

REDEMPTION IN THE FACE OF STALE CRIMINAL RECORDS USED IN  
BACKGROUND CHECKS

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# **REDEMPTION IN THE FACE OF STALE CRIMINAL RECORDS USED IN BACKGROUND CHECKS**

## **ABSTRACT**

As information technology has increased the accessibility of criminal-history records, and concern for negligent-hiring lawsuits has grown, criminal background checking has become an important part of the hiring process for most employers. As a result, there is a growing concern that a large number of individuals are handicapped in finding employment because of a stale criminal-history record.

This dissertation addresses the issue of “redemption”, which I define as the process as finally ignoring a stale record and focuses on “redemption time”, which is a measurement of how long it takes for an individual with a prior criminal record and no subsequent criminal involvement to be of no greater risk than appropriate benchmarks. The dissertation also provides the understanding of how the time to redemption relates to factors such as age at the prior event, the type of crime committed, and race; factors that are not only important in criminological research, but also relevant in the practice of background checking.

Chapter 2 deals with the measurements and estimation of redemption times. I propose two risk benchmarks against which the recidivism risk of those with a criminal record can be tested. One such benchmark represents the risk of arrest for similar members of the general population, and the other represents that for those who have never acquired a criminal record. The chapter introduces the primary dataset that is used in the dissertation, which is the arrest-history records of individuals who were arrested for the first time in 1980 in New York State. It discusses approaches to estimating redemption times and presents the resulting estimates by age at first arrest and the crime type of the first arrest. Finally, the chapter discusses the possibility of adjustments to the estimates due to the presence of out-of-state arrests and the mortality experience of arrestees.

Chapter 3 explores the issue of robustness of redemption time estimates. Additional data from 1985 and 1990 sampling years in New York as well as data from two additional states, Florida and Illinois, are used to test the sensitivity of the 1980 New York results to these alternative data. The results show that the redemption time estimates are reasonably robust across years and states, and the range of estimates is presented to summarize the results.

Chapter 4 addresses the relationship between race and longer-term recidivism risk, which is relevant to the concern of the Equal Employment Opportunity Commission (EEOC) that criminal background checks have a disparate impact on minorities. The results show that 1) the racial rearrest-risk ratio is smaller than the arrest-prevalence ratio, and 2) the rearrest-risk ratio declines over time, so that the recidivism risk of blacks approaches the risk of whites over time.

Chapter 5 examines the relationship between the crime type of the first crime event and the crime type of a possible second arrest. This recognizes that employers are concerned mostly about particular types of offense that their employees may commit, based on the nature of the job position. The results of analysis based on crime-switch matrices and crime type-specific hazards suggest that in general the type of prior crime tends to be related to a higher risk of rearrest for the same crime.

Chapter 6 discusses how the findings can help shape policies about the use and the distribution of criminal-history records, and Chapter 7 concludes this dissertation and discusses future work, particularly the consideration of multiple prior arrests, that can help move the research on redemption forward. This dissertation contributes to the criminology literature by examining the patterns and consequences of longer-term recidivism risk, which have not been addressed by prior studies on recidivism. This dissertation also contributes to the discussion of policies that are intended to deal with the consequences of widespread background checking, by providing the first explicit empirical estimates of redemption times and by addressing how that information can be used to generate evidence-based public policies related to redemption.

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# CHAPTER 1: INTRODUCTION AND OVERVIEW

## 1.1 Motivation

Background checking, especially checking of criminal-history records, is becoming increasingly ubiquitous in the U.S. Recent advances in information technology and growing concern about employer liability have combined to increase the demand for such background checks. Also, a large number of individual criminal records have accumulated and been computerized in state repositories and commercial databases. As a result, many people who have made mistakes in their youthful past, but have since lived a law-abiding life face hardships in finding employment.<sup>1</sup>

The concern is evidenced by the report from the Attorney General sent to Congress in June, 2006 on criminal history background checks (U.S. Department of Justice, 2006). In the report, there is a recommendation for time limits on the relevancy of criminal records, which reflects the fact that the potentially lasting effect of criminal records is a common concern among many governmental and legal entities that have a say in this issue. Such entities include the U.S. Equal Employment Opportunity Commission (EEOC), which is concerned about discrimination based on criminal records because those with criminal records are disproportionately racial/ethnic minorities.<sup>2</sup> The American Bar Association (ABA) is also concerned about the negative lasting effect of criminal records in employment settings. Both these organizations are taking an initiative to broaden the discussion about the problem of the way in which criminal

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<sup>1</sup> The concern has been raised at least since the 1970s (Maltz, 1976; Westin and Baker, 1972).

