THE PREDICTIVE VALUE OF CRIMINAL BACKGROUND CHECKS: DO AGE AND CRIMINAL HISTORY AFFECT TIME TO REDEMPTION?*

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Criminal record checks are being used increasingly by decision makers to predict future unwanted behaviors. A central question these decision makers face is how much time it takes before offenders can be considered "redeemed" and resemble nonoffenders in terms of the probability of offending. Building on a small literature addressing this topic for youthful, first-time offenders, the current article asks whether this period differs across the age of last conviction and the total number of prior convictions. Using long-term longitudinal data on a Dutch conviction cohort, we find that young novice offenders are redeemed after approximately 10 years of remaining crime free. For older offenders, the redemption period is considerably shorter. Offenders with extensive criminal histories, however, either never resemble their nonconvicted counterparts or only do so after a crime-free period of more than 20 years. Practical and theoretical implications of these findings are discussed.

A person's criminal history record is a good predictor of his or her future criminal behavior (Gendreau, Little, and Goggin, 1996). Consequently, as criminal history records become more available, employers, landlords, and others are increasingly using criminal history records to make decisions about individuals (SEARCH, 2005; Stoll and Bushway, 2008). In the United States, 40 state criminal history repositories reported doing 172 percent more name-based background checks for non–criminal-justice agencies in 2008 than they did in 2006, an increase from 15.5 million to 42 million (U.S. Department of Justice, 2008, 2009).[1] In the first 6 years since its launch in Britain in 2002, the Criminal Records Bureau (2009) did more than 19 million checks. In the Netherlands in 2005, the Criminal Records Office nearly tripled the number of checks it did in 1999, increasing from 95,000 to more than 250,000 (Brok, 1999; Valk et al., 2006).

The increased use of records has highlighted the need for research on how to use this information articulately. Simply excluding all individuals with a criminal history record from consideration might lead to charges

1. We only report the increase for the 40 states that reported in both 2006 and 2008. This number does not count the fingerprint-based searches that the state repositories do for employers and government agencies. It also does not count the civilian background checks done by the Federal Bureau of Investigation (10 million in 2005; SEARCH, 2005) or the checks done by private companies. The decentralized nature of the U.S. criminal justice system and the use of private venders by employers make it impossible to generate an accurate estimate of the number of checks done in the United States (SEARCH, 2005).

of racial discrimination based on disparate impact.[2] Moreover, failing to consider all individuals with criminal history records could impact the labor supply significantly, particularly for low-skill jobs. Indeed, survey research has shown that not all employers want to exclude all offenders (Holzer, Raphael, and Stoll, 2006; Pager, 2003). Instead, employers and other decision makers increasingly want to understand the relative risk posed by individuals with different types of criminal history records. This desire was articulated clearly by the National Task Force on the Criminal Backgrounding of America in 2005, which stated the following:

> (a)greed that a risk management analysis is the appropriate relevancy approach, as opposed to automatic rejection on the basis of a criminal record. More study is needed to determine which factors should be applied, and how they should be applied to different circumstances, to make appropriate relevancy decisions for various positions of employment, licensure, and other services. Little guidance is currently available to end-users on fair and prudent use of record check results in applicant selection decisions. The Task Force recommends that study is needed to develop relevancy guidelines for end-users. (SEARCH, 2005: 17)

The time that has passed since an individual's last conviction is one of the most important characteristics that can differentiate between individuals with criminal history records. For policy makers, the central question has become the following: How many years does it take before a person is "redeemed," or in practical terms, how many years of nonoffending does it take before an offender begins to resemble a nonoffender in terms of his/her probability of offending? We will refer to this period as "time to redemption." As we will document, court cases in at least two countries have appealed to empirical evidence to resolve this issue, but the research itself is in its infancy, and many questions remain.

In this article, we advance the existing research by applying formal hazard models to a sample of Dutch offenders. The hazard models efficiently consider other factors such as age at conviction and criminal history in addition to time since last conviction. The increased efficiency allows for

2. African Americans are six times more likely to be sentenced to prison than are Whites at some point in their lifetime (U.S. Department of Justice, 2003). Excluding individuals with a criminal history record, therefore, means that African Americans will be less likely to be employed. As a result, the U.S. Equal Employment Opportunity Commission has recommended that employers must show that the criminal history record is relevant for the job in question before excluding candidates (Equal Employment Opportunity Commission, 1987, 1990).

smaller standard errors than in the empirical hazard models that have been used in prior research. This move toward smaller standard errors, which makes differentiation easier, raises important questions about what it means to be similar. This article also advances the literature by using a sample of nonoffenders to create the comparison group. We follow the standard approach and compare people of similar age, but as we will make clear in the article, identifying the appropriate nonoffender group is an important and neglected part of the discussion. We will return to this issue in the conclusion. We proceed in the next section by reviewing the recent court cases as well as the small but growing literature in this area before presenting our results.

RECENT EVENTS

The relevance of the central question about "time to redemption" has been highlighted by the following recent events: 1) a ruling by the U.S. Court of Appeals for the Third Circuit in 2007 and 2) a British court case in 2008.

RECENT EVENT 1: U.S. COURT OF APPEALS FOR THE THIRD CIRCUIT IN 2007

In a Title VII court case, *El v. SEPTA* (2007), Mr. El, who was fired from his job as a bus driver for the Philadelphia area mass transit corporation (SEPTA) as a result of his 40-year-old conviction for murder, disagreed with SEPTA's decision. In this case, SEPTA justified its policy of a lifetime ban from employment for those convicted of a violent crime based on the argument that "(6) it is impossible to predict with a reasonable degree of accuracy which criminals will recidivate, (7) someone with a conviction for a violent crime is more likely than someone without one to commit a future violent crime irrespective of how remote in time the conviction is" (*El v. SEPTA*, 2007: 29).

In the decision, which supported a summary judgment in favor of SEPTA, the court made it clear that its decision rested on the empirical evidence surrounding point (7). SEPTA had provided expert testimony to this point based on the well-known Bureau of Justice Statistics recidivism study with a 3-year follow-up (U.S. Department of Justice, 2002). Mr. El's defense did not have an expert refute or challenge this assertion. The court, in an unusually pointed reply, stated the following:

> Thus, on this record, we have little choice but to conclude that a reasonable juror would necessarily find that SEPTA's policy is consistent with business necessity. This is not to say that we are convinced that SEPTA's expert reports are ironclad in the abstract. But El chose

> neither to hire an expert to rebut SEPTA's experts on the issue of business necessity nor even to depose SEPTA's experts. These choices are fatal to his claim. . .
>
> Had El produced evidence rebutting SEPTA's experts, this would be a different case. Had he, for example, hired an expert who testified that there is time at which a former criminal is no longer any more likely to recidivate than the average person, then there would be a factual question for the jury to resolve. (*El v. SEPTA*, 2007: 34)

This decision has sent a clear message that the next case in this line of cases on the discriminatory impact of rules against hiring individuals with criminal history records could rest on empirical work looking at the relative risk of those with criminal history records (offenders) versus those without records (nonoffenders).

RECENT EVENT 2: A BRITISH COURT CASE 2008

In an interesting analog to the *El v. SEPTA* (2007) case in the United States, an appeals case was brought by constables to a court in Great Britain. The constables were ordered by a government commissioner to delete old criminal history records that the commissioner believed were no longer relevant. The constables appealed on the grounds that the information was still relevant. As in the *El v. SEPTA* case, the tribunal states that the key question is the following:

> Does there come a point where the differences between the offender and the non-offender groups are of no practical significance? That is to say, does there come a point where those differences are so small that for practical purposes the offender and non-offender groups should be treated as equivalent in terms of their risk of future offending behaviour? (Angel, 2008: 31)

Contrary to the U.S. case, the British court decided that sufficient information was available already to find that a point does exist at which offenders become similar to nonoffenders in terms of offending risk. Based largely on empirical research on long-term conviction data from Britain done by Soothill and Francis (2009), the court found that it takes approximately 10–15 years for offenders to be "redeemed." Therefore the tribunal denied the appeal and upheld the decision that old criminal history record information should be discarded.

