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Offenders and SORN Laws

*Amanda Agan and J.J. Prescott**

Chapter 7 describes what we know about the effects of SORN laws on criminal behavior. A coherent story emerges from this review: there is virtually no evidence that SORN laws reduce recidivism or otherwise increase public safety. The chapter first delineates the various ways registration and notification alter the legal environment not only for registrants but also for nonregistrants, the public, and law enforcement. There are many channels through which SORN laws might impact the frequency of sex offenses, including some that would produce an *increase* in overall offending. The chapter assesses these possibilities in light of a large body of relevant empirical research, focusing on potential changes in registrant recidivism, nonregistrant criminal behavior, the geography of victimization, and the distribution of types of sex offenses and victims. Scholars have plumbed many different data sources using a range of methodologies, yet nearly every study finds no evidence that SORN laws – in particular, community notification laws – reduce sexual recidivism. In fact, notification laws may increase recidivism risk. The final section discusses registrant beliefs about the effects of SORN laws. In sum, the chapter comprehensively engages with the pressing question of whether SORN laws protect the public and concludes that they do not.

7.1 INTRODUCTION

Perhaps the central empirical questions regarding sex offender registration and notification (SORN) laws involve their direct effects on sexual offending. The basic premise advanced in support of SORN laws is that they will reduce the frequency (or perhaps severity) of sex offenses. Specifically, proponents contend that registration and notification will diminish the total harm of sexual offending by reducing the likelihood that an individual previously convicted of a registerable sex offense will recidivate or by increasing the likelihood that law enforcement will more quickly apprehend any registrant who does recidivate.

But a full analysis reveals that this hypothesis is just one of many potential outcomes – and that SORN laws may actually *increase* the frequency of sexual offending (Prescott, 2016). At the same time, SORN may also change criminal

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behavior in other important ways, including (1) by deterring sexual offenses by nonregistrants, who may wish to avoid the burdens of registration and notification that come with conviction, and (2) by altering the costs and benefits of re-offending in particular locations (and thus the geographic pattern of sexual offenses) or against particular victims. For policy makers who wish to understand the underlying determinants of sexual offending and to design and implement effective post-release regimes, it is crucial to consider this broad range of plausible behavioral consequences and relevant available evidence.

In this chapter, we discuss the various potential effects that SORN laws might have on sexual offending and related criminal behavior and summarize the available empirical evidence on the direction and magnitude of these effects. The story that emerges across dozens of papers, data sources, and empirical methods is coherent and convincing: there is scant evidence that SORN reduces recidivism or otherwise increases public safety, with the possible exception of some tentative evidence that registration alone (i.e., without notification, which no jurisdiction currently uses) might reduce sexual offense recidivism.¹ This conclusion is at odds with the basic premises upon which politicians and advocates have defended SORN laws from the outset, but it is not altogether surprising given what we now know about the very minor role recidivism plays in the overall incidence of sexual offending.

We also occasionally touch on evidence of the possible effects of SORN on non-sex offense crimes and on the role and usefulness of failure-to-register (FTR) offenses. Sex offender registration and notification “derivative” criminal behavior (such as FTR) is important to consider separately because, while technically criminal, such violations are not clearly connected to the goal of reducing the frequency of sex offenses. Finally, we address existing empirical research on registrant “beliefs” about the behavioral consequences of SORN laws. Although their value vis-à-vis the actual efficacy of SORN laws is uncertain, reported registrant beliefs may nevertheless be considered measures of the impact of SORN and can aid in our collective understanding of what drives the behavioral patterns we identify in the literature.

7.2 DELINEATING THE TYPES OF SORN EFFECTS ON SEXUAL OFFENDING

There are two basic ways SORN laws might affect the frequency and severity of sex offenses: (1) changing the behavior of potential sex offenders (e.g., deterrence); and (2) changing law enforcement or potential victim behavior. For example, if registration and notification only make it easier for the public to monitor and the police to apprehend registrants, we might observe the frequency of sex offenses decline for two reasons.² First,

¹ We define and distinguish registration (or “private” registration) and notification (or community notification or “public” registration) later.

² As other chapters demonstrate, there is considerable evidence that SORN has collateral consequences, and in theory, these changes can themselves have criminogenic effects.

individuals at risk of offending might alter their behavior in response to what they believe is a different information and enforcement environment.³ Second, at least in theory, potential offenders might *not* respond at all to SORN laws (perhaps because they are impulsive in the extreme), yet we might still observe fewer sex offenses if SORN helps police or potential victims prevent sex offenses or if SORN makes apprehension and conviction easier and allows earlier incapacitation (and thus the commission of fewer total crimes).

We can obtain a fuller picture of the potential effects of SORN laws by assuming the possibility of behavioral changes both by potential offenders (in anticipation of changes in monitoring, enforcement, and victim precautionary behavior) *and* by the public and law enforcement (via access to sex offender registry information). There are other moving parts in this analysis, however. To begin with, sex offenses vary along many dimensions (i.e., type of offender, type of crime, type of victim, and offense location), and SORN may affect the various offense scenarios very differently. In particular, some potential offenders are already registered under SORN laws (i.e., “registrants,” those previously convicted of registerable sex offenses), while others are not registered (“nonregistrants”) because they either have not previously committed or have not been convicted of a registerable sex offense. In addition, because SORN is supposed to work by making information available to the police and the public, a potential offender’s proximity to “informed” individuals may now matter – both to how a registrant (or even a nonregistrant) behaves and to how effective precaution-taking and monitoring by the public and law enforcement may be at disrupting any criminal behavior.

There are at least three other second-order dynamics at work in accounting for the impact of SORN on sexual offending. First, SORN laws may affect behavior by imposing affirmative administrative obligations on registrants and enforcing these requirements through separate criminal provisions (e.g., FTR penalties). Complying with SORN requirements can be burdensome and difficult, and inevitably, at least some individuals who are required to register will fail to register, keep their information current, or otherwise satisfy technical rules. Such failures produce an entirely new tally of (non-sex) offenses, and out-of-compliance status and convictions may, in turn, influence behavior. Second, and relatedly, potential offenders will have “beliefs” about SORN and its effects, and these beliefs may not align with reality. Because people operate based on their beliefs, even inaccurate beliefs about the requirements and enforcement of SORN laws may impact behavior. Third, the effects of SORN will depend at least in part on the accuracy of registry information, which is by no means perfect (e.g., Lasher and McGrath, 2012; Salmon, 2010).

A basic economic model of criminal behavior can help us understand what we need to measure empirically in order to fully account for the potential effects of

³ This is true even if the actual environment never changes – for example, if SORN laws were rarely enforced. Of course, if SORN does not produce real and lasting environmental changes, potential offenders might eventually realize this fact and revert to behavior suited to the pre-SORN world.

SORN laws.⁴ Following the approach of Prescott and Rockoff (2011) and Agan and Prescott (2014), imagine that an individual is considering whether to commit a particular sex offense against a particular victim in a particular location, which may require costly travel and may involve more or less uncertainty about the likelihood of success or detection.⁵ The potential offender faces a trade-off between either committing the crime against that victim in that place or choosing some other action instead (as a default, let's say "doing nothing").

In this setting, potential offender i 's decision whether to commit a crime against victim j in location l is a function of the probability of punishment for committing the crime against victim j in location l (p_{ijl}), the victim-specific level of punishment if caught and convicted (f_{ij}), the cost of carrying out the crime against victim j in location l (c_{ijl}), and the assumed victim- and location-invariant utility (benefit) of engaging in lawful alternatives relative to committing the sex offense (u_i). The frequency of sex offenses is thus essentially the sum of all of these decisions across all individuals. Accordingly, if SORN laws, by whatever means, cause fewer of these individual decisions to end in crime, then the total number of sex offenses should decline.⁶ To model how sexual offending behavior will react to SORN laws, we next isolate the ways that SORN might change the behavioral environment for potential registrants and nonregistrants alike.

We begin with sex offender registration alone – or what some refer to as “private” or “non-public” registration. At their core, registries assemble and store the identifying information of individuals previously convicted of sex offenses (e.g., name, physical description, age, home and work location, criminal history), and registration laws require that these individuals confirm and update their registry information on a regular basis. When unaccompanied by a community notification law that allows or requires public disclosure of registrant criminal history information, a registry (and the information it contains) can only be accessed by law enforcement entities and certain public agencies (as well as private employers who seek to access a specific individual's registry status for employment screening purposes).

In theory, easy access to registration information by law enforcement may allow officers to monitor registrants more effectively or easily and thereby increase the likelihood that registrants who re-offend will be detected and apprehended (i.e., registration may increase p_{ijl}). Officers might concentrate their monitoring efforts in predictable ways, too, potentially increasing detection probabilities even further for offenses against likely potential targets (e.g., former victims or victims similar to past victims) or for offenses committed in locations registrants are likely to visit (such as

⁴ This discussion draws on the analysis in Agan and Prescott (2015).

⁵ In this model, and in most of our discussion of existing evidence, we ignore the choice between different types of sex offenses, and we make some simplifying (but unnecessary) assumptions about how law enforcement works – for example, we assume in the text that the level of punishment may turn on the identity of the victim but that punishment severity is insensitive to a crime's location.

⁶ As we will see, with SORN laws in effect, certain plausible conditions produce more crime and others produce less crime. This implies not only that the total frequency of sex offenses may change but also that the *composition* of sex offenses may change when SORN laws are enacted.

any location within a mile of a registrant's address).⁷ Registration may also affect offending behavior through increasing the severity of punishment upon conviction for a sex offense: specifically, *nonregistrants* may change their offending behavior under a registration system if they perceive the prospect of complying with registration requirements in the future (and the associated law enforcement monitoring) as unpleasant (i.e., registration may increase f_{ij}). Finally, note that registration regimes impose burdens on *all* registrants, even those who would otherwise never recidivate. Requiring regular information updates – and thus imposing the significant costs that come with complying with these obligations, including anxiety over the serious consequences for even accidental oversights – can make staying on the straight and narrow more difficult (i.e., reducing u_i).

Community notification makes registration information available to the public. Today, registrants' identifying information is made publicly available primarily through free government websites often referred to as "internet registries." Governments have used other forms of community notification methods over the years as well (e.g., newspaper notification), some of which have been effectively superseded by internet registries (e.g., paper registries located in police departments). One form of notification that remains important is "active notification," in which officials proactively inform potential victims – usually neighbors – of the identity, history, and proximity of a registrant in person or by letter.

The motivation behind making registration information public is that it may encourage community members to monitor registrants and take protective precautionary measures. If the public uses the information to monitor potential recidivists, any sex offense (or potentially any offense) committed by a registrant will be more likely to be detected (further increasing p_{ijl}), especially in particular locations (e.g., near the registrant's home address) where relevant registry information may be better known and thus better able to help someone identify a registrant as the offender. Precautionary behavior by informed potential victims can increase the cost of committing an offense (e.g., by disrupting or diminishing registrants' offending opportunities), reducing the relative attractiveness of offending against those victims in those locations (increasing c_{ijl}). But, perversely, access to registry information might also distract potential victims or their caregivers from threats posed by nonregistrants (particularly those familiar to a victim, such as family members, neighbors, or coaches) and by registrants who live some distance away (reducing p_{ijl} for some potential offenders).

Although fear of being subject to community notification in the future is a nonissue for those already registered (unless the registration term is short or near its end), nonregistrants may find the total punishment associated with the commission of a sex crime much higher under a community notification regime (increasing

⁷ This approach to reducing sex offense frequency comports in the abstract with criminology research, which generally shows that greater certainty of punishment (via an enhanced threat of apprehension) is more important to deterring crime than increasing punishment severity (Nagin, 2013). As we note immediately below, SORN laws potentially operate along both dimensions.

f_{ij}). The possibility of being publicly shamed as a sex offender upon conviction may be very salient and perhaps more important than an ambiguous and uncertain increase in detection probability alone. Moreover, if community notification reduces employment, housing, and social opportunities following release (e.g., Levenson and Cotter, 2005), and nonregistrants anticipate these collateral consequences, we might observe even greater deterrence effects for these potential first-time sex offenders. Registrants, of course, must suffer the same adverse effects of community notification for many years whether or not they recidivate. Because these consequences do not depend on post-release behavior, community notification effectively reduces the threat of criminal punishment for recidivism, at least in a relative sense (reducing u_i). In other words, because post-release life is relatively much worse for registrants subject to notification, the gravity of the threat of returning any recidivist among them to prison for committing another sex crime is significantly diminished. This effect might be particularly important if community members use registration information not only to shame or discriminate against registrants but also to harass and victimize them.

