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RESEARCH ARTICLE

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The problem with assumptions: Revisiting "The dark figure of sexual recidivism"

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Abstract

What is the actual rate of sexual recidivism given the well-known fact that many crimes go unreported? This is a difficult and important problem, and in "The dark figure of sexual recidivism," Nicholas Scurich and Richard S. John (2019) attempt to make progress on it by "estimat[ing] actual recidivism rates . . . given observed rates of reoffending" (p. 171). In this article, we show that the math in their probabilistic model is flawed, but more importantly, we demonstrate that their conclusions follow ineluctably from their empirical assumptions and the unrepresentative empirical research they cite to benchmark their calculations. Scurich and John contend that their analysis undermines what they call the "orthodoxy in academic circles" (p. 173) of low sexual recidivism rates among individuals convicted of sexual offenses, but we underscore that their article does *not* analyze data in the traditional sense; instead, it just interprets past scholarly work through the use of strong assumptions in a way that, for practitioners, is likely to be opaque and misleading (and, for us, strays into speculation, argument, or advocacy and away from objective research). Our simple calculations show that their findings are highly sensitive to their assumptions, and we conclude that courts and others should recognize Scurich and John's work for what it is—a set of complex hypotheticals that are no more reliable than what judges and lawyers accomplish on their own by simply recognizing the basic problem that not all sexual offenses are reported.

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1 | INTRODUCTION

In their article "The dark figure of sexual recidivism," Nicholas Scurich and Richard S. John (2019) wrestle with a difficult and important problem: What is the actual rate of sexual recidivism given the well-known fact that many crimes go unreported? Scurich and John begin by describing the controversy over the incidence of sexual offense recidivism. Courts and the public believe that sexual offense recidivism is really high, whereas most empirical studies have found sexual recidivism to be quite low. "Based on the assumption that the empirical studies are correct in their conclusion that sexual recidivism rates are 'low'," Scurich and John write, "academic commentary has universally castigated the enactment of laws and statutes that are ostensibly predicated on a 'high' recidivism rate" (p. 159).

Yet Scurich and John are deeply critical of these recidivism studies. They offer no specific methodological challenge to any of them, except their general assertion that "the vast majority of the academic studies" use charges or convictions as a measure for recidivism, which "underestimates the actual rate of recidivism" (p. 160). Instead they claim that these studies are flawed because they fail to take into account unreported offenses. Scurich and John point to two types of data that "speak to the magnitude of the dark figure (of sexual recidivism)" (p. 161). They turn first to victim self-report data as measured by a variety of sources, including the National Crime Victimization Survey. These sources supposedly show that "*at a minimum*, a majority of sexual offenses do not get reported by victims to the police" (p. 162).[1] Scurich and John then look to offender self-report data. They cite several studies that they claim demonstrate how much more common actual sexual recidivism is relative to the rates reported by official crime statistics.

To try to estimate this "dark figure of sexual recidivism" with some measure of precision, Scurich and John carry out a probabilistic simulation exercise that incorporates victim self-report data and different assumptions about the chances of an individual being convicted of a new sexual offense (given that the crime is reported) in order to approximate a measure of *actual* sexual offense recidivism. Their model assumes that offending rates are constant over time, and they calibrate it with reported recidivism rates from four different studies. Scurich and John conclude that "under any realistic assumptions, the rate of *actual* sexual recidivism is substantially larger than the rate of *observed* sexual recidivism" (p. 173).[2]

Scurich and John maintain that their findings undermine what they term "the orthodoxy in academic circles," namely empirical claims that sexual recidivism rates are "single digit/low teen" (p. 173). Such claims, Scurich and John write, are "specious and seriously betray the reality of sexual recidivism" (p. 173). Research, they say, should be conducted differently: "[I]t is untenable for researchers to rely exclusively on sexual recidivism that is based on official crime statistics [because] the majority of sexual recidivism is not detected by such a definition" (p. 172). Court proceedings, they assert, have also been impacted: "[G]iven that observed recidivism may be such a small sliver of actual recidivism, serious questions arise about the appropriateness of using actuarial risk assessment instruments trained to predict *observed* recidivism to address legal issues that are concerned with *actual* recidivism" (p. 172).[3] The solution, Scurich and John imply, is to use *their* probabilistic model because it "allows us to estimate actual recidivism rates . . . given observed rates of reoffending" (p. 171).

Of course, any attempt to undermine the consensus of an entire field should be supported—like all scholarly research—by careful analysis and sound reasoning. It should also be scrutinized by peers to better understand the precise contours of the contribution. In this piece, we take a close look at Scurich and John's methods and, in particular, their assumptions. We are not the only ones who resolved to critically assess Scurich and John's analysis when it was published in 2019. Abbott (2020) discusses many of the same issues we identify (and many we do not), ultimately concluding that Scurich and John's mix of strategies and assumptions "effectively invalidates their findings" (p. 543). Nevertheless, our work, conducted concurrently with Abbott (2020), makes a number of distinct contributions to the conversation about the usefulness and reliability of Scurich and John's conclusions, including by formally replicating and probing the robustness of their underlying empirical estimates. Our goal is to ensure that nontechnical consumers of Scurich and John (2019) understand exactly what their research does and what it does not do, what sorts of claims it can be used to support, and which would be inappropriate. We hope our treatment proves to be a useful resource for practitioners, lawyers, and judges as they seek to better understand sexual recidivism.

We begin by explaining how Scurich and John's probabilistic simulation model works before evaluating the underlying data and the empirical assumptions that animate it. We then look more carefully at the mathematics behind the model itself. We conclude that there are significant limitations to their work and that, as we will explain, their inferences about the likely scope of the "dark figure" are interesting solely as an academic exercise. What is most critical is that their conclusions are essentially driven by assumptions that are not themselves rooted in data or empirical research. In this important sense, their paper is not an empirical one, and researchers, courts, and policy-makers should reject the inferences Scurich and John draw from their probabilistic modeling exercise as uninformative about *actual* sexual recidivism.

2 | PROBABILISTIC SIMULATION MODEL

To "estimate" the rate of actual sexual recidivism for non-incarcerated individuals previously convicted of sexual offenses, Scurich and John (2019) build a probabilistic simulation model. Their specific focus is the sexual offense rate of men who were convicted of a sexual crime at some earlier time and have since been released from custody after serving their sentence. Their model purports to estimate mean rates of actual recidivism, conditional on a distribution of the number of reoffenses (with mean μ_0) over the population and on fixed probabilities for reporting ($P_r < 1$) and successful prosecution ($P_c < 1$) for each offense (p. 164). In other words, there exists an unobserved actual recidivism rate, which is translated into an "observed" rate for a population by, in effect, multiplying the former by P_r and P_c. From this perspective, Scurich and John just repeat the very standard notion that actual recidivism is higher than observed (e.g., conviction) recidivism because some crimes are not reported or do not result in conviction. In very rough terms, they argue that one must "inflate" the observed rate by $1/(P_r \times P_c)$ to arrive at the "actual" rate, but they assume values of P_r and P_c rather than estimate them.[4]

2.1 | Modeling recidivism

To develop their model, Scurich and John conduct a series of simulations using different values for the key parameters: μ_0, P_r, and P_c. Everything in the paper follows from the assumptions about these values and their distributions. Each simulation consists of the following steps:

1. Start with 100 hypothetical "offenders" (each individual indicated by $i = 1$–100).
2. Assume that the average number of sexual offenses per offender across all offenders over the time period in question is μ_0.[5]
3. Randomly assign an average number of sexual offenses (λ_i) to each of the 100 offenders [$\lambda_i \sim \text{exponential}(\mu_0)$] over some number of time periods.[6]
4. Using the individual means (λ_i), randomly draw the number of offenses committed by each offender (S_i) [$S_i \sim \text{Poisson}(\lambda_i)$] in the given period.
5. Assume that all sexual offenses have the same probability of being reported (P_r), that whether each offense is reported is independent of whether other offenses are reported, and that the number of sexual offenses that are reported for individual i is R_i [$R_i \sim \text{binomial}(S_i, P_r)$].
6. Assume that all reported sexual offenses have the same probability of resulting in a conviction (P_c), that whether a reported sexual offense results in a conviction is independent of whether other offenses result in conviction, and that the number of sexual offense convictions for individual i is C_i [$C_i \sim \text{binomial}(R_i, P_c)$].

7. Determine whether each offender commits one or more offenses (N_s) in the simulation's draws from the distributions (i.e., $N_s = 1$ if $S_i \geq 1$ and $N_s = 0$ if $S_i = 0$)
8. Likewise, determine whether each offender is convicted for one or more offenses (N_c) (i.e., $N_c = 1$ if $C_i \geq 1$ and $N_c = 0$ if $C_i = 0$).
9. Calculate the "actual" recidivism rate by dividing the number of offenders that commit one or more offenses (N_s) by the total number of offenders (100).
10. Calculate the "observed" recidivism rate by dividing the number of offenders that are convicted for one or more offenses (N_c) by the total number of offenders (100).
11. Take steps 1 through 10 and repeat them 1,000 times, saving the calculated recidivism rates for each of the 1,000 trials.
12. Determine the "actual" and "observed" recidivism rates by taking the average of these 1,000 trials.

It is important to emphasize again that Scurich and John start with exactly three values (μ_0, P_r, and P_c) as well as the generic exponential, Poisson, and binomial distributions. There are no real-world "data" in these exercises. They run Monte Carlo simulations using a three-way factorial design of different values for the parameters μ_0, P_r, and P_c (p. 167). With nine possible values for μ_0, three values for P_r, and three values for P_c, they have ($9 \times 3 \times 3 =$) 81 separate combinations and thus 81 simulations. Again, each simulation occurs by conducting 1,000 independent trials to calculate average "observed" recidivism for the given values of μ_0, P_r, and P_c, although Monte Carlo simulations are not actually necessary—closed form solutions are possible given their approach; math alone would have been sufficient (Figure 1).

