SCARLET LETTERS AND RECIDIVISM: DOES AN OLD CRIMINAL RECORD PREDICT FUTURE OFFENDING?*

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Research Summary:
This research explores the issue of old prior records and their ability to predict future offending. In particular, we are interested in the question of whether, after a given period of time, the risk of recidivism for a person who has been arrested in the distant past is ever indistinguishable from that of a population of persons with no prior arrests. Two well-documented empirical facts guide our investigation: (1) Individuals who have offended in the past are relatively more likely to offend in the future, and (2) the risk of recidivism declines as the time since the last criminal act increases. We find that immediately after an arrest, the knowledge of this prior record does significantly differentiate this population from a population of nonoffenders. However, these differences weaken dramatically and quickly over time so that the risk of new offenses among those who last offended six or seven years ago begins to approximate (but not match) the risk of new offenses among persons with no criminal record.

Policy Implications:
Individuals with official records of past offending behavior encounter a barrier when they try to obtain employment, even if a person's most recent offense occurred in the distant past. There are many reasons for such obstacles, but they are at least partially premised on the concern that individuals with arrest records—even from the distant past—are more likely to offend in the future than persons with no criminal history. Our analysis questions the logic of such practices and suggests that after a given period of remaining crime free, it may be prudent to

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wash away the brand of "offender" and open up more legitimate opportunities to this population.

KEYWORDS: Collateral Consequences, Recidivism, Desistance

INTRODUCTION

Legal restrictions on employing ex-offenders in certain types of jobs are an example of what is known in the legal literature as a "collateral consequence" of an arrest or conviction.[1] Collateral consequences are ethically, if not legally, problematic because they amplify punishment beyond the sanctions imposed by the criminal justice system. There is also a pragmatic public safety concern that ex-offenders who are restricted from jobs might resort to further criminal activity. Although it is important not to overstate the evidence supporting a link between work and crime, most researchers do conclude that employment is at least moderately helpful in the desistance process (see Bushway and Reuter, 2002; Fagan and Freeman, 1999; Sampson and Laub, 1993).

Despite the growing evidence that employment might decrease crime, the use of criminal history records in employment decisions has been increasing over the last 10 years. A recent employer survey suggests that over 50% of employers now check some type of criminal history records in the Los Angeles area (Stoll et al., 2006), and another survey of large employers reports that over 80% now use criminal history records checks in the hiring process. Moreover, new federal rules about background checks for workers in the transportation industry have dramatically increased the number of employees covered by background checks.

Concern about this widespread access to criminal history records has led to a renewed national conversation on the topic. For example, Congress has asked the Attorney General for feedback on the proper use of criminal history records in background checks, and the national consortium of state criminal history record repositories (SEARCH) has commissioned two national task forces to look into different aspects of the use of criminal history records by employers. The Second Chance Act of 2005, currently in Congress, specifically calls on states that request funds for dealing with prisoner reentry to reconsider statutory guidelines that explicitly limit employment opportunities for ex-offenders.

Much of this attention has focused not on denying access to the records

1. In the narrow legal definition, "collateral consequences" are formal legal restrictions imposed by the state on such rights as voting, owning a firearm, parental custody, and employment. For a discussion of the collateral consequences related to employment, see Rubin (1971). For a discussion of collateral consequences more generally, see Burton et al. (1987).

but on better defining the relevance of criminal history records. There is a consensus that the blanket exclusion of individuals with criminal history records makes little sense. Indeed, such a blanket exclusion has been explicitly disallowed as discriminating against minorities under Title VII of the Civil Rights Act.[2] The question is how to decide when a criminal history record is relevant. The Equal Employment Opportunity Commission, while outlawing blanket exclusion, allowed the use of an arrest or conviction record as evidence in an employment decision provided the employer considers the nature and gravity of the offense, *the time that has passed since the arrest*, and the nature of the job held or sought. According to the *Report of the National Task Force on the Commercial Sale of Criminal Justice Record Information* (SEARCH, 2005):

> The relevancy model of the collection, use, and disclosure of criminal justice record information remains in a very nascent stage. Information is increasingly readily available, but relevancy determinations are unclear. As a society, we know very little about whether, and under what circumstances, criminal justice record information (and different kinds of criminal justice record information) is relevant to various determinations involving employment. . .. As a result, the current default, especially in an increasingly dangerous and risk averse society, is to allow all (or virtually all) criminal justice information to reach end-users and then permit end-users, based on their own needs, culture, and law, to sort out the relevancy of the information (SEARCH, p. 75).

The goal of this article is to contribute to the discussion about the relevance of criminal history records for predicting employment behavior. In particular, we focus on the issue of timing. We start with the observation that lifetime bans for all felony convictions are not consistent with the research about desistance from developmental criminology. Recent analysis of data on offenders from adolescence to age 70 shows that most offenders desist, with the bulk of offenders not experiencing additional arrests after age 40 (Blokland et al., 2005; Laub and Sampson, 2003). But if lifetime bans are not appropriate, what exactly is the appropriate "window" on the use of criminal history records? The most recent statistics from the U.S. Department of Justice indicate that over two thirds of prison releasees commit a new offense or violate parole within three years of release (Langan and Levine, 2002) and the probability of failure declines the longer the time since the last offense. Therefore, it is reasonable to ask, from the perspective of the employer, whether the risk of offending

2. The Equal Employment Opportunity Commission (EEOC) issued a policy statement in September 1990 explicitly disallowing the "blanket exclusion" of individuals with criminal records.

for an ex-felon ever becomes similar, or equal to, the risk of offending for someone who has never offended at all? If so, after what period of time since the last arrest or conviction does this occur?