<sup>2</sup> For example, see here: <http://www.eeoc.gov/abouteeoc/meetings/11-20-08/index.html>.

records are currently used and to address how to regulate the use of criminal records, including a time limit on their relevancy.<sup>3</sup>

It is our goal in this project to provide guidance on the possibility of “redemption”, (which we define as the process of lifting the burden of the prior record), and to provide guidance on how one may estimate when such redemption is appropriate. Numerous studies have shown in the past that recidivism probability declines with time “clean,” so there is some point in time when a person with a criminal record who remained free of further contact with the criminal justice system is of no greater risk than any counterpart, an indication of redemption from the mark of an offender. We henceforth call this time point “redemption time.”

The following sections discuss the recent trends about the practice of criminal background checking, particularly by employers, and the volume of computerized criminal records that are available for such background checks. It also addresses the problem of the lack of guidelines that could help employers understand how the “age” of a criminal record relates to the level of risk of a new crime. By discussing the trends, we demonstrate that the problem of redemption is a pressing public concern, and that empirically based guidance on redemption is urgently needed.

## **1.2 Prevalence of Criminal Background Checking**

With the recent advancement in information technology and the Internet, individuals’ criminal records have never been more easily accessible. The background-check industry is burgeoning. There are numerous companies that acquire and compile criminal justice information obtained from the police and the courts and assemble a database for commercial purposes (Barada, 1998; Munro, 2002). SEARCH (the National Consortium for Justice Information and Statistics) reports

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<sup>3</sup> For example, see here: [http://www.abanet.org/abanet/media/release/news\\_release.cfm?releaseid=234](http://www.abanet.org/abanet/media/release/news_release.cfm?releaseid=234)

that, “in addition to a few large industry players, there are hundreds, perhaps even thousands, of regional and local companies” that compile and/or sell criminal justice information to the end users (SEARCH, 2005: 7). They provide background- check services to private employers at their convenience in a timely manner at decreasing costs (SEARCH, 2005). A recent survey of firms from multiple cities in the U.S. reveals that about 50 percent check the criminal background of job applicants (Holzer, Raphael, and Stoll, 2004). Surveys on human resource professionals indicate that 80-90 percent of large employers in the U.S. now run criminal background checks on their prospective employees (Society for Human Resource Management, 2004, 2010).

Some employers may conduct criminal background checks on job applicants voluntarily to identify those who may commit criminal acts in the workplace in order to minimize loss and legal liability of negligent hiring that could result from such acts (Bushway, 1998)<sup>4</sup>. For some job positions involving vulnerable populations, such as children and the elderly, laws require employers to conduct such background checks (Hahn, 1991). In addition, employers may use criminal history records to assess character flaws such as lack of honesty and trustworthiness (Kurlychek, Brame, and Bushway, 2007; Pager, 2007). Also, occupational licensing laws could disqualify many individuals based on the requirement of “good moral character” (Harris and Keller, 2005; May, 1995).<sup>5</sup> As the use of criminal background checks by employers has become widespread, criminal records could have lingering effects on employment prospects as “invisible punishment” or collateral consequences of contact with the criminal justice system (Travis,

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<sup>4</sup> Criminal background checking is viewed as a routine practice by human resource professionals (Levashina and Campion, 2009).

<sup>5</sup> We do not elaborate more on employers’ concern over whether a criminal record signals a lack of good character. The investigation of such considerations and its relationship with time clean warrant future research on employer judgments.

2002).<sup>6</sup> Many employers show considerable reluctance to hire individuals with criminal records (Holzer, Raphael, and Stoll, 2003; Pager, 2003; Schwartz and Skolnick, 1962; Holzer, Raphael, and Stoll, 2004);<sup>7</sup> others have shown the relationship between criminal records and poorer employment prospects (Bushway, 1998; Grogger, 1995; Nagin and Waldfogel, 1995; Western et al., 2001).

