Preliminary; Comments Welcome; Please Do Not Cite Without Authors' Permission

Do Sex Offender Registration and Notification Laws Affect Criminal Behavior?

JJ Prescott
University of Michigan Law School
jprescott@umich.edu

Jonah E. Rockoff
Columbia Business School and NBER
jonah.rockoff@columbia.edu

January, 2010

In recent decades, sex offenders have been the targets of some of the most far-reaching and novel crime legislation in the U.S. Two key innovations have been registration and notification laws which, respectively, require that convicted sex offenders provide valid contact information to law enforcement authorities, and that information on sex offenders be made public. Using detailed information on the timing and scope of changes in state law, we study how registration and notification affect the frequency of sex offenses and the incidence of offenses across victims, and we check for any change in police response to reported crimes. We find evidence that registration reduces the frequency of sex offenses by providing law enforcement with information on local sex offenders. As we predict from a simple model of criminal behavior, this decrease in crime is concentrated among "local" victims (e.g., friends, acquaintances, neighbors), while there is little evidence of a decrease in crimes against strangers. We also find evidence that community notification deters crime, but in a way unanticipated by legislators. Our results suggest that community notification deters first-time sex offenders, but may increase recidivism by registered offenders by increasing the relative attractiveness of criminal behavior. This finding is consistent with work by criminologists showing that notification may contribute to recidivism by imposing social and financial costs on registered sex offenders and, as a result, making non-criminal activity relatively less attractive. We regard this latter finding as potentially important, given that the purpose of community notification is the reduction of recidivism.

* We thank Charles Calomiris, Ray Fisman, Brandon Garrett, Matthew Gentzkow, Sam Gross, Jill Horwitz, Amit Khandelwal, Rick Lempert, Justin McCrary, Rob Mikos, Tom Miles, Daniel Paravisini, John Pottow, Doug Staiger, Betsey Stevenson, and Toni Whited for their comments and suggestions, as well as seminar participants at Brooklyn Law School, University of Chicago Law School, Columbia Business School, Harvard Law School, Northwestern Law School, Syracuse University, University of Virginia Law School, the American Law and Economics Association Meetings, the Canadian Law and Economics Meetings, the 2008 Maryland Crime and Population Dynamics Conference, the Conference on Empirical Legal Studies, and the NBER Working Group on the Economics of Crime. Reid Aronson, Erik Johnson, Rembrand Koning, Nicholas Lee, Elias Walsh, Oliver Welch, and Julia Zhou provided excellent research assistance. JJ Prescott gratefully acknowledges the John M. Olin Center for Law & Economics at University of Michigan Law School for supporting this project. Jonah Rockoff thanks the Paul Milstein Center for Real Estate at Columbia Business School for research support.

1. Introduction

Criminal recidivism poses a serious risk to public safety. As many as two-thirds of released inmates return to prison within a few years (BJS (2002a)), and data from the National Corrections Reporting Program show that approximately 40 percent of all criminals sent to U.S. prisons over the last twenty years were already convicted felons. Recently, victims' advocates and others have argued that persons convicted of sex offenses are highly likely to "same crime" recidivate (see Langan et al. (2003) and Vásquez et al. (2008)). Although criminal behavior typically declines steeply with age after the early twenties, the decline for sex offenses may be more gradual (Hanson (2002)). Partly for these reasons, and because of a few high-profile crimes in the late 1980s and early 1990s, sex offenders have become the focus of considerable legislation and public spending aimed at reducing their recidivism.

In the 1990s, two sets of laws targeting sex offenders proliferated across the United States. A federal mandate in 1994 (the Jacob Wetterling Act, named after the victim of a crime in Minnesota) required that states create registries of sex offenders for use by law enforcement. Another federal mandate in 1996 (Megan's Law, named after a victim in New Jersey, Megan Kanka) required that states provide public notification of 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 supervising and apprehending sex offenders who may recidivate and to help local households protect themselves through monitoring and avoiding offenders in their neighborhoods.

Despite the widespread use 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., increasing the probability of arrest). It is also unknown whether sex offenders respond (or are able to respond) to these laws in other ways (e.g., selecting their victims differently). 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 legal sanctions.[1]

[1] Empirical work provides some support for the claim that criminals in general react 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 types of individuals (see McCrary and Lee (2005) on juvenile offenders), and whether these results extend to sex offenders in particular is unknown (see Bachman et al. (1992)).

The first studies that sought to measure the impact of registration and notification laws (Schram and Milloy (1995) and Adkins et al. (2000)) compared recidivism rates of offenders in Iowa and Washington State released just before and after registration and notification laws became effective. While neither study found a statistically significant difference in subsequent arrests for sex offenses between these two groups, both studies relied on small samples of offenders. More recent studies have examined the relationship between the timing of laws' passage and changes in the annual frequency of sex offenses across states using UCR data (Shao and Li (2006), Agan (2007), and Vásquez et al. (2008)). Taken together, these studies find little evidence that these laws had a significant impact on the number of sex offenses.[2]

While we also use the timing of changes in state laws to study the impact of those laws on criminal behavior, we are able to offer new evidence on a number of different questions because our analysis differs significantly from earlier work in both the data we use and the methodology we employ. First, we conducted extensive research into the sex offender legislation of various states, and found that earlier studies had used incorrect legal dates or had mischaracterized these laws. Understanding the timing and scope of this body of law is not easy, partially because sex offender laws have changed over time due to legislative amendments and judicial decisions.[3] We also take advantage of information on the exact dates when laws became effective by using monthly data and allowing for variation in crime frequency within years, in contrast to the earlier work using annual data.

Second, unlike existing work, our analysis distinguishes between sex offender registration and notification laws. Notification laws require the dissemination of information about sex offenders (e.g., criminal history, physical description, home address, etc.). Registration laws, in contrast, require that sex offenders provide such information to a public authority (e.g., local police), but this data is otherwise kept confidential. While registration requirements are intended solely to help law enforcement track and apprehend recidivist

[2] Only Shao and Li (2006) report any evidence that offender registration laws caused a statistically significant reduction in sex offenses. However, their findings are sensitive to empirical specification and they group registration and notification laws together 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.

[3] We describe the history of sex offender registration and notification laws in Section 2 and provide basic information on these enactments in Appendix Table 1.

offenders, notification laws aim both at reducing crime through greater public awareness and by facilitating capture conditional on the commission of a crime (Prentky (1996), Pawson (2002), Levenson and D'Amora (2007)). We also differentiate among various features of notification laws, e.g., access to paper registry, internet access, or proactive community notification.[4]

Third, we use variation in the number of offenders actually registered with authorities to identify different ways in which registration and notification may influence criminal behavior. For example, the main channel by which notification laws are expected to reduce recidivism is by making the public aware of nearby sex offenders, but they may also reduce crime by raising the punishment for first-time sex offenders (whose crimes and personal information will be made public upon release if they are caught and convicted). While notification should have little effect on recidivism when registries are empty, the potential effect on first-time or simply unregistered offenders should be invariant to the number of offenders actually registered.

Finally, we examine the effects of these laws on the relationship mix between offenders and victims in addition to the overall frequency of reported sex offenses. Neither registration nor notification were intended to affect the "incidence" of sex offenses across different types of victims, but some observers have suspected that notification laws might simply displace crime by changing the population of victims targeted by sex offenders (see Prentky (1996), Filler (2001)). We also study changes in the probability that an arrest is made given a reported sex offense, and that the offense reported is not prosecuted, despite arrest, because the victim refused to cooperate or the prosecution declined to pursue the case for some other reason.

We find evidence that actual registration of released sex offenders is associated with a significant decrease in crime. This is in line with predictions from a simple model of criminal behavior in which providing information on offenders to local authorities increases monitoring and the expected punishment for recidivism. Moreover, the drop in the overall frequency of reported sex offenses associated with registration is due primarily to reductions in attacks against "local" victims who are known to an offender (i.e., a family member, friend, acquaintance, or

[4] Shao and Li (2007) do not differentiate between notification and registration laws. Vásquez et al. (2008) state explicitly that they examine 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 include or consider other kinds of notification.

neighbor). Importantly, sex offenses by strangers appear unaffected by registration, indicating little or no substitution from local to more "distant" victims.

We also find that the implementation of a notification law (regardless of the number of registered offenders) is associated with a reduction in the overall frequency of sex offenses. One potential explanation for this effect, again consistent with our model, is that notification raises the punishment for future offenders. Importantly, we find no evidence that notification laws (as opposed to registration laws) reduced crime by lowering recidivism—the estimated effect is actually *weaker* when a large number of offenders are on the registry. This finding is potentially consistent with a number of explanations. But, as we show below, the evidence on balance supports the existence of a significant "relative utility" effect, in which convicted sex offenders become *more* likely to commit crime when their information is made public because the associated psychological, social, or financial costs make crime more attractive.

The rest of the paper proceeds as follows. In Section 2, we describe the variation in the timing and scope of state registration and notification laws. Section 3 lays out the potential effects of registration and notification using a simple model of criminal behavior and presents our basic empirical methodology. Section 4 describes our data. We present our results in Section 5 and robustness checks in Section 6. We conclude in Section 7.

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 painstakingly researched the evolution of these laws in states covered in the 1990s by the National Incident Based Reporting System (NIBRS), the data used in our analysis below.[5] We divide the legal changes we study into four categories: registration, public access, internet availability, and active notification. Registration requires that sex offenders

[5] See the Data Appendix. Determining accurate effective dates proved difficult. When we compare our legal analysis to the dates used by Shao and Li (2006), Agan (2007), and Vásquez et al. (2008), we find a low rate of agreement. Among the 50 states and DC there are only 13 instances (16 instances) in which all studies agreed on the exact date (calendar year) that registration became effective. For example, Utah's first generally applicable sex offender registry became effective on March 30, 1983. Shao and Li use May 19, 1987, a date we cannot locate in legislative history, but which is close to the enactment (as opposed to effective) date of a 1987 law that re-codified and amended the registration law. Agan (2007) 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. Vásquez et al. use the year 1996, when Utah passed a notification law granting public access to registry information.

provide authorities with identifying information upon release from custody or probation. Until notification laws were enacted, this information was held confidential. Police typically increase surveillance of registered offenders and can more easily locate, investigate, and apprehend them. The remaining three categories of laws—public access, internet availability, and active notification—are designed to make information about offenders available to the public, rather than to assist police directly. The public can, in theory, reduce sex offender recidivism by avoiding convicted offenders or by reporting suspicious behavior.

Since the late 1980s and early 1990s, states have typically proceeded from using basic registration laws, to relying on more restrictive and varied forms of public access, to eventually placing all sex offender registration information onto the internet.[6] Many states have also implemented "active notification" regimes.[7] At each stage, states also had to decide whether to make their registration or notification laws retroactive in their application. Retroactivity provisions specify which offenders are covered by the laws in light of the *timing* of their conviction or their release from custody. These provisions allow us to calculate the sizes of state sex offender registries.[8] We use all of this cross-state variation—in exact timing, scope, and registry size—to identify the causal effects of registration and notification laws.

Figure 1a 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 which agencies from each state began reporting to NIBRS. While sex offender laws have evolved in a similar fashion from state to state, Figure 1a demonstrates that the timing and content of these laws varies considerably. For example, Idaho began registration and (limited) public access simultaneously in 1993, but did not have an internet registry until 2001 or

[6] Sex offender "web registries" allow the public to search for offenders using an individual's name or alias, or search for all offenders living near a specific address.

[7] Examples of active community notification include announcing the release or residential moves of sex offenders through notices in local newspapers, by personal visits or letters to former victims or others considered at-risk, and opt-in provisions, which allow citizens to request notification if a certain sex offender or one satisfying certain conditions is released or moves.

[8] For example, Massachusetts' registration law, effective October 1996, required anyone convicted of a qualifying sex offense on or after August 1, 1981 to register. As a result, close to 8,000 offenders were registered when the registry became effective (Boston Globe (1996)). Other states made their laws prospective. For instance, Michigan's registration law, effective on October 1, 1995, required registration of individuals "convicted or released" on or after that date. As a result, when the law became effective, Michigan's registry was empty.

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.