In phrasing the question this way, we want to be clear from the beginning that this article is fundamentally a policy exercise and not an exercise in developmental criminology. The article is specifically designed to help employers and public policy makers determine the relevance of criminal history records for predicting future behavior, including but not limited to future arrest and conviction. Therefore, we base our assessment on the types of criminal history records to which employers might have access, although we acknowledge that these are not a perfect reflection of criminality.

To be specific, we use arrest data from the Philadelphia police records for a cohort of individuals born in 1958. We imagine a scenario in which a Philadelphia native applies to a Philadelphia employer for a job. Our data approximate what a Philadelphia employer would have found had he/she gone to the local courthouse and conducted his/her own search. Such a search is relatively easy to conduct, and it is considered the gold standard of searches by the private records industry (Peterson, 2005). We begin in the next section with a discussion of the literature on the use of criminal history records to predict future behavior.

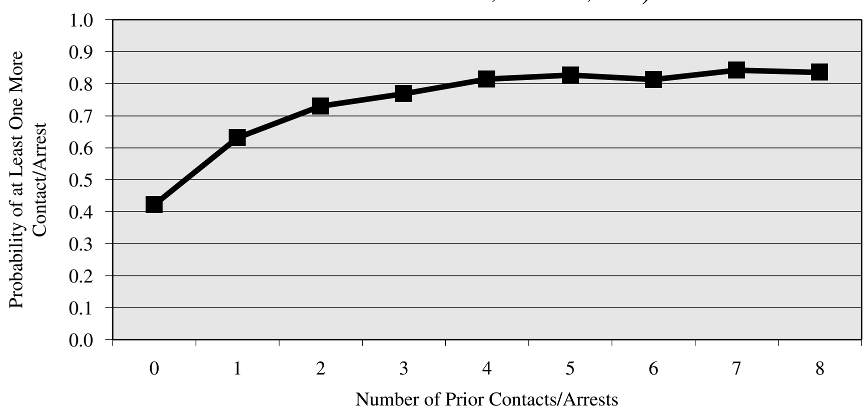
LITERATURE REVIEW

The notion that past behavior is one of the best predictors of future behavior has been accepted as fact in a variety of fields. For example, in the field of education, entrance to college depends on past academic performance in high school and on standardized tests to predict future success. In personal finance matters, creditors rely on an individual's past reliability in paying bills on time and meeting financial obligations to assign a credit score. This score is then used to determine future lending opportunities. Similarly, when applying for auto insurance, one is almost always asked a question such as: "Have you had any traffic violations in the past 3 years?" The answer to this all-important question directly impacts one's insurance premium.

The field of criminal justice has also relied heavily on this basic knowledge. For example, it is known that about 30% to 60% of juvenile delinquents go on to have at least one adult offense (Brame et al., 2003; Farrington, 1987; McCord, 1978; Shannon, 1982). Analysis of recidivism data in several cohorts reported by Blumstein et al. (1985) reveals that most individuals with multiple past official records of offending accumulate new official records of offending in the future [see also, Greenberg

(1991)]. Figure 1 illustrates this point with data from the 1958 (where individuals are followed through age 26). Knowledge of an offender's prior record is, therefore, used as a general indicator of dangerousness and propensity to reoffend at all key decision-making points in the criminal justice process from the police decision to arrest, to the prosecutor's charging decision, to the final sentence handed down by the criminal court judge (Blumstein et al., 1986:75–76; Gottfredson and Gottfredson, 1985).[3]

FIGURE 1. RISK OF NEW OFFENSES BY NUMBER OF PRIOR OFFENSE (1958 PHILADELPHIA BIRTH COHORT MALES, N=13,160)



Perhaps then it is also not surprising that employers would also want to use criminal history records to help them assess applicants. However, there are two primary differences between the employer use of criminal justice records and the other fields' use of past information. First, employers are using criminal justice records to predict employment behavior, whereas other fields rely more heavily on information specific to their own realm (educational achievement used to grant/restrict future educational opportunities, financial failures used to limit financial opportunities). Second, credit scoring companies and insurance companies explicitly restrict the time period for which prior behavior is considered relevant (e.g., credit scores typically look back seven years, whereas insurance records often limit their inquiry to three years).

