RESEARCH ARTICLE

The Effect and Implications of Sex Offender Residence Restrictions

Evidence from a Two-State Evaluation

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Research Summary

We evaluated the efficacy of sex offender residence restrictions in Michigan and Missouri using a quasi-experimental design with propensity score matching. First, we examined the implementation of the laws and found that sex offenders in both states were less likely to live in restricted areas after the implementation of the laws than the prerestriction sample, but the differences were not statistically significant. In our outcome analysis, we find little evidence that residence restrictions changed the prevalence of recidivism substantially for sex offenders in the postrelease period. In Michigan, trends indicate that the implementation of the laws led to a slight increase in recidivism among the sex offender groups, whereas in Missouri, this effect resulted in a slight decrease in recidivism. Technical violations also declined for both groups in Missouri. The small effect sizes, inconsistent results across states, and the null results between sex offender and non–sex offender models cast doubt on the potential usefulness of the laws to influence individual patterns of recidivism broadly.

Policy Implications

The results caution against the widespread, homogenous implementation of residence restrictions. Instead, we advocate individualization in sex offender programming and call for the development of risk-centered models of residence restrictions that draw on the established literature. In addition, the research highlights the practical challenges in defining restricted areas, enforcing restrictions, and promoting successful returns to the community. Furthermore, a call for reframing the focus of sex offender reentry to include collaborative treatment groups and enhanced communication and services between key stakeholders is made. Finally, we close with a discussion of several best practice models that provide alternative housing sources for individuals sentenced under residence restrictions without a suitable home plan.

Keywords

residence restrictions, recidivism, parole, sex offenders, geographic information systems, collateral consequences of incarceration.

Of the specialized forms of sex offender management, which have proliferated in the past 20 years, sex offender residence restrictions are among the most controversial. Residence restrictions are a specific form of specialized legislation that prohibits sex offenders from residing within a certain distance from places where children congregate, such as schools or day care centers. Residence restrictions were designed to enhance public safety by neutralizing the risk of recidivism posed by registered sex offenders released into the community (Levenson and Cotter, 2005; Sample, Evans, and Anderson, 2011; Simon, 1998; Socia, 2011). The rationale behind this goal is that sex offenders choose their victims from the available population of the area in which they reside. Thus, attempts by the criminal justice system to increase the distance between registered sex offenders and potential targets should correspond to a decrease in recidivism among this group (Kang, 2012). Statewide residence restrictions have been adopted in some form by 33 states and at the municipal level in several others (Mancini, Barnes, and Mears, 2013). With heightened public concern regarding sex offenders, such laws are considered to have widespread public support (Kernsmith, Craun, and Foster, 2009; Mancini, Shields, Mears, and Beaver, 2010; Schiavone and Jeglic, 2009). Support from parole boards is generally more muted (Tewksbury and Mustaine, 2012), despite community corrections' officials assessments of sex offenders as dangerous (Tewksbury, Mustaine, and Payne, 2012).

To date, there has been little research on the efficacy of residence restrictions in reducing recidivism among registered sex offenders. Scholars have focused primarily on the projected impact of residence restrictions on available housing, qualitative descriptions of the unintended consequences of legislation, and the aggregate effect of residence restrictions on sex crime trends (see Pacheco and Barnes [2013] for a review). Overall, scholars have

not substantiated a link between residence restrictions and reduced recidivism specific to sex offender populations.

The goal of the current study is to build on extant research and consider the efficacy of residence restrictions enacted in Missouri and Michigan. The analysis proceeds in two phases. The first phase of the analysis documents change in the residential locations of sex offenders and non–sex offenders before and after the implementation of the residency restriction laws. The second phase of the analysis was designed to examine change in recidivism patterns before and after the implementation of residence restrictions. The research questions are addressed using a quasi-experimental design to compare the outcomes of the postrestrictions sex offender sample with a prerestrictions sample and a contemporary control sample of non–sexual offenders selected using propensity score matching. The analyses are designed to inform current policy on residence restrictions and enhance our empirical understanding of sex offending and offenders more generally.

Relevant Literature

Goals and Assumptions of Residence Restrictions

States and municipalities adopt residence restrictions and other forms of sex offender legislation for practical and symbolic reasons (Levenson and Cotter, 2005; Sample et al., 2011; Simon, 1998; Socia, 2011). In the practical sense, the purpose of residence restrictions is to protect vulnerable populations (i.e., children) from sexual victimization by reducing the recidivism risk of sex offenders known to law enforcement. This task is accomplished by restricting potential opportunities to recidivate sexually. To be effective, residence restrictions operate under the assumption that recidivist sex offenders choose to reside close to clusters of victims (i.e., schools) and that the potential for recidivism will decrease if their residential proximity to these possible victims could be increased (Mancini et al., 2013). This reasoning is consistent with the "distance decay" hypothesis, which suggests that most crimes are likely to occur in proximity to the offender's home and the risk of offending declines as they move away from their residence (Brantingham and Brantingham, 1984; Van Koppen and De Keijser, 1997). Routine activities theory (RAT) (Cohen and Felson, 1979) also provides a theoretical rationale for residence restrictions. At the aggregate level, RAT predicts that the rate of recidivist sex crimes will decrease if known sex offenders' access to potential child targets is physically restricted to the extent that there are fewer opportunities for sex offenders to encounter potential victims in the absence of capable guardians.

To date, few scholars have tested the assumptions of residence restrictions using empirical data. In their study, Walker, Golden, and VanHouten (2001) found that among sex offenders living in an Arkansas county, those with child victims lived closer to schools, parks, and day care centers than those with adult victims. Another study observed that registered sex offenders living in Newark, New Jersey, resided closer on average to restricted locations than a random sample of community members (Chajewski and Mercado, 2008). The authors add, however, that no differences were found between child and adult sex

offenders in proximity to schools, suggesting that they offer only partial support for the assumptions of residence restrictions.

In contrast, research generally has cast doubt on the potential efficacy of residence restrictions to decrease sex offender recidivism. Several studies have noted that sex offenders were unlikely to live in census tracts with larger potential victim clusters (Red-Bird, 2009; Tewksbury and Mustaine, 2008), contradicting claims that they select residences based on victim availability. Residential proximity to schools has not been found to differentiate sex offender recidivists from nonrecidivists. In one study based in Florida, Zandbergen, Levenson, and Hart (2010) compared the residential proximity to schools and day cares of matched samples of sex offender recidivists and nonrecidivists. Controlling for recidivism risk indicators, they observed that "residential proximity to schools and daycares explains virtually none of the variation in sexual recidivism" (Zandbergen et al., 2010: 498; see also Colorado Department of Public Safety, 2004; Minnesota Department of Corrections, 2003). This research indicated that proximity to victim clusters was not found to be the risk factor it is posited to be under a residence restrictions framework.

Furthermore, of the 224 sex offenders recommitted for a new sex offense in Minnesota between 1990 and 2005, none had committed their crime by establishing direct contact with a victim younger than 18 years of age at a school, park, or day care center within 1,000 feet of the offender's residence (Duwe, Donnay, and Tewksbury, 2008). Research has suggested also that sex offenders, particularly those with child victims, are more likely to gain access to potential targets through friends and family and that initially they meet their victims in public or semipublic locations that are not specified as restricted locations under current legislation (Colombino, Mercado, and Jeglic, 2009). In combination, these findings have suggested that the implementation of residence restrictions might contribute only to small reductions in recidivism, especially among sex offenders convicted of offenses against children.

