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The effectiveness of Sex Offender Registration and Notification: A meta-analysis of 25 years of findings

Kristen M. Zgoba[1] · Meghan M. Mitchell[2]

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Abstract

Objectives Examine 25 years of Sex Offender Registration and Notification (SORN) evaluations and their effects on recidivism.
Methods We rely on methodology guidelines established by the Campbell Collaboration for meta-analyses to systematically synthesize results from 18 research articles including 474,640 formerly incarcerated individuals. We estimate the effect of SORN policies on recidivism from 42 effect sizes and determine if the effect of SORN varies by sexual or non-sexual recidivism when examining arrest or conviction as outcomes.
Results The random-effects meta-analysis model demonstrated that SORN does not have a statistically significant impact on recidivism. This null effect exists when examining a combined model and when disaggregating studies by sexual or non-sexual offenses, or conceptualizing recidivism by arrest or conviction.
Conclusions SORN policies demonstrate no effect on recidivism. This finding holds important policy implications given the extensive adoption and net-widening of penalties related to SORN.

Keywords Evaluation · Megan's Law · Meta-analysis · Sex Offender Registry and Notification · SORN · RSOs

✉ Kristen M. Zgoba
kzgoba@fiu.edu

Meghan M. Mitchell
mmitchell@ucf.edu

[1] Department of Criminology and Criminal Justice, Florida International University, 11200 SW 8th Street, Miami, FL 33199, USA

[2] Department of Criminal Justice, University of Central Florida, 12805, Pegasus Dr, Florida, FL 32816, USA

Introduction

Official statistics on sexual assault indicate approximately 500,000 assault incidents take place a year (Bierie & Davis-Siegel, 2015; NCJA-SOMAPI, 2017), although crime victimization surveys hint at higher numbers (Morgan & Truman, 2020). As a result of these heightened frequencies, and in the absence of empirical validation, policies governing individuals convicted of sexual offenses have been established (NCJA-SOMAPI, 2017). The importance of and reliance upon these policies, known as Sex Offender Registration and Notification (SORN) laws, has been firmly established in the American criminal justice system over the last two and a half decades. These laws have been found to be widely supported by the public, law enforcement, policymakers, and sometimes even those convicted of sexual offenses (Bierie, 2016; Levenson, D'Amora, & Hern, 2007a; NCJA-SOMAPI, 2017; Tewksbury & Lees, 2007). Whether through media prominence, political touting, or community sentiment, it is clear that SORN laws are a mainstay in American culture.

As the year 2021 marks the 25th anniversary of the federal passage of Megan's Law, the past two and a half decades have been witness to numerous modifications and additions to the law (Call, 2018; Lobanov-Rostovsy, 2015; Zgoba et al., 2018). While the objective behind SORN policies is to both provide law enforcement a list of registered sex offenders (RSOs) for investigation and to make the public aware of an individual registrant's address, SORN laws have spurred the growth of other sexual offense policies. These variations have included laws that focused on registrant residence restrictions, civil commitment, polygraph, internet activity, GPS tracking, and international travel (Lobanov-Rostovsy, 2015). In addition to the spill-over effect of these laws into other areas of registrants' lives, registries have grown in size and duration of the registration period. The most recent numbers available from the National Center for Missing and Exploited Children (NCMEC) indicated that over 935,000 people were registered and living in the USA (Bierie, n.d.)[1]. The escalation of laws and penalties focusing on sexual offenses, as well as the growing number of individuals placed on registries, highlights the recognition that SORN policies are likely here to stay.

As time passes and SORN laws continue to expand in breadth and net-widen to include more registrants for longer periods of time (Harris et al., 2020a), it becomes imperative to examine the comprehensive effectiveness of the law. Although informative, the extant literature lacks a singular defining trend or consensus on its efficacy (Zgoba et al., 2018). Despite widespread adoption, studies have not consistently demonstrated that SORN is associated with a decrease in recidivism or an increase in public safety (Bierie, 2016; NCJA-SOMAPI, 2017), which leads many to wonder, have we put the cart before the horse? Moreover, no individual study exists that combines the statistical effects and results of numerous studies into one comprehensive examination of the overall trend of SORN. This study aims to fill this gap in both the literature framework and the empirical analyses through a meta-analysis of 25 years of independent studies that examine the effectiveness of SORN policies. While Call

[1] This number was released via personal communication with Dr. David Bierie from the US Marshals Service. NCMEC data were utilized to create this count and the USMS approved the release and publication of the information.

(2018) recently published a systematic review, and Pawson (2002) conducted one approximately 20 years ago, the current study represents the only meta-analysis of SORN effectiveness on recidivism.

History of Sex Offender Registration and Notification legislation

During the preceding 25 years, states, as well as the federal government, have continuously modified and, in many cases, strengthened sex offense legislation as it pertained to the practice of registering individuals and notifying the public of their whereabouts (Harris et al., 2020a). While registration and notification are separate constructs, collectively, they are known as sex offender registration and notification laws. Since 1996, they have functioned in tandem as one law (i.e., Megan's Law and SORN). Registration refers to the statutory requirement that upon release, individuals convicted of sex offenses register their personal information with local law enforcement authorities, while notification refers to the process by which the public is informed of the released individual's address and personal information. While intrinsically linked, the two policies have been born from separate objectives—sex offender registration was intended to facilitate police apprehension of recidivists by maintaining a pool of potential suspects, as well as the deterrence of subsequent offending due to increased scrutiny. In contrast, the objective of notification was to improve the community's ability to protect themselves from RSOs, through altered personal behavior or reports to law enforcement regarding suspicious behavior by registrants (Matson & Lieb, 1997; Ragusa-Salerno & Zgoba, 2012).

While much of the focus has been on recent legislation for sexual offenses, original versions of the law were adopted many decades before, as California became one of the first states in 1947 to develop a sex offense registry (Call, 2018; Logan, 1999; Logan, 2009). Following a number of publicized sex crimes, Washington state gained national attention in 1990 for implementation of a contemporary version of the law known as the Community Protection Act (Matson & Lieb, 1997). Shortly thereafter in 1994, registration was extended to all 50 states when the Wetterling Act was enacted into federal law through the Title XVII of the Violent Crime Control and Law Enforcement Act (Logan, 2009). The Wetterling Act required that by 1997 all states establish a sex offense registry or risk losing federal criminal justice funding. Subsequently, the 1994 murder of Megan Kanka occurred a few months prior to the enactment of the Wetterling Act and changed the content of the original version of the Act to include a community notification requirement (Logan, 2009). One month later in October 1994, New Jersey enacted the Registration and Community Notification Law, more commonly known as "Megan's Law" (Petteruti & Walsh, 2008; Zgoba et al., 2018). In 1996, the federal version of Megan's Law was later endorsed as an amendment to the Wetterling Act (Kabat, 1998). As such, from 1996 to the present, registration and notification policies have existed in combination as one law.

As it was written, the federal law did not mandate standardization of implementation and states took the liberty to adopt individualized provisions for notifying the public about those they deemed to carry the most risk. As this occurred, the notification stipulations evolved in a variety of ways, for instance, producing different procedures for the tiering of RSOs or implementing broad notification procedures without a distinction between high and low risk (Chajewski & Mercado, 2009; Levenson,

Brannon, et al., 2007b; Lobanov-Rostovsy, 2015; Vasquez et al., 2008; Zgoba et al., 2016; Zgoba et al., 2018). These variations in SORN procedures among the states catalyzed Congress to enact the Adam Walsh Child Protection and Safety Act (AWA) (see Logan, 2009, for a more thorough discussion). The Adam Walsh Act sought to standardize SORN provisions with the objective of creating a widespread national system for the registration and notification of persons convicted of a sexual crime or a crime against a child (Zgoba et al., 2016). However, since the AWA functions similarly to the Wetterling Act by withholding funding to states that do not implement, currently only 17 states and three US territories have successfully implemented the provisions set forth by the AWA (Harris & Lobanov-Rostovsky, 2010; Harris et al., 2020b; Petteruti & Walsh, 2008; Zgoba et al., 2018).

Research exploring the efficacy of SORN

Research exploring the efficacy of SORN policies has focused primarily on individual states, with a small number of studies examining multiple state effects (NCJA-SOMAPI, 2017). Within this research, with limited exceptions (Barnoski, 2005; Bierie, 2016; Duwe & Donnay, 2008; Freeman, 2012), little evidence of a SORN impact has been found for either first-time sexual offending or reoffending (Adkins et al., 2000; Agan, 2011; Maddan et al., 2011; Sandler et al., 2008; Schram & Milloy, 1995; Vasquez et al., 2008; Zgoba et al., 2008; Zgoba et al., 2010; Zgoba et al., 2018).