Endogeneity in the timing of these laws is unlikely to be a problem for several 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. Indeed, many sex offender laws are named after the victim who sparked the legislative effort, and sex offense rates (like rates of other crimes) actually declined in the 1990s, the period in which most of these laws were passed. Second, federal laws passed in 1994 and 1996 (motivated in part by specific crimes against individual children in Minnesota and New Jersey) required states to pass registration and notification laws. These federal laws left states with discretion as to substance and timing, but had minimum requirements and imposed deadlines. Finally, the timing of passage was also partly dictated by pre-existing legislative schedules (e.g., the Kentucky, North Dakota and Texas legislatures meet only once every two years) rather than by sex offense trends. [9]

3. Conceptual Model and Empirical Framework

To generate testable hypotheses and to aid in the interpretation of our empirical results, we consider a simple model of criminal behavior. Criminal offenses committed by individual i against victim j are governed by a victim-specific probability of punishment (p_{ij}) and cost of targeting the victim (c_{ij}), a level of punishment if convicted (f_i), and utility from committing crime, relative to legal behavior (u_i) that is invariant across victims.[10] Sex offenses require victims, and 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,

[9] Whether a sex offender law is made retroactive is also unlikely to be endogenous to crime rates. Criminal laws with retroactive features can violate the U.S. and state constitutions. The decision whether to make a law retroactive in any particular state turned in significant part on governing judicial opinions in the state.

[10] This model is adapted from the structure of Becker (1968). The utility term is an analog to Becker's concept of the individual's "willingness to commit an illegal act." The assumption that the relative utility of criminal behavior is invariant across victims may not be true in a strict sense (a particular offender may prefer targeting, for example, a stranger to a neighbor, conditional on actual costs), but any variation can be reinterpreted as a difference in victim-specific targeting costs. In any event, because we focus on *changes* in these terms caused by registration and notification laws, the assumption is not critical to motivating or interpreting our empirical work.

offenses are increasing in the relative utility of crime commission (u_i), and decreasing in the cost of targeting a victim (c_{ij}), punishment probability (p_{ij}), and punishment severity (f_i).

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

Registration and notification laws are likely to influence the number of offenses through several specific channels. Registration may increase the ability of police to monitor and apprehend *registered* sex offenders (RSOs), raising p_{ij} for RSOs and particularly so for local victims. This feature of registration may also affect forward-looking, unregistered individuals because punishment (f_i) now includes a higher future probability of detection.[11] However, so long as registry information remains confidential, registration *alone* should not alter the cost to targeting victims (c_{ij}) or the utility of crime commission (u_i).

Notification—either via public access to registry information, an internet registry, or active community notification—may further affect criminal behavior. First, punishment for sex offenses now includes public airing of one's personal information and criminal history. This publicity has negative consequences for RSOs, including loss of employment, housing, or social ties; harassment; and psychological costs such as increased stress, loneliness, and depression (see Zevitz and Farkas (2000a), Tewksbury (2005), and Levenson and Cotter (2005)). Thus, for *potential* (or otherwise unregistered) sex offenders, punishment severity (f_i) would be higher because any conviction would *newly* subject the offender to this sort of publicity.

In contrast, RSOs already suffer notification costs. While committing another offense may prolong their registration in the distant future—the federal minimum registration period is 10 years—many states have lifetime registration for violent sex offenders, and some states have lifetime registration for all sex offenses. Moreover, the consequences of notification (e.g., difficulty finding employment) 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 increase slightly), while the relative utility of criminal behavior (u_i) would rise.[12]

[11] The registry might also lower the probability of punishment (p_{ij}) for first-time offenders if police shift resources towards monitoring RSOs. Unfortunately, we cannot determine whether this occurs.

[12] Although we conceive of these burdens on offenders as raising the relative utility of criminal behavior, one

In addition, 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 (c_{ij}). Indeed, a major motivation for the passage of Megan's Law was the belief that Megan Kanka would have avoided her fate had her parents been notified of her eventual attacker's presence in the neighborhood. However, it is unclear to what degree notification will reduce the *overall* number of sex offenses—notification may instead "accomplish nothing more than changing the neighborhood in which the offender looks for victims" (Prentky (1996)). In other words, notification may displace crime rather than reduce it.[13]

While we lack information on whether any particular crime in our data was committed by a registered sex offender, the ideas laid out above suggest how the effects of these laws can be identified and distinguished by using variation in the size of sex offender registries. Specifically the effect of *registration* on crime via increased probability of punishment should be small when relatively few offenders are registered, and should grow with the relative size of the registry. The potential impact of registration on the punishment level (f_i) for forward-looking, unregistered individuals, however, would not depend (or depend very little) on the size of the registry. Likewise, *notification* may raise the punishment (f_i) for potential (or unregistered) sex offenders, reducing aggregate crime, irrespective of the size of the sex offender registry.[14] Notification may also have several, offsetting effects on the behavior of registered sex offenders by increasing the cost of targeting local victims (c_{ij}), increasing the probability of punishment for local crimes (p_{ij}), slightly increasing the level of punishment (f_i), and increasing offenders' relative utility of crime commission (u_i). The consequence of these combined effects for overall crime is indeterminate, but it is likely to grow in magnitude with registry size.

---

could also think of them as lowering punishment levels because they make life in prison seem relatively more attractive. Regardless, both effects would increase the number of offenses committed by registered sex offenders.

[13] In addition to raising the cost of targeting local victims, notification may also raise vigilance and knowledge of an RSO's actions within his neighborhood. This "community policing" effect (Lieb (1996)) could increase the likelihood that an offender is apprehended if he attacks a local victim. Again, if the likelihood of punishment only rises for crimes against local victims, offenders may simply offend in other neighborhoods. Crime displacement has been an important consideration in other empirical research on criminals' responses to changes in their environments (e.g., Jacob et al. (2004), Di Tella and Schargrodsky (2004), Iyengar (2007)).

[14] This response may in turn affect aggregate offender-victim relationships if offenders' probability of punishment is correlated with their relationship to victims. For example, the reporting rate to police, and hence the probability of punishment, may be lower for crimes committed against children within families (see Filler (2001)).

To carry out this analysis, we estimate the following reduced-form equation:

$$Crime_{jt} = \alpha_j + \gamma_t + \lambda X_{jt} + \sum_s D_j^s \left(\beta_0 Rg_t^s + \beta_1 Nt_t^s + \left(\beta_2 Rg_t^s + \beta_3 Nt_t^s\right) * RgSize_{jt}\right) + \varepsilon_{jt} \tag{2}$$

$Crime_{jt}$ is a measure of crime frequency (e.g., offenses per 10,000 people) for reporting area *j* in time period *t*. α_j is a reporting area fixed effect to capture any persistent heterogeneity in crime across areas, γ_t is a time effect to capture secular changes in crime over time, and X_{jt} are time-varying reporting area characteristics that are likely to impact crime. Rg_t^s and Nt_t^s are vectors indicating that state *s* had a sex offender registry or a notification law in place during time period *t*, and $RgSize_{jt}$ is a vector measuring the size of the offender registry in area *j* in time period *t*. D_j^s indicates that reporting area *j* is located in state *s*.

β_0 represents the deterrent effect of an offender *registry* on *non*-RSOs and should be negative in sign if the registry increases the punishment faced by unregistered individuals (due to the threat of future registration).[15] A stronger prediction of our model, however, is that β_2 should be negative—an increase in the probability of punishment for RSOs should lower crime by more when there are relatively more RSOs, i.e., when the registry is relatively large because relatively more potential recidivists are registered.[16] β_1 captures the effect of *notification* on potential (or unregistered) sex offenders. We hypothesize that this coefficient should be negative, reflecting

[15] We use the term non-RSO to refer to both first-time offenders and individuals previously convicted of a sex offense but not required to register (e.g., offenders released prior to the passage of a non-retroactive registration law). Also, the identification of β_0—the effect of a registry with no registrants—has no in-sample variation from a few of the states in our analysis. Massachusetts' and South Carolina's registration laws retroactively affected large numbers of released offenders, and Texas and Utah joined the NIBRS well after their registries had begun.

[16] Registry size could, in theory, influence the behavior of *potential* sex offenders if their behavior responded either to contemporaneous registry size or the expected size of the registry in the future. In that case, β_2 and β_3 would capture mixes of deterrence and recidivism reduction. Potential offenders *might* have believed that resources devoted to tracking each offender would grow with registry size, or they might have believed the opposite or nothing. They may have believed a large registry would overwhelm the public with information or reduce the stigma against sex offenders as more offenders were "revealed," or they may have believed that larger registries would attract more attention and more stigma surrounding sex offenders (which is what seems to have occurred). Nevertheless, we know of no evidence that potential offenders considered registry size in making their decisions, and it seems to us improbable. Still, although we interpret β_2 and β_3 as recidivism effects alone in the remainder of the paper, some small portion of the effects we identify could result from non-registered offender behavior.

the greater punishment for unregistered individuals from notification.[17] In contrast, there is no clear prediction for β_3 due to notification's offsetting effects on RSO behavior. A finding that β_3 is negative would indicate that notification reduces the availability of victims and would bolster claims made by proponents of notification—after all, protecting the public from recidivism was the law's intended effect. However, if offenders shift to more distant victims or commit more crime because publicity has made criminal behavior relatively more attractive, our estimate for β_3 could be close to zero or even positive.[18]

We can also use Equation 2 to examine the impact of registration and notification on the distribution of relationships between victims and offenders. Because registration laws apply to sex offenses regardless of the victim-offender relationship, if a law increases punishment for non-RSOs (β_0 and β_1), the effect should be similar across all relationship types. However, with respect to RSO behavior, the impact of a registry should be greatest (β_2 most negative) for offenses against "local" victims. We would expect to find a smaller negative effect with respect to distant relationship offenses or, potentially, a positive effect if offenses are being displaced from local to distant victims (e.g., strangers). How the effect of *notification* on RSO crimes (β_3) should vary across victims is unclear. If the increased cost of targeting local victims is a dominant effect, then a negative effect for local victims and a zero or positive effect for distant victims is likely. However, if the increased relative utility of crime commission is the dominant effect, then we might see a positive effect across all victims.

---

[17] The identification of β_2 relies to a greater extent on out-of-sample variation because only a few of the NIBRS states passed a "full notification" statute at the same time as their registration law, and most states did not do so until several years after their registration law became effective. Nevertheless, our data does include a considerable number of observations in which the number of registered offenders per 10,000 persons is quite low relative to the sample mean. For example, the 10th percentile registry size among observations with a full notification law in place is about 6 offenders per 10,000 persons, and the 10th percentile within each state is at or below 6 per 10,000 in nine of the fifteen NIBRS states. Our estimates, presented below, suggest that a notification law with this low level of registration would still have a negative and statistically significant effect on crime frequency.

[18] One concern with the use of registry size is the potential for reverse causation, given that registry size will be influenced by the number of sex offenses committed in the past. In practice, however, registry size is primarily a function of 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. Additionally, the lag with which new offenders are added to the registry is likely to be quite long. For individuals sent to prison in 2002 whose first listed offense was Rape, Sexual Assault, and Child Molestation, the median sentence length was 120, 72, and 68 months, respectively, and the fraction with a sentence of one year or less was 1.7, 2.5, and 1.8 percent, respectively (authors' calculations, 2002 National Corrections Reporting Program data). Thus, only a very small amount of registry size growth is due to recent convictions, and any short-run change in the frequency of sex offenses driven by other factors is unlikely to be correlated with short-run changes in registry size.

When examining arrests and dropped cases to interpret the crime frequency results, we use incident-level data to estimate similar equations. For example:

$$Arrest_{it} = \alpha_j + \gamma_t + \lambda X_{it} + \sum_s D_i^s \left(\delta_0 Rg_t^s + \delta_1 Nt_t^s + \left(\delta_2 Rg_t^s + \delta_3 Nt_t^s\right) * RgSize_{it}\right) + \varepsilon_{it} \tag{3}$$

Subscript i denotes an incident in reporting area j. Incident-level variables (X_{it}) include details about the crime known to police (e.g., characteristics of the victim, offender, and their relationship, the offense type, the number of victims and offenders involved, etc.).

When predicting the effect of registration and notification laws on arrests, both police and offender behavior are relevant. A registry or a notification law should not influence the likelihood of arrest for non-RSOs (δ_0 and δ_1) through changes in police behavior because police have no additional information about these individuals.[19] However, if the punishment level (f) rises, potential offenders may offend less, substituting away from marginal victims where the probability of punishment (p) is relatively high—suggesting that any effects on the likelihood of arrest (δ_0 and δ_1) should have the same signs as on offense frequency (β_0 and β_1).

The predicted effect on arrest rates for RSO crimes is also unclear. The direct effect of the registry (δ_2) should be to increase arrest rates, reflecting increased monitoring and apprehension. However, RSOs may respond by forgoing offenses against victims where arrest is relatively more likely. Therefore, the effect on arrests should be positive if there is no change in recidivism, but small if RSOs offend less or shift toward victims where the probability of punishment has not risen. A similar analysis applies to notification. A direct effect (δ_3) on RSOs via “community policing” should increase the arrest probability. However, if local victims use notification to avoid RSOs, increasing targeting costs (c), offenders will only attack local victims with low probabilities of punishment, and average *observed* arrest rates will drop. Notification may also increase the number of offenses by increasing the relative utility of crime commission (u). This would increase arrest probabilities, theoretically, as offenders victimize those for whom the probability of punishment was previously too great.