In contrast, employers are given wide discretion to make decisions about the relevance of the record. The Fair Credit Reporting Act, which

3. At the same time, most researchers warn about the limits of these predictions, given that most measures of predictive accuracy are modest at best (Gottfredson and Gottfredson, 1994). This concern about the limits of our ability to predict future offending is absent in the discussion about employer use of criminal history record.

governs the use of consumer information like criminal history records, was amended in 1998 to eliminate any restrictions on how far back conviction records could be reported (SEARCH, 2005). Moreover, many (but not all) of the statutory prescriptions against employment by ex-offenders are lifetime bans. For example, 24 states have laws mandating lifetime disqualification from unarmed private security guard jobs for any felony conviction, with only 4 states providing offense age limits (Emsellem, 2005). This point becomes particularly significant when considering the criminological findings regarding past criminal behavior. Only about 5% to 10% of young offenders actually go on to become "chronic" criminals over time (see, e.g., Dunford and Elliott, 1984; Moffitt, 1993; Shannon, 1982; Wolfgang, Figlio and Sellin, 1972). Most people with a criminal justice contact at some point early in life actually pose little or no risk of going on to become long-term recidivists. Moreover, existing research suggests that the ignored element of "time since last arrest/conviction" may indeed prove to be useful for understanding the connection between past and future criminal activity.

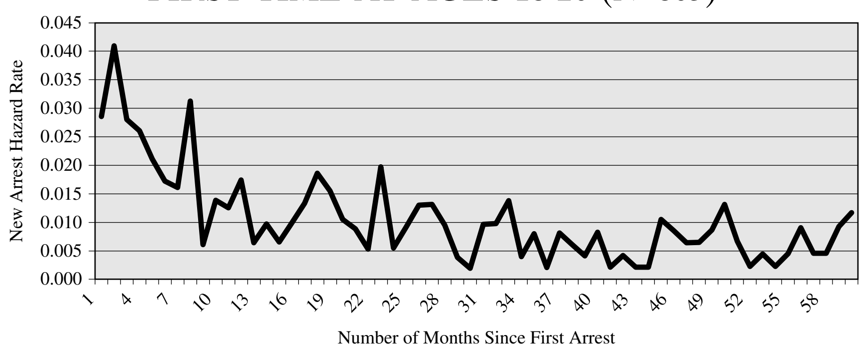
For example, in an analysis of a sample of the original 1945 Philadelphia birth cohort, Raskin (1987) found the hazard rate for reoffending, defined as the probability of offending this period given that the individual has not yet offended, decreases steadily with time since last incident. The hazard rate for a new police contact was the greatest during the first six months following a previous contact, after which time it continually decreased. In fact, during the last month of the study, he found that none of the prior offenders who had "survived" to this point were rearrested. These findings lead Raskin (1987:63) to conclude that, "the longer an individual is able to survive without committing his next offense, the better his chances of desisting from crime."

There is considerable ambiguity about why individuals who have refrained from offending for an extended period of time tend to recidivate at lower rates than individuals who last offended recently. One possibility is that the actual experience of offending abstinence has a causal effect on risk of reoffending; the more a life is lived crime-free, the more one comes to see the benefits of desistance. Another possibility is that individuals with a high risk of recidivism tend to recidivate quickly, whereas others who sincerely try to avoid new offenses tend to dominate the population of lower risk individuals. Regardless of the reason, however, it is clear that individuals who have offended in the distant past seem less likely to recidivate than individuals who have offended in the recent past.

Classic volumes on recidivism by Maltz (1984) and Schmidt and Witte (1988) are especially emphatic in pointing out that parametric models of time to the next recidivism event should be chosen with typical features of recidivism data in mind, the most prominent of which is a highly skewed time-to-recidivism distribution. For example, Schmidt and Witte (1988) followed two cohorts of North Carolina prison releasees to estimate the percentage of released inmates who return to prison. Their analysis shows that the percentage of inmates returning to prison peaked before those inmates had been in the community for 10 months. At the 20-month mark, the percentage dropped to half of the peak level. By the 40-month mark, the estimated percentage returning to prison was half of its 20-month level. These results imply that risk of recidivism for a cohort of offenders returning to the community peaks fairly quickly and then diminishes considerably with the passage of time. Many studies exhibit this same time-to-recidivism pattern (see, e.g., Greenberg, 1978; Harris and Moitra, 1978; Harris et al., 1981; Lattimore and Baker, 1992; Maltz, 1984; Schmidt and Witte, 1988; Visher et al., 1991). In addition, most of the studies of which we are aware indicate that the percentage of the population recidivating begins to approach zero after several years of follow-up (see, e.g., Schmidt and Witte, 1988:50).

Figure 2 summarizes the five-year time-to-recidivism distribution for adult male offenders arrested for the first time between ages 18 and 20 in the 1958 Philadelphia cohort data examined later in this article. Over the five-year follow-up period, a total of 47.4% of these young adult arrestees were rearrested. But, as Figure 2 indicates, the risk of rearrest is not evenly distributed over the five-year follow-up period. The hazard rate plotted in Figure 2 represents the probability that an individual who successfully makes it to a particular time point in the follow-up period is arrested at that time point. This analysis indicates that time-to-recidivism patterns in the Philadelphia data are broadly congruent with those in other recidivism studies.