### **1.3 Prevalence of Criminal Records**

In 2007, according to the Uniform Crime Report (UCR), law enforcement agencies across the U.S. made over 14 million arrests (Federal Bureau of Investigation, 2008). On December 31, 2006, over 81 million criminal-history records were in the state criminal-history repositories (Bureau of Justice Statistics, 2008).<sup>8</sup> The increasing automation of criminal history records in the repositories has increased the number of records that are electronically accessible. At the end of 2006, about 91 percent of the records were automated (Bureau of Justice Statistics, 2008).

Prior research suggests that the general public's chance of being arrested in their life time is rather high. Over forty years ago, it was estimated that fifty percent of the U.S. male population would be arrested for a non-traffic offense sometime in their lifetime (Christensen, 1967). Among those who have an arrest record, some have an isolated record that was acquired

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<sup>6</sup> Collateral consequences of contact with the criminal justice system occur mostly outside the public view and affect ex-offenders beyond the imposed sentences (Travis, 2002: 16). They include restrictions on professional and occupational licensing, which are possibly important means for ex-offenders to increase their employment opportunities. The occupations that are affected by the restrictions range from health care, nursing, and education, to plumbing and barbering. Collateral consequences could also include denial of governmental benefits, such as welfare and public housing, termination of parental rights, and revocation or suspension of driver's licenses (Kethineni and Falcone, 2007; May, 1995; Petersilia, 2003; Samuels and Mukamal, 2004; Wheelock, 2005).

<sup>7</sup> Some evidence suggests that the negative effect of criminal background checks on the hiring of ex-offenders is strongest for employers who are legally required to conduct such background checks (Stoll and Bushway, 2008).

<sup>8</sup> An individual offender may have had records in multiple states.

years ago and have maintained a clean record since then, but the evidence of contact with the criminal justice system, even if it was in the distant past, could remain in the repositories forever.

#### **1.4 Relevance of Criminal History**

One rationale behind the practice of checking the criminal background of job applicants is that the employers recognize the strong positive relationship between past and future criminal offending. The continuity in criminal behavior has been validated by many studies (Blumstein et al., 1985; Brame et al., 2003; Farrington, 1987; Piquero et al., 2003). While these studies lend support to employers who would avoid any potential employees with a criminal-history record, these employers would also be well advised by some interlinked lines of research in criminology, which present equally strong evidence of desistance from crime in a subpopulation of those with past offenses. One line of research argues that changes in the life course of offenders affect their risk of future involvement in crime. For example, it is well established that a stable marriage and employment are powerful predictors of such desistance (Sampson and Laub, 1993; Sampson et al., 2006; Uggen, 1999; Wallman and Blumstein, 2006; Warr, 1998). Also, in another line of research, the age-crime curve demonstrates a steady decline in criminal activity after a peak in the late teens and young-adult period, and aging is one of the most powerful explanations of desistance (Farrington, 1986; Hirschi and Gottfredson, 1983; Sampson and Laub, 1993; Sampson and Laub, 2003).

Most importantly for the current study, time clean since the last offense strongly affects the relationship between past and future offending behavior. Studies on recidivism consistently demonstrate that those who have offended in the past will have the highest probability of reoffending within several years, and the probability will decline steadily afterwards (Maltz,

1984; Schmidt and Witte, 1988; Visser et al., 1991). Two studies that tracked released U.S. prisoners show that of all those who were rearrested in the first 3 years, approximately two-thirds were arrested in the first year, indicating the declining recidivism rate over time (Beck and Shipley, 1997; Langan and Levin, 2002). Another study examined the effects of sentences on 962 felons convicted between 1976 and 1977 in Essex County, New Jersey, by following their recidivism (measured by rearrest) for over 20 years (Gottfredson, 1999). This study shows that while half of those rearrested were arrested within 2.2 years, 30 percent of the offenders remained arrest-free after the original sentence. The calculation based on the Essex data reveals that among those felons who stayed free of crime for 10 years after the original conviction, only 3.3 percent were reconvicted within the next 10 years (Community Legal Services, Inc., 2005).

There have been numerous other studies showing that recidivism occurs relatively quickly. However, little attention has been paid to the smaller population of ex-offenders who stay crime-free for an extended period of time. Recent papers by Kurlychek and her colleagues have shed some light on the population characterized by long-time avoidance of crime (Kurlychek, Brame, and Bushway, 2006, 2007). Examining the hazard rate, they show that the risk of offending for those with criminal records converges toward the risk for those without a record as substantial time passes.<sup>9</sup>

Kurlychek, Brame, and Bushway (2006) used the longitudinal data from the Second Philadelphia Birth Cohort Study (Tracy et al., 1990). The major advantage of such longitudinal samples is that they have a representative population of non-offenders, which makes it possible to compare the hazard of those with records to those without. However, longitudinal samples are often limited in size and the follow-up might not be as complete as one desires.