2.2 | Distribution assumptions

Scurich and John do not know how recidivism risk varies across individuals based on observable characteristics (e.g., crime of conviction, treatment status, etc.), and they do not know how recidivism varies over the course of an individual's lifetime or other temporal periods (e.g., age, time elapsed since release, etc.). To introduce differences at the individual level, Scurich and John assume that behavior is governed by draws from known distributions rather than assuming that all offenders are identical. These distributions introduce noise, but if we are interested in "average rates" and we are willing to stipulate to a large number of hypothetical offenders, this variation at the individual level eventually cancels itself out.

In any event, Scurich and John make two key assumptions regarding the distribution of individual propensities to recidivate in order to run their simulations. These assumptions allow them to pick a single parameter (and vary it to explore the consequences of changing the parameter) to manufacture a range of different individual tendencies. First, they assume that individual propensities to reoffend (λ_i) follow an exponential distribution (p. 164). The exponential distribution has a single parameter, μ_0, which represents the average number of sexual offenses committed per individual across all individuals during the relevant time period. With μ_0 and the distribution, one can create a "fan" of hypothetical individuals around an "average" individual. Second, Scurich and John assume that the number of sexual offenses for a specific individual in the population of offenders in a given time period follows a Poisson distribution (p. 164). The Poisson distribution of offenses for each offender has a single parameter, the mean, which is assumed to remain constant over time (p. 165).

In other words, the simulations begin with Scurich and John *assuming* the average number of offenses committed by potential recidivists, and then selecting off-the-shelf distributions of random variables to abstract away from a single representative individual in order to generate the appearance of a real-world population. But these off-the-shelf distributions have well-known and straightforward properties, and with enough draws (e.g., 1,000), the population will have precise and predictable behaviors (i.e., the expected value and variance of reoffending is knowable in advance without any simulations whatsoever), meaning that there are no surprises. The conclusions follow ineluctably from these assumptions.

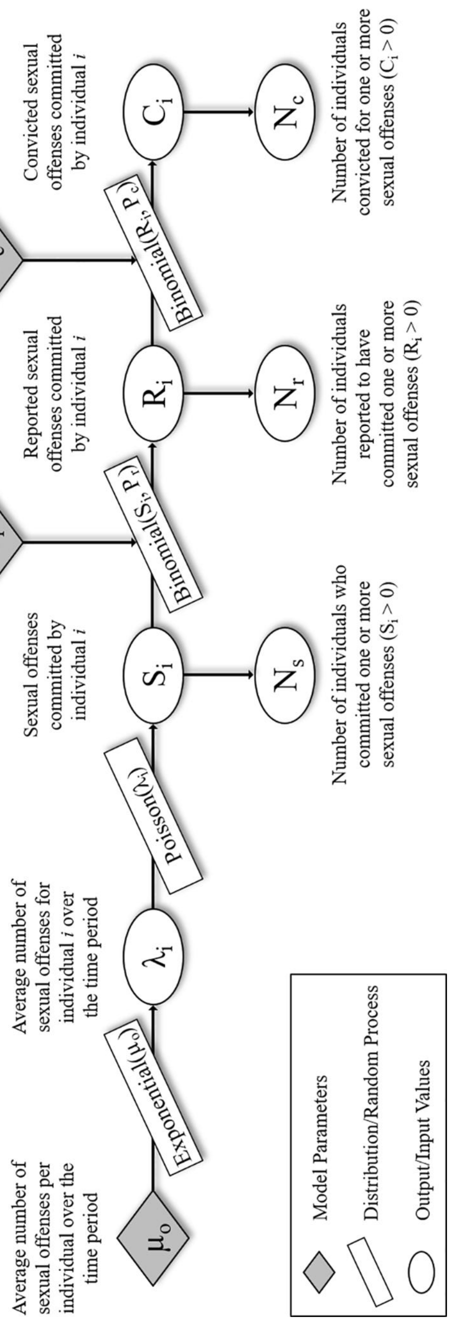

FIGURE 1 Simulation process

2.3 | Parameter assumptions

In order to simulate a recidivism rate—that is, in order to calculate the consequences of their assumptions—for hypothetical individuals at risk of reoffending, Scurich and John must also assume values for their three unobserved parameters.

The first parameter, μ_0, is the average number of offenses per offender across all offenders over the period, and choosing a specific value operationalizes the exponential-distribution assumption the authors make. In effect, Scurich and John constrain the sum of the number of offenses committed by all 100 hypothetical individuals (and divided by 100) to equal μ_0. Each offender's average number of offenses (λ_i) in a time period is determined by a random draw from the exponential distribution with the population average (μ_0) set to a specific value.

The true population average of offenses per offender is unknown, so Scurich and John consider an expansive range of possible values and focus on those values that generate, under strong assumptions, the observed rates of sexual offense recidivism indicated by a select few papers in the literature. Specifically, they consider nine different exponential distributions of populations with means of 1/16, 1/8, 1/4, 1/2, 1, 2, 4, 8, and 16 sexual offenses per year (p. 166). For example, in the simulation that sets μ_0 at 16, Scurich and John *assume* that the average number of offenses across all 100 hypothetical individuals is 16 (only some of which will be reported and result in conviction), but they allow individuals to have different *individual* averages over time (so long as, at the aggregate level, the number of offenses averages out to 16 across all 100 hypothetical individuals), drawn from the exponential distribution, so that the 100 individuals are not identical in all respects.

The second parameter, the probability of an offense being reported to law enforcement, P_r, attempts to account for unreported sexual offenses. Scurich and John do not know the likelihood that a sexual offense will be reported, and they do not know what percentage of sexual offenses become known to the police. To simplify matters, in assigning possible values to the variable on reporting, P_r, Scurich and John assume that the probability that any committed sexual offense will be reported is *constant* across all offenses and individuals (p. 167). They then select three possible values: a low estimate (0.15) from a study by Daly and Bouhours (2010), an intermediate estimate (0.35) from the National Crime Victimization Survey (Planty et al., 2013), and what they call an optimistic estimate (1.0) in which every sexual offense is reported. The estimates Scurich and John borrow from Daly and Bouhours and the National Crime Victimization Survey are the only real-world *data* in their model other than the "observed" recidivism estimates (drawn from a few papers in the existing literature) that the authors use for benchmarking, which we discuss in depth later.

Finally, Scurich and John must assume a likelihood that a reported offense will result in a conviction, P_c—that is, a crime that would be "counted" in a conviction-based measure of sexual recidivism. For various reasons, the police may become aware of a crime and even be confident about the individual who committed it but may not be able to secure a conviction that ultimately counts as an observed instance of recidivism. Just as with P_r, Scurich and John assume the chance of success is constant across individuals and across different offenses (p. 167). This assumption makes their effort a simple one; they need only pick possible values for the probability that a reported sexual offense ends with a successful prosecution. Scurich and John consider the following values for P_c: 0.25, 0.50, and 0.75. They do not explore the consequences of an "optimistic estimate" such as $P_c = 1.0$.

3 | UNDERLYING DATA: RECIDIVISM

With this overview of Scurich and John's model in hand, we are ready to critically examine the objectives, assumptions, and conclusions of their analysis. We begin with the only remaining (but necessary) input to their model: an estimate of the "observed" recidivism rate. This input allows them to back-out an underlying "actual" recidivism rate (given their assumptions about reporting and prosecution success probabilities), which they use to inform their choice of μ_0 in their simulations. We raise two concerns about their paper here. First, Scurich and John include P_c in their model on the assumption that observed recidivism rates in the literature are conviction-based recidivism rates. This assumption is unsound. Second, Scurich and John select studies for their benchmarking that report "observed" recidivism rates that are systematically higher than an even-handed and comprehensive review of the literature tolerates. These choices have the effect of dramatically increasing their constructed "actual" recidivism rates.

Scurich and John state that the "primary objective" of their paper "is to stimulate thought and critical reflection about the near-universal practice of limiting the definition of sexual recidivism to a new legal charge or conviction for a sexual offense" (p. 172). This is a surprising claim given that researchers have called mere rearrest "one of the most common measures found in recidivism research" (Sample & Bray, 2006, p. 92). An arrest entails some law enforcement activity but does not require a prosecutor to file a charge or a judge to enter a conviction, and rearrest seems especially likely to follow a report when the suspected individual is known, has a record, and is possibly publicly registered. Indeed, false or misattributed reports might even lead to an arrest-based recidivism rate exceeding the actual recidivism rate.

Although it is true that some of the studies Scurich and John cite in their article use charges and convictions to measure recidivism, many do not. Indeed, the largest and most recent recidivism studies—which Scurich and John omit from their article and which we discuss below—use arrests. These studies examine arrest patterns to reduce the chance of arriving at a false negative or making a type II error—that is, classifying someone as not having committed a new crime when in fact they have, exactly Scurich and John's concern. Langan et al. (2003) are explicit about these potential benefits in their 2003 Department of Justice study: "Between rearrest and reconviction as the recidivism measure, the one less likely to make that type of error is rearrest. One reason is that the rigorous standard used to convict someone—'proof beyond a reasonable doubt'—makes it certain that guilty persons will sometimes go free. Another reason is record-keeping: the justice system does better at recording arrests than convictions in RAP sheets. For such reasons, this study uses rearrest more often than reconviction as the measure of recidivism" (p. 6).[7]

At the outset, our critique of what might be called Scurich and John's misplaced emphasis on a known issue in recidivism studies may seem a bit persnickety. However, as we show below, their focus on conviction-based measures of recidivism and their choice of which studies to incorporate into their benchmarking analysis, along with many other choices along the way, turn out to matter *a lot*—in predictable ways—in terms of their ultimate conclusion that actual sexual recidivism rates are much higher than observed recidivism rates.

3.1 | Omitted studies

The Department of Justice's Bureau of Justice Statistics has published three important studies on sexual recidivism. All of them use arrest as their primary measure of recidivism. In their oft-cited 2003 study, Langan et al. (2003) follow 9,691 individuals who had been incarcerated for a sexual offense and were released from prison in 1994 in 15 states across the country. They find that 5.3% were rearrested for a new sexual crime within three years. In their well-known report, Durose et al. (2016) study 20,422 individuals imprisoned for sexual offense convictions who were released across 30 states in 2005. They find that 5.6% were arrested for a new rape or sexual assault within five years. In May 2019—after the publication of Scurich and John's article—the DOJ released an additional report examining an estimated 20,195 individuals released from prison in 30 states in 2005 following a term of incarceration for one or more sexual offenses. The authors of the report find that 7.7% were rearrested for a new rape or sexual assault within nine years (Alper & Durose, 2019).