[19] Importantly, if monitoring RSOs or responding to citizen complaints about RSOs becomes so expensive or time consuming that the police are not able to investigate crimes or perform other activities (e.g., foot patrols) likely to deter crime, then non-RSOs would eventually perceive the effects of these changes and might react by committing more crime.

Thus, the impact of these policies on arrest probabilities must be interpreted in light of their impact on overall crime frequency. In general, our analysis of arrest data (and of dropped cases) aids in verifying the robustness of our offense frequency findings. Table 1 gives the predicted relationships between the registration and notification variables in Equations 2 and 3 and the model parameters and outcomes of interest.[20]

4. Data

NIBRS, our primary data source, is a part of the FBI's Uniform Crime Reporting Program (UCR), but presents several opportunities for research that are unavailable with standard UCR data. First, 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, we are able to examine effects on the relationship mix between offenders and victims (or the "incidence" of sex offenses) and on the ability of police to secure an arrest for a reported crime. Second, the timing information of each incident is superior in the NIBRS data, allowing us to better exploit within-year variation in the timing of sex offender laws to identify our results. While UCR data are available by month, the UCR date reflects when an incident was reported, whereas NIBRS reports the date the incident occurred.[21]

NIBRS does suffer from significant limitations, however. First, like most crime data, NIBRS only contains information on incidents recorded by police. Changes in reported crime may be driven by true changes in victimization or by changes in reporting. We return to this issue when interpreting our findings. Another limitation is that NIBRS data only started to become available in the 1990s, and only a subset of states participates in the program. In 1995—

[20] We also examine "dropped cases" following arrest that are due to non-cooperation by victims or decisions by prosecutors not to pursue a case. These variables are available in the NIBRS and are of interest to us, but are not part of our model and so we leave discussion of these issues until we present our results below.

[21] If the date of occurrence is not known to the police—this occurs for about 20 percent of sex offenses—then NIBRS reports the date on which the crime was reported. Unfortunately, NIBRS does not report both dates, so we cannot directly measure the lag between occurrence and report. However, we can get a sense of the gap between incident and report dates by exploiting the fact that a subset of crimes reported in NIBRS took place in a prior calendar year (i.e., some crimes that occurred in year *T* are reported in the data from year *T+1*). We examine all sex offenses (excluding 2005) by the calendar month in which they took place and measure the fraction reported in the following year. Of the sex offenses that took place in December, 11 percent were reported the year after, while the corresponding figures for November, July, and March were 7, 2 and 1 percent, respectively. Thus, most crimes are reported within a few months after they take place but a non-trivial fraction is reported with a considerable lag. In any event, our qualitative results are not sensitive to dropping crimes for which an incident date is unavailable.

one year after the federal government required that states create sex offender registries and one year before it required the registry information to be public—there were just nine states; by 1998, eight more states had joined, and 30 states were included as of 2004. Our analysis focuses 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] Third, within each state, the number of reporting agencies (or ORIs) generally increases over time. We include ORI fixed effects in all of our regressions. Thus, in addition to taking account of the growth in the number of reporting agencies over time, we also control for persistent heterogeneity in ORI characteristics.[23]

In NIBRS, multiple offenses can be reported in a single incident, and so we classify an incident as a sex offense if any of the reported offenses fell under one of three sex offense categories: rape and sexual assault, sexual molestation (called "forcible fondling" in NIBRS), or other non-violent sex offenses (i.e., "incest" and "statutory rape").[24] We also create variables for the relationship between victims and offenders by dividing 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 offenders "otherwise known," 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 (which is distinct from "missing").

[22] These states are geographically diverse, but they do not include any states from the far west (e.g., California) or the "deep" South (e.g., Mississippi). Tennessee and West Virginia also joined the NIBRS in 1998. However, both had passed registration and notification laws by that time, so we did not pursue them further.

[23] Although the NIBRS surveys ORIs on a monthly basis, an ORI may not complete every report in a year. We exert considerable effort to ensure that our results are not driven by reporting errors, a full accounting of which is given in the Data Appendix.

[24] Incidents of other crimes are used in our analysis to control for other time varying factors that cause changes in crime rates within an ORI over time. We classify non-sex crimes as either ordinary assault or "other" crime, in order to control for overall rates of crime and a type of violent crime arguably more similar to sex offenses. We classify an incident as an assault if one of the offenses listed fell under an assault category but none of the offenses was a sex offense. This latter condition affected only a small number of incidents: only 0.3 percent of incidents with a sex offense also had an assault and just 0.02 percent of assaults also had a sex offense. Likewise, incidents of "other" crime do not contain either a sex offense or an ordinary assault.

Table 2 shows summary statistics on the sample of incidents we examine.[25] For purposes of comparison, we also include information on ordinary assaults. Assaults are more common than sex offenses, with more than 14 assaults for every sex offense. 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 (19 vs. 13 percent). Arrests are less common for sex offenses (26 vs. 37 percent) and the time to arrest—conditional on the arrest occurring at least one day after the incident—is considerably longer (24 vs. 14 days).

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 (reported) incidents with an acquaintance is somewhat higher for sex offenses (31 vs. 24 percent) but incidents of sex offense are less common between all family members (26 vs. 30 percent) and significant others (8 vs. 18 percent). For both sex offenses and ordinary assaults, in about 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 51 percent of sex offense victims were below age 15, the corresponding figure for assault is only 9 percent. Sex offense victims are also more likely to be female (87 vs. 58 percent) and white (78 vs. 68 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 (96 vs. 77 percent) and somewhat more likely to be white (69 vs. 62 percent).

Finally, we classify each incident based on the laws in effect and the number of offenders on the registry at the time the crime took place. When analyzing data aggregated to the reporting agency-month level, our legal variables reflect the law as of the 15th day of the calendar month. While complete historical data on the size of registries is unavailable (particularly for early years), we have gathered an extensive amount of information on state registry sizes at various points in time. Under the assumption that registries grew smoothly over time, we can use this

[25] Additional data detail can be found in Appendix Table 2, which provides summary statistics on the aggregated (to the ORI-month level) NIBRS data, the legal data, and the county and state demographic data we use in most of our empirical work below.

data to estimate how registries evolved during our sample period. Details on this estimation procedure are given in the Data Appendix.

5. Crime Frequency and Relationship Mix Results

We estimate the effects of registration and notification laws using the regression specifications outlined in Section 3. All regressions include ORI fixed effects, year and month fixed effects, and control for annual per-capita income, unemployment, and poverty rates and the fraction of the population in five ethnicity categories and five-year age categories at the county level. In addition, for some specifications, we include the number of ordinary assaults and of other crimes committed per 10,000 persons as proxies for ORI-specific time-varying factors that influence crime rates and may be correlated with the legal variables. Though we do not report the coefficients, both assault rates and "other crime" rates are always positively related to sex offenses and are highly statistically significant.[26]

The registry indicator signifies that the state has an active offender registry, and registry size is measured using our empirical estimates, as explained in the Data Appendix.[27] For notification, recall that there are three types of notification: public access, internet access, and community notification. Within these categories, we focus on statutes that implied widely available or "full" public access to sex offender information, meaning access was not subject to the discretion of local authorities and where the public could inquire about local offenders in general, as opposed to a specific person inquiry. Full internet access indicates that the internet

[26] Including assault and other crime may be problematic in that these may also be affected by sex offender laws, depending on their substitutability/complementarity in both their commission and reporting. However, their inclusion turns out to have little influence on our results and, if anything, decreases the size of our estimated coefficients. We therefore view them as appropriate controls for time-varying unobservable factors.

[27] Internet and active notification laws do *not* necessarily apply to all registered sex offenders. In our sample, as of 2008, seven of the fifteen states we study listed 100% of their registered offenders on their web registry and ten listed more than 95% of all registered offenders. However, five states listed significantly less than 95% (specifically, Colorado, Iowa, Massachusetts, Nebraska, and North Dakota) on their internet registries. Overestimating the size of the (internet access and active) notification-relevant registry should only affect our estimates of the coefficient on the interaction of relevant notification provision with registry size and, if anything, the overestimate should bias these results toward zero (making our estimates reported below a lower bound). To verify this logic, we ran regressions using a rough proxy—the ratio of individuals on a state's web registry to the total number of sex offenders registered—to calculate a separate registry size for the interactions with an internet site and active notification. As predicted, the estimates (discussed below) grew in magnitude and retained similar statistical significance levels. These results are available from the authors upon request.

registry is on-line and generally complete.[28] Full community notification means a law that makes notification mandatory and requires either neighbors or the media be provided with sex offender information. Figure 1b shows the timing of the effective dates of these laws. In our regressions below, we define having a notification law to mean that at least one of these “full” versions of notification is in place and effective.[29]

The unit of observation in our analysis is an ORI-by-month cell, and the dependent variable is measured as *annualized* incidents per 10,000 persons covered by the ORI (i.e., we multiply monthly incident rates by 12, for ease of interpretation).[30] 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 account for likely heteroskedasticity.[31] Finally, because our sample includes a small number of states and our registry size variable is empirically estimated, using standard OLS estimates of standard errors or even clustering by state can lead to incorrect statistical inference (Donald and Lang (2007), Cameron et al. (2007), Murphy and Topel (1985)). To correct our standard errors for both of these problems, we use a bootstrap procedure, outlined in the Data Appendix.

[28] We located news articles in six states suggesting that the internet registry was incomplete when launched, i.e., it was missing information on a large share of registered offenders. For two of these states, we found notice of when the web registry was completed. For the four 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.

[29] Given the limited number of states and the fact that notification laws are designed to work in a similar fashion—lowering information costs and increasing dissemination—our primary specification uses any full notification law in effect. One complication that arises from this framework is that two states in our sample (Texas and Ohio) had registration and notification laws in place prior to the start of the NIBRS data period. Thus, variation in crime frequency within these states does not contribute to the identification of the main effects of these laws. Dropping these states from our sample has very little impact on our results, and we report replications of our main results with a restricted 13 state sample in Appendix Table 3.

[30] Studies of crime frequency often examine the natural log of crime as a dependent variable in regression analysis (see, e.g., Shao and Li (2007)). This transformation is problematic in our case because we use monthly data from very disaggregate areas and therefore have many observations in which zero sex offenses occur. However, for comparison purposes, we also estimate regressions where the dependent variable is the natural log of offenses plus one per 10,000 persons. The results from these specifications are quite similar in sign and significance to our measure of crime per 10,000 persons, and are available upon request. The similarity of the results is not surprising, given that we weight our analysis by covered population and thereby rely more on larger areas that are unlikely to have months without the occurrence of at least one sex offense.

[31] 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.

Our results for the overall frequency of sex offenses are shown in Table 3. We find no evidence that registries deter first-time sex offenders. Specifically, the impact of an (empty) sex offender registry is estimated to be positive, but imprecisely estimated. Importantly, however, we do find that requiring registration reduces *recidivism*, presumably by increasing monitoring and the likelihood of punishment for potential recidivists. The interaction of the registry indicator with the size of the registry is negative and statistically significant, as predicted by our model. The estimate in column (2) of -0.096 implies that each additional sex offender registered per 10,000 people reduces reported annual sex offenses per 10,000 by 0.096 crimes. This is a substantial (1.1 percent) reduction and, if correct, would support placing information about offenders in the hands of local law enforcement to combat recidivism.

Notification laws also appear to affect the frequency of sex offenses. The estimates in Table 3 suggest that notification makes a noticeable difference in criminal behavior, but not in the way that proponents of these laws intended. The estimated effect of the *existence* of a notification law on the frequency of sex offenses is negative and statistically significant. If our identifying assumptions are correct, the estimate in column (2) suggests that notification laws reduce crime frequency by -1.05 crimes per 10,000 persons per year (about 11.5 percent) via a deterrent effect on individuals not currently registered as sex offenders. But the interaction of notification with registry size is positive and statistically significant. In the context of our model, this implies that any beneficial impact of registration on recidivism is dampened by the use of notification. This also suggests that the punitive aspects of notification laws may create perverse effects (as discussed in Section 3). A basic trade-off may exist in the sex offender notification context—while some first-time offenders are deterred by notification sanctions, the imposition of those sanctions on convicted offenders ex post *may* make them more likely to recidivate.[32]

Assuming our evidence is accurate and our interpretation is valid, how should a legislature approach this trade-off? For a simple back-of-the-envelope analysis, imagine a state

[32] Another possible explanation for the increase in crime frequency associated with an increase in registry size is that either the state authorities or citizens become overwhelmed with the number of offenders as the registry size grows. This strikes us as unlikely. First, with respect to the police, we continue to see a reduction in the number of offenses as the registry grows under a registration regime, suggesting that, although surely costly, police are not being overwhelmed to the point where an additional registrant actually reduces the overall effectiveness of the system. Second, notification regimes are primarily local. Therefore, most of the increase in registry rolls amounts to an increase from zero registered offenders to two or three in a neighborhood or zip code. Citizens are not expected to track thousands of offenders, and, indeed, notification systems are not designed to work that way.

that must decide 1) whether to enact a registration law, 2) whether to enact a notification law, and 3) how many offenders to cover with these laws. We estimate that an empty registry has an insignificant positive effect on annual sex offenses (0.24 per 10,000 people, p-value 0.62). However, actual registration of offenders (absent notification) appears to reduce crime. For example, the estimated total effect of registration with an average-size registry (16 offenders per 10,000 people) is a reduction of 1.3 sex offenses per 10,000 people (p-value 0.07). On the other hand, notification laws appear to be attractive only when the size of the registry is relatively small. We estimate that implementing a notification law deters 0.64 yearly sex offenses per 10,000 people (p-value 0.04) for a registry with 4.7 offenders per 10,000 persons (the 10^{th} percentile of registry size). In contrast, combining a notification law with a registration law when the registry is of average size has an insignificant *positive* effect on sex offenses (0.34 per 10,000 people, p-value 0.33), though the overall impact of the combined laws is still a reduction of 0.94 yearly sex offenses per 10,000 people (p-value 0.18).