EARLIER RESEARCH

In this article, we build directly on four studies that tried to identify the "time to redemption" for young, first-time offenders (Blumstein and Nakamura, 2009; Kurlychek, Brame, and Bushway, 2006, 2007; Soothill and Francis, 2009). The work by Kurlychek, Brame, and Bushway (2006, 2007)—instigated by inquiries from advocates after *El v. SEPTA* (2007)—took a first step to answering this question using cohort studies in the United States. Looking only at youthful, first-time offenders, Kurlychek, Brame, and Bushway used two different cohort data sets from Philadelphia, Pennsylvania, and Racine, Wisconsin, to show that the hazard rates for offenders decline rapidly and intersect the nonoffender hazard rates approximately 7 years after arrest or contact with the police. Their work was the first to show that individuals with a criminal history record eventually do look like nonoffenders. Although the time span of 7 years is longer than the 3-year period studied by Langan and Levin (U.S. Department of Justice, 2002), it is much shorter than the 40 years suggested by the employer in the *El v. SEPTA* case.

Although groundbreaking, the Kurlychek, Brame, and Bushway (2006, 2007) studies were limited by the use of older birth cohorts that contain relatively few offenders. Moreover, Kurlychek, Brame, and Bushway relied on arrests and contact with police, measures that are unlikely to be used by employers. Blumstein and Nakamura (2009) have advanced this line of research substantially using a larger, more recent sample from the New York criminal history repository. This article continued to use arrest rather than conviction, but it had the advantage of 27 years of follow-up using data from the official repository, which is used by some employers in New York State. One disadvantage of using the repository record is the lack of a nonoffender group. However, Blumstein and Nakamura showed that young, first-time arrestees who manage to avoid rearrest in New York eventually do look like the general population of the state of New York as well as people with no arrest record in the state in terms of their probability of rearrest. The actual number of years before this happens depends marginally on the type of crime and the nature of the cutoff.

One other recent study by Soothill and Francis (2009) looked at this question using data from Britain and Wales. They relied on official data, as in the case of Blumstein and Nakamura, but they used conviction as opposed to arrest data. They also have data for the entire country, as opposed to one city (Kurlychek, Brame, and Bushway, 2006, 2007) or one state (Blumstein and Nakamura, 2009). Like each of the earlier studies, they focused on youthful offenders (under age 20). As in the previous cases, they found that the hazards of offending decline for the groups with prior records and eventually converge within 10–15 years with the hazard of those of a

comparison nonoffending group. Although some groups, strictly speaking, never converge with the hazard of the nonoffending group, they argued that the hazards are essentially nondistinguishable by the 10–15-year mark.

So, at this point, four studies have been published that address this question (Blumstein and Nakamura, 2009; Kurlychek, Brame, and Bushway, 2006, 2007; Soothill and Francis, 2009) with at least 7 years of data. All articles reached the following similar conclusion: Offenders do eventually look like nonoffenders, usually after a spell of between 7 and 10 years of nonoffending.

However, although these articles represent an important first step, the existing research has several serious shortcomings. First, these studies all focused on a narrow subset of individuals with no criminal history: youthful, first-time offenders. Yet we know that age and number of prior offenses are the two biggest predictors of recidivism in the short run (Gendreau, Little, and Goggin, 1996). We need to understand how age and prior offending affect risk for offending after longer periods of nonoffending. Second, the previous articles relied on nonparametric empirical hazards created simply by counting the number of people who fail in a period and then by dividing by the number of people eligible to offend in that period. This approach is unbiased by assumptions, but it is inefficient. It is worth exploring whether more efficient statistical hazard models can be applied profitably to this question. Third, three articles used arrest or police contact data rather than the conviction information most likely to be used by employers and policy makers. Finally, the studies that relied on official repository data lack a comparison sample of true nonoffenders with which to compare the offending of those with a criminal history.

THIS ARTICLE

Our data set—the official repository records of a snapshot of individuals registered in the courts in a given year in the Netherlands—continues the movement away from birth cohorts used in initial work on this topic by Kurlychek, Brame, and Bushway (2006, 2007). Blumstein and Nakamura (2009) as well as Soothill and Francis (2009) were the first to rely on official repositories of criminal history records, but this approach led to problems creating a relevant comparison group by which to evaluate redemption. We introduce the idea of using a matched control group of nonoffenders that can be used to create a relevant comparison group. We match on age and sex, but we recognize that this is not the only possible control group. We will discuss other possible comparison groups in the conclusion.

Like our predecessors, we find that youthful, first-time offenders "look like" their nonoffending counterparts after approximately 10 years and that older first-time offenders take even less time to converge with their

nonoffender counterparts. However, we also find that the risk of offending differs across persons with different ages and with a different number of prior convictions. Most notably, we find that those with multiple prior offenses never have the same level of risk of offending as those with no records. This finding raises the possibility that some people might never be redeemed, if redemption means that they have the same level of risk as those people of the same age with no criminal history record.

DATA

The analyses in this article are based on data from the Criminal Career and Life-course Study (CCLS) data set. The CCLS is a representative sample, selected from a population of all persons who had a criminal case adjudicated in the Netherlands in 1977. The CCLS data set follows people from age 12 to age 87 or death, with a prospective follow-up period of 25 years and retrospective data to age 12. The data set includes a 4 percent random sample of criminal cases that in 1977 either were ruled on by a judge or were decided on by the public prosecutor.[3] For each sampled case, data were obtained based on the way the 1977 case was dealt with, the type of crime in the 1977 case, as well as the gender and nationality of the suspect.[4]

CRIMINAL RECORD INFORMATION AND RECIDIVISM

The criminal conviction careers of the sampled people were reconstructed using information from the General Documentation Files (GDFs) of the Criminal Record Office ("rap sheets"). The GDFs contain information on every criminal case registered by the police at the Public Prosecutor's Office and on all adjudications that led to any type of outcome (not guilty, guilty, sentence, prosecutorial decision to drop because of a

3. In the Dutch criminal justice system, the public prosecutor has the discretionary power not to prosecute every case forwarded by the police. The public prosecutor might decide to drop the case if prosecution probably would not lead to conviction because of a lack of evidence or for technical considerations (procedural or technical waiver).The public prosecutor also is authorized to waive prosecution "for reasons of public interest" (waiver for policy considerations).The Board of Prosecutors-General has issued national prosecution guidelines under which a public prosecutor might decide to waive a case for policy reasons; if, for example, measures other than penal sanctions are preferable or more effective, then prosecution would be disproportionately unjust or ineffective in relation to the nature of the offense or the offender or prosecution would be contrary to the interest of the state or the victim (Tak, 2003).
4. For more information on the methods of the CCLS, see Blokland, Nagin, and Nieuwbeerta (2005), Blokland and Nieuwbeerta (2005), and Nieuwbeerta and Blokland (2003).

lack of evidence, prosecutorial decision to drop for policy reasons, and prosecutorial fines). For prior criminal history, we use only those registrations that pertain to a criminal law violation and ended with a guilty finding, a prosecutorial waiver for technical reasons, a prosecutorial waiver for policy reasons, or a fine. We call these four outcomes "conviction" and exclude acquittals, registrations for misdemeanors, and all registrations for noncriminal law violations like traffic offenses.[5] Waivers can be used to make a determination of nonsuitability for employment in the Netherlands.

We use the same broad measure for our measure of recidivism, including not only registrations that ended in a guilty verdict but also counting both types of prosecutorial waivers. In addition to the fact that waivers are used to make judgments about suitability for employment, we preferred a denser measure of offending, which helps to create a more detailed hazard function.[6]

Our use of official data for our recidivism measure introduces an interesting case of measurement bias. Our measure of offending does not capture all criminal activity. By itself, this is not a large problem, provided that the missingness is random with respect to the independent variable. However, if convicted offenders are watched more closely by the police than nonoffenders, then it is possible that criminal activity generates arrests and registrations differently for those with a prior registration than for those without one. As a result, we might estimate a longer time to redemption than we would if we had a more complete measure of criminal risk for both offenders and nonoffenders.

