Sex Offender Registries: Fear without Function?*

Amanda Y. Agan
University of Chicago Department of Economics

December 2008

* I would like to thank Alex Tabarrok, Steven Levitt, Emily Oster, and seminar participants at the University of Chicago for helpful comments and discussion; and Eric Helland for help with data.

Abstract

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

1. Introduction

Sex offender registries that contain the names, addresses, and photographs of sex offenders are now easily accessible on the internet.[1] People appear to value this information and pay to avoid living near a registered sex offender; homes within close proximity of a registered offender sell for about $5,500 less than comparable homes (Lindon and Rockoff 2006). The major justification for the existence of registries is to protect the public. Are sex offender registries effective at serving this purpose or do they create fear without increasing public safety? Two plausible channels exist through which registries could be effective. Registration implies a larger penalty for sex offenders which could deter new offenders and recidivists not yet on the registry. Once on the registry, registries could reduce recidivism through target hardening or through increase police monitoring.

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

I find little evidence to support the effectiveness of sex offender registries, either in practice or potentially. Rates of sexual offense do not decline after the introduction of a registry

[1] See for example the sex offender registry for the state of Virginia: http://sex-offender.vsp.virginia.gov/sor/index.htm

or public access via the internet, nor do sex offenders appear to recidivate less when released into states with registries. The data from Washington, DC indicates that census blocks with more offenders do not experience statistically significantly higher rates of sexual abuse, implying that there is little information we can infer from knowing that a sex offender lives on our block.

I use three different datasets and designs to test these different aspects of registry effectiveness. First, I answer whether the existence of a sex offender registry or public access to this information via the internet decreases the rate of rape and other sexual offenses. I use state panel data from 1985-2003 which includes rape incidences and sexual abuse arrests as well as control variables and run a fixed effects regression to examine whether these measures declined after the introduction of a registry or public access via the internet.

Second, I determine whether sex offender registries reduce the recidivism rate of offenders required to register compared with those who do not. I use a dataset which tracks arrests and convictions of individual sex offenders for three years after their release from prison in 1994. I compare subsequent criminal records for sex offenders who should have been registered in the state sex offender registry upon release with criminal records for sex offenders who would not have been required to register upon release. I compare the two groups on two measures of recidivism: arrest and conviction. The variation arises from the fact that five of the fourteen states in the study had registration laws in effect by 1994, three enacted registration laws in 1994, and 6 had no registries by the end of 1994.

Third, I want to test whether sex offender registries are *potentially* effective by determining whether locations with more registered sex offenders have higher rates of sexual abuse crimes. Or, put another way, does knowing the location of sex offenders in a region help predict the locations of sexual abuse crimes. I combine data on sex offenses in Washington, DC which include crime location with the addresses of registered offenders from the Washington DC Sex Offender registry. This locational data is then used to determine if blocks with registered offenders are subject to increased rates of sexual assault. This data is useful for understanding whether releasing information on the *location* of a sex offender is useful or helpful to the public.

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

More recent work by Prescott & Rockoff (2008) and Walker, et al. (2005) use data across states and exploit the variation in timing of the start of registration to attempt to determine the effectiveness of sex offender registries. Prescott & Rockoff (2008) use data from the National Incident Based Reporting System (NIBRS), a subset of the UCR, which allows them to use incident level data rather than arrests, and find mixed results on the effects of registration.[2] They attempt to separate out the effects of registration and notification and between registered and non-registered offenders and the effects on the relationship between offenders and victims. On the one hand, they find that sex offenses committed against known victims (relatives or others with a prior relationship to the offender) were reduced. They find no effect on sex offenses

[2] NIBRS data covers a small subset of states, and within a state only represents cities with less than 1 million occupants. Recent research suggests NIBRS data suffers from bias and tends to over-represent certain subpopulations.

committed against strangers. In terms of recidivism, the authors find that registered offenders may actually be more likely to recidivate when subject to community notification. They find no evidence that the registration requirement deters first time offenders, but do find evidence that registration itself reduces recidivism of registered sex offenders. When looking at arrests rather than incidents they find no significant affect of registration or notification.

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

In addition to the few empirical papers, some surveys have been conducted to extract opinions from sex offenders and professionals who work with sex offenders about the registries and community notification. A survey of the opinions of mental health professionals that work with sexual abusers found that most believe that community notification will be ineffective in preventing future sex crimes (Malesky and Keim, 2001)[3]. Surveys of sex offenders themselves reveal that a majority of sex offenders experience some type of negative consequences from harassment, loss of employment, or feelings of isolation. The surveys reveal mixed feelings about the effectiveness of registration and community notification. (Zevitz, Crim & Farkas 2000, Levonson & Cotter 2005).

The paper continues as follows: Section 2 provides background on sex offender registries. Section 3 presents data and results from the state-level panel data. Section 4 presents

[3] For example, a majority (59.4%) of respondents did not believe that community notification will deter offenders on the registry because they believe they are being monitored. A majority (67.7%) did not agree that community notification would deter offenders not on the registries with the threat of being put on the registry.

data and results from recidivism analysis. Section 5 presents data and results from the locational data in Washington, D.C. Section 6 concludes.

2. Background

A sex offender is anyone who is convicted of a sex crime. Which crimes qualify as sex crimes varies by state, but most include rape (forced and statutory), sexual assault or battery, child molestation or any sexual conduct with a minor, production or possession of child pornography, and attempts to commit any of these crimes. Even though California began registering sex offenders as early as 1947, sex offender registries did not become widespread until the 1990s after state registries became federal law with the passing of the Jacob Wetterling Act in 1994 (California Office of the Attorney General 2001).[4] Over the next few years more states enacted sex offender registries to comply with the provisions of the Wetterling Act and by the end of the 1990s every state was registering sex offenders. These registries contain information about convicted sex offenders including name, address, physical characteristics of the offender, and sex crime committed. Sex offenders must periodically report to a local authority, usually a local police department, and verify their current address. States vary on how many times per year a sex offender must verify their address, from once a year to quarterly. [5] Although these registries were originally meant for use by police, community notification about registered sex offenders was pioneered by Washington in 1990. Community notification subsequently became part of federal legislation in 1996 with the addition of Megan's Law, an amendment to the Jacob Wetterling Act.[6] This notification can be handled through a variety of

[4] In October 1989 in St. Joseph Minnesota Jacob Wetterling was abducted by an armed, masked man. To this day it is unknown what happened to Jacob, but his parents began the Jacob Wetterling Foundation and have been advocating for child safety and protection laws ever since.
[5] Sometimes within states this requirement varies by type of sex offender. For example, in Arkansas offenders must verify their address every 6 months, however sexually violent predators must verify their address every 3 months.
[6] Megan's Law is named for Megan Kanka who was murdered at age 7 by a sex offender in her neighborhood, although since community notification was not in effect no one knew he had committed offenses in the past.

both passive and active means including community meetings and flyers. In 1997, some states began putting registry information on the internet. By the summer of 2006 all states had some subset of the registry available for public search on the internet. In many states the entire contents of the registry are available for search on the website (e.g., Alabama, Florida, Idaho, North Carolina and Texas among others); others include only sexual offenders in a certain "tier" or category (like Arizona which only lists level 2 and 3 offenders or Minnesota which only lists level 3 offenders).

States also differ on whether the registration or public notification is retroactive. For example, in Connecticut the registry became effective on October 1, 1998, but applies to anyone released from state custody on or after October 1, 1988. However, in Delaware the law only applied to those convicted on or after the date the registry became effective. Registration and retroactivity is not without controversy, however. Several law suits have been filed on behalf of sex offenders, and two went to the Supreme Court of the United States. In both cases the Court upheld the constitutionality of registration. The Missouri Supreme Court found similarly with one key exception - the registration requirement could not be applied retrospectively to sex offenders convicted before the laws inception on January 1, 1995. [7]

State governments cite public safety and protection as the reason for having sex offender registration and notification laws. These justifications for registration and public access typically relate to the behavior of sex offenders that are required to register, i.e. specific deterrence or target hardening, rather than to general deterrence among those that have yet to commit a crime. The first line of Maryland's Sex Offender registry website notes that the information on the

[7] The two Supreme Court cases were Smith et al. v. Doe (Alaska) and Connecticut Department of Public Safety et al. v. Doe. The Missouri Supreme Court case was Jane Doe I, et al., Appellants, v. Thomas Phillips, et al., Respondents, 11 where Missouri sex offenders filed to challenge the constitutionality of Missouri's version of Megan's Law.

website is "…provided as part of the State's effort to protect children and others from those with histories of crimes against children and other sexual offenses." [8] Several states also refer to sex offender's high rates of recidivism. For example, Mississippi's legal code states that "[t]he Legislature finds that the danger of recidivism posed by criminal sex offenders and the protection of the public from these offenders is of paramount concern and interest to government" (Miss. Code Ann. § 45-33-21 (2001)). Most studies, however, have shown that sex offenders are less likely to reoffend than many other types of criminals (Langan, Schmitt, and Durose 2003, Langan and Levin 2002, Ohio Department of Rehabilitation and Correction Bureau of Planning and Evaluation 2001, Arizona Department of Corrections 1998)

A public registry could increase public safety through several means including: reducing the incentive to commit sexual offenses, reducing recidivism, target hardening, and increased police monitoring of known sex offenders. Registration and public access increases the cost of committing a sexual offense for an offender not on the registry. Once released from jail an offender would have to report to a local authority at least once a year (if not more) in order to verify his location for anywhere from 10 years to life. His identity may be revealed to the public via the internet, or through community meetings and flyers. Levenson and Cotter (2005) found that one-third of the sex offenders they surveyed in Florida experienced some sort of adverse event from having to register, including harassment and job loss; and a majority suffered negative emotions such as stress or hopelessness. This increased cost could help deter non-registered offenders from committing a sexual offense and thus increase public safety.

The registry could lower recidivism through its affects on the offenders already on the registry. Since offenders are required to register with a local authority, the local police or sheriff knows them and is aware of their current location. This increased monitoring could raise the

[8] http://www.dpscs.state.md.us/onlineservs/sor/

probability of an offender being caught if he chooses to recidivate and thus increase the expected cost of reoffending conditional on being on the registry. Having sex offender's information on the web for public access could also decrease further crime through target hardening. The increased awareness of the offender among members of the neighborhood could cause them to change their behavior and thus increase the cost of finding a victim for offenders on the registry. In their survey of sex offenders, Levonson and Cotter (2005) did find that some sex offenders (22%) thought that registration and community notification helped them to prevent reoffending.

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

Further, being on the registry already eliminates the possible deterrent effect of having to register for committing a sex crime. So again the opportunity cost of committing a future sexual offense is lowered because the offender is already experiencing the costs of reintegration and will continue to do so whether or not they reoffend. Prentsky, Knight, and Lee (1997) reflect the attitude that community notification may increase recidivism through increased stressed caused to offenders by "threats of bodily harm, termination of employment, on-the-job harassment, and forced instability of residence," all of which were experienced by at least some offenders in the above cited surveys (Prentsky, Knight, and Lee 1997 p. 9). A similar conclusion is reached by Prescott and Rockoff (2008) who find evidence that recidivism rates of registered offenders may be higher rather than lower.