If a state uses both registration and notification, the level of coverage, assuming equal coverage, turns out to be somewhat unimportant if the only criterion is the total number of crimes committed. This is because the notification interaction coefficient is similar in magnitude to the registration interaction coefficient, and the difference is not statistically significant. As a result, our data do not indicate that a larger registry—when combined with notification—reduces crime. In fact, a larger registry may increase overall sex offense frequency. Given the significant social and individual costs of maintaining a large registry, one possible implication of these estimates is that states should consider narrow notification regimes, in which all or most sex offenders are required to register, but only a small subset of those offenders are subject to notification.[33] Alternatively, states might consider notification substitutes capable of similar deterrence gains, but that avoid notification-related recidivism. Because notification laws were enacted to reduce recidivism, not to deter, our results suggest a reevaluation of notification requirements may be necessary.

[33] As noted above, one-third of the states in our sample do not apply their most stringent notification requirements to all RSOs. Unfortunately, our empirical strategy precludes our saying anything about whether these states are closer to a crime-minimizing mix of sex offender policies. Of course, in the context of our model, reduced application of the stringent notification provisions may reduce their *deterrent* effect as well.

We also estimate regressions in which we disaggregate our previously singular notification measure into three different types of notification regimes—full public access, full internet access, and full active notification.[34] As one would expect, disaggregating results in less precision, and yet the basic pattern of results is unchanged. The coefficients on all three types of notification laws and their interactions with registry size have the same sign—negative main effect, positive interaction—and the standard errors are too large to reject the hypothesis that they are equal. Nevertheless, it is worth noting that the coefficient on the main effect of active community notification is noticeably larger in magnitude, implying greater deterrence of first-time offenders in the context of our model. The strength of the active notification result makes sense, as active notification is perhaps the most intrusive form of notification and therefore may have particular deterrence value.

In Table 4, we investigate the extent to which registration and notification laws may have affected the relationship mix of offenders and victims. As noted in Section 4, we divide victims into three groups based on their relationship with the offender: “close,” “near,” and “stranger.” 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. Accordingly, we examine whether (as lawmakers hoped) the frequency of crimes against 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.

To interpret our results and understand the drawbacks of our approach, it is important to recognize that the NIBRS relationship variable has one geographic element (neighbor), but is generally organized by the level of familiarity the victim has with the offender (e.g., friend, acquaintance, etc.) or by family status (e.g., spouse, sibling, etc.). For our purposes, we are interested in the effect of sex offender laws on crime in those relationships where the existence of a notification regime would help a victim to learn of a potential offender’s status or where registration would help the police interfere with a crime or apprehend an offender.

For notification-based recidivism reduction, our model suggests that the “neighbor” and “acquaintance” relationships, although they are both under- and over-inclusive, are the most

[34] These results are available from the authors upon request.

susceptible to change. Family members (spouses, siblings, etc.) presumably already *know* of an offender's status (although perhaps not in every case—e.g., step parent), and strangers (who are not neighbors, by assumption) would have less reason to learn of an offender's status. Moreover, web registries require either a request about a specific person or a localized search around an address (i.e., a neighbor) and active notification regimes typically notify only neighbors (those within a specified radius of an RSO's address) or former victims. Both facts decrease the likelihood that individuals would have significant knowledge regarding offenders who do not live nearby and with whom they do not have any personal connection.

Thus, a reduction in recidivism due to notification should primarily reduce the frequency of offenses against "near" victims (neighbors, acquaintances, or "otherwise known"), *even though* the categories that make up "near" victims are a hodgepodge of geographic and familiarity considerations. Similarly, under registration, our model indicates that RSOs may be more easily monitored (both around their families and in their neighborhoods) and more easily identified as suspects when a nearby crime occurs. Therefore, we predict that a registration regime is most likely to impact "close" and "near" offenses.

The results of our incidence analysis support these predictions and the interpretation of the results presented in Table 3. According to our model, the deterrent effects of registration and notification laws should not alter the relationship mix of sex offenses because, by definition, first-time offenders are not currently registered (so neighbors, for example, cannot protect themselves). The results in Table 4 are consistent with this prediction—notification has a deterrent effect that is, percentage-wise, similar in magnitude across relationship groups, although the estimate for strangers is not precisely estimated. In any event, there is no evidence that the effect differs across groups, and, for all groups, the estimated coefficients on the indicator for having a registry are not statistically significant.

However, consistent with the hopes of policymakers, the interactions between the registration law indicator and the size of the registry are negative and of similar magnitude for both the close and near victim groups. In contrast, the estimated interaction for the stranger group is slightly positive, and, though statistically indistinguishable from zero, fairly precisely estimated. The effects in Table 4 for "close" and "near" victims are marginally significantly different from zero (p-values of 0.15 and 0.10, respectively), consistent with the idea that registration helps reduce crime by local offenders against local victims or family members. The estimates imply that each additional registered sex offender per 10,000 persons reduces these group specific sex offense rates, in total, by 0.063 per 10,000 persons. Registration of sex offenders with law enforcement does not appear to reduce crimes committed against more distant individuals, but the estimated coefficient in column (4) also does not indicate an increase in sex offenses against strangers due to displacement, as some critics feared might happen.[35]

Our model provides an ambiguous prediction for the effect of the interaction between the notification law indicator and the size of the registry. If notification laws make it more costly for a sex offender to target local victims (raising c_{ij}), then we should see negative effects on the frequency of sex offenses for "near" and "close" victims, but less of a reduction or even an increase (if there is displacement) for stranger victims. Alternatively, if notification laws instead primarily reduce the relative utility of legal behavior for RSOs—by making life outside of prison less attractive—crime might increase as the registry grows. Furthermore, if notification laws do not alter the relative cost of attacking certain victims, crime increases should be similar across groups. Table 4 favors this last scenario. The notification-registry size interaction is positive and statistically significant across all groups, and, as percentages, the increases are almost identical (with stranger crimes increasing by 0.77 percent, while crimes against close and near victims rose by 0.90 percent and 0.69 percent, respectively). Thus, notification may serve as a deterrent against non-registered offenders, but may be less effective at reducing recidivism by prompting local victims to protect themselves. In fact, a relative utility effect, one that increases recidivism of sex offenders subject to notification, seems plausible.

The estimated effects of registration and notification laws on various arrest variables for all sex offenses are shown in Table 5.[36] Neither of the arrest variables shows a statistically

---

[35] We have also conducted an analysis that disaggregates "close" relationships into "family" and "non-family" (i.e., significant others and friends). The results for both groups were similar in sign and statistical significance to the results for all "close" relationship crimes.

[36] For analysis of arrests and dropped cases, as noted in Section 3, we examine incident-level data instead of ORI-month aggregates. We also drop the controls for assaults and other crimes (which are aggregate statistics) and include incident-specific variables in addition to victim and offender age indicators, victim and offender sex and ethnicity indicators, indicators for the type of offender-victim relationship, indicators for the number of victims and the number of offenders (capped at four), and indicators 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 covariates 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.

significant relationship with either type of law. If the decrease in crime frequencies associated with registration was indeed caused by increased probability of punishment, the response by offenders to this changed probability must undo (in equilibrium) any detectable change in arrest probability and in the time to arrest. The notification coefficients show the same pattern. A notification law may reduce the number of crimes, but does not appear to increase the probability of arrest. The coefficient on the interaction of notification with registry size is also statistically insignificant. The estimate is positive, as would be predicted by a change in crime due to a relative utility effect, but it is also very imprecisely estimated.[37]

At a minimum, our analysis provides evidence to support the claims of those who argue that registration and notification laws matter. Registration laws seem to reduce recidivism, and notification laws appear to deter those not currently registered. Our work also suggests that notification laws may harden registered sex offenders, however, making them more likely to commit additional sex offenses, perhaps because criminal behavior is relatively more attractive for registered sex offenders living under a notification regime.

6. Robustness Checks

In this section, we address possible concerns about our results and our interpretation of them. We inquire first into the possibility that our results are due to some omitted variable or trend by checking whether our identification approach generates similar results for non-sex related crimes. We also discuss the robustness of our specification and sample choice. Second, we consider whether our results, especially with respect to the "relative utility effect," might be driven by changes in reporting behavior. We cannot completely rule out this possibility, but we present a number of findings that seem inconsistent with this explanation.

[37] With respect to dropped cases, we find little evidence that the probability a victim would not cooperate was associated with the registration and notification variables. Looking at the probability that the prosecution decides not to prosecute someone for lack of evidence, we find suggestive evidence that this occurred more frequently in areas with a registry in place but few registered offenders, but less frequently as the number of registered offenders rose, at least until the advent of notification. This result could make sense. For example, prosecutions may suffer at the start of a registration regime as police personnel are used and criminal justice resources are spent on the construction and introduction of the registry, but as the number of registered offenders rises, police may have access to more and better information about local offenders, leading to stronger average cases. Later, with the arrival of notification, prosecutors may find that the advantages of registration information to the state became degraded when community members also had this information, perhaps because of false accusations (to which we will return). The evidence here, however, allows only speculation.

6.1. Falsification Tests and Specification Checks

One concern with our basic results is that there may be changes in crime frequency due to other unobservable factors correlated with registration and notification laws. We control for a number of local economic and demographic variables and for contemporaneous crime, but our empirical strategy may still suffer from omitted variables bias. To increase our confidence that there is indeed a relationship between sex offenses and registration and notification laws, we repeat our analysis on the overall frequency of other types of crime that we believe are far less likely to be affected by the criminal behavior of sex offenders or individuals on the margin of committing a first sex offense. We expect these "placebo test" estimates to show no statistically significant relationships between the laws and crime frequencies.[38]

We selected auto theft, drug offenses, fraud, weapons violations, forgery, and larceny because, while our intuition is that these crimes are quite different from sex offenses, many of them occur with roughly similar frequency. Of the 24 coefficients we estimate, we find only two statistically significant relationships between reported crime rates in these non-sex offense categories and our registration and notification variables (which might easily occur randomly), and neither of these coefficient estimates mimics our basic results.[39] More importantly, the signs and magnitudes of all of the coefficients increase our confidence that our earlier results are not driven by spurious correlation with general trends in crime. For example, the coefficients on registry size are all positive or very close to zero in these specifications.

Another concern when dealing with relatively few states in a quasi-experimental setting like ours is that one large state might plausibly account for all of the relevant results (see Currie

[38] According to our model, the relative utility effect generated by notification might cause registered sex offenders to commit more crime in general. In that sense, with respect to the interaction of notification laws and registry size, this exercise should not be viewed as a falsification check in the usual sense. However, we purposefully selected a number of comparison crimes that are very different from sex offenses to reduce the likelihood that changes in the behavior of registered sex offenders would show up in their overall frequencies. Released sex offenders do commit many other types of crime besides sex offenses. For example, among all sex offenders released in 16 states in 1994, subsequent arrests for auto theft occurred half as frequently as arrests for another sex offense (Authors' Calculations, data from ICPSR Study #3335 "Recidivism of Prisoners Released in 1994"). However, while released sex offenders are far more likely to be arrested for sexual offenses than other released criminals, they are considerably less likely to be arrested for the crimes we consider here. Given that released sex offenders constitute a relatively small portion of all released criminals, it seems likely that the portion of incidents committed by sex offenders in these "placebo" categories is very low.