5. Researchers sometimes control for time free in their measure of criminal history. Most studies of recidivism, however, simply count the number of prior convictions or arrests. Employers in the Netherlands (and the United States, for that matter) do not have access to the incarceration information. To be thorough, we checked the incarceration history for our sample. In 1977, 68.5 percent of offenders convicted had been imprisoned prior to 1977. For the 31.5 percent imprisoned at least once prior to 1977, the total time spent in prison ranged from .10 years to 13.27 years, with a mean of .90 (1.37) and a median value of .44 years. In other words, 54.9 percent of all offenders imprisoned prior to 1977 served a total prison term of less than 6 months; 73.6 percent of all offenders imprisoned prior to 1977 served a total prison term of less than 12 months. Therefore, we did not modify our measure of prior convictions to control for exposure time on both conceptual grounds—employers cannot control for incarceration—and practical grounds—incarceration is not a big part of the story in the Netherlands.
6. In the Netherlands, approximately 90 percent of the cases initially registered by the police ultimately end up registered in the Public Prosecutor's office (Blom et al., 2005).

OFFENDERS AND NONOFFENDERS

For our analysis of *offenders*, we use only those 3,603 people in the CCLS sample who were convicted—as defined in this study—as a result of the criminal case registered in 1977. We begin our observation about recidivism with the date of conviction. When data collection stopped on January 1, 2003, most surviving individuals were older than 50 years. The age range at the end of data collection was between age 37 and age 87.

To construct a comparison group of *nonoffenders* with which to generate a comparison sample of nonoffenders, we took the unique step of gathering a nonoffender sample from the draft records of the Dutch military. Prior to 1996, all males born in the Netherlands were drafted for military service at age 18. Draft records are kept by the Dutch Ministry of Defence and are archived based on date of birth. We randomly selected 799 men (~20 percent) from the CCLS data set and then ordered these data by date of birth. We then looked up each of the randomly selected CCLS men in the military archive and extracted the personal information of the person immediately after the CCLS men in the files of the military archive sorted by date of birth. If the CCLS man was Person #150,668 in the archives by date of birth, then we selected Person #150,669 to be in our control sample. Therefore, the 799 men taken from these archives are a random sample of the Dutch male population matched by age to the CCLS men.

Of these control men, 49 have at least one offense on their criminal history record up to, and including, 1977. As to be expected in a random sample of the population, 12 of these individuals were convicted in 1977. This amount is an annual conviction rate of 1.50 percent, which is close to the estimate of the 1.75 percent probability of conviction that we generated using aggregate statistics.[7] Because we are interested in a sample of nonoffenders, we do not consider anyone who was convicted prior to 1978 in our analysis. Our current analyses are restricted to the 3,243 males in the CCLS sample who were convicted in 1977 and the 750 controls with no criminal history record in 1977.

We look at time since conviction or, for the few people who were incarcerated after the incident offense, time since release from prison. In the text, we use the term "time since conviction" for the sake of clarity. We use age at conviction or age at release from prison for the incident conviction for our conviction sample, although we refer to this number as the "age

7. The total number of individuals convicted (guilty verdicts and policy waivers) in 1977 (according to van der Werff [1989: 25, table 1b]) was 101,980. Based on the CCLS study, we know that approximately 7 percent of registered individuals in 1977 were female and, according to Statistics Netherlands, the number of men aged 12–79 years in 1977 was 5,417,626 [(101,980 × .93) / 5,417,626 = 1.75%].

at conviction" for clarity. We use the individual's age in 1977 as the age variable for our nonoffender sample.

METHODS

EMPIRICAL HAZARDS

We are interested in creating predicted hazard curves for different subgroups of offenders by age at conviction and history. Following the form initiated by Kurlychek, Brame, and Bushway (2006, 2007), we compare these curves (with confidence intervals) with that of a group of nonoffenders to determine whether and when they will cross. Up to this point, researchers have produced empirical hazards generated simply by counting the number of failures in a given year by the number of people who have lasted that long. For example, the hazard for the second year post offense is created by dividing the number of people who failed in year 2 by the number of people who still had not failed by the start of the second year. The general formula is given in the following equation:

$$\begin{aligned} h(t) &= \Pr(T = t \mid T \geq t) \\ &= \frac{\text{\# of the sample who have a new arrest in time period } t}{\text{\# of the sample who have not had a new arrest before } t} \end{aligned} \tag{1}$$

A different empirical hazard is created for each different subgroup of interest until the data get too thin to create a separate hazard. This approach is intuitive but inefficient. The standard errors will be large, particularly for those cells in which relatively few people are present.[8]

PROPORTIONAL HAZARD MODELS

In this article, we use a Cox proportional hazard model, which will allow us to explore the impact of criminal history more efficiently as well as age at the time of conviction on the hazard rate. The standard errors for the coefficients as well as the predictions will be smaller than the standard errors using the empirical hazards. The Cox proportional hazard models represent an important subclass of hazard models; they allow the estimation of effect coefficients without a formal consideration of the hazard function (Cox, 1972; Schmidt and Witte, 1988). The advantage is that no assumptions about the shape of the hazard are needed. The potential disadvantage of this class of model is that researchers must assume that the effects are

8. An excellent discussion about the estimation of standard errors for empirical hazards is found in Blumstein and Nakamura (2009).

proportional reflections of the baseline hazard. For example, the estimate of the difference between the hazard rates for a male versus a female is based on the assumption that the hazard function for men is a multiple of the hazard for women. If this assumption is valid, then the researcher can avoid the problems associated with identifying the underlying hazard function. This assumption can be assessed both graphically and statistically (Garrett, 1998).

The CCLS data have only the year in which the next registration occurs, so we use a proportional hazard model equipped for discrete time periods.[9] This version of a proportional hazard can be estimated simply using a logit model for failure with time dummy variables for each period, as shown in equation 2. The time dummy variables capture the baseline hazard, and variables can be added to the model as in a standard hazard model. Equation 2 is expressed as follows:

$$\Pr(\text{Failure} = 1 \mid T, X) = \ln\left(\frac{p_{it}}{1 - p_{it}}\right) = \beta_0 + \beta_1 T_t + \beta_2 X_i \qquad (2)$$

where T is a vector of time dummy variables counting the years since conviction and X is a vector of time-stable variables such as age at conviction or the number of prior offenses. Predicted curves then can be estimated using the coefficients from this simple logit model.

Modeling Strategy: Separate Models for Nonoffenders and Offenders

As mentioned previously, one strength of our article involves our creation of a nonoffender sample, which will allow us to compare the hazard of offenders with that of nonoffenders of the same age and sex. Yet a comparison of the nonoffender hazards with that of the offenders in the time since the conviction in prior work (e.g., Kurlychek, Brame, and Bushway, 2006, 2007) shows clearly that the two hazards are not parallel.[10] Therefore,

9. This type of model implies that many people fail at the same time, which can be a problem because we need to know how many people were "at risk" when the person failed. The simplest method is to assume all were at risk for each failure. This assumption can be problematic if many ties are found but generally is thought to be reasonable.

10. See, for example, figure 4 from Kurlychek, Brame, and Bushway (2006), which compares the hazard for those who were arrested at age 18 with the hazard for those not arrested at age 18. The hazard for the offenders was high right after the arrest followed by a decline. We have no reason to expect that the hazard of the nonoffenders will spike in the period in which the matched offender was arrested. Rather, the hazard for this group of nonoffenders is a relatively flat line that declines slowly with age, which means that the two hazards are not parallel.

if we are to use the proportional hazard model on our sample of individuals with convictions, then we first will need to estimate a model separately for the nonoffenders. This process will be less efficient but still more so than with an empirical hazard model.

