THE INFLUENCE OF SEX OFFENDER REGISTRATION AND NOTIFICATION LAWS IN THE UNITED STATES

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Abstract:

This paper explores the impact of sex offender policies by examining sex offenses through time series analysis. Using monthly count data of rapes aggregated at the state level, this analysis uses Box-Jenkins autoregressive integrated moving average (ARIMA) models to conduct ten separate intervention analyses on the enforcement of Megan's Law. The results of the interrupted time series analyses are mixed with regard to whether the enforcement of sex offender registration had a statistically significant effect on the number of rapes reported at the state level. Although several states showed a non-significant increase in the number of rapes, only three states had a significant effect on decreasing the number of rapes. Policy implications of this research are discussed in terms of the efficacy of sex offender registration and whether changes in these laws should be considered.

INTRODUCTION

In the middle 1990's, the United States passed a series of laws to deal with the sex offender threat to the public. The legislative solution for the problem of sex offenders was found in sex offender registration and notification laws. This legislation stemmed from a series of highly publicized incidents where the offender had a prior record of committing sexual offenses and where the crimes often resulted in a murder of a child in addition to the sex offense. As well, the common perception that sex offenders pose a higher risk of re-offending than other types of criminals helped to spur the passage of registration and notification laws.

Because sex offender laws are relatively new, research examining the efficacy of these laws is limited. Using time series analysis, this study examines sex offender registration and notification laws in the United States. The goal of this study was to examine the general deterrent nature of these laws on the number of sex offenses committed as measured by the increase of decrease in the number of rapes.

REVIEW OF THE LITERATURE

The adequacy of current sex offender policies are based on the idea that registries, through notification, help communities to better protect themselves from sex offenders. This idea is based on the assumption that sex offenders are more likely to recidivate than other offenders, thus the community should be on guard from these individuals. These lines of reasoning add certain dimensions when evaluating the utility of sex offender laws. This section explores sex offender recidivism and provides a review of the existing evaluations of sex offender registration and notification laws.

Sex Offenders and Recidivism

One of the major tenets of sex offender registration and notification laws is the idea that sex offenders are more likely to recidivate than other types of offenders. This is also one of the biggest myths about sex offenders according to the Center for Sex Offender Management (2001). From a review of sex offender recidivism studies, Sample (2001, 106) argued that because of "methodological difficulties, differences in sample size, and variability in follow-up lengths, most studies report inconsistent levels of reoffending among sexual offenders."

Hanson and Bussiere (1998) conducted a meta-analysis of studies on sex offender recidivism. From an international sample of 87 research projects (representing 28,972 sex offenders), the average recidivism rate for sex offenses was only 13.4%, while the average recidivism rate for any offense was 36.3%. Findings on offender characteristics showed that only age and marital status predicted sex offense recidivism. This was particularly true if an offender had prior sexual offenses, victimized strangers, had an extrafamilial victim, began offending at an early age, had a male victim, or had engaged in diverse sexual crimes. General recidivists (sex offenders who committed new crimes that are non-sexual in nature) were those most likely to have used force against their

victims and less likely to have chosen child victims. Hanson and Bussiere (1998, 357) argued that their findings "contradict the popular view that sexual offenders inevitably re-offend . . . even in studies with thorough searches and long follow-up periods the recidivism rate almost never exceeded 40%."

Walker and McLarty (2000) examined the characteristics of sex offenders on the Arkansas sex offender registry from 1997 to 1999; from this, they were able to describe the characteristics of offenders in the Arkansas' registry. They found that the majority of the offenders (97%) were male, white (75.4%), between the ages of 30 and 69, and the majority of the offenders were from Arkansas (54.9%). The average offender committed 1.55 sex offenses and the majority of the offenders (53.3%) were charged with 1st Degree Sexual Abuse. Most importantly, in relation to recidivism, sex offenders were predominantly first time offenders (73% of sample). This could indicate that this was the sex offender's first time offending or that this was the offender's first offense that resulted in arrest.

Sample (2001) explored the social construction of sex offenders and sex offender registration laws in Illinois. She did this by incorporating a three-pronged analysis. Sample performed a content analysis of three newspapers, interviewed policy makers, and analyzed police arrest records in an effort to compare common media and policy maker beliefs.