In 2016, the Probation and Pretrial Services Office of the Administrative Office of the U.S. Courts published a study on the recidivism of individuals convicted of sexual offenses under federal supervision (Cohen & Spidell, 2016). The data came from 94 federal judicial districts and involved the records of 7,416 males with sexual offense convictions who were released from federal prison and placed on supervision during fiscal years 2007 through 2013. The authors of the study define recidivism as arrest for a new crime.[8] Cohen and Spidell take a subsample of those on active supervision and find that 2.8% were rearrested for a new sexual offense within three years.

There have also been recent large state-wide studies that use arrests to measure sexual offense recidivism. For instance, in 2007, the Minnesota Department of Corrections published research evaluating the recidivism patterns of 3,166 individuals released in Minnesota following conviction and punishment for a sexual offense. The follow-up periods the researchers use range from 3 to 16 years, with an average of 8.4 years. During that period, 12% of these releasees were rearrested for a new sexual offense; 10% were reconvicted, and 7% were reincarcerated. Most individuals who recidivate do so within the first five years (Minnesota Department of Corrections, 2007). Sample and Bray (2003) use arrest data on approximately 953,000 arrestees from 1990 to 1997 (collected by the Illinois State Police) and find that approximately 6.5% of those in the sample who were originally arrested for a sexual crime were rearrested for a new sexual crime within five years.

Other than mistaking the 2003 DOJ study as measuring recidivism using convictions when it explicitly focuses on arrests (p. 159), Scurich and John do not include any of the above studies in their research. Instead, they rely on studies that have much smaller sample sizes and mostly concern individuals who were released decades earlier. Even more problematic, as they do with the 2003 DOJ study, Scurich and John claim that all of the studies they cite use charges or convictions as their measure of recidivism notwithstanding the fact that many use other measures less susceptible to type II errors. Their decision to leave these large-scale recidivism studies out of their reckoning with the literature as well as their analysis is all the more surprising given that they also characterize a small study crafted to measure the accuracy of a particular predictive instrument (i.e., not recidivism) as a useful benchmarking recidivism study.

3.2 | Included studies

Scurich and John use four studies to calibrate the probabilistic simulation model on which their analysis relies. The first is a study by R. K. Hanson et al. (1993) that looks at the long-term recidivism rate of 197 individuals previously convicted of child molestation who were released from a maximum-security prison in Ontario, Canada, between 1958 and 1974. The maximum follow-up period in the study is 31 years. The offenders were divided into three groups, and the average follow-up periods were 19 years, 28 years, and 20 years respectively. This study finds that 42% of the sample were reconvicted for a “sexual offense, a violent offense, or both during the follow-up period” (p. 648).

This study has two problems beyond its age and small sample size. First, R. K. Hanson et al. (1993) include violent offenses in their reoffense measure, which means we have no way of ascertaining from their study the actual *sexual* offense recidivism rate. Second, they mention that there are 22 potential recidivists in their data for whom the 1989 and 1991 Royal Canadian Mounted Police (RCMP) records are missing. Because these individuals were over the age of 50 at the time of release, R. K. Hanson et al. assume they are dead. However, it is also possible that their records are missing because they did not recidivate. As R. K. Hanson (2006) wrote in a later article, “[t]he RCMP policies for retaining criminal records have changed through the years, but they have always been more inclined to keep the records of active offenders (recidivists) than to keep those of inactive offenders (non-recidivists)” (p. 103).[9] By characterizing these people as dead instead of as possible nonrecidivists, R. K. Hanson et al. inflate the observed recidivism rates in their sample. On 10 July 2019, one of us (TRL) wrote to R. K. Hanson to ask whether he agreed with this criticism. On 11 July 2019, R. K. Hanson responded, “I actually agree with your criticism of my 1993 article. There is no easy answer to missing records, and I have coded things both ways in different studies (one inflates and other deflates estimates).”[10]

Scurich and John also refer to a study by Prentky et al. (1997) that looks at the recidivism rates of individuals with sexual offense convictions released over a 25-year period (1959–1984) from the Massachusetts Treatment

Center for Sexually Dangerous Persons in Bridgewater, Massachusetts, which was established "under special legislation for the purpose of evaluating and treating individuals convicted of repetitive and/or aggressive sexual offenses" (p. 637).[11] Prentky et al. (1997) follow 251 individuals (136 previously convicted of rape and 115 of child molestation) and measure recidivism by charge for a new sexual offense. In presenting this study, Scurich and John should have clarified that, "[w]ithin the Commonwealth of Massachusetts, charge is a more inclusive category than arrest (i.e., you may be charged without being arrested)" (p. 639-640). They also should have explained that Prentky et al.'s (1997) research studies an atypical, high-risk sample, lest the reader think the article's results reflect the long-term risk posed by the average individual with a sexual offense conviction in their history. Indeed, Prentky et al. explicitly warn of the dangers of such conflation, commenting that "[s]exual offenders sampled from general criminal populations, from offenders committed to a state hospital, and from a maximum security psychiatric hospital are likely to differ in ways that would affect their recidivism rates and make cross-sample comparisons difficult" (p. 636; internal citations omitted).

The study by R. K. Hanson and Bussière (1998) is a meta-analysis of 61 studies released between 1943 and 1995 from the United States, Canada, the UK, Australia, Denmark, and Norway. The sample sizes range from 12 to 5,000 individuals, with a mean of 475 and a median of 198. The median follow-up period is 4 years, and the average is 5.5 years. The average sexual offense recidivism rate is 13.4%. Although Scurich and John are correct that "84% of the studies contained in the meta-analysis by R. K. Hanson and Bussière defined recidivism as a new conviction" (p. 160), they fail to mention that 27 of the 61 studies (44%) use multiple indices of recidivism, and 15 studies (25%) do not report the source of the recidivism information. A more accurate portrayal is that 54% of the studies define recidivism as an arrest; 25% define it as a self-report, and 16% define it as a parole violation–all of which are broader than a charge- or conviction-based definition. In addition, Scurich and John do not explain that the study with the largest sample size (4,381–5,000) uses "an unusually broad definition of recidivism" because it includes "treatment failure" (p. 351). This overbroad definition would seem to pose a problem for Scurich and John, since presumably some individuals in the sample are classified as recidivists despite never committing a new sexual crime.

The final study that Scurich and John cite is a 2014 study by R. K. Hanson, Lunetta, et al. (2014). This study does not seek to measure recidivism rates but instead aims to assess the predictive accuracy of the Static-99 and Static-99R on a sample of 475 individuals with sexual offense convictions who were released from the California Department of Corrections and Rehabilitation in 2006–2007. R. K. Hanson, Lunetta, et al. (2014) follow the scoring rules of the Static-99 and exclude individuals whose only registrable sexual offense is child pornography, statutory rape (consensual intercourse with a minor aged 14–17 years), or other consensual sexual activity with a minor aged 14–17 years. In addition, R. K. Hanson, Lunetta, et al. (2014) define recidivism as arrest for a sexual offense—both contact offenses like child molestation and noncontact offenses like exhibitionism (but not failure to register). They find that 4.8% (23/475) of individuals in the sample were arrested for a new sexual offense within five years. Breaking that down further, 3.2% (15/475) were arrested for contact offenses and 1.7% (8/475) for noncontact offenses.[12]

3.3 | Significance

Scurich and John tend to rely on studies with higher-than-average, unrepresentative recidivism rates and overlook studies that report low recidivism rates. They use the studies we summarize above to benchmark their analysis, and our review of available research suggests that important gaps in that effort are likely to bias their calculations in a predictable way. In addition, even if we assume their selection of benchmaking studies is somehow approximately representative, each of the articles includes important features or limitations that Scurich and John do not take into account and that tend to inflate the measured recidivism rate—for example, the inclusion of nonsexual offenses as "recidivism." Finally, by incorrectly describing these studies as by and large measuring conviction-based recidivism, Scurich and John encourage policy-makers to inappropriately adjust "observed" rates by taking into account the chance of successful prosecution (given that a victim has reported), which is an irrelevant consideration when the recidivism rate in question is measured before prosecution. Indeed, three of the studies they cite use arrest or an even more expansive definition of recidivism (like treatment failure). Not surprisingly, this biases upward their hypothetical "actual" reoffending rate.

4 | UNDERLYING DATA: OFFENDER SELF-REPORTS

Scurich and John also discuss conclusions from the literature using offender self-report data to defend the idea that "actual" recidivism rates are much higher than "observed" recidivism rates. To understand why their argument is problematic, and why much of the research they cite is ultimately irrelevant, it is necessary to consider the definition of recidivism. According to the National Institute of Justice webpage, "Recidivism … refers to a person's relapse into criminal behavior, *only after the person receives sanctions or undergoes intervention for a previous crime*" (National Institute of Justice, 2020; emphasis added). Admittedly, individuals who repeatedly offend but who are never caught and convicted are "recidivists" in an important sense, but these individuals are ignored by laws that explicitly target individuals who have a criminal justice record of having committed a sexual offense. In the domain of research that attempts to understand and document *postconviction* criminal behavior, the policy-relevant challenge is to determine how likely an individual is to return to crime after being processed and punished by the criminal justice system.