[39] For weapons crimes, there is a significant negative coefficient on the indicator for whether a registry is effective, but we previously found no evidence that the existence of a registry alone had any effect on sex offense frequency. Registry size also has a significant effect on the number of larcenies, but the sign is wrong.

and MacLeod (2007)). We check the robustness of our findings to this possibility by running a series of regressions in which one state is dropped from the sample. We find that the estimated coefficients remain quite robust to the exclusion of each state.

Our analysis controls for ORI effects, month and year effects, local economic and demographic characteristics, and for contemporaneous crime trends, but it remains possible that our approach attributes the consequences of some unknown trend to our registration and notification variables. Up to this point, we have not included state-specific trends in our analysis because one of our key variables—registry size—closely approximates a state trend (see Appendix Figure 1). By including a state trend as a control variable, we effectively throw away a great deal of information in an attempt to account for a possible, but entirely hypothetical and unknown state-level linear trend. Nevertheless, even though we view our earlier results as more reliable, estimating such specification is a useful exercise.

In Table 6, we present our original results, along with the results from a specification that includes state-specific linear trends (in columns (3) and (6)). Our findings are fairly robust to the inclusion of the trends. The magnitude of the direct notification effect drops, but remains marginally statistically significant (p-value of 0.16).[40] The coefficient on registry size is smaller but similar in magnitude relative to the other specifications and marginally significant (p-value of 0.13). The interaction of notification and registry size remains highly significant regardless of the specification. The inclusion of linear state-specific trend controls thus changes the picture slightly, but, in most important respects, the basic results from Table 3 remain intact.

Equation 2 assumes that the effects of registration and notification laws on RSO behavior (but not non-RSO behavior) increase linearly with registry size. This could be problematic for at least two reasons. First, notification regimes with very large registries might overwhelm potential victims, making it less useful than a narrow notification regime. Second, and more likely, a large registry could stretch police resources, making registration and notification less effective (and even counterproductive), at least once a registry reaches a certain size. We believe these effects are unlikely in practice. The maximum registry size in our sample is less than 40

[40] It is worth noting that the coefficient on notification is statistically significant at the 5% level when we restrict our sample to the 13 states that actually passed a notification statute during our sample period. With the addition of state-specific linear trends as controls, it makes less sense to include the two states (Texas and Ohio) for which there is no legal variation in registration and full notification during our sample period.

per 10,000, while the average number of police per 10,000 people in the U.S. during this time period was about 20 (Evans and Owens (2007)). However, to explore these possibilities, we estimate a specification similar to Equation 2 in every way except for the inclusion of two new quadratic terms in registry size:

$$
\begin{aligned}
Crime_{jt} = \alpha_j + \gamma_t + \lambda X_{jt} + \\
& \sum_s D_j^s \left(\beta_0 Rg_t^s + \beta_1 Nt_t^s + \left(\beta_2 Rg_t^s + \beta_3 Nt_t^s \right) * RgSize_{jt} \right) + \\
& \sum_s D_j^s \left(\left(\beta_4 Rg_t^s + \beta_5 Nt_t^s \right) * RgSize_{jt}{}^2 \right) + \varepsilon_{jt}
\end{aligned}
\tag{4}
$$

The results (available upon request) indicate that our prior specification is appropriate. The quadratic coefficient estimates are extremely small and statistically insignificant, while the estimates for the main effects of registration and notification and the linear interactions with registry size are nearly the same in magnitude and statistical significance.

6.2. Reporting Behavior

Like most studies of criminal behavior, we can only examine the frequency of *reported* crime, and it is therefore possible that our findings are affected by changes in victim reporting behavior. Reporting may be a particularly important issue for sexual offenses; data from the National Crime Victimization Survey (1996-2005) indicate that less than 50 percent of rape and sexual assault victimizations are reported to the police.

Sex offender notification laws may reduce reporting by victims who have a personal relationship with the offender and consider the law to be an overly harsh new punishment. But this seems unlikely to be the entire story, given the effects we find for neighbors and acquaintances, and that we find a decrease in crime associated with registration alone.[41] Registration and notification laws may also cause offenders to move away from family, friends, and acquaintances due to a “shaming” effect. Thus, a decrease in crimes against these groups could be explained by reduced contact with offenders. However, it seems more likely, *a priori*, that this type of mechanical change in the relationship mix of reported offenses would be greater

[41] Changes in reporting behavior might also lead us to underestimate the effects of registration on recidivism. For example, if registration leads victims to believe that their reports are more likely to lead to an arrest, they might be more likely to report crimes. We do not regard this story as likely. While victims may know of a registry’s existence, they are probably unlikely to know whether their assailant is registered. However, the estimated effect of simply having a registry, while positive, is never statistically significant.

for community notification than registration, which does not match our findings. Finally, a community notification law could decreases sex offenses regardless of the number of registered offenders if the law increases awareness of sex offenses in general and makes all potential victims more cautious. Unfortunately, we cannot test this hypothesis directly.

More importantly, our finding that the interaction of notification and registry size is associated with an increase in crime could be generated by two different plausible changes in reporting behavior. As a registry grows and an increasing number of individuals are notified that a sex offender lives nearby, there could be both (1) an increase in the *frivolous* reporting of sex offenses because the proximity of a sex offender is made known and salient and (2) an increase in the number of *true* reports for crimes that would otherwise have gone undetected. Both of these hypotheses are possible, but we find little evidence to support them in the data.

If frivolous reports of sex offenses had increased, holding offender behavior constant, we would expect to see either a reduction in the likelihood that a report led to an arrest (because the average case reported has, all else equal, less merit) and/or an increase in the probability that a case was dismissed by the prosecutor, because the average case became weaker. In our analysis reported in Table 5, we found that the estimated interaction of notification with registry size was positive for the prosecutorial dismissal rate but it was also positive and of the same magnitude for the arrest rate. Thus, it is hard to reconcile our results on the interaction of notification and registry size with a simple story about reporting bias.

We can also use the fact that shifts in reporting behavior may affect different types of sex offenses differently to test the robustness of our results. While registration and notification laws apply uniformly to rapists and child molesters, one might hypothesize that both types of reporting biases described above (frivolous reporting and meritorious reporting of previously undetected crime) are a concern primarily in the molestation context. Information that a sex offender lives nearby may make parents more aware of their children's behavior or whereabouts, and lead parents to investigate their suspicions more than they would have without that information. But knowledge of the proximity of a sex offender alone may be less likely, all else equal, to change reporting behavior substantially in the case of forcible rape.

When we separately examine child molestation ("forced fondling" in NIBRS) and forcible rape, which constitute the bulk of all sex offenses (see Table 2) using the specification in

Equation 2, we see no evidence of shifts in reporting behavior.[42] If increased reporting accounted for our findings, we would expect to see the notification-registry size interaction effect only in forced fondling cases, or at least primarily in those cases. Instead we find identical point estimates (both statistically significant) for both types of sex offenses. The notification-registry interaction effect (in percentage terms) is somewhat higher for fondling than it is for rape, but that difference is small.

7. Conclusion

Using detailed data on state laws 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 that registration laws reduce the frequency of sex offenses, particularly when the number of registrants is large, suggesting there are benefits of providing information on convicted sex offenders to local authorities. We estimate that an average-size registry (16 offenders per 10,000 people) results in a reduction of almost 1.3 sex offenses per 10,000 people, approximately a 14 percent reduction from the sample mean. This reduction in crime was locally concentrated, in line with the intentions of policymakers, with greater reductions among victims with a personal connection to offenders.

We also find evidence that notification laws reduce crime, but do so by deterring potential criminals, not necessarily recidivists. Our estimates imply that notification laws reduce crime when the size of the registry is small, but that these benefits dissipate when more offenders become subject to notification requirements. At the average registry size, in fact, we find that a notification regime (not including the distinct effects of a registration system) *increases* the number of sex offenses by more than 0.3 offenses per 10,000 people, or 3 percent. These results suggest that notification may serve as a deterrent to unregistered individuals, but registered offenders subject to notification may commit more crime, perhaps because of social and financial costs associated with the public release of their personal information.

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

[42] These results are available from the authors upon request.

established the 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 (2008) and Pope (2007) document declines in property value for households living close to registered offenders, and several studies find little evidence that community notification alleviates concerns among community members who have been notified of an offender's presence (Zevitz and Farkas (2000b), Beck and Travis (2004) (2006)).

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 among sex offenders. These more restrictive policies clearly impose higher costs on sex offenders and their families than registration and notification laws, and future research is needed to understand the impact of these policies on criminal behavior.

References

Adkins, G., Huff, D., Stageberg, P. (2000) “The Iowa Sex Offender Registry and Recidivism,” Report of the Iowa Department of Human Rights, Division of Criminal and Juvenile Justice Planning and Statistical Analysis Center.

Agan, A. (2007) “Sex Offender Registries: Fear without Function?” Unpublished Manuscript.

Bachman, R., Paternoster, R., Ward, S. (1992) “The Rationality of Sexual Offending: Testing a Deterrence/Rational Choice Conception of Sexual Assault,” *Law and Society Review* 26(2): 343–372.

Beck, V.S. and Travis, L.F. (2004) “Sex Offender Notification and Fear of Victimization,” *Journal of Criminal Justice* 32(): 455–63

Beck, V.S. and Travis, L.F. (2006) “Sex Offender Notification: A Cross-State Comparison,” *Police Practice and Research* 7(4): 293–307

Becker, G.S. (1968) “Crime and Punishment: An Economic Approach,” *Journal of Political Economy* 76(2): 169–217.

Boston Globe (October 5, 1996) “High-risk Sex Offenders to be Named on TV,” by Jordana Hart, Globe Staff.

Bureau of Justice Statistics (2002a) “Recidivism of Prisoners Released in 1994.” U.S. Dept. of Justice, Office of Justice Programs (March): NCJ 193427

Bureau of Justice Statistics (2002b) “Fact Sheet: Summary of State Sex Offender Registries, 2001.” U.S. Dept. of Justice, Office of Justice Programs (March): NCJ 192265.

Cameron, C., Gelbach, J.B., Miller, D.L. (2007) “Bootstrap-Based Improvements for Inference with Clustered Errors” Florida State University Law and Economics Research Paper Series, Paper No. 07/002.

Currie, J. and MacLeod, W.B. (2008) “First Do No Harm? Tort Reform and Birth Outcomes,” *Quarterly Journal of Economics* 123(2): 795–830

Di Tella, R. and Schargrodsky, E. (2004) “Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a Terrorist Attack,” *American Economic Review* 94(1): 115–133.

Donald, S.G. and Lang, K. (2007) “Inference with Difference-In-Differences and Other Panel Data,” *The Review of Economics and Statistics* 89(2): 221–33.

Edwards, W. and Hensley, C. (2001) “Contextualizing Sex Offender Management Legislation and Policy: Evaluating the Problem of Latent Consequences in Community

Notification Laws," *International Journal of Offender Therapy and Comparative Criminology* 45(1): 83–101.

Evans, W.N. and Owens, E.G. (2007) "COPS and Crime," Journal of Public Economics 91(1): 181–201.

Filler, D.M. (2001) "Making the Case for Megan's Law: A Study in Legislative Rhetoric," *Indiana Law Journal* 76: 315–345.

Freeman-Longo, R. (1996) "Prevention or Problem," *Sexual Abuse: A Journal of Research and Treatment* 8(2): 91-100

Hanson, R.K. (2002) "Recidivism and Age: Follow-Up Data from 4,673 Sexual Offenders," *Journal of Interpersonal Violence* 17(10): 1046–62.

Iyengar, R. (2007) "I'd Rather be Hanged for a Sheep than a Lamb: The Unintended Consequences of 'Three-Strikes' Laws," Unpublished Manuscript.

Jacob, B.A. Lefgren, L. and Moretti, E (2004) "The Dynamics of Criminal Behavior: Evidence from Weather Shocks," NBER Working Paper #10739.

Kessler, D. and Levitt, S.D. (1999) "Using Sentencing Enhancements to Distinguish between Deterrence and Incapacitation," *Journal of Law and Economics* 42(1): 343–63.

Langan, P.A., Schmitt, E.L., Durose, M.R. (2003) "Recidivism of Sex Offenders Released from Prison in 1994," Bureau of Justice Statistics Report NCJ 198281.

Levenson, J.S. and Cotter, L.P. (2005) "The Effect of Megan's Law on Sex Offender Reintegration," *Journal of Contemporary Criminal Justice* 21(1): 49–66.

Levenson, J.S. and D'Amora, D.A. (2007) "Social Policies Designed to Prevent Sexual Violence: The Emperor's New Clothes?" *Criminal Justice Policy Review* 18(2): 168–99

Levitt, S.D. (1998) "Juvenile Crime and Punishment," *Journal of Political Economy* 106(6): 1156–85.

Lieb, R. (1996) "Community Notification Laws: 'A Step Toward More Effective Solutions,'" *Journal of Interpersonal Violence* 11(2): 298–300.