FIGURE 2. 5-YEAR ARREST RECIDIVISM HAZARD RATE AMONG OFFENDERS ARRESTED FOR THE FIRST TIME AT AGES 18-20 (N=805)



We are, therefore, led to the basis for a useful policy implication: Individuals who have official records of past offending are relatively more likely to offend in the future, but individuals who have managed to refrain from offending for a long period of time, even though they too offended in the past, consistently exhibit much lower risk of future offending than individuals who have offended in the recent past. This finding implies that the length of time that has passed since the last record of offending should accompany information about prior offending records. However, this information cannot be properly interpreted in a vacuum. Even individuals whose last offense record occurred years ago will, as a group, generally exhibit some nonzero risk of reoffending in the future. A logical point of comparison is needed. The likelihood that an individual who has no record will offend can serve as a comparative benchmark. For example, an individual whose last offense record was seven years ago may have much lower objective risk of new offenses now than six years ago. But such an analysis cannot, on its own, tell us anything about whether that person presents a substantially greater risk to the community than someone who has no record of offending.

In this article, we use data from the Second Philadelphia Birth Cohort Study to examine recidivism patterns for people who have a record of past offending in comparison to onset patterns for people who have no record of past offending. In the following sections, we further describe the data, present our analytical results, and offer concluding thoughts and priorities for future research.

DATA DESCRIPTION

For this study, we use a dataset of all males born in the city of Philadelphia in 1958 and who resided in the city between the ages of 10 and 17 years old (N = 13,160). The dates of juvenile police contacts for criminal events were collected on all subjects through age 17. After age 17, arrest dates were collected on all subjects through age 26.[4] Although some collateral consequences are dependent on a conviction, employers are not explicitly barred from taking arrests into account. Alternative data sources would include the FBI NCIC database that is mandated for truck drivers carrying hazardous materials, or the state repository background check from Pennsylvania that is mandated for private security guards. Although the Philadelphia search is less expansive geographically, it is more inclusive; prior research shows that there is substantial "slippage" as records move from the police to the courts and then finally into the repository systems (Briggs et al., 2006; Geerken, 1994). It also contains complete information on arrest, which can be used in employment background

4. Maximum age of subject in dataset is 26.9 years.

checks, and involves a broader measure of criminal activity. Having said that, we also accept that this is a first attempt to answer the question, and we hope that future research will help to answer the question more completely.

Other strengths of this dataset for this particular study include the availability of information about the offense that led to each contact or arrest, which allows us to assess potential differences across several types of offense categories and the inclusion of a population of both offenders and nonoffenders to provide a logical comparison group. .

One potential weakness of our analysis is that some individuals may have moved out of the city after age 17, leading to attrition in the dataset. The extent to which this issue is problematic depends on whether moving is more or less likely for those who get arrested versus those who do not. Generally speaking, wealthier individuals and whites are more likely to move out of a city as they age. These characteristics are negatively correlated with arrest. Therefore, it is reasonable to assume that those who are arrested are less likely to move than those who are not arrested at age 18 or 19 (Geerken, 1994). As a result, our estimates are likely to be overestimates rather than underestimates of the recidivism probabilities.

Finally, the results are unadjusted for periods of incarceration (Eggleston et al., 2004). On the one hand, it is not necessarily a problem. Most statutes and other restrictions are specifically tied to the time since conviction, not the time since release from prison. Therefore, the relevant framework for this policy analysis is the time since conviction. And information about incarceration is typically not available to employers, which makes it hard to think about incorporating incarceration information in any decision rule about past records. However, like developmental criminologists, we want to assess the current criminality of the people in our sample. As a result of this problem, the recidivism probabilities are likely underestimated (Eggleston et al., 2004). In this cohort, we expect the underestimation to be a minor problem.

We rely on two different but complementary analytic frameworks to study the Philadelphia data. First, we use the concept of a hazard rate. As our data are arrayed in discrete time, the hazard rate definition used in this article is straightforward. For any given group, G, comprising $i = 1, 2, \ldots, N$ individuals observed at discrete time points, $t = 1, 2, \ldots T$, we estimate the hazard rate by

$$h(t \mid G) = \frac{\text{\# of Individuals in Group } G \text{ Arrested at Time } t}{\text{\# of Individuals in Group } G \text{ Avoiding Arrest Prior to Time } t}$$

This formula means that individuals who are arrested at time $t - 1$ are no longer considered to be at risk for experiencing a new arrest at time t. That is, once they are rearrested, they are removed from the at-risk population.

The hazard rate as defined above is particularly useful for policy purposes because it represents the case with which a decision maker is often faced. Someone with a criminal record at some point in the past who has avoided new criminal activities for a particular period of time seeks a favorable decision. In this situation, an estimate of the hazard rate would provide helpful information above and beyond simply knowing that an individual had offended at some point in the past. Our hazard rate analysis divides the adult follow-up period into four-month periods through age 26.

Next we calculate the conditional probability that an individual is arrested during the two year period of ages 25 and 26. We denote this probability by $p(a \mid G)$, which implies that we condition our estimate of the probability on membership in a particular group G:

$$p(a \mid G) = \frac{\text{\# of Individuals in Group } G \text{ Arrested at Age 25-26}}{\text{\# of Individuals in Group } G}$$

Our objective here is to determine whether different groups of individuals can be distinguished by their probability of experiencing new arrests during the 25–26 age period.