One of the first examinations of the effectiveness of SORN legislation was published by Schram and Milloy (1995, prior to federal passage of the law). They used a sample of 180 high-risk individuals in Washington State who were either subjected or not to community notification under Washington's Community Protection Act. The authors determined that individuals subjected to community notification were rearrested more quickly for new sexual offenses, compared with those offenders who were not subject to notification; however, overall recidivism rates between the two groups did not differ. Similar state studies in New Jersey and Iowa also found negligible results associated with SORN (Adkins et al., 2000; Tewksbury et al., 2012; Zgoba et al., 2008; Zgoba et al., 2010), while a time series analysis in New York found that 95% of all sexual offense arrests were committed by first-time offenders (Sandler et al., 2008).

Adkins et al. (2000) examined the effects of registration in Iowa. A preregistration group (n = 201) and a post-registration group (n = 233) comparison indicated that sex offense registration appeared to have varied effects on recidivism rates over the follow-up period of 4.3 years. Sex offense recidivism was low at 3% for the registry sample and 3.5% for the pre-registry sample. Of those who were convicted of sex offenses, the registry sample had a lower volume of recidivism per person than the pre-registry sample; however, the differences in recidivism were not found to be statistically significant. Similarly, researchers in New Jersey followed a sample of 550 individuals released both pre- and post–SORN implementation for an average period of approximately 7 years (Zgoba et al., 2010). The authors determined that multiple measures of recidivism, in addition to community tenure (the amount of time spent in the community before a rearrest) and harm reduction (decreased number of victims), were not significantly different between the cohorts. Building upon these results, Tewksbury et al. (2012) created matched samples of sex offenders in New Jersey released pre- and post–Megan's Law (N = 495). The groups were matched on a number of variables

theoretically linked to recidivism including age, race, education, family history, marriage, employment, substance use, mental health issues, sexual offense characteristics, and criminal history. The timeframe was extended to allow for an 8-year post-release examination. No significant differences were noted between the groups for both general and sexual reoffending. Sandler et al. (2008) utilized a time series analysis to examine differences in sexual offense arrest rates before and after the enactment of New York State's Sex Offender Registration Act. The authors found no support for the effectiveness of SORN laws in reducing sexual offending by rapists, child molesters, sexual recidivists, or first-time sex offenders. In fact, the authors discovered that over 95% of all sexual offense arrests were first-time offenses, thereby creating uncertainty that SORN is an effective method to target those who repeatedly commit sexual offenses.

A study by Petrosino and Petrosino (1999) approached an evaluation of SORN from a slightly different angle by determining how many repeat sex crimes may potentially have been avoided if SORN was in effect in Massachusetts. The authors examined criminal records of "sexual psychopaths" ($N = 136$) and found that 27% of the sample had a prior conviction that met the requirements of the Massachusetts Registry Law before their most recent sex crime. The authors conclude and that a small number of cases would have potentially been prevented if SORN was in effect. Similarly, Levenson and Zgoba (2016) examined the rate of repeat sex crime arrests in Florida using aggregate data for the period 1990 to 2010. The average annual sexual repeat arrest rate prior to and after the implementation of sexual offender registration laws in 1997 was 4.9% and 7.5%, respectively, indicating a statistically significant increase post–SORN implementation.

Similar to the discussed quasi-experimental analyses, time series analyses of sexual offending rates pre- and post–SORN enactment also find limited to no evidence of effectiveness. Vasquez et al. (2008) compared rates of forcible rape in 10 states. One of the particular strengths of this study was the inclusion of the multiple states and the increased ability to generalize beyond a single state. Overall, the authors concluded that SORN had no influence on the number of rapes reported after the law was implemented, although several states demonstrated a non-significant increase in the number of rapes, a significant reduction in rates was only found in three states studied. Comparably, Agan (2011) used the Bureau of Justice Statistics data from 15 states that contained information on the subsequent arrests of previously convicted sex offenders released from prison in 1994. The results yield small, insignificant differences in recidivism for those required to register and those who were not, causing the researcher to conclude that registration does not appear to reduce reoffending rates among RSOs.

Although the majority of prior research has found the effectiveness of SORN to be futile in reducing sexual recidivism, of note are the few studies that have found limited positive outcomes associated with SORN. For example, Barnoski (2005) examined the criminal histories of 8359 individuals released before and after the passage of Washington state's SORN policy using three measures of recidivism. The author discovered that both violent and sexual felony recidivism decreased since the passage of the 1997 SORN statute; however, he notes that a causal link cannot be determined as other factors, such as decreasing crime rates and increasing incarceration, can be a contributing factor. Zgoba et al. (2008) discovered a similar finding in New Jersey, where sexual offense rates steadily decreased after the implementation of SORN;

however, the finding cannot be isolated to SORN alone, as all crime rates decreased during similar years. That same year, Duwe and Donnay (2008) compared three groups of people in Minnesota: high-risk previously convicted sex offenders released with SORN provisions, individuals released prior to implementation of SORN, and RSOs released after SORN implementation, but not subject to community notification. Although a comparison of the notification group and the prenotification group suggested a reduction in general and non-sexual recidivism, there was no reduction in recidivism for the notification and non-notification group. The authors conclude that community notification yielded a specific deterrent effect for those offenders registered under its provisions only.

A small number of studies have also documented different outcomes associated with the law. A subsequent analysis of more than 300,000 sex offenses in 15 states found that registration reduced the *frequency* of sex offenses because it provided law enforcement with information on local sex offenders (Prescott & Rockoff, 2011). Similar to Duwe and Donnay (2008), these results demonstrated a narrow effect—a decrease in crime concentrated among "local" victims (e.g., friends, acquaintances, neighbors), with little evidence of a decrease in crimes against strangers. Conversely, Freeman (2012) found that notification status had an effect on recidivism, as well as a potential positive benefit from SORN's effects on timing to rearrest in New York. Among each of the samples examined, individuals were rearrested for non-sexual offenses *more quickly* after the implementation of SORN policies.

Current study

Given the widespread adoption and use of SORN policies and variation that exists between studies, the present study seeks to examine the comprehensive effectiveness of SORN through standardization of findings. Although the research reviewed above is informative, it lacks a singular defining theme or a consensus among the findings. Currently, no individual study exists that synthesizes the statistical effects and results of numerous studies into one inclusive examination on the overall trend of SORN. This study fills this gap through a meta-analysis using a number of independent studies that examine the effectiveness of SORN policies. Accordingly, this study represents the first meta-analysis of SORN effectiveness[2].

Methods

A meta-analysis is conducted to examine the effect of SORN policies on recidivism and to systematically synthesize results from numerous research articles. This approach is well recognized for providing a "transparent, objective, and replicable framework" for systematic and quantitative reviews (Borenstein et al., 2009, p. xxiii). To do so, we rely

[2] It is important to note that, while important, a small number of the studies outlined in the research review are not included in the meta-analysis due to expiration of data storage rules and data output reporting styles that inhibited calculation of an effect size. Efforts were made with many authors to retrieve all relevant data for inclusion in the current study. Some of the studies not included in the analysis are Adkins et al., (2000); Petrosino and Petrosino (1999); Sandler et al., (2008); Schram and Milloy (1995); Vasquez et al., (2008); and Zgoba et al., (2010).

on methodology guidelines established by the Campbell Collaboration (for review see Campbell Collaboration, n.d.).

Inclusion and exclusion criteria

To examine the effect of SORN on recidivism, several criteria were used to gather eligible studies. First, the target population was limited to adults who had been incarcerated for a sexual offense and released from a correctional facility—this included both prisons and jails[3]. As juveniles are treated differently from adults in the criminal justice system and subjected to varying forms of registration and notification, studies examining their recidivism were excluded from this analysis. Second, the outcome variable had to measure "recidivism" as indicated by failure to register[4], revocation, arrest, charges, conviction, or incarceration. Third, searches were limited to published journal articles, books, dissertations, technical reports, and other grey literature. Fourth, eligible studies had to provide the common statistics or raw data necessary for the calculation of effect sizes. It is important to note that studies were not excluded based on the rigor of their methodological design. Fifth, research articles were limited to those written in the English language. Sixth and finally, publication dates were limited from 1996 to 2020, as the federal version of SORN was implemented in 1996. Studies were excluded if they did not include the relevant statistics for inclusion (authors were contacted to provide statistical data for inclusion) and if they did not meet the inclusion criteria.

