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The Influence of Sex Offender Registration and Notification Laws in the United States

A Time-Series Analysis

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Although federal legislation for the implementation of sex offender registration and notification systems is now a decade old, empirical studies on the efficacy of this policy are relatively nonextant. This article explores the impact of registration legislation on the incidence of forcible rapes. Using monthly count data of rapes aggregated at the state level, this analysis uses Box–Jenkins autoregressive integrated moving average (ARIMA) models to conduct 10 intervention analyses on the enforcement of Megan's Law. The results of the analyses are mixed on 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 nonsignificant increase in the number of rapes, only three states had a significant reduction in rapes. Policy implications are discussed in terms of the efficacy of sex offender registration and whether changes in these laws should be considered.

Keywords: *sex offender registration and notification laws; sex offenders; interrupted time-series analysis; ARIMA*

Throughout the 1990s, laws intending to address the threat of sex offenders to the public were instituted in all 50 states in the United States. These legislative solutions came in the form of sex offender registration and

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notification laws, and usually stemmed from a series of highly publicized sex crimes against a child in which the perpetrator had some previous record of committing sex offenses and where the crime often resulted in the murder of the child. Public perception that sex offenders, when compared to other types of offenders, pose a much higher risk of reoffending also helped spur the passage of registration and notification laws.

Because sex offender laws are relatively new, however, empirical research that examines the efficacy of these laws is limited. Studies have chiefly described (a) the characteristics of currently registered sex offenders (Sample, 2001; Walker & Ervin-McLarty, 2000), (b) law enforcement and public perceptions of registration and notification (Matson & Lieb, 1996; Phillips, 1998; Zevitz & Farkas, 2000), and (c) sex offender recidivism through official data sources and rehabilitation treatment data (Petrosino & Petrosino, 1999; Schram & Milloy, 1995).

This article adds to this area of study by taking advantage of the natural or quasi experiment that took place as a result of the implementation of the legislation. In response to this "natural" design, the interrupted time series analysis technique is used to compare the number of monthly rapes reported to the Uniform Crime Report (UCR) at the state level before the intervention to the number being reported after the intervention. This technique allows us to test the general deterrence hypothesis, that is, the hypothesis that sex offender registration and notification laws reduce the overall incidence of rapes.

Review of the Literature

Current sex offender policies are based on three related assumptions. First, sex offenders are much more likely to recidivate than other offenders. The community, therefore, should be especially aware of these individuals. Second, by providing the means of surveillance, registries are thought to help protect communities from sex offender residents. Third, the offender is deterred in the presence of a community that is aware of their sex offender status. We review the literature and evidence surrounding these assumptions. Although questions concerning repeat offending cannot be answered with the data we use in this article, sex offender recidivism is discussed briefly. Findings from recidivism studies illuminate the problematic measurement and definitional issues involved in sex offender research. We then provide a review of evidence concerning the effect of sex offender registration and notification laws.

Sex Offenders and Recidivism

The assumption that sex offenders recidivate more than other offenders is a central motivation for registration and notification laws. Evidence to support the arguments that when compared with other types of offenders, sex offenders pose either a greater or lesser threat of recidivism exists in the literature (e.g., Furby, Weinrott, & Blackshaw, 1989; Greenfeld, 1997; Hall & Proctor, 1987; Hanson & Bussiere, 1998; Hanson & Morton-Bourgon, 2005; Langevin et al., 2004; Maddan, 2005; Meloy, 2005; Nisbet, Wilson, & Smallbone, 2004; Sample, 2001; Seto, 2005). The general evidence-based conclusion, however, is that there is no general conclusion regarding reoffending habits among sex offenders.