Between 1991 and 1998, Sample (2001, 34) noted a 128% increase in articles pertaining to sex offenders and offending. From the articles examined, Sample (2001, 66) found that sex offenders were portrayed as men in their mid-thirties, there was a lack of information on the race and social class of the offender, offenders were compulsive, the sex crimes themselves appeared to be changing (child pornography, pedophilia, and cyberporn), and the victims were portrayed as the innocent, the aged, and the infirm. The increase in these types of articles gives the public the impression of a growing sex offender problem (Sample, 2001, 69).

Sample then observed the relationship between media driven perceptions and the policy making process. Sample interviewed 35 legislators from Illinois. Of these 35 policy makers, only four were confident that sex offender registration and notification laws were effective; however, nearly all of the sample agreed "that current sex offender legislation . . . successfully addressed the public's demand for action"(Sample 2001, 96). For these public officials, "the media indirectly influenced the enactment of sex offender legislation by affecting the public's perceptions" and the media directly influenced policy making because the politicians "freely admit that the media serve as their major source of information"(Sample 2001, 99).

Sample (2001) then examined aggregate data sources to examine further the relationship between media accounts and policy maker legislation. She found that from 1990 to 1997, sex offenders represented only 1.2% of the total criminal charges in Illinois, and the number of sex offenses remained stable over this time period. The typical offender illustrated in the data was male, of no discernable race, and similar in age to other types of offenders. Most importantly, sex offenders did not re-offend at a higher rate than other types of offenders, which goes against the commonly held perceptions of the public.

When examining sex offenders who committed only sex offenses, Sample (2001) found that the majority of crimes included adult victims, not adolescent victims as

popularly portrayed in the media. More importantly, sex offenders of any type (rapists, pedophiles, etc.) had greater than a 6% re-arrest rate within 5 years of the same offense but most sex offenders were not re-arrested for a sex offense. Finally, sex offenders "with child victims had lower rates of re-arrest for any sex crime than those who victimized adults, the one exception being child pornographers"(2001, 162).

In conclusion to her multifaceted study on sex offender registration and notification laws, Sample wrote:

> Popular conceptions of sex offending influence public officials' reactions to the problem. Officials' reactions [to the public] lead to the passage of the statutes that law enforcement personnel use to enact arrest charges. The arrest statistics, themselves a product of the social construction of sex offending, can be used to assess the extent to which legislative intent has been realized and the accuracy of popular perceptions of the problem. Information regarding the sex offending arrests is then used to reaffirm or reformulate popular perceptions of the problem (2001, 28).

This quotation shows the interrelatedness of these entities, media, policy makers, and arrest data, and how they affect one another.

Bynum (2001) noted that sex offender recidivism has been measured in three ways: Subsequent arrest, subsequent conviction, and subsequent incarceration. Because of this, reliance on "measures of recidivism as reflected through official criminal justice system data obviously omit offenses that are not cleared through an arrest or that are never reported to the police"(Bynum 2001, 2). From his examination of the literature, Bynum (2001, 8) concluded that studies on "sex offender recidivism vary widely in the quality and rigor of the research design, the sample of sex offenders and behaviors included in the study, the length of follow-up and the criteria for success of failure." Further, due to "these differences, there is often a perceived lack of consistency across studies of sex offender recidivism."

Because of the conceptual problems outlined by both Bynum and Sample, the literature is inconclusive as to the magnitude of recidivism for sex offenders. Research seems to indicate that sex offenders are not any more likely to recidivate than other types of offenders. It is important to note the homogeneity between sex offender recidivism and other offender type recidivism. The perception that sex offenders pose a higher risk of recidivism than other types of offenders formed the backdrop of the movement for sex offender registration and notification legislation. With this fact in mind, the next section examines studies that have been conducted on the effectiveness of registration and notification laws themselves.