ANALYSIS RESULTS

In this section, we present several analyses based on records of juvenile police contacts for criminal offenses and adult arrests in the Philadelphia data. As noted, we first estimate the probability that an arrest occurs at a particular time, conditional on no arrest having occurred prior to that time (i.e., the hazard rate). We then estimate the probability that an arrest occurs during the age-25–26 time period for various groups of past offenders and nonoffenders.

HAZARD RATE ANALYSIS

Although there are many ways of dividing a population like the Philadelphia cohort, several are of particular interest to us and we will be referring to them throughout our presentation of the results. Table 1 presents a summary of three different groups used in our hazard rate analysis. Each of these groups can be described in terms of their age-18 arrest records. Our analysis will compare the post-age-18 arrest experiences of the first two groups; in a supplementary analysis, we will also study the post-age-18 arrest experiences of the violent arrestee group.

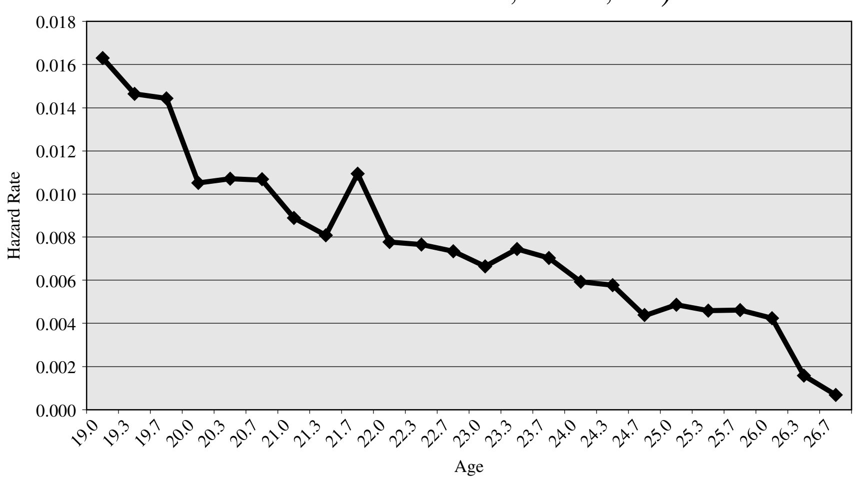
TABLE 1. GROUPS OF INDIVIDUALS USED IN HAZARD RATE ANALYSIS

Group Description	Number of Cases	Percent of Population
Exactly Zero Arrests at Age 18	12,151	92.3
At Least One Arrest at Age 18	1,009	7.7
At Least One Arrest for a Violent Crime at Age 18	375	2.8
At Least One Arrest at Age 18 But No Violence	634	4.8

NOTE: Violent Offenses include homicide/non-negligent manslaughter, rape, robbery, aggravated assault, and simple assault.

Our hazard rate analysis divides the entire period from age 19 to 26 into 24 consecutive four-month periods. At the beginning of each of those time periods, we identify all individuals who have not yet been arrested and the subset of those individuals who are arrested during the time period. The hazard rate at any of these 24 time points is obtained by dividing the latter number by the former. Figure 3 presents the arrest hazard rate from age 19 through age 26 for those individuals who were not arrested at all when they were age 18. The hazard rate for this group declines in nearly monotonic fashion over this eight-year period. At age 19, for example, the hazard rate is approximately 1.5%, which implies that about 1.5% of individuals at risk to be arrested for the first time since turning age 19

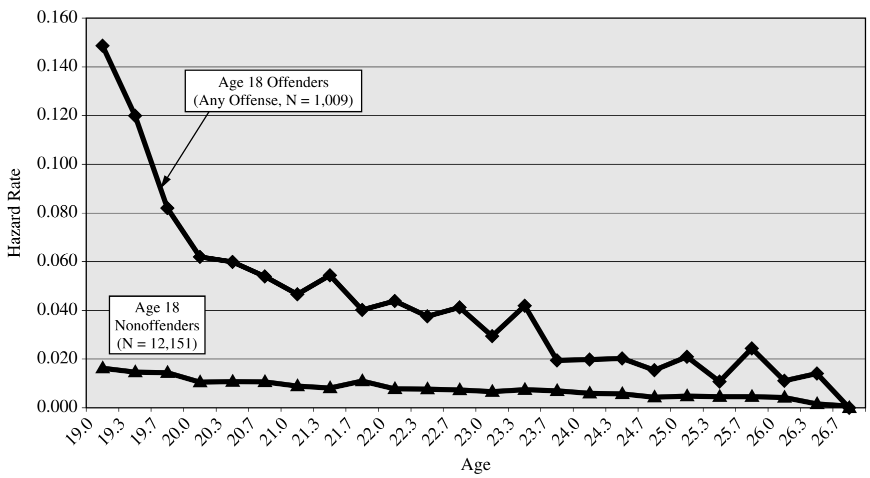
FIGURE 3. ARREST HAZARD RATE BY AGE (AGE 18 NONOFFENDERS, N=12,151)



actually are arrested. By age 25, however, the hazard rate has dropped to less than one half of 1%.