3. Panel Data

3.1. Data Description

In order to test the effectiveness of registries I used state panel data which includes information on when each state enacted their registry, when they first allowed public access on the internet, crime rates and controls. No comprehensive repository of information on each state's sex offender registry existed and so for each state I collected data on the date the registry was enacted, the date the registry was placed on the internet for search by the public, and whether the registration requirement was retroactive (and if so, to what date). I collected the data through a variety of means including state websites, personal contact with relevant representatives through phone or email, information collected by the Klass Kids Foundation, and state legal codes. Prescott and Rockoff (2008), Table 1, show that verifying these dates can be a difficult task by showing that researchers tend not to agree on the dates registries became effective. Wherever feasible I have cross-checked my dates to ensure that they were as accurate as possible. The dates of enactment of both the registry and public access via the internet, as well as retroactive dates, are presented in Table 1.[9]

Most states enacted their registry laws after Congress passed the Jacob Wetterling act in 1994. Some states, such as California, Washington, and North Dakota, had registries before the 1994 law. The earliest a state put any portion of the registry on the internet was 1997, and Oregon and South Dakota were the last to do so in the summer of 2006. Figures 1 and 2 show CDF graphs which illustrate the rapid growth in registration and internet access after their initial introductions.

States vary as to whether their registration requirement was retroactive or not, that is some states require registration for convictions that took place prior to registry enactment. For the purpose of this study I define a state's registration requirement to be not retroactive if the state's registry was theoretically empty on the day it began. For example, Alabama's sex offender registry was enacted on September 6, 1967 and requires sex offenders released on or after September 6, 1967 to register and was therefore theoretically empty on the day it began even though it applied to crimes that were committed before the enactment date. 18 states have registry requirements that are retroactive, while 33 do not (Washington, D.C. is included). Of those that are retroactive, 9 are completely retroactive – i.e. the law applies both retrospectively and prospectively to any sex offender ever convicted of a sex offense.

The number of sex offenders on the registry may also be important to this analysis. However, finding data on historical registry sizes is difficult. Spotty information exists, with individual data points coming from many varying sources. Since we would be collecting data from differing sources it is hard to control for how the agencies verify the information and

[9] Prescott and Rockoff (2008) only use the 13 NIBRS states in their data, and we disagree on dates of enactment for 3 states - Connecticut, Kentucky, and North Dakota. I have included in Appendix A more thorough justifications for those 3 state registration dates.

where they get it from, so it is not clear we would be getting an accurate picture. One thing that can be measured with a better degree of accuracy is the stock of years of offenders that could possibly be on the registry, which will be correlated with the size of the registry (more possible years of offenders should lead to more registered offenders). So, for example, in 1994 Alabama's registry had been in place for 28 years and was not retroactive, and therefore had 28 possible years worth of released sex offenders on the registry; in 1994 New Hampshire's registry had been in place for 1 year, but was retroactive to 1988 and therefore had 6 possible years worth of offenders on it. For each state-year I calculated this value.

To complete the panel data I combined the registry date information I collected with data on forcible rape incidences (as opposed to statutory rape) and forcible rape/sex offense arrests per state from 1983 to 2003 from the Federal Bureau of Investigation's Uniform Crime Reports (FBI UCRs) and demographic characteristics from the Census Bureau. The FBI breaks up UCRs into two categories, Part I and Part II. Data on all known offenses are reported for Part I crimes, whereas only data on arrests are reported for Part II crimes. Forcible rape is considered a Part I offense, thus states report data on all known forcible rapes. This means that if a rape is reported to a police department but an arrest is never made that rape is still reported to the UCR as an incident. Sex offenses excluding rape and commercial prostitution are considered Part II offenses. Thus, for sex offenses other than forcible rape only arrest rates are available.

3.2. Empirical Strategy

I use a fixed effects regression model to determine the effectiveness of registration and public access in reducing sexual crime rates. Two difference specifications are used with two different dependent variables: the natural log of the rape incidence rate and the natural log of the sex offense arrest rate, natural logs are used so that coefficients can be read as percentage change in the crime rate. A dummy variable for the existence of a registry is set equal to 1 if a registry existed in that state in that year and 0 otherwise and is used to test the effect of the registry. Another dummy variable, web, equals 1 if the registry was searchable on the internet in that state and that year and 0 otherwise. Data from Massachusetts and Arizona are dropped from the regression.[10] Controls include state and year fixed effects, population density, real per capita income, real per capita unemployment payments, one year lagged incarceration rates, and demographic characteristics (Aryes and Donohue (2003), Kuziemko and Levitt (2004)**)**.

The regression specification is then as follows:

$$\ln crime_{st} = registry_{st}\beta + internet_{st}\theta + stock_{st}\alpha + \delta_t + \gamma_s + X_{st}\varphi + \varepsilon \qquad (1)$$

Where the subscript *s* represents the state and *t* represents the year, 1985 to 2003. Registry is a dummy set equal 1 if the state had a registry in that year, similarly internet is a dummy equal 1 if the state allowed access to the registry on the internet in that year. $stock_{st}$ measures the stock of years of offenders that could possibly be on the registry.[11] δ_t are time fixed effects, γ_s are state fixed effects. X is the vector of controls. The dependent variables takes on either of 2 definitions of sex offense rates as defined above.

I will also consider that the effects on crime rates may be different in states with retroactive registries versus states with non-retroactive registries. One might expect a difference because with an empty registry there is no effect of police monitoring or increased probability arrest yet, there is only the deterrent effect on new offenders. On the other hand, a state whose

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

[11] There may be reason to believe that the stock of offenders should have a varying effect on crime as the stock grows, but adding in a squared term did not have any effect on the coefficients and the coefficient on the squared term was extremely close to zero, and thus I used a linear specification.

registry is full would maintain both effects**.** I will do this in two ways; one is to add in interaction term between the registry beginning and the registry being retroactive (so this is essentially a dummy which equals one in the year in which the registry begins if that registry was also retroactive**.** I will also do these regressions separately for states with retroactive registries and states with non-retroactive registries, with the data restricted to three years prior to and three years after registry beginning, to see if there is a difference in the effects.

Endogeneity can be a problem when dealing with crime policy like this. If the introduction of a registry was endogenous to the rate of sexual offenses then states with higher sexual offense rates would have implemented registries earlier, thus distorting the results. The history of sex offender registries suggests that the date of creation is largely exogenous and due to random shocks. It is revealing that all major laws pertaining to sex offender registration were enacted based on high profile cases, not on a multitude of cases in general. New Jersey pioneered Megan's Law after the brutal rape and murder of Megan Kanka by a convicted sex offender living in her town; not because New Jersey was experiencing a high incidence of these events. Similarly, the Jacob Wetterling Act, Jessie's Law, and the Adam Walsh Act are named after children who were part of highly publicized sexual crimes. These singular events can be considered random shocks and not representative of the amount of crime in each state. Further, the passing of the Jacob Wetterling Act in 1994 left the mid-1990s the decision as to whether to have a registry out of the state's hands. Those states that enacted their registries before 1994 – California, Idaho, Kansas, Louisiana, Minnesota, Missouri, Montana, New Hampshire, North Dakota, Oregon, Texas, and Washington – are on the whole not states considered to have high rates of sex offense. In fact, if you rank the states by their 1995 rape incidence rate, these states tend to fall in the middle of the distribution. The endogeneity issue will also be explored further in the empirical results.

3.3. Results

Figures 3 and 4 show a graphical analysis of the data to give a preliminary idea of whether registration or internet access to the registry had any effect on crime rates. Each graph shows crime rates over time, with time zero being the point in time when registration (or internet access) began, and the x-axis thus showing time until or since the registry (internet access) began. Rape rates, sexual offense arrest rates, violent crime rates, and property crime rates are graphed. In figure 3 we see the results with time 0 indicating beginning of registration. For rape rates we see no real trend difference after the registry was introduced, and as expected the result is similar for violent crime and property crimes. With sex offense arrest rates, however, there does appear to be a downward change after registration begins. Figure 4 shows results with time 0 indicating the beginning of internet access to the registry. This time there appears to be no break in trend for either rape, sex offense arrests, or violent crime after time 0. There does, however, seem to be a leveling off of property crime rates. The overall result from these graphs seems to indicate that registration and internet access had little effect on sexual offense crime rates.

The results from the fixed effects regression using the state panel data are presented in Table 2. The first specification uses the natural log of the rape incidence rate as the dependent variable. The variables registry and web are the dummy variables which are set equal to 1 when a state had a registry or public access via the web in that year. The coefficient on the registry variable is positive though extremely small, indicated about a 0.3% increase in rapes. The web coefficient is larger and positive, indicating about a 5.4% increase in rape after internet access.

However, neither of these coefficients is statistically significant. The stock of offenders also has no statistically significant effect on crime, though the point estimates are positive indicating if anything having more offenders on the registry increases crime rates.

Although no measure of crime is perfect, incidence reports are one of the better proxies that we have. Since incidence reports were only available for rapes and not for other sex offense, the second specification in Table 2 uses arrests for sexual offenses as the dependent variable to try to determine the effect on a broader range of sex crimes. In this specification there is a positive coefficient on the existence of a registry in the state, with the coefficient indicating a 10% increase in sexual assault arrest rates after the registry went into effect, although it is not statistically significant. However, putting the registry on the internet has a significant negative effect, showing a significant decrease in arrests for sexual offenses of approximately 19%.[12]

Though the interpretation of the coefficients on rape incidences is rather clear, it is not obvious how to interpret the coefficients on arrests, and thus what that significant decline truly means. The interpretation depends on the percentage of crime incidences for which there is an arrest, or the arrest ratio, $ArrRatio = \frac{arrests}{incidents}$. If arrests decrease and the number of actual incidents stay the same then a decline in arrests does not imply that registries are effective. If arrests decrease and incidents decrease then registered could be seen as effective. A clearer picture could be seen if there was data on incidents of sexual assault, but unfortunately the UCR only reports arrests for Part II data. If we assume that arrests are a good proxy for incidences, however, this could indicate that putting the registry contents on the internet did decrease sexual offenses.

[12]Per a referee's suggestion, I also ran each regressions without control variables to see how concerned we should be about unobservable and found coefficients were all similar in sign and magnitude even without controls.

Although I believe the history of sex offender registries points to a lack of an endogeneity issue**,** the panel data also can be used to address the possibility of endogeneity empirically. In order to do this I added a variable endog which equals 1 in the year *before* the registry began and 0 elsewhere, and webendog which equals 1 in the year before the registry went on the internet and 0 otherwise, to the two regressions. The coefficients and standard errors are shown in the fourth and fifth specification in Table 2. If anything, the coefficient indicates that the year before a state enacted registries rape incidents were slightly lower, a similar result is found for sexual assault arrest rates, suggesting that the creation of the registry was not driven by higher rape or sexual assault rates and thus may be treated as plausibly exogenous. Admittedly it could be that sex offenders knew a registry was going to be enacted and changed their behavior accordingly; however the previous results do not show offenders changing behavior much in response to actual registries, so it seems unlikely that there was a lot of behavioral change in anticipation of the registry. However, history of enactment laws is probably a stronger argument and at the very least the empirical results do not dispute the assumption of non-endogeneity.

Panel B of Table 2 runs the same regressions for other crimes – violent crime incidences (murder, robbery and aggravated assault – rape is excluded) and property crime incidences (burglary, larceny, auto theft, and arson). The results for the most part show small, statistically insignificant effects of the registry on other crimes, as we would expect. They do, however, suggest that violent crime incidences increased significantly after the registries were made available on the internet.