Modeling Strategy: Separate Models for People Younger and Older Than Age 32

To validate the proportional hazards assumption of age at conviction and prior convictions in the offender sample, we estimated a model in which age at conviction and prior record were included as dummy variables for the sample of males who have a conviction in 1977. We created eight age groups based on age at conviction (12–16 years, 17–21 years, 22–26 years, 27–31 years, 32–36 years, 37–41 years, 42–46 years, and 47 years and older) and five groups based on the number of prior convictions (no prior convictions, 1 prior conviction, 2 or 3, 4–7, and >7 prior convictions). The numbers in each group are provided in table 1. The global test for the proportionality assumption is soundly rejected, and the individual tests make it clear that the problem lies with the age variables (results not shown). Although the younger offenders start out with higher recidivism rates than older offenders immediately after conviction, no meaningful difference is noted in offender risk by age in 1977 after 10 years of nonoffending. In other words, the hazards are not parallel throughout the entire time period. To account for this nonproportionality, we conduct all subsequent analyses on the following samples: those who were younger than 32 years at conviction in 1977 and those who were older than 32 years at conviction. Regressions conducted on these samples pass the proportionality test.[11]

WHEN DO OFFENDERS BECOME EQUAL TO NONOFFENDERS?

Our main aim is to establish how many years of nonoffending it takes before an offender begins to equal nonoffenders in terms of their offending rate. To do so, we have to decide when the hazards will be "equal"; in other words, we need to set the standard for redemption. If we had population data, then setting this standard would be straightforward. Offending rates then can be regarded as equal when the hazard of the offender group becomes exactly the same as that of the nonoffender group or, in other words, when the actual hazard curves of these groups cross. In this framework, we also could decide, á la Blumstein and Nakamura (2009), to allow a small difference δ, which would indicate a certain level of risk tolerance.

11. We did these test using Schoenfeld residuals (Schoenfeld, 1982). The results of the tests can be requested from the first author.

Table 1. Number of People and Percentages by Sample, Age, and Number of Prior Convictions

	Offenders Age 12–31 $n = 2{,}534$	Offenders Age 32+ $n = 709$	All Offenders $N = 3{,}243$	Nonoffenders $N = 750$
Age at time of 1977 conviction[a]				
12–16	370 (14.60%)	—	370 (11.41%)	64 (8.53%)
17–21	1,019 (40.21%)	—	1,019 (31.42%)	229 (30.53%)
22–26	701 (27.66%)	—	701 (21.62%)	167 (22.27%)
27–31	444 (17.52%)	—	444 (13.69%)	108 (14.40%)
32–36	—	255 (35.97%)	255 (7.86%)	68 (9.07%)
37–41	—	164 (23.13%)	164 (5.06%)	56 (7.47%)
42–46	—	121 (17.07%)	121 (3.73%)	36 (4.80%)
>47	—	169 (23.84%)	169 (5.21%)	22 (2.93%)
Prior convictions				
None	1,077 (42.50%)	204 (28.77%)	1,281 (39.50%)	750 (100.00%)
1	383 (15.11%)	112 (15.80%)	495 (15.26%)	—
2–3	418 (16.50%)	122 (17.21%)	540 (16.65%)	—
4–7	363 (14.33%)	104 (14.67%)	467 (14.40%)	—
>8	293 (11.56%)	167 (23.55%)	460 (14.18%)	—

[a]In this sample, 306 people served some time after conviction. For these people, we only started following them after their release from prison, so we used their age at release. Of this subsample, 75 percent served less than 1 year.

However, it is less straightforward to decide when the hazard rates of offenders and nonoffenders are equal when using samples. With samples, researchers need to take into account the fact that the point estimates are estimated with error. The standard errors are used to generate confidence intervals. Because the models are nonlinear, the actual width of the confidence interval will depend on the "value" of the prediction. For example, the standard errors (in percentage point terms) will become smaller as the prediction gets closer to zero, even when the sample size remains the same. Furthermore, the standard errors become larger when the groups of nonoffenders become smaller. Consequently, with small sample sizes, even large differences in point estimates will not be statistically significant.

In this article, we try to account for these issues by examining the following curves: the upper bound, the point estimate, and the lower bound

of the offender hazard. We compare all three with the upper bound of the hazard for the nonoffenders. Our strategy results in three different measures of convergence. The first method argues that the two groups are "equal" the year in which the lower bound of the offender group confidence interval crosses the upper bound of the nonoffender confidence interval. The point estimates still might be substantially different when this condition is met. The second method is more conservative, and it marks redemption as the year for which the point estimate of the offender group hazard first is equal to or less than the upper bound of the confidence interval for the nonoffender group. The final method, adapted from Blumstein and Nakamura (2009) defines convergence when the upper bound of the confidence interval is less than or equal to the upper bound of the nonoffender hazard. This method is extremely conservative; it could result in cases in which the point estimate for the offender hazard is actually less than the point estimate of the nonoffender hazard before equivalence is declared.

We chose to focus on the upper bound of the nonoffender group rather than on the point estimate for the nonoffenders to account for the uncertainty surrounding the nonoffender estimate. Other options are clearly possible, and we will return to the discussion of the appropriate baseline rate for identifying "redemption" in the Conclusion section.

RESULTS

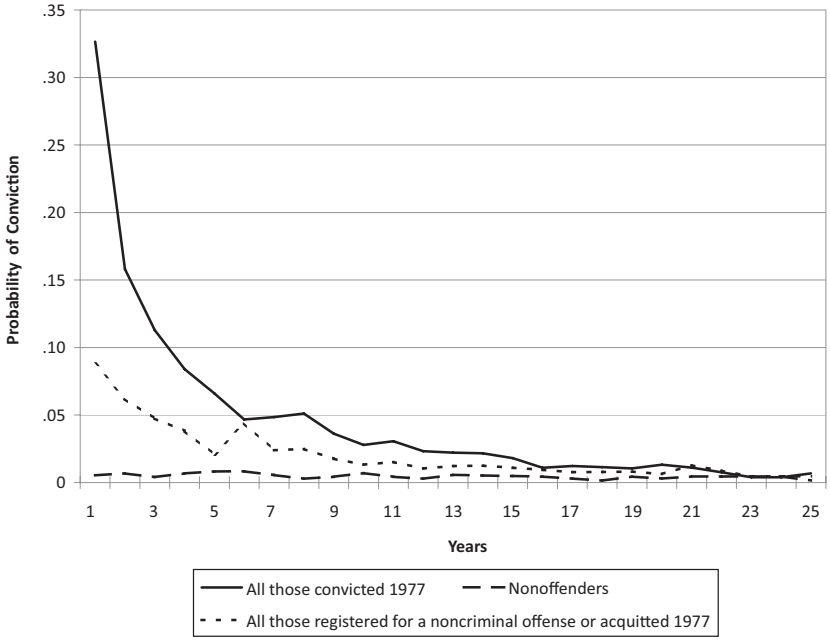
RECIDIVISM OF NONOFFENDERS

We start with a model without covariates for our nonoffender sample, consisting of the matched sample of men without any convictions prior to and including 1977.

The model thus consists only of time dummy variables. Unlike previous articles, we do not find that the nonoffender hazard declines over time. All coefficients on time and age at 1977 are insignificant, and the model itself is insignificant. The only coefficient that is significantly different from zero is the constant, which suggests that the nonoffenders are at a stable but low probability of conviction (i.e., the point estimate is below .01 at all times).

Figure 1 provides the graphical description of the nonoffender curve. The flatness of the curve for the nonoffenders is surprising when compared with the curve found in other studies (Blumstein and Nakamura, 2009; Kurlychek, Brame, and Bushway, 2006, 2007; Soothill and Francis, 2009). Conviction simply might not be dense enough to evaluate differences in criminal propensity among a sample of people chosen for their lack of a conviction by a certain year.