In their review of 49 articles principally concerned with sex offender recidivism, Furby et al. (1989, p. 27) note the "truly remarkable" difference in reported recidivism across studies. Some studies report recidivism rates above 50% whereas other findings suggest a marginal reoffending rate. This wide range of rates is not a discovery. In a prior literature review of recidivism among only rapists, Quinsey's (1984) conclusion resonates even today (e.g., Furby et al., 1989; Sample & Bray, 2006) and applies to sex offender recidivism knowledge in general. He states, "The difference in recidivism across these studies is truly remarkable; clearly by selectively contemplating the various studies, one can conclude anything one wants" (p. 101).

Criminologists simply do not know whether sex offenders are more or less likely to recidivate than other classifications of offenders. Bynum (2001) argues that the special case of sex offender recidivism, in relation to recidivism in general, has been defined and measured in three primary ways: subsequent arrest, subsequent conviction, and subsequent incarceration. 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, p. 2). And although "methodological difficulties, differences in sample size, and variability in follow-up lengths" are also cited as reasons for inconsistent recidivism rates (Sample, 2001, p. 106), the more basic and obvious problem is that such a wide span of conclusions is simply an expected by-product of assuming sex offenders are homogeneous while systematic differences across both offenders and offenses are likely.

A violent rapist, for an intuitive example, is likely to be fundamentally distinct from an adult offender whose victim is their 10-year-old family member. The difficult task of identifying these differences must receive considerable attention so that subtypes can be measured and highlighted in analyses. The casual use of the term *sex offender* leads to a definitional and measurement problem and one that necessarily obscures any differences among offenders and offenses. Beyond scientific investigation, policy makers as well as the community in general may benefit from knowing whether different types of sex offenders respond differently to various treatments or community responses to them. By recently adding other sex offenses (e.g., sex crimes against adults and possessing child pornography) warranting the same societal response (i.e., community notification), however, sex offender legislation implies similarity across all types of sex offenders and offenses regardless of type of offense, age of victim, or age of offender in relation to victim.

Whether all sex offenders share the same pattern of recidivism regardless of offense has been investigated (e.g., Furby et al., 1989; Quinsey, 1984; Sample & Bray, 2006; see also Craig, Browne, Stringer, & Beech, 2005; Langan, Schmitt, & Durose, 2003). In the most extensive review of the evidence and using Illinois sex offender data, Sample and Bray (2006) conclude that sex offenders are not the homogeneous group of offenders that laws seem to assume. In terms of general recidivism, where general recidivism is measured with any felony-related rearrest within 5 years of the qualifying sex offense, offenders in the child molestation category (i.e., the touching or fondling of victims younger than 18 years) had the highest rate of recidivism at 51.9% (Sample & Bray, 2006). Offenders in the pedophilia category, which includes offenses involving sexual penetration of victims 12 and younger, had a much lower recidivism rate at 31.4% (Sample & Bray, 2006). Among offenses involving sexual penetration, those who victimized adults 18 and older (rape) had higher recidivism rates than offenders who victimized children (pedophilia) and teens (hebophilia). Sample and Bray, in short, report statistically significant differences in recidivism rates across different types of offenders.

Studies analyzing a variety of sex offenders while assuming homogeneity are likely to produce misleading conclusions. In addition, there seems to be definitional inconsistency in measuring recidivism. For example, Furby et al. (1989, p. 7) note that to "recidivate is to relapse into former patterns of behavior," although the recommission of any criminal offense seems to be recidivism as well. There is "no single best definition of what constitutes recidivism for sex offenders," but "in the majority of cases it will be advisable to define recidivism as the re-commission of any sex offense" (p. 7). Furthermore, the homogeneity assumption suggests that sex offenders are particularly likely to recommit their specific offenses (Sample & Bray, 2006).

With regard to sexual recidivism, Sample and Bray (2006) report that within the 5-year period studied, those in the rape category have the highest same-offense rearrest rate (5.8%). As the authors state, "This evidence suggests that rapists recommit rape with greater frequency than pedophiles recommit pedophilia or hebophiles recommit hebophilia, again suggesting that sex offenders are not the homogeneous group that sex offender laws lead us to believe" (p. 94). When compared to general recidivism, same-offense rearrest rates are fairly small. Walker and McLarty (2000) examined the characteristics of sex offenders in the Arkansas sex offender registry from 1997 to 1999 and found that most (73%) sex offenders were first-time offenders. Either the sex offense was the first or the first to be reported.