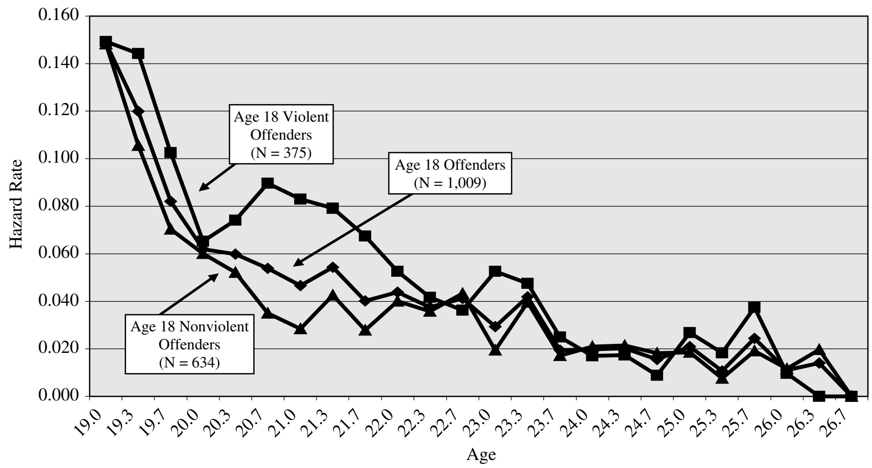
Despite the impressive decreasing trend in the hazard rate from Figure 3, the actual hazards are all very small. This point is best illustrated by comparing the hazard rate of these nonoffenders with those of the age 18 offenders (N = 1,009). Figure 4 presents this comparison. The analysis indicates that the hazard rate for the age-18 offenders is much higher than the age-18 nonoffender hazard rate during the early years of our follow-up period. Like the nonoffenders, the hazard rate for the age-18 offenders declines throughout the early twenties. However, unlike the nonoffenders, the hazard rate decreases in a much more dramatic fashion so that by age 24 the hazard rate for the age-18 offenders drops below 2%. Although this hazard rate is still higher than the comparable hazard rate for the age-18 nonoffenders, the magnitude of the difference is substantively small.

FIGURE 4. ARREST HAZARD RATE BY AGE



To explore the possibility that violent and nonviolent age-18 offenders have different underlying hazard rate patterns, we created two groups: (1) individuals with at least one violent arrest at age 18 (N = 375) and (2) individuals with at least one arrest but no arrests for violence at age 18 (N = 634). As Figure 5 indicates, the hazard rate for the age-18 violent offenders tends to be somewhat higher than for the age-18 offender group. On the whole, however, they are hard to distinguish statistically.

FIGURE 5. ARREST HAZARD RATE BY AGE AMONG AGE-18 OFFENDERS (N=1,009)



Conditional Probabilities at age 25–26

Next, we turn our attention to a comparison of age-25–26 arrest probabilities for several different groups of individuals. Table 2 provides a description of each group used for this analysis. The first group includes individuals who have no record of any juvenile criminal contacts or adult arrests prior to age 25. This group of "clean record" individuals represents a logical point of comparison with groups with some type of juvenile police contact or adult arrest record. Another reasonable comparison group includes individuals in the first group as well as individuals who have a record of at least one juvenile contact for a criminal offense but no adult arrests through age 24. This group is relevant for policies excluding consideration of juvenile offense records.

We also consider a variety of groups defined by the type and last occurrence of officially recorded criminal activity. The first and largest of these groups is comprised of individuals with at least one juvenile police contact for a criminal offense but no adult arrests through age 24 (N = 2,197). In addition, we study the subset of this group with juvenile contacts for nonviolent offenses only (N = 1,517). Next, we turn our attention to individuals who were arrested at least once at age 18 but had no new arrests through age 24 (N = 432). A subset of this group including those who were arrested exclusively for nonviolent offenses at age 18 was also examined (N = 257). Finally, we identified individuals who were, prior to age 25, last arrested at ages 19 (N = 341), 20 (N = 292), 21 (N = 361), 22 (N = 403), 23 (N = 497), and 24 (N = 594).

TABLE 2. CONDITIONAL POSTERIOR PROBABILITY OF ARREST AT AGE 25–26

Group	N=	Proportion Offending at Age 25–26	Median of Distribution	Lower 95% Limit	Upper 95% Limit
No Record	8,043	0.0133	0.0134	0.0110	0.0160
No Record + Juvenile Contacts Only	10,240	0.0204	0.0204	0.0178	0.0233
Juvenile Contacts Only	2,197	0.0464	0.0467	0.0384	0.0560
Juvenile Non-VO Contacts Only	1,517	0.0435	0.0439	0.0343	0.0549
Last Arrested at Age 18	432	0.0718	0.0730	0.0511	0.1001
Last Arrested at Age 18 (No VO Record)	257	0.0623	0.0645	0.0388	0.0987
Last Arrested at Age 19	341	0.1085	0.1100	0.0798	0.1460
Last Arrested at Age 20	292	0.0890	0.0909	0.1091	0.1273
Last Arrested at Age 21	361	0.1413	0.1425	0.1091	0.1810
Last Arrested at Age 22	403	0.1861	0.1871	0.1511	0.2270
Last Arrested at Age 23	497	0.1871	0.1879	0.1553	0.2238
Last Arrested at Age 24	594	0.2963	0.2967	0.2609	0.3342