Figure 1. Empirical Hazards for Sample



RECIDIVISM OF TOTAL REGISTERED AND TOTAL CONVICTED GROUP

Figure 1 also provides two other curves for the sake of comparison; the first is the predicted curve for the sample of people convicted in 1977 (as defined previously), and the other is for those in the 1977 sample registered for a noncriminal offense or acquitted of a criminal offense. These again are the baseline hazards with no other covariates included using the logit model described in equation 2 (estimated coefficients of these models are not shown). The extra curves highlight the lessons learned from other studies. First, the curves show that, despite massive early differences in risk immediately after an event, both groups experience fairly dramatic declines in offending risk if they remain free. Second, both curves eventually cross that of the nonoffender hazard after 22 years and are close within 15 years.

The two curves for the convicted and the registered sample, however, cannot be used to address our main research question about the time until redemption. Unlike previous studies, our data on the offenders and nonoffenders do not represent groups of people with similar age and criminal history. Our sample of convicted people, for example, ranges in age from 12 years to 70 years and in number of prior convictions from 1 to 57 (see

also table 1). The variation in age and criminal history in our sample is relevant because it is both possible and likely that the recidivism rate of a first-time offender is lower than that of someone with five prior convictions. The same possibility is also true for age in 1977; older persons are likely to have lower recidivism rates than younger people. Therefore, the goal of this article is to regenerate figure 1 for different subsets of people, relying on the Cox proportional hazard model that take the effects of age at conviction and criminal history on recidivism patterns into account. Only then can we provide an estimate for the number of years of nonoffending it takes before an offender begins to resemble a nonoffender in terms of his/her probability of offending.

PROPORTIONAL HAZARD MODELS WITH COVARIATES

To take into account adequately the effects of differences in age at conviction and criminal history on recidivism patterns, we estimate Cox proportional hazard models that include covariates for both the age at conviction and the number of prior convictions. We estimate the same model for the following samples: the matched sample of men without any convictions prior to and including 1977, convicted men from CCLS younger than 32 years at time of conviction, and convicted men from CCLS older than 32 years at time of conviction. Dummy variables for age at conviction and prior convictions are included in each model. The coefficients of the three models are provided in table 2.

Two things are obvious from the results. First, the pseudo R^2 of the model for the nonoffenders is nearly zero, indicating that age in 1977 has almost no impact on the future conviction patterns of this group of people. Second, the two models for offenders have a good fit as measured by the pseudo R^2. Age at conviction and the criminal history variables have a substantial and significant impact on recidivism. Third, the model for the older sample of offenders has a lower constant than the model for the younger sample, meaning that an overall lower level of offending exists for the older offenders. This result is expected, given both the theory and the empirical hazards described earlier. Fourth, prior offending has a substantial effect on recidivism, and the effects are similar across the two offender samples.[12]

12. The only coefficient that was different across models was the variable for more than seven prior convictions. This result can be explained at least partially by the nature of the variable, which represents the catchall category for everyone who has more than seven convictions. The actual average number of prior convictions for the older group in this category is 17.5 convictions, whereas for the younger offenders, the average is 13.7, which is a statistically significant difference of 27.9 percent.

Table 2. Proportional Hazard Model for Time to Reconviction

	Nonoffenders	Offenders Age 12–32	Offenders Age 32 and up
Constant		−.936	−2.480
		(.078)	(.194)
Age 12–16 in 1977	Omitted	Omitted	—
Age 17–21 in 1977	.002	−.184	—
	(.429)	(.083)	
Age 22–26 in 1977	−.133	−.478	—
	(.455)	(.092)	
Age 27–31 in 1977	−.275	−.747	—
	(.505)	(.102)	
Age 32–36 in 1977	−.920	—	.135
	(.691)		(.144)
Age 37–41 in 1977	−.031	—	Omitted
	(.558)		
Age 42–46 in 1977	−.285	—	−.403
	(.692)		(.181)
47 and up in 1977	−.041	—	−.737
	(.805)		(.178)
No prior convictions	—	Omitted	Omitted
1 prior conviction	—	.474	.449
		(.081)	(.203)
2 or 3 prior convictions	—	.961	.673
		(.078)	(.193)
4–7 prior convictions	—	1.342	1.345
		(.086)	(.185)
>7 prior convictions	—	1.979	2.647
		(.104)	(.175)
1977	Omitted	Omitted	Omitted
1978 2	.230	−.782	−.927
	(.673)	(.079)	(.195)
1979 3	−.277	−1.122	−1.169
	(.766)	(.094)	(.222)
1980 4	.239	−1.540	−.875
	(.673)	(.114)	(.218)
1981 5	.431	−1.750	−1.155
	(.648)	(.129)	(.249)
1982 6	.439	−2.108	−1.423
	(.648)	(.154)	(.289)
1983 7	.039	−1.992	−1.573
	(.709)	(.129)	(.317)
1984 8	−.651	−1.933	−1.425
	(.868)	(.154)	(.308)
1985 9	−.241	−2.332	−1.607
	(.766)	(.187)	(.343)
1986 10	.278	−2.526	−2.071
	(.673)	(.209)	(.430)
1987 11	−.229	−2.652	−1.297
	(.766)	(.227)	(.318)
1988 12	−.630	−2.889	−1.640
	(.868)	(.258)	(.379)

Table 2. Continued

	Nonoffenders	Offenders Age 12–32	Offenders Age 32 and up
1989 13	.070	−2.850	−1.904
	(.709)	(.258)	(.430)
1990 14	—	−2.820	−2.071
		(.258)	(.468)
1991 15	—	−2.998	−2.266
		(.285)	(.519)
1992 16	−.207	−3.468	−2.920
	(.766)	(.359)	(.721)
1993 17	—	−3.338	−2.848
		(.339)	(.721)
1994 18	−1.302	−3.733	−2.066
	(1.119)	(.413)	(.519)
1995 19	−.199	−3.566	−2.725
	(.766)	(.383)	(.721)
1996 20	−.596	−3.292	−2.692
	(.868)	(.340)	(.721)
1997 21	−.181	−3.682	−2.236
	(.766)	(.413)	(.595)
1998 22	−.173	−3.852	−3.298
	(.766)	(.452)	(1.011)
1999 23	—	−4.349	—
		(.581)	
2000 24	−.162	−4.751	−3.230
	(.766)	(.710)	(1.011)
2001 25	−1.251	−4.721	−2.039
	(1.120)	(.710)	(.597)
N	14,827	21,450	8,166
(Chi-squared *p*-value)	(.974)	(.000)	(.000)
Pseudo R^2	.000	.250	.223

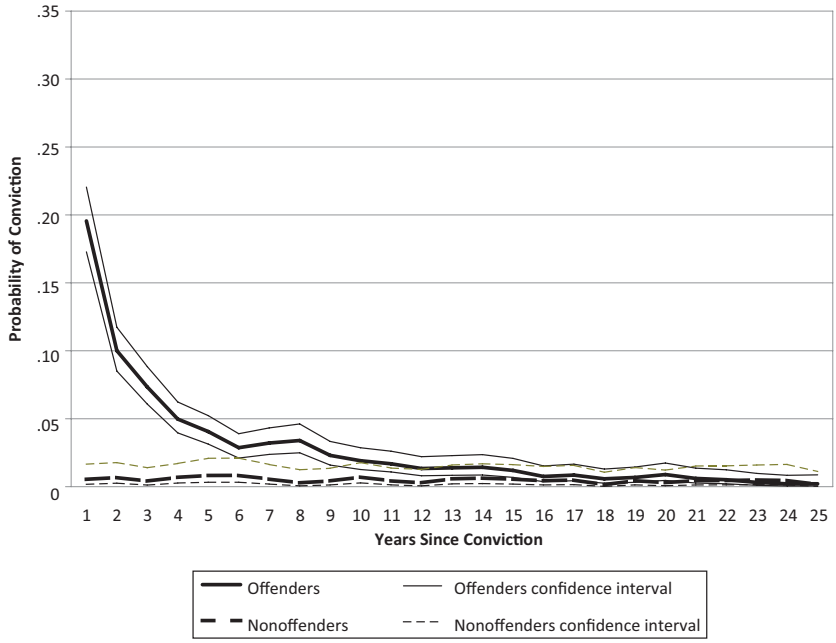
NOTE: Standard errors in parentheses.