The Effect of Residence Restrictions on Recidivism

If the assumptions behind residence restrictions were to hold, then their implementation would be expected to decrease sexual recidivism, at either the individual or the aggregate level. To date, research examining the direct effect of these policies on recidivism has been relatively rare. Much of the existing research has considered aggregate rates of sex crime, as opposed to evaluating the effect of residence restrictions on the recidivism of individuals.

Of the few studies that have been conducted, results have been mixed. Blood, Watson, and Stageberg (2008) observed a positive effect of residence restrictions on minor-involved sex offense charges and convictions in Iowa, indicating that there was an increase in such incidents in the postrelease period. As Socia (2012b) noted, Blood and colleagues did not differentiate the sex offense convictions of registered sex offenders (i.e., those under the jurisdiction of the residence restrictions) from first-time (i.e., nonregistered) sex offenders. The possibility is that Iowa's residency restriction law did affect registered sex offenders, but this effect was obscured by increased offending by first-time sex offenders. Also, it is

possible that the passage of residence restrictions increased criminal justice system attention on child sex crimes, resulting in additional charges.

Nobles, Levenson, and Youstin (2012) examined the effect of increasing the boundary of Jacksonville, Florida's residency restriction law from 1,000 feet to 2,500 feet on the aggregate number of sex crime arrests. Comparing arrests for all sex crimes and recidivist sex crimes for roughly 2 1/2 years prior to and after the policy change, they found no significant effect for the expanded residency restriction law on either sex crime variable after controlling for the race, sex, age, and felony status of arrestees. A quasi-experimental interrupted time-series analysis also revealed no change in sex crime trends after the expansion of the policy.

Furthermore, Socia (2012a) observed mixed effects of residence restrictions on sexual recidivism. More specifically, his analyses compared monthly rates of arrests for recidivist and nonrecidivist sex crimes against both children and adults, comparing New York counties with and without residence restrictions, and comparing the same counties before and after their residency restriction law was implemented. Across a 12-year period, Socia (2012a) noted that county-level residence restrictions were statistically unrelated to recidivist sex crimes against children or adults and to nonrecidivist sex crimes against children. In contrast, the residence restrictions were associated with a decrease in nonrecidivist sex crimes against adults. Although Socia (2012a) interpreted this effect as possible general deterrence, it also indicates that residence restrictions might not have targeted effects on sex crimes.

Finally, Kang (2012) examined the effectiveness of North Carolina's 1,000-feet boundary zone policy using a large sample of sex offenders and non–sex offenders. Difference-in-difference tests suggested that sex offenders released postrestrictions were significantly more likely to recidivate with violent and property offenses, relative to non–sex offenders. No analogous effect on sexual recidivism was observed. Although important, this research was not conducted with equitable comparison groups, did not control for differences in residential locations among the sample, and estimated outcome models using addresses at time of conviction. The current study was designed to address design gaps in previous studies by including multiple states, and more important, it identifies specifically both sex offenders and non–sex offenders in both preimplementation and postimplementation time frames, offering a more comprehensive portrait of effectiveness. The dearth of policy research on residency restriction laws despite initial implementation in the mid-1990s suggests additional research is necessary (Meloy, Miller, and Curtis, 2008).

Current Study

The states of Michigan and Missouri are the focus of the current study. The states were chosen for the analysis because they were among the first to implement statewide residence restrictions, maintain comprehensive sex offender registries, and use community notification programs.[1] Missouri has passed civil commitment legislation, and Michigan uses driver's

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1.   Michigan passed the first of a series of sex offender registration acts on October 1, 1995, and registry information became publicly available on the Internet in 1999. The registration act requires a minimum

license notation. Michigan and Missouri are 2 of 13 states that maintain residence restriction zones of 1,000 to 1,999 feet. In comparison, 7 states have set residency boundaries at 500 to 999 feet, 6 states have 2,000-feet boundaries, and 7 states maintain ad hoc boundaries based on offender and victim characteristics that are determined by judicial and correctional officials (Mancini et al., 2013).

The state of Michigan passed a residence restriction law in October 2005, and it was implemented on January 1, 2006 (Michigan Sex Offender Registration Act, 2005). The law prohibits sex offenders from residing, working, or loitering within 1,000 feet of school property, which also is deemed the school safety zone. Residence restrictions in Missouri became law on June 5, 2006. Sex offenders in Missouri are prohibited from living within 1,000 feet of a "public or private school up to the 12th grade or state-licensed childcare facility which is in existence at the time of the offender establishing his or her residency" (Revised Statutes of Missouri, 2006). Sex offenders also are barred from working or loitering within 500 feet of a school, childcare facility, or public parks with playground equipment or a swimming pool.

In both states, the laws are broad in scope and apply to offenders who committed crimes against children and adults. In Michigan, registerable offenses are those that fall under the criminal sexual conduct statute (Michigan Sex Offender Registration Act, 2005) and a variety of "other assaultive" person offenses that include a sexual component (e.g., accosting, enticing, or soliciting a child for immoral purpose and indecent exposure). Similarly, in Missouri, the restrictions apply to felony offenses in Chapter 566, RSMo, including rape, sodomy, sexual misconduct, sex trafficking, and several other offenses, such as child pornography, not captured under the sexual assault category. For the purposes of the study, a "sex offender" was categorized as anyone who was paroled after serving a sentence for a registerable sex offense, which largely consisted of criminal sexual conduct and assault with intent to commit criminal sexual conduct. Although some states have local towns that enact stricter boundary zones than the statewide law (Meloy et al., 2008), there are no local enhancements in Michigan or Missouri.

The goal of the current study is twofold. The first phase considers the implementation of residence restrictions. We use geographic information systems to document the magnitude of the change in residential patterns of sex offenders after the enactment of residence restrictions. The second phase includes an outcome evaluation. The central concern with

registration of 25 years and lifetime registration for second and subsequent offenses; individuals convicted of a felony sex offense must verify their address four times per year and misdemeanants once per year. Although not specifically part of the student safety zone language, the Michigan Department of Corrections gives all sex offenders a parole condition requiring them to remain 500 feet from any licensed day care center and precludes housing placements within that boundary as well. Failure to register is punishable with a felony, and residency or loitering mandates a 1-year misdemeanor. Missouri passed its first registration legislation in January 1995. The law requires all offenders convicted of criminal sexual conduct to register with the state police; offenders who victimized adults must register and verify their address every 6 months, and offenders who assaulted juveniles or have been deemed persistent sexual offenders must register every 90 days. Missouri requires lifetime registration for all sex offenders.

this aspect of the study is how patterns of recidivism compare with what would have taken place in the absence of the legislation. We use a quasi-experimental design with propensity score matching to estimate the efficacy of residence restriction legislation. This research design allows for a multistate evaluation of policy further increasing the internal and external validity of the research observations (Shadish, Cook, and Campbell, 2002).

Data, Sample Selection, and Measures

Data for the current study were obtained from two sources. Recidivism and parolee movement information were culled from official records maintained by the Michigan and Missouri Department of Corrections. Address data for schools and childcare facilities were obtained from the Missouri State Police, the Missouri Statistical Analysis Center, and the Michigan Department of Information Technology and Michigan Center for Educational Performance and Information.