Finally, I reran the specifications in Table 2, columns 1 and 2, but added a dummy which equals one in the year the registry begins if the registry was also retroactive, to see if there are differing effects on retroactive and non-retroactive states. The results are presented in Table 3.

The results show that retroactive states experienced slight declines over non-retroactive states, but the difference is not statistically significant. I also reran each specification for only those states where the registration requirement is retroactive and for those that are not, limiting the data to three years prior to and three years after registry the registry begins. None of the results are significant, though imply that rape incidences decreased more in retroactive states than in non-retroactive states; and that sex offense arrests decreased in retroactive states but increased in non-retroactive states after the registry.

On the whole the panel data does not present strong evidence that registries were effective in reducing rape or sexual offenses, though does offer some limited evidence of reducing sexual offense rates.

4. Recidivism Analysis

4.1. Data description

In order to test the effect of registration requirements on sex offender recidivism I used a dataset that tracked the behavior of individual offenders. The Bureau of Justice Statistics (BJS) collected data on a subset of prisoners released from prison in the year 1994 in fifteen states: Arizona, California, Delaware, Florida, Illinois, Maryland, Michigan, Minnesota, New Jersey, New York, North Carolina, Ohio, Oregon, Texas and Virginia, which included *all* sex offenders released into those states (Langan, Schmitt, and Durose 2003). [13] Arizona, however, is not used in my analysis.[14] The information in the dataset includes demographic characteristics on the released prisoner such as race, date of birth, and gender as well as criminal history, including

[13] For an application of this data to Three Strikes laws in California see: Helland, E. and Tabarrok A. 2006. Does Three Strikes Deter? A Non-parametric Estimation. Journal of Human Resources. 42(2): 309-330.

[14] Arizona was dropped for the same reason it was not used in the panel data analysis. Determining when Arizona began its sex offender registry proved an impossible task. Mixed results were found from different sources including talking to different people within the Arizona Department of Public Safety, because no date could be cross-checked I chose to not use Arizona in the analysis.

past arrests and convictions. The BJS then tracked these prisoners for three years after their release adding information on all subsequent arrests, convictions and sentences.

The BJS dataset is useful because I can compare the behavior of offenders who would have had to register upon release in 1994 with those that would not have been required to register. California, Minnesota, Oregon, Texas and partially Florida enacted registries before 1994, the beginning of the study period. [15] Virginia, New Jersey and Delaware enacted registries during 1994, the year of initial release. The law in Delaware, however, only applies to offenders convicted on or after June 27, 1994, and thus does not apply to any prisoners in this study. Virginia and New Jersey laws include anyone in state custody on the date the law was enacted, thus any offender released in Virginia after July 1, 1994 and any offender released in New Jersey after October 31, 1994 would be required to register.[16]

The BJS data contains information 10,510 prisoners whose offense for which they were released in 1994 was a sex offense. Because registration laws vary across the states, I further restricted the data to only those offenders who appear to have remained in the state that released them. I did so by dropping offenders who were arrested out of state at any point during the three year follow up (N=620).[17] Also, most states do not require registration of and/or public access to information about juvenile offenders, and juvenile offenders are often subject to different length and penalty requirements. Due to lack of information about whether the offender was tried as a juvenile or adult, and to avoid ambiguity, I also dropped offenders who were juveniles at the

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

[16] Virginia and New Jersey could have served as ideal states within which to test the effect of registration by comparing the two populations (registered and unregistered but released in the same year and same state). However, this analysis suffers from an extremely small sample size. Of the 247 sex offenders released into Virginia, only 12 were rearrested for a sex offense, 5 that would have had to register and 7 who would not have. It would be difficult to conclude anything from this small sample.

[17] Running the regressions with the prisoners arrested out of state included does not significantly change the results

time of their admission (N=108). Finally the 138 offenders released into Arizona were dropped. After these adjustments 9,623 sex offenders released in 1994 are included in the analysis.[18]

By examining the crime the offenders committed, the date the registry law was enacted in the state that released them, and the relevant laws regarding registration in that state, I determined that 4,626 (47.39%) offenders should have been required to register upon their release in 1994. All offenders released in Minnesota, California, Oregon and Texas would have had to register because the laws in those states went into effect before 1994 and indicated that offenders released after that date would have to register. Offenders released in Virginia and New Jersey would have had to register if they were released after the date the respective states registry went into effect; 130 of VA's 247 released offenders had to register, 56 of NJ's 404 had to register. No offender released in DE would have had to register because the law applied to only those offenders *convicted* on or after 6/27/1994. Similarly, Texas law in 1994 indicated that only those convicted after 9/1/1991 would have to register. I used admission date as a proxy for their conviction date and only those admitted to prison after 9/1/1991 then released in 1994 were considered as having to register; 366 of TX's 665 offenders had to register. Florida began registering sexual predators (a more harsh distinction than sexual offender) for those predators who *committed* offenses on or after 10/1/1993. 60 offenders released in Florida were admitted after 10/1/1993, however to qualify as a sexual predator the offense must have been serious, and due to the short time served (approximately one year) I assumed none would qualify.

4.2. Empirical Strategy

I first compare subsequent recidivism rates of offenders in each group (registered and unregistered) using three definitions of recidivism: subsequent arrest for rape, subsequent arrest for any sexual offense, and subsequent conviction for a sexual offense conditional on an arrest

[18] I also dropped the 21 prisoner's who died during the 3 year follow-up period.

after release.[19] After this straightforward comparison of rates across the two groups I use a probit model to compare the recidivism of the two groups so that controls may be included for differences across the groups and across states. I created a dummy variable, register on first release (rofr), in order to test the effect of registering. For those offender's who would have had to register upon their release rofr=1, for all others rofr=0. I ran the model using the three definitions of recidivism.

Due to the nature of the variable that indicates registration, being 0 or 1 for entire states in some cases, I cannot utilize state fixed effects to account for differences across states. Therefore, in the probit model state level controls are used in addition to individual level controls to try to control for some of those differences. Individual level controls consist of age at first arrest; race; gender; sentence length; whether the individual went to jail for rape, sexual abuse, child molestation, or sodomy; and the number of rape arrests and other sex offense arrests before their release in 1994. State level controls are included for the state releasing the offender. These include the police rate per 1000 in the state population, percent of the population of the state residing in urban areas, punishment severity (average sentence length for rapist in study), past rape incidence rates and sex offense arrest rates in the state, a dummy that indicates whether a state elects rather than appoints their judges, and variables that help determine the general social climate of the state.

The regression equation is:

$$recidivist = rofr\beta + I\theta + S\phi + \varepsilon \qquad (2)$$

[19] There are some prisoners in the dataset who were convicted of a crime after their 1994 release for which they were arrested before they entered prison for their 1994 release. These types of situations are not included.

Where recidivist is a dummy indicating whether the offender recidivated according to one of the three definitions (each is done individually). Rofr is the dummy variables indicated above. *I* is the vector of individual controls and *S* is the vector of state level controls.

I also look at the results of a Cox Proportional Hazard Model. For any time t, this model reports the hazard of an individual proportional to the baseline. The hazard is defined as

$$h_i(t) = \exp(rofr\beta + I\theta + S\phi)h_o(t) \qquad (3)$$

Where $h_o(t)$ is the baseline hazard at time t, $h_i(t)$ is the hazard of the i[th] individual at time t, and $rofr, I,$ and S are defined as above. The estimated coefficient β can then be used to determine the ratio of the hazard for those on the registry and those not on the registry at any given time:

$$\frac{h(t \mid rofr = 1)}{h(t \mid rofr = 0)} = \frac{\exp(rofr\beta + I\theta + S\phi)h_o(t)}{h_o(t)} = \exp(rofr\beta + I\theta + S\phi) \qquad (4)$$

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

4.3. Results

Summary statistics for demographic information and criminal records are presented in Table 4 Panel A broken down into registered and unregistered offenders. There do appear to be general differences in these categories across registered and non-registered offenders, which can probably be attributed to the state they were in. The large difference in the percent of sex offenders who are Hispanic is most likely because California and Texas both enacted their registry laws prior to 1994 thus all offenders released in those two states would have had to register and both states have a relatively high percentage of Hispanic releasees (30.9% and 21.8% respectively) populations. Table 4 Panel B presents a tabulation of the original offense of the sex offenders that had to register and those that did not. Sexual abuse, rape, and child molestation make up a majority of the crimes. The proportion of rapists, attempted rapists, sexual abusers, child molesters, attempted child molesters, and sodomizers is significantly different between the two groups. Based on the evidence in these tables, whether the sex offenders original crime was rape or attempted rape, child molestation or attempted child molestation, sexual abuse, or sodomy as well as demographic characteristics will be controlled for in the regression analysis.

Table 5 uses the BJS data and three definitions of recidivism to compare the percent of sex offender released in 1994 that recidivated within 1, 2, and 3 years after release between those that had to register and those that did not. The three definitions are: arrested for a sex offense, convicted of a sex offense conditional on arrest after release, and arrested for rape. The differences are small and not statistically significant for the percent arrested for a sex offense or percent convicted of a sex offense between the groups, indicating that registration caused no change in behavior. However, it appears that sex offenders that had to register are significantly more likely to be arrested for rape than those not registered. Interpretation of this difference is difficult. If registries were effective in the way that policy makers expected them to be, then reduced recidivism by registered offenders would imply the difference should be the other way around.

This preliminary analysis does not fully support the hypothesis that registration reduces recidivism among sex offenders. However, summary statistics showed that there are some fundamental differences between the registered and unregistered groups that we were unable to control for in Table 5. Table 6 presents results from the probit models with the same three definitions of recidivism used as well as various control variables at the individual and state level. The more arrests an offender had for rape or other sex offenses the more likely they are to recidivate across all definitions of recidivism. More police per capita seems to cause less recidivism when arrest is used as the definition, but has an insignificant effect on convictions. More sex offense arrests in 1993 in a state decreases recidivism across all categories.

The variable of interest, however, is the dummy variable which represents whether the offender should have had to register upon their release in 1994, “registered on release”. In the three specifications that refer to sex offense recidivism the coefficient is positive but insignificant. This result pretty much mimics the straight forward comparison of rates in Table 5 except that the coefficient on rape arrest is insignificant. But the main conclusion is that it appears sex offender recidivism is not reduced by having to register and if anything it is slightly increased. Interestingly the coefficient on arrest for an offense other than a sex offense is significant, indicating that registered offenders are almost 8% more likely to be arrested for an “other” offense. This could be the result of increased monitoring or general difficulty reintegrating that causes them to commit other crimes as well.

The results from the Cox Proportional Hazard model yield similar conclusions and are shown in Table 7. For each definition of recidivism, at any given time t the hazard for those offenders on the registry is significantly *higher* than the hazard for those not on the registry. Again, these are not the results one would expect if registries were effective. An offender who should have had to register appears to behave no differently, or possibly worse, from one that did not have to register; having to register does not seem to reduce recidivism among sex offenders.

The definitions used for recidivism all involve arrests or convictions, not actual rates of offense. Obviously, it is impossible to measure the true number of offenses that a sex offender committed after release. In the case of this BJS data, arrests are the closest proxy we have to a measure of offenses committed. It is possible that offenders on the registry are more likely to be arrested for a crime due to the fact that they are on the registry and thus under scrutiny for any sex offense that arises. The results also showed, though, that conditional on arrest after release the two groups had no statistically significant difference in conviction rates.