Despite the conventional wisdom that SORN laws will reduce recidivism and thus decrease the total number of sex offenses (see, e.g., Prescott, 2016), the analysis above shows that SORN laws can have multifaceted and ambiguous effects on offending behavior – with difficult-to-predict ramifications for recidivism, total sex offense frequency, and the specific kinds of sex crimes committed.⁸ These effects may magnify or offset each other in the aggregate; effects may also be heterogeneous across groups of potential offenders, potential victims, and sex offenses. Thus, for example, SORN laws may work more or less as intended for some registrants, may increase recidivism risk among others, and may have offsetting effects for the rest. Changes in nonregistrant offending behavior may have an entirely different character (e.g., SORN may lead to higher recidivism levels but fewer first-time offenses).

It is therefore inappropriate to conceive of SORN laws, even in a single jurisdiction at a particular time, as having just one ex ante outcome of interest – for example, the recidivism rate. Even a simple behavioral model shows that SORN laws may produce (1) spillovers to other forms of criminal behavior (through changes in u_i), (2) re-ordering in the overall composition of sex offenses (e.g., variation in the types of offenses and victims making up any particular category of crime), (3) repercussions for the geographic distribution of sex offenses (e.g., relative to the location of registrants), and (4) a shift in the relative role of registrants and nonregistrants in driving the total frequency of sex offenses. In effect, SORN laws seem likely to have

⁸ Questions about the effectiveness of SORN laws are distinct from – albeit not unconnected to – questions about sex offender recidivism risk generally. If, on average, SORN laws reduce or increase rates of recidivism or total sex offense frequency, policy makers should not care whether recidivism rates start at a relatively high or low level. For example, if research demonstrates that SORN makes recidivism worse, the fact that recidivism began at a high level would not make SORN any more attractive (unless the legislature's primary motivation was simply to soothe the public by "doing something" and it was willing to tolerate greater actual risk in doing so).

at least some “distributional” consequences, with certain potential victims “winning” but others – perhaps a larger group – “losing.”⁹

7.3 EMPIRICAL EVIDENCE ON THE EFFECTS OF SORN LAWS

Over the last few decades, a considerable body of empirical work has studied how SORN laws affect the frequency of sex offenses and the likelihood of registrant recidivism. The methods and data these studies employ vary a great deal. Most studies look at data from one or two states and follow offenders over time, using criminal history trajectories (arrest, prosecution, conviction, incarceration, and FTR incidents) and the enactment timing or scope of SORN laws to identify their consequences for recidivism. Other studies use data from multiple states to unravel the potentially confounding role of state-level idiosyncrasies and control for countrywide trends by leveraging different laws’ effective dates. Among these multistate studies, researchers often present evidence on the effects of SORN on total offense frequency – rather than just recidivism among registrants – but a few researchers have deployed strategies capable of isolating recidivism effects alone.

Examining the impact of SORN laws on *total* criminal incident frequency has advantages. In particular, this approach accounts for changes in nonregistrant sexual offending, avoids any bias resulting from prosecutorial charging and offender plea-bargaining behavior, and at least partly addresses any effects the laws may have on victim reporting and cooperation (e.g., victim refusal to cooperate with prosecutors, which makes charging and conviction less likely).¹⁰ In the end, policy makers should care about *all* effects of SORN laws. But for our purposes, it is useful to distinguish evidence of effects on recidivism among registrants from evidence of effects on the deterrence of nonregistrants (e.g., potential first-time offenders). As we will see, these effects may offset each other in the case of notification, and this highly policy-relevant empirical possibility can be missed in studies that do not separately consider SORN effects on registered and nonregistered potential offenders.

⁹ This complexity still reflects only a narrow slice of the potential costs (and benefits) SORN laws may create for society. A full accounting is beyond the scope of this chapter, but it would include much more than just the direct effect of the laws on sexual offending. For instance, administering and enforcing registration and notification laws are quite costly for governments. These costs amount to dollars not being spent on other crime reduction policies, many of which are presumably cost effective. Chapter 4 of this volume discusses the challenges SORN laws create for law enforcement. As a further example, SORN imposes costs not only on registrants but also on their families, neighbors, and communities. These consequences, reviewed in Chapter 8, ripple through society in many potentially noncriminogenic but important ways – for example, families lose bread winners, employers lose employees, and governments lose taxpayers. Such effects, beyond recidivism, are important in assessing whether SORN laws are worth their considerable costs.

¹⁰ A few studies also use registrant surveys and interviews to draw tentative conclusions about the effects of SORN laws. In these analyses, registrants are expressly asked about the effects of SORN on their behavior and sexual offending generally. We discuss this line of research later in the chapter when we address registrant beliefs about the effects of SORN on sexual offending.

In what follows, we summarize existing empirical research concerning the various behavioral dimensions we identify above – in particular, the frequency, composition, and geographic effects of SORN laws on registrant recidivism and on the deterrence of nonregistrants. Rigorous published research on the effects of SORN has accreted for almost 25 years; we cannot exhaustively cite, much less discuss, all of the relevant research. Instead, we aim to present an overarching perspective and the consensus views of the experts in the field. We note, where it exists, any evidence running counter to these views, and we catalog the reasons to feel confident in the reliability of the scholarship standing behind the consensus.

The body of research on the consequences of SORN laws strongly suggests that typical SORN laws have essentially no effect on registrant sex offense recidivism. While empirical work necessarily produces results that retain some margin of error, there is enough peer-reviewed research behind this conclusion to feel reasonably certain that any reduction in recidivism has been, at best, very small. If anything, there is stronger evidence that SORN laws – particularly community notification laws – counterproductively increase rather than decrease the likelihood that registrants will commit future sex crimes. While this possibility may seem counterintuitive, it emerges naturally from a comprehensive model of offending behavior. Sex offender registration and notification burdens can themselves be criminogenic in their effects, and unless the monitoring and precaution-taking benefits of information-sharing more than offset these negative consequences, we should expect to observe no change or perhaps even increases in re-offending rates.

7.3.1 SORN Recidivism Effects: Evidence from Federal Crime Reports

The two largest studies of the effects of SORN laws to date are Agan (2011) and Prescott and Rockoff (2011). Both studies use data from many states over many years and take into account offense- or offender-level information in at least some of their analyses. Both sets of authors explore the robustness of their findings and investigate the plausibility of various alternative interpretations of the patterns they identify in the data. Both pay careful attention to the differences in SORN laws across states and over time. In the end, neither study detects evidence that community notification (as opposed to registration alone) reduces sex offense recidivism. However, Prescott and Rockoff find some evidence that registration alone may reduce registrant recidivism and that community notification may actually increase registrant recidivism (with the two effects offsetting each other in states with relatively small registries).¹¹ To

¹¹ Agan (2011) and Prescott and Rockoff (2011) are seemingly the only studies to evaluate the independent effect of registration *alone* on registrant recidivism before Bouffard and Askew's (2019) Texas-based study, which we discuss later in this chapter. Recent work by Bieri and Budd (2020) examines the effect of registration alone on the closure speed of sex offense incidents reported as involving stranger perpetrators using incident-level data from six states. The authors present evidence that law enforcement use of registration databases is associated with a reduction in the time to case closure.

better explain the admittedly nuanced findings of Prescott and Rockoff's work, we describe their empirical approach and conclusions in more detail.

Prescott and Rockoff evaluate the implementation of SORN laws in fifteen states using data from the National Incident-Based Reporting System (NIBRS), the same data the FBI use to construct Uniform Crime Reporting (UCR) Program statistics. The authors separately collect and catalog the enactment and effective dates of both registration and notification laws (including various types of notification laws) to identify their possibly distinct effects on criminal behavior.¹² Prescott and Rockoff study offense reports to law enforcement agencies, which allows them to abstract away from any independent effects SORN laws may have on case processing.¹³ Because NIBRS includes information on the reported relationship between the offender and any victim, Prescott and Rockoff can assess SORN-induced changes in victim-type composition. Unfortunately, NIBRS data do not indicate whether an offender is a recidivist. Thus, to identify SORN effects on recidivism, Prescott and Rockoff incorporate registry size data by state over time as well as any retroactivity provisions affecting the scope of SORN coverage into their analysis. They reason that SORN laws cannot reduce recidivism in the immediate aftermath of their implementation when the provisions only apply prospectively to convicted or released individuals because the registry would be effectively empty on day one. They further assume that the relative importance of any recidivism effect grows with the size of the registry since any deterrence effect would at least plausibly remain constant over time.¹⁴ (We discuss the potential for SORN-law deterrence of non-registrants later in this chapter.)

Prescott and Rockoff find that registration alone appears to reduce registrant recidivism. They report that private registration laws are associated with an average

¹² Agan (2011) also recognizes the important distinction between registration and community notification in her work, but most other studies conflate the two in their data analysis, despite their potentially disparate effects. Today, all states use community notification, so it is understandable that most research has concentrated on the consequences of the public release of registrant information, but at the outset of the SORN era many states employed registration alone.

¹³ One perennial issue in crime research is the potentially distorting role of sharp changes or even trends over time in victim reporting behavior. Victims must report a sex offense to law enforcement for it to be recorded as an incident in federal crime data, for it to result in arrest and prosecution, and for it to be reflected on an individual's criminal record. Therefore, any measured change in the frequency of reported incidents will actually be the combination of any change in victim reporting propensity and any change in offending behavior. According to National Crime Victimization Survey data (Bureau of Justice Statistics, 2018), victim reporting propensity for sex offenses appears to have declined over the last twenty years, potentially as a consequence of SORN laws. Although this apparent trend may have other explanations, lower reporting rates would imply that any measured reduction in sex offense frequency might be the spurious result of fewer reports, not fewer crimes. By the same logic, evidence of SORN-caused increases in crime frequency may be biased downward (meaning increases in recidivism may be greater than measured) and a lack of evidence of any effect of SORN laws would presumably be consistent with higher offending rates.

¹⁴ This assumption is admittedly nontrivial because registry size may affect nonregistrant awareness of the registry and its effects. This might cut both ways; as a registry grows, it might become better known but also be considered less severe by potential registrants.

reduction in the reported sex offense rate of approximately 1.1 percent for each additional registrant per 10,000 people.¹⁵ Because registration laws require that any registry information be kept confidential, public monitoring and potential victim precaution-taking cannot explain this reduction in offense frequency. Given that there is little other rigorous work on the effects of registration alone, it is difficult to describe the study's result as a consensus finding.¹⁶ Moreover, because private registration without notification was in effect for only a short span of years at the dawn of the SORN era, relatively less information stands behind their conclusion. It is also impossible to know whether the detected reduction in recidivism was simply a temporary response to a new, untested form of police monitoring.

Using a similar empirical strategy, Prescott and Rockoff also evaluate the *marginal* effect on recidivism of sharing registry information with the broader public – that is, the added value of community notification. They find that notification appears to *increase* registrant recidivism (in terms of reported sex offense incidents) by about the same amount that registration alone appears to reduce recidivism, such that the two effects, operating together on the same registrants, roughly offset each other.¹⁷ Assuming that making registry information public neither *inhibits* victim precautionary behavior nor *reduces* the total monitoring level,¹⁸ the model suggests that notification laws may increase recidivism by increasing traditional risk factors, such as unemployment, unstable housing, and social isolation (reducing u_i). As we indicate in various places below, there is significant evidence that community notification effectively reduces the benefits of law-abiding behavior for registrants and thus may make recidivism relatively more likely.

Agan (2011) offers a useful comparison to Prescott and Rockoff (2011). The two analyses are complementary and mutually reinforcing because they study different states, use different data sources, and incorporate independently collected law and registry information yet draw the same basic conclusions about the effects of SORN laws on sexual offending. Agan relies on UCR data, allowing her to study a longer time period and almost all states, but her analysis is limited to the state-year level. By

¹⁵ But see note 13 on the role of changes in reporting propensities.

¹⁶ To our knowledge, one of the very few other studies on the effects of registration alone on recidivism is Bouffard and Askew (2019). They specifically study the effects on recidivism of Texas's private registration law in 1991 (notification did not arrive until 1995) using data only from Harris County, Texas. They find no evidence of private registration reducing recidivism. It is also worth mentioning recent NIBRS-based work by Bieri and Budd (2020), which focuses on identifying the effects of registration alone on the time it takes for law enforcement to close a case involving a stranger perpetrator. The authors' evidence that, conditional on the occurrence of a sex offense involving a stranger, registration alone may speed apprehension is distinct from evidence that registration reduces recidivism in the first instance, and indeed could be explained by higher recidivism levels among less careful sex offenders, but both outcomes are admittedly policy relevant.