The study sample includes parolees who were released from prison before and after residency legislation was enacted. We use a quasi-experimental design with propensity score matching to estimate the efficacy of the restriction policy. As such, we selected four comparison groups for the study, including a postrestriction intervention sex offender sample, a prerestriction control group of sex offenders, and prerestriction and postrestriction control samples of nonsexual offenders. The prerestriction and postrestriction date ranges varied across the two sites. The Michigan preintervention sample includes individuals released between January 1, 2003 and December 31, 2005, and the postrelease data represent January 1, 2006 through December 31, 2007. The preintervention sample in Missouri includes parolees released between July 1, 2004 and June 5, 2006, and the postintervention sample was selected from parolees released from June 6, 2006 through June 5, 2008.

The non–sexual offender comparison samples were generated using propensity score matching. Propensity scores are used to balance treatment and control groups by modeling the conditional probability of receiving treatment given a set of observed covariates and then comparing individuals with similar balancing scores (Rosenbaum and Rubin, 1983). We used a two-step modeling technique commonly used in developing propensity scores. In the first phase of the matching process, we selected a group of variables theoretically and empirically associated with recidivism (Gendreau, Little, and Goggin, 1996; Langan, Schmitt, and Durose, 2003; Quinsey, Rice, and Harris, 1995).

Next, to winnow the large non–sex offender samples, two non–sex offenders were matched to each sex offender using a caliper threshold of 0.001.[2,3] Matching was stratified

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2. Consistent with approaches suggested in the literature, we only included covariates that were antecedent to the current conviction offense. This approach avoids using a procedure that would attempt to predict group membership (i.e., whether a subject is a sex offender or non–sex offender) based on covariates that were direct outcomes of that group membership (see DeLisi, Barnes, Beaver, and Gibson [2009] and Rengifo and Stemen [2013] for similar approaches).

3. Initially, extremely large samples of non–sex offenders were received from the Michigan and Missouri Departments of Corrections. Propensity scores were estimated for 2,793 sex offenders and 50,522

by county type as delineated by the U.S. Census Bureau (i.e., metro area, urban, or rural), so that sex offenders paroled to a particular type of environment were matched to non–sex offenders paroled to the similar community structures. The matching procedure was performed a second time using only those sample members with complete demographic and recidivism data. The results presented in Appendices A and B indicate that after the second round of matching, the sex offender and non–sex offender samples were statistically similar on all observed covariates as indicated by the standardized bias statistic (Rosenbaum and Rubin, 1983). The resulting Michigan sample consisted of 1,703 sex offenders matched with 1,703 non–sex offenders ($N = 3,406$). The final Missouri sample includes 2,224 non–sex offenders and 2,224 sex offenders ($N = 4,448$).

Dependent Measures

Recidivism is the primary focus of the study and is measured in several ways, including technical violation, reconviction, and rearrest. Recidivism is broadly conceptualized to include sexual and non–sexual offenses. Each dependent measure is dichotomous and captures a different aspect of postrelease criminality during a 2-year parole term. We also obtained data on the date of the recidivism event to facilitate hazard models. *Technical violations* represent failure of the parolee to comply with the conditions of release (drug tests, peer association, employment, etc.). We include a measure of technical violations for several reasons. First, sex offenders have a low base rate of reoffending, particularly reconvictions for sexual recidivism (Friendship and Thornton, 2001; Langan et al., 2003). It is important to identify low-level behaviors that might signal challenges on parole (English, Pullen, and Jones, 1997). Second, criminal justice actors have discretionary power in the parole revocation process, and recent research has suggested that revocation decisions might be based on offense type (e.g., sex offender) and extralegal factors (e.g., gender and race) (Lin, Grattet, and Petersilia, 2010). As such, changes in legislation might have influenced the manner and extent to which technical violations are enforced within and between offender populations. We observed variation in the technical violation rate across time and study state (see Table 1). In Michigan, 20.7% of sex offenders (denoted as "SO" in tables) and 13.5% of non–sex offenders (denoted as "NSO" in tables) incurred a technical violation in the preintervention period; the rates were 22.5% for sex offenders and 9.8% for non–sex offenders after the residency laws were passed. In Missouri, the prelegislation (27.8%, SO; 37.4%, NSO) rates of technical violation were much higher than the postrestriction period (16.5%, SO; 18.6%, NSO).

In addition, we include a measure of *new reconviction* that represents a new crime substantiated in court. Overall, reconviction rates were higher in both states in the postresidency restriction periods. Reconviction rates in Michigan increased from prelegislation

non–sex offenders in Michigan and for 5,997 sex offenders and 85,785 non–sex offenders in Missouri. Address and recidivism information were then collected for the resulting matched subsamples.

TABLE 1

Descriptive Statistics for Michigan and Missouri Samples

Variables	Michigan				Missouri			
	Prerestriction		Postrestriction		Prerestriction		Postrestriction	
	NSO	SO	NSO	SO	NSO	SO	NSO	SO
Dependent Measures								
Technical violation	13.5%	20.7%	9.8%	22.5%	37.4%	27.8%	18.6%	16.5%
Days to violation	366.3	319.6	315.0	276.3	371.18	338.4	254.5	221.8
	(198.5)	(194.7)	(155.5)	(200.8)	(191.8)	(208.6)	(167.5)	(169.8)
New conviction	2.4%	2.3%	4.4%	5.3%	8.2%	8.0%	7.7%	7.1%
Days to conviction	391.4	280.0	369.7	256.0	297.2	318.1	300.7	314.6
	(228.1)	(209.5)	(181.8)	(176.0)	(217.2)	(209.9)	(202.3)	(200.4)
New arrest	20.3%	14.0%	26.9%	17.8%	–	–	–	–
Days to new arrest	314.3	331.0	332.8	375.0	–	–	–	–
	(197.2)	(196.1)	(192.0)	(221.2)				
Independent Measures								
Age	35.9	35.9	35.8	34.3	40.3	40.4	40.2	39.8
	(10.3)	(10.1)	(11.3)	(10.7)	(11.1)	(9.7)	(10.7)	(9.6)
Female	3.4%	2.6%	3.6%	5.7%	13.7%	13.5%	13.0%	11.1%
Non-White	30.0%	29.8%	33.1%	28.4%	22.5%	23.1%	24.9%	24.6%
Married	45.1%	47.7%	44.7%	39.7%	23.2%	24.1%	26.2%	25.5%
HS/GED	60.6%	60.7%	53.1%	49.1%	64%	64.9%	68.7%	63.1%
Months incarcerated	45.1	70.9	46.8	65.6	7.7	8.2	11.1	17.0
	(45.5)	(48.4)	(55.5)	(50.0)	(7.1)	(8.0)	(10.6)	(14.4)
General misconduct	63.4%	64.3%	60.4%	69.1%	11.0%	33.8%	18.5%	35.0%
Sexual misconduct	5.0%	5.6%	5.1%	7.4%	1.0%	2.5%	1.9%	3.9%
Prior convictions	1.5	1.2	3.1	1.8	1.9	1.8	1.9	1.9
	(4.3)	(3.0)	(5.1)	(3.7)	(0.9)	(0.9)	(0.9)	(0.9)
Child victim	–	22.3%	–	21.5%	–	14.1%	–	16.3%

Notes. Standard deviations in parentheses; days to recidivism for recidivists only.