5. Locational Analysis

5.1. Data Description

To understand whether registries are potentially effective, I combine data on locations of crime in Washington, D.C. with data on locations of registered sex offenders. I have data on locations of crime in Washington, D.C. from 1/1/1997 to 7/30/2003 from the Washington, D.C. Metropolitan Police Department.[20,21] This data contains the type of offense committed (sexual abuse, arson, burglary, theft, theft from auto, stolen auto, homicide, assault with a deadly weapon or robbery), the date and hour of the day the crime took place, and the block where the crime happened.

The Washington, D.C. Metropolitan Police Departments sex offender registry website has information about all Class A and B sex offenders. [22] I obtained information on each offender's home and work address as well as date of initial registration from the website.[23] Unfortunately, I only have information on offender's currently registered addresses; I do not have information about changes in the registry information over time. Thus, I assume that offenders have not moved, i.e. that the address listed on the website has been their address since

[20] The MPD requires the following disclaimer: "These data reflect preliminary crime reports made by individual police districts to the MPD's Central Crime Analysis Unit. These data DO NOT reflect official index crime totals as reported to the FBI's Uniform Crime Reporting Program. These data are subject to change for a variety of reasons, including late reporting, reclassification of some offenses, and the discovery that some offenses were unfounded."
[21] See Klick, Jonthan and Tabarrok, A. 2005. Using Terror Alert Levels to Estimate the Effect of Police on Crime. *Journal of Law and Economics*. 48(1) for another application of this data.
[22] Washington DC uses a 3 tiered classification system, A, B and C. C is the lowest tier. Examples of Class A crimes are forcible rape and first degree child abuse; examples of Class C crimes are kidnapping with the intent to commit a sexual offense and threatening to commit a sexual offense.
[23] http://mpdc.dc.gov/mpdc/cwp/view,a,1241,q,540704,mpdcNav_GID,1523,mpdcNav,|.asp

their initial registration.[24] Figure 3 presents a map of where registered sex offenders in Washington DC live. While there are some parts of DC with few or no sex offenders, they appear to be fairly spread out over the city. Figure 4 presents a map of where registered sex offenders work in Washington DC, if the offender has reported a work address.

5.2. Empirical Strategy

I broke the data up into year-month time groups, and counted the number of offenders living in a particular census block in any given year-month. This can change over time as new offenders begin to register. I similarly count the number of sex abuse incidents in a given year-month-census block, and use this as the dependent variable. First I run regressions with different crime rates per 1000 in the population as the dependent variables, including sex abuse incidences and violent and non-violent crime incidences. The regressions include a variable for the number of sex offenders living in that census block-month to determine the effects of the registered sex offenders themselves on crime rates. Also included in the regressions are demographic controls at the block level such as percent of the population that is Black, Hispanic, female, or in a certain age group as well as the percent of housing that is renter occupied. I then run the same regressions with census block fixed effects instead of the controls.

Second, I try to understand the difference in the effects on sex crime rates between the time when there was just a registry versus when there was a registry and public access via the internet. I do so by creating a variable that is the number of registered offenders "before web" and another that represents the number of registered offenders that are both registered and on the internet. I then use a similar specification as in previous regressions with census block fixed

[24] Admittedly, this is a big assumption, but I believe given the short time span I am analyzing it may not be entirely unreasonable. I hope in the future to be able to repeat this analysis with changes over time in offender addresses

effects. The results will help us understand if knowing the location of sex offenders in Washington, DC helps to predict the location of sexual abuse.[25]

5.3. Results

Table 8 presents the results from the first set of regressions. The variable of interest is offenders per 1000 living in the block-group. The first specification uses sex abuse per 1000 people in a census block as the dependent variable. The results indicate a slight decrease in sex abuse crimes near where sex offenders live that is not statistically significant. Note that the average block has approximately 0.92 sex abuse incidences per 1000. However, the second specification in Table 8 runs the same regression for incidences of violent and non-violent crime per 1000 and shows that having one more sex offender living in a block decreases the rates of non-violent and violent crime, though again this is not statistically significant. This seems to indicate that, if anything, sex offenders tend to live in less crime-ridden areas. More importantly, it shows that knowing where a sex offender lives does not tell us much about where sex crimes will take place. The last specification in Table 8 presents the results when fixed effects are used to control for differences across blocks instead of observables. These results are similar to the first specification.

The data offer a unique opportunity to study the difference in the effect on sex crime rates when there was just a registry versus a registry and public access to it via the web. There were nine months in Washington, D.C. between June 2000 and March 2001 where the registry existed but the contents were not available online. Table 9 attempts to differentiate between these two periods, with census block fixed effects used to control for differences across blocks. The results

[25] I also imitated the analysis from the panel data section, by regressing sex abuse on whether a registry existed as well as time and season dummies, and found that, similar to the national data, the registry appeared to have no statistically significant impact on sex abuse rates across the city.

show that there is little difference in the effect of having a registered sex offender in a census block when this information is not public versus when it is public.

The results from analysis of the data on crime location along with sex offender location in Washington, DC indicate that knowing that a sex offender lives on your block does not give you information about rates of sexual abuse in that block.

6. Conclusion

The data in all three datasets does not strongly support the effectiveness of sex offender registries. The national panel data did not show a significant decrease in the rate of rape or the arrest rate for sexual abuse after implementation of a registry or allowing access via the internet. The BJS data which tracked individual sex offenders after their release in 1994 did not show that registration had a significantly negative effect on sex offender recidivism. And the results from the DC crime data did not show that knowing the location of sex offenders in different census blocks can help predict locations of sexual abuse. The fact that this pattern of non-effectiveness shows up in all the datasets does not support the conclusion that the sex offender registries are successful in meeting their objectives of increasing public safety and lowering recidivism rates.

Aside from the direct policy interest, understanding whether sex offender registries work is potentially important because they are a precedent for other types of registries. Tennessee, along with Illinois, Montana and Minnesota, recently began a public registry for methamphetamine offenders, and several other states are considering such registries.[26] Not only could different types of criminals be registered but technological advances, such as global positioning systems (GPS), mean that the actual physical location of individual criminals can be tracked on a twenty-four hour basis. Several states have passed laws that require high risk

[26] http://www.tennesseeanytime.org/methor/, http://www.star-gazette.com/apps/pbcs.dll/article?AID=/20060824/OPINION01/608240320/1004

offenders to be monitored by GPS so police know their exact location at any given moment.[27] Another use of GPS involves trying to keep children away from registered offenders through GPS capabilities in their cell phones. CATS Communications, a telecommunication company based in California, now offers parents a service that will alert them if their children, armed with a GPS enabled cell phone, come within a certain distance of a registered sex offender's address.[28] In addition to these technological changes to sex offender management, Congress recently passed the Adam Walsh Child Protection and Safety Act of 2006. This new act requires states to enact stricter registration requirements, including an increase in the penalty for not registering and more frequent verification of sex offender locations. With all these developments in registration, sex offender registries should be evaluated before committing to further extensions and we should look critically at any attempts to extend registration to other criminals.

[27] Examples include: Florida, Missouri, Ohio & Oklahoma. For more information see for example: http://www.mass.gov/courts/probation/pr051105.html
[28] https://www.catscommunication.com/news/28.htm

References:

Adkins, Geneva, Huff, D. and Stageberg, P. December 2000. The Iowa Sex Offender Registry and Recidivism. Iowa Department of Human Rights, Division of Criminal and Juvenile Justice, Planning and Statistical Analysis Center. Available online at: www.state.ia.us/government/dhr/cjjp/images/pdf/01_pub/SexOffenderReport.pdf

Arizona Department of Corrections. 1998. Sex Offender Recidivism. Fact Sheet 98-06. Available online at: http://www.azcorrections.gov/FACTSHEETS/Fact%20Sheet%2098-06.htm.

Ayres, Ian and Donohue, J. 2003. Shooting down the More Guns, Less Crime Hypothesis. *Stanford Law Review* 55: 1193.

Bertrand, Marianne, Duflo E. and Mullainathan, S. 2004. How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics* 119(1): 249-275.

Braithewaite, John. 1989. *Crime, Shame, and Reintegration*. New York: Cambridge University Press.

California Office of the Attorney General. (n.d.). Megan's Law - Sex Offender Registration and Exclusion Information. Available online at: http://www.meganslaw.ca.gov/sexreg.htm.

Donnelly, Sheila and Lieb, R. December 1993. Washington’s Community Notification Law: A Survey of Law Enforcement. *Washington State Institute for Public Policy*. Available online at http://www.wsipp.wa.gov/rptfiles/surv_law.pdf.

Friedman, David and Sjostrom, W. 1993. Hanged for a Sheep: The Economics of Marginal Deterrence. *The Journal of Legal Studies* 22(2): 245-266.

Hellend, Eric and Tabarrok, A. 2004. Using Placebo Laws to Test “More Guns, Less Crime”. *Advances in Economics Analysis & Policy* 4(1): Article 1.

Kuziemko, Ilyana and Levitt, S. 2004. An Empirical Analysis of Imprisoning Drug Offenders. *Journal of Public Economics* 88(9-10):2043-2066.

Langan, Patrick and Levin D. 2002. Recidivism of Prisoners Released in 1994. Special Report. NCJ 193247. Washington, D.C.: U.S. Department of Justice, Office of Justice Programs, Bureau of Justice Statistics.

Langan, Patrick, Schmitt, E., and Durose, M. 2003. Recidivism of Sex Offenders Released from Prison in 1994. NCJ 198281. Washington, D.C.: U.S. Department of Justice, Office of Justice Programs, Bureau of Justice Statistics.

Levonson, Jill and Cotter, L. 2005. The Effect of Megan’s Law on Sex Offender Reintegration. *Journal of Contemporary Criminal Justice* 21(1): 49-66.

Linden, Leigh and Rockoff, J. 2006. There Goes the Neighborhood? Estimates from the Impact of Crime Risk on Property Values from Megan's Laws. NBER Working Paper 12253. Available online at: http://www.nber.org/papers/w12253

Malesky, Alvin and Keim, J. 2001. Mental Health Professionals' Perspectives on Sex Offender Registry Web Sites. *Sexual Abuse: A Journal of Research and Treatment* 13(1): 53-63.

Mears, Bill. 2003. Supreme Court Upholds Sex Offender Registration Laws. *CNN*. http://www.cnn.com/2003/LAW/03/05/scotus.sex.offenders/index.html

Ohio Department of Rehabilitation and Correction Bureau of Planning and Evaluation. April 2001. Ten-Year Recidivism Follow-Up of 1989 Sex Offender Releases. Available online at: www.drc.state.oh.us/web/Reports/Ten_Year_Recidivism.pdf.

Prentsky, Robert, Knight R. and Lee, A. 1997. Child Sexual Molestation: Research Issues. NCJ 163390. Washington, DC.: U.S. Department of Justice, National Institute of Justice.

Prescott, J. and Rockoff, J. 2008. Do Sex Offender Registration Laws Affect Criminal Behavior? NBER Working Paper Series No. 13803.

