DOES A WATCHED POT BOIL?
A Time-Series Analysis of New York State's Sex Offender Registration and Notification Law

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Despite the fact that the federal and many state governments have enacted registration and community notification laws as a means to better protect communities from sexual offending, limited empirical research has been conducted to examine the impact of such legislation on public safety. Therefore, utilizing time-series analyses, this study examined differences in sexual offense arrest rates before and after the enactment of New York State's Sex Offender Registration Act. Results provide no support for the effectiveness of registration and community notification laws in reducing sexual offending by: (a) rapists, (b) child molesters, (c) sexual recidivists, or (d) first-time sex offenders. Analyses also showed that over 95% of all sexual offense arrests were committed by first-time sex offenders, casting doubt on the ability of laws that target repeat offenders to meaningfully reduce sexual offending.

Keywords: sex offender, registration, notification, time-series analysis, public policy

Few types of crime command the same public attention and evoke the same level of outrage as sexual offenses. This fact is reflected in the unique handling of such offenses both by legislative bodies and media outlets. The most obvious example of differential legislative treatment is the relatively recent rise of registration, community notification, and residency restriction laws for sex offenders released back into local communities, as well as civil commitment laws for offenders about to be released. Such regulations have been largely inspired by public reactions to particularly heinous sexual offense cases (e.g., Adam Walsh Children Protection and Safety Act, 2006; Megan's Law, 1996; The Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act, 1994; The Pam Lychner Sexual Offender Tracking and Identification Act, 1996), yet registration and notification are not required for perpetrators of other heinous crimes such as murder or domestic violence.

Evidence of the differential treatment of sexual crimes can also be found in the media. For example, research has shown the media to overreport sexual crimes such as rape by a factor of almost 14 times compared with their actual rate of

incidence (Ditton & Duffy, 1983) and to present sexual crimes in a manner that inspires fear significantly more often than it does when reporting a homicide, robbery, or assault (Dowler, 2006). Although researchers debate the exact reason(s) for this overrepresentation, most explanations center around the idea that sexual offenses are deemed to be more newsworthy and of interest to the public (Greer, 2003). Regardless of the rationale behind such differential reporting, however, some researchers are concerned about its impact. For example, Jones (1999) argued that "the media's desire to highlight only the most violent and horrendous sex crimes (with little attention focused on the rehabilitation of sex offenders) plays a large role in perpetuating society's belief that sex offenders cannot be rehabilitated" (p. 86). According to Jones, this perception, in turn, evokes an emotional response to sexual offenses, causing the public to lobby for legislation such as registration and community notification, which may have little meaningful impact on the actual rates of sexual victimization, given that the majority of sexual offenses are committed by a relative or acquaintance (Greenfeld, 1997; Snyder, 2000). Furthermore, this perception that sex offenders cannot be rehabilitated is not supported by research, which has found cognitive-behavioral therapy to significantly reduce rates of sexual recidivism (see Craig, Browne, & Stringer, 2003; Hatch-Maillette, Scalora, Huss, & Baumgartner, 2001; Lösel & Schmucker, 2005; McGrath, Cumming, Livingston, & Hoke, 2003).

Associated with the special level of attention given to sexual offenses (probably both contributing to and resulting from it) is a belief that released sex offenders pose a greater threat to communities than other released offenders. In his detailed analysis of sexual crime media coverage, Greer (2003) found "there is a clear assumption of recidivism, a taken-for-granted notion that sex offenders *will* recidivate" (p. 138). As with the assumption that sex offenders cannot be rehabilitated, this assumption is also not supported by research. For example, in its detailed analysis of 15 states, the Bureau of Justice Statistics found that only 5.3% of the 9,691 sex offenders released in 1994 were re-arrested for a new sex offense within 3 years of being released (Langan, Schmitt, & Durose, 2003), compared with re-arrest rates of 73.8% for property offenders and 66.7% for drug offenders (Langan & Levin, 2002).

Based on the assumptions mentioned earlier, however, it is not surprising that a great deal of effort has been spent crafting legislation that seeks to minimize future sexual victimization. Registration and community notification laws represent two such legislative initiatives. As a means to increase public safety, registration and notification laws seek to: (a) allow residents to know the whereabouts of convicted sex offenders, (b) serve as a deterrent for future sexual offenses, and (c) assist local law enforcement agencies in investigating and solving future sexual offenses (Lovell, 2001; Phillips, 1998). Despite the widespread use of these laws, little empirical research has investigated whether they are, in fact, increasing public safety. Such investigations are important given continuing, and often expensive, legislative efforts directed at convicted sex offenders, such as the Adam Walsh Child Protection and Safety Act (2006; which the Congressional Budget Office estimates will cost $1.5 billion over 5 years to implement). Thus, to address this gap in the current literature, the present study examined differences in sexual offense arrest rates before and after the enactment

of New York State's Sex Offender Registration Act (SORA) to determine whether these laws are increasing public safety.

Effectiveness of Registration and Community Notification Laws

All 50 states and the District of Columbia have laws requiring the registration and community notification of convicted sex offenders (Lovell, 2001; Thomas, 2003). Surprisingly, however, little research has evaluated whether registration and community notification laws make released sex offenders more law abiding than they would be without such laws, and whether these laws do, in fact, increase public safety (Welchans, 2005).