¹⁷ Importantly, this makes Prescott and Rockoff's results consistent with other work that treats registration and notification as a single policy.

¹⁸ Notification might have this latter repercussion, for example, if the release of registry information causes law enforcement to rely on the public to look out for itself, which it might do poorly.

contrast, Prescott and Rockoff parse their NIBRS data by month, county, and even reporting agency, but they analyze a shorter sample period and must work with data from many fewer states. Agan also codes registration and notification laws differently; she treats registration law start dates much as Prescott and Rockoff do, but she considers community notification to begin in earnest only when registry information is posted online, which, given how much easier internet registries are to access and their social salience, may be a more appropriate choice. Any earlier forms of notification are commingled with the use of registration alone, and later forms of more active notification (e.g., receiving a postcard indicating that a new neighbor is a registrant) are not treated separately, but Agan also avoids any bias that may result from misattributing laws to the fastidious categories Prescott and Rockoff employ.

In her UCR-based research, Agan principally studies the overall effect of registration and notification (via the internet) on total offense frequency rather than their effect on recidivism specifically. She finds no evidence that registration and internet notification reduce the total frequency of reported rape incidents; if anything, the data Agan studies suggest that SORN laws are associated with more reported rape. Unfortunately, UCR data only contain incident-level information for rape, which is underinclusive of all registerable sex offenses. Agan addresses this data limitation by also studying UCR arrest reports for a broader category of sex offenses and finds some evidence that internet notification reduces the number of sex offense arrests. But this result is difficult to interpret because the relationship may capture only a reduction in the arrest rate and not in the number of underlying incidents. More importantly, the reduction in arrests might simply capture the deterrence of nonregistered potential offenders. Indeed, when Agan attempts to distinguish recidivism effects from deterrence effects in her work (using the same strategy, although not the same data, as Prescott and Rockoff), the reduction in arrests disappears, leaving a positive, statistically significant effect on recidivism.¹⁹ Although Prescott and Rockoff focus primarily on offense rates, when they conduct a similar analysis of sex offense arrest rates, their results are similar to Agan's arrest-rate findings.

There are a handful of other multistate studies that examine the effects of SORN laws using federal data like the UCR and NIBRS, but none of them raises serious questions about the reliability of the conclusions in either Agan (2011) or Prescott and Rockoff (2011). Vasquez, Maddan, and Walker (2008) is the earliest of these other studies.²⁰ The

¹⁹ Although much of the research on SORN laws studies the effects of the laws on arrest and conviction rates, these outcomes are less attractive in some sense because any measured effect also captures any behavioral changes by police, prosecutors, and other criminal justice actors.

²⁰ Zgoba, Witt, Dalessandro, and Veysey (2008) is a single-state study, but it uses time-series methods like Vasquez, Maddan, and Walker (2008) to examine trends in UCR offense reports in New Jersey counties. The authors conclude that 1994, the year New Jersey enacted the original Megan's Law, is the statewide change point in sex offense trends (with offense rates going down after 1994). This appears to be a coincidence, though, with only two counties actually showing 1994 as the peak in offenses (and only three other counties showing 1993 or 1995). Other factors may therefore account for the changes, and the authors do not control for other differences across counties. Moreover, the authors cannot distinguish between changes in recidivism and changes in deterrence.

authors evaluate the effect of “registration and notification laws” on UCR rape reports from 1990 to 2000 (at the state-year level) using time-series methods. Their evidence is mixed at best. For most of the states they evaluate, they detect no impact of SORN on reports of rape, though they do observe some evidence that SORN may have led to fewer offenses in three states. However, their analysis conflates registration and notification laws and does not separately consider the recidivism of registrants and the deterrence of nonregistrants. In addition, their coding of SORN laws does not appear to be congruent with the timing and scope of actual state enactments.²¹

Ackerman, Sacks, and Greenberg (2012) conduct an analysis similar to Agan (2011) for UCR-reported incidents of rape, though they do not distinguish between registration and notification for at least some states and they do not account for any deterrence effects on nonregistrant behavior. They do, however, control for other changes in relevant criminal justice policies, including the rather common implementation of sexually violent predator commitment laws at the state level. They similarly find no effect of SORN laws on reported rape. Most recently, Sandler, Letourneau, Vandiver, Shields, and Chaffin (2017) study the effects of SORN laws on juvenile offense counts using time-series methods and NIBRS data from four states. Their study also finds no evidence of any effects on sexual offending.²²

To reiterate, none of these studies – despite different methods, data, and assumptions – detect any reliable reduction in sex offense recidivism as a result of SORN

²¹ For instance, Vasquez, Maddan, and Walker (2008) treat Idaho’s intervention as occurring in 1993, but registration also became effective at this time, and public access to registrant information was only available upon written request. Thus, Vasquez, Maddan, and Walker’s findings may speak more directly to the effects of registration alone on recidivism. They also treat Ohio’s and Hawaii’s “registry” interventions as occurring in 1998, but notification in those states was limited in its coverage and actually became effective in the middle of 1997, not 1998. Maurelli and Ronan (2013) also employ this strategy, using a longer time series (UCR data from 1960 to 2008). They find quite different results. In particular, after correcting the dates for Ohio, Idaho, and Hawaii, the findings of fewer sex offenses in these states disappear. However, the larger data set allows them to conduct a similar analysis for forty-nine states, and they claim to offer other evidence that SORN laws reduce the frequency of forcible rape. While they do not conclude that notification laws necessarily reduce sex offense frequency across all states, they contend that seventeen states out of forty-nine show some evidence of a reduction. Yet, like Vasquez, Madden, and Walker (2008), Maurelli and Ronan’s results *also* commingle recidivism and first-time deterrence effects. There may also be mistakes in their coding of state laws. For instance, among the states for which they find reductions in forcible rape rates, which they specifically attribute to notification laws, they record Virginia’s SORN law as beginning in 1994, but public access did not commence in that state until 1998. Likewise, they combine private registration and notification, recording Delaware, Michigan, Missouri, and South Carolina as beginning notification in 1994, 1995, 1995, and 1994, respectively, instead of 1999, 1997, 1999, and 1996. Texas is miscoded in the opposite way: Maurelli and Ronan code Texas as beginning its intervention in 1995. This is when notification began, but registration started in 1991. And they code Florida as 1997 instead of 1995.

²² A working paper by Shao and Li (2006) using UCR data to study the effect of registries on the frequency of sex offenses was never published and is no longer easy to locate (and so we do not describe its findings, which are subject to criticism). The working paper is cited often in the early literature, however, so we include it here for completeness. Shao and Li’s draft is cited and discussed in Prescott and Rockoff (2011, pp. 163–65, note 2, 4, 5).

laws, either when evaluating registration and notification *combined* or when evaluating community notification on its own. Importantly, none of these multistate federal crime data studies – other than Agan (2011) and Prescott and Rockoff (2011) – attempt to separately identify the effects of SORN laws on registrant recidivism versus the laws' potential to deter nonregistrants. Because these two effects may point in opposite directions, undifferentiated results are consistent both with (1) neither registration nor notification having any effect on offense frequency and with (2) their just having no *net* effect when their individual effects are aggregated. In other words, any crime-reducing effects of registration alone may be offset by the plausible criminogenic effects of notification, as found by Prescott and Rockoff (2011). In that sense, all the studies are consistent with each other.

7.3.2 SORN Recidivism Effects: Evidence from Criminal History Data

Another way to assess the effects of SORN laws on recidivism is to study individual-level criminal histories — that is, assemble the subsequent criminal behavior of individuals previously convicted of sex offenses and compare the future offense trajectories of those actually subject to SORN to the trajectories of a control group of individuals not subject to SORN. Most papers studying the effects of registration and notification on sex offense recidivism rely on this empirical strategy. This is not surprising, given that it may be the most direct, natural, and intuitive way to evaluate the effects of SORN on sex offense recidivism levels.

From the perspective of policy makers, however, this approach has significant limitations. One drawback of focusing exclusively on individuals with registration-qualifying sex offense *convictions* is that the analysis discounts any behavioral effects the laws might have on nonregistrants. Another is that it is unable to isolate the impact of SORN on all reported sex offense incidents; the approach can only analyze detected criminal behavior that is also linked to a particular offender (e.g., an incident resulting in an arrest). Criminal records only contain data on arrests, charges, convictions, and sentences. Thus, any measure of the “effect” of SORN laws using this approach is actually an aggregate of effects on offender behavior, law enforcement success in apprehension, and case processing efficiency and accuracy. An increase in sex crime arrests following the enactment of SORN laws could imply *greater* levels of recidivism caused by the burdens SORN imposes (reducing u_i), or it could signify improved law enforcement ability to detect and apprehend recidivists (increasing p_{ij}). Still, for better or worse, researchers have not faced this dilemma in practice in light of their findings: the dozen-plus studies in this category all essentially conclude that SORN laws have no effect on the likelihood that a registrant recidivates, regardless of the recidivism measure one prefers.

Agan (2011) includes the largest analysis of this sort. She leverages Bureau of Justice Statistics (BJS) data that follow all individuals imprisoned for a sex offense *and* who were released from state prisons in 1994 in fifteen states (Langan, Schmitt,

and Durose, 2003). Agan takes advantage of the fact that some individuals were released in states where SORN laws were already in effect (and were therefore required to comply with SORN) while others were released at the same time but were not subject to SORN because such laws were not yet enacted where they were released. Agan examines any differences in the likelihood and timing of rearrest for rape, rearrest for any sex offense, and reconviction for any sex offense. Despite her large sample size, Agan finds no statistically significant difference between the two groups of releasees on any of these measures. If anything, the results hint at *higher* arrest and conviction rates for those subject to SORN laws. Importantly, this post-SORN increase in “recidivism” outcomes is more consistent with registrants committing additional sex crimes than with better police monitoring leading to more arrests per incident (but fewer incidents overall) because in Agan’s analysis the individuals subject to SORN laws also appear more likely to be arrested for non-sex offenses (i.e., pointing to SORN being generally criminogenic). On the other hand, the timing of Agan’s analysis (the offenders in the study were released circa 1994) means that the implicated SORN laws mostly entailed registration alone, at least at the inception of the recidivism window. Thus, any increase in arrest or conviction rates is also consistent with Prescott and Rockoff’s (2011) finding that registration alone may have public safety benefits via improved law enforcement monitoring and apprehension effectiveness.²³

Other studies using individual criminal histories to examine recidivism effects analyze data (and laws) from just one or two states and typically rely on a pre- and post-implementation comparison strategy. By and large, these studies address the effects of notification, not registration. The authors in these papers essentially compare the offending trajectories of those subject to notification laws to the trajectories of those not subject to them, using various methods to try to make these different groups otherwise comparable. Most of these studies have small sample sizes and assume away the possibility of other confounders. This latter assumption is troublesome in a single-state scenario because the comparison-group members usually committed crimes, were convicted, or were released at a different point in time. This is particularly problematic in single-state settings when other policy interventions in the jurisdiction accompany the introduction of notification. But, despite their potential shortcomings, these empirical studies still tell the same consistent and mutually reinforcing story: they find no evidence that notification has any effect on recidivism.²⁴ Where there is any glimmer of evidence in one direction or another, it is consistent either with an increase in recidivism or

²³ Again, Agan’s sample of potential recidivists includes only individuals released from prison, so one must take into account the fact that individuals who serve a prison sentence following a sex offense conviction are only a subset – admittedly, surely a very large subset – of all registrants and were probably convicted of more serious crimes on average. Some number of registrants may spend time only in jail and still others may never be incarcerated for their crimes. Therefore, to be more precise, SORN laws do not appear to reduce the probability of re-offending at least for state prison releasees.

²⁴ None of these studies are able to examine the deterrence effects of SORN laws on nonregistrant behavior. We discuss the smaller body of research on this question later, in Section 7.3.5.

with the idea that only narrow, risk-based public notification regimes are effective (as in Minnesota, see below).