Turning to the research Scurich and John cite—only one, the Langevin et al. (2004) study, even purports to measure recidivism in the relevant sense. The Abel et al. (1987) study, for instance, provides self-reporting data from paraphiliacs seeking treatment, but it does not offer any information about whether they were ever arrested, charged, convicted, or punished for a crime. Likewise, Bourke et al. (2015) explicitly state that "[n]one of these individuals (in any of the three samples) previously were arrested for a sexual offence" (p. 360). Buschman et al. (2010) use polygraph tests to characterize undetected sexual crimes that were committed by 25 individuals (unknown to the police before being arrested) who were receiving treatment for possession of child abuse images. Similarly, Bourke and Hernandez (2009) use polygraphs to measure self-reporting of undetected contact sexual offenses by individuals with child pornography convictions in a voluntary, intensive, residential sexual offender treatment program at a medium security prison, but "all disclosures are made post-conviction and post-sentencing" (p. 189). Some of the participants had committed prior sexual offenses, but Bourke and Hernandez do not tell us whether any admitted to undetected conduct that occurred after their first conviction. Ahlemeyer et al. (2000) also use polygraphs to measure self-reporting, but they "examine[] only the number of past victim and offense admissions data" (p. 128). Although some of the subjects had prior criminal history, Ahlemeyer et al. (2000) provide no information by which to discern whether the self-reported undetected sexual offenses occurred before or after their formal criminal history began—necessary information if we are to determine whether the self-reported offenses in their data ought to count as postconviction recidivism.

The Langevin et al. (2004) study presents different problems. In a 2006 article, Webster et al. (2006) describe several "fundamental methodological weaknesses [that] seriously jeopardize the degree of confidence that we may have in [Langevin et al.'s (2004)] findings" (p. 89). The first deficiency has to do with the generalizability of their findings. All of the individuals in the study were referred for psychiatric assessment or treatment, which "… is typically reserved for a minority of those charged with sex offences. In all likelihood, this sub-group represents an unusually high proportion of the more serious or atypical sex offenders charged during this period" (p. 83). Although Langevin et al. (2006) defend Langevin et al. (2004) by pointing out that "treatment has been widely provided to sex offenders in [Canadian] federal penitentiaries" (p. 110), they do not supply any information about the kinds of psychiatric disorders for which the individuals in their sample were receiving treatment, nor do they disclose the hospital providing treatment, or the kinds of treatment the hospital provided.[13] We do know that some of the comparison subjects had been charged with violent offenses like murder (p. 537), which indicates that the hospital was treating high-risk individuals and not typical sexual offenders.

In addition, the Langevin et al. (2004) study has limited relevance for assessing risk in a sentencing hearing, and it should not be used to justify control measures like registration requirements or residency restrictions imposed on individuals with prior sexual offense convictions. Langevin et al. (2004) measure the lifetime criminal history of 320 individuals with sexual crime convictions and 31 individuals with nonsexual violent crime convictions who received treatment at an unidentified hospital in Canada. Notably, "[t]he first arrest, charge, or conviction was used as the index marker event, and all subsequent charges or convictions were considered instances of recidivism" (p. 538). Using an arrest or charge is unusual in a recidivism study, as most use at least an initial conviction to mark an individual as someone who is at risk of reoffending following justice-system intervention. More importantly, Langevin et al. (2004) acknowledge that "a total of 46 (14.6%) of the 316 sex offenders on whom data were available had *never* appeared in court on a sexual offence" (p. 542).[14] An additional 87 people (27.5%) appeared only once, and the study provides no indication that these individuals were ever convicted of a sexual crime. As a conviction for a sexual crime is a threshold requirement for sentencing and the subsequent attachment of registration obligations, community notification requirements, and residence restrictions in the United States, "recidivism" of people who have never been convicted or even charged with a sexual offense is of limited relevance to assessing the risk of the policy-relevant population—individuals with sexual offense convictions.

5 | EMPIRICAL ASSUMPTION: CONSTANT RATE OF OFFENDING OVER TIME

In calculating their hypothetical "actual" recidivism rates, Scurich and John assume that an individual's probability of reoffending remains constant over time. In other words, a person's chance of reoffending is the same at year one as it is at year 25. Scurich and John acknowledge that this assumption may be problematic: "For short periods of time (a few years), this may be a reasonable assumption. However, for longer time periods of several years or decades, this assumption may well be violated" (p. 171). As it turns out, their assumption of a constant rate of reoffending is demonstrably wrong over any period, and this simplifying assumption has nontrivial consequences, again, inflating their estimates of recidivism—specifically, ignoring the dramatic reduction in criminal propensity over time implies that individuals who were released even a few years prior have much higher recidivism levels than they actually do.

5.1 | Time since release

One of the most well-established findings in criminology is that the likelihood that someone will reoffend declines with each year after release that the individual has remained offense-free (Blumstein & Nakamura, 2009). Kurlychek et al. (2012) report that "[t]he general tendency for recidivism risk to decline over time is among the best replicated results in empirical criminology. It is probably not an exaggeration to say that any recidivism study with more than a 2- or 3-year follow-up period that did not find a downward-sloping marginal hazard would be immediately suspect" (p. 75). There is ample evidence that this general conclusion applies just as well to those convicted of sexual offenses.

For instance, a steep downward trend in recidivism risk is evident in the 2003 and 2019 DOJ studies. (Unfortunately, the 2016 DOJ study does not provide the information we need to analyze this trend.) According to the 2003 DOJ report, individuals with a prior sexual offense conviction in the United States are most likely to be arrested in the first year after their release, with the recidivism rate declining every subsequent year. Of the 517 individuals released following a sexual offense conviction and who are later rearrested, 40% (205) are rearrested in the first year, 34% (176) in the second, and 26% (136) in the third.

TABLE 1 Cumulative and marginal sexual offense rearrest rates[15]

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Cumulative rearrest rate	1.9%	3.5%	4.4%	5.1%	5.9%	6.3%	6.9%	7.6%	7.7%
Marginal rearrest rate		+1.6	+0.9	+0.7	+0.8	+0.4	+0.6	+0.7	+0.1

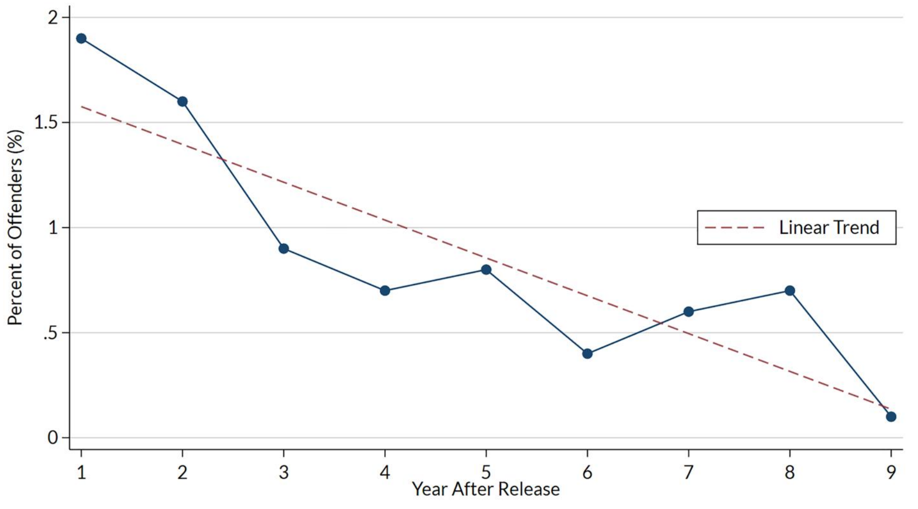

FIGURE 2 Sexual offense recidivism rate post-release[17] [Colour figure can be viewed at wileyonlinelibrary.com]

The 2019 DOJ study documents a similar downward trend. Table 1 shows the percentage of prisoners released in 30 states in 2005 after serving a sentence for rape or sexual assault who are rearrested for rape/sexual assault by year following release. The first row shows the cumulative percentage of prisoners arrested each year, up to a total of 7.7% within the 9-year follow-up period. The second row presents the percentage change of individuals rearrested each year. Table 1 demonstrates that released individuals who will *eventually* be rearrested are most often rearrested within the first year (1.9%), and other than blips in years 5, 7, and 8, the percentage of those newly rearrested declines steadily in subsequent years.

This phenomenon holds true without regard to the initial reoffense risk that an individual with a sexual offense conviction poses at the time of release: whether it begins high or low, recidivism risk appears to decline over the sexual offense-free years following release (R. K. Hanson, Harris, et al., 2014; R. K. Hanson et al., 2018).

5.2 | Age

In addition, research comfirms that, just as with individuals convicted of other kinds of crime, the reoffense risk of someone with a prior sexual offense conviction drops with age. In a 2002 study, R. K. Hanson uses data from 10 follow-up studies of adult males aged 18–70 and above with sexual offense convictions (generating a combined sample of 4,673 individuals) to study the relationship between age and sexual recidivism (R. K. Hanson, 2002). He finds that, "[i]n the total sample, the recidivism rate declined steadily with age," and "[t]he association was linear" (p. 1053).

Prentky and Lee (2007) also explore the age effect on a cohort of 136 individuals previously convicted of rape and 115 individuals convicted of child molestation, both groups with multiple priors, who had been civilly committed in a Massachusetts prison, released in 1959, and followed for the subsequent 25 years. They establish that, for individuals convicted of rape, recidivism drops linearly as a function of age. For individuals convicted of child molestation, recidivism increases from age 20 to age 40 and then declines at age 50 and significantly by age 60. The 2019 DOJ study came to similar conclusions. Alper and Durose (2019) discover that 11.8% of individuals age 24 or younger with a prior sexual offense conviction are rearrested for a new sexual offense as compared with 4.6% of those age 40 or older—that is, younger individuals are slightly more than twice as likely to be rearrested for a new sexual offense. Other researchers report similar results (see, e.g., Barbaree et al., 2009; Lussier & Healey, 2009; Lussier et al., 2010; Monahan et al., 2017; Wollert et al., 2010).

5.3 | Significance

Assuming the rate of reoffending remains constant over time ignores the abundant evidence showing that the opposite is usually true. Scurich and John's reliance on this assumption falsely elevates the projected hypothetical "actual" recidivism levels they calculate over longer periods of time, making potential recidivists appear more dangerous than they really are as the years pass—and increasingly so as they age. For the same reason, measures such as "5-year recidivism rates" are misleading in the sense that any tendency toward reoffending is heavily concentrated in the first few years. This aspect of the temporal pattern of recidivism matters as policy-makers consider how to set certain legal requirements such as the minimum duration of registration and notification obligations. Using the 2019 DOJ study as a benchmark, policy-makers could very reasonably conclude that if an individual convicted of a sexual offense has not recidivated within 9 years, his registration and notification requirements, as well as his residency restrictions, should be terminated or at least dramatically curtailed.