Linden, L.L. and Rockoff, J.E. (2008) "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws," *American Economic Review* 98(3): 1103–27.

McCrary, J., and Lee, D.S. (2005) "Crime, Punishment, and Myopia," NBER Working Paper #11491.

Murphy, K.M., and Topel, R.H. (1985) “Estimation and Inference in Two-Step Econometric Models” *Journal of Business and Economic Statistics* 3(4): 370–79

Nagin, D.S. (1998) “Criminal Deterrence Research at the Outset of the Twenty-First Century,” *Crime and Justice* 23: 1–42.

North Carolina State Bureau of Investigation, “NC Sex Offender and Predator Statistics by County” 1996 – 2001, Online at: sbi2.jus.state.nc.us/crp/public/other/sexofsum.htm

Ohio Legislative Service Commission (2002) “Ohio Facts 2002.”

Pawson, R. (2002) “Does Megan’s Law Work? A Theory-Driven Systematic Review,” ESRC UK Centre for Evidence Based Policy and Practice, Working Paper 8

Pope, J.C. (2007) “Do Scarlet Letters Lead to Scarlet Homes? Household Reactions to Public Information from Sex Offender Registries,” Unpublished Manuscript.

Prentky, R.A. (1996) “Community Notification and Constructive Risk Reduction,” *Journal of Interpersonal Violence* 11(2): 295–98.

Presser, L. and Gunnison, E. (1999) “Strange Bedfellows: Is Sex Offender Notification a Form of Community Justice?” *Crime Delinquency* 45(3): 299–315.

Schram, D.D. and Milloy, C.D. (1995) “Community Notification: A Study of Offender Characteristics and Recidivism,” Washington State Institute for Public Policy Working Paper.

Shao, L. and Li, J. (2006) “The Effect of Sex Offender Registration Laws on Rape Victimization,” Unpublished Manuscript.

Teichman, D. (2005) “Sex, Shame, and the Law: An Economic Perspective on Megan’s Laws,” *Harvard Journal on Legislation* 42(1): 335–415.

Tewksbury, R. (2005) “Collateral Consequences of Sex Offender Registration” *Journal of Contemporary Criminal Justice* 21(1): 67–81.

U.S. Census Bureau (2002), Table CO-EST2001-12-00 - Time Series of Intercensal State Population Estimates: April 1, 1990 to April 1, 2000, Release Date: April 11, 2002.

U.S. Census Bureau (2002), Intercensal Estimates of the United States Population by Age and Sex, 1990-2000: All Months.

U.S. Census Bureau (2006), Table 1: Annual Estimates of the Population for the United States, Regions, and States and for Puerto Rico: April 1, 2000 to July 1, 2006 (NST-EST2006-01), Release Date: December 22, 2006.

U.S. Census Bureau (2006), County Population by Age, Sex, Race, and Hispanic Origin: April 1, 2000 through July 1, 2006.

U.S. Dept. of Justice, Federal Bureau of Investigation. NATIONAL INCIDENT-BASED REPORTING SYSTEM, 1995-2005 [Computer files]. Compiled by the U.S. Dept. of Justice, Federal Bureau of Investigation. ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor]

Vásquez, B.E., Maddan, S., and Walker, J.T. (2008) "The Influence of Sex Offender Registration and Notification Laws In the United States: A Time Series Analysis," *Crime & Delinquency* 54: 175–92.

Winick, B.J. (1998) "Sex Offender Law in the 1990s: A Therapeutic Jurisprudence Analysis," *Psychology, Public Policy, and Law* 4(1): 505–70.

Zevitz, R.G., Farkas, M.A. (2000a) "Sex Offender Community Notification: Managing High Risk Criminals or Exacting Further Vengeance?" *Behavioral Sciences and the Law* 18: 375–91.

Zevitz, R.G., Farkas, M.A. (2000b) "Sex Offender Community Notification: Assessing the Impact in Wisconsin" National Institute of Justice Report, December 2000

Table 1: Model Predictions for Registration and Notification Coefficients

	Variables of Interest			
	Registration Law Effective	**Registration Law * Registry Size**	**Notification Law Effective**	**Notification Law * Registry Size**
Impact on Model Parameters (caused by an increase in the column variable)	Non-RSOs: future punishment rises (f↑); likely small effect as costs appear low	RSOs: likelihood of punishment rises (p↑) due to easier police monitoring and apprehension	Non-RSOs: future punishment rises (f↑); possibly large effect as costs appear significant	RSOs: likelihood of punishment rises (p↑) due to community policing; costs of crime increase (c↑) due to victim awareness; relative attractiveness of crime increases (u↑) due to shame/publicity
Predicted Effect on Frequency of Offenses (caused by an increase in the column variable)	Prediction: Reduction in frequency	Prediction: Reduction in frequency	Prediction: Reduction in frequency	Prediction: Indeterminate effect on frequency – more community policing and more victim avoidance versus higher relative attractiveness of crime
Predicted Heterogeneity Across Victims	No predicted differences across different types of offender-victim relationships	Prediction: Stronger effects for "local" victims; potential displacement of crimes onto "distant" victims	No predicted differences across different types of offender-victim relationships	Prediction: Stronger effects on "local" victims due to community policing and victim avoidance; no predicted differences for any relative utility effect
Predicted Change in the Probability of Arrest (caused by an increase in the column variable)	Prediction: Any change should have the same sign as the effect (if any) on the frequency of offenses	Prediction: Reduction in the probability of arrest, but effect should approach zero as crime declines or is displaced onto more distant victims	Prediction: Any change should have the same sign as the effect (if any) on the frequency of offenses	Prediction: Indeterminate effect on probability of arrest – offsetting effects of community policing (positive, but zero with displacement), victim avoidance (negative, but zero with displacement), and relative attractivness of crime (positive)

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

Table 2: Summary Statistics on Reported Crime Incidents

	Sex Offenses	Assaults
Total Number of Incidents in Sample	328,260	4,757,118
Rape and Sexual Assault	37.9%	*n/a*
Forced Fondling (i.e., Sexual Molestation)	41.8%	*n/a*
Other Non-Violent Sex Offenses	20.3%	*n/a*
Percent of Incidents with Report Date	18.9%	12.8%
Percent of Incidents Leading to Arrest	25.7%	37.3%
Average Days to Arrest	24.3	13.7
Prosecution Drops Charges	7.1%	4.8%
Victim Refuses to Cooperate	5.1%	6.6%
Offender-Victim Relationship		
Immediate Family (e.g., spouse, child, sibling)	11.5%	24.9%
Extended Family (e.g., grandparent, in-laws)	9.3%	3.7%
Stepfamily	4.7%	1.3%
Friend	7.0%	2.8%
Significant Other	8.0%	17.6%
Acquaintance	31.0%	23.7%
Neighbor	2.4%	1.8%
Otherwise Known	9.6%	9.3%
Stranger	8.4%	9.7%
Relationship Unknown	11.8%	10.2%
Missing Relationship Information	4.3%	4.5%
Victim Characteristics		
Female	86.5%	58.4%
White	77.6%	68.2%
Black	17.9%	28.3%
Aged 0-4	8.7%	0.7%
Aged 5-9	14.8%	1.6%
Aged 10-14	27.1%	6.9%
Aged 15-19	23.8%	15.3%
Aged 20-29	13.1%	30.2%
Aged 30-39	7.2%	24.0%
Aged 40-49	3.6%	14.5%
Aged 50-65	1.1%	5.6%
Aged 65+	0.5%	1.3%
Offender Characteristics		
Male	95.9%	76.8%
White	69.0%	61.5%
Black	24.1%	33.9%
Aged 0-9	2.3%	0.5%
Aged 10-14	11.2%	6.2%
Aged 15-19	20.1%	15.9%
Aged 20-29	25.5%	31.3%
Aged 30-39	20.5%	25.4%
Aged 40-49	12.0%	14.5%
Aged 50-65	6.5%	5.1%
Aged 65+	1.9%	0.9%

Notes: Sample includes all sex offenses and assaults reported in the 15 NIBRS states that we include in our analysis. Relationships total to more than 100% in this table because some incidents involved more than one relationship.

Table 3: Effects of Registration and Notification on Sex Offense Frequency

	Sex Offenses per 10,000	Sex Offenses per 10,000	ln (Sex Offenses per 10,000)	ln (Sex Offenses per 10,000)
	(1)	(2)	(3)	(4)
Registry Effective	0.300	0.247	0.023	0.020
	(0.442)	(0.487)	(0.033)	(0.035)
	[.51]	[.62]	[.50]	[.58]
Registry Effective * Registry Size	-0.078	-0.096	-0.006	-0.007
	(0.033)	(0.04)	(0.002)	(0.003)
	[.04]	[.04]	[.04]	[.03]
Notification	-1.116	-1.050	-0.075	-0.072
	(0.354)	(0.364)	(0.027)	(0.027)
	[.01]	[.02]	[.02]	[.02]
Notification * Registry Size	0.082	0.087	0.006	0.006
	(0.028)	(0.031)	(0.002)	(0.002)
	[.01]	[.02]	[.02]	[.02]
Assault/Crime Controls		✓		✓
Mean Offense Frequency	9.17			
(Standard Deviation)	(9.70)			
Mean Registry Size	15.99			
(Standard Deviation)	(11.15)			
Observations	210,209	210,209	210,209	210,209
R-squared	0.35	0.36	0.68	0.68

Notes: The dependent variable is annualized incidents per 10,000 persons in columns (1)-(2). In columns (3)-(4), the dependent variable is the natural log of annualized incidents plus one per 10,000 persons. The unit of observation is a reporting agency (ORI) by month cell. Registry size is measured in offenders per 10,000 persons (mean registry size is reported). 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 Data Appendix. All regressions control for county income and demographics, ORI fixed effects, year fixed effects, and month fixed effects. Columns, as indicated, also control for rates of assault and other crime. Regressions are weighted by the covered population in each ORI. Standard errors (in parentheses) are estimated via bootstrapping. P-values shown in brackets.

Table 4: Effects of Registration and Notification on Sex Offense Frequency and Relationship Mix

	All Victims	"Close" Victims	"Near" Victims	"Stranger" Victims
	(1)	(2)	(3)	(4)
Registry Effective	0.247	0.038	0.130	-0.126
	(0.487)	(0.167)	(0.207)	(0.169)
	[.62]	[.82]	[.54]	[.48]
Registry Effective * Registry Size	-0.096	-0.032	-0.031	0.006
	(0.04)	(0.02)	(0.017)	(0.01)
	[.04]	[.15]	[.10]	[.57]
Notification	-1.050	-0.339	-0.297	-0.244
	(0.364)	(0.099)	(0.162)	(0.126)
	[.02]	[.01]	[.10]	[.08]
Notification * Registry Size	0.087	0.031	0.026	0.014
	(0.031)	(0.013)	(0.014)	(0.007)
	[.02]	[.04]	[.09]	[.06]
Mean Offense Frequency	9.17	3.46	3.78	1.83
(Standard Deviation)	(9.70)	(5.58)	(5.67)	(3.47)
Mean Registry Size	15.99	15.99	15.99	15.99
(Standard Deviation)	(11.15)	(11.15)	(11.15)	(11.15)
Observations	210,209	210,209	210,209	210,209
R-squared	0.36	0.20	0.21	0.29

Note: The unit of measurement for the dependent variables is annualized incidents per 10,000 persons, and the unit of observation is a reporting agency (ORI) by month cell. Registry size is measured in offenders per 10,000 persons. 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 Data Appendix. The regressions control for rates of assault and other crime, county income and demographics, ORI fixed effects, year fixed effects, and month fixed effects, as described in the text. In columns (2) to (4), the assault and other crime variables are 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 estimated via bootstrapping. P-values shown in brackets.

Table 5: Effects of Registration and Notification on Arrest Outcomes

	Arrest Made	Time to Arrest (in days)	Victim Refuses to Cooperate	Prosecution Drops Charges
	(1)	(2)	(3)	(4)
Registry Effective	-0.0013	-1.6311	0.0194	0.0512
	(0.0378)	(1.5563)	(0.0126)	(0.0185)
	[.97]	[.32]	[.16]	[.02]
Registry Effective * Registry Size	-0.0008	0.0240	-0.0010	-0.0039
	(0.005)	(0.1113)	(0.001)	(0.002)
	[.88]	[.83]	[.34]	[.08]
Notification	-0.0154	0.4400	-0.0166	0.0020
	(0.0381)	(2.3799)	(0.0138)	(0.0221)
	[.70]	[.86]	[.26]	[.93]
Notification * Registry Size	0.0030	-0.0472	0.0018	0.0033
	(0.0032)	(0.1124)	(0.0011)	(0.0019)
	[.36]	[.68]	[.12]	[.10]
Mean of Dependent Variable	0.23	15.81	0.07	0.10
(Standard Deviation)		(23.89)		
Observations	287,789	65,702	287,789	287,789
R-squared	0.10	0.12	0.10	0.14

Notes: The unit of observation is a reported sex offense. The dependent variables in columns (1), (3), and (4) are zero-one indicators, respectively, for whether an arrest was made in connection with a report, for whether the report was cleared because the prosecution declined to pursue the case, and for whether it was cleared because the victim did not cooperate. In column (2), the sample is restricted to reported sex offenses that lead to an arrest, and the dependent variable is the number of days from the report/occurrence of an incident until an arrest was made. The regression includes controls for victim and offender characteristics, victim-offender relationship, type of sex offense, ORI fixed effects, year fixed effects, and month fixed effects, as described in the text. Standard errors (in parentheses) are calculated via bootstrapping. P-values are given in brackets.