Strategies for searching the literature

Systematic procedures were used to conduct an exhaustive search of studies while limiting bias. Keywords were searched using Google Scholar. The keyword Boolean phrase used was "sex offender registration and notification" OR "sexual offender registration and notification" AND registration OR sex offender registration OR Megan's Law OR Adam Walsh Act OR prisoner OR adult prisoner OR offender OR incarceration OR inmate OR corrections OR sex offender NOT juvenile. Although historically other databases were relied upon for meta-analyses, Google Scholar has become remarkedly robust and commonly relied upon by scholars as a comprehensive resource. We used this database for a number of reasons (Campbell, 2010; Haddaway et al., 2015; Kendall, 2020; Krug, 2020; Merguerian, 2020). First, it has a robust ability to search grey literature including dissertations/theses, conference proceedings, books, and reports in addition to research articles. Second, Google Scholar searches the full text of articles for keywords as opposed to simply searching the title, abstract, and tagging information. Third, the database searches across all disciplines; hence, it is not limited to specific areas of inquiry. Lastly, the materials that appear on the search

[3] No exclusions were made solely based on this category. Meaning, no study was excluded because they had studied parole or probation released offenders in exclusion. Parolees were included in this analysis because in many states sex offenders are held on parole supervision for life (PSL) or community supervision for life (CSL) after release from prison. Individuals who received only probation would not have been; however, no studies were excluded for this reason.

[4] FTR is a felony in many states and with the federal government. It was not included in any recidivism analyses, however, because the studies were excluded for various other reasons.

engine are curated by a machine and algorithm as opposed to journals selected by humans.

The first round of the online keyword searches was conducted between August 24, 2020, and August 25, 2020. These searches produced 1280 related publications, which were later reduced to 228 after removing unrelated topics based on a review of the title and summary from Google Scholar. Of this number, only six duplicates existed and those were removed resulting in an article pool of 222. Following a review of the abstracts, only 40 relevant articles remained. A review of article references generated an additional 24 articles; however, 22 articles were removed after a careful review of their methods, resulting in 42 articles. After thorough examinations of each publication by both authors, 18 articles contained the appropriate inclusion criteria necessary for the meta-analysis.

It should also be noted that the authors utilized their extensive foundation of colleagues who work in legal practices, clinical and therapeutic offices, and research institutions to provide suggestions of studies that may not have been on the list originally (e.g., file drawer papers). Furthermore, references were searched and published authors were contacted to review the sample and ensure that studies were not missed.

Coding procedures

To determine the effect of SORN policies on recidivism, 18 studies were manually coded for the effect sizes and outcome variables.[5] To examine variations, two outcome variables were categorically coded and included in this analysis. Public concern surrounding sex offense recidivism is centered on a person's likelihood to reoffend for another sexual offense (Przybylski, 2015); consequently, an *offense type* variable was created to measure sexual and non-sexual recidivism. Additionally, some studies combined both types of offenses; hence, a combined indicator for both was created. Some forms of recidivism may be more or less likely among individuals (Hepburn & Griffin, 2004); thus, an indicator of the *type of recidivism* (i.e., revocation, arrest, charge, convicted, reincarcerated, or multiple indicators, which could include two or more of the aforementioned outcomes) was also used.

Effect size coding and meta-analytic strategy

In the current study, we first calculated the mean effect of SORN on recidivism based on 18 studies. Overall, 42 effect sizes were available throughout the studies because some studies had multiple dependent variables. For example, a study could have a

[5] Meta-analysis procedures were originally developed to demonstrate a bivariate and isolated effect of a treatment on an outcome. These effects are ideal in experimental and quasi-experimental designs; however, those study designs are not always feasible in social sciences which has resulted in researchers relying on multivariate designs to control for confounding effects. Within multivariate analyses, controls may be operationalized or measured differently resulting in an increase in bias and inability to compare controls consistently across studies (Borenstein et al., 2009). Due to this difference in statistics and operationalizations, multi-variate effects were not included in this meta-analysis. These studies include Ackerman et al., 2012; Freeman & Sandler, 2010; Maurelli & Ronan, 2013; Park et al., 2014; Vasquez et al., 2008; and Zgoba et al., 2018.

unique effect for rearrest for any charge, rearrest for a sex offense, and conviction of a sex offense. Within this example, this one study could provide three effects.[6] Although substantial heterogeneity existed in offense type and type of recidivism, those variations existed *within*-study and did not commonly exist *between* studies. If the heterogeneity would have been between studies, moderator analyses would have been ideal; however, a sufficient number of studies did not exist within each offense type and type of recidivism to accurately examine each outcome and its moderating effect. For example, we could only examine the moderating effects for type of recidivism for arrest, conviction, and charge—with limitations when examining conviction and charge because they only have a *k* of 3 and 2 respectively—but we cannot include multiple types of recidivism or reincarceration because both of those only had a *k* of 1. Thus, any attempt at moderation would be incomplete. Additionally, when conducting meta-analyses, it is important not to include multiple effects from one study within one meta-analysis because those effects will be highly correlated with one another. Moreover, a multilevel approach is not advisable given the small number of level 2 clusters (i.e., $k = 18$) and the small number of effect sizes within each of those clusters (i.e., n = 42 effect sizes). Thus, to examine variation in study outcomes, we instead conducted four additional meta-analyses to determine SORN effects based on certain outcomes. All analyses were conducted using The Comprehensive Meta-Analysis Program. Random-effects models were estimated for all models.[7] These models were utilized because it is theoretically unlikely that *all* studies included in the analysis are identical, due to their variation in sample size, follow-up period, and recidivism measures. Moreover, this type of study is useful when effect sizes are drawn from a subset of the population in examination (Borenstein et al., 2009). Based on this fluidity, it is not viable to assume that the true effect size is consistent for the entire sample of studies. Moreover, the I^2 statistic indicates that a high degree of heterogeneity exists between studies.[8]

Results

As reported in Table 1, 18 studies and effect sizes were included in the analysis, and these studies included a total of 474,640 offenders who were subjected to SORN and had their recidivism tracked longitudinally. Regarding study specifics for the combined effects ($N = 18$), six studies measured sexual recidivism, one examined non-sexual recidivism, and 11 had a combined recidivism measure of both sexual and non-sexual

[6] In instances where multiple effects existed, if the authors could combine those effects to create an combined study effect, they did so. In studies where effects could not be combined, the authors chose one effect for the main model and noted that in Table 1.

[7] The authors recognize that when the number of studies is small, it is difficult to properly apply the random-effects model given the large variability in effect size (Borenstein et al., 2009). Thus, we also provide the fixed-effects for each model although we feel confident that the random-effects models are most appropriate for our data given theoretical and statistical rationales.

[8] Heterogeneity demonstrates the variation that exists in the true effect size underlying a certain population. This is essentially the effect that would exist for an infinite number of cases (Borenstein et al., 2009). I2 statistics are interpreted as a ratio and demonstrate inconsistency across studies by determining the extent to which confidence intervals overlap—determining the true effect underlying studies (Higgins et al., 2003; Higgins & Thompson, 2002).

Table 1 Primary study characteristics, random-effects sizes, and confidence intervals for 18 SORN and recidivism studies (N = 474,640)

Study	Offense type	Recidivism type	Sample size	Odds ratio	95% CI
Adkins et al., 2000	Both	Multiple	434	0.648	[0.426–0.984]
Agan, 2011	Both	Arrest	9623	1.141	[1.051–1.239]
Barnoski, 2005	Both	Conviction	8359	1.288	[1.169–1.418]
Bouffard & Askew, 2019	Sexual	Charge	424	0.182	[0.126–0.263]
Carr, 2015	Both	Conviction	2005	0.846	[0.647–1.106]
Cohen & Spidell, 2016	Both	Arrest	93,524	0.463	[0.426–0.504]
Duwe & Donnay, 2008	Both	Arrest	310	0.559	[0.351–0.890]
Freeman, 2012	Both	Arrest	17,165	0.641	[0.602–0.682]
Fundack, 2019	Sexual	Arrest	1985	25.503	[20.588–31.591]
Letourneau et al., 2010	Sexual	Charge	6064	1.149	[1.048–1.258]
Levenson & Zgoba, 2016	Sexual	Arrest	180	1.964	[1.149–3.355]
Maddan et al., 2011	Both	Arrest	2920	0.544	[0.458–0.647]
Prescott & Rockoff, 2011	Sexual	Arrest	328,260	1.499	[0.737–3.048]
Tewksbury & Jennings, 2010	Sexual	Conviction	1582	1.034	[0.864–1.236]
Tewksbury et al., 2012	Both	Arrest	495	0.754	[0.525–1.083]
Zevitz, 2006	Both	Reincarceration	213	0.982	[0.514–1.876]
Zgoba et al., 2018	Non-sexual	Arrest	547	0.802	[0.495–1.301]
Zgoba et al., 2008	Both	Arrest	550	0.716	[0.511–1.004]