Predicted Recidivism Rates of Various Groups

We next use these three models to predict the probability of conviction at each point in time. For nonoffenders, we can generate eight different hazard trajectories because we have eight different age groups. For offenders, we can generate 40 different hazard trajectories because we have eight different age groups and five different history categories. This level of complexity would be impossible to do with empirical hazards in our data because of the relatively small cell sizes.

To make this discussion more concrete, we display some predicted curves in figures 2a–c. Figures 2a–c display the predicted curves with their confidence intervals for 26-, 36-, and 46-year-olds with zero prior convictions together with the predicted curve for nonoffenders of a similar age. The graphs are presented separately because the probability of conviction for

Figure 2a. Predicted Hazard for 26-Year-Old Offenders with No Prior Convictions

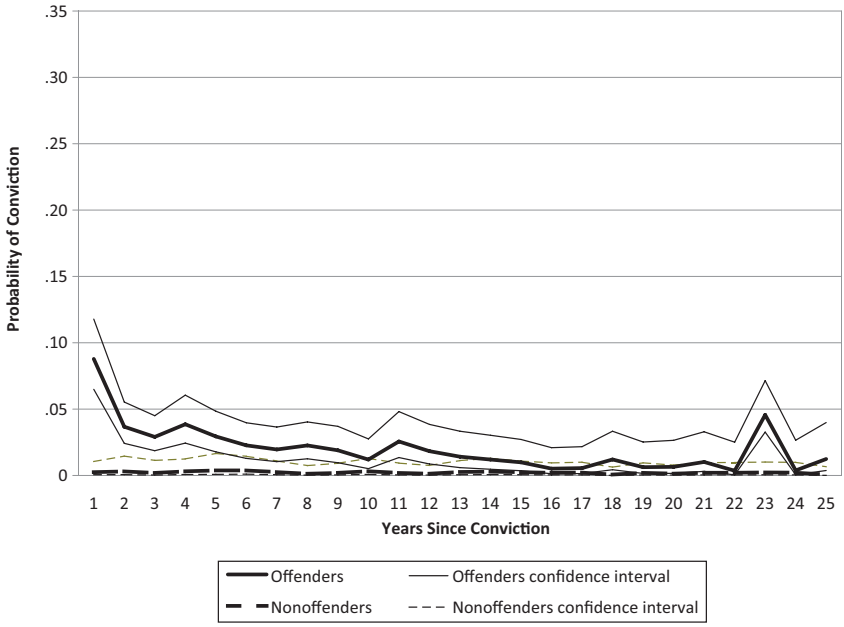


the nonoffenders decreases as the nonoffenders age. The confidence intervals also change because of differences in sample size.

The first finding is that the probability of offending in the first year after conviction is dramatically different across age. The 26-year-olds with no prior convictions have a 19.6 percent chance of conviction in the first year after their first conviction. Thirty-six-year-olds with no prior convictions have an 8.8 percent chance of conviction in their first year after conviction, a 55.0 percent reduction in risk over the 26-year-olds. The risk of offending in that first year is an even lower 5.3 percent for those who were 46-years-old with no prior convictions in the sample, a 39.0 percent reduction in comparison with the risk for those age 36 years at conviction. Recall that the curves for the nonoffenders show no such differences.

Another important finding is that all curves for those with no prior offending eventually converge with the nonoffender curves, although the time that it takes for the convergence to occur differs dramatically by the age at conviction. For the 26- and 36-year-olds, the point estimates converge 16 years after the conviction, whereas for the 46-year-olds, the point estimates converge at 10 years. The fact that the convergence in the

Figure 2b. Predicted Hazard for 36-Year-Old Offenders with No Prior Convictions

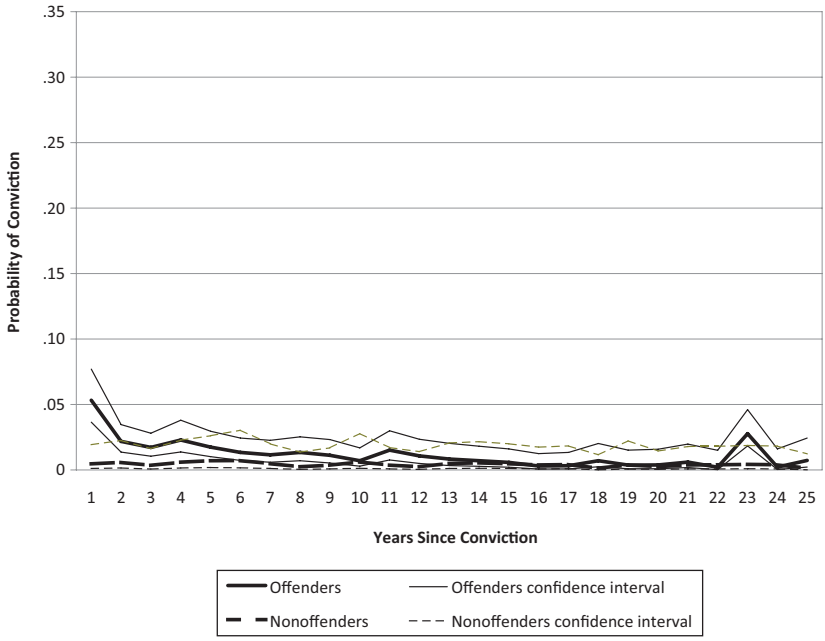


curves takes far less time for the older offenders is a natural consequence of the lower recidivism rates for older offenders.

The story is more complex for prior offending, as presented in figure 3.[13] The first year reconviction rate again varies widely, ranging from 19.6 percent for those with no prior convictions to a whopping 63.7 percent for those with more than seven prior convictions, and rather strikingly, the point estimates of the offender hazard never converges with the nonoffender curve for those with more than seven prior convictions during the 25-year follow-up period. Although not shown, this finding is actually true for all offenders with three or more prior convictions. These estimates clearly indicate that prior offending is an important variable when considering the risk of offending after conviction and rerelease.

13. These results can be presented in one graph because all three groups have the same nonoffender hazard for comparison (because all nonoffenders have no prior convictions, by definition).

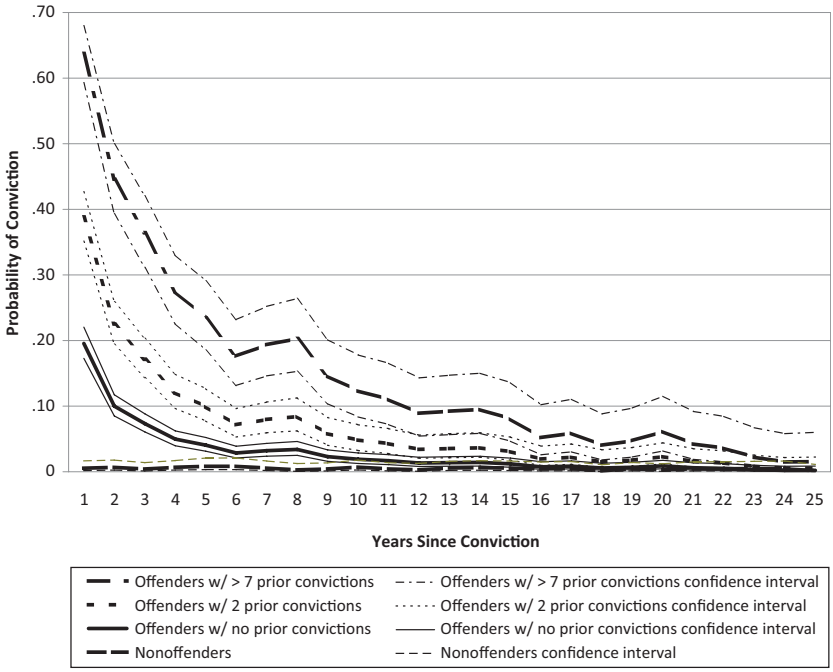
Figure 2c. Predicted Hazard for 46-Year-Old Offenders with No Prior Convictions



When Do Offenders Become Similar to Nonoffenders?

