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Time-Series Analyses of the Impact of Sex Offender Registration and Notification Law Implementation and Subsequent Modifications on Rates of Sexual Offenses

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Abstract

Sex offender registration and notification (SORN) laws were implemented to protect communities by increasing public awareness, and these laws have expanded over time to include registration by more types of offenders. Despite widespread implementation, research provides only inconsistent support for the impact of SORN laws on incidence of sexual offending. Using data from a large metropolitan area in Texas over the time period 1977 to 2012, and employing a number of time-series analyses, we examine the impact of the initial SORN implementation and two enhancements to the law. Results reveal no effect of SORN, or its subsequent modifications, on all sexual offenses or any of several specific offenses measures (e.g., crimes by repeat offenders). Implications for effective policy and future research are presented.

Keywords

sexual offending, criminal justice policy, registration and notification

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Introduction

Beginning in the 1990s, increased attention to sexual offenders led to the implementation of sex offender registration and notification (SORN) laws throughout the United States. These laws attempt to more effectively monitor and supervise convicted sexual offenders upon release, as well as to increase community awareness of these offenders (Terry, 2015). Despite widespread adoption and considerable public attention to their implementation, the existing research on the effectiveness of these laws in reducing rates of sexual offending in general, and recidivism by sexual offenders in particular, had produced at best, inconsistent evidence of their impact (Drake & Aos, 2009; Terry, 2015).

In evaluating the impact of SORN laws, a number of previous studies have utilized an interrupted time-series approach to examine trends in the number of sexual offenses before and after SORN implementation, typically using data from a particular state or across a number of states (see Sandler, Freeman, & Socia, 2008; Vasquez, Maddan, & Walker, 2008). As has been the case in research using other methodologies, interrupted time-series studies have generally produced results that fail to support the effectiveness of SORN laws. At the same time, these studies also have suffered from several methodological limitations, including examining SORN impacts across large geographical areas (i.e., within an entire state, which could obscure intrajurisdictional implementation differences), incorrect identification of the correct start dates for SORN laws, and failure to examine impacts of these laws on different types of sexual offenders (e.g., first time vs. repeat offenders). Our study addresses a number of these methodological limitations, by examining the impact of SORN law implementation (as well as two subsequent amendments to the law), on several different measures of sexual offending derived from monthly counts of initial charges of a sexual offense, filed in adult criminal court in Harris County, Texas (Houston), from 1977 through 2012.

Development of SORN Laws

A number of states implemented sex offender registration requirements as early as 1947; however, the first comprehensive, state-level legislation on this issue was enacted in Washington state in 1990 (Terry, 2015). Later, at the federal level, the Jacob Wetterling Act, implemented in 1994, was the first national legislation to establish guidelines mandating that states establish requirements to register convicted sexual offenders upon release from custody (Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act, 1994). Concerns regarding the maintenance of

public safety prompted the implementation of these sex offender registration laws, along with a number of subsequent amendments, including expanding the types of offenses requiring registration and applying the registration requirement to "registerable" crimes committed prior to the implementation of the initial legislation (Fabelo, 1998; Sandler et al., 2008; Terry, 2015).

Prior to federal efforts in this area, some states (e.g., California, Washington) maintained their own policies on sex offender registration. For instance, the 1990 Washington State Community Protection Act required registration for sexual offenders within 30 days of their release from incarceration (Felver & Lieb, 1991). Federal legislation to create SORN laws began in the early 1990s, primarily in response to a number of high-profile sexual crimes (i.e., Megan's Law, 1996; Adam Walsh Child Protection Act, 2006; Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act, 1994; see Sandler et al., 2008). Individual states then typically followed this federal guidance, developing largely similar policies to create registration systems intended to increase immediate reporting of suspicious behavior in communities and deter first-time sexual offenders while reducing recidivism in repeat sexual offenders (Sandler et al., 2008; Socia, 2012).

Although there are some variations across states, the sex offender registry process typically involves (a) notifying the offenders of their responsibility to register and remain forthcoming with any changes in residence, (b) offender registration itself, (c) notifying the offenders' community of release from custody, and (d) some form of public notification (Fabelo, 1998). This process usually requires jail, prison, and/or court officials to notify offenders of their obligation to register, and to notify local law enforcement agencies of their release and status as a convicted sexual offender. After an offender is notified of this responsibility, he or she is generally required to complete a sex offender registration form, and be photographed and fingerprinted by his or her local law enforcement agency. The agency then notifies the community of the offender's release and an offender's release and residential location is made public once the aforementioned steps are completed.

Federal interventions and amendments to SORN. In 1994, the Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act established federal guidelines for states to track sexual offenders by requiring offenders to confirm their place of residence each year for 10 years after their release, and quarterly if the nature of the offense was violent (Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act, 1994). Since their initial implementation, state- and federal-level laws regarding SORN have periodically been modified in attempts to improve

their intended deterrent effects. For instance, procedures for released sexual offenders working, attending school, or relocating to a different state were amended within the Wetterling Act in 1997 (The Wetterling Improvements Act, 1997). In particular, any relocation for the previously mentioned reasons required the offender to register in compliance with the requirements in the new state of residence. SORN amendments such as The Jacob Wetterling Improvements Act of 1997, provided states with the discretion to require offenders whose offense(s) were not previously outlined as "registerable" in the provisions of the initial Wetterling Act to register.These kinds of modifications would conceivably add to the number of sexual offenders who are required to register and are thus subject to the anticipated deterrent effects of SORN laws.

A second important modification to SORN laws came in 1996, with the passage of Megan's Law (1996), which required public dissemination of information from states' sex offender registries, required information collected under state registration programs to be disclosed for any purpose permitted under a state law, and required state and local law enforcement agencies to release any relevant information needed to protect the public from those registered under the Jacob Wetterling Act of 1994. While the passage of Megan's Law did not significantly affect the numbers of offenders required to register, it did attempt to increase public awareness of convicted sexual offenders who were reentering local communities.

In addition, the Adam Walsh Child Protection and Safety Act, enacted in 2006, is the latest modification to federal SORN legislation, requiring new standards for jurisdictions implementing SORN programs (Adam Walsh Child Protection Act, 2006). Specifically, the Walsh Act expanded the definition of jurisdiction to include Indian Tribes, and also expanded the number of sexual offenses classified as "registerable" within local, state, federal, and foreign jurisdictions.

Theoretical rationale for SORN laws. Vasquez et al. (2008) discussed some of the assumptions about sexual offenders that underscore how SORN laws might help to reduce sexual offending, in particular, that sexual offenders are highly likely to recidivate, that SORN laws provide the community with increased information about sexual offenders, and that sexual offenders are thus deterred by the registration requirement. In essence, these assumptions suggest that sex offender registration laws are intended to reduce sexual offending through a deterrence process. Requiring sexual offenders to register is theorized to increase the punitive value of a conviction for sexual offenses because the individual is both formally punished by the legal system and is publicly labeled as a "sexual offender" (i.e., public humiliation; Schultz, 2014).