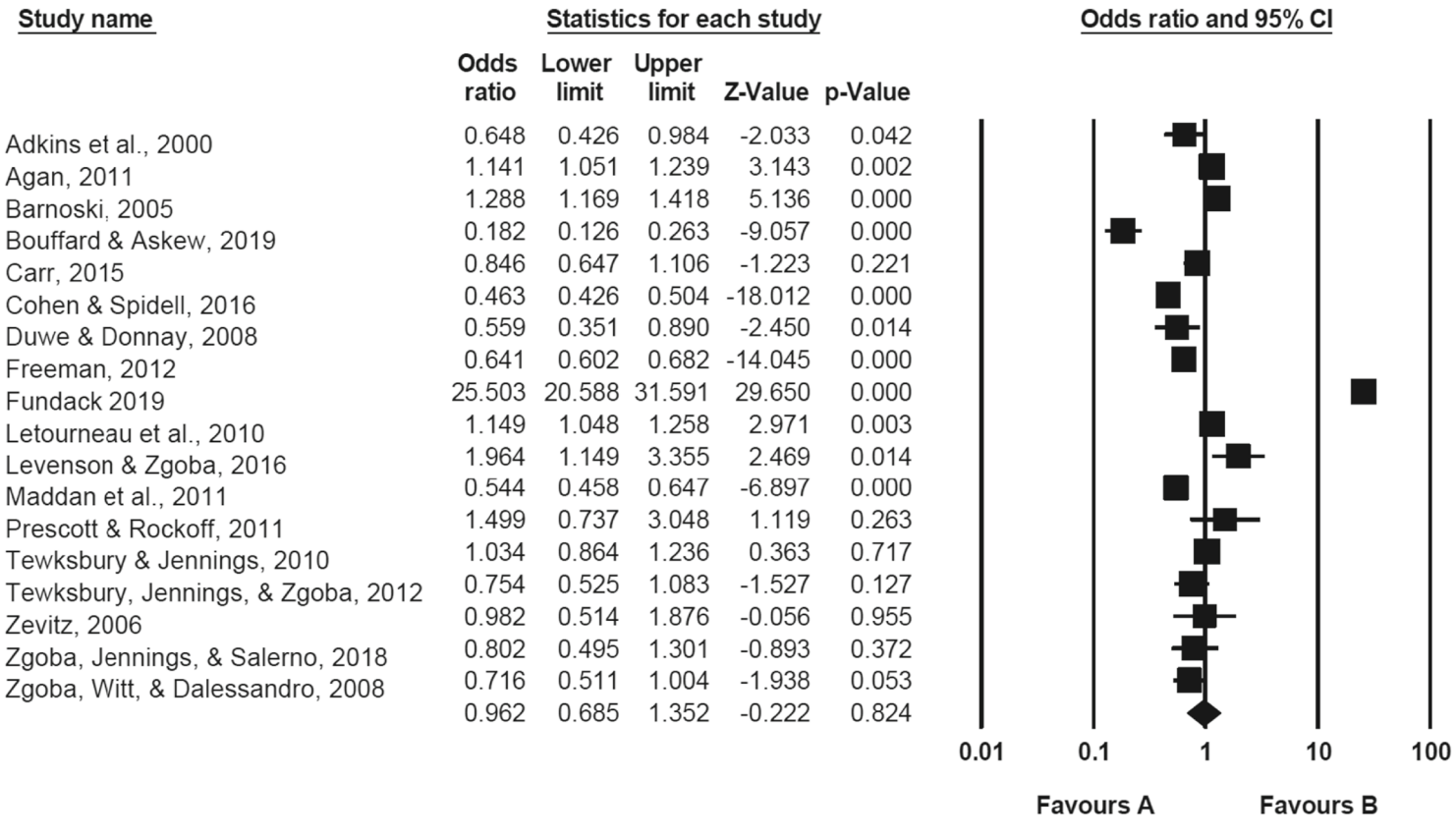
Note: Odds ratio = 0.962; CI= [0.685–1.351]; Z = −0.222; p = 0.824; I^2 = 98.89; Q = 1527.37, $p < 0.05$.

recidivism. When operationalizing recidivism, one study did so using multiple indicators, two did so using a charge, 11 relied on arrest, three used conviction, and one measured reincarceration.

Individual and combined effect sizes

The effect sizes and confidence intervals for each of the 18 studies are included in Table 1 and visually depicted in Figure 1. The effects from 11 studies were statistically significant at p-value < 0.05 and one study was very close to this arbitrary cut point with a p-value of 0.053. Within these studies, seven of them found that SORN decreased recidivism and this effect ranged from an 82% reduction in recidivism (Bouffard & Askew, 2019) to a 28% reduction (Zgoba et al., 2008). Other statistically significant studies (n = 5) reported an increase in recidivism as a result of SORN. These increases ranged from a 14% increase (Agan, 2011) to an increase in the odds by 25.50 (Fundack, 2019).[9] The remaining six studies had non-significant findings of which an equal number of studies has odds ratios above and below one. The Q-statistic for the model was significant (Q(18) = 1527.37, $p < 0.05$), indicating heterogeneity of effect sizes throughout studies (I^2 = 98.89). The random-effects model demonstrated that

[9] Although this study had an effect size that was much larger than our sample, we retained it for analyses.



Notes. Favours A associates with reductions in recidivism, whereas Favours B corresponds with increases in recidivism.

Figure 1 Overall random-effects model evaluating SORN on recidivism

Table 2 Study characteristics, random-effects sizes, and confidence intervals for SORN and sexual and non-sexual offense type recidivism studies

Study	Recidivism type	Sample size	Odds ratio	95% CI
Sex offense[a]				
Adkins et al., 2000	Multiple	434	0.858	[0.296–2.490]
Agan, 2011	Arrest	9623	1.009	[0.837–1.216]
Barnoski, 2005	Conviction	8359	0.553	[0.452–0.676]
Bouffard & Askew, 2019	Charge	424	0.182	[0.126–0.263]
Carr, 2015	Conviction	2005	0.766	[0.361–1.628]
Cohen & Spidell, 2016	Arrest	93,524	0.550	[0.454–0.667]
Duwe & Donnay, 2008	Arrest	310	0.297	[0.129–0.684]
Freeman, 2012	Arrest	17,165	0.768	[0.682–0.864]
Fundack, 2019	Arrest	1985	25.503	[20.588–31.591]
Letourneau et al., 2010	Charge	6064	1.149	[1.048–1.258]
Levenson & Zgoba, 2016	Arrest	180	1.964	[1.149–3.355]
Maddan et al., 2011	Arrest	2920	0.867	[0.661–1.137]
Prescott & Rockoff, 2011	Arrest	328,260	1.499	[0.737–3.048]
Tewksbury & Jennings, 2010	Conviction	1582	1.034	[0.864–1.236]
Tewksbury et al., 2012	Arrest	495	0.719	[0.410–1.259]
Zevitz, 2006	Arrest	213	1.720	[0.723–4.090]
Zgoba et al., 2008	Arrest	550	0.747	[0.413–1.352]
Non-sex offense[b]				
Adkins et al., 2000	Multiple	434	0.642	[0.416–0.992]
Agan, 2011	Arrest	9623	1.140	[1.046–1.243]
Carr, 2015	Arrest	2005	0.986	[0.742–1.311]
Cohen & Spidell, 2016	Arrest	93,524	0.213	[0.168–0.270]
Duwe & Donnay, 2008	Arrest	310	0.763	[0.471–1.235]
Freeman, 2012	Arrest	17,165	0.686	[0.645–0.730]
Levenson & Zgoba, 2016	Arrest	180	1.969	[1.152–3.364]
Tewksbury et al., 2012	Arrest	495	0.873	[0.613–1.242]
Zgoba et al., 2018	Arrest	547	0.802	[0.495–1.301]

Note: [a] Odds ratio = 0.978; CI= [0.609–1.572]; Z = −0.091; p = 0.928; I^2 = 98.51; Q = 1074.52, $p < 0.05$.
[b] Odds ratio = 0.768; CI= [0.555–1.063]; Z = −1.594; p = 0.111; I^2 = 96.39; Q = 221.46, $p < 0.05$.