Research on the Efficacy of Sex Offender Registration Laws

Proponents of sex offender registration and notification argue that such laws are effective because they inform the public of the presence of sex offenders in the community, thereby enabling them to take action to protect themselves. These laws are also thought to reduce sex crimes because the public is able (and more likely) to report

suspicious behavior by sex offenders. This was supported by the research of Phillips (1998), who found that over 60 percent of the respondents to a survey felt that sex offender laws made sex offenders act better than if their criminal history was not known; the majority also felt safer with the laws in place.

Matson and Lieb (1996) conducted a survey of law enforcement officials in Washington utilizing a qualitative design. Answers from the survey instrument were categorized into advantages and disadvantages. Law enforcement agencies noted several advantages to sex offender registration and notification: They felt the laws provided better community surveillance, created better public awareness, deterred future crimes by the offender, and promoted child safety. Although law enforcement agents found several advantages to the registration and notification laws, they noted several disadvantages. Law enforcement agents felt that the laws created more work. Adding to this was the problems inherent in collecting information from courts and other agencies dealing with sex offender registration. Matson and Lieb (1996) found that sometimes there could be an overreaction to the notification in neighborhoods. This could lead to harassment and embarrassment of sex offenders or their families.

True empirical research on sex offender registration and notification laws has been limited (Zevitz and Farkas 2000, 1). There has only been one true study on the effectiveness of sex offender registration and notification laws; this study was conducted on Washington's laws. Schram and Milloy (1995) compared 139 Level 3 sex offenders to 90 sex offenders who were not subject to notification in an effort to gain basic demographic characteristics of Level 3 sex offenders and compare recidivism between the two groups.

Schram and Milloy (1995) looked at the demographic characteristics of both juvenile (N=14) and adult (N=125) Level 3 sex offenders. Most of the juvenile sample was white, had histories of sex offenses and nonsexual offenses, and had a single sex conviction for an offense involving a child victim. Adult offenders, were generally white, male, in his or her mid-30's, had never been married, unemployed, likely to have a history of all kinds of offenses, typically was a child molester, and the typical offender was likely to have committed other sex offenses for which he or she had not been convicted.

Both juvenile and adult Level 3 sex offenders were also examined for recidivism. A high percentage (79%) of juvenile offenders recommitted any kind of offense while on the registry, but only 43% of the sample committed a new sex offense. Of the adult sample, almost one-half (43%) recidivated but not for sex offenses. Only 14% of the adult sample committed a new sex offense. When the entire Level 3 sample was compared to a control group of sex offenders who were not notification eligible, Schram and Milloy (1995, 17) found that community notification had little effect on sex offender recidivism. Further, "the estimated rates for sex offenses are remarkably similar for each group throughout the follow-up period." Although Schram and Milloy (1995) concluded that community notification had little effect on the sample, one significant limitation with this study is that it was completed prior to the enactment of all of the federal sex offender laws and the completion of all the court actions, thus findings may not be applicable to sex offender registries today.

An extensive study of the potential influence of registration and notification on sex offenders was presented by Petrosino and Petrosino (1999). Petrosino and Petrosino

evaluated how well sex offender laws would work on a sample of 136 offenders in Massachusetts. Petrosino and Petrosino examined criminal history records of each offender; the data was used to "determine how many of the serious sex offenders would have been in the registry before the instant offense . . . how many of the offenders committed stranger-predatory instant offenses . . . and if the Massachusetts Registry Law might have prevented them."

The offenders were all male, mostly white, and most offenses involved children. Cumulatively, the sample contained 291 prior arrests (0-19 per offender), which ranged from property to sexual to nonsex violent offenses. Only 74 of the 291 prior arrests were for sexual offenses. Only 27% of the offenders would have been eligible for registration, thus, "prevention by notification or police investigation could not have occurred for most cases"(Petrosino and Petrosino 1999, 148). Petrosino and Petrosino (1999, 154) concluded that "the public safety potential of the Massachusetts Registry Law to prevent stranger-predatory crimes . . . is limited." Further, of the "instant offenses committed by 136 serious sex offenders, we rated the potential of notification reaching the eventual victim as good in only four stranger-predatory cases and as poor to moderate in two others."