Schram and Milloy's (1995) work appears to be the first empirical study to evaluate the effects of notification on recidivism using criminal history records. They study a small sample of public registrants ($n = 125$) from one state – Washington, one of the earliest adopters of community notification, which at the time subjected only high-risk registrants (approximately 20 percent of all registrants) to notification.²⁵ They detect no difference in sex offense rearrest rates for those subject to SORN laws as compared to similar individuals convicted of a sex offense and released prior to the implementation of Washington's SORN law.²⁶ Adkins, Huff, and Stageberg (2000) similarly compare recidivism outcomes of 233 registrants to those of 201 individuals convicted of a sex offense prior to the implementation of the registry law and find no evidence of any effect on their measures of recidivism over an average of 4.3 years. Zevitz (2006) uses plausibly exogenous variation in local policies to study the effect of "extensive" notification relative to more limited forms of information disclosure in Wisconsin ($n = 213$). Like others, he finds no differences in arrest or reconviction rates across the groups. Maddan (2005; 2008) and Maddan, Miller, Walker, and Marshall (2011) investigate the potential effects of notification in Arkansas on almost 3,000 registrants over 5 years. They, too, find no effect of that state's SORN law on the likelihood of rearrest or reconviction for a sex offense.²⁷

In an important and well-known study, Sandler, Freeman, and Socia (2008) evaluate the effects of New York's SORN laws on sex offense recidivism by tracking the behavior over time of all individuals convicted of registerable sex offenses. They capitalize on the fact that only those convicted or released post-1996 would be subject to the state's registration and notification laws. The authors are able to

²⁵ Barnoski (2005) revisits data from Washington. He examines whether there had been a change in sex offense recidivism (measured by a new conviction) over time (during which a SORN law had been implemented), but he does not explicitly study the role of an individual's notification status. He also admits that any reduction in sex offense recidivism over time may have many causes, including presumably lower enforcement levels or other secular trends in offending behavior.

²⁶ But Schram and Milloy (1995) do detect some evidence suggesting that registrants subject to notification were more likely to be rearrested for *non-sex* offenses. There are two potential interpretations of these findings. The first is that those subject to notification were disproportionately burdened by SORN publicity (through, e.g., unemployment and housing difficulties) and therefore became more at risk of committing non-sex crime – possibly property, auto theft, or other "profitable" crimes (consistent with a lower u_i in our model). The second is that non-sex recidivism patterns provide a baseline for what we should expect to occur with sex offense recidivism absent notification. With this interpretation, their finding implies that SORN laws effectively produced lower sex offense recidivism rates. Schram and Milloy give us no good reason to believe this interpretation, however, and one might *also* expect SORN-related monitoring by police to have effects on non-sex recidivism. Perhaps a more important limitation of the study is the narrow use of notification in this early Washington State SORN law, targeting only the 20 percent of registrants deemed particularly high risk.

²⁷ Like Schram and Milloy (1995), these two studies find some evidence that notification is associated with an increase in non-sex crime recidivism, a pattern consistent with its burdens being criminogenic. Letourneau and Armstrong (2008) find something substantively similar using data from South Carolina in the juvenile SORN context.

examine the role that registrants play in all sex offenses for which there is a recorded arrest (an important caveat) and test whether the relative significance of recidivism in accounting for all cleared sex offenses changed after community notification went into effect. They find no evidence that SORN laws affect recidivism levels (if anything, their findings indicate that SORN laws increase sex offense recidivism). Crucially, their study also reveals little room for potential improvement on this dimension. Specifically, reducing recidivism can only make a small contribution to reducing total victimization levels in their data because approximately 95 percent of sex offense arrests in New York involved individuals with no sex offense-related criminal history (and who were thus not subject to registration).

Subsequent studies – using an assortment of methods and criminal history data – also determine that SORN laws (particularly notification) have essentially no effect on recidivism. For example, Letourneau, Levenson, Bandyopadhyay, Armstrong, and Sinha (2010) use a survival analysis framework and data from South Carolina in the 1990s and 2000s to assess whether SORN laws influence recidivism rates. The authors argue that, unlike other traditional predictors of criminal recidivism risk (e.g., age and criminal history), registration status does not affect the likelihood or timing of another sex crime arrest or conviction.

Other studies arrive at the same verdict. Tewksbury and Jennings (2010) consider a large, matched sample of pre- and post-SORN individuals convicted of a sex offense in Iowa. Because notification laws may affect the behavior of different types of potential re-offenders (e.g., high risk v. low risk) differently, Tewksbury and Jennings sort everyone into a risk group based on their future offense trajectory and then they compare and contrast the composition of the risk groups pre- and post-SORN. They show that SORN laws have no apparent effect on any group's recidivism risk nor do they seem to alter the composition of the groups. Zgoba, Veysey, and Dalessandro (2010) test for differences in recidivism behavior (using arrest, conviction, and incarceration measures) in New Jersey ($n = 550$) according to notification status. They find no differences with respect to sex offense recidivism. Tewksbury, Jennings, and Zgoba (2012) apply the methods of Tewksbury and Jennings (2010) to the New Jersey data studied in Zgoba, Veysey, and Dalessandro (2010), again finding no evidence that SORN affects recidivism patterns either on average or for particular groups of potential offenders.²⁸

Reaching the same conclusion by a slightly different inferential path, Freeman (2012) examines pre- and post-notification recidivism patterns over time for New York registrants and finds that those subject to community notification (i.e., high-risk registrants) were likely to be arrested twice as quickly as comparable individuals who – because they committed their registerable sex offenses prior to

²⁸ Zgoba, Jennings, and Salerno (2018) return to this New Jersey criminal history data, using a similar methodology on a longer trajectory period – 15 years (sample size = 547). The authors uncover no evidence that SORN laws have any effect on recidivism, even when they assume there might be heterogeneous responses, with SORN effects perhaps emerging for only a particular subgroup.

the law's effective date – were not subject to notification.²⁹ Freeman acknowledges that this finding could be read to be inconsistent with the consensus of the foregoing scholarship: even if community notification does not reduce sex offense recidivism (as measured by arrests), it might nevertheless improve law enforcement *detection* of sex offense recidivism, which would presumably reduce harm in the future. But Freeman also posits other interpretations that are equally consistent with these patterns in the data, including that notification may either lull potential victims into a false sense of security (reducing p_{ijt}) or exacerbate recidivism risk factors (reducing u_i), or both. These scenarios would lead to additional *incidents* for which a registrant is at risk of being arrested (if these are marginal crimes – and therefore more risky on average – it would make even more sense that arrest would typically occur earlier under notification). Bolstering this latter reading, Freeman's research also indicates that being subject to notification is associated (albeit imprecisely) with higher non-sex offense recidivism as well, so more recidivism leading to earlier arrests on average seems most likely to be the correct explanation, which Freeman readily accepts.

Levenson and Zgoba (2016) examine sex offense rearrest rates in Florida, deploying an approach somewhat similar to that of Sandler, Freeman, and Socia (2008). Levenson and Zgoba use the aggregate fraction of rearrests (for the same type of crime as an earlier conviction) over total arrests in a given crime category as a recidivism index. They investigate whether SORN laws cause the index for sex offenses to decline relative to the indexes for other crimes between 1990 and 2010. They uncover no evidence to support this possibility – in fact, they find that sex offense rearrest rates increase, although the rate of increase post-SORN was not statistically different from the comparable rate of increase for other crimes. Bouffard and Askew (2019) follow in Levenson and Zgoba's footsteps by conducting a time-series analysis (1977–2012) of recidivism in Harris County, Texas. They find that, while the total number of sex offense charges increased absolutely over time, interrupted time-series methods indicate that SORN-law enactments and amendments played no role in this increase. Specifically, Bouffard and Askew detect no change in the number of charges against repeat offenders, against first-time offenders, for all offenses against children, or for all sexual assaults.

Duwe and Donnay (2008) provide the only potentially probative evidence in support of notification-induced recidivism reduction. They study Minnesota's early, highly targeted notification regime (covering only 10 percent of all individuals convicted of a sex offense) using a small sample ($n = 155$) of high-risk registrants. With these caveats, and after matching these high-risk potential recidivists to arguably comparable sets of pre-notification and lower-risk non-notification individuals, the authors conclude that notification is associated with lower sex offense recidivism. Duwe and Donnay also present evidence suggesting that re-offenders covered by notification are arrested and reconvicted sooner.

²⁹ Using New York data, Freeman and Sandler (2010) examine whether male recidivism rates differ across Adam Walsh Act risk tiers. They find that they do not but leave open the possibility that better risk-tiering could be used to target those most likely to recidivate, possibly with positive results.

To our knowledge, Duwe and Donnay's 2008 work is the only plausibly credible evidence that – under some circumstances – notification may reduce recidivism. However, context and lack of data reduce the salience and significance of the paper's findings. For instance, all potential re-offenders in Minnesota during the study's "post period" (both treatment and control groups) were subject to active notification provisions of some sort, and the pre-notification comparison group's behavior came earlier in time and so these individuals may have faced a very different environment. Furthermore, significant discretion existed under Minnesota law in how registrants were assigned to notification during the study period, which may interfere with control-group comparability. In addition, the substantive requirements of Minnesota's SORN laws changed between 1997 and 2000; these changes, which include the launch of an internet registry in 2000, may contaminate Duwe and Donnay's inferences. Finally, Duwe and Donnay are unable to offer evidence on the relationship, if any, between notification and total sex offense frequency, which opens up their results to alternative interpretations.³⁰

7.3.3 *SORN Recidivism Effects: Alternative Approaches*

Agan and Prescott (2014) adopt a new approach to identifying notification's effects on recidivism using the preexisting variation in where registrants live in Baltimore County, Maryland, and the area's sex offense incident report data.³¹ They find that, on average, reported sex offenses rise in neighborhoods with more registrants relative to neighborhoods with fewer registrants following the implementation of Maryland's notification regime. This result is at a minimum consistent with the idea that community notification leads to greater levels of recidivism.

Agan and Prescott include two crucial caveats in their work. First, while the average number of reported sex offenses increases post-notification in neighborhoods that are dense with registrants, the number of reported adult rapes and sex offenses against children *falls* with notification in those neighborhoods relative to other neighborhoods. This pattern could indicate either a reduction in recidivism or displacement (in which registrants travel to other neighborhoods where they remain unknown in order to commit crimes). Second, because Agan and Prescott investigate *reported* incidents, they cannot exclude the possibility that nonregistered potential offenders seek out neighborhoods with many registrants to commit crimes (assuming they will be more likely to evade detection as not the "usual suspects" in those neighborhoods). In the end, the authors concede that the ability of

³⁰ One alternative interpretation of these findings is that certain potential recidivists subject to notification became better able to avoid detection (perhaps by offending outside of their neighborhood), in which case Duwe and Donnay's findings of "reduced recidivism" would actually represent just a reduction in law enforcement detection.

³¹ Agan and Prescott's (2014) identifying assumption is that any difference in sex offense frequency between neighborhoods where registrants live relative to the frequency in neighborhoods where they do not live can be taken as a proxy for registrant recidivism.

nonregistrants to react to a notification regime makes it difficult to draw strong inferences about the effects of online notification on recidivism levels using their approach; however, their findings do allow them to say something precise about overall risk levels in registrant-heavy neighborhoods.³²

Also taking a different approach, Carr (2019) uses plausibly exogenous variation in the duration of registration to study the effect of SORN laws on recidivism. In 2006, North Carolina extended the length of its registration requirement for anyone who was *currently* registered as of December 1, 2006, which effectively randomly kept a group of slightly more recently registered individuals on the registry for an additional 20 years. Using sex offender criminal history data, Carr finds no evidence of any difference in the likelihood of sex offense recidivism (after 10 years) between those required to register for 10 years versus those who remained subject to SORN for many more years. Her work suggests that applying SORN requirements more than 10 years following release has no public safety payoff.

7.3.4 *SORN Recidivism Effects: Summary*

Dozens of studies to date have sought to assess whether and how SORN laws affect sex offense recidivism. Multistate studies – some national in scope – using federal crime data and deploying panel data methods or time-series approaches have found no evidence that notification reduces recidivism and some evidence that it may increase recidivism. In single-state studies, using many different empirical research tools and data sources and examining different measures of sex offense recidivism in different jurisdictions (including Arkansas, DC, Florida, Iowa, Maryland, Minnesota, New Jersey, New York, North Carolina, South Carolina, Texas, and Wisconsin), researchers from different disciplines, working independently, have essentially all failed to detect any evidence that notification reduces recidivism. The sole exception to this scholarly consensus is Duwe and Donnay (2008), who, using a small sample of potential recidivists, in a jurisdiction (Minnesota) employing an atypical, narrow, individual risk-based approach, find at least some nontrivial evidence that notification may reduce recidivism.