In fact, proponents of SORN laws suggest that both general and specific deterrence processes may arise from the implementation of these laws (Drake & Aos, 2009). For instance, would-be first-time sexual offenders should be subject to "general" deterrent effects, in terms of committing an initial sexual offense, because they know they would face not only legal, but also social repercussions (i.e., public stigmatization). As such, requiring sexual offenders to register, and then notifying the public of their crimes, anticipates a reduction in the number of sex crimes in a given community, through a general deterrence process, in light of the fear of the stigma associated with the "sexual offender" label (Schultz, 2014).

At the same time, convicted sexual offenders who must register upon return to the community should theoretically be subject to "specific" deterrence effects because they are more effectively monitored by informed community members, who can then report on their suspicious behaviors before sexual reoffending occurs. In essence then, increasing public awareness of offenders returning back to the community should also reduce the offender's opportunity to commit subsequent crimes (i.e., provide some amount of "incapacitation," as well as deterrence) in that the public should conceivably be in a better position to engage in avoidance and protective measures against victimization by known offenders in the community. In light of these two possible mechanisms of action, it is important to examine the impact of implementation on specific types of offenders (e.g., first-time and repeat offenders).

Research on the Effectiveness of SORN Laws

Although SORN laws have been in place at the federal and state level for over two decades, research on the effectiveness of these policies is somewhat limited. For instance, in a review of evidence, Drake and Aos (2009) found only nine existing credible studies, while more recently Terry (2015) identified 12 credible outcome evaluations. Despite some important methodological flaws in several of these existing studies, both reviews (Drake & Aos, 2009; Terry, 2015) generally concluded that this body of research has suggested that SORN laws have no effect on rates of sexual offending or on rates of other types of crimes.

Much of the early research in this area utilized either descriptive methods, or in some cases quasi-experimental designs to compare (typically) small groups of registered sexual offenders with similar offenders not subject to registration requirements. The comparison groups utilized in some of these quasi-experimental studies were, in some cases, comprised of sex offenders from years prior to SORN implementation, or of less serious sexual offenders

(i.e., those with lower risk levels), potentially introducing selection biases into these designs (Maddan, Miller, Walker, & Marshall, 2011; Schram & Milloy, 1995).

In an attempt to examine the potential impact of SORN requirements on sexual offenders in Massachusetts, Petrosino and Petrosino (1999) collected data on a small sample of 136 sexual offenders, but there was no comparison group. Among their sample of sexual offenders, the authors found that only about 27% would have been required to register under the new law, and very few of the victims would have been made aware of the offender prior to the offense occurring. In one of the early quasi-experimental studies of SORN effectiveness, Schram and Milloy (1995) compared 139 adult sexual offenders who were required to register with 90 similar offenders who were not, and found that the two groups' sexual reoffense rates during a 54-month follow-up were not significantly different (57% vs. 47%, respectively). In a similar study, Maddan et al. (2011) compared three groups of sexual offenders who were subject to registration requirements (after Arkansas's SORN implementation in 1997, $n = 2{,}165$) with another three groups of sexual offenders from earlier, preregistration eras (pre-1997, $n = 755$) and found that their sexual offense recidivism rates were not significantly different (9.5% vs. 10.8%, respectively). However, sexual offenders required to register did have significantly lower rates of nonsexual reoffending (27.5%) than sexual offenders who had not been required to register in earlier years (41.1%).

Several more recent research efforts (Prescott & Rockoff, 2011; Shao & Li, 2006) have attempted to utilize larger samples derived from official crime data (e.g., National Incident Based Reporting System [NIBRS] data from the FBI) to more accurately estimate the effect of SORN requirements within a jurisdiction, or across a number of different states. For instance, Prescott and Rockoff (2011) used NIBRS data (over 328,000 sexual offenses) from 15 different states to estimate the effect of both registration and notification requirements, separately on sexual offending frequencies. The results from their multivariate regression models suggest that, whereas community notification policies may have a deterrent effect on first-time offenders (i.e., general deterrence), there was no effect on convicted sexual offenders (i.e., no specific deterrence). Interestingly, these authors also found that there may be an increased risk of recidivism for those who have their information released to the public.

Shao and Li (2006) also examined national data (from the FBI's Uniform Crime Reports [UCRs]) from all 50 states to estimate the effect of registration laws on the number of rapes reported to the police from 1970 to 2002 using a multivariate model that controls for differences between states. They found that the implementation of registration laws was associated with a significant,

but small (2%) reduction in the number of rapes reported to police. Although some evidence points to SORN having a small impact in reducing the prevalence of sexual offending, some authors have argued that the irreparable harm of social stigma for the registered offender, postrelease and during reintegration, and the "false sense of security" SORN may create for communities, limit the impact of these laws (Bierie, 2015, p. 6; Craun & Bierie, 2014).

Whereas these large-scale studies provide some evidence for small deterrent effects of SORN policies, some equally rigorous studies using interrupted time-series methods suggest little effect of SORN laws (Sandler et al., 2008; Vasquez et al., 2008). Interrupted time-series analyses are particularly useful for examining the implementation of a particular policy change because they can be used to examine the trend in a given crime after a specific intervention (in this case SORN implementation) controlling for the existing trend in that same crime rate prior to the legal change. For instance, Vasquez et al. (2008) used time-series analyses to examine the impact of SORN implementation on the number of forcible rapes in 10 states, using monthly data from the UCR, following the enactment of Megan's Law in 1996. Results from data collected during the 10 years from 1990 through 2000, reveal that six of the 10 states they examined experienced no change in the monthly count of rapes reported to police, and one had a significant increase in reported rapes, while three states exhibited significant reductions in the number of forcible rapes reported to police.

Overall, Vasquez and colleagues (2008) concluded that SORN laws had little if any deterrent effect on the monthly number of rapes as reported to the UCR. Although their study did not provide evidence to support SORN policies, these authors did suggest that it might be useful for future research to examine smaller jurisdictions (e.g., city/county, as opposed to entire states), because time-series designs in particular are subject to a methodological problem called "binning." Specifically, there may be differences in the registration and notification process (or other aspects of sexual offender supervision) within a large jurisdictional unit (e.g., a state) that lead to differences in the impact of SORN policies across different areas within that unit. These within-jurisdiction differences are thus less likely to influence the impact of the law if smaller jurisdictional samples are examined, where there is an assumption of more similar, within-jurisdiction procedures.

Vasquez and colleagues (2008) also noted that while using UCR data, they were unable to determine whether SORN policies may have been more effective for certain types of offenders (e.g., first-time vs. repeat offenders, those offending against specific types of victims). Finally, Vasquez and colleagues (2008) also suggested that future research using time-series methods should examine the relationship between SORN implementation and the number of

other (nonsexual) crimes (known as control series) to help rule out the alternative explanation that any reductions in crime associated with SORN implementation are simply coincidental, reflecting underlying downward trends in crime more generally or other changes in criminal justice policy more broadly that could have occurred at the same time.