Discussion

The literature on sex offender registration and notification laws in the United States provides several different insights. First, the major premise of sex offender laws is that sex offenders should be required to register because they pose a significant risk of re-offending. This is corroborated by public opinion and anecdotal evidence that support the assumption of the effectiveness of such laws at reducing sex crimes. The research reviewed here, however, indicate that there is little empirical evidence regarding the influence of sex offender registration and notification; the few empirical studies that have been conducted found no significant influence of these policies on sex offenders re-offending patterns.

Second, another problem with sex offender registration laws is the net widening effect inherent in such policy. Sex offenders are required to register for sex offenses; kidnapping is also a registerable offense consolidated into sex offender registration laws throughout the country. This means that people who are not necessarily sex offenders are subject to registering as sex offenders. Also, registration and notification laws open the door for attempts to include other crimes; in Arkansas, for example, the legislature tried to regulate drug offenders by requiring them to register with the state. The legislation was inevitably turned down by policy makers, but this instance shows the potential net widening effect registration statutes can have.

This research attempts to fill the void in the literature by examining the efficacy of sex offender registration and notification laws. Specifically this research explored the general deterrent effects of current sex offender laws using time series analysis. Although there has been some research that has examined the effectiveness of sex offender laws in particular states, to date there has been no formal examination of the influence sex offender registration and notification laws may be having nationwide in deterring the commission of sex offenses. While this research represents a preliminary

analysis of the effectiveness of sex offender laws, it does provide information concerning whether these laws are having any influence on the number of sex offenses that are occurring in the U.S. The next section outlines the research methodology incorporated in this study.

RESEARCH METHODS

The current research attempts to study the general deterrent effects of sex offender registration laws in the U.S. This project examined sex offenses, as measured by the incidence of rapes, in the 50 states as they related to the implementation of sex offender registration and notification laws. The goal of this research was to determine if sex offender laws have reduced the number of rapes committed in the states that have passed such laws. The primary question driving this research is: What is the relationship between passage of sex offender registration and notification laws and the number of sex crimes committed?

Data

The first step the research team had to complete in this project was to determine in what year each state implemented a sex offender registry which included a notification strategy. The year in which all states, as well as the District of Columbia, implement a notification component is provided in Table 1.

Table 1. Sex Offender Registration and Notification Implementation Dates by State or District

State	Year	State	Year
Alabama	1998	Montana	1995
Alaska	1994	Nebraska	1997
Arizona	1996	Nevada	1998
Arkansas	1997	New Hampshire	1996
California	1996	New Jersey	1993
Colorado	1998	New Mexico	1995
Connecticut	1998	New York	1995
Delaware	1994	North Carolina	1996
Washington D.C.	1999	North Dakota	1995
Florida	1997	Ohio	1997
Georgia	1996	Oklahoma	1998
Hawaii	1998	Oregon	1993
Idaho	1993	Pennsylvania	1996
Illinois	1996	Rhode Island	1996
Indiana	1998	South Carolina	1999
Iowa	1995	South Dakota	1995
Kansas	1994	Tennessee	1997
Kentucky	1994	Texas	1999
Louisiana	1992	Utah	1996
Maine	1995	Vermont	1996
Maryland	1995	Virginia	1997
Massachusetts	1999	Washington	1990
Michigan	1995	West Virginia	1993
Minnesota	1998	Wisconsin	1997
Mississippi	1995	Wyoming	1999
Missouri	1995		

As can be seen in Table 1, most of the states generated laws for sex offender registration and notification after the passage of Megan's Law in 1996. Almost half of the states (21), however, had notification schemes associated with their sex offender registries prior to Megan's Law. Due to federal mandates and judicial decisions, the nature of registration and notification is fairly uniform across all states, with the exception of some subtle nuances (what information is gathered, the process of notification by law enforcement, and notification on the internet).

State level data came directly from the Federal Bureau of Investigations (FBI). The FBI provided monthly breakdowns of rapes from all reporting states. Because the data were provided by the same agency that is responsible for the completion of the Uniform Crime Report (UCR), we were able to separate rapes from general sex offenses.

Although other sex offenses are collected as an aggregated measure by the FBI, this measure of sex offending was not particularly useful because oftentimes, the data were missing. Because rape is a Type I offense in the UCR, monthly counts were almost always readily available to the research team.

