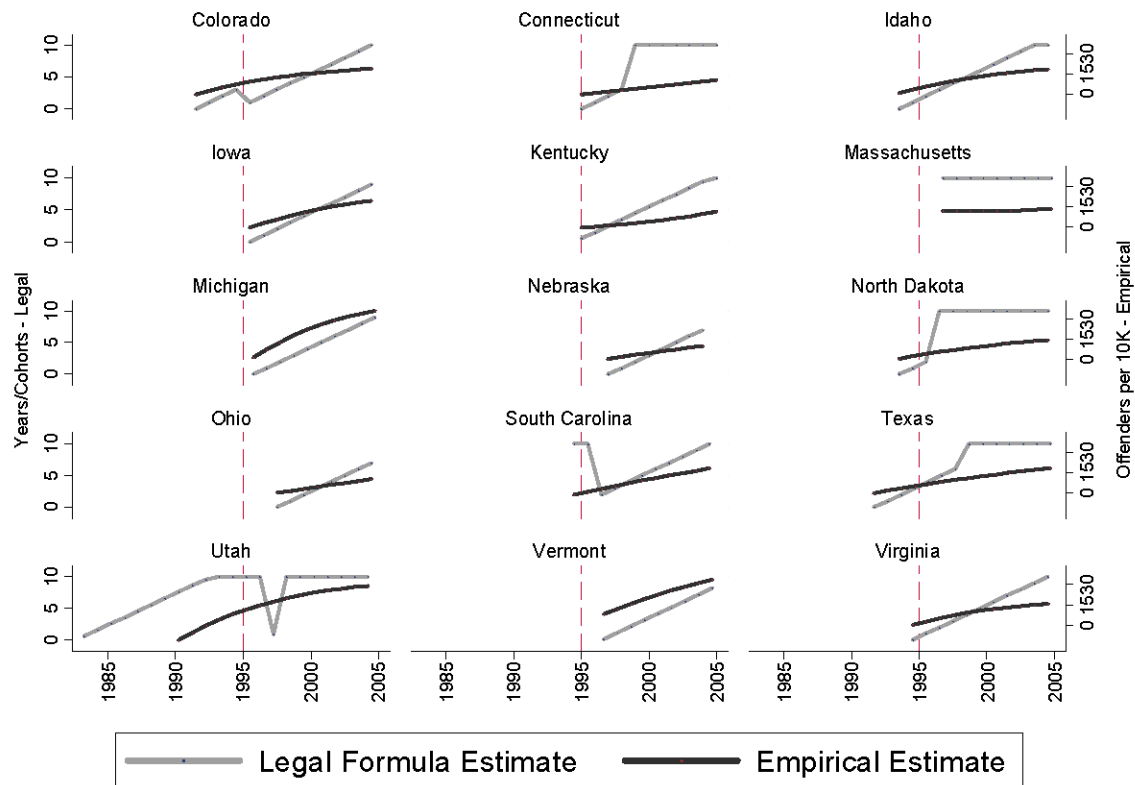