Sandler and colleagues (2008) used an interrupted time-series analysis to examine the trends in the number of arrests for sexual offenses in New York State, before and after the enactment SORN laws there. These authors incorporated a number of refinements as suggested by Vasquez and colleagues (2008). For instance, they examined the impact of New York's SORN requirement on a number of other, nonsexual crime types, to rule out other explanations for any reductions in sexual offenses. They also examined the impact of SORN implementation on not only a global measure of all sexual offenses but also on a number of specific types of sexual offending, including rapes and offenses of child molestation, as well as sexual offenses committed by first-time offenders and those committed by repeat offenders, separately.

Using data from the New York State criminal history system, the authors collected data on monthly numbers of arrests, including arrests for registrable sexual offenses (RSOs) from 1986 to 2006 (252 months of data). Using a number of interrupted time-series analyses, these authors found that SORN implementation in 1996 was not associated with significant reductions in any of their measures of sexual offending (e.g., all sexual offenses, and specifically for rapes, cases of child molestation, sexual offenses by first-time offenders or by repeat offenders). Likewise, the authors concluded that none of the control series, which examined whether SORN implementation may have coincided with changes in the monthly number of arrests for other kinds of nonsexual crimes (e.g., assaults, robberies, burglaries, larcenies) showed any significant changes in these crime types after SORN implementation.

Sandler and colleagues (2008) also found that the overwhelming majority of RSOs in their data were committed by individuals who were first-time sexual offenders (e.g., 95.5% of all RSO arrests). As such, none of these first-time offenders would have appeared on the sex offender registry prior to their sexual offense, which these authors suggest calls into question the ability of sex offender registries to affect incidence of sexual offending (i.e., general deterrence). While Sandler and colleagues (2008) noted that their study included a number of methodological improvements (i.e., examination of specific sexual offense variables, use of several control series), they also noted that their results represent only one state, and thus they suggested replication of their efforts in other locations.

Texas SORN Laws

As was outlined previously, since the initial implementation of SORN laws in the United States, there has been an ongoing attempt to revise and expand them to improve their effectiveness. The same evolution has occurred in the State of Texas over the years since it first implemented its sex offender registry requirements in 1991 (prior to the development of federal laws in 1994). Given that the current study will examine data from a large urban jurisdiction in Texas to address the issue of "binning" outlined by Vasquez and colleagues (2008), a brief review of the implementation and some of the major revisions to Texas's SORN laws is presented to provide context for the analyses that follow. Specifically, according to the Texas legislature, the state's statute requiring the registration of sexual offenders became effective on September 1, 1991, and this law applied to those in custody for a sexual offense on that date. Changes to individual state SORN laws have often paralleled the changes that occurred first at the federal level. For instance, in the years since 1991, the Texas legislature has amended its SORN requirements a number of times. Many of these modifications followed from changes in federal legislation and effected the offenses that would require offender registration, requirements about who would receive notification of an offenders' release (e.g., victims), the offender's disclosure requirements and disclosure of the details for the offense, as well as modifying protocols for public notification (Code of Criminal Procedure, 2005; Fabelo, 1998).

Of these multiple modifications to Texas's SORN laws, the Texas Department of Corrections identifies three dates that represent legislative interventions that substantially affected SORN laws in the state (Texas Department of Public Safety, 2015). The first of these is the initial establishment of the SORN requirements, which became effective on September 1, 1991. The second important date represents a substantial modification to the initial law that became effective on September 1, 1997, when the law was revised to require registration as a sexual offender retroactively, that is, for any person whose sexual offense conviction happened on or after September 1, 1970, if they were in the Texas criminal justice system, rather than only for those convicted after 1991. This SORN modification in particular might be expected to substantially increase the number of sexual offenders registered, which might then conceivably have some additional deterrent effect on the rate of sexual offenses in the state (Fabelo, 1998). Finally, the third important date in the history of Texas SORN laws is September 1, 2005. On this date, the law was changed to require those convicted of a sexual offense to register as a sexual offender in Texas, even if their sexual offense conviction occurred in another state (Code of Criminal Procedure, 2005; Fabelo, 1998). Again,

this legislative modification could be expected to increase the number of registered sexual offenders and thus reduce the rate of sexual offending in the state by placing more individuals under the potential deterrent effect of being on the registry.

It should be noted that offender registration and offender notification (an additional requirement often applied to only a subset of "high-risk" offenders) are two distinct components of the Texas SORN law, and like the federal SORN laws, the Texas sex offender registry maintains a tiered system, based on the offender's assessed level of risk to the community (Texas Department of Public Safety, 2015). The current study examines changes to the laws related to both these specific activities in that we model the impact of the implementation (and modification) of Texas's sexual offender registration and notification law; however, the data used in this study do not include information on the numbers of offenders in this jurisdiction who were required to register only, relative to those who were also subject to community notification requirements. While the two modifications to the state's SORN law (in 1997 and 2005) increase the numbers of individuals who are subject to registration requirements (thus potentially expanding the deterrent effect of such requirements) we have no reason to believe that the offenders who were subject to registration after each modification were any more or less likely to be subject to the additional notification requirements. In other words, we do not expect that these modifications altered the proportion of offenders who are required to register and to have community notification, after either the 1997 or 2005 modifications. Thus, if the SORN law affects the rate of sexual offending in this jurisdiction partly because of registration requirements and partly because some of these offenders are also required to have community notification, the proportion of sexual offenders subjected to these two different requirements should be similar over time. As such, the data available for the current study do not allow us to dissect the particular impact of registering as compared with registering and being subject to community notification.

Current Study

While the existing body of research suggest little impact of SORN laws on incidence of sexual offending (Drake & Aos, 2009; Terry, 2015), a number of authors have also suggested that additional research is needed to address a number of common shortcomings in these studies. For instance, research should examine variations in the incidence of different types of sexual offenses (e.g., sexual crimes against children vs. sexual assaults against adult victims) that occur before and after SORN implementation. Research is also

needed that examines possible differences in SORN impact on sexual offending by first-time offenders and repeat offenders, to separate potential general and specific deterrent effects. Likewise, researchers have suggested that analyses of single, smaller jurisdictions could help control for possible "binning" effects that can occur within larger state- or nationwide samples (Lytle, 2015; Sandler et al., 2008; Socia, 2012; Terry, 2015; Vasquez et al., 2008). Finally, some authors have suggested that prior research has utilized incorrect start dates for SORN implementation in their analyses (Prescott & Rockoff, 2011).

The current study uses SORN implementation and amendments dates, taken directly from the state legislation, to accurately reflect the potential impact of SORN implementation and subsequent refinements. The current study examines the initial implementation of the Texas SORN law requiring the registration of sexual offenders starting in September 1991, as well as the two modifications, in 1997 and 2005, that each expanded the number of offenses for which an individual would have to register as a sexual offender. Each of these intervention points are also derived directly from the state legislation in terms of their respective effective dates. Next, our study adds to the existing literature in that we examine trends in sexual offenses before and after each date, using a number of interrupted time-series models to determine whether the implementation of these policy changes were associated with any change in the number of initial charges for sexual offenses filed in this large county's adult criminal court system from January 1977 through April 2012.