For most states, the FBI supplied data from 1990 to 2000 because prior to 1990, the data were either missing or were not available electronically. In cases where additional data were needed, efforts were made to collect data from the individual state. These efforts were not entirely successful, however. To keep the nature of science close at hand and to discuss how data collection limitations played a role in our analysis, we will expose the steps that resulted in only analyzing ten states. Ten states were not selected arbitrarily.

For instance, because of the time frame when states passed their laws, some states did not have an adequate number of observations surrounding the implementation of the law. A decision was made, therefore, that states with data from at least three years before and three years after would be included in the analysis. Even using this strategy, some states lacked available data. Prior to conducting any analysis, 13 states and the District of Columbia were necessarily excluded for three reasons.

First, when states implemented relevant legislation late in the 1990s, an adequate number of post-intervention observations were not yet available. Second, when states implemented its legislation early in the 1990s, it was not possible to obtain an adequate number of pre-intervention observations. In all, eight states (Kentucky, Louisiana, Massachusetts, New Jersey, South Carolina, Texas, Washington, and Wyoming) and the District of Columbia were excluded from the analysis for one of these two reasons.

Third, five states were excluded from the analysis because rape data was not collected in monthly format. The states excluded due to unavailability of monthly data were: Florida, Illinois, Montana, Kansas, and Wisconsin. After excluding these states, 37 states were left for examination. Due to technical reasons that will be discussed below, it should be noted that we analyzed data from only ten states.

Analytic Strategy

For our purposes the data will be kept as counts. Counts can not be compared across states with the same ease of using rates or some other standardized measure of rapes (i.e. we are not controlling for changes in population when we use counts). Therefore, we refrain from using a fixed effect panel design. Regardless of the incomparable measures, we are in fact interested in analyzing each intervention separately. In the spirit of strengthening the quasi-experimental design, analyzing an intervention's effect across varying places and times has the potential to minimize chance error.

For example, assume a significant reduction in rape counts is found. It is possible that the notification component prevents rapes and therefore corresponds to a significant decrease in rape counts. It is also possible that this significant decrease is simply due to chance error or some other mechanism that took place at the same point in time. If we find several states that witness a significant decrease after the introduction of the intervention, however, the likelihood of chance error or simultaneity being the true

explanation for the decrease is reduced. Therefore, each state's monthly data will be analyzed individually. Due to the different time points in which the intervention occurred, this approach will ideally strengthen the notion of a quasi-experiment.

The primary analytic strategy in estimating the effect of sex offender registration and notification laws is an interrupted time series analysis. When using time series data and a least squares estimator, several assumption violations are likely. The most common and perhaps the most problematic is systematic variation in the estimated disturbance term. This serial correlation leads to the well known adverse effects of negatively biased standard error estimates and as a result, an increased likelihood of Type I errors. In fact, it has been noted that when using biased standard error estimates, the resulting t statistic value can be inflated by 300% or more (McDowall, McCleary, Meidinger, Hay 1980, p. 13). And as a direct result, the statistical significance of an intervention's effect is vastly overstated.

Hence, we used univariate autoregressive integrated moving average (ARIMA) processes as noise models for this variation. These models are commonly referred to as Box-Jenkins models because Box & Jenkins contributed largely to their development (Box & Jenkins 1976, see also McDowall et al. 1980). In addition to contributing to the development of ARIMA models, Box & Jenkins (1976) also popularized a three stage model selection process (Enders 2003, p. 76). This three stage iterative process consisting of identification, estimation and diagnosis phases was used in the current analysis.

The identification phase consisted of examining the autocorrelation function (ACF) or correlogram. Because we have monthly data, we examined the ACF and based on the behavior of the autocorrelation we preliminarily identified an ARIMA$(p,d,q)(P,D,Q)_{12}$ process. We then continued to the next phase and estimated the model. In the third phase, we examined the ACF as well as the Ljung-Box Q-statistics in order to assess the adequacy of the model. If the model adequately mimicked the true data generating process, the residuals exhibited independence or reflected purely random noise. In these instances, the model was accepted and the respective state was kept for later analysis.