6 | UNDERLYING DATA: VICTIM SELF-REPORT

The third variable in Scurich and John's probabilistic model is P_r, the probability that a victim or some other party will report a sexual offense to the police. In identifying reasonable values to assume for P_r, Scurich and John make use of victim-self report data. However, their use of these data in their article is problematic, with predictable consequences for their results.

6.1 | Omitted and dated studies

Scurich and John leave the most recent national study of child sexual abuse known to the police out of their discussion. Finkelhor et al. (2012) investigate a nationally representative sample of 4,549 children from ages 1 month to 17 years living in the contiguous United States and estimate that 76.1% of sexual abuse by a nonspecified adult and 13.1% of peer sexual abuse is known to the police. These numbers are significantly higher than those Scurich and John report in their article (pp. 161–162); they refer only to a much earlier study by R. F. Hanson et al. (1999), which suggests that 12% of child sexual assaults in their sample were reported to police, and to an even earlier study, by Mullen et al. (1993), which finds that 8% of child sexual assaults they study were reported to authorities. Assuming the accuracy of the the older studies, Finkelhor et al. (2012) explain why there has been such a sharp increase in reporting: "Considerable efforts have been made during the last generation to encourage children and their families to report victimization to authorities" (p. 1).

Scurich and John also omit any discussion of a recent study of victim reporting by Jones et al. (2009). Over an 18-month period, Jones et al. (2009) interviewed female sexual assault victims who presented at an urban clinic or emergency department in Michigan. These interviews started in 2001, which is decades later than most of those in the Daly and Bouhours (2010) study. Jones et al. (2009) study 424 women, and of these, 75% (318) reported their sexual assault to the police. These women may be unrepresentative, but at least for certain groups of women and certain serious types of sexual offenses—those resulting in medical evaluation or treatment—reporting rates may be closer to the "optimistic" estimate rather than the much lower values pushed by Scurich and John. We mention this study in particular only to point out that such high reporting rates have been found in some contexts in the literature, even though Scurich and John claim otherwise, and to challenge their assertion that, *"at a minimum*, a majority of sexual offenses do not get reported by victims to the police" (p. 162).

6.2 | Questionable assumption

The studies Scurich and John cite offer insight into the probability that a sexual crime is reported conditional on it occurring, P_r or P(Report). However, given the motivation for their work, Scurich and John are not actually interested in their analysis in the rate of reporting for *all* sexual crimes; critical to their work is only the probability that a sexual offense is reported by the victim conditional on it being committed by an individual previously *convicted* of a sexual offense, $P(\text{Report}|\text{CSO})$. By choosing instead to incorporate estimated reporting probabilities about sexual offenses generally, and not sexual offenses that count as sexual recidivism, Scurich and John implicitly assume that the probability that a victim (or someone else) reports a sexual crime to law enforcement is independent of whether the alleged offender has been convicted of a sexual offense in the past—that is, $P(\text{Report}) = P(\text{Report}|\text{CSO})$.

There are good reasons to believe this assumption is incorrect. The most important reason is that victims may be much *more* likely than they normally would be to report a crime committed against them by someone with a criminal history of sexual offending, especially if that individual is subject to community notification requirements. Because most sexual offenses are perpetrated by intimates or acquaintances, victims are likely to know if the person who victimizes them has been convicted of a sexual crime in the past—even if the individual is not listed on a public sexual offender registry [National Crime Victimization Analysis Tool (NVAT) Report, 2020].

Awareness that an offender has a criminal history is particularly relevant to reporting behavior because one of the primary reasons victims give for not reporting their victimization is that they assume the police will not believe them or will blame them for what happened. Jones et al. (2009), which we discuss above, conduct a cross-sectional survey of female sexual assault victims presenting at an urban clinic or emergency department after their attack to identify the reasons women might choose not to report their rape to the police. They find that 51% of nonreporting victims were reluctant to report because "police would be insensitive or blame me"; 49% explained that "[s]ome people will not believe me"; 63% indicated that "[i]t would be just his word against mine"; and 73% worried that "[o]ther people will think I am responsible" (p. 420).

The fact that an individual has a criminal record for other sexual offenses may make a victim more confident that her account will be taken seriously and thus may make her more likely to report being victimized. It certainly seems unlikely that police would begin by assuming that an allegation that a convicted sex offender committed a new sexual offense was frivolous. The findings emerging from Jones et al. (2009) suggest that such confidence in a victim's account could lead to a massive increase in reporting. Moreover, a relatively high chance of reporting is also probable for an offense committed by a stranger because victims are generally much more likely to report sexual offenses perpetrated against them by strangers (Chen & Ullman, 2010).

There are two other reasons to suspect that average sexual offense reporting probabilities are systematically lower than the reporting probabilities for offenses committed by individuals with sexual offense convictions. First, when it comes to publicly registered individuals, we might understandably anticipate some number of reports occurring even when the potential recidivist commits no crime—either because of mistaken identity (with victims identifying "usual suspects") or because of hostility toward individuals previously convicted of sexual offenses. This bias cuts in the opposite direction, nudging reporting rates closer to actual offense rates. Second, further undermining Scurich and John's assumption is the possibility that victims who know the offender—that is, most victims—may be relatively *less* likely to report a crime if that person is *not* an already-convicted and labelled "sex offender." In particular, victims may hesitate to report a crime for fear of "ruining someone's life" due to the stigma and burdens associated with a sexual offense conviction.

Finally, a large majority of sexual crimes (perhaps > 90%) are committed by individuals who have not been previously convicted of a sexual crime (Sandler et al., 2008). Consequently, the average reporting probabilities in the studies cited by Scurich and John are made up primarily of the reporting probabilities for crimes committed by individuals without a criminal history of sexual crime convictions. This fact—combined with the special considerations surrounding reporting a crime against an individual with an extant sexual offense record—suggests that "average" reporting rates are a poor proxy for the reporting rates for crimes committed by sexual recidivists.

6.3 | Significance

If victims are systematically more likely to report crimes committed by individuals previously convicted of a sexual offense, then the reporting probabilities Scurich and John use in their simulations are lower than they should be because $P(\text{Report}|\text{CSO}) > P(\text{Report})$. This relationship is important because the difference between the "actual" and "observed" recidivism rates is proportional to the reporting and conviction probabilities they assume. The greater the probability of reporting (or conviction), the smaller the difference between actual and observed rates. By using smaller reporting probabilities, Scurich and John necessarily inflate their hypothetical "actual" recidivism rates.

7 | UNDERLYING ASSUMPTION: CHANCE OF SUCCESSFUL PROSECUTION

The final variable in Scurich and John's probabilistic model is P_c, the likelihood of a successful prosecution given that the sexual offense was reported to the police. Scurich and John cite no studies to benchmark their assumptions about this variable, and they proceed instead by hazarding three possible values: $P_c = 0.25$, 0.50, and 0.75. We note just how important using, say, their low value is to generating their hypothetical "actual" recidivism rates—it effectively assumes that recidivism rates are many times higher than what we observe. Although we agree that it requires some effort for prosecutors to secure convictions in sexual crime cases, the likelihood of a conviction (conditional on a report—i.e., law enforcement becoming aware of an actual victimization) in a case involving an alleged offender who has already been convicted in the past of a sexual crime seems likely to be very high, especially if we include the possibility of plea bargains, which may be easier for defendants to stomach if they already have a sexual offense conviction in their past, and the possibility of a wrongful conviction of an innocent individual, which may be significantly more likely when the defendant is publicly registered as a "sex offender."

When a victim reports that they have been sexually assaulted by an individual previously convicted of a sexual offense, the alleged offender is already known to the criminal justice system, which makes a difference at every stage of the process. The police are gatekeepers, and we acknowledge that they are notoriously reluctant to believe victims of sexual crimes (Campbell, 2005). If a report is made about an individual with a prior conviction for a sexual offense, however, that person's criminal record, which the police can easily access, will inevitably make the allegation seem more credible. Moreover, the police can consult already-collected fingerprints and/or DNA, more easily secure a search warrant and gather inculpatory evidence, and locate and arrest the individual with less difficulty. An arrest, in turn, allows the police to conduct a search incident to arrest (which might lead to more evidence) and interrogate the suspect (which might lead to more evidence and/or a confession, perhaps for other crimes for which the police had *not* received a report). Someone with an existing record may be less likely to be released on bail, making it more difficult to assemble a defense down the road. All of this makes the police more likely to refer the case for prosecution.

The prosecution in turn will be more likely to file charges because they know they are highly likely to secure a conviction—even if the case goes to trial. Their chances are not just very high because of the evidence likely to be compiled by the police, but also because prosecutors have the upper hand in any plea bargaining that occurs. The Supreme Court held in *Bordenkircher v. Hayes* (1978) that a prosecutor may carry out a threat made during plea negotiations to add additional, more serious charges if a defendant rejects an offer, and there are many possible additional charges and sentencing enhancements from which to choose at both the state (National Conference of State Legislatures, 2006) and federal levels (Myers, 2015). Therefore, it seems highly likely, if not almost certain, that a case referred by the police involving a sexual crime will end in a conviction of some sort, and prosecutors are unlikely to use their discretion to reduce any sexual offense charge to a nonsexual charge when the allegation is sexual in nature and the defendant already has a prior sexual offense conviction.

Should the defendant decide to opt for a trial, evidentiary rules increase the prosecution's chance of winning, which of course influences the defendant's decision of whether to decline a plea offer and choose a trial. (We note that, conditional on a prosecutor pursuing charges, the defendant is very likely—well in excess of 90% in many jurisdictions—to plead guilty.) Under the Federal Rules of Evidence and most state analogs, prosecutors are allowed to introduce propensity evidence in sexual offense cases under the theory that individuals convicted of sexual crimes (especially those convicted of child molestation) are especially likely to reoffend (Lave & Orenstein, 2013). There is virtually no time bar to evidence of prior sexual misconduct, and it can take the form of uncharged allegations, dismissed charges, convictions, and even acquittals. Such evidence is specifically designed to make it easier for the prosecution to prevail in an otherwise weak case.