(2.3%, SO; 2.4%, NSO) to postrestriction periods (5.3%, SO; 4.4%, NSO). Less change was observed in Missouri with a small decline from prerestriction (8.0%, SO; 8.2%, NSO) to postrestriction periods (7.1%, SO; 7.7%, NSO). *Rearrests* correspond to events where the parolee was taken into custody by the police pursuant to an alleged crime. Rather than being a measure of whether a crime has actually taken place, rearrests are better viewed as an indicator of contacts with law enforcement.[4] Arrest data were only available for the Michigan sample. In total, 14.0% of sex offenders were arrested in the prerestriction period

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4. Soothill (2010) recently cautioned against the use of arrest data as measures of sex offense recidivism because an arrest for a sex offense might reflect simple suspicion on behalf of law enforcement, whereas registered sex offenders routinely are suspected when a new sex offense has occurred in the community.

compared with 17.8% postimplementation. The arrest rates for non–sex offenders were 20.3% preimplementation and 26.9% postrestrictions.

Independent Measures

The primary independent measure of interest is whether the parolee was released during the *residence restrictions* (0 = prerestriction release; 1 = postrestriction release). As noted, we use a propensity score matching procedure to balance the sex offender sample and the non–sex offender comparison group on observed covariates. After performing this procedure, several variables were unbalanced between the prerestriction and postrestriction sex offenders and non–sex offenders, introducing a selection bias threat to internal validity. As such, we control for these unbalanced measures in our multivariate analyses.[5] These measures are detailed in the subsequent discussion. Our goal was to create two equivalent groups of parolees who would differ only on the nature of their offending behavior.

Consistent with research of this type, we control for several factors that could influence selection into the sample and postrelease behaviors. Criminal history is measured using a continuous measure of *prior convictions*, and sample members, in both states and all groups, averaged less than two convictions. We include three measures of prison context and behavior including *incarceration length* (log transformed months), *general misconduct* (1 = one or more general misconduct citations; 0 = zero misconducts sustained during instant incarceration term), and *sexual misconduct* (1 = one or more sexual misconduct citations; 0 = no reported sexual misconduct citations).[6] For the sex offender models, an additional criminal history indicator of victim age, *child victim* (1 = victim younger than 13 years of age; 0 = victim 13 years or older), was used.[7] In total, 21% of offenders in Michigan had a child victim, and 14% of individuals in Missouri were serving time for a crime against a child. Finally, we include several demographic controls including *age* (in years), gender (1 = *female*), and race (1 = *non-White* including Black, Asian, and other; 0 = White).[8] Measures of marital status (1 = *married*; 0 = single, divorced, or widowed) and education (1 = greater than high-school education HS/GED) also are entered into the models. More information on the samples used is presented in Table 1.

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5.   The propensity models varied by state as did access to data. We elected to use the same independent measures in the final models to enhance consistency in our analyses.

6.   The sexual misconduct and general misconduct measures are mutually exclusive.

7.   Michigan uses age 13 as a statutory classification for seriousness of sexual offense. This measure was designed as a proxy as this subgroup of sex offenders are (a) the primary targets of residence restriction legislation given the language used in the laws, (b) more likely to commit a sex crime against a child given their prior conviction for a child sex offense (indicative of some unobservable/latent propensity), and (c) likely to be subject to surveillance from the community, including community correctional and law enforcement personnel.

8.   Data on ethnicity were not made available in either state.

Analyses

Geographic Residential Patterns

As noted, residence restriction legislation is based on the assumption that sex offenders could gain access to victims by selecting to reside close to schools/day care centers. This phase of the analysis was designed as a partial test of implementation fidelity. If the residence laws are enacted and carried out as planned, then we would expect to find very few, if any, parolees living in restricted areas after the implementation of the law.

To examine residential patterns, we used geographic information system (GIS) software (ArcGIS; Esri, Redlands, CA) to document the residential locations of the samples before and after the enactment of residence restrictions and the address of schools and licensed day cares. In the current analysis, our focus was on the offender's first residential address after release from prison. The first residential address has practical importance as it is the culmination of prerelease planning and decision making, and it has the ability to influence future movements (Massoglia, Firebaugh, and Warner, 2013).

For these particular analyses, we used a street network data model, or "street geocoding," which is the most used address geocoding procedure in GIS services and research (Zandbergen, 2008, 2009).[9] Addresses are identified by locating the street referenced in the address (e.g., Main Street), locating the segment of that street identified by the address number (e.g., 300–400), and then placing a point along the street segment based on the street number within the segment (e.g., 350, or halfway through the 300–400 segment). Additional indicators point to which side of the street the address is on and adjust the point accordingly (Zandbergen, 2008).[10]

Next, we drew 1,000-feet perimeters around public and private K–12 schools and licensed day cares, and we denoted the members of the sample who inhabited these areas.[11] As with any analyses of this type, there is potential measurement error concerning the extent of restricted property and the placement of sex offender addresses either inside or outside

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9.   For instance, Google Maps (Google, Inc., Mountain View, CA) uses street network data geocoding.

10.   Alternatives to our measurement scheme include the use of parcel data, in which the boundaries of geographic units (e.g., property lines and residential units) are mapped, and address point data, which places a single point at the centroid of a given parcel (Zandbergen, 2008), but we were not given access to these data. This procedure has clear implications for the validity of the analyses. In a comparison of the procedures, Zandbergen (2008) observed that street network data and address points tended to have higher geocoding match rates than parcel geocoding, but parcel geocoding was considered to be the most spatially accurate (Rushton et al., 2006). An exception to this was for addresses falling within multiunit residential complexes, in which street geocoding is considered to be more accurate than parcel data. Many sex offenders in our analyses resided in multiunit apartments. Our residency measurements were made as 1,000 feet from the center of the street in front of the school or day care center. We used a side offset of 20 feet to increase geocoding accuracy (Zandbergen et al., 2010).

11.   Our residency measurements were made as 1,000 feet from the center of the street in front of the school or day care center. In the study states, residency restrictions are enforced as the distance in feet from the property line of the restricted addresses, meaning that our analysis might vary slightly from the boundaries used by state officials.

of those boundaries. The analyses might be an underestimation of violations as we use only one point of measurement for each school and day care, instead of the property lines of each parcel, which are broader in scope. We could have potentially overestimated residency violations. Day care licenses in each study state are issued by the government and are valid for a set period of time. We extracted day care addresses during the postrestriction period and updated these data annually to account for changes. There is concern that some small private day care facilities change on a daily basis—particularly in urban areas.[12] Similarly, some of the sex offender addresses that fell within 500 or 1,000 feet of a restricted address might have been a special case, such as a nonresidential address (e.g., transitional housing, treatment center, etc.). We conducted sensitivity analyses (not shown) that excluded these addresses from the analysis. When these nonresidential addresses are removed, the violation rates decreased slightly but not in a statistically significant manner. In essence, a conservative interpretation of our results is warranted.

In Michigan, the sample includes 3,247 individuals: 1,596 sex offenders and 1,651 non–sex offenders. In Missouri, the geographic sample includes 3,608 individuals: 1,879 non–sex offenders and 1,729 sex offenders.[13] Table 2 provides an overview of residential locations of the study sample.

Overall, sex offenders in Michigan and Missouri were less likely than non–sex offenders to live in restricted areas before and after the implementation of the residence restrictions. However, many sex offenders retained residence in restricted zones postrestriction. Among the postrestriction sex offender sample, 22% of the first addresses in Michigan and 21% of addresses in Missouri were within the restricted zones. In both states, sex offenders in the postrestriction period were no more or less likely to be living in a restricted zone when compared with prerestriction sex offender addresses.

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12. The extent of childcare closures during the study period is not known. Whitebook and Sakai (2003), in a study of the National Child Care Staffing Study, found that 30% of 266 centers studied closed in a 9-year period. We anticipate that the closing rate is much smaller in an annual period.