Schram, Donna D. and Milloy, C. October. 1995. Community Notification: A Study of Offender Characteristics and Recidivism. *Washington State Institute for Public Policy*. Available online at: http://www.wsipp.wa.gov/rptfiles/chrrec.pdf.

Tabarrok, A., and E. Helland. 1999. Court Politics: The Political Economy of Tort Awards. *Journal of Law and Economics* XLII,1(April): 157-188.

Vargas, Theresa. 2006. Sex Offender Sues VA to Keep Name off Web. *Washington Post*, B01, 11 September.

Walker, J.T., Maddan, S., Vasquez, B.E., VanHouten, A.C., Ervin-McLarty, G. 2005. The Influence of Sex Offender Registration and Notification Laws in the United States. Arkansas Crime Information Center Working Paper.

Zevitz, Richard G., Crim D. and Farkas, M. 2000. Sex Offender Community Notification: Managing High Risk Criminals or Exacting Further Vengeance? *Behavioral Sciences and the Law* 18:375-391.

Table 1 *Summary of significant dates*

State	Registry Begins	On Internet	Retro	State	Registry Begins	On Internet	Retro
AL	9/6/1967	8/1/1998	No (rel)	MT	1989	2001	No
AK	8/10/1994	6/12/1997	No (con)	NE	1/1/1997	2000	No (con)
AZ	6/1/1996	1998	All	NV	1/1/1998	5/1/2004	7/1/1956 (con)
AR	8/1/1987 (h) 8/1/1997 (o)	1/1/2004	No (con)	NH	1993	2001	1/1/1988
CA	1954	12/15/2004	No (con)	NJ	10/31/1994	2/21/2002	No
CO	7/1/1991 (c) 10/1/1998 (o)	7/1/2001	No (rel,c) No (con,o)	NM	7/1/1995	7/1/2000	No
CT	10/1/1998	1/1/1999	10/1/1988 (rel, v) No (o)	NY	1/21/1996	5/11/2000	No
DE	6/27/1994	11/1/1998	No	NC	1/1/1996	5/11/2000	No
DC	6/1/2000	3/1/2001	No	ND	1991	11/1/2001	7/31/1985 (con)
FL	10/1/1993 (v) 10/1/1997 (o)	10/1/1997	No (comm, p) No (rel, o)	OH	7/1/1997	1/1/2001	No
GA	7/1/1996	1998	No (con)	OK	11/1/1989	1/29/2005	No (comm)
HI	1/1/1996 (v) 7/1/1997 (o)	5/1/2005	All	OR	10/3/1989	6/29/2006	No
ID	7/1/1993	2002	No (con)	PA	4/21/1996	11/24/2004	All
IL	8/15/1986	7/1/2002	No (con)	RI	1992	4/13/2005	All
IN	7/1/1994	1/1/2003	All	SC	7/1/1994	6/21/2005	No (con)
IA	7/1/1995	7/1/1998	No	SD	1994	2006	All
KS	7/1/1993	4/24/1997	No (con)	TN	1/1/1995	7/1/1997	No
KY	7/15/1994	4/1/2000	No	TX	9/1/1991	1/1/1998	9/1/1970 (con)
LA	6/18/1992	5/1/2000	No (cust)	UT	3/30/1983	7/1/1998	No (rel)
ME	9/1/1996	10/1/2003	1/1/1982 (sen)	VT	9/1/1996	10/1/2004	No
MD	10/1/1995 (c) 7/1/1997 (o)	2002	No (comm)	VA	7/1/1994	7/1/1999	No (con)
MA		8/1/2004	8/1/1981 (rel)	WA	2/28/1990	3/1/2005	All
MI	10/1/1995	1999	No (con)	WV	1993	9/1/1998	All
MN	7/1/1991	1/1/1997	No	WI	12/25/1993	6/1/2001	No (con)
MS	1994	1997	All	WY	1994		1/1/1985 (sen)
MO	7/1/1979	6/18/2004	No (con)				

Notes:
I use the following abbreviations:

c: child offenders	v: violent offenders	h: habitual offenders
o: other offenders	rel: released	comm: committed
con: convicted	cust: in custody	sen: sentenced

Several states have different dates for different levels of offenders. This is indicated by the single letter in parenthesis. For example, Maryland began registering child sex offenders before other offenders. The Retro column indicates whether the registry was retroactive. If it was, a date is listed and when known whether it refers to the offender being released, convicted, sentenced, in custody, or whether the crime had to be committed on or after that date to be registratable. For example the law in Maine requires that anyone sentenced for a registratable offense on or after January 1, 1982 has to register; and this date is before the registry enactment date of September 1, 1996 and is therefore considered retroactive. For non-retroactive states, defined as having a theoretically empty registry on the date the registry became effective, they are listed as "no" and when known again list whether it was released on or after the date of enactment etc… So for example the law in Michigan is not retroactive, and applies to anyone convicted on or after 10/1/1995. Some laws apply both prospectively and retroactively to all offenders ever convicted of a sex offense, these are listed as "all".

Information was obtained from various sources including official state websites, www.klaaskids.org, legal resources and personal contact via telephone and email with state officials. When only a month and year were available the date is listed as the first of that month. When only year is available it is listed as such. Documentation of where information for each cell was obtained available upon request. Information for when Massachusetts began its sex offender registry left out because Massachusetts went through a series of court cases which alternately suspended and reenacted sex offender registration through the late 1990s, thus making any analysis of the effectiveness of the registry difficult. Information for when Wyoming first put its registry on the internet was unavailable, even after several phone calls and emails with Wyoming Sex Offender Registration ProgramUtah: Registry was made available to the public via the internet in 7/1998, but in 9/1998 a federal appeals case was filed (Femedeer vs. Haun) and the website was frozen, with no new data added or old data removed, until 12/2000 when the case ended in the State's favor and the website was once again operational. 1998 date is used because I assume offenders would not be as informed about court case as website and would assume they would be put on website.

Table 2: *Effect of existence of registries and internet registries on rape rates and sexual offense arrest rates*

Panel A: Regression Results

	(1) Rape	(2) Sex Offense	(3) Rape	(4) Sex Offense
registry	0.003	0.105		
	(0.024)	(0.072)		
web	0.054	-0.187		
	(0.041)	(0.096)		
Stock of years of offenders	0.012	0.032	0.010	0.033
	(0.010)	(0.028)	(0.010)	(0.027)
Population per square mile of land area	-0.004*	-0.002	-0.004*	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)
Real PC Income ($1000s)	-0.006	-0.018	0.269	-0.067
	(0.006)	(0.931)	(0.436)	(0.022)
Real PC Unemployment Insurance ($1000s)	-0.973**	-0.214	-0.973**	-0.260
	(0.305)	(0.668)	(0.308)	(0.664)
Real PC Income Maint. ($1000s)	0.283	-0.140	-0.007	-0.015
	(0.436)	(0.022)	(0.006)	(0.956)
Percentage of state population that is black, male, and aged 10-19	-0.248	0.686	-0.211	0.611
	(0.213)	(0.569)	(0.207)	(0.604)
pbm2029	0.256	-0.241	0.255	-0.240
	(0.204)	(0.457)	(0.202)	(0.467)
pbm3039	0.324	-0.102	0.283	0.035
	(0.200)	(0.493)	(0.197)	(0.509)
pbm4049	0.155	0.497	0.206	0.375
	(0.165)	(0.300)	(0.156)	(0.316)
pwm1019	0.022	0.189	0.020	0.194
	(0.066)	(0.142)	(0.066)	(0.150)
pwm2029	0.022	0.159*	0.025	0.133
	(0.044)	(0.075)	(0.046)	(0.080)
pwm3039	0.077	-0.035	0.078	-0.039
	(0.044)	(0.095)	(0.047)	(0.089)
pwm4049	0.344*	-0.115	0.351*	-0.117
	(0.130)	(0.179)	(0.132)	(0.184)
Incarceration rate per 1000 (lagged one year)	-0.043	0.047	-0.045	0.048
	(0.036)	(0.042)	(0.034)	(0.046)
Police per 1000	-0.092	-0.021	-0.094	-0.016
	(0.106)	(0.337)	(0.104)	(0.346)
endog			-0.017	-0.002
			(0.018)	(0.047)

webendog			0.013	0.023
			(0.025)	(0.038)
Constant	-2.017	1.621	-2.049	1.788
	(1.310)	(2.059)	(1.356)	(2.034)
Observations	886	886	886	886
R-squared	0.98	0.67	0.98	0.67
State fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Robust standard errors in parentheses				
* significant at 5%; ** significant at 1%				

Panel B: Other Crimes, controls suppressed for display

	(1)	(2)
	Violent Incidences(not rape)	Property Crime Incidences
registry	0.030	-0.017
	(0.037)	(0.021)
web	0.095*	-0.004
	(0.044)	(0.025)
Constant	2.031*	3.257**
	(0.936)	(0.802)
Observations	886	886
R-squared	0.97	0.99
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes
Robust standard errors in parentheses		
* significant at 5%; ** significant at 1%		

Notes: All dependent variables are natural logs of rates (i.e. rape is the natural log of the rape rate), so coefficients are interpreted as percentage change in the crime rate. Robust standard errors in parenthesis clustered by state. Registry (web)=1 if a state had a registry (internet access to the registry) in that year, 0 otherwise. Endog =1 in the year prior to the registry going into effect. Stock of years of offenders is the number of years worth of criminals that could possibly be on the registry. Data on demographic statistics from the US Census Bureau. Crime rates from FBI's *Uniform Crime Reports*. Violent Incidences are murder, robbery, and aggravated assault (the UCR also includes rapes, but that is excluded for the falsification test). Property incidences are burglary, larceny, auto theft and arson. All controls from Panel A regression are included in Panel B but coefficients are not displayed.