By contrast, evidence on the effects on recidivism of registration alone is more mixed. Although Prescott and Rockoff's (2011) results indicate that registration may reduce registrant recidivism, Agan's (2011) analysis of mostly private registration laws does not detect any effects worthy of comment. Some support for registration alone can also be gleaned from the studies that conflate registration and notification (e.g., Vasquez, Madden, and Walker, 2008). But generally we have far less evidence on registration's efficacy at reducing recidivism than we do on the recidivism-reducing

³² See also Agan (2011), who conducts a similar type of study in Washington, DC, although she only has information on registrants' current addresses and therefore has to assume registrants do not move very often over time. Agan finds no evidence that the risk of victimization changes near where registrants live when the city implemented community notification.

effectiveness of notification, and there are decent reasons to surmise that the former might succeed where the latter fails. We need more research.

7.3.5 SORN Effects on Nonregistrant Criminal Behavior

Perhaps because the principal aim of SORN laws is to reduce sex offense *recidivism* by registrants, there is much less work on the deterrence effects of SORN laws on *nonregistrants* – that is, whether the threat of becoming subject to registration and notification upon conviction for a sex crime might cause potential sex offenders who have never been convicted (or are at least not currently registered) to offend less often on average. In theory, SORN laws might discourage potential “first-time” sex offenders from ever offending in the first place, which would be important because the overwhelming number of sex offenses are committed by first-time sex offenders (Sandler, Freeman, and Socia, 2008).

However, it is appropriate to be skeptical of this possibility. Evidence is very thin that *lengthening* already long prison sentences improves deterrence (see, e.g., the review in Durlauf and Nagin (2011)), and sex offense sentences are generally long. Because the burdens of SORN laws on those newly convicted of sex offenses would come *after* any prison sentence (potentially many years in the future), any deterrence gains appear likely to be second-order. This seems especially true for any threatened use of registration alone, which potential offenders may not distinguish from parole and supervision conditions generally (if indeed they are aware of the requirement at all prior to their first sex offense conviction). But notification has greater potential to change nonregistrant behavior. The prospect of being listed publicly on a registry as a sex offender is quite salient. A potential offender may not distinguish between prison sentences of say 10 and 12 years but may very well view at least 10 years of notification – and the public pariah status it carries – as a much worse consequence than two additional years of prison.

While relatively little evidence exists on the deterrence effects of registration alone, available data indicate that any beneficial consequences are trivial at best. Prescott and Rockoff (2011) consider the possibility, using NIBRS data from a dozen states, but they find no evidence of any first-time offense deterrence effect. Using a similar approach and data from UCR states, Agan (2011) also fails to uncover any evidence that the threat of early, pre-internet SORN laws deters potential first-time sex offenses. Bouffard and Askew (2019) examine the impact of a registration law implemented in Harris County, Texas, in 1991 using interrupted time-series methods (notification did not commence until sometime in 1995) and similarly detect no evidence of any effect on the number of first-time offenses. Other studies of registration either do not address first-time offense deterrence (focusing only on recidivism) or actually speak only to the deterrence effects of notification policies.

More research exists on the first-time deterrence effects of community notification (or a combined measure of registration and notification). The evidence that notification deters first-time offenses is best characterized as mixed, but unlike the less-

than-negligible possibility that community notification may reduce sex offense recidivism, there is enough evidence of socially beneficial deterrence effects resulting from notification to take the idea seriously. Importantly, studying first-time offense deterrence questions requires data on the total number of sex offense incidents committed, reported, charged, or resulting in conviction, not just instances of sex offender recidivism. There are at least two strategies by which one could draw inferences about the effects of SORN laws on the criminal behavior of nonregistrants. Both approaches require assumptions, so they are usefully juxtaposed.

First, one could start with data on all sex offenses that include an indication of whether the offender in question was previously convicted of a qualifying sex offense (as in, e.g., Sandler, Freeman, and Socia (2008)). With these data, one can essentially track the number of offenses committed by *nonregistrants* during any period of time. One can then evaluate whether SORN implementation changes this number (or the relative importance of this number). Unfortunately, data sets like these are rare.³³ Perhaps the first study to use this strategy to focus significant attention on first-time sex offense behavior is Sandler, Freeman, and Socia (2008), which relies on criminal history data from New York. Besides calculating that over 95 percent of sex offenses in New York are committed by first-time offenders (again, making clear the importance of inhibiting first-time offending), they find no evidence to support the claim that SORN laws reduce the total number of first-time offenses. But the study's results do reveal some heterogeneity across types of crimes. In particular, the authors estimate a large negative effect of notification for first-time child molestation offenses. The coefficient, which is only borderline statistically significant ($t\text{-stat} = -1.81$), offers at a minimum tepid support for notification's deterrence effects, at least with respect to these particular crimes.

Building on this research, Letourneau, Levenson, Bandyopadhyay, Armstrong, and Sinha (2010) examine similarly structured South Carolina data. They identify 1995 as the year that South Carolina implemented a public sex offender registry (via mandatory public access to paper-based registry information). Unlike Sandler, Freeman, and Socia (2008), these authors present more consistent evidence that notification results in fewer first-time offenses. The authors report that first-time arrests – not incidents³⁴ – drop by 11 percent relative to the pre-notification era.

³³ To our knowledge, researchers to date have only been able to acquire data of this sort for a single state at a time. Moreover, an additional limitation of this approach – a limitation that runs throughout empirical work on criminal behavior – is that it can only account for changes in the number of first-time offenses *resulting in an arrest*. After all, apprehension (and possibly conviction) is necessary for the researcher to be able to discern whether the offense in question was committed by a first-time offender. If many sex offenses do not result in (accurate) arrests or convictions, the researcher is forced to make assumptions about the stability over time of sex offense clearance rates.

³⁴ It is worth remembering that the number of arrests for sex offenses can drop post-SORN whether or not the number of actual offenses drops, such as when there are SORN-induced reductions in sex offense reporting by victims or less effective police investigation of reported crimes or apprehension of suspects. See note 13.

However, they fail to observe any incremental reduction in first-time offenses from the launch of an online registry in 1999, which one might have expected to generate the largest impact given the salience of internet registries and the publicity surrounding their launches.³⁵ This inconsistency may imply either that some early versions of public access laws are equal to internet registries in their deterrence potential or that the drop in first-time offenses post-1995 was due primarily to chance. To explore these possibilities further, Park, Bandyopadhyay, and Letourneau (2014) return to these South Carolina data with new methods and find more convincing evidence in favor of deterrence of first-time offenses following the launch of the state's online registry in 1999. In any event, whether we read these three papers together as presenting conflicting evidence or as reinforcing the idea that notification laws may be able to deter potential first-time sex offenders, this research is only able to speak to the laws' effects on the frequency of sex offenses that lead to *arrests* because they all rely on criminal history data.

A second approach to examining first-time deterrence effects is to use data on all reported sex offenses in a particular jurisdiction and leverage the number of individuals listed on the relevant sex offender registry to isolate any SORN effects on first-time offenses. In essence, this empirical strategy assumes that any SORN effects on sexual offending that vary with registry size (whether private or public) over time and across jurisdictions (controlling for other potential confounders) are attributable to changes in levels of sex offense *recidivism*. In other words, this research design assumes that the size of the registry does not predictably affect nonregistrant behavior.³⁶ If this assumption is approximately correct or if any departures are likely to offset each other, the residual – what stays constant across registries of different sizes at different times – captures the fixed “threat” of future SORN burdens (deterrence) if one is convicted of a sex offense.

Prescott and Rockoff (2011) use NIBRS data on all reported incidents from fifteen states – exploiting the variation in registry sizes over time and across states – to test for

³⁵ Letourneau, Levenson, Bandyopadhyay, Armstrong, and Sinha (2010) find no independent reduction in sex offense arrests beginning in 1994 (p. 547), the year in which private registration began (in July) in South Carolina, adding another attempt to test for first-time deterrence effects of registration alone to Prescott and Rockoff (2011) and Agan (2011). Separately, Letourneau, Bandyopadhyay, Armstrong, and Sinha (2010) study first-time deterrence effects of community notification in the context of potential first-time *juvenile* offenders. They conclude there is little evidence that notification reduces first-time juvenile arrests for sex offenses, especially in light of other procedural changes affecting juvenile prosecutions in South Carolina around that time that make it difficult to draw reliable inferences from their data about notification specifically. More recently, Letourneau, Shields, Nair, Kahn, Sandler, and Vandiver (2018) study juvenile charges and adjudications in Maryland and Oregon and find no evidence of SORN-related first-time deterrence effects.

³⁶ Prescott and Rockoff (2011, p. 170, n. 17) acknowledge that this assumption may not hold. For instance, large public registries may be more salient to the public – i.e., nonregistrants – in some way. Or large registries may instead lead to registry fatigue, habituating the public to the existence of known registrants. In either case, we might expect any deterrence of first-time offenses to vary with the size of the registry. Even so, the latter possibility does not appear to have occurred yet, and it is certainly unclear whether an individual (especially someone at risk of committing a sex crime in the future) is more (or less) likely to be aware of SORN obligations if the registry is larger or smaller given the public fanfare around sex offender laws and particularly the implementation of notification policies.

the existence of notification effects on the frequency of first-time offenses. They unearth sizeable effects (a 12.8 percent reduction), surprisingly close to Letourneau, Levenson, Bandyopadhyay, Armstrong, and Sinha's (2010) finding of an approximately 11 percent reduction. Prescott and Rockoff's results remain intact even when they focus only on the number of sex offense arrests. Agan (2011), employing the same identification strategy to examine sex offense trends in UCR states, also finds statistically significant evidence of first-time deterrence effects from the implementation of notification (specifically, internet registries). This finding is not inconsistent with Letourneau, Levenson, Bandyopadhyay, Armstrong, and Sinha's (2010) failure to discern internet-registry deterrence effects in light of Park et al.'s (2014) alternative results but also because internet registries in many states significantly increased the public availability of registry information and were more likely to be implemented concurrently with paper-based forms of notification in contrast to the timing in South Carolina.

Importantly, evidence of notification's beneficial effects on deterring first-time offenses cannot be viewed in isolation. There is little evidence that notification reduces recidivism and some evidence that it actually increases recidivism by exacerbating risk factors; meanwhile, very large numbers of people continue to commit first-time sex crimes, despite the threat of being subject to notification. Moreover, notification is financially costly to maintain and burdensome to many third parties, including registrants' families, employers, and landlords. In light of the foregoing, any first-time deterrence effect may not be worth the candle. Making simple calculations from their results, Prescott and Rockoff (2011) argue that the deterrence benefits of notification are likely offset by its criminogenic effects, even when notification applies to a relatively small population of registrants – specifically, to a registry that was below average in size in the United States around the year 2000 – far smaller than the public registries in almost every state today.

7.3.6 SORN Effects on Geographic and Victim Incidence

In the behavioral model we introduce in Section 7.2, we describe potential offender i 's decision to commit a sex offense not as something abstract but instead as a decision to commit a particular type of crime against a particular type of victim (j) in a particular location (l). Offenders may be willing to commit some crimes under certain circumstances (a combination of the offense type, the victim type, and the location) but not others depending on the probability of punishment (p_{ijl}), the level of punishment (f_{ij}), the cost of carrying out the crime (c_{ijl}), and the utility (benefit) of lawful behavior relative to sexual offending (u_i).

A critical but relatively understudied implication of this framework is that SORN laws may change *how* registrants (and nonregistrants) offend – which sex crimes, where, and against whom – without necessarily changing the overall frequency of sex offenses. While policy makers might be primarily interested in reducing the total

number of offenses, investigating these distributional consequences of SORN laws is important because – like any law with consequences that vary across people – these policies will create winners and losers.³⁷ Moreover, because potential offenders will likely respond to their legal and social environment in ways both expected and unexpected, policy makers ought to probe potential behavioral responses comprehensively and try to anticipate unintended consequences of SORN laws.

Agan and Prescott (2014) analyze whether SORN (and particularly notification) laws produce geographic shifts in where sex offenses occur and whether any such effects depend on the specific type of sex offense. They use historical data on registrants' residences, the locations of reported sex offenses (of different types), and the implementation timing of online notification in Maryland to study these questions. In particular, the authors match reported crime incidents to neighborhoods to observe whether neighborhoods with more registrants at a given time have more or less sex crime in subsequent periods.³⁸

Complex behavioral predictions emerge from a model like the one outlined in Section 7.2 of this chapter. With registration alone (marked by increased police but not public monitoring), we might expect fewer sex offenses to occur (on average and controlling for other differences) in neighborhoods with more registrants (relative to a world without registration – see Bernasco (2010)). In effect, registration makes it relatively less likely that registrants will be detected if they offend farther away from home where they remain unknown to law enforcement. Thus, assuming private registration does not affect where first-time offenses occur, sex offense victimization risk should be *lower* where there are relatively *more* registrants under registration alone. Essentially, registrants have incentives to avoid police detection by offending elsewhere (when it is not too costly to travel to another neighborhood), so high-density registrant neighborhoods should be safer all else equal.