13. In Michigan, we geocoded addresses prior to propensity score matching. The following metrics refer to the original Michigan sample and not just the propensity-score-matched subjects. The sex offender sample included a total of 7,917 addresses. Of these, 7,699 (97.2%) were successfully geocoded. Any addresses that were not automatically matched were manually geocoded. Of the 218 addresses that could not be geocoded either manually or automatically, 171 were mapped to zip code centroids. For Michigan, the final sex offender geographic sample included 7,870 data points. In addition 7,112 addresses were obtained for the non–sex offender sample. Of these, 6,809 (95.7%) were successfully geocoded. Of the 303 addresses that could not be matched successfully, 133 were mapped to a zip code centroid. The final non–sex offender dataset included 6,942 data points. In Missouri, the original sample included 4,448 individuals. In total, 10.5% of the original sample was dropped from the geographical analyses because the Department of Corrections did not provide a viable address (5.8%), the offender was paroled out of state (2.7%), or the offender was paroled to jail confinement (2.0%). Of the remaining addresses (*n* = 4020), 86.8% were matched and used for geographic analyses. The remaining 13.2% were matched via postal codes or by street names. Although this matching approximation gives some contextual confidence, it is not appropriate for specific geographical analyses that examine violation rates. The final sample of 3,609 represents individuals with complete street address and city data and/or zip code information.

TABLE 2

Sex Offenders and Non–Sex Offenders in Violation of Residence Restrictions, First Address

Group and Period	Prerestrictions	Postrestrictions	Prerestrictions	Postrestrictions
Sex Offenders	$n = 1,244$	$n = 352$	$n = 881$	$n = 848$
Schools	12%	13%	10%	11%
Day care centers	13%	12%	22%	19%
Overall	23%	22%	26%	21%
Child Molesters	$n = 280$	$n = 79$	$n = 116$	$n = 127$
Schools	13%	14%	11%	9%
Day care centers	15%	8%	22%	17%
Overall	25%	19%	26%	21%
Non-Sex Offenders	$n = 1,331$	$n = 320$	$n = 1,063$	$n = 816$
Schools	16%	15%	14%	12%
Day care centers	13%	13%	25%	23%
Overall	26%	24%	31%	30%

Notes. Michigan: $n = 3,247$; Missouri $n = 3,608$.

We also considered whether the residence restriction laws were applied differently to individuals with child victims. Our results indicate that although the overall proportion of offenders with child victims living in restricted areas declined in the postrestriction period, the differences were not statistically significant. Additionally, in both the prerestriction and postrestriction periods, individuals with child victims were not more or less likely to have a first address within 1,000 feet of a school or day care center when compared with offenders with adolescent or adult victims.

Recidivism

The analyses of recidivism proceed in two parts. The first phase is designed to estimate the change in the prevalence of recidivism after the implementation of residence restrictions. A 2-year postrelease follow-up period was used to equate the period of observation for all groups. We use logistic regression to estimate these models (Long, 1997). Next, we use proportional hazard models to consider the time of failure. These models are ideal for the current analysis as they account for censoring, which is common in recidivism studies (Cox, 1972; Singer and Willett, 2003). Proportional hazards models account for the variation in release dates by modeling the time interval between release from prison and recidivism.

Tables 3 and 4 include results of the outcome analysis. In Michigan, residence restriction legislation was associated with a statistically significant increase in reconvictions for the sex offender cohort, net of controls (see Table 3). A similar result was not observed for the non–sex offender sample, and no significant changes in arrests and technical violations were observed in the postrestriction period. However, there was a significant decrease in the

T A B L E   3

Logistic Regression and Survival Analysis Recidivism Models for Michigan Non-Sex Offenders and Sex Offenders

Variables	Logistic Regression						Survival Analysis					
	Technical Violation		Reconviction		Rearrest		Technical Violation		Reconviction		Rearrest	
Model number	1	2	3	4	5	6	7	8	9	10	11	12
	NSO	SO	NSO	SO	NSO	SO	NSO	SO	NSO	SO	NSO	SO
Postrestrictions	−0.42*	−0.04	0.53	0.64*	0.18	0.12	−0.27	0.06	0.68*	0.68*	0.13	0.08
	(0.18)	(0.14)	(0.32)	(0.30)	(0.14)	(0.16)	(0.17)	(0.11)	(0.30)	(0.28)	(0.11)	(0.14)
Age	−0.02	−0.04***	−0.08***	−0.09**	−0.03***	−0.06***	−0.01	−0.04	−0.08**	−0.09**	−0.03***	−0.06***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)
Female	−0.07	−1.50**	0.63	−0.11	−0.12	−0.44	−0.07	−1.35**	0.52	−0.13	−0.16	−0.39
	(0.46)	(0.54)	(0.83)	(0.78)	(0.38)	(0.46)	(0.42)	(0.51)	(0.75)	(0.75)	(0.32)	(0.42)
Non–White	0.07	0.43**	0.51	0.32	−0.07	0.25	0.08	0.37**	0.53	0.30	−0.04	0.21
	(0.16)	(0.13)	(0.32)	(0.31)	(0.14)	(0.15)	(0.15)	(0.11)	(0.30)	(0.29)	(0.12)	(0.13)
Ever married	0.28	0.39*	0.45	−0.14	−0.36*	0.23	0.23	0.33**	0.29	−0.19	−0.30*	0.21
	(0.18)	(0.15)	(0.37)	(0.39)	(0.16)	(0.17)	(0.17)	(0.13)	(0.35)	(0.36)	(0.13)	(0.15)
HS/GED	−0.20	−0.56***	0.37	0.26	−0.03	−0.12	−0.17	−0.51***	0.60	0.18	0.01	−0.14
	(0.16)	(0.13)	(0.33)	(0.31)	(0.13)	(0.15)	(0.14)	(0.14)	(0.30)	(0.28)	(0.11)	(0.13)
Months incarcerated (ln)	−0.20*	−0.29**	−0.00	−0.23	−0.30***	0.09	−0.20*	−0.23*	−0.03	−0.19	−0.25***	0.09
	(0.10)	(0.11)	(0.20)	(0.27)	(0.08)	(0.16)	(0.09)	(0.10)	(0.19)	(0.26)	(0.07)	(0.12)

TABLE 3

Continued

	Logistic Regression						Survival Analysis					
Variables	**Technical Violation**		**Reconviction**		**Rearrest**		**Technical Violation**		**Reconviction**		**Rearrest**	
General misconduct	1.59***	0.74***	1.47**	1.59**	0.78***	0.43*	1.46***	0.66***	1.39**	1.53**	0.64***	0.39*
	(0.23)	(0.16)	(0.48)	(0.55)	(0.16)	(0.19)	(0.22)	(0.14)	(0.47)	(0.54)	(0.13)	(0.17)
Sexual misconduct	0.36	0.54*	0.17	0.74	0.54	0.19	0.31	0.51**	0.28	0.72	0.45*	0.20
	(0.30)	(0.25)	(0.61)	(0.53)	(0.28)	(0.29)	(0.27)	(0.20)	(0.56)	(0.50)	(0.22)	(0.26)
Prior convictions	0.28	0.03	0.16***	0.18***	0.13***	0.14***	0.23	0.03*	0.12***	0.17***	0.09***	0.11***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)	(0.10)	(0.01)	(0.02)	(0.01)	(0.01)
Child victim	—	−0.17	—	0.30	—	−0.32	—	−0.13	—	0.28	—	−0.27
		(0.16)		(0.40)		(0.20)		(0.14)		(0.38)		(0.17)
Intercept	−1.87	0.75	−3.33	−1.98	0.05	−0.64	—	—	—	—	—	—
	(0.38)	(0.38)	(0.84)	(0.98)	(0.30)	(0.44)						
Cox and Snell R^2	0.052	0.085	0.053	0.054	0.119	0.081	—	—	—	—	—	—
−2 Log Likelihood	1,193.3	1,680.3	358.4	384.2	1,579.0	1,300.5	3,005.9	5,093.5	649.2	705.4	5,265.0	3,639.2

Notes. Unstandardized coefficients presented with standard errors are in parentheses.