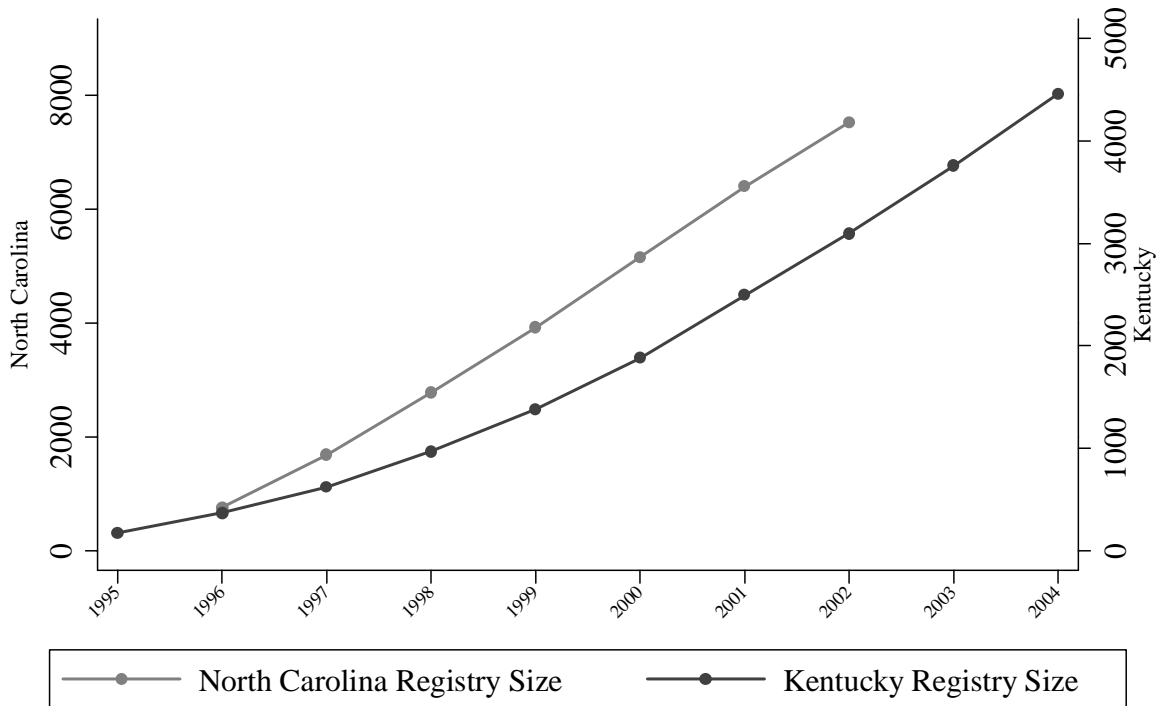