SORN reduced the mean of recidivism by 3.8%, (OR = 0.962 [0.685 − 1.351]) which was not statistically significant ($Z_{18} = -0.222$, $p = 0.824$).[10]

Effect sizes for sexual or non-sexual offense types

The effect sizes and confidence intervals for studies by sexual and non-sexual offense type are included in Table 2. Seventeen studies provided estimates for sexual offenses

[10] The fixed-effects model demonstrated that SORN reduced the mean of recidivism by 13.5%, (OR = 0.865 [0.837–0.894]) which was statistically significant ($Z_{18} = -8.612$, p = 0.000).

with various types of recidivism (i.e., arrest, conviction, charge, or multiple types). Within this grouping of studies, eight studies were statistically significant at p-value < 0.05. All but three of the studies found that SORN decreased recidivism. The effects ranged from an 82% reduction in recidivism (Bouffard & Askew, 2019) to a 23% reduction (Freeman, 2012). Statistically significant increases were found to range from a 15% increase (Letourneau et al., 2010) to an increase in the odds by 25.50 (Fundack, 2019). The remaining nine studies had non-significant findings. The Q-statistic for the model was significant ($Q(17) = 1074.52$, $p < 0.05$) with a model I^2 of 98.51. The random-effects model demonstrated that (see Figure 2), for sexual offenses, SORN reduced the mean of recidivism by 2.2% (OR = 0.978 [0.609 − 1.572]), which was not statistically significant ($Z_{17} = -0.091$, $p = 0.928$).[11]

For non-sexual offenses, nine studies provided estimates with various types of recidivism (i.e., arrest or multiple types). Five of the nine studies were statistically significant at p-value < 0.05. Three studies found that SORN decreased non-sexual recidivism with effects ranging from a 79% reduction in recidivism (Cohen & Spidell, 2016) to a 31% reduction (Freeman, 2012). Statistically significant increases were found for two studies and those effects ranged from 14% (Agan, 2011) to an increase of 97% (Levenson & Zgoba, 2016). The remaining four studies had non-significant findings. The Q-statistic for the model was significant ($Q(9) = 221.46$, $p < 0.05$) with a model I^2 of 96.39. For non-sexual offense types (see Figure 3), the random-effects model demonstrated that SORN reduced the mean of recidivism by 23.2% (OR = 0.768 [0.555 − 1.063]), which was not statistically significant ($Z_9 = -1.594$, $p = 0.111$).[12]

Effect sizes for arrested or convicted recidivism types

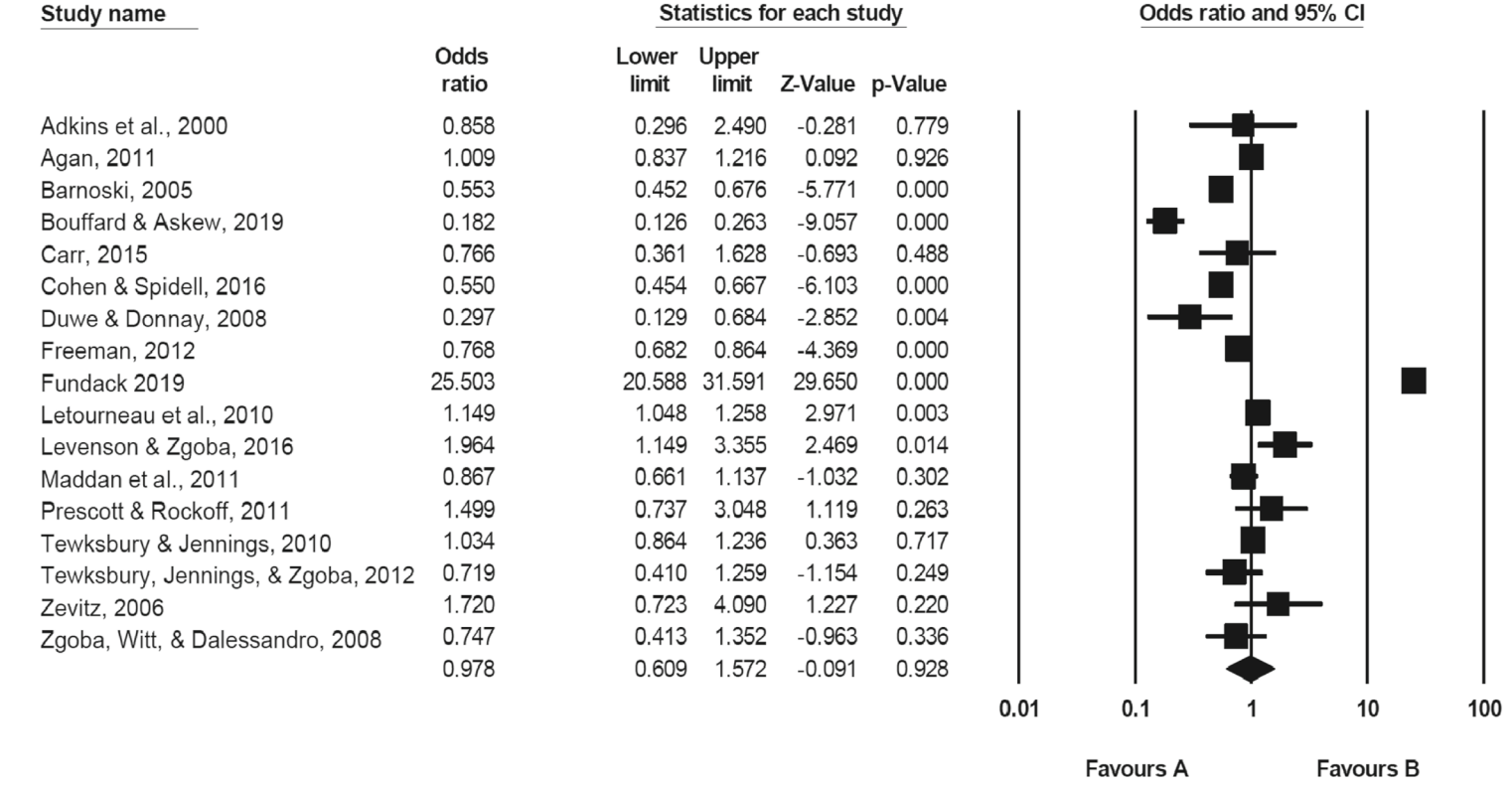
The effect sizes and confidence intervals for studies disaggregated by recidivism type are presented in Table 3. When examining arrest as a recidivism outcome, thirteen studies were included of which seven had significant findings (p-value < 0.05). Significant and negative effects ranged from a 54% reduction in recidivism (Cohen & Spidell, 2016) to a 36% reduction (Freeman, 2012). Conversely, statistically significant increases in arrests were found to range from a 13% increase (Agan, 2011) to an increase in the odds by 25.50 (Fundack, 2019). The remaining six studies had non-significant findings. The Q-statistic for the model was significant ($Q(13) = 1320.40$, $p < 0.05$) with a model I^2 of 99.09. The random-effects model (see Figure 4) demonstrated that SORN increased the mean of recidivism by 11.9%, (OR = 1.19 [0.697 − 1.797]) which was not statistically significant ($Z_{13} = 0.467$, $p = 0.641$).[13]

Five studies examined conviction as an outcome using various type of recidivism with three of them being statistically significant at p-value < 0.05. All but one of the studies found that SORN decreased convictions with all three effects hovering around a 46% reduction. The sole study that reported a statistically significant increase found

[11] The fixed-effects model demonstrated that for sexual offenses SORN increased the mean of recidivism by 6.9%, (OR = 1.069 [1.014–1.126]) which was statistically significant ($Z_{17} = 2.475$, p = 0.013).