*$p < .05$. **$p < .01$. ***$p < .001$.

T A B L E   4

Missouri: Logistic Regression and Survival Analysis Recidivism Models for Non–Sex Offenders and Sex Offenders

	Logistic Regression				Survival Analysis			
	Technical Violation		Reconviction		Technical Violation		Reconviction	
	1	2	3	4	5	6	7	8
Variables	NSO	SO	NSO	SO	NSO	SO	NSO	SO
Postrestriction	−0.89***	−0.66***	−0.07	−0.14	−0.68***	−0.52***	−0.09	−0.24**
	(0.11)	(0.11)	(0.16)	(0.17)	(0.09)	(0.10)	(0.12)	(0.09)
Age	−0.03***	−0.02***	−0.03***	−0.02*	−0.02***	−0.02***	−0.02***	−0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Female	−0.21	0.01	−0.49	−0.38	−0.18	−0.03	−0.25	−0.25
	(0.15)	(0.16)	(0.27)	(0.29)	(0.12)	(0.13)	(0.18)	(0.14)
Non-White	0.01	0.17	−0.24	0.11	−0.03	0.15	−0.09	0.01
	(0.12)	(0.12)	(0.20)	(0.18)	(0.09)	(0.11)	(0.14)	(0.10)
Married	−0.43***	−0.04	−0.30	−0.11	−0.36**	−0.02	−0.21	−0.18
	(0.12)	(0.13)	(0.20)	(0.20)	(0.10)	(0.11)	(0.14)	(0.10)
HS/GED	−0.39***	−0.23*	−0.15	−0.23	−0.30	−0.20*	−0.08	−0.09
	(10)	(0.11)	(0.17)	(0.16)	(0.08)	(0.09)	(0.12)	(0.09)
Months incarcerated	−0.21*	−0.11	0.04	−0.15	−0.15*	−0.09	−0.01	−0.09
	(0.08)	(0.07)	(0.13)	(0.11)	(0.06)	(0.06)	(0.09)	(0.06)
General misconduct	−0.82***	−0.70***	0.16	0.25	−0.65***	−0.59***	0.35*	0.26
	(0.18)	(0.13)	(0.23)	(0.18)	(0.16)	(0.12)	(0.15)	(0.10)
Sexual misconduct	−0.09	−0.24	0.18***	0.30	−0.10	−0.20	−10.16	−0.41
	(0.57)	(0.38)	(0.04)	(0.38)	(0.52)	(0.35)	(0.72)	(0.30)
Prior convictions	0.12*	0.09	0.20*	0.18	0.08	0.08	0.14*	0.19***
	(0.06)	(0.06)	(0.10)	(0.10)	(0.05)	(0.06)	(0.07)	(0.05)
Child victim	–	−0.37*	–	−0.11	–	−0.31*	–	−0.53**
		(0.16)		(24)		(0.14)		(0.19)
Intercept	.1.28***	0.38	−1.50***	−1.63***				
	(0.28)	(0.30)	(0.45)	(0.46)				
Cox and Snell R^2	0.09	0.05	0.02	0.01				
−2 Log Likelihood	2,480.59	2,281.58	1,207.43	1,189.58	9,895.97	7,544.96	4,610.09	9,599.21

Notes. Unstandardized coefficients presented with standard errors are in parenthesis.

*$p < .05$. **$p < .01$. ***$p < .001$.

likelihood of technical violations for the non–sex offender sample in the postintervention time period. In Missouri, sex offenders and non–sex offenders in the postrestriction sample were less likely to sustain a technical violation in the 2 years after release when compared with the prerestriction sample, and the differences were statistically significant (see Table 4). In supplementary models (not shown), z scores were calculated to evaluate the differences in coefficients by offense group (see Paternoster, Brame, Mazerolle, and Piquero, 1998). The differences between pre-restriction and post-restriction technical violations approached

statistical significance ($z = -1.41$).[14] This finding suggests that, in Missouri, there might have been less of a decline in the likelihood of technical violations in the postlegislative period for sex offenders than for non–sex offenders. No differences emerged in the models estimating reconviction. Finally, sex offenders with child victims were less likely to sustain a technical violation when compared with parolees with young adult or adult victims. Victim age was not statistically significant in the reconviction model.

Turning to the proportional hazard models, we explore how legislative changes influenced the time to failure among sexual and non–sexual offenders. In Michigan, sex offenders and non–sex offenders released in the postresidency restriction period were reconvicted more quickly than preintervention cohorts. Subsequent z-score analyses indicate that the effect of the intervention was not significantly different for sex and non–sex offenders. In addition, no significant differences emerged for the arrest or technical violation analyses among sex offenders and non–sex offenders. In Missouri, the time to technical violation increased in the postrestriction period for both sample groups, but the difference between groups was not statistically different. In addition, sex offenders were reconvicted less quickly in the postintervention period. Victim age was not statistically significant in any of the models estimated for Michigan. In Missouri, sex offenders with child victims failed less quickly, for a new conviction or technical violation, when compared with parolees with adolescent or adult victims.

Conclusion

Although all offenders have been affected by recent punitive policy mandates, changes in the philosophies of the criminal justice system have virtually separated the sexual offender from every other type of criminal (Edwards and Hensley, 2001). Despite sex offender residence restriction laws generating substantial attention in the media and public policy arenas, few empirical studies have evaluated the implementation of these laws or the efficacy of this legislation for public safety. This lack of research creates an important challenge for public policy. Tremendous monetary and resource costs are being allocated to the development and implementation of sex offender residency legislation. Yet, the effects are not understood, making the return on investment unclear.

The goal of the study is twofold. First, we documented the residency locations of sex offenders and non–sex offenders before and after the implementation of the residency restriction laws. The findings challenge the fundamental assumptions of the residency restriction laws. In the current study, residence restrictions did not significantly reduce the number of sex offenders who reside near schools or day care centers. Although there was a decline in the number of individuals living in restricted areas after the implementation of

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14. The models do not include the child victim measure. We did not observe significant variation across groups in the Michigan models.

the laws, less than one third of prerestriction sex offender and non–sex offenders lived in restricted areas before implementation of the laws.

Researchers have continued to document the difficulties in monitoring and enforcement of residence restrictions using samples of sex offenders within one or more counties. In Ohio, 31% to 45% of sex offenders were in violation of residence restrictions (Grubesic, Mack, and Murray, 2007). Roughly half of sex offenders maintained a residence within a restricted zone in Florida (Tewksbury and Mustaine, 2006) and Texas (Maghelal and Olivares, 2005), and noncompliance rates were even higher in New York (Berenson and Appelbaum, 2011). The enforcement of these laws becomes particularly difficult with the enactment of community-level restrictions, and several states have passed legislation that allows judges and local authorities to determine the size and nature of residence restrictions (Mancini et al., 2013).