Rather than cluttering the scientific record with analyses that are marginal at best, we first focused on states whose autocorrelation processes could be reasonably modeled with conventional ARIMA patterns. In addition, once an adequate noise model was identified and diagnosed, the Jarque-Bera (JB) test was used to ensure that the estimated disturbance term was Normally distributed with specific regard to its skewness and kurtosis. When adequate conventional ARIMA models were identified and the residuals failed to reject the joint null in the JB test, the state was considered for the interrupted time series analysis.

Initially, only seven states had adequate noise models and had residuals that reflected Normality. The remaining 30 states either had unconventional ACF patterns, highly skewed residuals, or both. Rape counts in three states, Connecticut, Idaho, and Nebraska, had disturbance estimates that were particularly characteristic of count data with regard to their skewness. Adequate noise models had been identified and diagnosed for these states, however. Therefore, we reduced the skew by taking the natural logarithm of the rape counts. In these three states, each monthly observation was not less

than unity. As a result, adding an arbitrary value in order to log the data was not necessary.

The transformed rape counts had residuals that failed to reject the joint null in the JB test and therefore, the following 10 states remained for the interrupted time series analysis: Arkansas, California, Connecticut, Hawaii, Idaho, Nebraska, Nevada, Ohio, Oklahoma, and West Virginia. Ten separate interrupted time series analyses were conducted. In each, the introduction of the intervention is modeled as an abrupt and permanent change. Parenthetically, the intervention is dichotomous and is coded as a conventional dummy variable where zero indicates the absence of the law.

In sum, ARIMA models were used to remove systematic variation in the residual term. When series were non-stationary, both the intervention and the series were differenced[1]. After estimating an adequate noise model, all ten series fail to reject the joint null of the JB test and therefore, the stochastic component is independently and Normally distributed. Although the data are in the form of counts, all technical issues that are of major importance in Normal theory regression have been addressed.

For the ease of replication, Table 2 contains all relevant univariate information discussed thus far.

Table 2. Univariate Statistics of States Included in the Analysis

State	Noise Model	Sample Size	Intervention Year
Arkansas	ARIMA $(2,0,0)(1,0,0)_{12}$	$n = 120$	1997
California	ARIMA $(0,0,0)(2,0,0)_{12}$	$n = 120$	1996
Connecticut*	ARIMA $(0,0,1)(0,0,0)_{12}$	$n = 72$	1998
Hawaii	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 72$	1998
Idaho*	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 120$	1993
Nebraska*	ARIMA $(0,0,0)(1,0,0)_{12}$	$n = 96$	1997
Nevada	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 72$	1998
Ohio	ARIMA $(0,0,0)(0,1,1)_{12}$	$n = 96$	1997
Oklahoma	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 72$	1998
West Virginia	ARIMA $(2,0,0)(0,0,0)_{12}$	$n = 120$	1993

* indicates that the data have been logarithmically transformed.

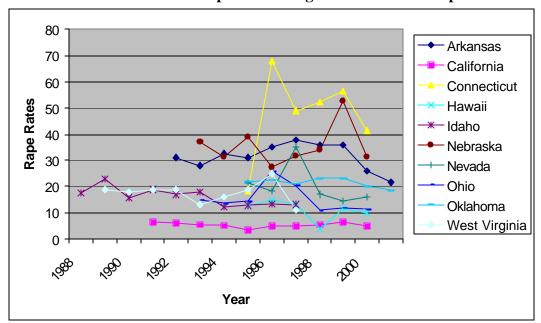[1] Difference non-stationary processes are the most logical non-stationary processes that criminological time series data follow.

Each state is listed in alphabetical order with noise models noted immediately to the right of the state name. Also included in Table 2 is the number of months to be analyzed. As stated earlier, the samples are symmetric in terms of the number of pre-intervention and post-intervention observations. For example, a sample size of 120 months (10 years of data, 5 years before and after the intervention) in Arkansas indicates that there are 60 pre-intervention observations and 60 post-intervention observations. The year in which the enforcement of the sex offender notification laws began in each state is noted in the table.

ANALYSIS AND FINDINGS

To glean an initial understanding of sex offending in our sample, Chart 1 indicates the rates of rapes that occurred in the 10 states.