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<sup>9</sup> Soothill and Francis (2009) reached a similar conclusion using data from Britain and Wales.

## 1.5 Overview

This dissertation focuses on estimating the time to redemption and understanding how the estimates vary with factors such as age at the prior event, the type of crime committed, and race that are not only important in criminological research but also relevant in the practice of background checking. Another thrust of the dissertation is to test the robustness of the redemption times against a number of the conditions under which redemption times are estimated such as sampling year and jurisdiction (states). The robustness tests are essential in strengthening the generalizability of the findings that we believe can help shape important policies relevant to redemption.

Chapter 2 deals with the measurements and estimation of redemption times. How should we determine a time point when the declining risk of recidivism is sufficiently low for redemption to be achieved? We propose two risk benchmarks that can be compared with the recidivism risk of those with a criminal record. One such benchmark represents the risk of arrest for similar members of the general population, and the other represents that for those who have never acquired a criminal record. The chapter discusses the data requirements for the estimation of redemption time and introduces the primary dataset that is used in the dissertation, which is the arrest history records of individuals who were arrested for the first time in 1980 in New York State. The chapter then introduces survival function and hazard, two fundamental quantities that are used to estimate and contextualize recidivism risk. It discusses approaches to estimating redemption times and presents the resulting estimates by age at first arrest and the crime type of the first arrest.

Since the duration of time clean is only accounted for within the state, it is necessary to examine the extent to which the redemption estimates might need to be adjusted for out-of-state

criminal records. One approach for such adjustment is proposed and discussed, the redemption estimates that correct for the out-of-state arrests are presented. Given our finding that there is an appreciable effect of out-of-state arrests on the redemption estimates in New York, the correction should be applied. The possibility of another similar adjustment, which can account for the mortality experience of the arrestees, is explored. The magnitude of the effect of mortality is found to be negligible and would not change the estimates of redemption times by a meaningful amount.

Chapter 3 explores the issue of robustness of redemption times. The estimates of redemption times that are discussed in the previous chapters are based on data from a particular sampling year (1980) and a particular state (New York). In order to help formulate reasonable guidance on redemption times for employers who run and interpret background checks, it is important that we understand how sensitive the estimates are to the changes in the circumstances. Additional data from 1985 and 1990 sampling years in New York as well as data from two additional states, Florida and Illinois, are used to test the robustness. Several approaches to examining the robustness are proposed and discussed. Despite considerable changes in the level of arrest rates across sampling years, we find that the patterns of recidivism risk and the redemption time estimates are robust across years. Although we observe more differences in the patterns of recidivism risk across states, the estimates of redemption times are robust and the differences are, on average, within only 1-2 years.

Chapter 4 addresses the relationship between race and redemption. Race is known to be a strong predictor of arrest prevalence. However, it has also been shown that race is much less associated with offending frequency (Blumstein and Graddy, 1981). This observation prompts the examination of the relationship between race and the redemption process, the patterns of

recidivism after a substantial length of crime-free time. Then, the difference in redemption patterns between whites and blacks, which is represented by the black-to-white ratio of the hazards for a new arrest, will be compared with the difference in the arrest prevalence between the two races. The results show that 1) the hazard ratio is smaller than the prevalence, and 2) the hazard ratio declines over time, which suggests that the recidivism risk of blacks approaches the risk of whites over time.

Employers are concerned mostly about particular types of offense that their employees may commit, based on the nature of the job position and the industry. Violent crimes are of a primary concern for positions involving frequent one-on-one contact with clients especially those who are vulnerable such as children and the elderly. Whereas property crimes pose a main concern for positions involving cash and other financial assets. Chapter 5 addresses those concerns by constructing crime switch matrices and examining which crime type of the prior event elevates the likelihood of committing a new crime of a particular crime type. We then estimate crime type-specific hazards and redemption times as a function of the combination of crime types of first and second crime events. Prior violence is found to be a significant predictor of committing a new violent offense, while a prior record of property crime seems to be an important predictor of future property crime only for those who have their first arrest at relatively old ages.