Because of empirical and theoretical differences across sex offenders, analyses using specific offense data are more worthwhile. In the current article, for example, rapists and rapes are central; rape involves the carnal knowledge of a female forcibly and against her will (Federal Bureau of Investigation [FBI], 2004). For reasons discussed earlier, conclusions are not generalizable to other offenses.

Research on the Efficacy of Sex Offender Registration Laws

Proponents of sex offender registration and notification laws argue that such laws are effective because they inform the public of the presence of sex offenders and therefore danger in the community. These laws are also thought to reduce sex crimes because the public is able (and more likely) to report suspicious behavior by sex offenders. These viewpoints are supported by Phillips (1998), who reports that more than 60% of survey respondents agree that (a) sex offender laws make sex offenders "behave better" than they would have if their criminal history was not known, and (b) the majority of respondents feel safer with the laws in place.

Using a qualitative design, Matson and Lieb (1996) conducted a survey of law enforcement officials in the State of Washington. Answers from the survey instrument were categorized into advantages and disadvantages. Surveyed officials 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 (Matson & Lieb, 1996). 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 were the problems inherent in collecting information from courts and other agencies dealing with sex offender registration. Matson and Lieb found that overreactions to the notification in neighborhoods were possible. This could lead to harassment and embarrassment of sex offenders or their families.

As indicated by Zevitz and Farkas (2000), empirical research in this area has been limited. In a study of Washington's laws and in an effort to gain some basic demographic descriptions and differences across sex offenders, Schram and Milloy (1995) compared 139 Level 3 sex offenders with 90 sex offenders who were not subject to notification. The demographic characteristics of both juvenile ($N = 14$) and adult ($N = 125$) Level 3 sex offenders were obtained. Most of the juveniles in the sample were White and had histories of both sex and nonsex offenses. The sexual offending usually consisted of a single incident involving a child victim. Adult offenders were generally unemployed White males in their mid-30s who had never been married and who had a history of various offenses. They were typically child molesters and they had likely committed other sex offenses for which they had not been convicted.

Recidivism among both juvenile and adult Level 3 sex offenders was also examined. The vast majority (79%) of the juvenile offenders reoffended generally whereas fewer (43%) repeated sex offenses. The recidivism rates for adults were lower. Still, 42% of offenders recommitted either a general or a sex offense, and 14% of the offenders committed a sex offense. When the entire Level 3 sample was compared with a control group of sex offenders who were not notification eligible, Schram and Milloy (1995) found that community notification seemed to have little effect on sex offender recidivism. Furthermore, "the estimated rates for sex offenses are remarkably similar for each group throughout the follow-up period" (Schram & Milloy, 1995, p. 17). They concluded that community notification had little effect but conceded that their report is preliminary and that with a longer follow-up period and a larger sample, findings are likely to be different. Replication, therefore, is especially useful.

Petrosino and Petrosino (1999) presented an extensive study of the potential influence of registration and notification on sex offenders. They evaluated how well sex offender laws would work on a sample of 136 offenders in Massachusetts. Criminal history records of each offender were examined and the data were 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" (p. 146).

The offenders were all male and mostly White, and most offenses involved children. Cumulatively, Petrosino and Petrosino's (1999) 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" (p. 148). Petrosino and Petrosino concluded that "the public safety potential of the Massachusetts Registry Law to prevent stranger-predatory crimes . . . is limited" (p. 154). Furthermore, of the "instant offenses committed by 136 serious sex offenders, [they] 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" (p. 154).