Chart 1. Rates of Rapes Occurring in the 10 State Sample

As can be seen from Chart 1, most of the states (California, Hawaii, Idaho, Ohio, Oklahoma, and West Virginia) had very consistent rape rates across the years before and after the implementation of registries with notification capabilities. Arkansas, Connecticut, Nebraska, and Nevada showed inconsistency in rape offending patterns. The average rape rates in these 10 states ranged from 5.31 to 42.53 rapes per 100,000 people.

The results of the interrupted time series analyses are mixed with regard to whether the introduction of Megan's Law had a reductive effect on the number of reported rapes. Table 3 presents the results of these analyses.

Table 3. ARIMA Models for each State

State	Coefficient of Intervention	Standard Error	t-value	Probability
Arkansas	9.91	8.91	1.11	0.27
California	41.63	17.69	2.35**	0.02
Connecticut†	0.25	0.16	1.54	0.13
Hawaii	-1.72	0.87	-1.98*	0.05
Idaho†	-0.18	0.08	-2.27**	0.02
Nebraska†	0.26	0.19	1.36	0.18
Nevada	-0.22	1.4	-0.16	0.87
Ohio	-37.49	17.19	-2.18**	0.03
Oklahoma	2.36	6.41	0.37	0.71
West Virginia	-2.1	3.23	-0.65	0.52

* indicates $p < .10$
** indicates $p < .05$
† indicates logarithmically transformed data

Of the ten analyses, the following six show that the intervention corresponded to no statistically significant change in the monthly incidence of rapes: Arkansas, Connecticut, Nebraska, Nevada, Oklahoma, and West Virginia. Of these six states, only Nevada and West Virginia saw a decrease. Arkansas, Connecticut, Nebraska and Oklahoma had increased incidences of rapes associated with the sex offender notification laws. We report that the sex offender notification laws in these six states had no effect on the number of monthly rapes, however. The analysis suggests that the sex offender laws did not deter potential and repeat rapists from committing rapes in these six states.

The following three states- Hawaii, Idaho, and Ohio- have statistically significant decreases associated with the introduction of the sex offender notification laws. In addition, these states implemented the notification laws at different points in time. When examining these results independently of the other analyses, this scenario provides strong support for the notion that sex offender notification laws deter sex offenders. The idea that chance error or simultaneity took place in three states at three different points in time is difficult to argue. Particularly because a known intervention was implemented at this point in time, this analysis provides evidence for the notion that sex offender notification laws deterred potential sex offenders from offending and therefore, caused the observed decrease in rapes.

However, we are not examining these three states independently of the other seven states. To this point, in fact, only three of the nine discussed states experienced any significant decrease from the time of the intervention. The tenth state, California, actually had a statistically significant increase in the number of monthly rapes after the intervention. Based on the results of the analysis, the monthly number of rapes reported

in the state of California increased by an average of approximately 41 rapes per month. With regard to the significant decreases noted above, the significant increase in California occurred at yet another point in time.

In sum, we have five states, on one hand, that show decreases in the number of monthly rape counts associated with the implementation of sex offender notification laws. Of the five states that experience these decreases, three states have statistically significant decreases. On the other hand, the remaining five states show increases in the monthly number of rapes after the implementation of said laws. Of these five states, one state had a statistically significant increase. Although more discussion is provided below, the results do not offer a clear unidirectional conclusion as to whether sex offender notification laws prevent rapes.

A potential problem inherent in this analysis is how to interpret the data. As was illustrated in the review of the literature, a side effect of these laws should be a general deterrent effect on sex offenders, or indeed potential sex offenders, which should lower the overall number of sex offenses committed in the U.S. An alternative effect, however, may be that the number of sex crimes will increase as more attention is placed on potential sex offenders and their activities are more readily brought to the attention of the criminal justice system. A third alternative is that these two competing, but both positive, outcomes will offset each other—a reduction in the number of offenses, but a higher proportion of offenses discovered. It is anticipated that a careful analysis of sex offense data will reveal some of these competing assumptions. The next section of this report discusses the findings from the analysis.