Figure 3b: Two Estimates of Sex Offender Registry Size



Appendix Figure 1: Growth in Offender Registries since Inception in Kentucky and North Carolina



Note: Depicted are the number of registered sex offenders in North Carolina and Kentucky at the end of each year following the start of these registries in January of 1996 and 1995, respectively. North Carolina figures are from reports available on the internet registry website and Kentucky figures are taken from a report by Luallen (2004). Unlike North Carolina, offenders in Kentucky had to be both released and convicted after the law's passage to be required to register.

Table 1: Disagreement Among Researchers on "Registry Dates"

Definition:	Prescott and Rockoff (2007) <i>Registration Effective</i>	Shao and Li (2006) <i>Registry Effective</i>	Agan (2007) <i>"Registry Begins"</i>	Walker et al. (2005) <i>"Registration and Notification Implementation"</i>	Researchers All Agree	Researchers Agree Within Calendar Year
Alabama		1967	5/26/1996	1998	No	No
Alaska		8/10/1994	8/10/1994	1994	Yes	Yes
Arizona		1951	6/1/1996	1996	No	No
Arkansas		8/1/1987	8/1/1997	1997	No	No
California		1944	1955	1996	No	No
Colorado	7/1/1991	7/1/1991	1996	1998	No	No
Connecticut	01/01/1995	10/1/1994	1998	1998	No	No
Delaware		6/27/1994	6/27/1994	1994	Yes	Yes
District of Columbia			6/1/2000	1999	No	Yes
Florida		10/1/1993	10/1/1993	1997	Yes	No
Georgia		7/1/1994	7/1/1996	1996	No	No
Hawaii		6/14/1995	7/1/1997	1998	No	No
Idaho	7/1/1993	7/1/1993	7/1/1993	1993	Yes	Yes
Illinois		8/15/1986	1/1/1996	1996	No	No
Indiana		7/1/1994	7/1/1994	1998	No	No
Iowa	7/1/1995	7/1/1995	7/1/1995	1995	Yes	Yes
Kansas		7/1/1993	7/1/1993	1994	No	No
Kentucky	1/1/1995	7/15/1994	7/15/1994	1994	No	Yes
Louisiana		6/18/1992	6/18/1992	1992	Yes	Yes
Maine		7/13/1992	9/1/1996	1995	No	No
Maryland		10/1/1995	10/1/1995	1995	Yes	Yes
Massachusetts	10/1/1996	10/1/1996		1999	No	No
Michigan	10/1/1995	10/1/1995	10/1/1995	1995	Yes	Yes
Minnesota		7/1/1994	7/1/1991	1998	No	No
Mississippi		8/1/1991	1994	1995	No	No
Missouri		1/1/1995	7/1/1979	1995	No	No
Montana		7/1/1989	1989	1995	No	No
Nebraska	1/1/1997	7/1/1997	1/1/1997	1997	No	Yes
Nevada		1961	1/1/1998	1998	No	No
New Hampshire		1/1/1993	1993	1996	No	No
New Jersey		10/31/1994	10/31/1994	1993	No	No
New Mexico		7/1/1995	7/1/1995	1995	Yes	Yes
New York		1/21/1996	1/21/1996	1995	No	No
North Carolina		1/1/1996	1/1/1996	1996	Yes	Yes
North Dakota	7/1/1993	8/1/1991	1991	1995	No	No
Ohio	7/1/1997	1963	7/1/1997	1997	No	No
Oklahoma		11/1/1989	11/1/1989	1998	No	No
Oregon		1/1/1990	10/3/1989	1993	No	No
Pennsylvania		4/21/1996	4/21/1996	1996	Yes	Yes
Rhode Island		7/1/1992	1992	1996	No	No
South Carolina	7/1/1994	7/1/1994	7/1/1994	1999	No	No
South Dakota		7/1/1994	1994	1995	No	No
Tennessee		1/1/1995	1/1/1995	1997	No	No
Texas	9/1/1991	9/1/1991	9/1/1991	1999	No	No
Utah	3/30/1983	5/19/1987	7/1/1984	1996	No	No
Vermont	9/1/1996	9/1/1996	7/1/1996	1996	Yes	Yes
Virginia	7/1/1994	7/1/1994	7/1/1994	1997	No	No
Washington		6/7/1990	2/28/1990	1990	Yes	Yes
West Virginia		7/10/1993	1993	1993	Yes	Yes
Wisconsin		12/25/1993	6/1/1997	1997	No	No
Wyoming		1/1/1995	1994	1999	No	No

Note: Dates given by Matson and Lieb (1996) are not shown; they review state laws but do not examine criminal behavior.

Table 2: Predictions on Registration and Notification Coefficients

	Variable of Interest			
	Registration Law	Registration Law * Registry Size	Notification Laws	Notification Laws * Registry Size
Model Parameters	p↑ non-RSOs	p↑ (local) RSOs	f↑ non-RSOs	c↑ (local) RSOs u↑ RSOs
Overall Frequency of Offenses	Negative	Negative (Zero w/ Displacement)	Negative	c↑ Negative (Zero w/ Displacement Positive w/ u↑ Brutalization)
Relationship Mix	No Differences Across Relationships	Stronger Local Effects	No Differences Across Relationships	c↑ Stronger Local Effects u↑ No Differences
Probability/Speed of Arrest	Negative (if Frequency also Negative)	Zero/Negative (if Frequency also Negative)	Negative (if Frequency also Negative)	Zero/Negative (if Frequency Zero/Negative) Positive (if Frequency Positive)

Note: For details on how these predictions were made, see text of Section 3.