In this section, we will formally use the predicted curves for each subgroup to estimate a time to redemption for all subgroups in our sample, using our three different measures of convergence. Table 3 presents the convergence information in a comprehensive manner, and it takes into account the confidence intervals around the curves of the offenders and nonoffenders. As discussed earlier, we use three different methods to establish convergence. All three methods use the upper bound of the hazard for the nonoffenders as the baseline but use the lower bound (method 1), the point estimate (method 2), and the upper bound (method 3) of the offender hazard curve as comparisons. A scan of the fifth column of table 3 shows that our strictest standard—the upper bound of the offender hazard (method 3)—leads to the conclusion that the curves never cross in most cases. This method is extremely conservative, especially for the cells with fewer people and larger confidence intervals, and we do not think that it can be justified on any standard statistical grounds. Therefore, we refer only to the other two standards in the remainder of the article.

Figure 3. Predicted Hazards of a 26-Year-Old Offender with Different Criminal History Records



Age and Reoffending

Young offenders with no prior convictions have been the almost exclusive focus of our earlier work and represent a sizeable proportion of our sample. We find results that are entirely consistent with this earlier work—the confidence intervals of the hazards converge at 10 years after the conviction for the three youngest groups of offenders (26-years-old) with no prior convictions using method 1 and 13 years after conviction using method 2. Despite the fact that we are using Dutch data with convictions instead of arrest, we find nearly identical times to convergence for the types of people studied by Kurlychek, Brame, and Bushway (2006, 2007) and by Blumstein and Nakamura (2009).

Starting at 27-years-old, however, our results show the time to redemption declining rapidly to 6 and 10 years after conviction using methods 1 and 2, and it reaches 2 years after conviction using either method for those aged 42–46 years. These results show clearly that older offenders with no prior crimes are at a low absolute risk. Furthermore, they become "redeemed"

Table 3. Year After Conviction in Which the Offender Hazard Crosses Upper Bound of the Nonoffender Hazard

Prior Convictions	Age	Method 1 Lower Bound	Method 2 Point Estimate	Method 3 Upper Bound	General Population Point Estimate (1.75%)
0	12–16	10	13	22	16
1	12–16	13	19	Never	18
2 or 3	12–16	16	23	Never	23
4–7	12–16	21	23	Never	24
More than 7	12–16	23	Never	Never	Never
0	17–21	10	13	23	15
1	17–21	13	16	Never	16
2 or 3	17–21	16	23	Never	23
4–7	17–21	19	23	Never	23
More than 7	17–21	23	Never	Never	Never
0	22–26	10	13	21	11
1	22–26	13	16	23	16
2 or 3	22–26	16	22	Never	23
4–7	22–26	16	23	Never	23
More than 7	22–26	22	24	Never	24
0	27–31	6	10	16	10
1	27–31	10	16	23	12
2 or 3	27–31	15	18	Never	16
4–7	27–31	16	23	Never	18
More than 7	27–31	21	24	Never	23
0	32–36	6	10	Never	10
1	32–36	10	16	Never	15
2 or 3	32–36	10	22	Never	16
4–7	32–36	16	Never	Never	22
More than 7	32–36	22	Never	Never	Never
0	37–41	2	5	Never	7
1	37–41	5	10	Never	10
2 or 3	37–41	6	10	Never	15
4–7	37–41	10	16	Never	16
More than 7	37–41	16	Never	Never	Never
0	42–46	2	2	6	3
1	42–46	2	6	10	9
2 or 3	42–46	5	6	Never	10
4–7	42–46	6	10	Never	16
More than 7	42–46	16	Never	Never	Never
0	≥47	1	2	2	2
1	≥47	2	2	5	6
2 or 3	≥47	2	2	6	7
4–7	≥47	5	6	Never	15
More than 7	≥47	10	22	Never	Never

much quicker than younger offenders, regardless of the definition of redemption. This fact can be explained from a criminal career perspective, which focuses on the rates of offenders at different ages. People who offend for the first time at 15 years of age make up a heterogeneous group. For some, this instance will be their only offense, but a sizeable number are

frequent offenders who will go on to have lengthy criminal careers. In contrast, those who have their first conviction at 40-years-old are all similar; they are low-frequency offenders who are almost indistinguishable from the general population (Piehl and Bushway, 2007).

Prior Offending and Reoffending

The analysis of prior offending tells a surprising story for those with four or more offenses (our upper two prior categories). For those younger than 37 years with four or more offenses (23.4 percent of the sample), even the least conservative method (method 1) does not find a point of convergence of less than 16 years. Using perhaps the most reasonable method (method 2), we find that those with four or more offenses either never converge or converge after a minimum of 23 years. Simply put, many convicted offenders with numerous prior convictions take more than 25 years before they look like nonoffenders again. The results for the offenders older than 36 years raise the possibility that eventual convergence occurs (e.g., the oldest group with four to seven prior convictions converges at 5 years using method 1 and at 6 years using method 2).[14]

The story is less stark for those in the middle, with one to three prior offenses. For these individuals, convergence occurs, but the waiting period is longer than what typically has been observed in the literature. For example, for those 17–21-years-old in 1977 with one offense, the convergence takes 13 years using method 1 and 16 years using method 2. The cutpoint for those with between one and three prior offenses does not drop below 10 years using any standard until we look at individuals who are older than 36 years at their conviction.

This case highlights the value of using criminal history. If we used a cutpoint of 10 years based on those with no prior convictions, then policy makers might face greater levels of risk than they might find acceptable. For example, the point estimate of the hazard (probability of conviction) for a 26-year-old with two or three prior convictions 10 years after conviction is at 4.8 percent, whereas the upper bound of the nonoffender curve is at 1.8 percent, a difference of 3 percentage points and more than 150 percent. This difference in risk might be big enough to matter to policy makers (Blumstein and Nakamura, 2009; Soothill and Francis, 2009). Although the exact nature of the comparison group and the level of acceptable risk will

14. Small numbers of offenders at these years and the prospect of early death for those with the most prior convictions (Nieuwbeerta and Piquero, 2008) make us reluctant to reach for this conclusion. Death might be the great equalizer (Ecclesiastes 9:2–3, 12–13), but policy makers no doubt are looking to find when individuals with convictions look like individuals with no convictions during their lifetime.

determine the cutpoints for any given problem, the number of prior convictions clearly matters in a way that has not previously been recognized.

CONCLUSION

With criminal history record information increasingly available, employers, governmental bodies, and other decision makers have been confronted with the difficult task of determining how to make use of this information. Several courts have asked whether offenders ever resemble a nonoffender in terms of risk of offending, and if so, how long this process takes. Answers to these questions also have theoretical value, as they speak to topics central to life-course and criminal career criminology.

Using a Dutch data set comprising individuals convicted in 1977 and a matched control group of previously unconvicted men, we showed that, although the risk of reconviction for offenders initially is high, most offenders eventually resemble nonoffenders in terms of their conviction risk. We find that the actual time it takes to be "redeemed" depends heavily on the age of the individual at the time of the incident conviction as well as on the number of prior convictions. Redemption time was shortest for older offenders and those with less extensive criminal histories. In line with previous studies focusing on young, first-time offenders, we find that it takes approximately 10 years before offenders age 12–26 years with no prior convictions start to resemble their never-convicted counterparts. Older offenders with no prior crimes begin to look like nonoffenders after 2–6 years. However, offenders with four or more offenses either never resemble nonoffenders or only begin to do so after a minimum of 23 years. Their history of frequent offending continues to resonate in their life course even after they have been crime free for more than 20 years.