In light of recent research calling for more detailed analysis of the effects of SORN laws (i.e., on repeat vs. first-time offenders), we also examine the impact of these policy changes on several different dependent variables, including on the number of charges filed monthly for all sexual offenses, and then on specific counts of the monthly number of charges filed for sexual assaults, specifically, and for sexual offenses against children, and also for the monthly number of charges filed for sexual offenses committed by first-time offenders and repeat offenders. Following, in particular, from the work of Vasquez et al. (2008) and Sandler et al. (2008), the current study adds to the existing literature on SORN laws' effectiveness by utilizing data gathered from the court system (monthly counts of charges initially filed by police at the time of booking) in a single, large metropolitan county in Texas. As Sandler and colleagues (2008) suggested, we examine the impact of SORN implementation in a single jurisdiction to minimize within-jurisdiction differences in implementation that may occur in larger units of analysis (i.e., states) and mask the impact of SORN over different jurisdictions. Finally, we also model the impact of SORN implementation/modification on a comparison series comprised of the monthly number of nonsexual assaults to rule out

other, more general changes to criminal justice system or community-level processes that might work to reduce levels of violent crime more generally (including sexual offenses).

Based on prior research, the specific hypotheses advanced in this study are as follows. First, implementation of Texas's SORN law in 1991 will be associated with a reduction in the total number of all sexual offenses filed in Harris County's court system per month. To determine whether the SORN law has a differential impact across types of sexual offenses, we also examine this implementation effect on sexual offenses against children, and sexual assaults, specifically; and, to examine whether SORN laws produced general and/or specific deterrent effects, we also examine the impact of SORN implementation on first-time and repeat sexual offenders, separately. Our second and third hypotheses mirror these predictions (including the focus on different types of offenders) related to the two revisions to Texas's SORN laws, first in 1997 and again in 2005, to determine whether expanding the number of sexual offenders who were required to register improved SORN effectiveness. Finally, given that SORN laws target sexual offenders specifically, we propose that SORN implementation (and subsequent modifications) will have no impact on the monthly number of cases filed for nonsexual assaults (our set of control series).

Hypothesis 1: SORN implementation in 1991 will reduce monthly counts of cases filed for all sexual offenses and, specifically, for the number of cases of offenses against children, sexual assaults, and crimes by first-time and repeat offenders.

Hypothesis 2: SORN modification in 1997 will reduce monthly counts of cases filed for all sexual offenses and, specifically, for the number of cases of offenses against children, sexual assaults, and crimes by first-time and repeat offenders.

Hypothesis 3: SORN modification in 2005 will reduce monthly counts of cases filed for all sexual offenses and, specifically, for the number of cases of offenses against children, sexual assaults, and crimes by first-time and repeat offenders.

Hypothesis 4: SORN implementation in 1991 and modifications in 1997 and 2005 will have no significant effect on the number of nonsexual assault cases filed per month.

Method

Following the suggestion of Vasquez and colleagues (2008) to examine SORN effectiveness within a particular, smaller jurisdiction (i.e., city rather

than state), we examine data from Harris County (Houston) Texas. While these data were admittedly appealing because they were readily available from the NeuLaw program (Ormachea, Haarsma, Davenport, & Eagleman, 2015), they were also well suited to the current research questions. Harris County is clearly not necessarily representative of other large urban jurisdiction, any more than Texas is representative of other states. These data, however, are particularly useful for several reasons. First, the data allow us to assess a single smaller jurisdiction to avoid issues with "binning" that can occur in analyses of state-level data. At the same time, use of this single large urban area still provides a data set containing more than 69,000 sexual offense cases (from more than a 34-year period) that allows for the estimation of an appropriate interrupted time-series model. In fact, Houston is the fourth largest city in the United States (U.S. Census Bureau, 1980-2015), and although these results may not generalize to other large cities (e.g., New York, Chicago, or Los Angeles) or to other medium or small-sized cities, they nonetheless can be used to assess the effect of SORN laws on a sizable population and over a considerable time period.

In Texas, the SORN law was implemented on September 1, 1991, and established registry requirements for those convicted of sexual offenses (Code of Criminal Procedure, 2005; Fabelo, 1998). The initial law defined RSOs and also established offender disclosure and public notification requirements for certain types of offenders. The Texas legislature has modified these laws during every subsequent legislative session, in attempts to increase the deterrent effects of SORN (Texas Department of Public Safety, 2015). We conducted several time-series analyses to determine whether SORN implementation (in 1991) and later modifications (in 1997 and 2005) were related to changes in the number of charges filed for sexual offenses in a large urban area of the state.

Data Source

The data for this study were retrieved from the NeuLaw Criminal Records Database (NCRD; Ormachea et al., 2015). This database included all criminal charges filed in adult criminal courts in Harris County (Houston area) Texas, during the period from January 1, 1977, through April 30, 2012 (424 total months). These data represent charges filed by police, at the time the individual was booked into the county jail.[1] When looking at the time period that corresponds to the years of crime data used in this study, the U.S. Census Bureau reports an estimated county population of 2,409,547 in 1980, which had risen to an estimated 4,538,028 by 2015 (U.S. Census Bureau, 1980-2015). Based on census estimates, the demographic characteristics of the

county were as follows: average age of 33 years, median income of US$54,230 annually, 55.7% of homeownership within the county, and 17.3% of citizens living in poverty. Racial composition of the county in 2015 is estimated at 70.2% White residents and 19.6% African American residents, with 42% of the population identified as Hispanic (U.S. Census Bureau, 1980-2015).

During the period in question, a total of 69,510 sexual assault cases were filed. In cases where more than one charge was filed for a given arrest, we considered the case a "sexual offense" case if any of the charges filed were for a sexual offense (e.g., aggravated sexual assault, sexual abuse of a child) regardless of what other kinds of charges might have also been filed. From these data, we created five different measures of sexual offenses; first, a count of all sexual offense cases filed per month (using the date of case filing to assign cases to a particular month). Next, we also computed two specific monthly counts, one for the number of cases of sexual assaults against adults filed per month, and another for the monthly number of cases involving sexual offenses against children. Finally, the crime records data used for the study include a randomly generated unique individual identifier, which allows researchers to determine whether a person has been charged in this jurisdiction previously (although it cannot be used to determine the person's actual identity). We use this unique identifier to determine whether an individual is a "first-time" versus "repeat" sexual offender, at least within this specific jurisdiction. As such, we also examined the monthly number of all sexual offense cases filed against individuals who appear to be "repeat" offenders (i.e., those whose identifier appeared in this data file previously) and also the total number of cases filed against individuals who appear to be "first-time" offenders (i.e., those whose identifier did not appear in this data before), again for all types of sexual offenses.

All sexual offenses. Among the 69,510 sexual offense cases included in these data, the monthly average number of cases filed over this 35-year period was 143.9 ($SD = 40.5$; range = 52-308). Felony sexual offenses made up 68.1% ($n = 47,317$) of the cases filed (e.g., aggravated sexual assault, sexual abuse of a child). Among the cases filed, 47.3% ($n = 32,888$) were sexual offenses against children, 14.6% ($n = 10,118$) were sexual assaults against adults, while the remaining 38.1% ($n = 26,504$) were other sexual offenses not involving physical contact (e.g., possession of obscene material, public lewdness). In addition, 66.9% ($n = 46,499$) of the sexual offense cases filed involved those who appear to be first-time sexual offenders (i.e., they did not appear in these court records prior to the current offense), while 33.1% ($n = 23,011$) of the sexual offense cases filed involved repeat sexual offenders (i.e., those who had a prior sex offense charge in this jurisdiction).