Table 3: Summary Statistics on Reported Crime Incidents

	Sex Offenses	Assaults
Total Number of Incidents in Sample	335,880	4,452,967
Annual Crimes per 10K Population	8.6	122.3
Percent of Incidents with Report Date	19.5%	13.8%
Percent of Incidents Leading to Arrest	25.2%	36.0%
Average Days to Arrest	57.8	9.9
Rape and Sexual Assault	53.9%	<i>n/a</i>
Sexual Molestation	39.4%	<i>n/a</i>
Other Non-Violent Sex Offenses	6.7%	<i>n/a</i>
Offender-Victim Relationship		
Family Member	22.5%	26.1%
Friend	6.9%	2.6%
Significant Other	7.9%	16.3%
Acquaintance	30.3%	22.8%
Neighbor	2.1%	1.7%
Otherwise Known	9.6%	9.4%
Stranger	7.9%	8.8%
Relationship Unknown	12.5%	10.9%
Missing Relationship Information	4.2%	4.6%
Victim Characteristics		
Female	87.5%	57.7%
White	77.5%	68.5%
Black	17.8%	27.8%
Aged 0-4	8.4%	0.6%
Aged 5-9	13.5%	1.6%
Aged 10-14	25.5%	16.0%
Aged 15-19	8.4%	16.3%
Aged 20-29	8.5%	25.2%
Aged 30-39	5.5%	20.1%
Aged 40-49	2.0%	8.9%
Aged 50-65	0.5%	2.5%
Aged 65+	1.8%	1.0%
Offender Characteristics		
Male	95.4%	76.1%
White	68.2%	61.4%
Black	24.4%	33.6%
Aged 0-4	0.2%	0.1%
Aged 5-9	2.2%	0.5%
Aged 10-14	11.2%	6.9%
Aged 15-19	21.6%	17.0%
Aged 20-29	25.8%	30.5%
Aged 30-39	19.3%	24.3%
Aged 40-49	11.7%	14.5%
Aged 50-65	6.3%	5.2%
Aged 65+	1.8%	1.0%

Notes: Sample includes all sex offenses and assaults reported in the 15 NIBRS states included in our analysis.

Table 4: Impacts of Registration and Notification on Crime Frequency and Relationship Mix

	All Victims		"Close" Victims		"Near" Victims		"Stranger" Victims	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Registry Effective	-0.068 (0.031)*	-0.057 (0.034)	-0.030 (0.017)	-0.028 (0.019)	-0.009 (0.012)	-0.006 (0.015)	-0.027 (0.023)	-0.023 (0.019)
Registry Effective * Registry Size	-0.011 (0.003)**	-0.009 (0.002)**	-0.005 (0.001)**	-0.004 (0.001)**	-0.005 (0.002)**	-0.005 (0.001)**	-0.001 (0.002)	0.000 (0.002)
Public Access	-0.012 (0.058)	0.008 (0.041)	0.022 (0.020)	0.028 (0.019)	-0.022 (0.019)	-0.020 (0.016)	-0.013 (0.038)	-0.001 (0.027)
Public Access * Registry Size	0.002 (0.005)	0.001 (0.004)	-0.001 (0.001)	-0.001 (0.001)	0.002 (0.002)	0.002 (0.001)	0.001 (0.003)	0.001 (0.002)
Internet	-0.018 (0.057)	-0.026 (0.037)	-0.031 (0.016)+	-0.032 (0.015)+	-0.004 (0.023)	-0.002 (0.019)	0.016 (0.039)	0.008 (0.028)
Internet * Registry Size	0.005 (0.004)	0.005 (0.003)	0.003 (0.001)*	0.003 (0.001)**	0.002 (0.002)	0.001 (0.001)	-0.000 (0.003)	0.000 (0.002)
Community Notification	-0.271 (0.124)*	-0.176 (0.081)*	-0.106 (0.022)**	-0.074 (0.021)**	-0.076 (0.047)	-0.050 (0.042)	-0.100 (0.069)	-0.092 (0.047)+
Community Notification * Registry Size	0.011 (0.007)	0.007 (0.004)+	0.004 (0.001)*	0.002 (0.002)	0.003 (0.002)	0.002 (0.002)	0.005 (0.004)	0.005 (0.003)
Assaults as Control		√		√		√		√
Observations	176382	176382	176382	176382	176382	176382	176382	176382
R-squared	0.30	0.32	0.16	0.17	0.18	0.18	0.23	0.24