The Washington State Institute for Public Policy (Schram & Milloy, 1995) conducted the first outcome study examining the effects of community notification. Re-arrests of 90 sex offenders who received the highest level of notification were compared with 90 matched sex offenders who were released prior to the enactment of the law and, thus, were not eligible for community notification. Although at the end of a 54-month period there was no significant difference in re-arrest rates between the two groups, the researchers determined (from survival curves) that sex offenders who were subject to community notification requirements were re-arrested more quickly than those not subject to notification. However, this study examined only recidivism as an outcome and had a very limited sample.

More recent research has continued to evaluate the effects of registration and community notification laws on sex offender recidivism rates. These studies have (a) examined convicted sexual psychopaths (a legal, statutory label, not a psychological label following clinical diagnosis) in an attempt to determine the likelihood that community notification would prevent future sexual offenses (Petrosino & Petrosino, 1999), (b) compared registered sex offenders subject to registration and community notification requirements with convicted sex offenders who would have been subject to such requirements had the laws been in effect at the time of their convictions (Adkins, Huff, Stageberg, Prell, & Musel, 2000), and (c) compared sex offenders who received extensive notification with those who received limited notification (Zevitz, 2006). Despite the differences in methodologies, all of these studies found limited support for the effectiveness of registration and community notification laws to reduce sex offender re-arrest and reconviction rates.

A recent study by Barnoski (2005), however, did find that community notification laws significantly reduced certain types of recidivism by sex offenders in Washington State. Specifically, Barnoski found that the 1990 enactment of Washington's Community Protection Act significantly reduced rates of sexual felony recidivism by sex offenders, and that the 1997 amendment of the notification law significantly reduced rates of both violent felony and sexual felony recidivism by sex offenders. However, this study had a number of weaknesses. First, as with the earlier analysis by Schram and Milloy (1995), it looked only at sex offender recidivism, ignoring the possibility that notification laws had any effect on rates of first-time offending. Second, as the analyses examined rates of recidivism at three separate points in time through percentage comparisons and a binary logistic regression, they did not take into account natural changes in

patterns of offending over time (McDowall & Loftin, 2005). As such, the reductions in offending noted by Barnoski (2005) may have simply been due to historical crime rate trends and, therefore, may have been unrelated to the enactment and amendment of Washington's registration and notification law.

In an attempt to account for any historical trends in crime rates, Walker, Maddan, Vásquez, VanHouten, and Ervin-McCarthy (2005) used time-series analysis to examine the number of rapes reported monthly through the Uniform Crime Reporting (UCR) system in 10 states to determine the general deterrent effect of registration and community notification laws. Consistent with previous research, results of their analyses indicated no systematic effects for registration and community notification laws to reduce incidents of sexual victimization. More specifically, there was no significant difference in the number of rapes before and after the passage of registration laws for six out of the ten states examined, and although three states did experience a significant decrease in rapes after the enactment of registration laws, one state experienced a sharp and significant increase. However, as the study did not model any nonsexual offense series, the observed changes could be the result of general interventions directed at all offending (e.g., changes in policing, crime reporting, sentencing) and not specifically a result of the enactment of registration laws. Furthermore, due to limitations of UCR data, the study was not able to model crimes committed by repeat versus first-time sex offenders, thereby not allowing for differential effects of the registration laws on these different offender groups.

Thus, taken as a whole, the results of the various studies cited above support the view of Zevitz (2006) that, "the anticipated preventive benefits of the community notification policy initiative would appear to be limited" (p. 205).

Purpose

Given the lack of conclusive research regarding the effectiveness of registration and community notification laws to increase public safety, the current study sought to build on the extant literature by examining sexual offense arrest rates before and after the enactment of New York State's SORA. Thus, the primary research question was: Are there differences in sexual offense arrest rates before and after the enactment of SORA? Two additional research questions were: (a) Are registration and notification laws decreasing re-arrest rates for convicted sex offenders? and (b) Are registration and notification laws deterring nonregistered offenders from committing registerable sexual offenses?

Method

New York State Registry

New York State, in compliance with federal regulations, established SORA in 1995, which became effective January 21, 1996. Under Correction Law Article 6c, individuals convicted of registerable sexual offenses are required to release specified information to the state, such as their name and current home address. Sex offenders who were convicted, were under probation or parole supervision, or were discharged, paroled, or released on or after January 21, 1996, are mandated to register under this Act (Division of Criminal Justice Services, 2004).

In New York State, sex offenders are classified into three risk levels based on the court's assessment regarding offenders' likelihood to repeat the same or similar registerable offense. Decisions regarding risk levels are made based on, amongst others, offender's relationship to the victim, duration of the offense, use of a weapon, age of the victims, and extent to which the victim was injured. Level 1 represents a low risk of repeat offense; Level 2 indicates a moderate risk of repeat offense; and Level 3 represents a high risk of repeat offense. Risk levels determine both the length of registration as well as the extent of community notification, with Level 2 and Level 3 offenders registering for life and being subjected to the highest extent of community notification practices.