Sexual assault cases. In terms of the sexual assault cases (n = 10,118), the average number of cases filed per month was 22.2 (SD = 10.7). Of the sexual assault cases filed, 42.0% (n = 4,252) were cases of sexual assault and 41.2% (n = 4,171) were for charges of aggravated sexual assault, while 16.7% (n = 1,695) were for other sexual offenses categorized as sexual assaults (e.g., 9.4%, n = 948 attempted sexual assaults; 7.4%, n = 747 burglaries with sexual intent).

Sexual offenses against children. Among the cases of sexual offenses against children (n = 32,888), the mean number of cases filed per month was 62.2 (SD = 22.0) per month. Specific charges are as follows: 56.5% (n = 18,580) involved a charge of sexual assault against a child and 37.1% (n = 12,189) were cases of child indecency. The remaining cases of sexual offenses involving children included very small percentages of offenses such as possession of child pornography (3.4%, n = 1,132), soliciting a child (0.8%, n = 279), attempted indecency with a child (0.7%, n = 226), and attempted sexual assault against a child (0.6%, n = 184).

First-time and repeat offenders. Among the set of all sexual offense cases filed against "first-time" sexual offenders (n = 46,499), the average monthly number of cases filed was 109.7 (SD = 30.9). These cases included all sexual offense cases filed against individuals who did not have a previous sexual offense case in this jurisdiction. These first-time sexual offender cases represented 66.9% of the total number of sexual offense cases filed. The remaining 33.1% of all sexual offense cases (n = 23,011) were filed against individuals who were found to have a prior sexual offense charge in this jurisdiction, with a monthly average of 54.3 (SD = 23.7) cases filed.

Nonsexual assault cases. In addition to modeling the relationship between SORN implementation and modification dates on monthly numbers of sexual offense cases filed, we also estimated a series of comparison time-series models, using data on the monthly number of (nonsexual) assault cases filed over the same 35-year period (e.g., assault, aggravated assault, deadly conduct charges; see Sandler et al. (2008) for an example of the use of similar control series when investigating the effect of SORN laws). This control series is important because it can help explore whether there were any changes in the monthly rate of interpersonal offenses generally, that may have coincided with SORN implementation (or revision). If the dates of SORN implementation and/or modification were found to also correspond to changes in the rate of nonsexual crimes, this might suggest some other alternative explanation (e.g., increased police efforts against violence

in general) rather than the particular effect of SORN requirements. As such, our comparison series then help rule out potential confounding explanations for any noted relationship between SORN implementation/modification and incidence of sexual offending. While we would expect SORN requirements to potentially be related to lower levels of sexual offending through the various deterrence effects outlined previously, SORN laws are unlikely to affect the rates of other kinds of violent crimes, in this case monthly numbers of assault cases filed. As these kinds of offenses are not subject to registration requirements if they are committed, there is little reason to expect any potential deterrent effect of SORN laws to emerge in relation to nonsexual assaults. From 1977 through April 2012, these data contain 266,370 nonsexual assault cases, with an average of 628.2 (SD = 352.5) cases filed per month. Of the 266,370 nonsexual assault cases filed, 72.0% (n = 191,883) were cases of simple assault or attempted simple assault, 17.9% (n = 47,669) were cases of aggravated or attempted aggravated assault, and 10.0% (n = 26,818) were for other types of assault (e.g., threats, deadly conduct).

SORN intervention variables. A set of 18 separate time-series models used these five different measures of monthly rates of sexual offense case filings, and one measure of nonsexual assault cases, to examine the underlying trends in the number of cases filed for each crime type and then to examine the impact of SORN implementation and two subsequent modifications on these monthly crime numbers. Specifically, SORN requirements were implemented in Texas effective on September 1, 1991. Cases filed before that date were coded "0" in our data, while those filed on or after this date were coded "1." A total of 176 months of data are available for the period prior to SORN implementation, and 248 months after the initial implementation. Likewise, September 1, 1997, represents the effective date for modification to Texas's SORN law that required convicted sexual offenders to register retroactively (i.e., for any sexual offense conviction on or after September 1, 1970). Again, for purposes of analyzing the impact of this modification, cases filed before September 1, 1997, were coded "0" (248 months) and those on or after this date were coded "1" (176 months). Finally, September 1, 2005, is the effective date for modification to Texas's SORN law that required sexual offenders convicted on or after September 1, 1997, to register, regardless of whether they were convicted of their RSO in Texas or some other state, and again cases are coded "0" if they were filed before this date (344 months of data) and "1" if they were filed on or after this date (80 months).

Analytic Plan

Interrupted time-series modeling approaches are a widely used method for assessing the impact of a "full coverage" program or policy intervention (i.e., where it is not feasible to develop a control group of cases not subject to the new policy for use in a traditional experimental or quasi-experimental design; Rossi, Lipsey, & Freeman, 2004). The use of interrupted time-series models to assess the impact of a given policy change is comprised of two main components. First, the preexisting, underlying pattern in the event of interest over time (i.e., sexual offense cases filed per month) is modeled. In particular, this modeling approach is dependent on having data available on the event of interest (e.g., monthly number of sexual offense cases) that predates the intervention, as well as a sufficient number of observations from the postintervention period so as to examine any changes in the preintervention trend. Specifically, a specific, discrete intervention (e.g., the introduction of Texas's SORN requirement on September 1, 1991) is used to separate the time period being examined (i.e., 1977 to April 2012) into those observations that occur prior to, and after the intervention date. The time-series model then compares the monthly number of cases filed in the pre- and postintervention periods.

We use monthly counts of sexual offense cases filed for several reasons. First, the existing research on SORN effectiveness using interrupted time-series models has consistently used monthly sexual offense numbers. Beyond simply using the same kind of data past research has used, however, there are several methodological benefits of using monthly data, including the ease of controlling for seasonality in the data when monthly counts are used, as well as the need to aggregate cases to a time frame (e.g., monthly) in order that sufficient variation exists from one month to the next so that changes in the overall trends can be ascertained by the modeling strategy itself. At the same time, this modeling approach is able to account for any preexisting trends in the monthly number of sexual offenses cases filed. In relation to our model of the impact of SORN implementation in 1991 on the number of all sexual offense cases filed, it is possible that the number of filed cases was changing systematically (either increasing or decreasing) prior to the implementation of SORN. Time-series analyses allow for separation of the preexisting pattern or trend in the number of such cases, from the potential impact of the policy change itself (see McCleary & Hay, 1980, for a thorough discussion of time-series modeling approaches).