Our objective for each of these groups is to estimate the probability of an arrest during the two-year period of ages 25 and 26. This analysis framework maps onto the following policy problem: a 25-year old individual approaches a decision maker and seeks a favorable decision. The individual has an official record of some type (i.e., a juvenile record only, or an arrest at age 18). The question is whether the estimated probability of an arrest at age 25–26 [$p(a|G)$ as described] differs between that individual compared to someone with no record at all. To develop inferences about the probability of an arrest at age 25 or 26, we calculate the full posterior probability distribution of this parameter for each of the groups described. The posterior distribution is given by

$$p(a|G) = \pi \times \binom{N_G}{r_G} P_j^{r_G}(1-p_j)^{N_G - r_G}$$

where π represents our prior uninformed belief about the magnitude of $p(a|G)$, which we assume to be identical for each value of $p(a|G)$ between 0.0001 and 0.9999

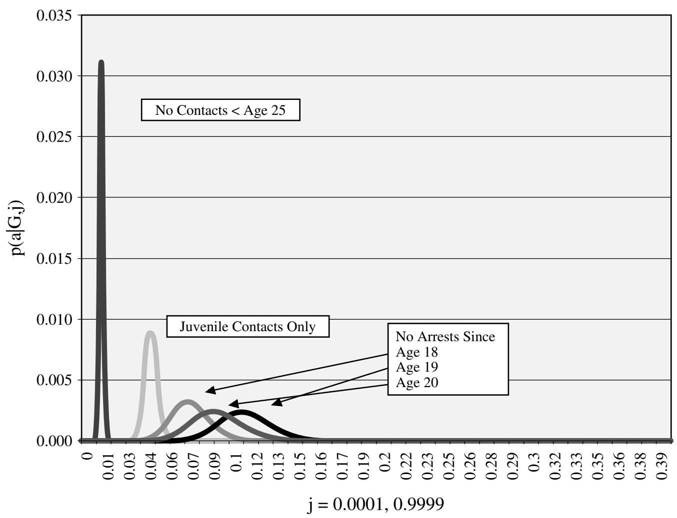
$$(\text{i.e., } \pi = \frac{1}{9999}).$$

Next, we allow j to index the binomial probability from 0.0001 to 0.9999; this allows us to calculate the full posterior probability distribution of $p(a|G)$ conditional on N_G individuals in group G where a subset of the individuals in that group, r_G, are arrested at ages 25 or 26. With an uninformed or flat prior distribution (π), the value of p_j that maximizes the posterior probability of $p(a|G)$ is simply

$$\frac{r_G}{N_G}.$$

But, as Table 2 indicates, the proportion of individuals arrested at age 25–26 is less than 0.08 for six groups in the analysis.[5] Figure 6 displays the full posterior probability distribution for $p(a|G)$ for these five different groups of individuals: those with no record at all; those with juvenile contacts only; and those whose last arrest occurred at ages 18, 19, and 20, respectively.

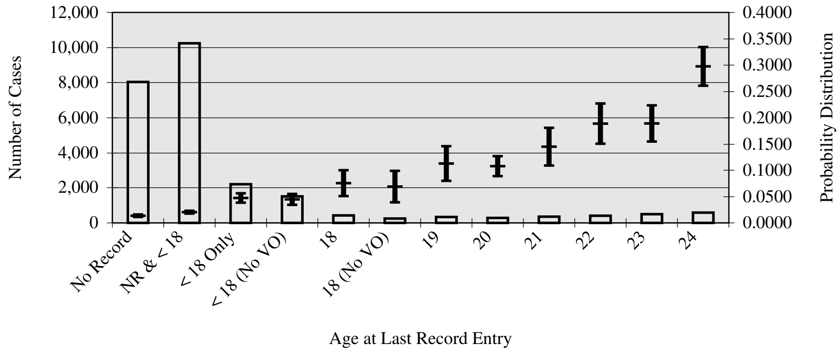
FIGURE 6. POSTERIOR DISTRIBUTION OF p(a|G) FOR 5 GROUPS



The most salient feature of these distributions is the amount of separation between those with and without offending records and their close proximity to zero (i.e., the probability of an arrest at age 25–26 is low regardless of the group to which one belongs). Figure 7 summarizes the analysis results for all groups, including the maximum posterior estimates, the posterior medians (i.e., the 50th percentile of the posterior distribution), and the 95% confidence limits (2.5th and 97.5th percentiles). Based

5. In cases where $p(a|G)$ lies close to the boundary of the parameter space (i.e., in this case, 0), standard confidence interval calculations can yield negative numbers at various confidence limits).

FIGURE 7. PROBABILITY OF ARREST AT AGE 25-26



on this information, we conclude that individuals with no record have a statistically lower risk of arrest at ages 25–26 than all other groups. We also conclude that individuals last arrested in the few years leading up to age 25 are much more likely to be arrested than individuals who were last contacted as juveniles or arrested as 18-year-olds. In other words, the groups included here represent a continuum of risk where those with no record at all have the lowest risk and those with recent records have much higher risk. Individuals in the middle, such as those who were last arrested at age 18, occupy a position on the continuum that is much closer to the no-record group than the recent-record group.