Agan and Prescott (2014) find evidence consistent with this prediction in their data – indeed, using various neighborhood definitions, each additional registrant in a neighborhood appears to be associated with 7.5 percent fewer reported sex offenses in that neighborhood. This finding is consistent with both recidivism reduction (Prescott and Rockoff, 2011) and displacement of recidivism to other neighborhoods (Barr and Pease, 1990; Canter, 1996) as well as potentially non-SORN related explanations, such as the possibility that registrants are disproportionately more

³⁷ See, for example, Linden and Rockoff (2008), which identifies the negative home value consequences of community notification when a registrant lives nearby.

³⁸ Agan (2011) and Stucky and Ottensmann (2016) also address this descriptive question – whether, after taking into account other features of neighborhoods and their residents, it is accurate to assume that victimization risk is higher in areas closer to where registrants live – for Washington, DC, and Indianapolis, Indiana, respectively. Agan exploits changes in the community notification regime, but she has to make strong assumptions about residential continuity because of her fixed “snapshot” data on registrant addresses. She analyzes data on crimes from both before and after DC implemented online public notification. By contrast, Stucky and Ottensmann study a period in which registration and notification (but not yet residency restrictions) were already in place in Indiana, so they cannot speak to the *changes* in behavior resulting from implementing SORN laws.

likely to live in safer neighborhoods. Agan and Prescott's research accounts for some of the more likely alternative possibilities, such as the notion that neighborhoods with more registrants may contain fewer potential victims (such as children). They also investigate whether their estimates vary across different types of sex crimes (including forcible rape; other sex offenses against adults; peeping, prostitution, and pornography; and sex offenses against children). They find a lower frequency for all types of offenses in registrant neighborhoods, with the largest differences in their data emerging from peeping, prostitution, and pornography.

Agan and Prescott (2014) next evaluate how the relationship between victimization risk (based on reported sex offenses) and registrant density in a neighborhood changes when a jurisdiction implements community notification – in their case, Maryland's launch of its online registry. Their focus is less on how total offense frequency changes than on how the geographic distribution of offenses and, thus, the geography of victimization risk change in response to notification. Because notification makes registrant information available to the public (including to all prospective first-time offenders – that is, nonregistrants – whose sex offense behavior may also change in response to registry information and the threat of being subject to notification), the behavioral model's predictions are ambiguous.

If notification informs the public of the identities of nearby registrants (e.g., neighbors), making it easier to monitor and avoid them, one might expect registrants to be more likely to recidivate away from home, all else equal, because the likelihood of their crimes being detected (p_{ijt}) near home and the difficulty of committing crime locally due to SORN-generated victim precaution-taking (c_{ijt}) would be higher. Offsetting considerations include notification-driven increases in recidivism risk factors (u_i), which might be powerful enough to produce higher victimization levels in registrant neighborhoods. Then again, these risk factors may produce even higher levels in other neighborhoods. As always, any local reduction in sex offense frequency may be partially offset by crimes displaced to other neighborhoods.

Unexpectedly, depending on whether and how the law's potential beneficiaries use the registry's information, potential first-time offenders may find it either more or less attractive to offend in registrant-filled neighborhoods under a community notification regime (e.g., Kernsmith, Comartin, Craun, and Kernsmith, 2009). If the police and public focus on registrants and otherwise let their guard down, committing a first-time offense in a registrant-dense neighborhood might become more attractive. Alternatively, if monitoring and precaution-taking make all sex offenses harder to commit, potential first-time offenders might avoid places with more registrants, driving risk down in those areas. The geographic offending behavior of nonregistrants is particularly important because, as we note above, first-time offenders commit a very large percentage of sex crimes (Sandler, Freeman, and Socia, 2008).

Agan and Prescott (2014) analyze reported crime incidents, not criminal history information that would allow them to determine where recidivists and first-time offenders actually commit their crimes. Nevertheless, they are still able to

conclude that online notification moderately increases the total number of sex offenses in registrant neighborhoods (albeit not by enough to make those neighborhoods more risky than other neighborhoods on balance).³⁹ They acknowledge, however, that their estimates may be distorted somewhat by any effect online public registries have on victim reporting behavior, a recurrent problem in the study of sexual offending.

Agan and Prescott (2014) also investigate whether notification's effects on the geography of risk vary by sex offense type. They find that online registries seem to reduce the number of adult rape and child molestation incidents in registrant neighborhoods but perhaps by displacing them elsewhere – the number of rape and molestation incidents grows post-notification in other, less registrant-dense neighborhoods (consistent with Yeh (2015)). These findings counter alternative theories that rely on shifting victim reporting behavior. By contrast, other sex offenses, including peeping, prostitution, and pornography, tend to increase in registrant-dense neighborhoods following notification. This pattern may be a function of reporting-behavior changes, but Agan and Prescott (2014, p. 812) observe that notification really ought to affect the frequency and geography of different sex offenses differently: “[S]ex offenses are diverse. Some require planning or involve money; others are impulse crimes or involve intimidation or violence.” Perhaps because it implies inherent trade-offs between types of victims, this heterogeneity of effects has received little treatment in the literature. But the possibility that SORN laws may affect *where* offenders commit sex crimes as well as *whether* they commit a sex offense at all is too important for academics and policy makers to ignore.

Sex offender registration and notification laws may also affect whom offenders victimize. The laws were specifically designed to provide local law enforcement and community members with actionable information on registrants living or working nearby. Implicit in this policy approach are the ideas that (1) some people (e.g., family members) will already know of a potential recidivist's past crimes even without notification and (2) some people (e.g., strangers living in other neighborhoods) will be too far removed from a particular registrant to make effective precautionary use of registry information. If SORN laws work as intended, by influencing the behavior of police and potential victims thought likely to benefit from registry information, we should expect to observe disproportionately less recidivism occurring against victims in the target category – otherwise unknowing neighbors and acquaintances – than against family members and strangers.

Prescott and Rockoff (2011) explore this idea using the offender–victim relationship information recorded in NIBRS to determine whether there are heterogeneous effects of registration and notification across offender–victim relationship types. Specifically, they test whether SORN laws affect the frequency of sex offenses

³⁹ Yeh (2015) corroborates this basic finding using a similar approach with crime data and registrant address information in Lincoln, Nebraska.

against people likely to already be aware of a prospective registrant's criminal history (e.g., family, significant others, and friends), against those who could theoretically benefit from the information (e.g., neighbors, acquaintances, and otherwise known persons), and against individuals who are unlikely to be able to efficiently access or use a registrant's registry information (e.g., strangers). By allowing better local law enforcement monitoring and apprehension, and by informing those who can actually use registry information, SORN laws – if they operate as proponents argue they do – should reduce offense frequency against primarily the second set.

Prescott and Rockoff find evidence that registration alone reduces recidivism against acquaintances and neighbors, but it does not appear to reduce recidivism against either family members and friends or strangers.⁴⁰ Their results do not support the idea that registration displaces crime from acquaintances and neighbors to other victims such as strangers: while the estimated effect on stranger-crime frequency is positive, it is close to zero and not statistically significant. By contrast, they uncover no evidence that notification reduces recidivism against any group of victims. Instead, recidivism appears to increase across all groups of victims following notification's implementation, a finding coinciding with the supposition that the heavy burdens of notification render it criminogenic on the whole (reducing u_i). As the behavioral model predicts, the effects of notification on first-time offenses appear to be similar across relationship groups. Prescott and Rockoff argue these results bolster the causal interpretation of their findings on the overall effects of registration and notification on first-time offenses and recidivism. Their victim-type findings, moreover, are independently important because they constitute some of the only evidence on SORN laws' distributional consequences.

7.3.7 *SORN and Failure to Register*

A modest body of research has assessed the recidivism consequences of an individual's failure to register (FTR) upon release or to verify or update registry information at a later time, a serious crime under all SORN laws. It is often hard to find statistics on FTR offenses. In 2008, nationwide, an estimated 16 percent of individuals required to register were not in compliance with their registration obligations (Levenson, Letourneau, Armstrong, and Zgoba, 2010). When considering this estimate, it is important to recognize that many individuals who fail to register are not "absconders" running from the law or committing new crimes (Bierie and Detar, 2016). Indeed, because noncompliance is often unintentional, and only a paucity of

⁴⁰ This particular result seems to be in tension with recent evidence assembled by Bierie and Budd (2020) that registration alone reduces the time to closure in cases involving strangers. However, if the sole effect of registration is to reduce closure times by a day or two (in line with Bierie and Budd's estimates), it may have little effect on offender behavior. Indeed, their findings are even consistent with higher recidivism levels, if the additional offenses committed by stranger offenders are more marginal and therefore easier for law enforcement to detect.

research supports the claim that public notification reduces recidivism in the first place (and may in fact increase it), it is unclear whether FTR offenses should be an important public policy concern. Failure-to-register violations can be explained by the many burdens of satisfying registration requirements and the life circumstances of many registrants. Registrants move a lot (Yeh, 2015) and are more likely to live in poor, socially disorganized neighborhoods than nonregistrants (Mustaine, Tewksbury, and Stengel, 2006). All of these considerations suggest that failing to register may signal many things about those who are subject to SORN, but not necessarily that they are relatively more likely to recidivate.

Is there an empirical relationship between failing to register under SORN laws and recidivism? Does failing to register actually increase a potential offender's likelihood of recidivating? Put another way, can law enforcement use FTR violations to identify the registrants who are most likely to commit additional sex crimes in the future? The research presents a mixed picture. There is some evidence that failing to register increases recidivism risk, but it may signify that an offender is more likely to engage in crime generally rather than sex crime specifically. Unfortunately, there is little work on the recidivism effects of absconding, which only becomes an FTR conviction if the individual is later caught.

Barnoski (2006) examines FTR convictions in Washington State. Although details in the report are thin, Barnoski documents that FTR convictions are common, that it is difficult to predict from simple demographic characteristics who is likely to commit an FTR offense, and that FTR convictions are associated with higher felony, violent felony, and sex offense recidivism, at least in his sample (which exceeds 10,000 individuals). At odds with this research is Duwe and Donnay (2010), who use propensity score matching to contrast comparable FTR and non-FTR potential recidivists in Minnesota ($n = 1,561$). They find that FTR status is not predictive of recidivism except with respect to the crime of failing to register again. Failure-to-register risk grows with a prior FTR – a finding that is at odds with the conjecture that FTR occurs as a result of confusion about the process (i.e., lack of experience or sophistication) rather than a registrant's inattentiveness, challenging life circumstances, or intentional noncompliance.

Levenson, Letourneau, Armstrong, and Zgoba (2010) study the consequences of an FTR conviction for registrants (relative to compliant registrants) in South Carolina ($n = 2,970$). Their data show that an FTR conviction is predictive of general recidivism, but they find no evidence of any statistically significant difference in sex offense recidivism risk between the groups. The authors also use the time to re-offense to estimate survival models, and they find no difference in the timing of sex offense recidivism with this approach either.

Levenson, Sandler, and Freeman (2012) address essentially the same question using New York data ($n = 7,055$) and come to somewhat different conclusions. In their data, an FTR "charge" (presumably highly correlated with an FTR conviction) is associated with both general recidivism and sex offense recidivism, but the latter relationship is much weaker. And when sex offense recidivism is broken down into

new crimes against adult victims and new crimes against child victims, an FTR charge is only predictive of recidivism against adults. The authors maintain that FTR charges appear to be more indicative of general criminality than of sexual deviancy; they note that no element of an FTR charge is sexual in nature, and FTR charges are generally more common among registrants who are more likely to commit crime as a general matter. Socia, Levenson, Ackerman, and Harris (2015) develop empirical support for this idea by using Florida data to demonstrate that FTR convictions are associated with registrant transience, which they describe as a “major impediment to reentry success” (p. 559).