Results

Pre–post Comparisons

An initial comparison of the number of sexual offense cases filed before and after the implementation of SORN requirements in 1991 indicates a significant

increase in the number of cases filed, from a monthly average of 123.1 (176 months, from 1977 through September 1991) to 158.7 per month (248 months, from October 1991 through April 2012; $t = -9.530$, $p < .01$). The same pattern was observed for the number of sexual offense cases filed before and after each of the other two intervention points, specifically the modifications to SORN requirements that occurred in September of 1997 (136.7 cases vs. 154.1 post-modification, $t = -4.783$, $p < .01$) and in September 2005 (138.4 cases per month before vs. 167.5 cases after, $t = -8.030$, $p < .01$).

At first glance, these simple comparisons might seem to suggest that SORN implementation and modifications were related to *increased* numbers of sexual offense case filings; however, these pre–post comparisons cannot statistically distinguish between an effect of SORN implementation (or modification) and some other underlying process occurring over the time period in question (e.g., overall population growth leading to higher numbers of crimes occurring, increased public awareness of sexual offending leading to higher rates of reporting, or more consistent police enforcement activities). In other words, it is possible that there was a general increasing trend in the monthly number of sexual offense case filings taking place throughout this time period that was not related to the implementation or modification of SORN requirements. For example, there are significantly higher numbers of nonsexual assault cases filed after each intervention point, as well (e.g., about 268 prior to SORN implementation, and 884 per month afterward, $t = -36.553$, $p < .01$), suggesting that during the period 1977 to 2012 there was a generally increasing trend in the number of monthly violent crime cases filed in this jurisdiction.

Interrupted Time-Series Results

Figure 1 provides a representation of the monthly number of all sexual offense cases filed during the time period from January 1977 to April 2012. Before assessing the impact of SORN implementation/modification, the dependent series (i.e., sexual offense case filings over time) must be reduced to a white noise process (see Cochran, Chamlin, & Seth, 1994, for a more detailed discussion of this procedure). The raw series for our first model of all sexual offense cases filed appeared to be nonstationary in level, requiring differencing, and seasonal differencing was also indicated. Specifically, these differencing procedures help account for the overall trend and any seasonal trend (e.g., similar patterns of high or low values in certain months that repeat yearly). This series also appeared to be nonstationary in its variance (i.e., the variance was not constant throughout the length of the series). A natural log transformation of the series was used to account for this. Finally, an

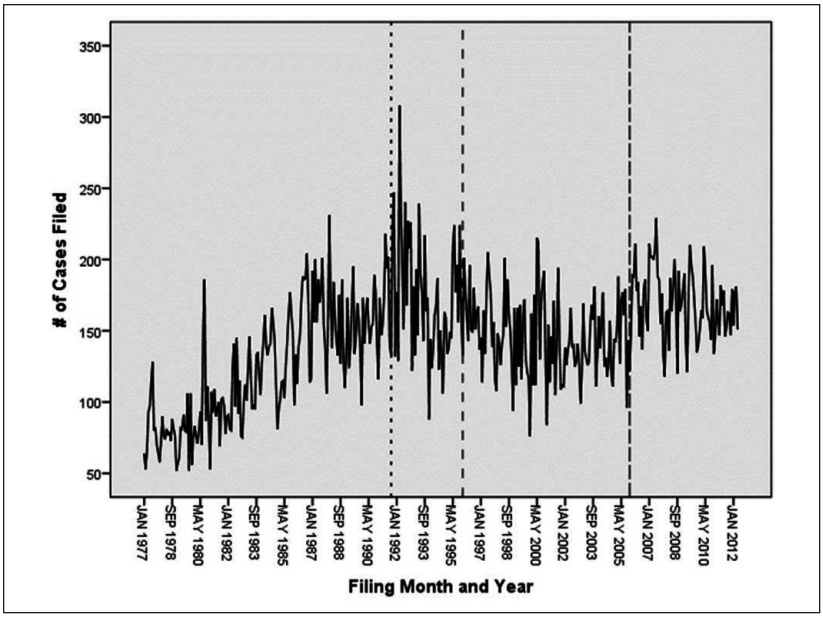

Figure 1. Monthly number of case filings for all sexual offenses from 1977 to 2012.
Note. Dashed lines represent the approximate timing of SORN implementation on September 01, 1991, and modifications on September 01, 1997, and September 01, 2005.

examination of the autocorrelation function (ACF) and partial autocorrelation function (PACF) for the log-transformed series for all sexual offenses indicated the presence of a first-order moving average (MA) process in which each current observation was influenced by the previous observation (McDowall, McCleary, Meidinger, & Hay, 1980). In addition, a seasonal MA component was indicated. The final univariate model for this time series (all sexual offenses) took the form of an autoregressive integrated moving average (ARIMA) (0, 1, 1) (0, 1, 1) model with a log transformation (as did our models for cases filed against first-time offenders, and in our comparison series involving nonsexual assault case filings). This model generally suggests an overall increasing trend in the number of cases per month over time and a seasonal trend in which the number of cases filed appears to be higher during the fall months and somewhat lower during the winter months.

Subsequent models for other measures of the number of monthly sexual offense cases filed (e.g., sexual assaults specifically) were essentially similar in their characteristics, ARIMA (0, 1, 1) (0, 1, 1), although several did include

some small adjustments to these parameters. For example, the final models for sexual assault cases filed, and for all sexual offense cases filed against repeat offenders took the form of (0,1,1) (0,0,0), while those for offenses involving children took the form of (0,1,1) (1,1,1). In each case, an examination of the models' residuals indicated that all systematic autocorrelation was removed (i.e., the model residuals were white noise as indicated by the ACF, PACF, and Ljung-Box Chi-Square). Based on a preliminary examination of the raw data series and the nature of the change, it was expected that the implementation of SORN requirements should have an abrupt and permanent impact on sexual offense case filings, as would each of the two subsequent modifications examined here. In each case, these abrupt permanent changes were represented by a zero-order transfer function (see Cochran et al., 1994).[2]

Initial SORN implementation, 1991. Results from the basic model of all sexual offense cases filed are presented in Table 2 and indicate that the introduction of SORN requirements on September 1, 1991, was not related to a significant change in the number of sexual offense case filings per month (Est. = .000, *SE* = .000, *ns*, see the second column labeled "SORN Intervention"), above and beyond the preexisting, significant upward trend in monthly case filings (Est. = .848, *SE* = .027, $p < .01$). In fact, the estimate from this model of the relationship between SORN implementation and the number of sexual offense cases filed (Est. = .000) is essentially 0, meaning no relationship whatsoever, once the overall increasing trend in the number of these kind of cases over time is controlled for.

To further explore the differential impacts proposed in Hypothesis 1, results in Table 2 demonstrate the impact of SORN implementation in 1991 on the monthly number of cases filed for sexual assaults (Est. = −.004, *SE* = .003, *ns*), sexual offenses against children (Est. = .000, *SE* = .000, *ns*). In each of these models, there again appears to be no relationship between SORN implementation and specific types of sexual offenses. Likewise, SORN implementation in 1991 was also not significantly related to the monthly number of sexual offense cases filed against first-time (Est. = .000, *SE* = .000, *ns*) or against repeat offenders (Est. = .001, *SE* = .003, *ns*), separately. These results suggest no specific, or general, deterrent relationships arising from the implementation of SORN requirements in 1991.