Table 3: *Effect of existence of registries on rape rates and sexual offense arrest rates: retroactive vs non-retroactive states*

	(1) Rape	(2) Sex Offense	(3) Rape	(4) Rape	(5) Sex Offense	(6) Sex Offense
			Retro	Not Retro	Retro	Not Retro
registry	0.008	0.093	-0.025	-0.0002	-0.049	0.046
	(0.027)	(0.077)	(0.041)	(0.024)	(0.125)	(0.126)
Registry begins x retroactive	-0.021	0.038				
	(0.031)	(0.076)				
Stock of years of offenders	0.011	0.035	0.159*	0.035	0.306	0.120
	(0.010)	(0.027)	(0.062)	(0.024)	(0.177)	(0.096)
Constant			-13.637**	1.028	2.922	5.484
			(3.275)	(1.857)	(10.278)	(12.002)
Observations			105	203	105	203
R-squared			0.99	0.99	0.90	0.78
Controls			yes	yes	yes	yes
State fixed effects			yes	yes	yes	yes
Year fixed effects			yes	yes	yes	yes
* significant at 5%; ** significant at 1%						

Notes: All dependent variables are natural logs of rates (i.e. rape is the natural log of the rape rate), coefficients are interpreted as percentage change in the crime rate. Robust standard errors in parenthesis clustered by state. In the last 4 columns data is limited to 3 years prior and 3 years after registry goes into effect. Retro indicates states that whose registries were retroactive, as listed in Table 1. Not Retro is states whose registries were not retroactive. Dependent variables are the same as Table 2, though their coefficients are suppressed

Table 4: *Summary Statistics for BJS Dataset by Registered or Not*

Panel A: *Demographic Summary Statistics*

	Not Registered On Release	Registered on release	Difference
Number in analysis	5135	4,488	
Avg. Age	36.70	37.34	-0.64**
Avg. Age at first release	25.17	25.81	-0.64**
Avg. Sentence length (months)	96.57	62.11	34.46**
Ave. number of rape arrests prior to 1994 release	0.367	0.482	-0.12**
Avg. number of sex offense arrests prior to 1994 release	0.851	0.839	0.012
Perc Black	38.1%	23.0%	15%**
Perc Hispanic	7.0%	25.6%	-18.6%**
Perc Male	98.7%	99.2%	-0.05%*

Panel B: *Tabulation of offenses for which prisoners were put in jail for release in 1994*

	Not Registered On Release		Registered on Release		Total	
Offense	Freq.	Percent	Freq.	Percent	Freq.	Percent
Rape*	1,071	20.86	1,066	23.75	2,137	22.21
Assault with intent to rape*	60	1.17	17	0.38	77	0.80
Statutory rape	120	2.34	122	2.72	242	2.51
Incest	47	0.92	0	0.00	47	0.49
Sexual abuse – assault*	2,734	53.24	755	16.82	3,489	36.26
Attempted sexual abuse	33	0.64	25	0.56	58	0.60
Conspiracy sexual abuse	0	0.00	3	0.07	3	0.03
Child molestation*	913	17.78	2,130	47.46	3,043	31.62
Attempted child molestation*	1	0.02	13	0.29	14	0.15
Forcible sodomy*	155	3.02	357	7.95	512	5.32
Morals offense	1	0.02	0	0.00	1	0.01
Total	5,135	100.00	4,488	100.00	9,623	100.00

Notes:
In Panel A:* significant at 5% ** significant at 1%. Significance for difference of means calculated using two sample t-test. Significance for difference of percentages calculated using two sample test of proportion. Avg. Age at first release is the age of the offender when they were released from jail for the first conviction, which is not necessarily the release in 1994. Avg. Age is the age of release in 1994 that got them put into the study.
In Panel B: * Indicates the proportions between the two groups are significantly different (at the 1% level) tested using a two sample test of proportion. Conspiracy sexual abuse, attempted child molestation and morals offense have very small sample sizes of 3, 14, and 1 respectively.

Table 5: *Percent of offenders who recidivated by group*

Registered On Release	Percent arrested for sex offense within ______ year(s) of release:			Percent convicted for sexual offense within _____ year(s) of release:			Percent arrested for rape offense within _____ year(s) of release:			Percent arrested for other offense within 3 years
	1	2	3	1	2	3	1	2	3	
No	1.73	3.14	4.15	0.51	1.69	1.69	0.41	0.74	0.93	33.03
Yes	1.63	3.07	3.94	0.36	1.65	1.65	0.78	1.38	1.76	31.08
Difference	0.1	0.07	0.2	0.15	0.04	0.04	-0.37*	-0.64**	-0.83**	1.94*

Notes:
* significant at 5% ** significant at 1%. Significance for difference of percentages calculated using two sample test of proportion. Any discrepancies in addition are caused by rounding.

Table 6: *Effect of registration on offender recidivism, defined 4 different ways*

	(1) Sex Offense Arrest	(2) Sex Offense Conviction	(3) Rape Arrest	(4) Other Arrest
Registered on release	0.009	0.008	0.002	0.079*
	(0.008)	(0.004)	(0.003)	(0.038)
Age at first release	-0.000	-0.000	-0.000	-0.014**
	(0.000)	(0.000)	(0.000)	(0.001)
# Previous rape arrests	0.020**	0.008**	0.010**	0.016
	(0.004)	(0.003)	(0.002)	(0.013)
# Previous sex offense arrests	0.035**	0.017**	0.005**	-0.004
	(0.003)	(0.002)	(0.001)	(0.019)
Black	-0.004	-0.002	0.001	0.164**
	(0.005)	(0.002)	(0.002)	(0.027)
Hispanic	0.001	-0.002	0.004	0.041**
	(0.005)	(0.003)	(0.003)	(0.013)
Male	0.025**			0.000
	(0.009)			(0.086)
Rapist	0.027**	0.022	0.007	0.100
	(0.010)	(0.026)	(0.011)	(0.052)
Sex Abuser	0.017*	0.016	0.010	0.127*
	(0.009)	(0.019)	(0.011)	(0.051)
Statutory Rapist	-0.003	0.021	0.004	0.229**
	(0.013)	(0.036)	(0.015)	(0.064)
Child Molester	0.013	0.021	0.005	0.058
	(0.007)	(0.025)	(0.010)	(0.063)
Sodomizer	0.035*	0.038	0.014	0.062
	(0.017)	(0.047)	(0.020)	(0.060)
Police per 1000 (1997)	-1.684**	0.195	-0.821	-2.684
	(0.604)	(0.228)	(0.646)	(4.003)
Rape Incidences per 1000 (1993)	-0.102	0.275	-0.182	-0.475
	(0.267)	(0.157)	(0.230)	(2.300)
Sex offense arrests per 1000 (1993)	-0.907**	-0.657**	0.491*	-5.522*
	(0.320)	(0.196)	(0.213)	(2.552)
Percent Urban	0.099	0.173**	0.013	0.726
	(0.108)	(0.045)	(0.068)	(0.472)
Percent Christian (2000)	-0.001	0.029	0.057*	-1.401**
	(0.045)	(0.020)	(0.023)	(0.347)
Elected Judges	0.004	0.019**	0.002	0.045
	(0.016)	(0.007)	(0.008)	(0.061)
Percent Republican Votes (1992)	0.179	0.363**	-0.029	0.831
	(0.214)	(0.065)	(0.128)	(0.967)
Average Rape Sentence (in years)	-0.001	-0.002**	0.000	-0.003
	(0.001)	(0.000)	(0.000)	(0.005)
Prisoners per 1000 (1994)	0.006	-0.037**	-0.010	-0.268
	(0.042)	(0.014)	(0.026)	(0.256)
Observations	9323	9227	9227	9323

Robust standard errors in parentheses, clustered on state

* significant at 5%; ** significant at 1%

Notes: Probit analysis is used with change in probability reported. Standard errors clustered on state are in parenthesis. Other arrest is arrest for an offense other than a sex offense or rape. Registered on release=1 if sex offender characteristics implied he would have had to register on his release in 1994. Sentence length is sentence length for prison stay for which the prisoner was released in 1994. Rapist, sexual abuser, child molester, sodomizer are dummy variables indicating what the crime for which the sex offender committed for which he was released in 1994. Police per 1000 is a state level control, data from *Bureau of Justice Statistics*. No females in the sample were later convicted of a sex offense nor arrested for rape, thus male is dropped from the second and third specification. Rape incidences per 1000 and Sex offense arrests per 1000 come from the "panel data" used in this paper, from the Bureau of Justice Statistics. Percent Christian is measured as percent Christian Church adherents from the 2000 Statistical Abstract of the United States. Elected Judges is a dummy=1 if a state elects their judges rather than appoints them, data from Tabarrok and Helland (1999). Percent Republican Votes is the percent of the state that voted for the Republican candidate in the 1992 presidential election, data from USElectionAtlas.org.

Table 7: *Effect of registration on offender recidivism, Cox Proportional Hazard Model*

	(1) Sex Offense Arrest	(2) Rape Arrest	(3) Rape Conviction
Registered on release	1.462*	2.195**	1.727*
	(0.308)	(0.838)	(0.508)
Age at first release	0.984***	0.970**	0.977***
	(0.006)	(0.012)	(0.009)
# Previous rape arrests	1.622***	2.374***	1.620***
	(0.097)	(0.179)	(0.142)
# previous sex offense arrests	2.073***	1.577***	2.436***
	(0.070)	(0.140)	(0.110)
Black	1.012	1.410	0.968
	(0.117)	(0.306)	(0.167)
Hispanic	1.009	1.438	0.867
	(0.149)	(0.370)	(0.195)
Male	4.116	2.288e+14	1.032e+16
	(4.125)	(4.527e+21)	(0.000)
Rapist	1.378	1.512	2.132
	(0.659)	(1.593)	(2.207)
Sex Abuser	1.375	2.055	2.127
	(0.648)	(2.145)	(2.178)
Statutory Rapist	0.881	0.992	1.670
	(0.525)	(1.189)	(1.876)
Child Molester	1.278	1.144	2.748
	(0.608)	(1.203)	(2.832)
Sodomizer	1.594	1.650	3.479
	(0.819)	(1.807)	(3.696)
Police per 1000 (1997)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Rape Incidences per 1000 (1993)	0.000***	0.000*	1.763e+08
	(0.000)	(0.000)	(2.271e+09)
Sex offense arrests per 1000 (1993)	0.357	0.000	2.721e+10**
	(2.290)	(0.000)	(2.793e+11)
Percent Urban	0.000***	2.584e+15	0.000***
	(0.000)	(7.447e+16)	(0.000)
Percent Christian (2000)	32.729*	5.983	910,668.528***
	(62.950)	(24.883)	(3277023.576)
Elected Judges	0.993	1.029	1.019
	(0.011)	(0.022)	(0.016)
Percent Republican Votes (1992)	1.144	1.755	3.608***
	(0.348)	(1.056)	(1.788)
Average Rape Sentence (in years)	2,867.587**	0.308	1.495e+12***
	(11,628.947)	(2.506)	(8.986e+12)
Prisoners per 1000 (1994)	0.954***	1.020	0.881***
Registered on release	(0.017)	(0.048)	(0.023)
	0.629	0.385	0.035***
	(0.524)	(0.717)	(0.045)
Observations	12427	12427	12427
Standard errors in parentheses			
* significant at 10%; ** significant at 5%; *** significant at 1%			

Notes: Cox proportional Hazard model is used. Reported coefficients are exponentiated for ease of interpretation. The coefficient can be read as the proportional change in the hazard rate with respect to that variable. Controls defined as in table 6.

Table 8: *Effect of registered sex offenders living or working in a census block group on crime rates*

	(1) Sex Abuse per 1000	(2) Nonviolent crimes per 1000	(3) Violent crimes per 1000	(4) Sex abuse per 1000
Offenders per 1000 living in block group	-0.011	-159.809	-15.277	-0.046
	(0.019)	(141.072)	(13.302)	(0.052)
population	-0.000	-0.036	-0.008	
	(0.000)	(0.219)	(0.020)	
Black	0.000	-5.666	-0.561	
	(0.001)	(4.555)	(0.415)	
Hispanic	0.001	-44.706	-4.193	
	(0.002)	(32.241)	(2.947)	
Female	0.000	-187.384	-17.080	
	(0.009)	(135.443)	(12.350)	
Age 18 to 39	-0.001	-40.227	-3.617	
	(0.002)	(29.859)	(2.719)	
Renter Occupied	0.000	0.641	0.062	
	(0.000)	(0.654)	(0.060)	
Violent Crime	0.009***			0.008***
	(0.001)			(0.001)
Non-violent Crime	0.001***			0.000***
	(0.000)			(0.000)
Constant	-0.060	11,981.755	1,096.468	-0.030
	(0.391)	(8,571.084)	(775.875)	(0.090)
Observations	15280	15280	15280	15280
R-squared	0.06	0.27	0.21	0.06
Block Group Effects	No	No	No	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Robust standard errors in parentheses, clustered on block group				
* significant at 5%; ** significant at 1%				

Notes: Non-violent crimes are: stolen auto, theft, theft from auto, arson and burglary. Violent crimes are: homicide, assault, and robbery. Demographic data is per 100 in the popultion. Crime variables are per 1000 in population Although using the natural log of the crime rate would have made interpretation easier, many blocks had no crimes and thus natural logs could not be used.