Discretion to monitor and enforce residence restrictions varies by state, community, and community supervision officer. Individual agents will differ in their perspectives and approaches (Berenson and Appelbaum, 2011; Gies et al., 2012; Shaffer and Miethe, 2011). For instance, agents who support residence restriction legislation are more likely to enforce restrictions (Payne, Tewksbury, and Mustaine, 2013). The extent to which agents align with the culture and goals of their agency can certainly influence discretionary strategies of monitoring and enforcement (see Rudes, 2012; Rudes, Lerch, and Taxman, 2011). Partnerships and working relationships between law enforcement and community corrections agencies also can guide how residence placements and violations are determined. In all, it remains important to consider why restrictions are strongly or loosely enforced through the lens of those tasked with the responsibility of approving placements and enforcing the law.

Second, we examined the occurrence and timing of recidivism. Overall, the findings suggest that if residence restrictions have an effect on recidivism, then the relationship is small. In Michigan, sex offenders in the postrestriction group failed more often and did so more quickly than in the preintervention sex offender group. Sex offenders in the postrestriction group in Missouri were less likely to sustain a technical violation or a subsequent reconviction. Non–sex offenders had a similar decline in technical violations. The lack of strong effects, and variation across groups and sites, further suggests that the residence restriction legislation had an inconsistent impact on individual patterns of recidivism.

Unfortunately, we could not examine differences in rates of sexual recidivism events—the explicit target behaviors of residence restrictions—because of a small rate of occurrence. The small number of sexual recidivism events was insufficient to detect statistical significance. The low observed sexual recidivism rate could be a result of a combination of factors. First, although we used a large sample of sex offenders, the distribution of sex offenders with child victims (i.e., victim younger than 13 years of age) was relatively low compared with other analyses of sex offense recidivism. For instance, nearly 80% of the sample used by Veysey and Zgoba (2010) was composed of such offenders. Because sex offenders with

a history of sexual offending against children have relatively higher sex offense recidivism rates (Serin, Mailloux, and Malcolm, 2001), this aspect of our sample means that we have a depressed, but perhaps more representative, rate of overall sex offense recidivism.

The results also indicate that a longer period of follow-up is needed. Offenders were released at different times; therefore, there is a variable time at risk. We used Cox proportional hazard modeling to enhance the equivalence of follow-up period used for recidivism rates. The 2-year follow-up allowed sufficient time for technical violations, but other risks of recidivism manifest with longer periods of observation. Researchers have noted consistently that longer follow-up periods are necessary to provide valid indicators of the prevalence of sex offense recidivism (Soothill, 2010). Indeed, Prentky, Lee, Knight, and Cerce (1997) observed that 30% of their sex offense recidivism events occurred after 5 years at risk. In this sense, our follow-up period might not provide the best indicator of the long-term effectiveness of sex offender residence restrictions in reducing sex offense recidivism.

In addition to the general concerns raised with the recidivism analysis, two policy changes could have influenced the study results. As with any study of this type, there is the potential for history to threaten internal validity (Shadish et al., 2002). During the study period, both states implemented comprehensive programming that begins while in prison and continues throughout parole. Michigan implemented the Michigan Prisoner Reentry Initiative (MPRI) statewide in 2008 (Pew Center on the States, 2011), and an executive order was signed in Missouri establishing a statewide reentry process in September 2005. The states provide funding to collaborative partnerships that enhance ex-offender self-sufficiency, reduce recidivism, and improve public safety and community health. Similarly, Michigan and Missouri made efforts to reduce technical violation rates during this period under the guidance of the Pew Foundation (Pew Center on the States, 2011). Beginning in 2004, the number of overall technical violations in the state of Michigan increased and then declined substantially beginning in 2007. The Missouri prison population has remained relatively stable since 2005, but the number of individuals returned to prison for a technical violation has been reduced by approximately 12% between 2005 and 2009 (Pew Center on the States, 2011). In both states, the overall statewide decrease in technical violations was observed around the time of the implementation of sex offender residence restrictions. These changes in policies governing technical violations have likely influenced the evaluation outcomes.

Policy Implications

It is important to consider refinements to existing proposals to enact residence restrictions and modify current policies. First, it would be appropriate to reexamine the universal application of residence restrictions. Although the containment approach to sex offender management (English et al., 1997) and the risk–need–responsivity model (Andrews and Bonta, 1994) advocate individualization, in practice, sex offenders often are treated as a homogeneous, high-risk group (Lynch, 1998). It is not appropriate to expect residence

restrictions to have a general effect on all sex offenders. By casting a wide net, anticipated effects of such policies are weakened partly by the mixture of sex offender subpopulations and risk levels. As this research has indicated, less than one third of sex offenders in the study states committed an offense against a child victim. A targeted residence restriction policy that narrows the scope to offenders with child victims has more potential, but it has not been substantiated empirically. In one assessment, Rydberg, Grommon, Huebner, and Bynum (2014) found that sex offenders with an offense conviction against a child victim experienced more residence mobility than other contact sex offenders in preresidency and postresidency restriction time periods. The challenge to the development of a targeted policy arises in how to identify a small group of offenders who specialize in child molestation and differentiate this population from the versatility observed among those convicted of sexual offenses (Harris, Knight, Smallbone, and Dennison, 2011; Harris, Mazerolle, and Knight, 2009). Even with knowledge of offense specialization, little research has examined systematically whether repeat child molesters use consistent strategies to gain access to victims (Leclerc, Proulx, and Beauregard, 2009). Finally, it is not clear whether child predators change their modus operandi after detection by law enforcement, given that residence restrictions can only be applied to known sex offenders.

Risk assessment and classification protocols can assist in the identification process. Several instruments are available for use among correctional populations, many of which can be completed in a short period of time using only information from file reviews (Struder, Aylwin, Sribney, and Reddon, 2012). Recent evidence from meta-analyses suggests that the Static-99, the Static-2002, and the MnSOST-R are among the best supported actuarial instruments for predicting sexual recidivism (Hanson and Morton-Bougon, 2009). The Static-99 seems to be particularly accurate when applied to the long-term sexual recidivism of child molesters, as opposed to sex offenders with exclusively adult victims (Parent, Guay, and Knight, 2011). Indeed, most sex offender recidivism risk assessments are more accurate when used with those offenders with exclusively child victims and are less useful for the prediction of behavior among individuals with adult victims (Parent et al., 2011). STABLE-2007 and ACUTE-2007 can be integrated to measure dynamic risk factors (i.e., access to victims, sexual preoccupations, and collapse of social supports) and to monitor changes in risk over time (Hanson, Harris, Scott, and Helmus, 2007; McGrath, Cumming, Burchard, Zeoli, and Ellerby, 2010).

Actuarial instruments are not without problems. Any risk score generated compares specific cases with members of the development sample, and these comparisons can be imperfect and ignore facts that might be unique to that particular offender (Struder et al., 2012). For instance, the Sex Offender Registration and Notification Act (SORNA, 2006) mandates a tiered structure for the classification of sex offenders. Zgoba et al. (2012) observed that for a multistate sample of sex offenders, Tier 2 SORNA offenders tended to have higher average Static-99R scores than Tier 3 offenders (i.e., the highest SORNA Tier). Because such instruments are applicable to child molesters and feasibly exclude a

subpopulation of low-risk sex offenders, a triangulated use of instruments can provide a starting point in the identification and differentiation of sex offenders to be subjected to residence restrictions.