Again, the number of cases filed monthly for each of these crime measures exhibited a significant upward trend (refer to the third column labeled Difference MA), which was not affected by the implementation of SORN requirements. Overall then, SORN implementation in 1991 does not appear to be related to any significant reduction in the number of all sexual offense cases filed, nor is it differentially effective in terms of specific crimes against

children or sexual assaults. Likewise, our analyses provide no evidence that initial SORN implementation in 1991 produced either general or specific deterrent effects (i.e., first-time and repeat offenders, respectively).

As expected, our comparison series for SORN implementation was also not significantly related to the number of monthly cases filed for nonsexual assaults (Est. = −.005, *SE* = .005, *ns*). This result is consistent with our prediction in Hypothesis 4 and suggests that SORN requirements may have little effect on other types of interpersonal violence. In general, each of these five models suggest no relationship between the implementation of SORN requirements in 1991 and any of our five measures of the number of sexual offense cases (e.g., all cases, sexual assaults, first-time offenders) filed per month, or in the number of cases filed for our general violence measure (i.e., nonsexual assaults), once the preexisting upward trend is controlled for.[3]

SORN modifications. Table 2 presents results from two additional sets of models examining the impact of modifications to Texas's SORN requirements in 1997 (related to Hypothesis 2) and again in 2005 (Hypothesis 3). Recall that in 1997, the original law was modified to retroactively require offenders to register if they were convicted of an RSO on or after September 1, 1970, rather than prior to the initial implementation of the law in 1991. Likewise, in 2005, the law was modified again, this time to require sex offender registration for those individuals whose RSO conviction occurred on or after September 1, 1997, even if the RSO took place in another state. As was the case for the models estimating the impact of SORN's initial implementation in 1991, neither modification appeared to be related to any significant change in the number of sexual offense cases filed in this jurisdiction, regardless of the measure examined (e.g., all sexual offenses, sexual assaults, offenses by first-time offenders), contrary to the predictions advanced in Hypotheses 2 and 3, respectively. In fact, each of the 10 estimates for the effect of SORN modifications on sexual offenses are at or near 0.000, as is the estimate of the impact of each SORN modification on our measure of general violence (the number of cases filed for nonsexual assaults, related to Hypothesis 4). As was the case for the models presented in Table 1, there was a significant increasing trend for each of our measures of monthly case filing (refer to the third column in Table 2), but in no instance did these two modifications for SORN requirements significantly affect these underlying trends.

Discussion

Consistent with most of the existing research on the effectiveness of SORN laws, the present results do not provide evidence in support of Texas's SORN

Table 1. Time-Series Models of the Impact of SORN Implementation in 1991 on Monthly Case Filings.

	Implementation of SORN in 1991			
Monthly case filings measures	SORN intervention (*SE*)	MA (*SE*)	Seasonal MA (*SE*)	Seasonal AR (*SE*)
All sexual offenses (0,1,1) (0,1,1)	.000 (.000)	.849** (.027)	.944** (.039)	—
Sexual assaults (0,1,1) (0,0,0)	−.004 (.003)	.858** (.025)	—	—
Offenses against children (0,1,1) (1,1,1)	.000 (.000)	.824** (.029)	.950** (.042)	−.125* (.054)
First-time offenders (0,1,1) (0,1,1)	.000 (.000)	.853** (.027)	.937** (.037)	—
Repeat offenders (0,1,1) (0,0,0)	.001 (.003)	.863** (.025)	—	—
Control series: *Nonsexual assaults* (0,1,1) (0,1,1)	−.005 (.005)	.549** (.042)	.856** (.031)	—

Note. SORN = sex offender registration and notification; MA = moving average; AR = autoregressive.
*$p < .05$. **$p < .01$.

law on the monthly numbers of sexual offense cases filed in one large metropolitan county, or for two subsequent modifications to the law, each of which expanded the numbers of offenders subject to registration/notification requirements. In fact, our results generally reveal a lack of relationship between SORN laws and rates of all sexual offenses, or with specific measures of sexual assaults against adults, or sexual offenses against children. Likewise, we found no relationship between SORN requirements or the two modifications that expanded them and the number of sexual offenses committed by repeat offenders (i.e., those who would conceivably have been subject to the specific deterrent effects created by these requirements), or first-time offenders who would be subject to general deterrent effects.

Perhaps more importantly, at least in terms of the ability of SORN requirements to produce specific deterrent effects on those considering a sexual offense for the first time, our results also show that as many as 70% of the sexual offenses were committed by individuals who had not previously been arrested for an RSO, at least in this particular jurisdiction. In other words, it is possible that more than two thirds of the sexual offenders in this county would not have been subject to the state's SORN requirements, and thus would not have had their opportunities to commit a sexual offense limited through increased community awareness (specific deterrence), nor do they appear to have been deterred by the overall threat of registration/notification

Table 2. Time-Series Models of the Impact of SORN Modifications in 1997 and 2005 on Monthly Case Filings.

	Modification of SORN in 1997				Modification of SORN in 2005			
Monthly case filings measures	SORN intervention (*SE*)	MA (*SE*)	Seasonal MA (*SE*)	Seasonal AR (*SE*)	SORN intervention (*SE*)	MA (*SE*)	Seasonal MA (*SE*)	Seasonal AR (*SE*)
All sexual offenses (0,1,1) (0,1,1)	.000 (.000)	.847** (.027)	.939** (.038)	—	.000 (.000)	.848** (.027)	.938** (.037)	—
Sexual assaults (0,1,1) (0,0,0)	−.002 (.003)	.853** (.026)	—	—	.002 (.005)	.852** (.026)	—	—
Offenses against children (0,1,1) (1,1,1)	.000 (.000)	.823** (.029)	.948** (.041)	−.125* (.054)	−.001 (.001)	.826** (.029)	.946** (.041)	−.127* (.054)
First-time offenders (0,1,1) (0,1,1)	.000 (.000)	.851** (.027)	.932** (.036)	—	.000 (.001)	.851** (.027)	.931** (.035)	—
Repeat offenders (0,1,1) (0,0,0)	.003 (.003)	.867** (.025)	—	—	.005 (.005)	.866** (.025)	—	—
Control series: *Nonsexual assaults* (0,1,1) (0,1,1)	−.005 (.010)	.548** (.042)	.856** (.031)	—	−.016 (.017)	.550** (.042)	.855** (.031)	—

Note. SORN = sex offender registration and notification; MA = moving average; AR = autoregressive.
*$p < .05$. **$p < .01$.

(general deterrence). Although our ability to determine whether an individual in this data was actually a "first-time" offender is limited by the fact that we only have access to data for this one county, the overall result is consistent with other published studies (e.g., Sandler et al., 2008) that have found that the majority (in some cases as many as 95%) of sexual offenders were first-time offenders. Beyond the finding of a large proportion of first-time offenders in this data (who would not be subject to specific deterrent effects of the SORN law), the time-series results showing no effect of SORN requirements on first-time offenders also suggests that the presence of these requirements does not create a general deterrent effect among "would-be" first-time sexual offenders, either.