Local law enforcement agencies in communities where sex offenders live have the discretion to decide what, if any, information to release to vulnerable populations related to the nature of the offense committed by the offender. However, only information pertaining to Level 2 and Level 3 offenders can be released to the public. Although SORA did not standardize notification procedures, there are four main forms of community notification: (a) the public sex offender registry Web site, (b) community notification meetings, (c) dissemination of flyers and other mailings, and (d) informal communication with residents or door-to-door visits.

Data Source

Data for this study were retrieved from New York State offender criminal history files, which were extracted by the New York State Division of Criminal Justice Services. These criminal history files contain information regarding characteristics related to arrest, conviction, disposition, and sentencing events. The criminal history files of every offender arrested for a registerable sexual offense between 1986 and 2006 (totaling over 170,000 sexual offense arrests and over 160,000 unique sex offenders) were used in this study.

Data

Data for the analyses consisted of 21 years (252 months) of New York State monthly arrest counts for several types of offending aggregated to the state level.[1] Arrest counts were chosen as the focus for the study as sexual crimes are less likely than nonsexual crimes to be reported to authorities, and many that are reported are never prosecuted (Romeo & Williams, 1985). Thus, using a measure of conviction instead of arrest might considerably underestimate rates of offending. However, it should be noted that simply because an offender was arrested for a sexual offense does not imply the offender was convicted of that crime and, therefore, it is possible that using arrest data produces false-positive results (Romeo & Williams, 1985). Given the serious nature of sexual offenses and the amount of public attention they command, risking a false-positive result (i.e., finding that sex offender registries do reduce rates of sexual offending when they in fact do not) may be more defensible than risking a false-negative result (i.e.,

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[1] Offenses were assigned to a month by the crime date whenever possible, and by the arrest date when a crime date was not available. Of the 894,002 total arrests aggregated for the current study, only 18,366 (2.05%) were missing a crime date.

finding that sex offender registries do not reduce rates of sexual offending when they in fact do).

The monthly arrest counts began with January 1986 (10 years before the enactment of SORA) and ended with December 2006 (11 years after the enactment of SORA). During this time period, New York State enacted no other special legislation to manage sex offenders, thereby allowing for clear interpretation of the impact of SORA's enactment.

In all, 17 different series of data were modeled: 9 test series and 8 comparison series. It is important to note, however, that SORA was amended twice since its 1996 enactment to include more offenses (eight were added in 1999 and seven were added in 2002). As the offenses added in these later amendments have different intervention dates, each of the nine test series modeled in the present study included only those offenses listed in the original 1996 version of SORA.[2] Also, the New York State consolidated criminal history files are top-charged based, meaning they only record the top charge associated with each arrest. Thus, if an offender had been arrested for fraud, robbery, and rape, only the rape charge would be recorded in the consolidated criminal history files. This fact is important to remember, especially with regard to the comparison series (i.e., sexual offenses are almost always the top charge for an arrest, while crimes such as larceny are not).

Test Series

Total number of registerable sex offenses (RSOs). This series included all arrests for any of the offenses that required registration as of January 1996 in New York State (see SORA, §168a), including rape, incest, sodomy, sexual misconduct, sexual abuse, and promoting sexual performance by a child. The mean number of total RSO arrests per month was 640.73 ($SD = 96.20$), with 169,051 different offenders having been arrested for a RSO from 1986 to 2006.[3]

Total number of rapes. As many studies and typologies of sex offenders have found rapists and child molesters to be characterized by different offending patterns (e.g., Hood, Shute, Feilzer, & Wilcox, 2002; Knight, Rosenberg, & Schneider, 1985), these types of offending were broken out from the total RSO variable and analyzed separately. Thus, this series included all arrests for rape in the third, second, or first degree (PL §130.25–130.35). According to these statutes, rape in New York State is generally defined as sexual intercourse (forcible or otherwise) either: (a) without the other party's consent, or (b) with a party incapable of giving consent. The mean number of rape arrests per month was 166.42 ($SD = 26.97$).

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[2] Attempts were made to model the offenses added in these later amendments separately (i.e., all the 1999 additions together and all the 2002 additions together), but the small number of monthly arrests for these few offenses made the analyses difficult, if not impossible, to reliably conduct and interpret. Those results that were interpretable, however, were similar to those generated for the original 1996 offenses.

[3] Of the 169,051 different offenders who were arrested for a RSO from 1986 to 2006, 68,617 different offenders (40.59%) were convicted of a RSO.

Total number of child molestations.   The monthly counts of arrests for child molestation were created by summing the monthly arrest counts for several sexual crimes committed against children, including sexual acts against children (PL §130.45–130.50), the use and promotion of children in a sexual performance (PL §263.05–263.15), and the possession of obscene material involving children (PL §263.16). By analyzing arrests for child molestation arrest separately from those for rape, the analyses were able to test whether the significant declines in child sexual abuse observed in the 1990s (Jones, Finkelhor, & Halter, 2006; Mitchell, Finkelhor, & Wolak, 2007) were related (at least in New York State) to the enactment of registration and notification laws. The mean number of child molestation arrests per month was 42.08 ($SD = 10.42$).