Prerelease planning committees could be formed to determine whether residence restrictions should be applied to specific cases or a particular offender; this type of model has been used with sex offender community notification programs (see Duwe, 2013; Duwe et al., 2008). Residence placement review and residence restriction decision committees can consist of treatment providers within a correctional facility and those used in the local community, law enforcement officials, sex offender case workers, victim services professionals, and members of the community. With the use of these partnerships, individualized risk and need determinations can be made and residence restrictions can be applied as an additional layer of risk mitigation rather than as a uniform policy.

Also, we need to determine how restrictions should be used. It might be useful to evaluate the length of residence restrictions. In the states studied, residence restrictions are enforced for the entire term of community supervision, a minimum of 2 years, and legislation mandates lifetime supervision for certain classes of sex offenders. An alternative approach would be to use time-ordered residence restrictions. This strategy can allow for enforcement of restrictions after release and the reduction in the size of boundary restrictions with continued compliance with supervision terms. Informed by signaling perspective (Bushway and Apel, 2012), this approach can help to reallocate operational resources to those offenders who display the need for more intensive supervision.

Second, it would be advantageous to practitioners, offenders, and communities to reframe the focus of sex offender management to sex offender reentry. As noted, researchers have argued that residence restrictions can undermine the reentry process. As a result, innovation is needed to develop, expand, or reallocate resources to assist with the unique transitional experience of sex offenders. Tewksbury and Copes (2012) suggested that sex offenders are poorly informed about residence restrictions prior to release, which leaves many to manage reentry with little planning or few viable solutions. In a survey of supervision agents who manage high-risk sex offender caseloads with global positioning system (GPS), Gies et al. (2012) observed that only 40% of agents mention inclusionary and exclusionary zones during their first meeting. The challenge with interventions of this type is that resources are rarely allocated to cover the costs of the intervention. Zevitz and Farkas (2000), in their study of sex offender community notification in Wisconsin, found that the new policy increased the workload of caseworkers. The need for training and support was particularly acute for agents in rural areas where it was difficult to employ a sex offender–specific intensive supervision agent. Increasing training and support for local parole officers could decrease the numbers of individuals residing in restricted areas. Additional staffing could be used to help demystify the reentry process and educate sex offenders of what they will face in the community postrelease.

Finally, housing can be a risk and protective factor for sex offender recidivism (Willis and Grace, 2008). Residence restrictions limit the available housing stock of a community. Yet, offenders supervised under the restrictions must maintain a residence. Shared living arrangements, which blend halfway housing and therapeutic community services, have been one approach to assist with housing (Colorado Department of Public Safety, 2004). In this model, offenders live together, receive direct treatment services, are monitored informally by their fellow roommates, and are monitored formally by periodic visits from a supervision agent. Unfortunately, little research has been done to evaluate the efficacy of these programs. As concerning is the inability of this approach to meet demand; only a small population of offenders can be placed in such programs. This result might partially explain why sex offenders perceive halfway houses as being inaccessible (Tewksbury and Copes, 2012). Housing services specifically for sex offenders need to be expanded and evaluated for their effectiveness.

In conclusion, this research adds to a growing body of knowledge that cautions the expansion of residence restriction legislation (Levenson and Hern, 2007; Nobles et al., 2012). Rigorous and mixed methodological research across multiple levels of analysis is needed to understand the various processes that influence the application and outcomes of residency restriction legislation in states and municipalities. This is particularly important given an evaluation of laws in one state might not generalize to another state given the breadth and diversity of the laws (Mancini et al., 2013). Considering the heightened public concern regarding sex offenders, complete removal of legislation regarding residence restrictions would be not only difficult but also unwise. Rather, like other correctional interventions of this type, efforts should be focused on high-risk and high-need sex offenders. Without attention to strategic development of legislation, policy makers should not expect much short-term positive benefit from residence restrictions. Even with a narrowed focus to sex offenders with child victims, this research suggests that residence restrictions might not accomplish the aims it hopes to achieve. Ideally, policy makers should continue to explore improvements to sex offender management that control recidivism risk while promoting successful reentry. Such alternatives must be pursued because evidence for the effectiveness of these approaches is building slowly, whereas the same cannot be said for residence restrictions. Investment in evidence-based approaches is critical.

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APPENDIX

TABLE A

Michigan: Mean Balance Across Sex Offenders and Non–Sex Offenders

Variables	Prematched			Matched			Unmatched		
	NSO	SO	Bias	NSO	SO	Bias	NSO	SO	Bias
Number of Cases	1,953	1,981		1,703	1,703		250	278	
Propensity Score	0.49	0.52	30.04	0.50	0.50	5.58	0.40	0.60	257.56
Observed Covariates									
Age	35.32	36.80	13.51	35.89	35.43	4.39	31.42	45.15	117.71
Female	0.04	0.03	5.35	0.03	0.03	0.32	0.06	0.04	9.34
Non-White	0.30	0.31	2.17	0.31	0.29	3.07	0.23	0.41	38.76
Marital status—single	0.55	0.54	2.01	0.55	0.55	0.83	0.57	0.50	13.64
Marital status—married	0.18	0.21	7.61	0.19	0.19	0.89	0.08	0.31	62.15
Marital status—divorced/widowed	0.27	0.25	4.55	0.26	0.26	1.74	0.36	0.20	35.36
HS/GED	0.58	0.57	0.63	0.59	0.57	1.75	0.52	0.57	7.19
Prior juvenile commitment	0.06	0.05	4.45	0.05	0.05	2.45	0.14	0.03	41.38
Prior juvenile probation	0.10	0.08	7.01	0.08	0.08	0.43	0.23	0.06	49.04
Prior juvenile escapes	0.01	0.01	0.00	0.01	0.01	0.69	0.01	0.01	1.76
Prior adult jail term	0.24	0.19	12.18	0.21	0.20	0.73	0.46	0.12	81.34
Prior adult probation	0.27	0.21	14.12	0.22	0.22	0.42	0.57	0.13	103.92
Prior adult escapes	0.02	0.01	8.77	0.01	0.01	4.47	0.05	0.04	5.84
Prior misdemeanors	0.26	0.20	14.28	0.22	0.22	0.14	0.57	0.08	123.23
Prior felonies	0.23	0.20	7.31	0.21	0.20	2.04	0.40	0.17	51.77
Prior prison term	0.16	0.18	5.34	0.17	0.16	2.88	0.14	0.29	36.93

T A B L E   B

Missouri: Mean Balance Across Sex Offenders and Non–Sex Offenders

Variables	Prematched			Matched		
	NSO	SO	Bias	NSO	SO	Bias
Number of Cases	15,846	5,997	–	2,224	2,224	–
Propensity Score	0.08	0.10	12.65	0.07	0.07	0.00
Observed Covariates						
Age	45.16	46.72	14.27	40.27	40.16	1.07
Female	0.13	0.07	20.58	0.13	0.12	2.95
Non-White	0.31	0.25	13.25	0.24	0.24	0.00
Marital status—single	0.43	0.42	2.02	0.47	0.47	0.00
Marital status—married	0.26	0.24	4.65	0.24	0.25	2.32
Marital status—divorced/widowed	0.31	0.34	6.47	0.29	0.28	2.21
HS/GED	0.64	0.71	14.86	0.67	0.64	6.26
Prior convictions	1.20	1.06	28.11	1.93	1.89	4.56
Prior felony conviction	0.85	0.81	10.69	0.94	0.92	7.84
Prior prison term	0.37	0.50	29.59	0.43	0.50	9.63