Note: The unit of measurement for the dependent variables is incidents per 10,000 persons, and the unit of observation is a reporting agency (ORI) by month cell. The notification laws represent "full" access by the public to information on offenders; for more details see the text in Section 5. Registry size is empirically estimated from registry data, as explained in the text in Section 3. The regressions control for state and county information, ORI fixed effects, year fixed effects, and month fixed effects, as described in the text. The control for assaults is measured as incidents per 10,000 persons in the ORI-month cell. In Columns 3 to 8, the assaults variable is specific to incidents with the same offender-victim relationship as the dependent variable. Regressions are weighted by the covered population in each ORI. Standard errors (in parentheses) are clustered at the state level. + significant at 10%; * significant at 5%; ** significant at 1%.

Table 5: Impact of Registration and Notification on Arrests

	Probability of Arrest	Probability of Quick Arrest Arrest	Ln(Days to Arrest) Arrest, No Quick Arrest
Registry Effective	0.013 (0.026)	0.015 (0.038)	-0.024 (0.136)
Registry Effective * Registry Size	-0.003 (0.002)	0.011 (0.004)**	-0.008 (0.012)
Public Access	-0.043 (0.021)+	0.024 (0.039)	-0.205 (0.291)
Public Access * Registry Size	0.004 (0.002)*	-0.003 (0.005)	0.014 (0.018)
Internet	-0.009 (0.019)	0.091 (0.037)*	-0.040 (0.230)
Internet * Registry Size	0.000 (0.001)	-0.006 (0.003)*	0.002 (0.014)
Community Notification	-0.121 (0.029)**	-0.066 (0.102)	0.557 (0.277)+
Community Notification * Registry Size	0.008 (0.002)**	0.001 (0.009)	-0.025 (0.018)
Observations	299035	12250	7422
R-squared	0.13	0.36	0.32

Note: The unit of observation is a reported sex offense. The dependent variable in Column 1 is an indicator variable for whether an arrest was made in connection with a reported sex offense. The dependent variable in Column 2 is an indicator variable for whether an arrest was made within 1 day of the report to police, conditional on an arrest being made. The dependent variable in Column 3 is the natural logarithm of the number of days until an arrest was made, conditional on an arrest being made at least two days after a crime was reported to the police. The regressions control for victim characteristics, crime characteristics, state and county demographic information, ORI fixed effects, year fixed effects, and month fixed effects, as described in the text. Standard errors (in parentheses) are clustered at the state level. + significant at 10%; * significant at 5%; ** significant at 1%.