For all of these reasons, the range of values Scurich and John assume for P_c is unreasonably low ($P_c = 0.25$, 0.50, and 0.75). Recall, we are assuming a reported sexual offense against an individual who has a record of committing sexual offenses in a world with great pressure on prosecutors to secure convictions, especially in these cases and especially against recidivists, and in a world of pervasive plea bargaining, expansive substantive criminal law, flexible evidentiary rules, and other procedures that make it particularly difficult for a defendant with a sexual offense record to prevail—even if wrongly accused. Even if we assume that Scurich and John's probabilistic model is otherwise valid, all three values they use for P_c artificially inflate their hypothetical "actual" recidivism by a significant amount.

8 | ASSESSING THE MATHEMATICS BEHIND THE MODEL

We demonstrate above that the data Scurich and John use and the empirical assumptions they make are problematic, and that they are likely, especially when combined to magnify each other, to produce inaccurate conclusions that are predictably biased in a certain direction—in particular, toward inaccurately high hypothetical sexual offense recidivism rates. We now briefly turn to the mathematics behind their model. In order to more fully understand Scurich and John's analysis and what drives their conclusions, we replicate their model. Scurich and John generously shared their work with us,[16] and we were able to reproduce their simulation and results in Stata using the distributions stated in their Figure 2 (p. 164). In what follows, we show that behind the seeming neutrality of their setup is a structural bias.

8.1 | Does the model actually assume different propensities of offending?

Scurich and John state that unlike early models of offending and recidivism, they "assume a diverse population of offenders with different propensities of offending" (p. 166). Such an assumption would be an improvement over earlier models because it would more accurately reflect the true universe of individuals at risk of reoffending. As we explain in Section 5 of this paper, individuals with sexual offense convictions are not monolithic. The likelihood of reoffending varies with the offense of conviction, prior offense history, family relationships, and various demographic factors such as age at time of release from custody (R. K. Hanson, 1998). As we show below, however, Scurich and John's approach does not seem to work in the way they report that it does. In fact, their complex process for estimating recidivism rates that we describe in Figure 1 can be simplified to a single step, which we show in Figure 3. Rather than assuming that individuals at risk of recidivating have different propensities for reoffending, at least on paper their model assumes that all 100 offenders in their simulation have the *same* probability of recidivating ($P[\text{Recid.}] = \mu/(1+\mu)$). Scurich and John use a Poisson distribution, where the parameter (λ) follows a gamma distribution; this is more commonly known as a gamma-Poisson distribution with parameters ($\alpha = \mu, \beta$). As Scurich and John describe it, they "assume that λ_i follows an exponential distribution, which is a special case of a gamma distribution" (p. 166). So, under this assumption, Scurich and John actually use a special case of the gamma-Poisson distribution with parameters ($\alpha = \mu,\ \beta = 1$), which is analogous to a geometric distribution with parameter ($p = 1/(1+\mu)$).[18]

In the context of their model, the number of offenses committed by offender *i*—denoted S_i—has a geometric distribution (p) in which $P(S_i = x) = p\,(1-p)^x$. Using this distribution, the probability that each offender commits no new offenses is $p\ [= P(S_i = 0) = (p)((1-p)^0) = p = 1/(1+\mu)]$. Conversely, the probability of recidivating (i.e., committing at least one new offense) is $1-p\ [= P(S_i \geq 1) = 1 - P(S_i = 0) = 1-p = \mu/(1+\mu)]$. As a result of the previous statement, we can say that Scurich and John's model assumes that each offender will recidivate with the same probability: $\mu/(1+\mu)$.

But Scurich and John choose μ, implicitly assuming the same probability of recidivism for all offenders. In light of this facet of the model, statements such as "even a mean rate of 1.0 sexual offenses over the time period per individual results in an actual recidivism rate of 50% over the time period" (p. 169) are neither surprising nor informative. As Table 2 shows, for a mean rate, μ, of 1.0, the authors simply assert that each offender has a 50% probability of recidivism (1/[1+1]). If each trial in their simulation consists of 100 offenders, with each assumed to have a 50% chance of reoffending, is it surprising that over 1,000 trials an average of 50 out of 100 reoffend?[19] Put another way, the model implicitly assumes a rate of recidivism by choosing a distribution and the parameters for that distribution. Table 2 shows this mapping.

We now turn to their Figure 6 (p. 168), which we recreate using our analysis that precisely mimics their model. As presented by Scurich and John, Figure 6 shows that as μ (i.e., the assumed average number of offenses per offender) increases, the recidivism rate produced by their model also increases. This relationship makes sense

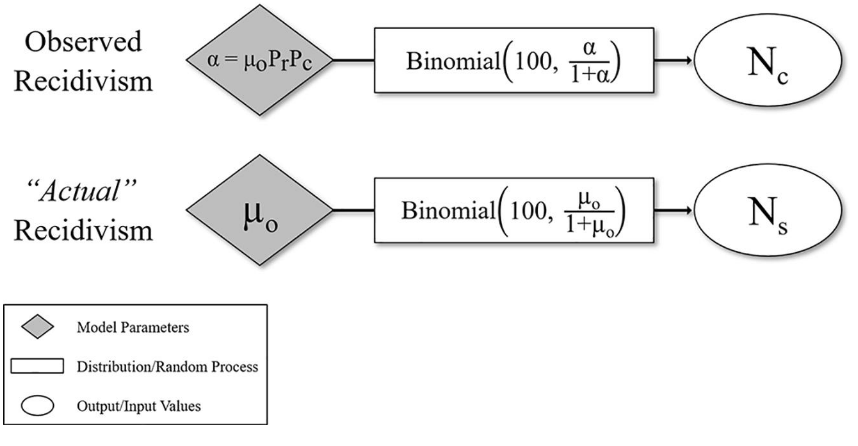

FIGURE 3 Simplified simulation process [Colour figure can be viewed at wileyonlinelibrary.com]

TABLE 2 Probability of recidivism assumed by Scurich and John's model

Model parameter (μ)	Assumed probability of recidivism	Average number of offenses per offender
1/16	5.9%	1/17
1/8	11.1%	1/9
1/4	20.0%	1/5
1/2	33.3%	1/3
1	50.0%	1/2
2	66.7%	2/3
4	80.0%	4/5
8	88.9%	8/9
16	94.1%	16/17

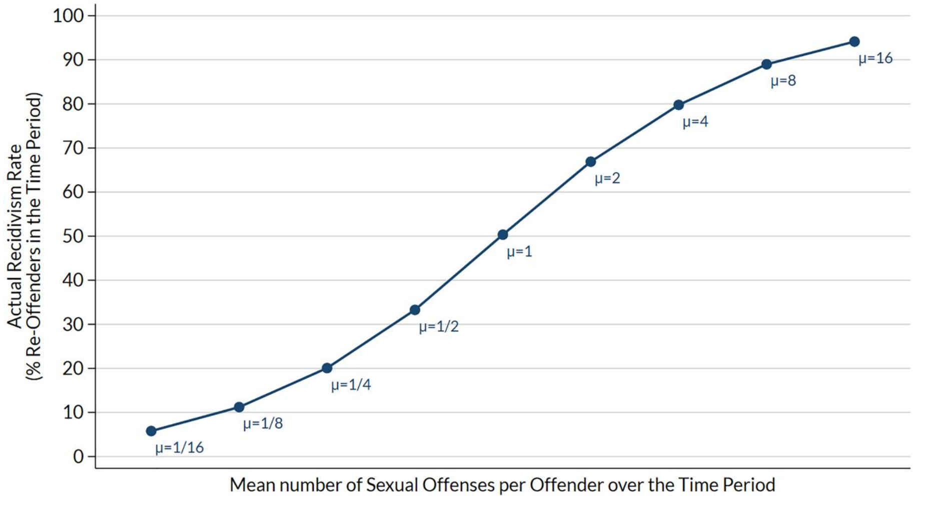

FIGURE 4 Scurich and John's Figure 6 (replicated)[20] [Colour figure can be viewed at wileyonlinelibrary.com]

intuitively, but it is also a mechanical relationship that derives from their assumptions. If we assume that offenders commit more crimes on average, then we should expect more potential offenders to commit at least one new crime over any period of time.

At first glance this relationship might appear reasonable (Figure 4). However, if you look closely you will notice that the horizontal distance between dots does not match the increase in μ. Each dot has the same horizontal distance from the previous dot despite different increases in μ (i.e., if the x-axis shows μ, then $\mu = 1/16$ to $\mu = 1/8$ should be closer together than $\mu = 16$ and $\mu = 8$). What Scurich and John have done is plot the natural log of μ, $\ln(\mu)$, which means moving one dot to the right doubles the value of μ. If you correctly plot the relationship between μ and recidivism, their analysis produces Figure 5.