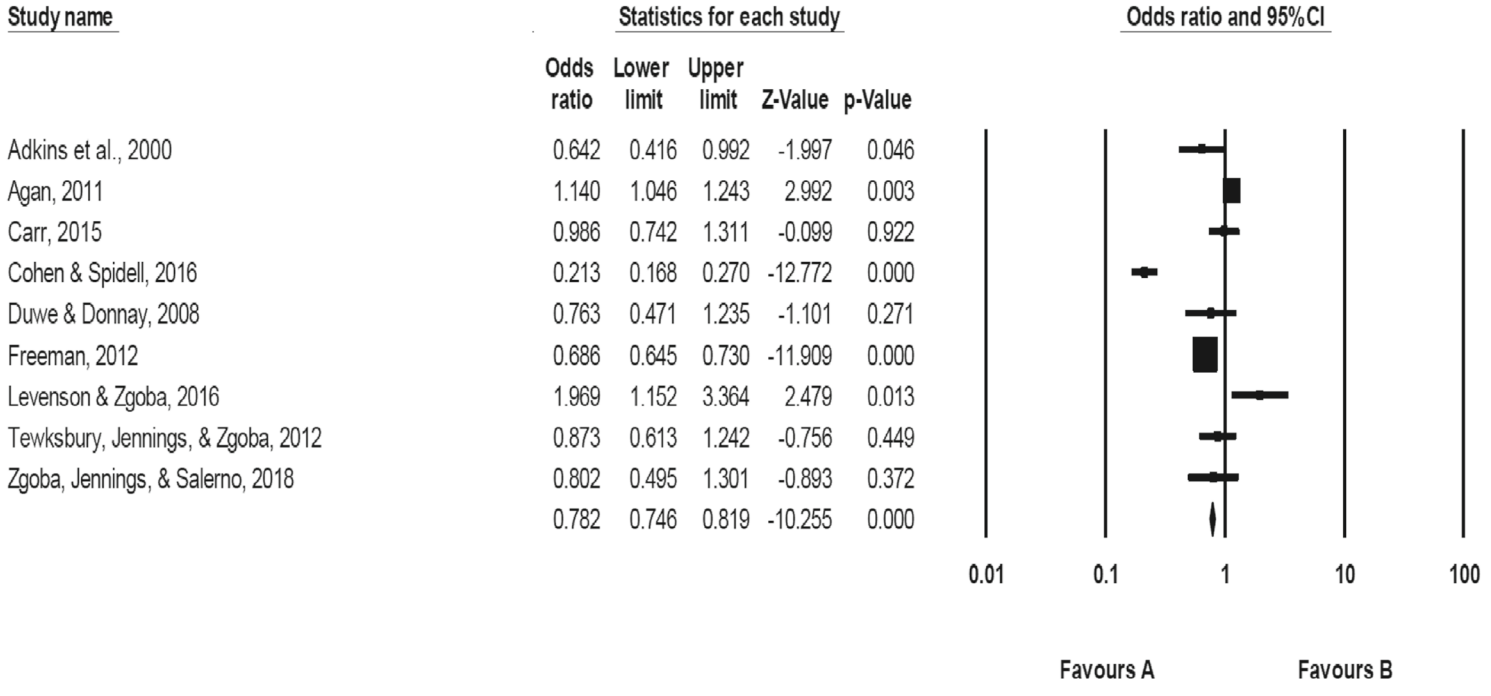
[12] For non-sexual offense types, the fixed effects model demonstrated that SORN reduced the mean of recidivism by 21.8%, (OR = 0.782 [0.746–0.819]) which was statistically significant ($Z_9 = -10.255$, p = 0.000).

[13] The fixed-effects model demonstrated that SORN decreased the mean of recidivism by 22.4%, (OR = 0.776 [0.746–0.807]) which was statistically significant ($Z_{13} = -12.646$, p = 0.000).



Notes. Favours A associates with reductions in recidivism, whereas FavoursB corresponds with increases in recidivism.

Figure 2 Random-effects model evaluating SORN on sexual offense type recidivism



Notes. Favours A associates with reductions in recidivism, whereas Favours B corresponds with increases in recidivism.

Figure 3. Random-effects model evaluating SORN on non-sexual offense type recidivism

Table 3 Recidivism type study characteristics, random-effects sizes, and confidence intervals for SORN and arrested or convicted recidivism studies

Study	Sexual or non-sexual recidivism	Sample size	Odds ratio	95% CI
Arrested[a]				
Agan, 2011	Both	9623	1.132	[1.041–1.230]
Carr, 2015	Both	2005	0.846	[0.647–1.106]
Cohen & Spidell, 2016	Both	93,524	0.463	[0.426–0.504]
Duwe & Donnay, 2008	Both	310	0.559	[0.351–0.890]
Freeman, 2012	Both	17,165	0.641	[0.602–0.682]
Fundack 2019	Sexual	1985	25.503	[20.588–31.591]
Levenson & Zgoba, 2016	Non-sexual	180	1.969	[1.152–3.364]
Maddan et al., 2011	Both	2920	0.544	[0.457–0.646]
Prescott & Rockoff, 2011	Sexual	328,260	1.499	[0.737–3.048]
Tewksbury et al., 2012	Both	495	0.754	[0.525–1.083]
Zevitz, 2006	Sexual	213	1.720	[0.723–4.090]
Zgoba et al., 2018	Non-sexual	547	0.802	[0.495–1.301]
Zgoba et al., 2008	Both	550	0.716	[0.511–1.004]
Convicted[b]				
Agan, 2011	Sexual	9623	1.115	[0.851–1.461]
Barnoski, 2005	Both	8359	1.501	[1.357–1.660]
Duwe & Donnay, 2008	Both	310	0.535	[0.333–0.857]
Maddan et al., 2011	Both	2920	0.542	[0.454–0.646]
Tewksbury & Jennings, 2010	Sexual	1582	1.034	[0.864–1.236]

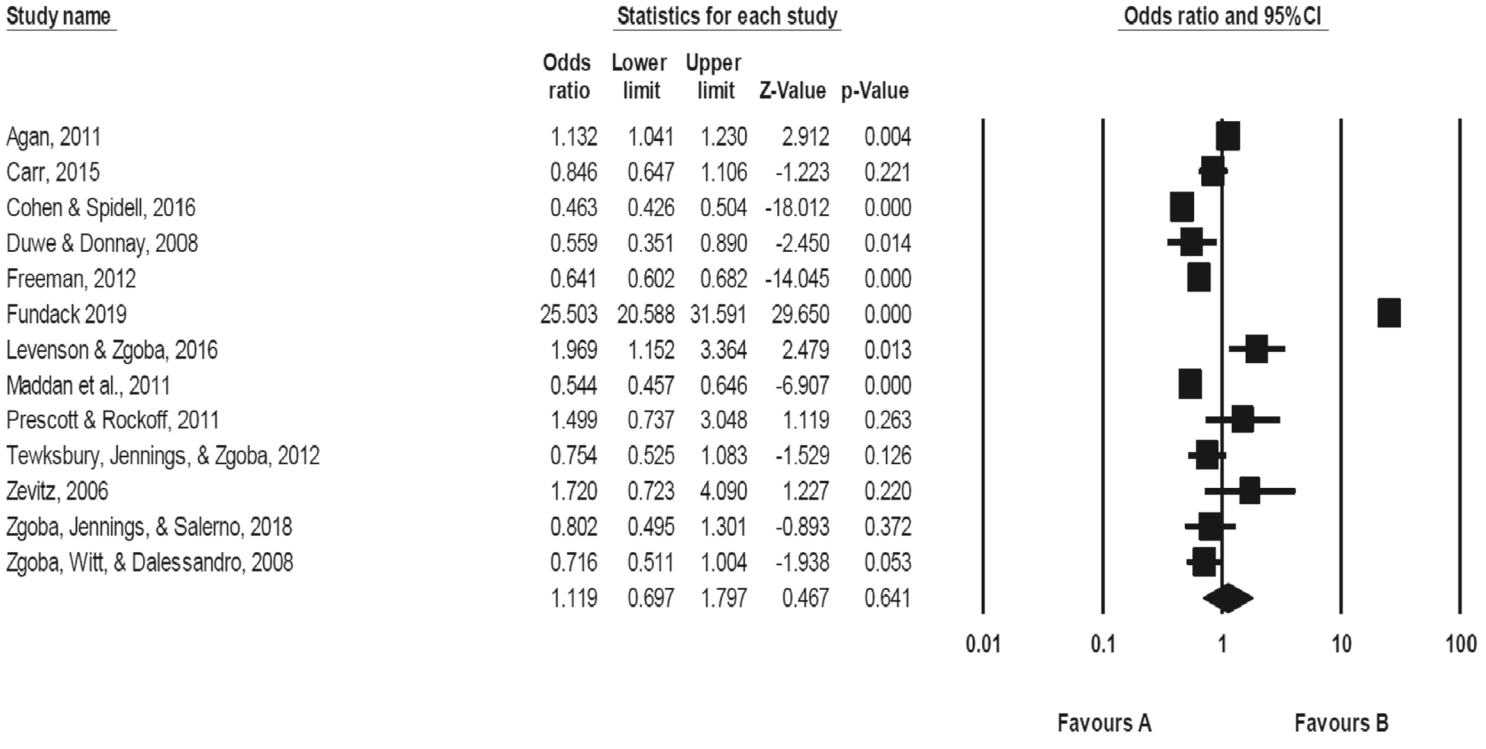
Note: [a] Odds ratio = 1.119; CI= [0.697–1.797]; Z = 0.467; p = 0.641; I^2 = 99.09; Q = 1320.40, $p < 0.05$.
[b] Odds ratio = 0.886; CI= [0.569–1.380]; Z = −1.534; p = 0.593; I^2 = 96.29; Q = 107.91, $p < 0.05$

that SORN increased convictions by 50% (Barnoski, 2005). The remaining two studies had non-significant findings. The Q-statistic for the model was significant (Q(5) = 221.46, $p < 0.05$) with a model I^2 of 96.39. For studies examining conviction as an outcome, the random-effects model demonstrated that SORN reduced the mean of recidivism by 11.4% (OR = 0.886 [0.569 − 1.380]), which was not statistically significant ($Z_5 = -1.534$, $p = 0.593$) (see Figure 5).[14]