RSOs by convicted offenders.   This series included all RSO arrests of offenders who had previously been convicted of a sexual offense. In other words, this series was a measure of general sexual recidivism. Previous conviction of a registerable offense was used as the prerequisite for recidivism rather than previous arrest, as only convicted sex offenders are added to the registry (see SORA, §168a). Thus, to accurately test the effect of registration on already registered offenders (or offenders who would have been registered, for those convicted of a RSO prior to SORA's enactment), the offender had to have a previous RSO conviction. The mean number of RSO arrests of offenders with a prior RSO conviction (i.e., sexual recidivism) per month was 26.43 ($SD = 10.50$), which represents 4.12% of all RSO arrests per month.

Rapes by convicted offenders.   This variable was calculated in the same way as RSOs by convicted sex offenders, but was specific to rape arrests following an RSO conviction. The mean number of rape arrests for offenders with any prior RSO conviction per month was 6.75 ($SD = 3.64$), which represents 4.06% of all rape arrests per month.

Child molestations by convicted offenders.   The mean number of child molestation arrests of offenders with any prior RSO conviction per month was 2.47 ($SD = 2.00$), which represents 5.88% of all child molestation arrests per month.

RSOs by nonconvicted offenders.   To examine the possible deterrent effect of SORA on those who were unregistered at the time of their offense, this series included only RSO arrests of those offenders who had not previously been convicted of a sexual offense. The mean number of RSO arrests of offenders without a prior RSO conviction per month was 614.31 ($SD = 90.96$), which represents 95.88% of all RSO arrests per month.

Rapes by nonconvicted offenders.   The mean number of rape arrests for offenders without any prior RSO conviction per month was 159.67 ($SD = 25.72$), which represents 95.94% of all rape arrests per month.

Child molestations by nonconvicted offenders.   The mean number of child molestation arrests for offenders without any prior RSO conviction per month was 39.61 ($SD = 9.94$), which represents 94.12% of all child molestation arrests per month.

The average number of monthly arrests in New York State for each of these nine series can be found in Figures 1 (RSOs), 2 (rapes), and 3 (child molestations).
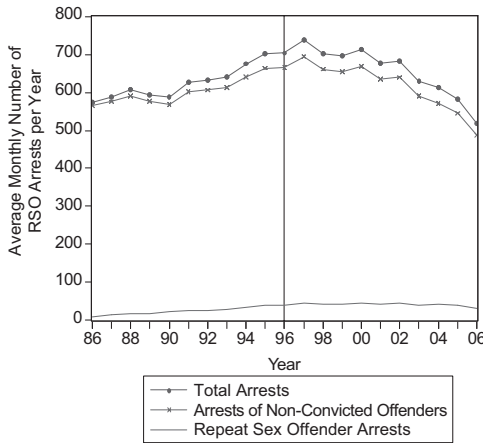
Figure 1. Average monthly registerable sex offense (RSO) arrests per year before and after the enactment of State's Sex Offender Registration Act (SORA).

Comparison Series

As this study specifically sought to examine the effectiveness of New York State's registration and community notification law in reducing sexual offending, it was necessary to examine the effect of other possible influences that might have also reduced rates of sexual offending independent of SORA. For example, changes in policing and sentencing styles over the last 20 years may have also altered offending patterns, such that all types of offending (or all types of interpersonal offending) declined over this period. If this were in fact the case, any reductions in sexual offending may be due to influences other than the enactment of SORA.

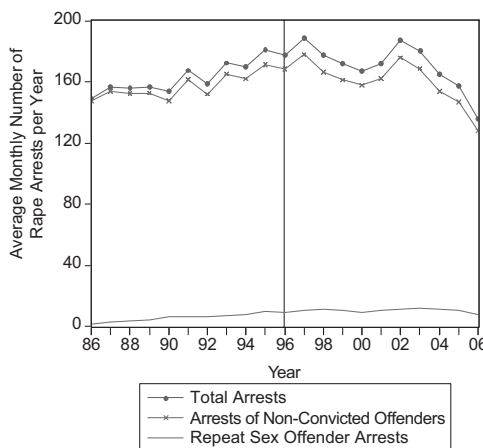Thus, to investigate other influences or alternative explanations for any

Figure 2. Average monthly rape arrests per year before and after the enactment of State's Sex Offender Registration Act (SORA).
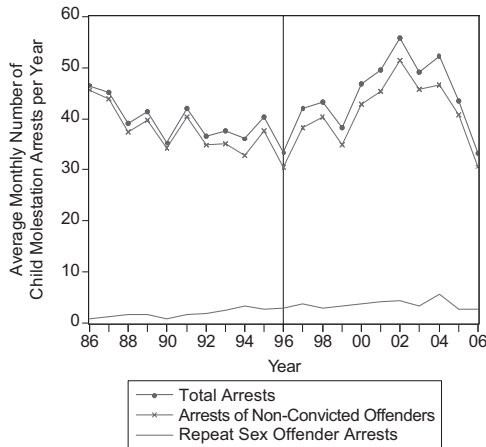
Figure 3.   Average monthly child molestation arrests per year before and after the
enactment of State's Sex Offender Registration Act (SORA).