Sample and Bray (2003) examined two of the underlying assumptions of sex offender registration and notification laws. The first assumption is that sex offenders are more likely to recommit their crimes (i.e., sex crimes) than other types of criminals. The second assumption is that some types of crime (drug use, burglary, etc.) serve as gateway offenses that lead to sexual offending. From an analysis of official criminal data in Illinois from 1990 to 1997, Sample and Bray found that of the sex offenders in Illinois, 93% were not rearrested for another sex offense. In terms of the latter preconceived notion, only 3% of offenders who were convicted of a nonsex offense were rearrested for a sex offense. Although the findings from this study may not be generalizable to other states, they do serve as a baseline for comparisons between other states' analyses of their sex offender registries.

Maddan's (2005) research indicates that sex offender registration and notification laws have no effect on sex offenders' recidivism rates. Using a quasi-experimental design in Arkansas, the author compared (a) three waves of sex offenders registered with the potential for community notification (i.e., treatment) to (b) three waves of sex offenders convicted a decade earlier. The group of sex offenders subject to community notification sexually recidivated 9.5% of the time whereas the group of sex offenders not subject to community notification recidivated 10.9% of the time. Maddan's findings indicate no support for the efficacy of sex offender registration and notification laws but he did report higher general recidivism among those in the treatment group.

As stated earlier, a major theme of the public support of sex offender notification laws is that because sex offenders pose a higher threat of danger and repeat offending, registration should be required. Once the laws are in place and the community becomes aware of sex offender residents, sex crimes will reduce for two reasons. First, an aware community reacts by minimizing opportunities for victimizations. Second, registered sex offenders, as opposed to nonregistered sex offenders, will be deterred as a result of perceiving a heightened level of community attention. The hypothesis to be tested in this article is that for various reasons and assumptions, sex offender registration and notification laws reduce sex crimes. We now turn to the empirical test of this hypothesis.

Research Method

The current research uses a quasi-experimental design to evaluate the general deterrent effect stemming from the notification component of sex offender registration laws. Interventions taking place in all U.S. states as well as the District of Columbia were the initial focus. For reasons discussed next, laws in only 10 states were analyzed.

Data

To define the before and after periods, we obtained the time when each state implemented a sex offender registry that included a notification strategy. Table 1 provides the year each state implemented a notification component.

Most of the states generated these laws after the passage of Megan's Law in 1996. Because of federal mandates and judicial decisions, however, the nature of registration and notification is fairly uniform across states.

Monthly state-level UCR rape data come directly from the FBI. As discussed earlier, the definition and operationalization of any concept deserves close attention. The definition of rape in these data is "the carnal knowledge of a female, forcibly and against her will." Statutory offenses are excluded (FBI, 2004, p. 19). Because rape is a Type I offense in the UCR, monthly counts were almost always readily available to the research team. The FBI supplied data from 1990 to 2000 for most states; before 1990, the data were either missing or unavailable. States with data from at least 3 years before and 3 years after could be included in the analysis.

With this strategy, some states lacked available data. Although efforts were made to collect data from the state in these cases, we were not entirely successful. As a result of poor or insufficient data, 13 states and the District of Columbia were excluded for three reasons. First, when states implemented legislation in the early 1990s, it was not possible to obtain an adequate number of preintervention observations. Second, when states implemented

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

State	Year	State	Year
Alabama	1998	Nebraska	1997
Alaska	1994	Nevada	1998
Arizona	1996	New Hampshire	1996
Arkansas	1997	New Jersey	1993
California	1996	New Mexico	1995
Colorado	1998	New York	1995
Connecticut	1998	North Carolina	1996
Delaware	1994	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	Washington, DC	1999
Minnesota	1998	West Virginia	1993
Mississippi	1995	Wisconsin	1997
Missouri	1995	Wyoming	1999
Montana	1995		

relevant legislation in the late 1990s, an adequate number of postintervention observations were not yet available. Third, some states did not report rape data in monthly format. Eight states[1] and the District of Columbia lacked data, and five states did not have data in monthly format.[2] After excluding these states, 37 states were left for examination. Discussed next are additional states that were excluded because of the behavior of the data.