DISCUSSION AND CONCLUSIONS

Sex offender registration and notification policies are a relatively recent development in the criminal justice system. As such, there has been neither a great deal of methodological development nor many empirical findings. This research has attempted to overcome this lack of research by empirically exploring the effect of sex offender registration and notification laws on the number of sex offenses, measured by rape, committed over time across the United States. Utilizing time series analysis (ARIMA), we examined the incidence of rape in a sample of 10 states to see if the sex offender laws had any noticeable influence on sex offenses.

The key finding of this research is that the passage of sex offender registration and notification laws have had no systematic influence on the number of rapes committed in these states as a whole. Most of the states in our sample (five of ten) showed no significant differences (increase or decrease) in the average number of rapes committed before and after the sex offender laws. These non-significant findings masked the fact that roughly half of these non-significant changes were actually increases in the average incidence of rape within a given state. Of the states (four of the ten states) that did indicate statistically significant findings, three of these states experienced a decrease in the number of rapes after the implementation of the laws. The fourth state, however, actually illustrated a steep, statistically significant increase in the number of rapes committed.

These findings beg the question of what is occurring concerning the relationship between sex offender registration and notification and subsequent sex offending. It is possible, as Sample's (2001) research indicated, that this was knee jerk legislation based simply on keeping the constituency happy. If that is true, sex offender registration and notification laws are failed legislation. As discussed above, there are alternative explanations of what could be occurring. These include a deterrent nature to these laws, a heightening of police awareness, both, or the rape counts simply decreased at the same time as the introduction of the law and therefore our model could be misspecified (i.e. confounded). We offer several suggestions of how these differing explanations may be producing the findings of this research.

First, sex offender registration and notification laws are based on the assumption that sex offenders are more likely to recidivate than other offenders. The research on the validity of this assertion is very mixed. While the nature of our data does not permit us to comment one way or the other in this area, sex offenders are assumed to be an identified group. As such, the rationale behind sex offender laws would assume that a majority of the offenses committed within the confines of any year would actually be sex offenders who are re-offending. A potential alternative assumption is that in those states with increases (roughly half of the states examined here), it would be expected that these offenders would be first time offenders or offenders who have never been caught before. This is supported by Walker and McLarty (2000) who found that 73% of sex offenders in their study committed a sex offense as their first offense. If this is the case, it would be expected that sex offender registration and notification is not able to control this population because a substantial number of them are committing sex offenses as their first offense. Hence, there is no name on the register and no way to inform the community.

Second, these laws may be having no influence on the number of sex offenses because communities are not actively using sex offender registries to protect their communities to the fullest extent possible. Community members may be desensitized to different notification strategies. For instance, Walker et al.'s (2001) research showed that in many cases in Little Rock, Arkansas, registered sex offenders in a majority of cases seemed to choose homes that clustered tightly around day cares, schools, and parks. This means that a potential exists for a motivated [sex] offender to come into contact with a suitable victim (children, in this instance) where there is a lack of some suitable guardian (teachers or parents) according to Cohen and Felson's (1979) work on the routine activities perspective. The significance of this finding is that the community is apparently doing nothing about these offenders.

Finally, increases in the average number of sex offenses may reflect an increased scrutiny from both communities and the police who are continually updated on the presence of sex offenders. Since there is an increase in the average number of sex offenses in half of the states examined here, police practices, in concert with community support, may now be focusing on more sex offenders. This would lead to an increase in the average number of sex offenses since law enforcement effort is now focused on a predetermined population that is relatively easy to find.

Based on the findings of this study and the potential conflicting explanations, future research on sex offender registration and notification policies should explore several different paths. First, a more appropriate unit of analysis would be the city.

Cities may be preferable because aggregate level time series data suffers greatly due to binning. As such, using smaller "bins," such as cities, might give more insight than using state level data, which loses detail in comparison. Second, because, sex offender registration is mainly addressing repeat offenders, future research should focus on sex offender recidivism before and after the enforcement of sex offender laws. Finally, future research should address sample size. While we intended to conduct time series analysis on all 50 states, after getting data from states with adequate follow-up time and excluding state data that did not conform to time series analysis, our final sample was only 10 states. Data mining techniques are constantly improving, and the follow-up periods in states now allow for at least a three year follow-up. Future time series research on sex offender policies should also explore these states.

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