changes in sexual offending behavior (and therefore clarify the impact of SORA),
comparison series of arrests were examined for four types of nonsexual offenses,
two interpersonal crimes (assault and robbery), and two property crimes (burglary
and larceny). Because these series were intended to control for influences oper-
ating both outside and within the group of offenders included in the sexual
offending analyses, each of these offending types were modeled two ways: (a) all
arrests for non-sex offenders in New York State (outside group influences), and
(b) all arrests for sex offenders in New York State (i.e., those who were arrested
for a RSO between 1986 and 2006; within group influences). That is, this second
group of comparison series was composed only of non-sexual arrests by sex
offenders, and did not include nonsexual arrests by non-sex offenders.

   Total number of assaults.   This series included all arrests for assault, which
in New York State is generally defined as recklessly, negligently, or intentionally
causing a person injury (PL §120.00–120.10). The mean number of assault arrests
per month for all non-sex offenders in New York State was 5,118.56 (SD =
887.24), whereas for this study's sample of sex offenders it was 453.61 (SD =
84.02).

   Total number of robberies.   This series included all arrests for robbery,
which in New York State is generally defined as using or threatening to use
immediate physical force during the commission of a larceny (PL §160.00). The
mean number of robbery arrests per month for all non-sex offenders in New York
State was 1,570.23 (SD = 381.31), whereas for this study's sample of sex
offenders it was 229.72 (SD = 84.06).

   Total number of burglaries.   This series included all arrests for burglary,
which in New York State is generally defined as knowingly and unlawfully
entering or remaining in a building with the intent to commit a crime therein (PL
§140.20–140.30). The mean number of burglary arrests per month for all non-sex
offenders in New York State was 2,425.53 (SD = 374.99), while for this study's
sample of sex offenders it was 243.35 (SD = 40.37).

Total number of larcenies.   This series included all arrests for larceny, which in New York State is generally defined as the wrongful taking, obtaining, or withholding of property from the property's rightful owner (PL §155.05). The mean number of larceny arrests per month for all non-sex offenders in New York State was 5,273.93 (SD = 671.25), whereas for this study's sample of sex offenders it was 210.15 (SD = 36.31).

Intervention Variable

The intervention variable (or variable of interest) in the present analysis was the enactment of SORA in January 1996. Thus, a dichotomous variable was created to represent the enactment of SORA, with the variable being coded as zero (*before SORA*) for all months prior to January 1996 and coded as one (*after SORA*) for January 1996 and all months thereafter.

Analysis

Univariate Box-Jenkins interrupted autoregressive integrated moving average (ARIMA) analyses were used to test the effect of SORA's enactment on all 17 offense series. This analytic approach was selected due to its ability to model the autocorrelation almost always found in time series data (McDowall, McCleary, Meidinger, & Hay, 1980). That is, although the analysis in its basic form is a comparison of the number of monthly arrests series before the enactment of SORA versus after the enactment of SORA, simply using ordinary least squares regression (OLS) to conduct the comparison would be unreliable. Specifically, using OLS on autocorrelated data will result in negatively biased standard errors, which then result in artificially and incorrectly inflated t values (McDowall et al., 1980). These inflated t values lead to deflated significance (p) values, meaning the analysis has a much greater chance of returning a false-positive result (i.e., finding significance when there is in fact none). ARIMA time series analyses are, therefore, an improvement over OLS when analyzing time series data, as ARIMA models remove the influence of the autocorrelation from the analysis.

The Box-Jenkins approach to ARIMA analyses involves a three-phase process: (a) identification, (b) estimation, and (c) diagnosis (Box & Jenkins, 1976). In the identification phase, the autocorrelation process or processes (autoregressive, integrated, moving average, or some combination thereof) at work in the data are identified by examining the autocorrelation function (ACF; or correlogram) and partial autocorrelation function (PACF) for the series. Once the autocorrelation process(es) has been identified, a model is then estimated in an attempt to remove the autocorrelation. Following the estimation, the possible presence of residual autocorrelation in the data is investigated in the diagnosis phase through examination of the ACF, PACF, and Ljung-Box Q-statistics for the estimated model. If there is no residual autocorrelation (i.e., all that remains is uncorrelated white-noise), the model is deemed to fit the data. If there is residual autocorrelation, however, the model is deemed to not fit the data and the identification, estimation, and diagnosis phases are repeated.