Publication bias

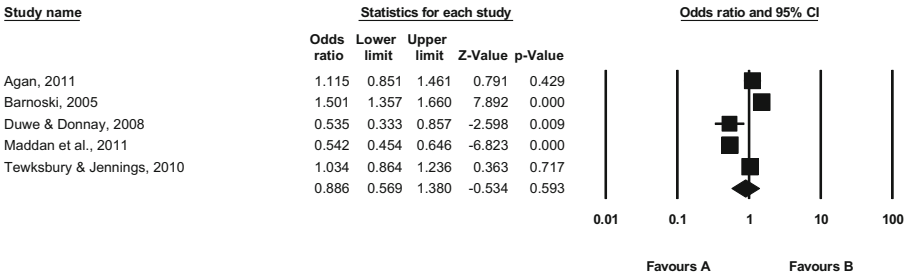
With any meta-analysis, the possibility for publication bias exists (Lipsey & Wilson, 2000) given the likelihood of significant findings to be published by journals over non-significant findings (Rosenthal, 1979). As previously mentioned, we relied heavily on informal conversations with a network of experts to ensure that any "file drawer" studies were included in the sample; however, we also wanted to examine the publication bias

[14] For studies examining conviction as an outcome, the fixed-effects model demonstrated that SORN increased the mean of recidivism by 11.7%, (OR = 1.117 [1.037–1.203]) which was statistically significant ($Z_5 = 2.905$, p = 0.004).



Notes. Favours A associates with reductions in recidivism, whereas Favours B corresponds with increases in recidivism.

Figure 4 Random-effects model evaluating SORN on arrest recidivism



Notes. Favours A associates with reductions in recidivism, whereas Favours B corresponds with increases in recidivism.

Figure 5 Random-effects model evaluating SORN on conviction recidivism

statistically. Thus, we examined any bias for our combined model (Table 1 and Figure 1) using a number of techniques. First, of the 18 studies included in this analysis, statistically significant and non-statistically significant findings were found in both positive and negative directions, suggesting that there is not a bias in terms of what types of findings are published on this topic. Second, a funnel plot was used to chart asymmetry of risk ratios and standard errors (Sterne & Egger, 2001). Studies were distributed around the mean suggesting that bias may not exist. To further test the asymmetry based on the intercept of a linear regression against the reciprocal of standard errors, the Egger et al. (1997) was used. Asymmetry was not present, and the model had an intercept of 2.22 (95% CI = −5.56, 10.00), p = 0.278), suggesting that publication bias does not exist. Fourth, the Orwin's *Fail Safe N* (Orwin, 1983) was used and equaled 28. This estimate indicates that 28 studies with a mean risk ratio of more than 1 would be needed to change the cumulative effect to be greater than a significance level of 0.05. Considering only 18 studies were included in this meta-analysis in total—all with various effects—increasing the number by 28 would be substantial. Fifth and finally, trim and fill estimates (Duval & Tweedie, 2000) were computed to estimate the effect of publication bias, on the outcome of the meta-analyses. The trim-and-fill procedure imputes the missing data and re-calculates the combined effect should publication bias exist. As demonstrated in Figure 6,

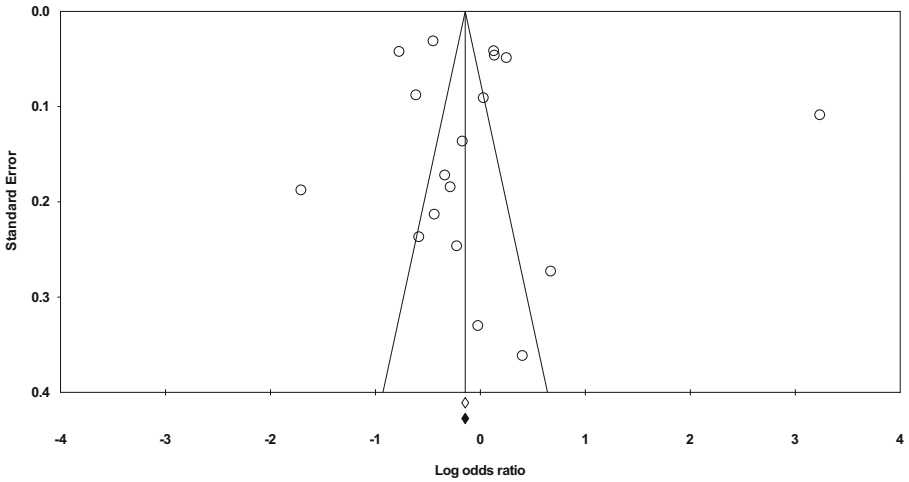


Figure 6. Funnel plot for asymmetry of risk ratios and standard errors

no imputed studies were needed for symmetry. Based on the aforementioned bias statistics, publication bias is negligent and not likely to substantially influence the mean effect size between SORN and recidivism.

Discussion

Study findings

The goal of the present study was to conduct a meta-analysis of SORN evaluations. Relying on the methodology guidelines established by the Campbell Collaboration, we systematically synthesized results from 18 research articles spanning approximately 25 years, from 1996 (the year of federal implementation of SORN) to 2020 including 474,640 formerly incarcerated sex offenders. To date, this is the most extensive review period of SORN and the only meta-analysis. We examined three different outcomes—combined recidivism, offense type by sexual recidivism and non-sexual recidivism, and recidivism type by arrest and conviction. The random-effects meta-analysis model demonstrated that the SORN does not have a statistically significant impact on combined recidivism. This null effect also exists when disaggregating studies by sexual or non-sexual recidivism, or conceptualizing recidivism by arrest or conviction. In sum, this finding indicates that the implementation of SORN over the last 25 years has demonstrated no effect on the deterrence or decrease of adult offending.

While these findings are consistent with previous research on the topic, they create a number of legislative and policy-related dilemmas that require attention and warrant a discussion on reform. First, the evolution of SORN policies over the last two and a half decades has had a net-widening effect to make more registrants eligible for registry placement and for considerably longer periods of time. These expansions are counterintuitive to the empirical research that demonstrates individuals convicted of sexual offenses age out of crime as other individuals do (Hanson, 2002; Hanson et al., 2014; Hanson et al., 2018). This link between age and general criminal behavior is firmly established in the field of criminology and desistance from criminal offending is "nearly inevitable" across offender types (Gottfredson & Hirschi, 1990; Hanson, 2018). In general, the risk of sexual re-offense decreases with advancing age. Specifically, recidivism rates for individuals in their 40s and 50s consistently demonstrate declines, with subsequent steep declines after the age of 60. Additionally, in large studies, the sexual recidivism rates of those over the age of 60 were shown to be less than 4% (Hanson, 2002; Helmus et al., 2012). In light of the aging registrant population, these findings suggest that longer registration periods under SORN policy changes may be inefficient and they have the potential to misallocate resources and supervision to those at varying and sometimes lower-risk levels (Booth, 2016). Since not all individuals are equally likely to reoffend, and because risk wanes with age, the heightened number of individuals remaining on the registry forces individuals into a "one size fits all" solution that may make it difficult for the public to discern true risk (Kahn et al., 2017; Zgoba et al., 2016). While well-intentioned, over-allocation of management strategies may also encourage collateral consequences among the population of registered sex

offenders, as lower-risk individuals may be forced into homelessness and unemployment. Accordingly, risk levels may increase for these individuals as a misuse of policies creates a barrier to successful community reintegration (Levenson, D'Amora & Hern, 2007).

In addition to desistance due to age, a growing body of research demonstrates that recidivism risk declines the longer individuals remain offense-free in the community. Hanson et al. (2018) found that individuals reach a "desistance threshold," whereby recidivism is decreased by half with every 5 years in the community offense-free, and that by 20 years, even high-risk registered individuals are less likely to sexually recidivate than a non-sexual offender who commits an "out of the blue" sexual offense (see also Hanson, 2018; Hanson et al., 2014). These findings indicate that certain individuals convicted of a sexual offense will present no significant risk at the time of sentencing, and if offense-free in the community for 10 to 15 years, even the highest-risk individuals will pose no significant risk. In essence, the risk of a sexual offense is "reset" to that of someone who has no history of a sexual offense. This is important since many SORN policies have what is referred to as "time bars" or lifetime registration requirements, both of which are unrelated to dynamic risk and duration offense-free in the community. These time-based requirements suggest that rule and law-abiding behavior are irrelevant and life consequences are set according to one singular criminal act, even if this offense occurred decades before. This policy response promotes the fundamental idea that individuals convicted of sexual offenses maintain high and enduring risk across their lifespan (Hanson et al., 2018; Kahn et al., 2017). Decades of empirical research fails to support this notion and experts in the field posit that communities would be better served by implementing strategies based on risk-need-responsivity paradigms, whereby resources are funneled to those most in need—individuals with a high risk of sexual recidivism (Bonta & Andrews, 2007). By not doing so, we dilute the efficacy of important management strategies and cannot be surprised when research indicates that the policies are ineffective, when in fact they are being applied ineffectively. It is also important to remember that as a society we run the risk of creating new problems, by misdiagnosing current problems. Maintaining low or reduced risk individuals on registries for a lifetime, or barring them from petitioning for termination, may promote collateral consequences in the form of housing instability, employment instability, and internet bans, thereby further increasing their risk of reoffending. As SORN laws currently exist, any public safety benefits we accumulate will likely not outweigh the collateral harm they promote (Hanson, 2018; Mercado et al., 2008).