Now we see a completely different relationship; recidivism increases dramatically when μ is small, with diminishing returns as μ increases. Most importantly, this figure shows that Scurich and John's model is extremely sensitive to their choice of μ when μ is two or less. If the "actual" propensity parameter is two or less, the recidivism rate produced by their model can be wildly inaccurate even when using a propensity parameter that is close to the

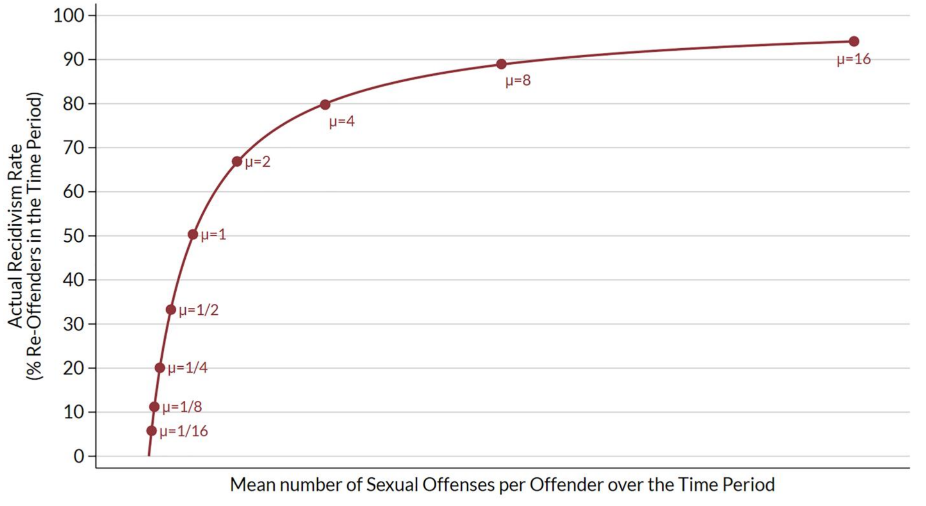

FIGURE 5 Scurich and John's Figure 6 (corrected) [Colour figure can be viewed at wileyonlinelibrary.com]

"actual" value. Suppose the "actual" μ is 0.5 (i.e., the mean number of sexual offenses across all offenders in a given period is 0.5), but Scurich and John believe it is actually equal to 1. Their model would suggest that the recidivism rate is 50%, but the "actual" recidivism rate would be 33.3%. Of the scenarios considered by Scurich and John (Table 1, p. 170), 58.6% (21/36) involve calibrating propensities of two or less, underscoring the relevance and impact of this limitation.

8.2 | Significance

Scurich and John claim that their model allows them "to estimate actual recidivism rates for sexual offenders given observed rates of reoffending" (p. 171). If this is true, it is true only in the narrowest sense. By assuming μ, the authors implicitly assume the same probability of recidivating for all individuals from the beginning. Thus, any relevance of their hypothetical "actual" recidivism rates to real-world "observed" recidivism rates is minimal. Beyond the simple fact that not all sexual offenses are included in existing recidivism rate calculations, their analysis is simply a set of hypotheticals turning on (mostly unreasonable) assumptions about the distribution of tendencies to reoffend and probabilities of reporting and conviction. This exercise considers a wide range of parameters—some of which are implausible—and can, unsurprisingly, produce virtually every possible recidivism rate, from low to very high, depending on how you combine the assumptions. Regardless of the parameter values chosen, a sensitivity analysis shows that the results are highly sensitive to small changes in many of the values, suggesting the model is as likely to mislead as it is to inform.

9 | PUBLIC POLICY IMPLICATIONS

In their paper, Scurich and John purport to show that individuals with a sexual crime conviction pose a far greater risk than most experts believe, the implication being that society may require strong measures to control sexual recidivism—and, thus, that we should be wary of any attempt to more accurately target existing approaches. However, in this response, we show that the model they use to measure what they call "the dark figure of recidivism" produces little of practical value to policy-makers or judges. The numbers they offer are inflated by the benchmarking choices they make, as well as the very strong (and inaccurate) underlying assumptions about reoffending behavior and victim and law enforcement behavior on which they rely.

Where does that leave us? Instead of imposing coercive measures like civil commitment, registration obligations, community notification, and residency restrictions that research shows are deeply problematic, ineffective, and perhaps counterproductive from a public-safety perspective, government should instead focus its limited resources on deterring the potential offenders who are now often overlooked and present, as a group, a much larger threat—intimates and acquaintances who have never been caught or convicted of a sexual crime. The vast majority of sexual offenses are committed by individuals who are not potential recidivists, and although some who have been convicted of such crimes do reoffend upon release, recidivism numbers are sufficiently low that it makes little public-safety sense to focus so much effort and so many resources on what is a relatively small population. This is true even if we leave to one side the fact that there is no evidence that registration, notification, and residence restrictions are effective at reducing recidivism (whether it is low or high) and even some evidence that these laws actually increase recidivism by exacerbating well-validated recidivism risk factors (Prescott, 2016; Prescott & Rockoff, 2011).

To confirm that there are far fewer victims of sexual recidivists than victims of individuals never before convicted of a sexual offense, we return to the 2003 and 2019 DOJ studies, both of which show that individuals released after committing a sexual offense commit considerably fewer new sexual crimes in total than individuals who were released after committing *other* crimes. (The 2016 study does not provide the information necessary to make similar calculations for its sample population.) Although individuals convicted of sexual offenses in the 2003 study were four times more likely to be rearrested for a sexual crime than other releasees, as a group they committed far fewer sexual crimes in ensuing years than releasees without prior sexual offense convictions. Of the 9,691 individuals with sexual crime convictions released in 2003, 517 (5.3%) were rearrested for a new sexual crime as compared with 3,328 (1.3%) of the 262,420 individuals with nonsexual convictions released that same year (p. 24). Langan et al. (2003) explain that if you assume each releasee victimized no more than one victim, the combined total number of new sexual crimes is 3,845 (517 + 3,328), implying that individuals with sexual crime convictions account for just 13% of the 3,845 sexual crimes committed by releasees, and individuals with no record of a sexual offense account for 87% of the total (p. 24). Put differently, individuals released for something other than a sexual crime commit more than six times as many new sexual crimes as released sexual offenders (Langan et al., 2003). Importantly, these back-of-the-envelope calculations—which ignore entirely all sexual offenses committed by individuals with no prior convictions whatsoever—align generally with research indicating that a large majority (>90%) of sexual offenses involve individuals with no prior sexual offense record (see, e.g., Sandler et al., 2008).

Moreover, according to the 2019 DOJ study, individuals with sexual offense records were only around three times more likely to be arrested for rape or sexual assault than other released prisoners (7.7% vs. 2.3%). (Alper & Durose, 2019, p. 1). (For those paying attention, the risk posed by individuals released after imprisonment for a sexual offense appears to be getting closer over time to the sexual offense risk posed by other releasees.) As before, individuals with a prior record involving a sexual offense committed far fewer sexual crimes in the aggregate than other releasees. The former accounted for 16% of the 12,000 arrests for rape or sexual assault that occurred among all prisoners released in 2005 in the study's 30 states. Other types of releasees were responsible for the other 84% (Alper & Durose, 2019, p. 11).

It is worth reiterating that sexual offense recidivism appears to be at best unaffected by postrelease laws like registration and notification (Logan & Prescott, 2021; Prescott & Rockoff, 2011). In such a light, the calculations above support investing in policies to reduce victimization by “first-time” offenders. Such policies are likely to be a much better use of society's time and resources than focusing on recidivists. Indeed, one could understandably worry that our focus on sexual offense *recidivism* has distracted policy-makers from the goal that really matters, which is reducing the overall frequency of sexual offending behavior.

10 | CONCLUSIONS

In this paper, we revisit Scurich and John's (2019) attempt to assess the size and scope of the "dark figure" of sexual recidivism. We offer criticisms of many aspects of their work, including their selection and interpretation of their benchmarking studies as well as their model's empirical assumptions and its basic construction, all of which operate to produce biased estimates of sexual recidivism. In the end, the work amounts to an inflation of the unrepresentative "observed" recidivism rates they cite by multiplying them by a function of P_r and P_c that is not well grounded in a thorough understanding of existing research and the criminal justice system.

The gravity of these problems is exacerbated by the context in which they occur. Individuals convicted of sexual offenses are some of the most maligned members of society, and they suffer under onerous criminal sanctions and civil obligations (Ellman, 2021). These policies and practices follow from fears about the "dark figure"; indeed, society has decided to use its limited law enforcement resources to incarcerate many of these individuals indefinitely in locked mental hospitals rather than devote these resources to the much more serious problem—disrupting first-time offenses, which make up as many as 90% of sexual offenses in this country.

A study that can be interpreted as rigorous support for a very large "dark figure" is likely to further distract policy-makers and judges in ways that put more victims at risk and make society less safe. Judges and policy-makers already understand the basic idea that "observed rates" do not capture all instances of recidivism. But decision-makers are likely to be comforted to see *actual numbers* in a peer-reviewed paper. Yet these numbers are driven entirely by Scurich and John's assumptions, which are not well justified, and ultimately misleading when brought together in their model. Judges and legislators must peek under the hood to see this, and we hope this paper offers a guide to what's going on in Scurich and John's analysis, and how relatively small tweaks to their choices will tell entirely different stories.

ENDNOTES

1 Emphasis in the original.

2 Emphasis in the original.

3 Emphasis in the original.

4 We say "in very rough terms" because we are technically conflating recidivism rates with the average number of recidivist crimes per offender for the population. More precisely, they "inflate" the value of μ_0 in their model (which is the mean number of offenses per offender for the population) by $1/(P_r \times P_c)$. They argue that we only observe $P_r \times P_c$ percent of (recidivist) offenses, so the population average number of offenses is much larger. The scaling factor to go from observed to actual recidivism rate is $R_{act} = R_{obs}/(R_{obs} + P_r P_c (1 - R_{obs}))$.

5 To help explain what the parameter μ_0 represents, consider a group of 100 offenders. If these 100 individuals have a value of $\mu_0 = 2$, then the average number of new offenses committed by an offender is two, and the total number of offenses committed by the group would be 200. This is an average because some will commit more than two offenses while others will commit fewer than two.

6 Each offender is assigned a value of λ_i. If a person has a $\lambda_i = 4$, then the average number of new offenses this person commits per trial is 4. This is an average because in some trials they will commit more than four offenses, while in other trials they will commit fewer than four (see Figure 3, p. 165).

7 We hasten to add that most convictions result from guilty pleas, and that wrongful convictions, especially with respect to guilty pleas to more minor sexual offenses, seem highly plausible in this context (for instance, when a prosecutor threatens more serious sexual offense charges against someone with a prior sexual offense conviction). Scurich and John do not deflate their recidivism estimates to account for this upward bias.

8 Arrests for technical violations of the conditions of supervision are excluded from the study. Also excluded are arrests for minor offenses including breaches against public peace, invasion of privacy, prostitution, obstruction of justice, liquor law violations, and traffic offenses.