Results

As stated earlier, the analysis of each series began with an examination of the series' ACF and PACF. In all 17 cases, both the ACF and PACF showed integrated processes to be present in the data. Thus, each series was differenced (i.e., lagged one time period and subtracted from itself) in order to control for the integrated autocorrelation. Differencing the equations also changed the analysis from being one of the raw arrest counts per month to being one of the change in arrest counts from month to month, which does not alter the shape of the analysis or its interpretation (McDowall et al., 1980). The ACFs and PACFs of these differenced variables were then examined, and all showed 12-month seasonal integrated patterns to be at work in the data (which is common when analyzing monthly data, as many crime rates rise and fall with the seasons). This 12-month seasonal integrated autocorrelation was then controlled for by seasonally differencing each of the normally differenced series (i.e., by lagging each series 12 months and subtracting it from itself). The ACFs and PACFs of each series were then clearer of autocorrelation, though they still showed both first-order moving average and first-order 12-month seasonal moving average processes present in the data. Thus, equations were estimated in which the dependent variables (arrest counts per month) were differenced both normally and 12-month seasonally, with each equation including components to control for both the first-order moving average and the first-order 12-month seasonal moving average processes, as well as a constant.[4] At this point the ACFs, PACFs, and Ljung-Box Q-statistics for each series showed no residual autocorrelation at any specific point in the data or in the dataset as a whole, meaning the final model ARIMA $(0,1,1)(0,1,1)_{12}$ controlled for all the autocorrelation in the analysis.[5]

Once all the autocorrelation was removed from the analyses (i.e., all that was left was white noise), the dichotomous intervention variable was included in the model. By being coded zero for all months before January 1996 and one for all months afterward, it is essentially a test of whether the average number of monthly arrests before the enactment of SORA differed from those after the enactment. If the coefficient for the variable was positive in an analysis, the number of arrests rose after the enactment of SORA, while a negative coefficient indicated the number of arrests dropped. Before the coefficient for the intervention variable was assessed in any of the analyses, however, the ACFs, PACFs, and Ljung-Box Q-statistics for the equations with the intervention were assessed to see if each remained free from autocorrelation. As in each case the series showed no residual autocorrelation at any specific point in the data or in the dataset as a whole (and the coefficients for both moving average components fit their neces-

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[4] If the constant was found to be insignificant (indicating the integrated process was most likely a random walk), it was removed and the equation was re-estimated in order to make estimation of the standard errors more efficient. Otherwise, if the constant was significant (indicating the integrated process was most likely a stochastic trend and the average number of arrests for that type of crime was significantly different than zero), it was retained in the model.

[5] This model, ARIMA $(0,1,1)(0,1,1)_{12}$, is sometimes referred to as the airline model (Box & Jenkins, 1976), and is fairly common in practice.

sary parameters), the intervention coefficients could be meaningfully interpreted (see Table 1).

As can be seen in Table 1, none of the intervention coefficients for any of the nine types of sexual offending reached significance and were, therefore, all statistically no different than zero. Likewise, as can be seen in Table 2, seven of the eight intervention coefficients for the comparison series were also insignificant, with only the number of robbery arrests within the sample of sex offenders showing a significant reduction following the enactment of SORA. Specifically, there was an average of 31.32 fewer robberies per month ($p < .05$) committed by those arrested for a RSO between 1986 and 2006 following the enactment of SORA than there was before SORA. This finding should be interpreted with caution, however, as it was the only series of the 17 tested to reach significance. That is, according to probability, 1 out of 17 series should reach the $p < .05$ level of significance purely by chance. If a Bonferroni correction were made to the significance level to account for the increased possibility of committing a Type I error (i.e., a false positive), the observed change in robbery arrests within the sex offender sample pre- and post-SORA would not reach significance.

While this study's finding of significantly reduced rates of robbery within the sample of sex offenders supports one finding of Barnoski (2005; i.e., that the enactment of Washington State's community notification law reduced rates of violent felony recidivism by sex offenders), the null findings with regard to the impact of registration and community notification on assault and sexual recidivism do not. These conflicting findings are likely due to two facts: (a) Barnoski's analytic technique did not account for historical crime trends, and (b) as Barnoski's regressions were performed on autocorrelated data, the coefficient standard errors were

Table 1

Monthly Arrest Averages and Times Series Results by Offender and Offense Type for Test Series

	Monthly arrests, mean (SD)	Percentage of total	Intervention coefficient[a]	t
Registerable sex offenses				
Total	640.73 (96.20)		9.85	0.31
Recidivisms	26.43 (10.50)	4.12	−0.14	−0.05
First-time offenses	614.31 (90.96)	95.88	10.49	0.35
Rapes				
Total	166.42 (26.97)		−1.58	−0.14
Recidivisms	6.75 (3.64)	4.06	0.36	0.50
First-time offenses	159.67 (25.72)	95.94	−3.12	−0.28
Child molestations				
Total	42.08 (10.42)		−10.00	−1.92
Recidivisms	2.47 (2.00)	5.88	0.17	0.26
First-time offenses	39.61 (9.94)	94.12	−8.88	−1.81

Note. All models were of the form autoregressive integrated moving average (ARIMA) $(0,1,1)(0,1,1)_{12}$, meaning they had both first-order and seasonal first-order integrated and moving average components.
[a] The intervention coefficient represents the average monthly change (after correcting for autocorrelation in the data) in arrests for each offense type after enactment of the Sex Offender Registration Act.