DISCUSSION AND CONCLUSIONS

We began our study with a specific policy question: How do we determine when a criminal history record is relevant to employment decisions? We base our approach on the knowledge that (1) a person who has offended in the past has been found to have a high probability of future offending, but (2) this risk of recidivism is highest in the time period immediately after arrest or release from custody and, thereafter, decreases rapidly and dramatically. This marked and consistent decrease in the risk of future criminal activity then begged the question as to whether this risk ever becomes so small as to be indistinguishable from the risk of persons with no prior offending record. If so, we implied that current social practices of continued civil and social consequences of arrest and conviction may be ill informed.

Our answer to this question based on the current analysis of a cohort of young males from Philadelphia is twofold. First, statistically, we must conclude that persons with a prior police contact or arrest do not, at any time in the given follow-up period, become completely indistinguishable from those without a prior contact in regard to risk of offending. In Figure 4, we see that although the hazard rate for persons with a prior offense rapidly approaches the lower hazard rate of persons without a prior record, at the five-year follow up, the two hazard rates are still separated by over 1 percentage point: a difference that achieves statistical significance in this population. Based on the age-25–26 outcome analysis, we again find that there is a statistically significant difference between those who have never been arrested and those whose first and last arrest occurred at age 18.

Second, the difference is substantively small in magnitude and decreases with time since last criminal event. That is, after some period of time has passed, the risk of a new criminal event among a population of nonoffenders and a population of prior offenders becomes similar. We are struck by the concordance between our results and the new federal statute on background checks for truckers driving hazardous materials. This statute explicitly limits the use of criminal history records to 7 years since the time of conviction. Although further research is clearly needed, we believe that our research supports explicit time limits in any statutory restrictions on employment.

Third, the substantive size of the difference depends on the length of the reference period. In the hazard analysis, we used an exposure period of 4 months and found that the difference in the probability of an arrest between those with no records and those with an arrest at age 18 is about one percentage point (2% vs. 1%) at age 26. When we use the entire two-year period of ages 25 and 26, the difference is almost 6 percentage points (7.2% vs, 1.3%). Although some of this difference can be explained by the fact that the hazard is continuing to decline somewhat rapidly as individuals age, the main reason for the difference is that the nonoffenders have an arrest probability that is close to zero. As we watch the offenders for longer periods of time, we expect that they will acquire disproportionately higher numbers of arrests than will the nonoffenders.

Suppose, for example, that we have two groups, Group A with a starting probability of being arrested in the next month of 0.004 and Group B with the probability of being arrested in the next month of 0.01. At first glance, this difference does not seem large. However, let us consider what happens if we expand our time horizons (assuming a continued declining arrest rate for both populations). After 6 months about 2% of Group A will have an arrest as compared with 7% of Group B. After 1 year, about 3.5% of Group A will have an arrest as compared with 12% of Group B. Moreover, this cumulative difference in arrests will continue to increase until such time, if ever, that the two hazards completely converge—a feat that was not observed within the 7-year time-frame of this particular analysis.

This empirical pattern suggests that the answer to the policy questions concerning the level of elevated risk that is acceptable will depend in part on the decision maker's time horizon. An employer in an industry with high turnover will rationally expect to have relatively short-term contact with the employee, and might therefore be more willing to tolerate the risk than an employer looking to hire individuals for longer time periods. In fact, employer surveys have shown that employers in the secondary market with high turnover are more willing to hire ex-offenders than are those in the primary labor market where employees have long tenure (Holzer et al., 2006).

We must also note that these findings are but a first look at this important question. Our analyses are limited to one cohort of individuals representing one location during one time period. We were also artificially limited to a pre-age-27 follow-up period. To further understand patterns of desistance, we encourage further inquiry into this issue. Areas for future research include the examination of alternative populations from other locations and other time periods. We encourage studies designed to examine longer follow-up periods as our analyses clearly reveal a continued converging trend over time in the risk of new offending for nonoffenders and one-time offenders. We would also encourage a more detailed examination of patterns of desistance as they relate to type of prior offense and demographic characteristics of the population. For example, research suggests that certain statuses such as "being employed" and "being married" promote desistance (Sampson and Laub, 1993).

In addition, a thorough analysis would focus on both employment and criminal history. It strikes us as counter-intuitive that the new statutes requiring background checks have required employees who have been stable employees for several years to be fired if they have a criminal history record. The implicit assumption here is that the past conviction tells the employer more about this individual than the present period of employment. Although we can only speculate at this point, this assumption strikes us as problematic. A simple review of the reentry literature demonstrates that ex-offenders often have a very hard time holding a job (Travis, 2002). The fact that someone keeps the same job for over a year is an excellent predictor of ultimate desistance.

Clearly, there is much more work to be done on this topic. Our analysis provides but one important step toward creating the necessary information for informed discussion about the relative risks of offending presented by individuals with fading scarlet letters.

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