Lastly, one of the cornerstones of SORN is that identification as an individual convicted of a sexual offense will deter reoffending. The notion here is that if we apply a label to an individual and tell everyone else to watch this person's behavior, they will desist from the unwanted behavior. However, research has not supported this assumption. The majority of empirical studies indicate that those convicted of a sexual offense have no previous sexual offense on their record, and in one instance over 95% of all sexual offense arrests were committed by first-time offenders (Hepburn & Griffin, 2002; Sandler et al., 2008). In addition, many have posited that individuals may internalize criminal labels, subsequently creating self-fulfilling prophecies. Braithwaite (1989) built upon labeling theory by arguing that the application of labels increases crime in certain circumstances, particularly when efforts are not made to reintegrate the

person back into society, when they are rejected or are labeled on a long-term basis. All of these notions ring true under modern-day SORN policies. These empirical and anecdotal research findings minimize the strength of one of the more basic principles of SORN legislation.

In sum, despite their futility and potential damage, SORN policies are likely here to stay due to their widespread public, law enforcement, and political support. This situation presents the metaphorical dilemma of shutting the stable door after the horse has left the barn. Support for the laws is unwavering, yet 25 years of empirical data point to the demonstrable ineffectiveness of SORN policies; how do we then reign the horse back in?

Since the implementation of the AWA, SORN policies have expanded in breadth and scope and show little likelihood of being scaled back. The NCMEC indicates that by now almost one million individuals in the USA are listed on the sex offense registry (Bierie, n.d.), and many for extended periods of time due to registration increases. It is imperative that researchers take steps beyond academic publishing to inform the public, law enforcement, and policy makers that governmental oversight of registrant presence is not a feasible solution to protecting potential sexual abuse victims. As upwards of ninety percent of victims and offenders know and prey on one another, we need to confront the uncomfortable truth that those who commit sexual offenses are usually not strangers. In addition to the overuse of tier levels and registration periods, a database and notification system to alert the public of stranger identification and addresses is inherently flawed and we should therefore not be surprised by the null effects of SORN policies.

Limitations

This study is not without limitations. Because many studies included in this analysis relied on official measures of recidivism, the findings must be interpreted with caution regarding undercounting. Recidivism is defined by offenses that are known to the police and appear on the criminal histories that are utilized for research purposes. Previous studies show that sexual offenses may be underreported to police and that the official records used in the majority of research may underestimate the number of sexual offenses that are actually occurring (Furby et al., 1989). This underreporting will subsequently affect base rate counts of offending, follow-up periods, and time at large (Langevin et al., 2004). However, it bears noting that official records are the most accepted measure of recidivism, as anecdotal, imputation techniques, and qualitative measures introduce ambiguity, particularly with sexual victimization. Additionally, utilizing a meta-analytic technique alleviates some of the low base rate concerns as well.

It should also be recognized that some advocates of registries believe they hold purpose in various areas of crime reduction that should not be overlooked or underestimated (Bierie, 2016). Particularly, limited research has shown that different SORN components have aided law enforcement investigations in the capture and identification of perpetrators, as well as a quicker time to arrest and specific deterrence among individuals on the registry (Bierie, 2016; Duwe & Donnay, 2008; Prescott & Rockoff, 2011). Researchers should have a balanced conversation about these potential benefits, as well as the null findings and collateral consequences cited in other research,

as we move toward having an informed debate on efficacy. As noted by Bierie (2016), it may be worth looking at what good can come out of the law if it works some of the time for some of those placed on it. It may be possible that modifications (i.e., targeted enforcement of registry placement, reduction in lifetime registration, and utilization of actuarial risk assessment tools to name a few) may be made that will still yield value in the law (see the work on Grand Challenges by Levenson et al., 2016).

Lastly, meta-analyses, though extremely useful in summarizing the effects of SORN and recidivism, are not without limitations. A small number of related studies could not be included in this meta-analysis because they did not contain the necessary information to calculate an effect size (see footnotes one and three). Even with reaching out to the authors of these papers, we were not able to gather the appropriate statistics. Related, only 18 studies were included in the analysis. Although this k is more than suitable for a meta-analysis, it makes examining variation between studies challenging which is related to the next point, moderating analyses. We were not able to examine moderation and subgroup analyses, which may reveal alternative findings. Finally, we did not exclude studies based on methodological rigor—doing so has the potential to produce various findings. Despite these limitations, this study provides the most expansive and up-to-date evaluation of SORN policies.

Conclusion

The present study adds to the existing research by completing an objective and replicable framework for a meta-analysis of 25 years of sex offense registration and notification evaluations. The study findings provide comprehensive evidence that SORN policies have no effect on sexual and non-sexual crime commission over their period of existence, thereby failing to deliver on the intention of increasing public safety. Given the vast support that exists for the laws, their lack of efficacy will likely create a false sense of security for the public and may ultimately create more harm than benefit. Communities rely on the government for determination of risk, regardless of its acuity, and those placed on registries may experience harm and increased risk of re-offense through societal disenfranchisement and loss of jobs and housing. As previous research has consistently found that clinical and actuarial assessments are better predictors of recidivism risk than state legislation, public safety would likely be maximized by focusing limited resources on the highest-risk individuals, rather than utilizing a one size fits all law (Andrews et al., 2011; Zgoba et al., 2018). Furthermore, this call for reform of SORN policies, and lifetime requirements and offense bars specifically, comes at a time when the country is experiencing both a decarceration and criminal justice reform movement. For much of the last four to five decades, incarceration growth and criminal justice sanctions were accepted as a necessary consequence of high rates of crime or a perceived deterrence (Zgoba & Clear, 2021). Recently, however, examinations of the connection between crime and criminal justice sanctions have questioned their effect on crime prevention (for a review, see National Research Council, 2014), while at the same time recognizing a broad range of undesirable consequences such as burgeoning prison populations, offense management strategies, and criminal justice sanctions (Clear, 2007; Western, 2007; Zgoba & Clear, 2021). It is important to the pursuit of science and empirical inquiry that our conversations for

reform begin to consider those who have committed low-level acts of violence, including low-risk sexual offenses. Due to the dynamic nature of risk across time, offense, and age, recognition that SORN policies may prove to be more hurtful than helpful is a necessary conversation that does not automatically signal society has gone soft on crime. Allocating resources to the highest-risk individuals will likely allow treatment providers and law enforcement to focus their efforts on those who need it the most, not on the diffusion of all individuals. It is time that we work as an empirically informed community, unhindered by emotion, to find a solution to reining the horse back into the barn.

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Kristen M. Zgoba is an Assistant Professor of Criminology and Criminal Justice at Florida International University. Prior she served as the research supervisor for the New Jersey Department of Corrections. Dr. Zgoba has studied sexual offense legislation in the United States and overseas through the National Institute of Justice and a Fulbright Research Scholar Award to the United Kingdom. She has published in the *Prison Journal*, *Race and Justice*, *Criminology & Public Policy*, *Sexual Abuse: A Journal of Research and Treatment*, *Justice Quarterly* and *Criminal Justice and Behavior*. She is also a member of the Research Council within the American Correctional Association and is a founding member of the Sex Offense Policy & Research group. Dr. Zgoba also serves on the Board of Directors at the Edna Mahan State Correctional Facility for women in New Jersey.

Meghan Mitchell is an Assistant Professor of Criminal Justice at the University of Central Florida. She received her PhD from Sam Houston State University. Her research examines corrections, subcultures, reentry, and research methodologies. She has published in the *British Journal of Criminology, Justice Quarterly, Sociological Methods & Research,* and the *Journal of Criminal Justice,* among others. She is a Co-PI on the CCWORK Study—an interdisciplinary evaluation of correctional workers' wellbeing, organizations, roles, and knowledge, and is the PI for the translation to the United States. She is also the Research Coordinator for the Florida Prison Education Project.