A number of authors (Sandler et al., 2008; Vasquez et al., 2008) have previously outlined what some consider the faulty assumptions about sexual offenders (e.g., that they have a high likelihood of sexual recidivism, are mostly repeat offenders, and typically offend against strangers) that underlie SORN laws. While the data examined here does not address the accuracy of these assumptions (other than the relatively low proportion of repeat offenders noted above), our results do add to the growing number of studies that fail to show significant relationships between levels of sexual offending and implementation of such laws. The analyses presented here also extend the existing literature on the effectiveness of SORN laws in that we have examined a single, specific jurisdiction, within which one would expect the implementation of the state law to be relatively uniform. Thus, while most other studies could be criticized for possibly missing significant deterrent relationships that may have emerged in particular parts of a large jurisdiction (like a state or series of states), the current results suggest that even in a single urban area, the implementation and subsequent modification of SORN requirements was not related to the number of sexual offenses occurring over time. Despite legislative attempts in this particular state to improve the functioning of SORN requirements, by applying them to increasing numbers of sexual offenders in the modifications of 1997 and 2005, neither of these expansions appear to have improved the effectiveness of the initial implementation of the law in 1991.

Limitations

Although the current analyses add to, and extend, the existing literature on the effectiveness of SORN requirements through the use of several methodological improvements, the study is not without limitations. First, as mentioned previously, our ability to accurately identify "first-time" and "repeat" offenders is limited by the availability of data for only those offenses

committed in this particular county. At the same time, none of our models for specific measures (e.g., all sexual offenses, sexual offenses against children) showed any signs of deterrence related to the implementation or two expansions of SORN requirements. The current effort is also limited in that we could not determine which offenders had been subject to registration requirements, as opposed to registration *and* community notification, such that we cannot examine the potential added deterrence that may accrue from community notification efforts, over registration requirements alone. Just as it is instructive to broadly examine the impact of SORN laws within a specific jurisdiction, (to avoid methodological problems like binning) it will also be important for future research to explore the potential distinct effects of registration *and* community notification, within a particular jurisdiction where they are (conceivably) implemented in a consistent manner within the jurisdiction.

Importantly, examining a given jurisdiction was seen as an important next step in the research on SORN effectiveness (i.e., to overcome possible issues of binning); as a result, our study examined only one particular county. Whether the operation and effectiveness of SORN laws are similar in other large cities, is an important question for future research to address. Likewise, the effectiveness of SORN laws in individual, medium- or small-sized cities (or rural areas, for that matter) should also be investigated. It may be, for instance, that the anticipated additional deterrent effects of social stigma are more powerful in smaller, more cohesive communities, and as such SORN laws may have greater impact in such localities than they do in a relatively large, diverse urban area where offenders may perceive more "anonymity." Finally, while we estimated only one control series to examine trends in the incidence of more general violent crime (i.e., nonsexual assaults), it may have been informative to determine the impact of SORN laws on other crime types. However, control series are usually used to determine if a change in a given trend is related to the intervention of interest. In this case, SORN requirements had no impact on sexual offending case filings, such that exploring possible alternative explanations of a change (that may have occurred among some other crime type) is not necessary.

Conclusion

A growing number of studies suggest that the implementation of SORN requirements has had little relationship to rates of sexual offending over time. Emerging research in this area has also begun to examine the unintended consequences of such requirements (see Lasher & McGrath, 2012, for example) that occur for those individuals who are subject to the requirements

(though, as Sandler et al., 2008 note, these offenders evoke little empathy from the community). While it may be unrealistic to expect SORN requirements to be eliminated, or even limited to use with the most serious offenders, continued reliance on these policies as the primary response to the threat of sexual offending comes with its own unintended societal consequences. Some have even suggested that reliance on these policies leads to a false sense of security and lack of realistic knowledge about the offense patterns and reoffense risks posed by sexual offenders that further endangers the public (see Prentky, 1996).

On the contrary, a number of policy approaches have demonstrated success at restricting sexual offenders' recidivism, including participation in cognitive-behavioral treatments (Lösel & Schmucker, 2005), as has provision of social supports (e.g., community volunteers who help supervise offenders after release from prison) to even high-risk sexual offenders (Wilson, Cortoni, & McWhinnie, 2009). Likewise, Sandler and colleagues (2008) suggested efforts should be made to provide scientifically accurate knowledge to the public about the most common forms of sexual offending (e.g., by would-be offenders who are likely already known to them). Public opinion surveys generally show broad support for SORN laws (Saad, 2005), as well as a belief in their effectiveness. Given this level of support, it seems unlikely that SORN laws would be rolled back in any way; however, policy makers interested in implementing efforts to protect public safety and reduce sexual offending should consider adding some of these other empirically supported approaches to the mix of responses. The current results, in conjunction with much of the existing research on SORN laws, suggest that these requirements alone may do little to combat the problems posed by sexual offenders in the community.

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Notes

1. Readers may question the use of a variable representing the number of cases filed in court as opposed to arrests for sexual offenses because the number of cases filed in court may underrepresent the actual number of arrests for sexual

offenses made in the jurisdiction. Harris County, Texas is unique in this regard, in that since 1973 (4 years prior to the beginning of the time period of the data used here) police officers have been required to prescreen all arrests (not just for sexual offenses, but for all crimes) with a member of the prosecutor's office prior to making the arrest and filing charges. As such, in this jurisdiction, there are no arrests that do not appear in the data for court filings of initial charges (P. A. Ormachea, personal communication, August 16, 2016).

2. While the existing research on the impact of sex offender registration and notification (SORN) laws using interrupted time-series models has consistently assumed and modeled any SORN effects as abrupt permanent changes, an anonymous reviewer suggested it might be useful to conduct some sensitivity analyses to explore whether SORN effects were temporary, not permanent. With no empirical or theoretical guidance to suggest how long the effects might last, we estimated models where SORN effects were assumed to last either 3, 6, or 12 months after initial SORN implementation in 1991 (on monthly counts of all sexual offenses, in particular). These supplemental models revealed similar results to those presented in the "Results" section, which modeled abrupt permanent changes in the number of sexual offenses filed. The authors would like to thank an anonymous reviewer for this suggestion.
3. At the suggestion of an anonymous reviewer, we also investigated the possibility that our start date for SORN implementation (September 1, 1991; derived from the statute itself) may have been inaccurate. Though we have no reason to believe our original start date is incorrect, to test this suggestion, we did model four alternative versions of our "all sexual offenses" time-series model, delaying the SORN implementation date by 1, 3, 6, and 12 months from September 1, 1991. None of these alternative start dates affected the trend in monthly numbers of sexual offense cases filed. Likewise, an anonymous reviewer also suggested that by modeling data going back to 1977, some very old trends in the data might confound our ability to detect changes in sexual offense case filings after SORN implementation from the trend in these numbers just prior to implementation. To examine this issue, we restricted the data included in our time-series model to roughly 4 years prior to, and 4 years after SORN implementation in 1991 and results again revealed no effect of the SORN law on number of all sexual offenses filed per month. Results not presented, but available upon request.

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