Design and Technique

Given time-series data and the lasting nature of the intervention, the design that results is the time-series experiment. It is the type of quasi experiment that is sometimes called a natural experiment (Campbell &

Stanley, 1963). The strength of the present study is in the design and not the statistical technique that conventionally accompanies this design. In the time-series experiment, as opposed to a classical experiment where one defines treatment and control groups, the participant experiences an interruption (albeit in theory). In this case, the interruption is legislative and defines the before and after periods, where the before is viewed as the baseline period and the after is viewed as the treatment period, to which the baseline is compared. Campbell and Stanley (1963) use an iron bar being dipped into nitric acid as their example of a time-series experiment. The question becomes: Is there a difference between the bar before and after the exposure? In much the same way, we assess whether there is a difference in rape counts before and after the sex offender registration laws.

Because the rape data are kept as monthly counts and we are interested in analyzing each intervention separately, we refrain from using a fixed-effects panel model. Although it is the difference within each state as opposed to the averaged internal difference across states that we seek to measure, analyzing the effect of an intervention across varying places and times strengthens the design and can minimize chance error and historical threats (Campbell & Stanley, 1963; see also Shadish, Cook, & Campbell, 2002). Assume, for example, a significant reduction in reported rape counts is associated with the passage of the sex offender legislation. One would like to believe that the legislation is responsible for the decrease. Yet it is also possible that this decrease is simply due to chance error or is confounded with some third contemporaneously occurring mechanism. If, however, different states implementing the law at different times experience a decrease after the law, the likelihood of chance error or simultaneity being the true explanation for the decline is reduced.

The statistical technique conventionally applied with this design is the interrupted time-series analysis. For three reasons, this type of analysis focuses on within-series variation. First, the series may be nonstationary; in which a long-term mean or equilibrium is undefined. Because the before and after comparison is of the means of the two periods, a comparison involving a nonstationary series is impossible. Differencing the series, however, removes nonstationarity and yields a series with a well-defined mean.

Second, the before and after portions of the series are likely to be correlated; the observations in the after period are likely to be a function of observations in the before period. An independent comparison, therefore, is not possible. Third, 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. It has been noted that when using biased standard error estimates, the resulting t statistic can be inflated by 300% or more (McDowall, McCleary, Meidinger, & Hay, 1980). As a result, the statistical significance of an intervention's effect is vastly overstated.

To overcome this known shortcoming, we use univariate autoregressive integrated moving average (ARIMA) processes as noise models for this variation, thereby controlling for serial correlation. These models are commonly referred to as Box–Jenkins models (Box & Jenkins, 1976; see also McDowall et al., 1980). In addition to contributing to the development of ARIMA models, Box and Jenkins (1976) 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 to analyze the monthly rape data. As a result of the inadequate noise models and the ill-behaved data discussed in the Appendix, additional states were excluded. In the end, 10 states were kept for analysis: Arkansas, California, Connecticut, Hawaii, Idaho, Nebraska, Nevada, Ohio, Oklahoma, and West Virginia.

In these states, ARIMA models were used to control for systematic variation in the residual term. When series were nonstationary in their means, both the intervention and the series were differenced.[3] After estimating an adequate noise model, all 10 series fail to reject the joint null of the Jarque–Bera test; 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 ease of replication, Table 2 contains all relevant univariate information. 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 analyzed. The samples are symmetric in terms of the number of preintervention and postintervention observations. A sample size of 120 months in Arkansas indicates there are 60 preintervention observations and 60 postintervention observations. The year the enforcement of the sex offender notification laws began in each state is noted in the table.

Analysis and Findings

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.