Given the results of the current as well as previous studies, the 40-year period put forward in the *El v. SEPTA* (2007) case discussed earlier generally seems too old of a score to be still in need of settlement. However, we would counsel against using "one-size-fits-all" expungement or sealing rules based on our results. Rules that expunge or seal records after a fixed period of time for all offenders without consideration of age or criminal history will unnecessarily disadvantage older offenders and those with few prior convictions and at the same time will fail to protect against offenders with extensive criminal histories who still might have increased levels of risk.

We believe that our results also explicitly tie the recidivism discussion into the larger life-course literature from which it is sometimes disconnected. For example, the empirical question of parallel paths raised by the use of the Cox proportional hazard model echoes the theoretical debate about the distribution of the overall age–crime distribution. Gottfredson

and Hirschi (1990) argued that individual trajectories of offending are largely parallel in the population and differ only in the level of offending risk over time. Although some researchers have disputed this conclusion (Bushway, Sweeten, and Nieuwbeerta, 2009; Thornberry et al., 2009), the consensus from the application of the semiparametric trajectory method to official records of offending is that parallel trajectories of offending (Piquero, 2008) are present. The idea of parallel declining hazards also corresponds well with the notion of a gradual process of desistance in which people follow a "glide path" toward a zero level of offending (Bushway et al., 2001; Laub and Sampson, 2001).

At the same time, the growing literature on long-term hazards raises questions about the value of true zero as a standard for desistance or redemption. This literature on redemption demonstrates that most people, even individuals with no criminal history, have at least a slight potential for offending. How would the study of desistance change if desistance was defined as the point when an individual's level of risk had declined to that of the general population of nonoffenders?

Of course, if we start to move away from zero as the comparison point, then we need to work hard to define the relevant comparison group. Simply put, which nonoffenders do the offenders need to "look like" before they are "redeemed?" The standard in this literature has been the use of nonoffenders of the same age as the offenders. This comparison group is difficult to estimate when using official repositories. Indeed, one of our contributions to the literature in this article is the collection of a sample of nonoffenders in the Netherlands. However, having accomplished this difficult task, it is still not clear to us that we have necessarily identified the right comparison group.

For example, employers arguably should be comparing the risk of the potential applicant with that of other applicants. Clearly, the applicant pool need not be of the same age as the current applicant. This point is particularly relevant for older offenders, who might be competing for relatively low-skill jobs with pools of younger applicants who will have greater levels of risk on average. In addition, age is not the only relevant factor. Offenders undoubtedly have lesser levels of education than the general population of nonoffenders, and therefore, they will not be competing with the average "nonoffender" for a job. Intuitively, we believe that most attempts to create a more similar comparison group will have the effect of increasing the risk of the comparison group and of shortening the time to redemption.

The relevant pool also will depend significantly on the type of background check. We use a formal criminal history from the official government-run repository because that is what employers do in the Netherlands. In the U.S. context, however, most employers do not use

repository data. Instead, they often buy court record data from private venders. These types of searches generate many false negatives (Bushway et al., 2007). In this situation, the "nonoffender" pool, from the employer perspective, will contain numerous offenders who simply were not detected by the search (but would be detected in the type of research study that relies on official records). The net result is that the actual comparison sample for the employer will have a greater level of risk than what the researcher would estimate using repository data with accurate matches, whereas the risk level of offenders will remain unchanged. Therefore, in practical terms, offenders will be comparable with nonoffenders earlier when employers use lower quality data.

The other complication in our strategy of using a sample of nonoffenders comes from the need to rely on a sample. Samples have error, which in some cases generated fairly wide confidence intervals, particularly for the older nonoffenders. We took this error into account in our convergence estimates by using the upper bound of the nonoffender hazard as our convergence standard. This decision clearly affected our answer. For example, our estimates for the time to redemption would have been longer if we had used the point estimate from the nonoffender sample rather than the upper bound, regardless of which method we used to accommodate error in the offender sample.

An alternative standard that would abstract from both the age problem and the sampling problem would be to use the probability of an arrest (or whatever measure of risk seems relevant) for the general population. A population estimate eliminates the need for a sample, and the general population is an intuitively appealing and understandable concept. In effect, such a standard would change the question to as follows: "How many years of nonoffending does it take before an offender begins to resemble a member of the general population in terms of his/her probability of offending?"

In the last column of table 3, we estimated the time to redemption using as the standard the annual probability of conviction for a male in the general population of the Netherlands, estimated as 1.75 percent. We simply estimated the point at which the point estimate of the offender sample crossed this constant threshold. A comparison of this column with method 2 shows that this new standard provides a similar set of estimates. In other words, the answer did not change much when we changed the question. Although this outcome might be unique to the Netherlands, it is interesting to note that we get largely the same answer when we use a much simpler comparison group. Future research in this area should pay explicit attention to the definition of appropriate comparison groups. Policy makers, like the courts cited in at the beginning of this article, also should be aware of the need to define this group explicitly when considering this issue.

Future research also should explore our finding concerning the continued impact of criminal history years after the last offense. This finding is truly puzzling. Why should high-rate offenders still be high risk 15 and even 20 years after their last offense? These long periods of nonoffending should be informative about changes in the underlying rate. Perhaps even long dormant criminal history records can increase the risk for a new bout of criminal activity. Ultimately, the information needed to resolve this puzzle is not in our data set. We need richer data on the life course to see whether proximate events, which might be correlated with prior offending, can explain the sudden reemergence of offending after years of dormancy.

Our analyses also could be augmented to include the type of initial offense or the nature of a person's criminal history. The latter might be especially relevant to evaluate special policies aimed at violent or sexual offenders. Dutch registration laws were amended recently for sexual offenders to allow life-long registration to prevent them from finding employment involving children and other vulnerable groups. In a related manner, one could argue that these policies are designed to prevent the reoccurrence of specific types of crime instead of just any crime. However, the hazard functions in this study, like other studies in this field, were based on general recidivism rather than on specific recidivism. A movement to specific types of offenses will put even more pressure on the data, particularly for the comparison samples. Convictions for serious offenses are relatively rare, so large samples of people will be needed to provide accurate estimates.

One final caveat for our work, and all work in this area, arises because our risk assessment was conducted during a period when background checks were being conducted.[15] In a perfect world (for science), risk assessment is conducted in the absence of policy interventions that are based on the characteristics used in the risk assessment (Bushway and Smith, 2007). In this perfect world, we get the true effect of the characteristic on the risk,

15. In the Netherlands, employers only can check criminal history records through the intercession of the national government. An employer who desires to filter offenders petitions the government for a Certificate of Good Behaviour. The certificate is a document indicating that no judicial objections exist against a person, as far as the purpose of the application is concerned. Prior to 2004, local authorities assessed whether or not persons could be given the certificate. After April 2004, the Certificate of Good Behaviour is issued by the Central Body for the Certificate of Good Behaviour, a part of the Ministry of Justice. Prior to 2002, which is the relevant period for this data, records for those older than 17 years were expunged after 4 years since a conviction for those who did not get sentenced to prison. Those who were sentenced to prison had their record expunged 8 years after release from prison. If the record check is causing problems with employment and if problems with employment lead to crime (Bushway and Reuter, 2001; Fagan and Freeman, 1999), then it is at least plausible that this expungement will lead to a decrease in recidivism at this time period.

uninfluenced by policy. In the imperfect world we study, the observed recidivism could be influenced by background checks in either direction. If offenders are banned from certain jobs that would reduce their opportunity to reoffend, then the policy will lessen the likelihood of recidivism and our hazards would underestimate recidivism risk. However, if offenders are banned and find that their conventional opportunities are blocked, then they might become more likely to recidivate. If this is the case, then our hazards overestimate the true risk posed by the offenders. The problem becomes more difficult to solve if the banning is being done in ways that are correlated with the factors included in the model, which could lead to over- or underestimates of the impact of risk factors on offending. Although studying the impact of this type of policy is a difficult empirical problem, policy makers feel strongly that background checks have an impact on the behavior of individuals. Future research needs to continue to be sensitive to the potential for policy actions to affect the results of risk assessment.

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