Table 9: *Differentiating between the time when there was only a registry (before the internet database) and after*

	Sex Abuse per 1000
Offenders per 1000 living in block group after internet	0.011
	(0.009)
Offenders per 1000 living in block group before internet	0.003
	(0.022)
Constant	0.039*
	(0.021)
Observations	15280
R-squared	0.07
Block group fixed effects	Yes
Time fixed effects	Yes
* significant at 10%; ** significant at 5%; *** significant at 1%	

Notes: Standard errors in parenthesis.

Figure 1: *States with registries*

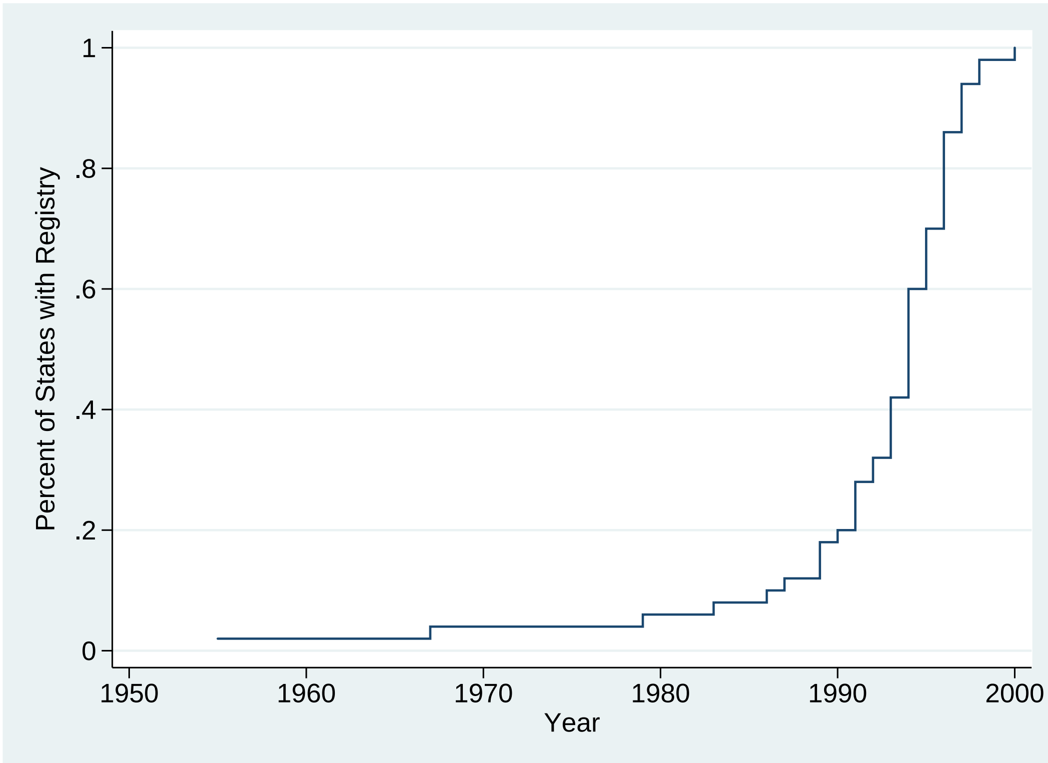


Figure 2: *States with public access to registries via the internet*

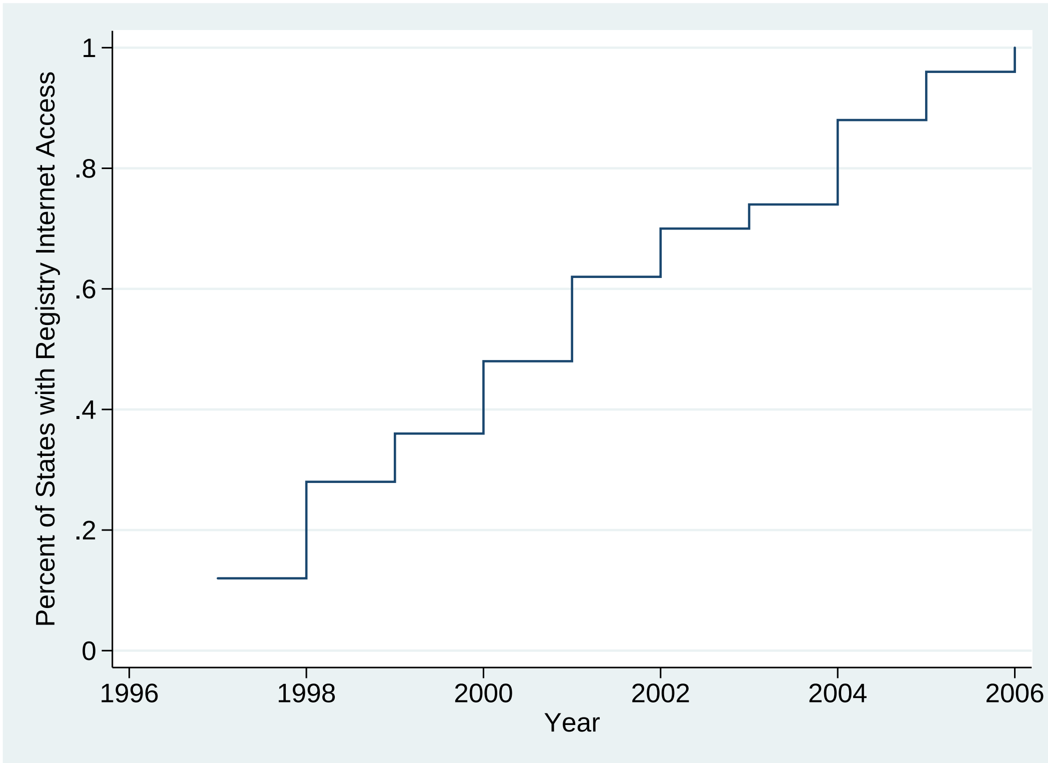
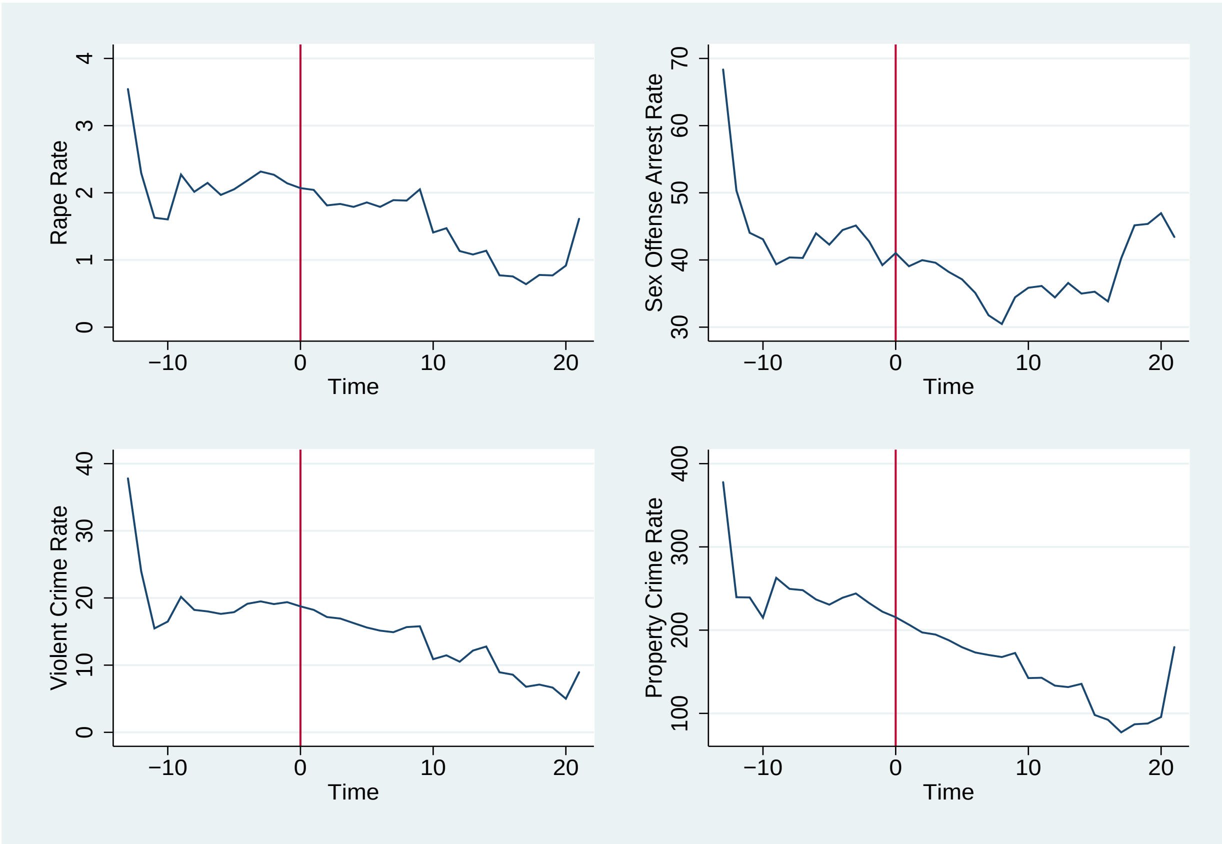
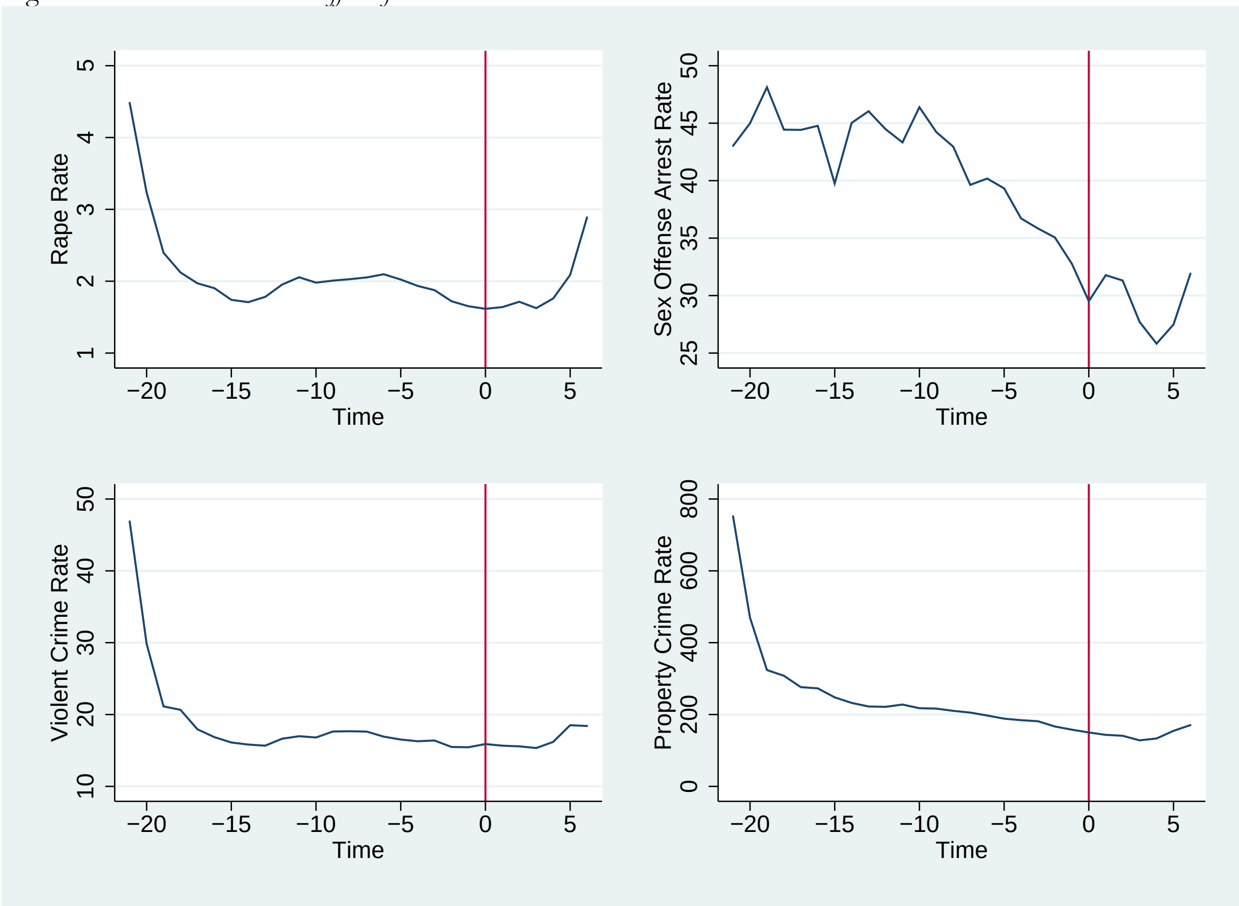