Table 6: Robustness of Registration and Notification Effects on Sex Offense Frequency

	Sex Offenses per 10,000	Sex Offenses per 10,000	Sex Offenses per 10,000	ln (Sex Offenses per 10,000)	ln (Sex Offenses per 10,000)	ln (Sex Offenses per 10,000)
	(1)	(2)	(3)	(4)	(5)	(6)
Registry Effective	0.300	0.247	-0.210	0.023	0.020	-0.011
	(0.442)	(0.487)	(0.285)	(0.033)	(0.035)	(0.02)
	[.51]	[.62]	[.48]	[.50]	[.58]	[.59]
Registry Effective * Registry Size	-0.078	-0.096	-0.060	-0.006	-0.007	-0.004
	(0.033)	(0.04)	(0.036)	(0.002)	(0.003)	(0.002)
	[.04]	[.04]	[.13]	[.04]	[.03]	[.06]
Notification	-1.116	-1.050	-0.430	-0.075	-0.072	-0.024
	(0.354)	(0.364)	(0.283)	(0.027)	(0.027)	(0.023)
	[.01]	[.02]	[.16]	[.02]	[.02]	[.33]
Notification * Registry Size	0.082	0.087	0.055	0.006	0.006	0.004
	(0.028)	(0.031)	(0.019)	(0.002)	(0.002)	(0.001)
	[.01]	[.02]	[.02]	[.02]	[.02]	[.02]
Assault/Crime Controls		✓	✓		✓	✓
State Linear Trends			✓			✓
Mean Offense Frequency			9.17			
(Standard Deviation)			(9.70)			
Mean Registry Size			15.99			
(Standard Deviation)			(11.15)			
Observations	210,209	210,209	210,209	210,209	210,209	210,209
R-squared	0.35	0.36	0.36	0.68	0.68	0.68

Notes: The dependent variable is annualized incidents per 10,000 persons in columns (1)-(3). In columns (4)-(6), the dependent variable is the natural log of annualized incidents plus one per 10,000 persons. The unit of observation is a reporting agency (ORI) by month cell. Registry size is measured in offenders per 10,000 persons (mean registry size is reported). 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 Data Appendix. All regressions control for county income and demographics, ORI fixed effects, year fixed effects, and month fixed effects. Columns, as indicated, also control for rates of assault and other crime and state-specific linear trends. Regressions are weighted by the covered population in each ORI. Standard errors (in parentheses) are estimated via bootstrapping. P-values shown in brackets.

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
Colorado	07/01/1991	06/05/1995	06/05/1995 [a]	07/01/1999	N/A	N/A	07/30/2001	12/01/1999	12/1/1999 [b]	05/30/2006	N/A	N/A	12/01/1999	N/A
Connecticut	01/01/1995	10/1/1998 [c]	N/A	10/1/1998 [c]	N/A	N/A	1/1/1999 [c]	10/01/1995	10/01/1995 [c]	N/A	N/A	N/A	10/01/1995 [c]	N/A
Idaho	07/01/1993	07/01/1993	N/A	07/01/1993	07/01/1993 [d]	07/01/1993 [e]	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	070/1/1995 [e]	03/16/2000	07/01/1998	07/01/1998	N/A	N/A	N/A	07/01/1998	N/A
Kentucky	01/01/1995	N/A	N/A	N/A	N/A	N/A	04/11/2000	01/01/1999 [f]	07/12/2006	01/01/1999 [g]	01/01/1999 [g]	01/01/1999 [g]	01/01/1999 [f]	01/01/1999 [g]
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
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
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
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 [h]	08/01/1997	N/A	08/01/2001	08/01/1995	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
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 [i]	6/30/1999 (newspapers)	N/A	N/A	06/18/1996	06/30/1999
Texas	09/01/1991	09/01/1995	N/A	09/01/1995	09/01/1995 [j]	N/A	01/11/1999	09/01/1995	09/01/2005 (newspapers)	09/01/1995 [k]	N/A	N/A	09/01/1999	09/01/1995 [l]
Utah	03/30/1983	04/29/1996	03/15/1996 [e]	07/01/1998	03/15/1996 [e]	03/15/1996 [e]	07/01/1998	N/A	N/A	N/A	N/A	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
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

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

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.

Appendix Table 2: Summary Statistics on ORI-Level Samples

	15 states (1991 start)	s.d.	13 states (1991 start)	s.d.	15 states (1995 start)	s.d.
ORI Population	18,750	38,570	17,168	29,277	19,052	39,503
Number of Offenses (annualized, per 10,000 people)						
Total Sex Offenses	9.17	9.70	9.20	9.89	9.09	9.67
Rape Offenses	4.71	6.29	4.69	6.45	4.65	6.25
Forced Fondling Offenses	3.85	5.88	3.92	6.16	3.83	5.85
Assaults	138.3	116.0	135.7	117.3	136.6	114.9
Other Crimes	514.6	338.3	504.7	337.7	509.6	333.1
Registration and Notification Laws						
Registration Effective	0.925	0.26	0.901	0.30	0.984	0.13
Full Notification Effective	0.770	0.42	0.696	0.46	0.832	0.37
Full Public Access Effective	0.560	0.50	0.489	0.50	0.605	0.49
Full Internet Access Effective	0.563	0.50	0.514	0.50	0.609	0.49
Full Active Community Notification Effective	0.202	0.40	0.130	0.34	0.218	0.41
Registry Size (estimated, incl. states with no registry)	14.79	11.52	14.66	11.71	15.87	11.22
ORI-Level Demographics						
Per-Capita Personal Income (in thousands)	28.96	15.40	27.29	16.59	29.85	15.65
Unemployment Rate	0.048	0.019	0.047	0.020	0.047	0.018
Male (ages 1-14)	0.219	0.026	0.220	0.027	0.218	0.025
Male (ages 15-29)	0.218	0.043	0.218	0.044	0.217	0.042
Male (ages 30-44)	0.232	0.025	0.231	0.024	0.231	0.025
Male (ages 45-64)	0.228	0.031	0.226	0.032	0.231	0.030
Male (ages 65+)	0.104	0.029	0.105	0.029	0.104	0.028
Female (ages 1-14)	0.201	0.024	0.202	0.025	0.200	0.024
Female (ages 15-29)	0.202	0.041	0.203	0.043	0.201	0.041
Female (ages 30-44)	0.225	0.024	0.224	0.024	0.225	0.025
Female (ages 45-64)	0.229	0.030	0.227	0.031	0.232	0.028
Female (ages 65+)	0.142	0.037	0.144	0.037	0.142	0.037
Black	0.122	0.143	0.119	0.147	0.118	0.140
White	0.798	0.149	0.815	0.144	0.798	0.148
Hispanic	0.053	0.064	0.044	0.049	0.056	0.065
Number of ORI-Level Observations	210,209		173,706		190,571	

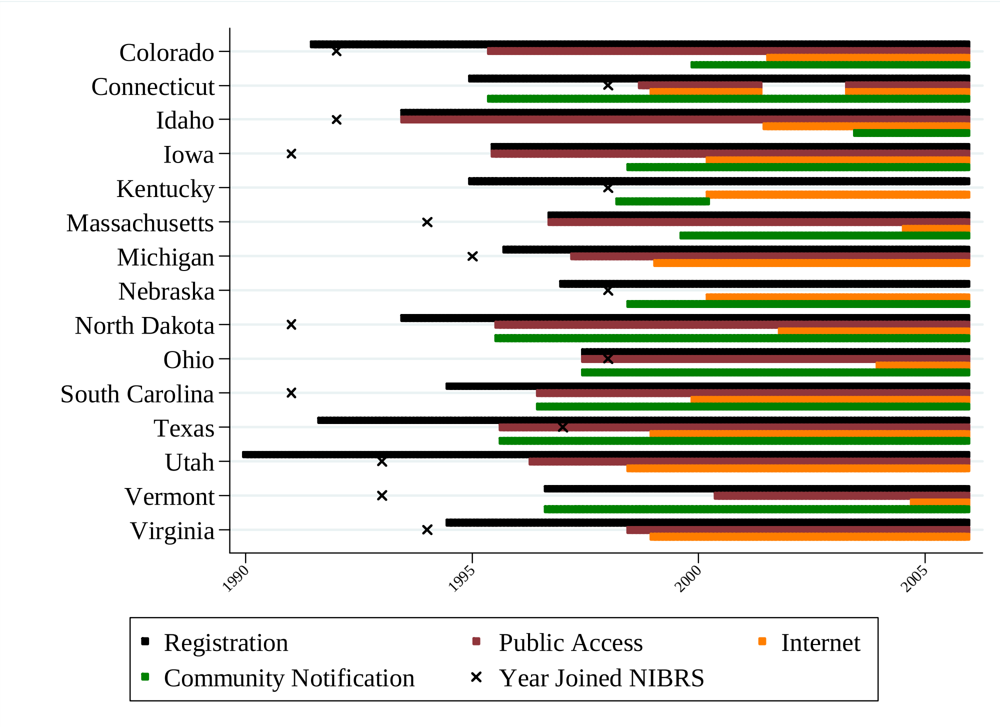
Notes: Samples are identical to the data used in the regression analyses reported in Tables 3, 4, and 6. Data are weighted by NIBRS covered population for number of offenses, registration and notification laws, and ORI-level demographics. Data are aggregated from incident level to ORI-month level. Mean registry size estimate is calculated over all ORI-months, including those months where no law was in place, in which registry size is always zero.

Appendix Table 3: Effects of Registration and Notification on Sex Offense Frequency (13 State Sample)

	Sex Crimes per 10,000	Sex Crimes per 10,000	Sex Crimes per 10,000	ln (Sex Crimes per 10,000)	ln (Sex Crimes per 10,000)	ln (Sex Crimes per 10,000)
	(1)	(2)	(3)	(4)	(5)	(6)
Registry Effective	0.295	0.215	-0.247	0.027	0.023	-0.010
	(0.422)	(0.467)	(0.298)	(0.032)	(0.034)	(0.024)
	[.50]	[.66]	[.43]	[.41]	[.51]	[.69]
Registry Effective * Registry Size	-0.071	-0.088	-0.058	-0.005	-0.006	-0.004
	(0.035)	(0.043)	(0.034)	(0.002)	(0.003)	(0.002)
	[.07]	[.07]	[.12]	[.04]	[.04]	[.05]
Notification	-1.100	-1.047	-0.460	-0.074	-0.071	-0.026
	(0.362)	(0.349)	(0.215)	(0.025)	(0.024)	(0.017)
	[.01]	[.01]	[.06]	[.01]	[.01]	[.15]
Notification * Registry Size	0.084	0.088	0.060	0.006	0.006	0.004
	(0.028)	(0.031)	(0.019)	(0.002)	(0.002)	(0.001)
	[.01]	[.02]	[.01]	[.02]	[.02]	[.01]
Assault/Crime Controls		✓	✓		✓	✓
State Linear Trends			✓			✓
Mean Crime Frequency			9.20			
(Standard Deviation)			(9.89)			
Mean Registry Size			16.27			
(Standard Deviation)			(11.22)			
Observations	173,706	173,706	173,706	173,706	173,706	173,706
R-squared	0.30	0.31	0.31	0.61	0.62	0.62