9 We describe R. K. Hanson's criticism of the RCMP policies for retaining criminal records in more detail in Section 3.2.

10 E-mail on file with authors. We are grateful to R. K. Hanson for his helpful and candid explanation.

11 The study period began in 1959 and formally ended on January 1, 1985. The last charge posted was in October 1984. Prentky et al. characterize it as a 25 year study, and so we do as well.

[12] On 11 April 2020, one of us (TRL) wrote to Amy Phenix, an author of the R. K. Hanson et al. (2014) study, to clarify how the authors define recidivism. On 13 April 2020, Phenix confirmed that the article uses arrest—not charge—as the recidivism measure. E-mail on file with authors.

[13] It was actually R. K. Hanson (2006) who clarified that all of the persons in the Langevin et al. (2004) study came from the same hospital (p. 103).

[14] Emphasis added.

[15] For a different depiction of the 2019 DOJ findings, see Figure 2. It provides a visual display of the percentage of those convicted of rape and/or sexual assault who were arrested for a rape or sexual assault by year after release. We include a trend line to show that the rate decreases over time.

[16] On 26 November 2019, we e-mailed Scurich and John requesting a copy of their data and programs to back up their modeling. In January 2020, they shared with us summary results for the nine simulations they used to produce Figure 6 (p. 169) in their paper. Unfortunately, they did not include the syntax necessary to reproduce the Monte Carlo simulations, such as commands to generate variable distributions. Fortunately, it turned out we did not need anything else from them; we were able to reproduce what they must have done. Scurich and John used a now-defunct Excel add-in tool to generate their results. We assume we have everything they had that was helpful to replicating their analysis, and we are grateful for their assistance.

[17] Source: Alper and Durose (2019).

[18] A unique property of the geometric distribution is that it is memoryless, meaning the number of offenses an individual commits before today does not impact the probability of reoffending in the future. For example, suppose an individual commits X new offenses in the first half of the period. The probability that the individual will commit at least one more offense (i.e., $S_i \geq X + 1$) by the end of the period is the same regardless of X. Using a geometric distribution, as Scurich and John have inadvertently done, assumes that the likelihood of an individual reoffending is the same for each day and does not depend on what happened yesterday.

[19] As an analogy, consider flipping a coin 100 times and counting the number of times it lands on heads. The probability of landing heads is 50% for each flip, so after 100 flips, we expect to have about half land heads. In any set of 100 flips the number of heads may be a few more or less than 50, but if we repeat this 1000 times the average number of heads will be 50 out of 100 or 50%.

[20] Scurich and John (p. 168): "Figure 6 mean number of actual reoffenders (N_s) from a population of $N = 100$ over a fixed time period by mean number of sexual offenses per offender over the time period (from 1/16 (= 0.0625) to 16)."

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APPENDIX

Theorem 1: If X ~ Poisson(λ) and λ ~ Exponential(μ) then X has a Geometric distribution with parameter $p = \frac{1}{1+\mu}$ and a probability mass function:

$$f_X(x) = p(1-p)^x \qquad x = 0, 1, 2, \dots,$$

Definitions:

$$\lambda \sim Exponential(\mu):\ f_\Lambda(\lambda) = \frac{e^{-\lambda/\mu}}{\mu} \qquad X \sim Poisson(\lambda):\ f_{X|\lambda}(x|\lambda) = \frac{\lambda^x e^{-\lambda}}{x!}$$

Proof: [1,2]

(1) $$f_X(x) = \int_0^\infty f_{X|\lambda}(x|\lambda)\, f_\Lambda(\lambda)\, d\lambda$$

(2) $$= \int_0^\infty \frac{\lambda^x e^{-\lambda}}{x!} \frac{e^{-\lambda/\mu}}{\mu}\, d\lambda$$

(3) $$= \frac{1}{\mu}\frac{1}{x!}\int_0^\infty \lambda^x e^{-\lambda/\mu} e^{-\lambda} d\lambda$$

(4) $$= \frac{1}{\mu}\frac{1}{x!}\int_0^\infty \lambda^x e^{-\lambda\left(1+\frac{1}{\mu}\right)} d\lambda$$

(5) $$= \frac{1}{\mu}\frac{1}{x!}\int_0^\infty \left(\frac{t}{1+\frac{1}{\mu}}\right)^x e^{-t}\frac{1}{1+\frac{1}{\mu}} dt$$

(6) $$= \frac{1}{\mu}\frac{1}{x!}\int_0^\infty \frac{1}{\left(1+\frac{1}{\mu}\right)}\frac{1}{\left(1+\frac{1}{\mu}\right)^x}\, t^x e^{-t}\, dt$$

(7) $$= \frac{1}{\mu}\frac{1}{x!}\frac{1}{\left(1+\frac{1}{\mu}\right)^{x+1}}\int_0^\infty t^x e^{-t}\, dt$$

(8) $$= \frac{1}{\mu}\frac{1}{x!}\frac{1}{\left(1+\frac{1}{\mu}\right)^{x+1}}\Gamma(x+1)$$

(9) $$= \frac{1}{\mu}\frac{1}{\left(1+\frac{1}{\mu}\right)^{x+1}}$$

(10) $$= \frac{1}{\mu}\left(\frac{\mu}{1+\mu}\right)^{x+1}$$

(11) $$= \frac{1}{\mu}\frac{\mu}{1+\mu}\left(\frac{\mu}{1+\mu}\right)^x$$

(12) $$= \frac{1}{1+\mu}\left(1-\frac{1}{1+\mu}\right)^x$$

(13) $$= p(1-p)^x$$

[1] The first equality is given by the Law of Total Probability.

[2] Substituting $t = \lambda\left(1+\frac{1}{\mu}\right)$ in step (5) and $p = \frac{1}{1+\mu}$ in step (12).

Theorem 2: Let S_i denote the number of new offenses committed by offender *i* and N_i indicate whether offender *i* committed one or more new offenses. If S_i follows a Geometric distribution with parameter $p = \frac{1}{1+\mu}$, then N_i is a Bernoulli variable with parameter $q = \frac{\mu}{1+\mu}$ and the probability mass function:

$$f_{N_i}(x) = q^x(1-q)^{1-x} \qquad x = 0, 1$$

Definitions:

From Theorem 1: $S_i \sim Geometric(p)$: $f_S(x) = p(1-p)^x, \quad where\ p = \frac{1}{1+\mu}$

Proof:

Probability of Committing No New Offenses (N_i = 0)	Probability of Committing 1 or More New Offenses (N_i = 1)
(1) $P(N_i = 0) = P(S_i = 0)$	(8) $P(N_i = 1) = 1 - P(S_i = 0)$
(2) $P(N_i = 0) = f_S(0)$	(9) $P(N_i = 1) = 1 - f_S(0)$
(3) $P(N_i = 0) = p(1-p)^0$	(10) $P(N_i = 1) = 1 - (p(1-p)^0)$
(4) $P(N_i = 0) = p$	(11) $P(N_i = 1) = 1 - p$
(5) $P(N_i = 0) = \dfrac{1}{1+\mu}$	(11) $P(N_i = 1) = \dfrac{\mu}{1+\mu}$
(6) $P(N_i = 0) = 1 - \dfrac{\mu}{1+\mu}$	(12) $P(N_i = 1) = q$
(7) $P(N_i = 0) = 1 - q$	

Taking the results from (7) and (12):

$$f_{N_i}(x) = \begin{cases} 1-q, & x = 0 \\ q, & x = 1 \end{cases}$$

$$f_{N_i}(x) = q^x(1-q)^{1-x} \qquad x = 0, 1$$

MONTE CARLO SIMULATION SYNTAX

```
#delimit;
set more off;
set matsize 1000;
mat drop _all;
clear all;
local folder "C:\Users\user\Desktop\Article Response";

program MCsim, rclass;
version 15;
drop _all;

syntax, μ(real) pr(real) pc(real);
return scalar μ = `μ';
return scalar Pr = `pr';
return scalar Pc = `pc';

set obs 100;
gen λi = rexponential(`μ');
gen Si = rpoisson(λi);
gen Ns = (Si > 0);

gen Ri = rbinomial(Si, `pr');
replace Ri = 0 if Ri = = . & Si = = 0;
replace Ri = Si if `pr' = = 1;
gen Nr = (Ri > 0);

gen Ci = rbinomial(Ri, `pc');
replace Ci = 0 if Ci = = . & Ri = = 0;
gen Nc = (Ci > 0);

tabstat Si Ri Ci, s(mean) save;
mat A = r(StatTotal);
return scalar Si = A[1,1];
return scalar Ri = A[1,2];
return scalar Ci = A[1,3];

tabstat Ns Nr Nc, s(sum) save;
mat B = r(StatTotal);
return scalar Ns = B[1,1];
return scalar Nr = B[1,2];
return scalar Nc = B[1,3];
end;

set seed 3242020;
```

```
local j = 0;
forvalues i = 0(1)8 {;
local mu = (2^`i')/16; /* 1/16 1/8 1/4 1/2 1 2 4 8 16 */
foreach r of numlist 1 .35 .15 {; /*P(Report) */
foreach c of numlist .75 .50 .25 {; /*P(Conviction) */

simulate Si = r(Si) Ri = r(Ri) Ci = r(Ci) Ns = r(Ns) Nr = r(Nr) Nc = r(Nc) Pr = r(Pr) Pc = r(Pc), reps(1000) nodots:
MCsim, μ(`mu') pr(`r') pc(`c');

gen μ = (2^`i')/16;
gen trial = _n;

if `j' = = 0 {;
tempfile data; save `data';
};

else {;
append using `data';
save `data', replace;
};

local `j++';

};
};
};

/* Alternative Method - Nc in each trial is Binomial(n = 100, q) */
gen p = μ/(1+μ);
gen alpha = μ*Pr*Pc;
gen q = alpha/(1+alpha);

gen Ns_eqn = rbinomial(100, p);
gen Nc_eqn = rbinomial(100, q);

order trial;
save "`folder'\CalibrationData.dta", replace;
```