Six states experienced no statistically significant change in the monthly incidence of rapes: Arkansas, Connecticut, Nebraska, Nevada, Oklahoma,

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[a]	ARIMA $(0,0,1)(0,0,0)_{12}$	$n = 72$	1998
Hawaii	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 72$	1998
Idaho[a]	ARIMA $(0,0,0)(0,0,0)_{12}$	$n = 120$	1993
Nebraska[a]	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

Note: ARIMA = autoregressive integrated moving average.
[a]Indicates the data have been logarithmically transformed.

and West Virginia. Although some of these six states experienced an increase in the incidences of rape after 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. This suggests that the sex offender registration and notification laws did not deter potential and repeat rapists from committing rapes in these six states.

The rape incidences in Hawaii, Idaho, and Ohio, however, significantly decreased after the introduction of the sex offender notification laws. With regard to chance error and design, it is important to note that these three states implemented the notification laws at different time points. The idea that chance error or simultaneity took place in three states at three different time points is difficult to argue. Particularly because a known intervention was implemented at this time point, this analysis provides evidence for the hypothesis that sex offender notification laws either deter potential sex offenders from offending or at least in some way cause the observed decrease in rapes. This scenario is ideal for providing support for the notion that sex offender registration and notification laws deter sex offenders.

However, we are not examining these three states independently of the other seven states. Only three of the nine states experienced any significant decrease from the time of the intervention. The rape incidences in California, the 10th state, significantly increased after the introduction of the sex offender notification laws. The number of rapes reported monthly in California increased by an average of approximately 41 rapes per month ($t = 2.35$, $p < .05$); with regard to the design, this increase occurs at yet another time point.

Table 3
ARIMA Models for Each State

State	Coefficient of Intervention	*SE*	*t* value	Probability
Arkansas	9.91	8.91	1.11	0.27
California	41.63	17.69	2.35*	0.02
Connecticut[a]	0.25	0.16	1.54	0.13
Hawaii	–1.72	0.87	–1.98†	0.05
Idaho[a]	–0.18	0.08	–2.27*	0.02
Nebraska[a]	0.26	0.19	1.36	0.18
Nevada	–0.22	1.40	–0.16	0.87
Ohio	–37.49	17.19	–2.18*	0.03
Oklahoma	2.36	6.41	0.37	0.71
West Virginia	–2.10	3.23	–0.65	0.52

Note: ARIMA = autoregressive integrated moving average.
[a]Indicates logarithmically transformed data.
†$p < .10$. *$p < .05$.

In sum, five states showed decreases in the number of monthly rape counts associated with the implementation of sex offender notification laws. Three of these five had statistically significant decreases. Data from the five remaining states show increases in the monthly number of rapes after the implementation of the laws. One state had a statistically significant increase. Although possible explanations for these results are discussed in the next section, the evidence does not offer a clear or unidirectional conclusion as to whether sex offender notification laws reduce rapes.

Discussion and Conclusions

Because sex offender registration and notification policies are a relatively recent development in the criminal justice system, this research has attempted to overcome the lack of empirical research on the effect of this legislation measured by monthly reported rapes across the United States. A potential problem inherent in this analysis is how to interpret the results and how to define rape. Although the definition of rape has been discussed, it should be emphasized that the data come from official sources. They are necessarily subject to the constraints associated with official data and are by definition offenses known to the police (see Biderman & Reiss, 1967). With these data and the preceding findings, a few scenarios are possible.

First, it remains possible that these laws present a deterrent effect on both sex offenders and potential sex offenders. This does not seem to uniformly be the case, however. Second, it is possible that as more attention is placed on potential sex offenders, their activities are more readily brought to the attention of the criminal justice system and the number of sex crimes seems to increase. This situation could be a result of an increase in the sensitivity of the measure and in turn results in the appearance of an increase in crime. A third possibility is that these two competing outcomes offset one another—a reduction in the number of offenses occurs but a higher proportion of offenses is discovered.