Figure 3: *Crime over Time – Effect of Registry*



Note: 0 represents the point in time when the registry was created. So -10 is 10 years before the registry was created etc…All crime variables are per 100,000 in the population. Violent crime, rape and property crime are incident rates. Violent crime in this graph does not include rape.

Figure 4: *Crime over Time – Effect of Public Access via the Internet*



Note: 0 represents the point in time when public access to the registry via the internet began. So -10 is 10 years before the registry was available on the internet etc…All crime variables are per 100,000 in the population. Violent crime, rape and property crime are incident rates. Violent crime in this graph does not include rape.

Figure 5: *Where registered sex offenders in DC live*

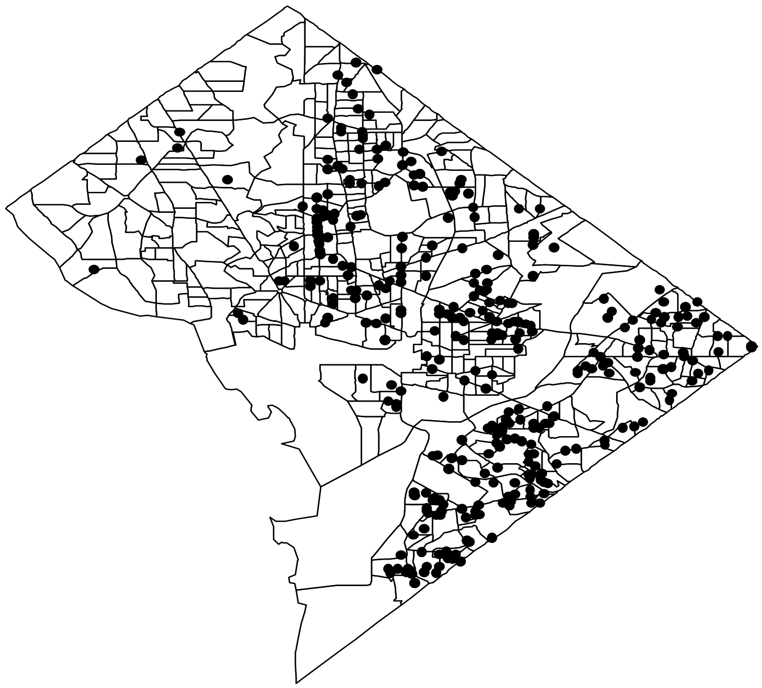
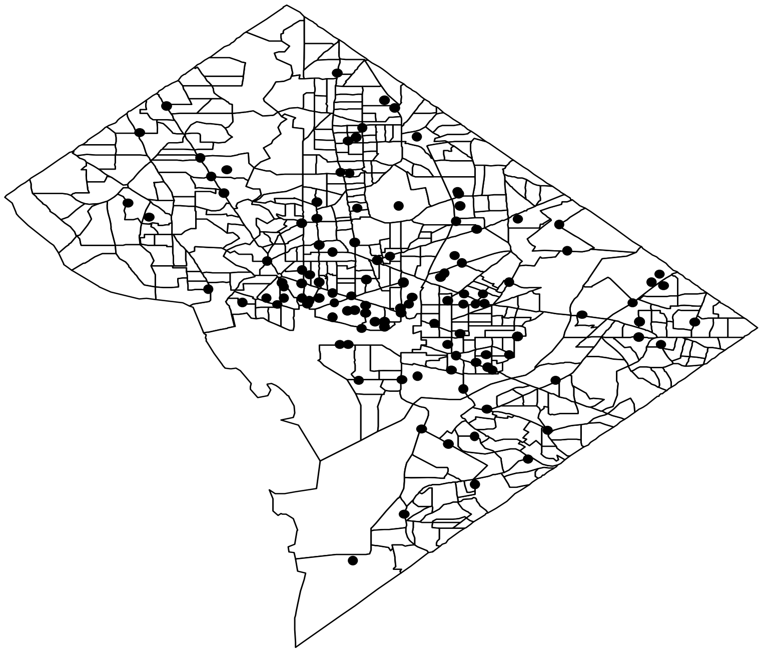


Figure 6: *Where registered sex offenders work in DC*



Appendix A: Registry Effective Date Justifications

Prescott and Rockoff (2008) and I disagree on three dates of registry effective. Below are justifications for the dates I use in my paper.

Kentucky: 7/15/1994

From the Kentucky Legislature Website: http://www.lrc.ky.gov/KRS/017-00/510.PDF
Kentucky revised statutes, Title III, Chapter 17.510 "Registration system for adults who have committed sex crimes or crimes against minors -- Persons required to register -- Manner of registration -- Penalties -- Notifications of violations required…. **History:** Amended 2008 Ky. Acts ch. 158, sec. 13, effective July 1, 2008. -- Amended 2007 Ky. Acts ch. 85, sec. 100, effective June 26, 2007. -- Amended 2006 Ky. Acts ch. 182, sec. 6, effective July 12, 2006. -- Amended 2000 Ky. Acts ch. 401, sec. 16, effective April 11, 2000. -- Amended 1998 Ky. Acts ch. 606, sec. 138, effective July 15, 1998. -- Created 1994 Ky. Acts ch. 392, sec. 2, *effective July 15, 1994*." (emphasis mine)

Connecticut: 10/1/1998
http://www.cga.ct.gov/ps98/Act/pa/1998PA-00111-R00SB-00065-PA.htm

According to http://www.cga.ct.gov/ps98/Act/pa/1998PA-00111-R00SB-00065-PA.htm, Connecticut Public Act 98-111: "An act concerning the registration of sexual offenders" was approved on May 17, 1998 and according to the Smith Law Firm was effective October 1, 1998. See: http://www.smith-lawfirm.com/registration.html
"The law, **(Public Act 98-111)** effective October 1, 1998, provides for mandatory registration by persons convicted of a broader range of sexual crimes toward children and sexually violent crimes."

See also Connecticut General Assembly Statutes Chaper 969: Registration of Sexual Offenders http://www.cga.ct.gov/2005/pub/Chap969.htm
PA 98-111 is listed first in the history for each section, as stated above PA 98-111 became effective October 1, 1998.

North Dakota: 1991
According to the FAQs on the North Dakota Sex Offender registry site, located at: http://www.sexoffender.nd.gov/FAQ/faq.shtml

" **1. WHEN DID THIS SEX OFFENDER LAW BEGIN?** The first sex offender registration statute in North Dakota was passed in the 1991 legislative session. Since then, there have been changes or additions made to the original statute in every legislative session."

Data appendix

UCR arrest data was provided by the BJS on a CD-ROM. Some data points for sexual offense arrest rates are missing on the FBI UCR CD-ROM. The missing information was filled in from data directly from the state wherever possible, otherwise linear interpolation was used to estimate remaining missing data. The following state-years had missing data for both sexual offense and rape arrests:

Florida: 88-89, 91, 96-03
GA: 90
IL: 99
IA: 91
KS: 93-99
KY: 87-88
MT: 94-96
NH: 95
SC: 91
VT: 96-97
WI: 98-00

Florida information was updated from:
http://www.fdle.state.fl.us/fsac/data_statistics.asp "UCR Arrest Data" Total Arrest by County 1989 Excel File (only non-forcible sex offenses, "forcible sex offenses" include forcible offenses other than rape)
http://www.fdle.state.fl.us/fsac/data_statistics.asp "UCR Arrest Data" Total Arrest by County 1991 Excel File (only non-forcible sex offenses, "forcible sex offenses" include forcible offenses other than rape)
http://www.fdle.state.fl.us/FSAC/UCR/1996/arrests.asp
http://www.fdle.state.fl.us/FSAC/UCR/1997/arrests.asp
http://www.fdle.state.fl.us/FSAC/UCR/1998/ucr_1998_arrest_totals.pdf
http://www.fdle.state.fl.us/FSAC/UCR/1999/ucr_1999_arrest_totals.pdf
http://www.fdle.state.fl.us/FSAC/UCR/2000/2000CIF_arr.pdf
http://www.fdle.state.fl.us/FSAC/UCR/2001/CIFA_annual01.pdf
http://www.fdle.state.fl.us/FSAC/UCR/2002/CIFA_annual02.pdf
http://www.fdle.state.fl.us/FSAC/UCR/2003/CIFA_annual03.pdf
accessed from: http://www.fdle.state.fl.us/fsac/UCR/index.asp Florida Dept of Law Enforcement Uniform Crime Reports

Kansas information updated from:
http://www.accesskansas.org/kbi/stats_crime_1997.shtml (1997 combined arrests pdf)
http://www.accesskansas.org/kbi/stats_crime_1998.shtml (1998 combined arrests pdf)

Wisconsin information updated from:
See - http://oja.state.wi.us/refcenter.asp#list search crime and arrests,
Crime and Arrests 1999, table entitled ADULT ARRESTS FOR INDEX CRIMES, STATE OF WISCONSIN (forcible rape)

Crime and Arrests 1999, table entitled ADULT ARRESTS FOR INDEX CRIMES, STATE OF WISCONSIN (forcible rape)

Crime and Arrests 2001, table entitled ADULT ARRESTS FOR INDEX CRIMES, STATE OF WISCONSIN (forcible rape)

Kentucky:
Updated from personal correspondence with Melissa Pratt Kentucky state Police Statistical Coordinator 8/25/2006 (email)