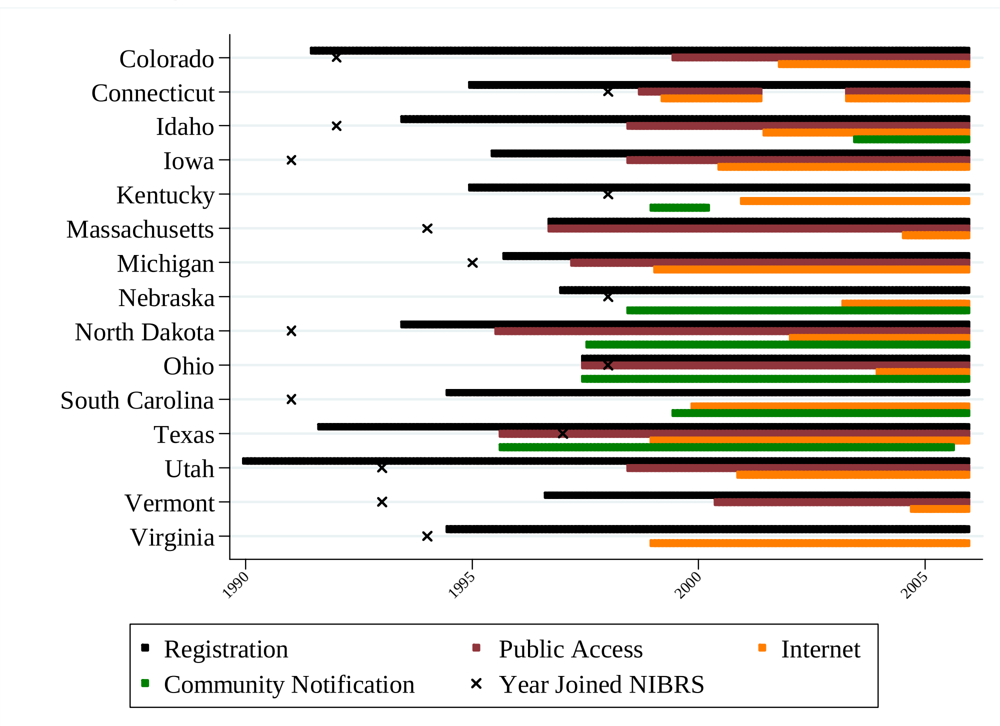
Notes: The dependent variable is annualized incidents per 10,000 persons in columns (1)-(3) and is the natural log of this value in columns (4)-(6). The unit of observation is a reporting agency (ORI) by month cell. Registry size is measured in offenders per 10,000 persons (mean registry size is reported). 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 Data Appendix. All regressions control for county income and demographics, ORI fixed effects, year fixed effects, and month fixed effects. Columns, as indicated, also control for rates of assault and other crime and state-specific linear trends. Regressions are weighted by the covered population in each ORI. Standard errors (in parentheses) are estimated via bootstrapping. P-values shown in brackets.

Figure 1a: Registration and Notification Laws in NIBRS States



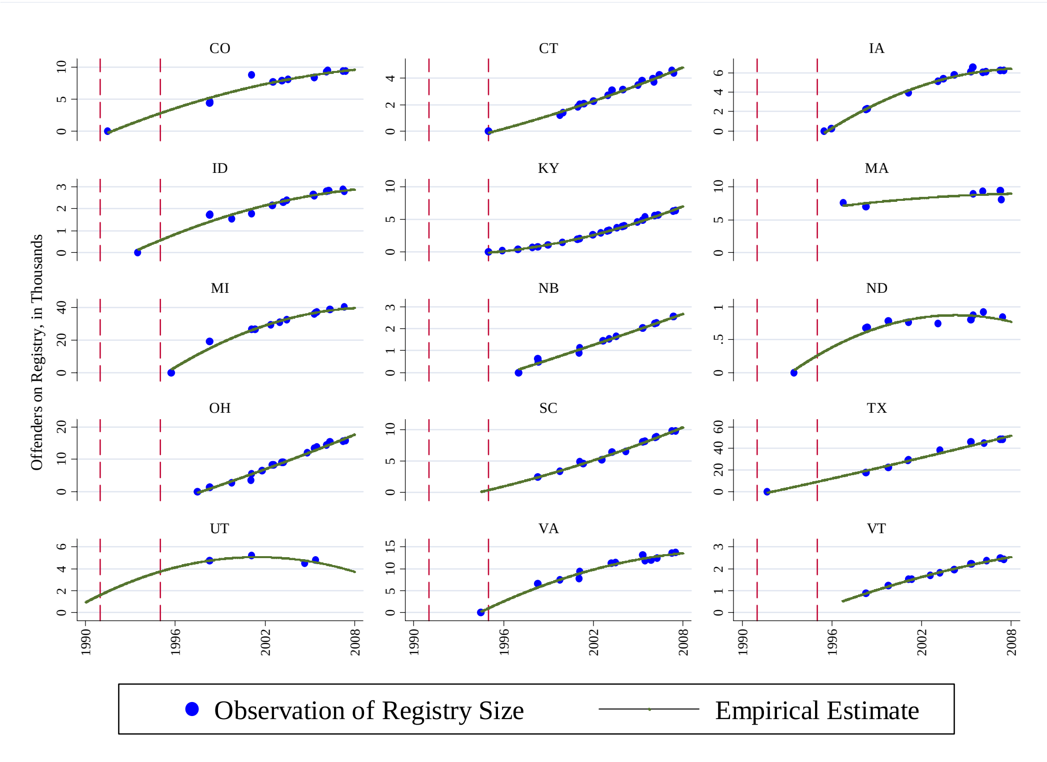
Note: Depicted are dates when registration, public access, and community notification laws are effective, and when an internet site goes live. These include all laws, regardless of special restrictions. Utah's registration law was effective in 1983. For details see Appendix Table 1.

Figure 1b: Registration and "Full" Notification Laws in NIBRS States



Note: Depicted are dates when "full" versions registration, public access, and community notification laws are effective, and when a complete internet registry site goes live. Utah's registration law was effective in 1983. For details see Appendix Table 1 and the text in Section 5.

Appendix Figure 1:
Observations of Registry Size and Empirical Estimates



Notes: This figure depicts our empirically estimated registry sizes for the NIBRS states in our sample. To calculate these registry sizes, we ran a least squares regression of registry size on quadratic function of date, allowing for state specific intercepts and slopes and using all data points available for each state. We then use the predicted values from this regression as measures of the state registry size for each month. See the Data Appendix for more details.

Data Appendix

1. Coding Registration and Notification Laws

To build our database of registration and notification laws, we constructed a detailed legal timeline for each state, relying principally on paper legislative sources, legal databases containing statutes and judicial opinions, news releases and stories, and conversations and email communications with state employees. We catalogued enactment dates, effective dates, and compliance dates for each legal change, and verified, where possible, that such changes took place in reality, and not just on paper. We cross-checked our research with other sources compiling 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 they differ across states on various dimensions.

As noted in Section 2, states invariably began regulating sex offenders with registration statues that required all sex offender information be kept confidential. Over time, however, state laws evolved toward making more of this information public. Most states began 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. By the end of our sample period, all states maintained a web registry, and many had enacted "active" community notification.

2. NIBRS and Demographic Data

Many of the advantages and disadvantages of NIBRS data are described in Section 4, but we met additional challenges and made additional choices in constructing our data for analysis. One limitation of NIBRS is that the first year for which NIBRS data are available (from the ICPSR) is 1995. To address this problem, we requested additional data, available for some states back to 1991, from the FBI, and have incorporated that data into our analysis. Excluding data from 1991 through 1994 does not affect our results. Another complexity, discussed briefly in Section 4, is that participation of law enforcement agencies can vary within a state and over time. Agencies are identified in NIBRS by an "Originating Agency Identifier" (ORI), and, within each state, the number of reporting ORIs increases dramatically over time. For example, the number of reporting ORIs from Nebraska more than quintupled between 1998 (its first reporting year) and 2005. Consequently, we include ORI fixed effects in our regressions.

In building our offense variables (sex offense, assault, or "other" crime) and our relationship groups ("close," "near," and "stranger"), we had to deal with the non-trivial number of sex offenses with multiple victims (8 percent) or multiple offenders (8 percent). The indicators we create for the relationship between the offender and the victim include all victim relationships. For example, if there were two victims, a family member and a friend of the offender, both the family member and friend indicators are set equal to one. When we examine arrests, we include victim and offender characteristics as controls. For incidents with multiple victims or multiple offenders we record the characteristics of the first victim and first offender listed. These non-relationship variables are only used in our analysis of arrests.

In addition to the information on victims, offenders, and arrestees from NIBRS, we use annual, county-level demographic data from the U.S. census on the fraction of the population in 18 age categories and five ethnicities as well as annual county-level data on income per capita, poverty rates, and unemployment rates as controls in our regressions. While some ORIs are smaller than counties, we believe these are the best annual data available to control for any demographic shifts that may have occurred in ORIs over our sample period. Two percent of ORIs are located in multiple counties. We assign to these ORIs a weighted average of county characteristics based on the population of the ORI in each county.

3. Addressing Reporting Errors in NIBRS

We use a multi-faceted approach to handle reporting errors in the NIBRS data we employ. First, we use the indicators provided in NIBRS for whether an ORI reported crime in a given month. Among ORIs reporting crime during any month of each year, the fraction reporting for all twelve months ranged from 68 percent in 1995 to 89 percent in 2004. We limit our analysis to crimes that took place during months when the ORI reported crime in the previous month, the current month, and the next four months. This restriction causes us to drop less than 5 percent of sex offenses that occurred between February 1991 and August 2005, and all offenses occurring outside this period.

Despite this initial cleaning of the data, we found a number of instances of apparent underreporting of crimes in NIBRS. For example, an agency might report about 500 crimes every month for many months, then report few or no crimes for one month, and then return to the previous pattern of 500 crimes. We also observe agencies that, according to the NIBRS indicators, started reporting officially on a given month, but do not start reporting crime until several months later. To address these concerns, we implement an algorithm to identify these kinds of misreporting. First, we take all agency-period cells with a given number of crimes reported, then we calculate the variance of the number of crimes reported in periods a given length of time from the current period, and then we flag all observations that are outliers given this variance (i.e., the observation has very small chance of occurring, assuming reports are normally distributed with given variance). We repeat this process for reports up to six periods away and flag observations twice: first with one in one million chances and second with one in one hundred thousand chances. The two-stage process is helpful because it allows us to recalculate the variance after eliminating very distant outliers. We also flag all adjacent months, to guard against the possibility that underreporting in one month leads to over-reporting in others.

4. Registry Size Estimates

Our data on historical registry sizes come from various sources. 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 (2002b)). 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 data from news articles and state government reports. We also know that some states did not apply their laws retroactively, and so we are able to include a zero at the start of their registries.

Thus, although we have collected data on the number of registered offenders in each state, there are some years for which we do not have this information. In order to estimate the historical size of the registries for the NIBRS states at the state level, we run a least squares regression of registry size on quadratic function of date, allowing for state specific intercepts and slopes and using all data points available for each state. We then use the predicted values from this regression as measures of the state registry size for each month. The results of these regressions are depicted in Appendix Figure 1.

We combine this state-level information on registry size with information on all registered offenders nationwide as of August, 2007. This data set was compiled by a private company (www.familywatchdog.com) that provides sex offender information to the public, and was given to us for research purposes. This data allows us to calculate the number of registered sex offenders by county within each NIBRS state. We then allocate sex offenders to each county under the assumption that the fraction of offenders by county today is reflective of the fractions by county in past years.

5. Bootstrap Method for Standard Error Estimates

In order to estimate unbiased standard errors in the presence of a small number of clusters, we follow Cameron et al. (2007) and employ a bootstrapping methodology.

Specifically, we repeat each regression in our analysis 500 times and calculate our standard errors using the variance of the resulting estimates. Let β_i be the estimated vector of coefficients from repetition *i*. Our variance estimate is $\hat{\sigma}^2_{\beta^*}$, where

$$\sigma^2_{\beta^*} = \tfrac{1}{N-1}\sum_i^N \left(\beta_i - \overline{\beta}\right)^2 \quad , N = 100$$

In the simulations carried out by Cameron et al. (2007), this technique, which they term "paired bootstrap-se," does not perform as well as other techniques, such as "wild bootstrap," in the sense that it finds a placebo to have a statistically significant relationship with the dependent variable at the 0.05 level in around 10 percent of their simulations. However, it is not clear from their work whether this difference is reflective of a general result that would apply to our situation, i.e., an unbalanced panel with groups of differing size and independent variables that have different variances across groups. We find the standard errors from a wild bootstrap are smaller than those from the paired bootstrap, and we therefore use the paired bootstrap estimates.

In addition to sampling our states (with replacement) in each repetition, we take account of any additional bias due to estimated regressors by using values for registry size calculated from randomly drawn values from the distribution of our estimator in the "first stage" where we estimate registry size. Specifically, we take the estimator of the *K* parameters from the first stage (γ_0) and use the Cholesky decomposition of the variance-covariance matrix (V) to draw a new vector (γ_i) where

$$\gamma_i = \gamma_0 + V^{1/2}R \ , \ R = [r_1 \ldots r_K], \ r_i \sim i.i.d.N(0,1).$$

We then use this vector of coefficients to re-estimate registry size for each regression.