Table 2

Monthly Arrest Averages and Times Series Results by Offense Type for Control Series

	Monthly arrests, mean (SD)	Intervention coefficient[a]	t
All New York State offenders			
Assaults	5,118.56 (887.24)	98.71	0.56
Robberies	1,570.23 (381.31)	−69.44	−0.84
Burglaries	2,425.53 (374.99)	69.47	0.69
Larcenies	5,273.93 (671.25)	−156.45	−0.95
Within the sex offender sample			
Assaults	453.61 (84.02)	−26.90	−1.12
Robberies	229.72 (84.06)	−31.32	−2.04*
Burglaries	243.35 (40.37)	−7.68	−0.40
Larcenies	210.15 (36.31)	1.42	0.11

Note.   All models were of the form integrated moving average (ARIMA) $(0,1,1)(0,1,1)_{12}$, meaning they had both first-order and seasonal first-order integrated and moving average components.
[a] The intervention coefficient represents the average monthly change (after correcting for autocorrelation in the data) in arrests for each offense type after enactment of the Sex Offender Registration Act.
* $p < .05$.

likely deflated and, therefore, appeared more significant than they in fact were (see above). The possibility of a natural drop in the crime rate or some non-sex offender related factor contributing to Barnoski's findings is supported by the fact that Washington State's rate of violent crimes (per 1,000 population) dropped each year from 1995 to 2006, while its rate of property crimes (per 1,000 population) dropped each year from 1995 to 2003 (Washington Statistical Analysis Center, 2008). Thus, it appears likely that the reductions in the sexual and violent felony recidivism of sex offenders observed by Barnoski may have been at least in part due to these trends, and once these trends were controlled for in the present study, the impact of registration and notification laws failed to reach significance.

Finally, to test whether the use of arrest counts was obscuring the impact of SORA's enactment, analyses were also conducted on series for: (a) number of monthly RSO convictions, and (b) the monthly ratio of RSO convictions to RSO arrests. As with the arrest analyses, nine different tests series (i.e., all RSOs, rapes, and child molestations examined by total number, sexual recidivisms, and first-time sex offenses) were modeled for each of these different count types. Although not shown, and as with the arrests series, none of these additional conviction series showed any significant change from before the enactment of SORA to afterward. Thus, it appears that the enactment of SORA had little, if any, impact on rates of general offending in New York State and no significant impact on rates of sexual offending.

Discussion

The present study used 252 months of arrest data and univariate ARIMA time series analyses to evaluate the impact of New York State's SORA. More specif-

ically, the study proposed the general question of whether there are differences in sexual offense arrest rates before and after the enactment of SORA, as well as the two specific questions of: (a) whether registration and notification laws are decreasing re-arrest rates for convicted sex offenders, or (b) whether registration and notification laws are deterring nonregistered offenders from committing registerable sexual offenses. According to the analyses, all three of these questions are answered negatively. That is, results of the analyses indicate that the 1996 enactment of SORA (and thus the beginning of the registry) had no significant impact on rates of total sexual offending, rape, or child molestation, whether viewed as a whole or in terms of offenses committed by first-time sex offenders or those committed by previously convicted sex offenders (i.e., repeat offenders). The only type of offending that was found to have significantly changed following the enactment of SORA was robbery within the present sample of New York State sex offenders (i.e., this finding did not hold for all offenders in New York State), with the number of robberies per month having significantly declined. Given the number of separate analyses conducted for this study (and, therefore, the increased chance of one reaching significance purely by chance), this finding should be interpreted with caution.

The current study also found that 95.9% of all arrests for any RSO, 95.9% of all arrests for rape, and 94.1% of all arrests for child molestation were of first-time sex offenders. Thus, as none of these offenders had any prior convictions for sexual offenses, none of them were on the sex offender registry (or would have been on the registry had it existed) at the time of their offenses. This finding casts doubts on the ability of sex offender registration and notification laws, as well as residency and occupational restriction laws, to actually reduce sexual offending. That is, these laws were specifically designed to limit the ability of convicted sex offenders to re-offend by limiting their opportunities to do so, and it appears that only a small portion of sexual offending (i.e., 4-5%) might be influenced by these legislative measures.

Thus, the results of the present study support those of prior research (e.g., Schram & Milloy, 1995; Walker et al., 2005) and cast serious doubts on the effectiveness of sex offender registries to significantly reduce rates of sexual offending. The limited effectiveness of registration and community notification laws may be due to the fact that these laws were largely based on commonly held myths and misconceptions regarding sexual offenses and sex offenders. First, community members commonly believe that most, if not all, sex offenders will inevitably re-offend (Levenson, Brannon, Fortney, & Baker, 2007; Levenson & Cotter, 2005a). However, as stated earlier, research has found relatively low recidivism rates for sex offenders (ranging from 5% to 19%; Hanson & Bussière, 1998; Langan et al., 2003). Furthermore, offenders without prior sexual offense convictions commit the majority of sexual offenses. In the current study, only about 4% of those arrested for a sexual offense had a prior sexual offense conviction. This finding is significant because it illustrates the limited reach of the sex offender registry. That is, approximately 96% of offenders arrested for sexual offenses have no prior sexual offense convictions and, thus, would not have been on a sex offender registry at the time of the offense.