Finally, Zgoba and Levenson (2012) investigate FTR offenses using data from New Jersey. In a somewhat small sample ($n = 1,125$), they find no evidence that FTR predicts sex offense recidivism. They also observe that those who fail to register are more likely to have targeted adult female victims and are less likely to have been convicted of offenses against minors. This is consistent with Levenson, Sandler, and Freeman’s (2012) conclusion that FTR is predictive of sex crimes against adults but not against children.

7.3.8 *Concluding Thoughts on Empirical Evidence on the Effects of SORN Laws*

In this section, we have sought to summarize and integrate the empirical evidence on the effects of SORN laws on objective measures of sexual offending: either incident report data (usually multistate federal data such as UCR or NIBRS) or data coming from criminal history records at the state level (and in a few cases – e.g., Agan (2011) – from federal sources). Many researchers from different academic fields have deployed many empirical tools and sources of data from different jurisdictions for many years. And, at least with respect to community notification’s potential effects on recidivism, researchers have arrived at a consensus.

In a nutshell, while there is some tentative evidence that registration alone might reduce sex offense recidivism, there is almost no evidence that notification laws (of various sorts: online registries, active notification, etc.) reduce recidivism, despite many attempts to find support for the proposition. The one potential exception may be narrowly defined notification laws that operate only on high-risk registrants (for example, the top decile). Instead, in alignment with reentry theory and research more generally, the data indicate that the common and acute burdens of SORN regimes increase recidivism by exacerbating criminogenic risk factors and by reducing the comparative harshness of reconviction and incarceration. Notification may deter potential first-time offenders; becoming a “known sex offender” these days is tantamount to entering a pariah caste. But this ancillary upside is cold comfort: using notification to regulate those who are not deterred in the first place is costly, onerous, and may make potential recidivists more dangerous.

Sex offender registration and notification laws also influence behavior beyond simply increasing or decreasing sex offense frequencies. They likely alter how,

where, and against whom registrants and nonregistrants commit crimes. There is less work in this area, but extant evidence clarifies the importance of these distributional ramifications. And because these findings cohere with the basic findings on SORN frequency effects, they are a basic robustness check on the idea that, for instance, registration alone — but not notification — reduces recidivism in the way SORN proponents expect. Finally, FTR research shows, somewhat surprisingly, that FTR has little association with sex offense recidivism. Failure-to-register may be better characterized as a signal of general criminal tendencies or perhaps the common disorganized life conditions experienced by registrants. In any event, it does not mesh well with the all-too-common assumption that those who do not comply with SORN laws are necessarily incorrigible rapists or child molesters.

7.4 REGISTRANT BELIEFS ABOUT THE EFFECTS OF SORN LAWS

Another plausible way to explore whether SORN laws influence criminal behavior (and specifically recidivism) is to ask registrants directly for their views on this question. A fairly large literature taking this approach now exists, but the findings that emerge from this body of work are mixed. They are also subject to respondents' various cognitive and self-serving biases, and they often reflect inconsistencies even within the same study, undercutting their utility. In the end, however, such beliefs can affect behavior and may provide a lens through which we can better understand the research laid out in this chapter.

Perhaps surprisingly, a number of these studies document that at least some registrants believe that SORN laws can encourage offender desistance and enhance public safety. In an important sense, however, this apparent concession is fairly predictable. A large fraction of registrants reject the label “sex offender” as inappropriate when it is applied to them (ten Bensel and Sample, 2017), perhaps especially when they consider themselves wrongly convicted, not dangerous, or not “really” a sex offender (Tewksbury and Lees, 2007). Individuals convicted of sex offenses are also members of society and potential victims — and they harbor the same illusory “stranger danger” angst causing a panic response in the general public.

Accordingly, registrant beliefs may mirror common (and often unfounded) societal opinions and fears about (other) people convicted of sex offenses. Indeed, registrants, like others, may view helpful aspects of SORN laws in isolation — that is, from a static perspective, ignoring the fact that SORN may also exacerbate recidivism risk factors (Prescott, 2016). In fact, the data provide good reason to believe that registrants might be evaluating the wrong counterfactual when considering the benefits of SORN: The minority of registrants with positive views about SORN's potential to reduce recidivism also appear to express strongly held and pervasive beliefs that SORN laws are unfair (e.g., Lasher and McGrath, 2012) and produce serious collateral consequences, including unemployment and financial difficulties, housing instability, loneliness, and stigmatization (e.g., Levenson and

Cotter, 2005). Both have been shown to lead to greater recidivism risk.⁴¹ Thus, even if many registrants were to believe that SORN laws can be helpful *given* existing levels of registrant unemployment, poverty, homelessness, and isolation, the research does not tell us whether these public safety upsides in the eyes of registrants entirely offset the significant criminogenic consequences of SORN laws.

The foregoing notwithstanding, in what follows, we summarize the corpus of research on registrant views of the relationship between SORN laws and public safety. Because no work to our knowledge explores registrant beliefs about the possible deterrence impact of SORN laws on first-time offenses or geographic or victim-type offense patterns, we concentrate on whether registrants believe SORN laws reduce recidivism risk, either their own or that of registrants generally. We do not describe the very large literature on the negative collateral consequences of SORN laws – much of which also builds on survey and interview data – nor related views about whether SORN laws are “fair.”

Researchers in this domain use either survey or interview methods. All subjects are registrants (or individuals convicted of a sex offense), but they vary along many demographic dimensions (age, race, type of crime, treatment status, etc.). Moreover, some subjects are in open communities while others are receiving residential treatment (and are thus less familiar with how SORN laws work “on the outside”). We mostly take the work on its own methodological terms, despite the fact that most analysis samples are neither random, nor representative, nor large, and virtually all have low response and participation rates. Some jurisdictions figure repeatedly in these studies, and some survey questions also appear repeatedly, as researchers build on previous work. We also put to one side the set of self-serving biases that can lead respondents to downplay the magnitude of their own criminal histories and that cause some of them to consider only whether SORN laws will reduce recidivism for “real” sex offenders (i.e., not them).

* * *

Zevitz and Farkas (2000) present some of the earliest work on registrant beliefs about the public safety consequences of SORN laws. Using interview data from thirty registrants from Wisconsin, the authors find that “[o]nly a few” subjects thought community notification was likely to “prevent reoffending” (p. 387). While two respondents in the study indicated that they felt that full disclosure made their treatment more effective, more viewed notification as counterproductive, likely to “drive many back to prison” (p. 388).⁴²

⁴¹ In our model, the burdens imposed by SORN laws on registrants (i.e., reducing u_i) are likely to increase recidivism all else equal, and procedural justice theories imply that compliance and law-abiding behavior turn significantly on whether SORN laws as they are applied are considered fair and appropriate by the registrant (Tewksbury and Lees 2007). Many chapters in this volume touch on these issues.

⁴² Elborgan, Patry, and Scalora (2003) survey forty registrants receiving residential treatment in Nebraska. Their sample is atypical – 48 percent of the participants were unfamiliar with notification and how it would work. Their offenses were serious, and it is unclear how many, if any, had lived under notification requirements. With these important caveats in mind, the authors find that a majority of participants (72 percent) viewed notification laws as providing them with an incentive

Tewksbury (2004) surveys forty female registrants in Indiana and Kentucky, asking them both about SORN's collateral consequences and notification's potential effect on their likelihood of re-offending. While "nearly two-thirds" of respondents agreed that community notification made it "less likely" that they would commit another sex offense, even more felt ashamed and "unfairly punished" by being listed on a registry. When respondents were asked whether the registry was a "good thing," responses averaged to 6.45 on a scale from 1 (strongly disagree) to 10 (strongly agree). They also claimed to "understand" why society wants registries. Tewksbury (2005) surveys 121 registrants in Kentucky, mostly men, with similar questions. He finds a more mixed reaction from these male registrants on whether notification is a good thing (5.75 on a 1–10 scale) and on whether SORN would reduce their likelihood of recidivating (5.60), with 37.9 percent completely disagreeing and 43.2 percent completely agreeing. In concluding, Tewksbury (2004) intimates that the serious collateral consequences reported are more likely to result in higher recidivism notwithstanding some of the positive reactions to notification.

Levenson and Cotter (2005) is typically cited for the proposition that SORN laws produce very serious collateral consequences for registrants, but the study also directly analyzes registrants' beliefs about the effects of notification on registrant behavior. Using survey data from a nonrandom sample of 183 registrants in Florida, Levenson and Cotter find that 36 percent of respondents attested to being more willing to "manage [their] risk factors" because they know their "neighbors are watching," 66 percent reported being "more motivated to prevent reoffense" to prove they are not "a bad person," and 22 percent indicated a belief that registration and notification help them "prevent reoffending." In addition, 26 percent reported that they have less access to victims because they are known sex offenders, and 32 percent agreed that "communities are safer when they know where sex offenders live."⁴³ As a point of comparison, however, these respondents simultaneously revealed significantly stronger beliefs in the criminogenic consequences of notification, including 71 percent agreeing that SORN interferes with recovery by "causing more stress," 72 percent reporting "less hope for the future," and 49 percent admitting feelings of hopelessness, ominously agreeing to the statement "[no] one believes

not to re-offend (an opinion that was correlated with their viewing the laws as relatively fair). A significant percentage also felt notification had a positive impact on their willingness and motivation for treatment, although the study also revealed many negative assessments of notification, including its many unfair requirements and the embarrassment resulting from being publicly listed.

⁴³ Interestingly, family members appear less likely to agree with this last estimate. In Levenson and Tewksbury (2009), which studies a nonrandom national sample of family members of registrants using an online survey, only 9 percent of family members agreed with the statement "communities are safer when they know where registrants live." Somewhat similarly, Burchfield and Mingus (2008), in a small interview study ($n = 23$), report that two (or "a few" – thus, roughly 10 percent) of their subjects indicated positive effects of notification publicity, including "public safety and awareness, as well as incentive for the registrants themselves to be accountable for their behavior" (p. 366).

I can change so why even try?" A number of other studies have subsequently replicated these qualitative findings with other samples in other states.⁴⁴

Tewksbury and Lees (2007) catalog registrant perceptions of SORN's ability to "enhance community awareness and promote public safety" using interview data from twenty-two Kentucky registrants (p. 380), including their views on how to improve registration and notification. The authors conclude that many registrants view registries positively in the abstract (i.e., leaving themselves out of the picture), noting that, by their lights, a properly designed registry "makes sense" (p. 392). However, registrants offer "mixed views" on whether community members can use the information effectively (e.g., because there might be too many registrants on registries) and are of "varying mind-sets" on whether notification is likely to reduce recidivism risk (p. 392). Specifically, Tewksbury and Lees conclude that only a "minority" of registrants in their data indicated that registries as currently constructed are able to reduce recidivism (p. 402).

Brannon, Levenson, Fortney, and Baker (2007) surveyed 125 registrants in outpatient treatment in Florida and compare their responses to those of 193 nonregistrants (i.e., the public). Registrant respondents were four times as likely as the public to view community notification as ineffective in reducing the number of sex offenses (41.6 percent v. 10.4 percent). Moreover, 70 percent of registrants viewed notification laws as somewhat unfair or unfair (relative to 22 percent of the public). Although these results establish that at least some registrants believe community notification

⁴⁴ Levenson, D'Amora, and Hern (2007) replicate this survey using 239 registrants in treatment programs in Connecticut and Indiana. While their numbers are not identical to those in Levenson and Cotter (2005), the qualitative results about both the positive and negative recidivism-related effects of SORN are very similar (both in magnitude and in the order of relative agreement across the relevant survey statements). With respect to respondent views about whether SORN laws help them to "prevent reoffending," the estimate is identical, with 22 percent of the sample agreeing or strongly agreeing with this statement. Mercado, Alvarez, and Levenson (2008) use a modified version of the Levenson and Cotter (2005) survey instrument to study 138 community registrants in New Jersey. The results of this study are not reported in a way that makes them directly comparable to the earlier studies, but the substance of the results appears similar, with the authors interpreting the data as showing that respondents "clearly disagreed with the notion that notification helped them prevent reoffending" (p. 196). Of course, not every respondent strongly disagreed with the idea that community notification can be effective. McGrath, Cumming and Lasher (2009) also use the Levenson and Cotter (2005) instrument to explore the views of 515 adult males in treatment programs in Vermont, some surveyed in 2004 shortly before internet notification began and some in 2008. Again, the results track with earlier studies, with a few exceptions, and with SORN laws generally faring better in the eyes of registrants. Importantly, roughly 34 percent (v. 22 percent) viewed notification as helping them avoid re-offending, higher than in earlier studies, although this was a group of respondents in treatment in a small, relatively liberal state. The implementation of online notification does not change registrant beliefs all that much (perhaps because less than a quarter of respondents were listed online in 2008), although it does seem to improve a respondent's reported motivation to prevent re-offense so as to "prove to others" that they are "not a bad person." Lasher and McGrath (2012) compare and contrast the results of these studies (see table 4, p. 17) and conclude that "[a]bout one third (37 percent) of all participants agreed that communities were safer when they know where registrants live and almost three quarters (74 percent) opined that community notification made them more motivated to prevent themselves from re-offending [to prove others wrong]" (p. 20).

has public safety benefits, the survey question appears to refer to SORN effectiveness with respect to sexual re-offending of registrants generally. In other words, this objective phrasing seems likely to be taken to refer to the probable response of “other” registrants and not to the respondents’ views of SORN effects on their own likelihood of re-offending.