Chapter 6 discusses how the findings can help shape policies about the use and the distribution of criminal-history records. For the users of criminal records such as employers and pardon boards, the findings about the redemption times can be used to create guidelines that reflect the fact that over time, criminal-history records have a diminishing value in differentiating those with a high risk of committing a crime and those with a low risk. For the distributors of

criminal records such as state repositories and commercial vendors, the findings can help them consider the possibility of limiting the distribution of records based on the age of the records because criminal-history records have possibly a long-lasting effect on individuals who have long turned their lives around.

This dissertation contributes to the criminology literature by examining the patterns and consequences of longer-term recidivism risk, which have not been addressed by prior studies on recidivism. This dissertation also contributes to the discussion of policies that are intended to deal with the consequences of widespread background checking, by providing the first explicit empirical estimates of redemption times and by addressing how that information can be used to generate evidence-based public policies related to redemption.

## CHAPTER 2: MEASURING REDEMPTION

### 2.1 Measures of Redemption

While past wrongdoings are a useful sign of future trouble, this information has decreasing value over time because the risk of recidivism decreases monotonically with time clean, and there can come a point where we can be confident that redemption has been reached, where the risk of reoffending has subsided to the level of a reasonable comparison group. The problem here is that there is very little empirical information that can help to establish that point. The absence of reliable empirical guidelines leaves employers no choice but to set their own arbitrarily selected cut-off points based on some intuitive sense of how long is long enough, inevitably with a conservative bias.<sup>10</sup> Given the importance of this issue, particularly for those individuals with other employment vulnerabilities, it becomes important to develop empirical estimates of a reasonable point of redemption or redemption time.

One such point, which we denote as  $T^*$ , is where the recidivism risk of someone with a prior criminal record (arrest, conviction) declines and intersects the level of the general population of the same age, and so can serve as a point of redemption. This can help an employer who has selected a job applicant for a position and wants to compare that individual's risk of

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<sup>10</sup> For example, the Transportation Security Administration requires maritime workers to obtain a Transportation Worker Identification Credential (TWIC) to access secure areas of port facilities. Individuals are disqualified from getting a TWIC if they have been convicted for certain disqualifying criminal offenses within 7 years of the TWIC application (Transportation Security Administration, n.d.). To the best of our knowledge, the choice of the cutoff points is arbitrary and not based on any empirical analysis. Although 7 years seems to be a common restorative period, perhaps based on a view that 5 years is too short and 10 years is too long, some evidence exists that the cutoff points set by users of criminal records could be much larger or could be “indefinite” (Carey, 2004: 50). The Fair Credit Reporting Act states that a vendor of criminal history records may not report arrest information that is older than 7 years (Hinton, 2004). However, Bushway et al. (2007) found that there is little evidence that employers adhere to the regulation. Many employers are found to claim that they can search arrest records that are more than 7 years back.

arrest with someone of the same age from the general population. There is a reasonable expectation that the intersection occurs because the general population includes people who have no criminal records as well as people who have recent arrests, and thus have a relatively high risk of reoffending, while a redemption candidate with a prior record has been arrest-free for  $T^*$  years, during which time the person's recidivism risk should have fallen considerably.

Now suppose an employer has multiple job applicants for a position, on all of whom a background check is run. Those with no prior record (whom we designate as the "never arrested") are inherently less risky than those with a prior record, but that difference can diminish with the time the individual with a prior arrest stays clean. This provides another point of redemption: when the recidivism risk of an individual with a criminal record is "sufficiently close" to one without, and we designate that point as  $T^{**}$ .  $T^{**}$  should be larger than  $T^*$  because the comparison group (the never arrested) are less risky than the general population.

It is reasonable to expect that  $T^*$  and  $T^{**}$  will vary with the crime type of the earlier arrest, denoted as  $C_1$ . Recidivism studies have shown that the crime type for which state prisoners were released was related to recidivism rates (Beck and Shipley, 1997; Langan and Levin, 2002). Prisoners who were released for "crimes for money": burglary, robbery, larceny, and motor vehicle theft had the highest recidivism rates in both studies.  $T^*$  and  $T^{**}$  could also vary with the age of the prior arrest, denoted as  $A_1$ , recognizing that criminological research consistently indicates that an earlier onset age is a good predictor of a serious criminal career which is characterized by a larger number of offenses and a longer career duration (Blumstein et al., 1986; Farrington et al., 1990; Farrington et al., 2003; Piquero et al., 2007). Since a prior record of violence, especially the record at younger ages, predicts more serious and chronic

offending (Elliott, 1994; Farrington, 1991; Piquero et al., 2007), recidivism risk is expected to be higher for those whose early arrest was for violence (Piper, 1985).