Second, registration and community notification laws are based on the false assumption that strangers commit most sexual offenses. However, the research

unequivocally finds that sex offenders are more likely to victimize family members, intimate partners, or acquaintances. In fact, according to a Bureau of Justice study (Snyder, 2000), 93% of child sexual abuse victims knew their abuser (34.3% were family members and 58.7% were acquaintances). In addition, approximately 9 out of 10 adult rape or sexual assault victims had a prior relationship with the offender either as a family member, intimate, or acquaintance (Greenfeld, 1997). With most sexual crimes being committed by family members or someone known to the victim, registration laws may be ineffective because they focus, almost exclusively, on sexual offenses committed by strangers. Despite the public perception that sex offenders are strangers stalking playgrounds and other areas where children congregate, the majority of offenses occur in the victims' home or the home of a friend, neighbor, or relative (Greenfeld, 1997).

In addition, some researchers have argued that registration and community notification may, in fact, discourage victims of sexual abuse from reporting the incidents to authorities (Edwards & Hensley, 2001). As previously stated, the vast majority of sexual offense victims know their perpetrator. Although unintentional, community notification can often lead to identification of the victim, especially when the victim is an offender's child. As such, incest victims may not report the offense to avoid dealing with the impact that public notification would have on their family (Freeman-Longo, 1996).

Because registration and community notification laws were based on false assumptions regarding sex offenders and sexual offenses, attention and resources are diverted from the most common types of sexual offenses (those committed by first-time sex offenders and those who have a pre-established relationship with the victim) to ones perpetrated by the stereotypical sex offender. In order to increase the effectiveness of these laws to protect public safety, reactionary policies (regardless of how well intended) should be replaced with policies based on empirical findings. Public education should also play a key role in enhancing the ability of registration and community notification laws to increase public safety. Community members should be taught accurate, scientifically validated information about sex offenders and the true risk they pose to society. Dispelling the myths currently held by the public could have a meaningful impact on effective sex offender management by influencing community leaders and policymakers.

Limitations

The major limitations of this study, as with most studies that use official data sources, are those of data availability. The most notable of these is that the outcome measure, arrest for a registerable sex offense, was only an approximation of the true behavior of interest: sexual offending. As stated earlier, sexual arrest was chosen as the proxy to sexual offending as, of the variables available, it was most likely to show the impact of registration and notification. However, it would be very useful for a study to replicate the analyses presented here with a true measure of offending.

Other limitations are that the analyses do not check for differential impacts of registration and notification laws by geographic area, offender risk level, or victim-offender relationship (e.g., offender and victim knew each other before). It

may be, therefore, that registration and notification laws have had a very strong impact in suburban and rural areas, but not urban areas. (This question is especially interesting as, although analyses in the present study investigate different types of sexual offending in greater detail than previous studies, they do so in only one state.) Likewise, it may be that registration and notification has impacted the offending of less serious (lower risk) offenders, but not more serious (higher risk) offenders. Future studies should investigate such possibilities by including measures of these variables in their analyses.

Conclusion

Sex offenders evoke little sympathy from the public and, as such, the popularity of registration and community notification laws is understandable. However, it is becoming increasingly clear from the growing body of research that registration and community notification laws are not an effective strategy for reducing sexual offenses. In fact, focusing attention and resources on the small number of known, registered sex offenders detracts attention from the more common types of sexual offenses that occur, leaving people vulnerable to sexual abuse and creating a false sense of security.

Furthermore, the results of this and previous studies indicate that sex offender legislation created without empirical research to support its ability (or possible ability) to reduce sexual offending can not only be ineffective and wasteful, but can also have unintended and often negative consequences. For example, community notification and residency restriction laws have been found to make it more difficult for released sex offenders to successfully integrate back into society (Levenson & Cotter, 2005b; Levenson, D'Amora, & Hern, 2007), thereby increasing their risk to re-offend (especially those subject to community notification; Freeman, 2008). Such findings are especially important in light of continuing legislative efforts directed at controlling convicted sex offenders such as the Adam Walsh Child Protection and Safety Act (2006), which lacks empirical research to support its effectiveness for increasing public safety.

Given the limited resources available for sex offender management, perhaps communities would be better served if their scarce resources were used for sexual abuse prevention initiatives designed to educate the public on the realities of sexual offenses and sex offenders. As Berliner (1996) noted, registration and community notification laws should not replace sexual abuse prevention efforts. Moreover, resources would be better spent on evidence-based sex offender management strategies that have been shown to reduce sexual offending, such as cognitive-behavioral treatment programs for offenders (Lösel & Schmucker, 2005; McGrath et al., 2003).

The overarching goal of sex offender legislation is to make communities safer and reduce the number of people who are sexually victimized. As Prentky (1996) clearly argues, "the singular consideration should be whether community notification will in fact reduce victimization rates or whether it will merely provide a dangerous false sense of security" (p. 297). Given the serious nature of sexual victimizations, policymakers should not be complacent with the current registration and community notification system. Registration and community notification should only be one element of the public response to sexual offending (Berliner,

1996). The question of how society can best be protected from sexual victimization remains, but empirical research, in both previous studies and the current one, indicates that existing registration and community notification laws are largely ineffective.

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