Ackerman and Sacks (2012) sent surveys to a large number ($n = 3,506$) of registrants in Nebraska, Montana, and Kansas, and they use their sample ($n = 246$; response rate = 7 percent) to relate the “strain” registrants experience from being subject to SORN laws to self-reported recidivism.⁴⁵ While they do not explicitly address registrants’ views on whether SORN is likely to reduce recidivism, they do so indirectly by connecting registrants’ subjective views of how much they are affected by SORN to whether they self-reported having returned to crime. The authors find that SORN-associated strain is positively linked to sex offense recidivism (albeit weakly in some models), consistent with the idea that SORN laws reduce u_i – that is, SORN exacerbates risk factors and/or reduces the deterrence effects of criminal law. They also report that SORN-caused collateral consequences are correlated with general recidivism, including violent and drug crimes. Ackerman, Sacks, and Osier (2013) examine sixty-six unsolicited responses/comments received from this same survey, analyzing any proffered respondent statements on SORN ineffectiveness. The authors report that “most offenders state emphatically that it is ineffective” (p. 36), although their reported data (Table 3) better support a later conclusion: “[h]alf of the participants acknowledge that registration does not work in its current form” (p. 39). Respondent opinions seem to indicate a view that registration *could* work if it were implemented differently (e.g., made available only to law enforcement or if it included only high-risk or repeat offenders), but at least some respondents argue that it only works “in theory not in practice” (p. 39) and that registration is more likely to generate crime than to deter it.

Bowen, Frenzel, and Spraitz (2016) analyze 286 surveys from registrants in Pennsylvania, Texas, and Wisconsin, asking them whether they were less likely to commit another sex offense “because of the registry.” On a scale from 1 to 10, respondents on average reported 5.26. The authors do not indicate the distribution of answers to this question nor whether responses were symmetrical around the mean, which matters for interpretation. For example, a mean of 5.26 is consistent with roughly 15 percent responding 1 (disagree strongly) and 85 percent responding 6 (agree slightly) but also with roughly 20 percent responding 10 (agree strongly) and 80 percent responding 4 (disagree slightly). At a minimum, however, a nontrivial share of respondents agreed with the idea that the registry was helpful to them in

⁴⁵ The authors define “strain” as “the removal of positive stimuli, the imposition of negative stimuli, or the failure to achieve personal goals” (p. 189). They proxy for SORN-related strain by creating a 25-item additive variable with each item linked to a known collateral consequence of SORN laws (e.g., “I have lost a job” and “I have been threatened or harried by neighbors”). In other words, the strain variable seeks to capture the burden of registration and notification – that is, “negative experiences they endured after inclusion on the sex offender registry” (p. 189).

reducing their likelihood of recidivating. To better understand this finding, the authors also examine whether registrant experiences under SORN were associated with their view of SORN effectiveness. Consistent with our model, they find that (1) never being contacted by the police (less monitoring – i.e., lower p_{ijt}) and (2) having a family member suffer negative consequences as a result of their registrant status (exacerbated recidivism risk factors – i.e., lower u_i) are associated with less agreement with the notion that SORN reduces recidivism.

Cooley, Moore, and Sample (2017) study seventy-seven registrants (who averaged 8.2 years out of correctional control with no self-reported re-offending) using informal conversational interviewing techniques. With respect to the act of registering (and other compliance-related obligations), the authors state that “registrants noted that SORN was more of an inconvenience; however, those with predatory contact offenses appreciated registration because it reminded them that they have a problem they need to control” (p. 148). Also, “[r]espondents’ comments infer the registry did not deter their behavior and does not play a role in their desistance, yet almost all of them (73 percent) mentioned how they recognize its value for perceptions of public safety, regardless of its inconveniences or inapplicability to them being a threat” (p. 148). Finally, focusing on notification, the authors report that “[n]o participants suggested that public notification deterred them from reoffending” (p. 149) whereas “[f]ifty-one percent of participants suggested notification created stress and strain among registrants and their family members in a way that may inhibit the desistance process” (p. 150). Cooley, Moore, and Sample’s work indicates that registration and notification may have different – even offsetting – effects on recidivism, consistent with the evidence in Section 7.3.⁴⁶

* * *

In our view, the evidence we assemble in this section complements the research we catalog in Section 7.2. There are tensions between the two sections, but we believe a consistent empirical story knits them together – one largely critical of SORN

⁴⁶ All of this work suggests that registration alone may be relatively more effective than community notification at inhibiting re-offending. Relevant to this possibility, Murphy and Federoff (2013) use open-ended questionnaires to study thirty registrants in Canada – where registration is non-public. Apparently, 9 (39 percent of the 23 who responded) felt that the registry “acted as a deterrent to reoffending” (p. 244). Perhaps more importantly, because of its significantly reduced burden, registration alone may not increase recidivism risk levels. Roughly two-thirds of the participants indicated that being privately registered “has little or no impact on their ability to successfully reintegrate into the community” (p. 244). This somewhat unusual sentiment may be the result of the applicable registration process being comparatively easy to navigate – just a yearly check-in that registrants analogized to updating their driver’s license. On the other hand, looking to Australia, Seidler (2010) finds that registrants do not believe registration alone is effective at reducing re-offense rates. Seidler interviewed registrants in New South Wales (which employed a non-public registry at the time of the study), orienting in part around the question: “Do you think the register is helpful or unhelpful in assisting you to manage your risk?” (p. 67). Seidler concludes that registrants view private registration as merely window dressing designed to satisfy the public, unlikely to deter anyone from re-offending and just another form of punishment. However, the author acknowledges that some registrants indicated that they “might think twice” before re-offending (p. 74).

legislation. The fundamental tension is that the large body of research finding no support for the idea that notification reduces registrant recidivism is at odds with the minority of registrants who claim to believe that SORN has some positive effect on reducing recidivism. Surely part of this assessment stems from registrants being people like everyone else who find the conventional wisdom about SORN laws convincing about *other* sex offenders, but even when they speak of effects on their own behavior, not all registrants view SORN laws entirely negatively.

One plausible answer to this contradiction springs from the apparent internal inconsistencies of registrant views contained within the belief studies. For example, registrants report feeling more motivated to avoid re-offending by SORN monitoring but also much more stressed, isolated, and hopeless. These registrant responses reveal the basic trade-off inherent in attempting to use community notification to reduce sex offense recidivism: while notification probably at least minimally improves monitoring and victim precaution-taking (making it at least not any easier for a registrant to re-offend without getting caught), the negative effects of notification (i.e., the significant economic, psychological, and social burdens, etc.) may render registrants much less capable of resisting any of the many pressures impelling them toward re-offending, including the possibility that the well-documented difficulties notification creates for registrants' lives "on the outside" simply cause them to care much less about the prospect of returning to prison if they are caught and convicted for a subsequent crime.

If the latter effects outweigh the former, we should expect SORN laws (or at least community notification) to result in more recidivism on average, not less. Thus, even if registrants acknowledge that SORN laws offer them something beneficial, it is another thing altogether to claim that notification's positive aspects more than offset its criminogenic features. We have reviewed the empirical research on the net effects of notification on criminal behavior at length, but in this chapter we do not review the very large literature on SORN's negative collateral consequences – nor the emerging evidence on the limited practical usefulness of providing notification information to potential victims. We hope it suffices to say that what we know about both topics may well identify the key mechanisms accounting for SORN's failure.⁴⁷

7.5 CONCLUSION

This chapter has sought to summarize and carefully review the existing empirical evidence evaluating SORN's effects on criminal behavior, broadly construed. The

⁴⁷ A few of the studies that document the criminogenic collateral consequences of community notification in particular include Ackerman and Sacks (2012), Bailey and Klein (2018), Bowen et al. (2014), Burchfield and Mingus (2008, 2014), Hanson and Morton-Bourgon (2005), Jeglic, Mercado, and Levenson (2012), Kilmer and Leon (2017), Klein, Tolson, and Collins (2014), Lasher and McGrath (2012), Levenson and Cotter (2005), Levenson, D'Amora, and Hern (2007), Mustaine and Tewksbury (2011), Mustaine, Tewksbury, and Stengel (2006), Robbers (2009), Socia, Levenson, Ackerman, and Harris (2015), Terry (2015), Tewksbury, Mustaine, and Rolfe (2016), Tewksbury and Lees (2007), and Tewksbury and Zgoba (2010).

accumulated evidence largely rejects the claim that SORN laws have achieved their goal of increasing public safety. This is particularly the case with the most controversial aspect of SORN laws – community notification. The jury is still out on whether notification laws deter first-time offenses by nonregistered individuals. But there is virtually no evidence that notification successfully reduces recidivism among actual registrants, the main target population, and it may increase re-offending by dramatically aggravating recognized recidivism risk factors and reducing the deterrence value of criminal punishment. Unfortunately, we know comparatively less about the effects of registration, standing alone, which may offer a plausible alternative to community notification: there is no evidence to indicate that registration deters first-time sex offenses, but it may reduce registrant recidivism by increasing actual or felt law enforcement monitoring.

Taking a broader view, the notion that government could noticeably reduce the frequency of sex offense victimization by publicly identifying individuals with any history of sex crime has always rested on shaky ground, at least here in the United States. At least 90 percent of arrests for sex offenses, if not significantly more, involve individuals who have never previously been arrested for (much less convicted of) a sex crime (Sandler, Freeman, and Socia, 2008; Levenson and Zgoba, 2016). We also know that the vast majority of sex offenses are committed by persons familiar to their victims (Prescott and Rockoff, 2011), an empirical reality that undercuts the foundational “stranger danger” premise of notification itself.

At the same time, same-crime recidivism rates for people previously convicted of sex offenses are relatively low, around 5–10 percent for those released from state prison (Langan, Schmitt, and Durose, 2003; Alper and Durose, 2019) – that is, the potential registrants who are presumably at the highest risk of re-offending. Sex offenses as a class do not appear to be driven by a small number of repeat offenders. Thus, the margin for potential improvement has always been small, and any detectable change in the sex offense rate would require a very large behavioral response from a group that already has a relatively low recidivism rate. In retrospect, it thus may not be surprising that there is little evidence that community notification reduces overall sex offense frequency – or even the likelihood of registrant recidivism (fewer than 10 percent of registrants were likely to re-offend in any event). Given that notification significantly increases recidivism risk factors even for those registrants who otherwise would have never re-offended, evidence of *increases* in recidivism risk is perhaps only slightly more surprising.

The evidence tells us, therefore, that policy makers would be wiser to focus their efforts on preventing first-time sex offenses. Importantly, the model we use to organize the potential consequences of SORN laws clarifies that registration and notification *may* impact nonregistrant behavior as well – that is, they might theoretically aid in reducing the number of first-time offenses. In line with this possibility, our summary demonstrates that there is at least some credible evidence that notification (but not registration on its own) can deter first-time offenses.

Unfortunately, the legal basis for applying SORN laws to individuals retroactively has always been their nonpunitive, regulatory character. The designation of SORN laws as civil and regulatory in nature, which frees the laws from many constitutional constraints that limit law enforcement prerogatives in the criminal domain, has relied critically on SORN laws' ostensible purpose of protecting the public by reducing individual registrants' likelihood of re-offending. Thus, the idea of explicitly justifying SORN as a punishment designed to deter first-time potential offenders presents obvious concerns. Distinguishing the burdens imposed by SORN laws from criminal punishment under the Constitution may become increasingly unworkable in any event, as it has become clear that SORN laws fail to prevent recidivism while nevertheless systematically imposing draconian burdens and disabilities on registrants. In sum, the contents of this chapter raise significant doubts about the future viability of SORN laws as a public safety strategy.

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