Appendix Table 1: Evolution of Registration and Notification Laws, by State

	(1) reg-eff-date	(2) pubacc- eff	(3) pubacc-disc	(4) pubacc- mand	(5) pubacc- writreq	(6) pubacc- specific	(7) internet- live	(8) comm-eff- date	(9) comm-disc	(10) comm- mand	(11) comm- opt- in	(12) comm- victim	(13) comm- neighbor	(14) comm- media
Virginia	07/01/1994	07/01/1998	N/A	07/01/1998	07/01/1998	07/01/1998	01/01/1999	07/01/2006	N/A	N/A	07/01/2006	N/A	N/A	N/A
Vermont	09/01/1996	05/29/2000	N/A	05/29/2000	N/A	N/A	10/01/2004	09/01/1996	05/26/2006	N/A	09/01/1996 (victim only)	09/01/1996 (if opt-in)	05/26/2006	N/A
Massachusetts	10/01/1996	10/01/1996	N/A	10/01/1996	10/01/1996	N/A	08/03/2004	09/10/1999	09/10/1999	N/A	N/A	N/A	09/10/1999	N/A
Nebraska	01/01/1997	N/A	N/A	N/A	N/A	N/A	03/30/2000	07/15/1998	7/15/1998 (lower risk)	7/15/1998 (high risk)	N/A	N/A	07/15/1998	07/15/1998
Michigan	10/01/1995	04/01/1997	N/A	04/01/1997	N/A	N/A	02/01/1999	01/01/2007	N/A	N/A	01/01/2007	N/A	N/A	N/A
Kentucky	01/01/1995	N/A	N/A	N/A	N/A	N/A	04/11/2000	01/01/1999 ^a	07/12/2006	01/01/1999 ^b	01/01/1999 ^b	01/01/1999 ^b	01/01/1999 ^a	01/01/1999 ^b
Idaho	07/01/1993	07/01/1993	N/A	07/01/1993	07/01/1993 ^c	07/01/1993 ^d	07/01/2001	07/01/2003	N/A	07/01/2003	N/A	N/A	N/A	07/01/2003
Iowa	07/01/1995	07/01/1995	N/A	07/01/1995	07/01/1995	07/01/1995 ^d	03/16/2000	07/01/1998	07/01/1998	N/A	N/A	N/A	07/01/1998	N/A
Texas	09/01/1991	09/01/1995	N/A	09/01/1995	09/01/1995 ^e	N/A	01/11/1999	09/01/1995	09/01/2005 (newspapers)	09/01/1995 ^f	N/A	N/A	09/01/1999	09/01/1995 ^g
Utah	03/30/1983	04/29/1996	03/15/1996 ^d	07/01/1998	03/15/1996 ^d	03/15/1996 ^d	07/01/1998	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Ohio	07/01/1997	07/01/1997	N/A	07/01/1997	N/A	N/A	12/18/2003	07/01/1997	N/A	07/01/1997	07/01/1997 (victims only)	07/01/1997 (if opt-in)	07/01/1997	N/A
Colorado	07/01/1991	06/05/1995	06/05/1995 ^h	07/01/1999	N/A	N/A	07/30/2001	12/01/1999	12/1/1999 ⁱ	05/30/2006	N/A	N/A	12/01/1999	N/A
South Carolina	07/01/1994	06/18/1996	N/A	06/18/1996	06/18/1996	06/18/1996	11/22/1999	06/18/1996	6/18/1996 ^j	6/30/1999 (newspapers)	N/A	N/A	06/18/1996	06/30/1999
North Dakota	07/01/1993	08/01/1995	N/A	08/01/1995	N/A	N/A	11/06/2001	08/01/1995	08/01/1995 ^k	08/01/1997	N/A	08/01/2001	08/01/1995	N/A
Connecticut	01/01/1995	10/1/1998 ^l	N/A	10/1/1998 ^l	N/A	N/A	01/01/1999 ^l	10/01/1995	10/01/1995 ^l	N/A	N/A	N/A	10/01/1995 ^l	N/A

Notes: a: repealed 04/11/2001, reeffective 07/12/2006; b: repealed 04/11/2001; c: repealed 07/01/2001; d: repealed 07/01/1998; e: repealed 09/01/1997; f: repealed 09/01/2005 for newspapers; g: discretionary after 09/01/2005; h: repealed 07/1/1999; i: repealed 05/30/2006; j: except for newspapers as of 06/30/1999; k: repealed 08/01/1997; l: enjoined 05/17/2001 until 05/03/2003.

Columns (1)-(7): (1) the effective date of the first registration law; (2) the effective date of the first public access law of any kind; (3) the date that a discretionary public access law, if applicable, became effective; (4) the date that mandatory public access law, if applicable, became effective; (5) the date on which a "written request" requirement, if applicable, became effective; (6) the date on which "specific person request only" restriction, if applicable, became effective; (7) the date on which public access was moved onto the internet, thereby removing all previous access restrictions.

Columns (8)-(14): (8) the effective date the first active community notification provision; (9) the date the notification law, if discretionary, became effective; (10) the date the notification law, if mandatory, became effective; (11) the date that a notification law that required that people "opt-in" to the notification system, if applicable, became effective; (12) the date that notification law that notified former victims, if applicable, became effective; (13) the date that a notification law that informed neighbors specifically, either by a written notice or by a personal visit, became effective, if applicable; (14) the date that a notification law that used the media to deliver any notification, if applicable, became effective.