The empirical finding of this research is that the sex offender legislation seems to have had no uniform and observable influence on the number of rapes reported in the states analyzed. Most of the states in our sample (6 of 10) showed no significant change in the average number of reported rapes before and after the sex offender laws. Of the 4 states that did experience statistically significant changes after the legislation, 3 experienced a decrease in the number of rapes and 1 experienced a steep increase. Taken collectively, the findings reported here indicate that sex offender registration and notification laws may have had little general deterrent effects on the incidence of rape offenses analyzed.

As Sample (2001) indicates, it is possible that this was knee jerk legislation that simply became more attractive as public support increased. There is no doubt that these notification schemes provide effective means for surveillance and, legally speaking, regulation. If one were analyzing the legislation from the perspective of arming a community to reduce offenses as opposed to legislation that deters offenders, however, one would need to consider the possibility that when communities do not actively use sex offender registries to protect their members, the legislation fails to affect offenses.

It is also possible that 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. Because 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 more on sex offenders. This would lead to an increase in the average number of sex offenses because 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. Because aggregate-level time-series data suffer greatly because of binning, for instance, smaller "bins," such as cities, might give more insight. Differences in law enforcement notification practices and dissemination (e.g., the Internet, community meetings, fliers, and postcards) are more detectable with such units. In addition, sex offender registration may be more effective on repeat offenders or certain classes of offenders. Future research should focus on sex offender recidivism before and after the sex offender laws while considering offender and offense type.

Finally, no study design or statistical technique can control for all excluded variables. In the preceding analysis, within-series variation (i.e., the data-generating process) is explicitly modeled and held constant. Analyzing an intervention differing in place and time reduces historical threats; it does not eliminate all possible confounding variables. Future studies may examine this possibility with the use of "control" series. To more thoroughly understand effects of sex offender policy, additional empirical investigation is needed before evidence-based policy changes can be suggested.

Appendix

Although the identification phase consists solely of examining the autocorrelation function (ACF) or correlogram, we formally test for unit roots in the series with the augmented Dickey–Fuller test. We then identified a preliminary autoregressive integrated moving average (ARIMA) ARIMA$(p,d,q; P,D,Q)_{12}$ process based on the behavior of the ACF. After focusing on states whose autocorrelation processes could be reasonably modeled with conventional ARIMA patterns, we estimated the model. In the third phase, we examined the ACF and the Ljung–Box Q statistics to diagnose the adequacy of the model. The Jarque–Bera test was used to assess whether the estimated disturbance term was normally distributed with regard to its skewness and kurtosis. If the model adequately mimics the true data-generating process, the residuals exhibit independence and normality. In these cases, the state was considered for the interrupted time-series analysis. If we could not identify an adequate noise model, the state was excluded from later analyses.

Initially, only 7 states had adequate noise models and residuals that reflected normality. The remaining 30 states either had unconventional ACF patterns, highly skewed residuals, or both. Although the serial correlation for the data from Connecticut, Idaho, and Nebraska could be modeled adequately, the distributions were skewed. A logarithmic transformation reduced skews to normal levels. Each monthly observation in these states was not less than unity; therefore, adding an arbitrary value was not necessary for the transformation.

In all, 10 states remained for the interrupted time-series analysis: Arkansas, California, Connecticut, Hawaii, Idaho, Nebraska, Nevada, Ohio, Oklahoma, and West Virginia. In each of the 10 interrupted time-series analyses, the law was modeled as a dummy variable, where zero indicates the absence of the law. This variable, a transfer function, measures the abrupt and permanent change that is consistent with an analysis principally concerned with a before and after comparison.

Notes

1. These states are Kentucky, Louisiana, Massachusetts, New Jersey, South Carolina, Texas, Washington, and Wyoming.
2. These states are Florida, Illinois, Montana, Kansas, and Wisconsin.
3. Difference nonstationary processes are the most logical nonstationary processes that criminological time-series data follow.

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