Age and crime type of the prior arrest should also be taken into account in estimating  $T^*$  and  $T^{**}$  because the information about these factors usually appears on the criminal background reports that employers obtain, and so the information is available to be used in the hiring decision.

We are interested in developing estimates of  $T^*$  and  $T^{**}$  as a function of these characteristics of the earlier record. This approach is related to the more familiar approach of estimating recidivism probability. It is more complicated, however, because one must examine the record over an appreciably longer period of time. In recidivism studies, it is usually sufficient to track individuals for as short as five years because the large majority of individuals who will recidivate will do so within the first several years (e.g., Beck and Shipley, 1997; Langan and Levin, 2002). However, the estimation of  $T^*$  and  $T^{**}$ , particularly as a function of  $A_1$  and  $C_1$ , requires observation over a much longer interval, long enough for the recidivism probability to become small enough, and this requires much larger initial samples than those used in past studies (Kurlychek, Brame, and Bushway, 2006, 2007) so that we can estimate the recidivism probability with sufficient precision after the large majority of any initial cohort has already recidivated (Pager, 2006).

This section first introduces the data used in the analysis to estimate hazard. It then describes the hazard estimation procedure. Next, an approach to comparing redemption candidates with the general population and the resulting estimates of  $T^*$  are discussed. Then, an approach to comparing redemption candidates with those who have never been arrested and the resulting estimates of  $T^{**}$  are discussed.

## 2.2 Data

Our research approach requires starting with criminal-history records initiated long enough ago that we can be confident that after having been free and clean of arrests, the individuals with those records have a low risk of recidivism. On the other hand, we would like records from a time when the computerization of rap-sheet information was sufficiently advanced so that the computer records would provide an appropriate sample. Thus, we contacted the criminal-history repository in New York State in 2007 asking for a sample of individuals arrested for the first time as adults in 1980. This provided an interval of 27 years to follow the individuals and assess their recidivism probabilities. This also provided a large enough population to disaggregate into a reasonable number of interesting crime types and age at first arrest and still have an adequate number of individuals who have remained clean of crime 10, 20, and even 25 years later.

Over 75,000 individuals were recorded as experiencing their first arrest in 1980 in New York State.<sup>11</sup> From this total population, we focus on individuals whose age at first arrest was between 19 and 30.<sup>12</sup> The records are de-identified so that identifiers such as name and social

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<sup>11</sup> The data received include all individuals with an arrest recorded in the New York State Division of Criminal Justice Services repository of criminal-history records. There are other individuals with one or more arrests that were sealed but with no unsealed arrests; these individuals were not included in the files we examined. In a background check, these individuals would presumably appear as never arrested. It is also possible that individuals with an initial arrest in 1980 that was sealed before they had an opportunity for a second arrest after 1980, and then appeared at a later time with an arrest that was not sealed; in that case, their second arrest would have been recorded with a different ID number and would not have been included in our 1980 sample. We were unable to link the two components of such an individual's records. In order to avoid the complication caused by the sealing, we only use data on individuals with *unsealed* records.

<sup>12</sup> The reason to focus on the 19-30 age range is that the arrestees whose ages are between 16 and 18 are considered "youthful offenders" in NY and their criminal records are often sealed (in contrast to most other jurisdictions, New York considers 16-year-olds to be "adults.") The examination of national records from the FBI indicates that a number of those with older  $A_1$ 's (especially over 30) had an adult arrest record prior to 1980 in NY, a recording anomaly that would disqualify them as "first-time arrestees" in

security number are not included. Instead, individuals are assigned a unique state identification (SID) number, which is scrambled and this allows us to link their criminal records over time and to retrieve it in the future.

The crime types we focus on here have to be sufficiently numerous, reasonably serious (e.g., we avoid DUI), and less than permanently damaging (e.g., murder). Our analysis of  $T^*$  focuses on three offense groups: violent, property, and drugs.<sup>13</sup>

### **2.3 Conviction vs. Arrest**

In the U.S. criminal justice system, the distinction between arrest and conviction is important. An arrest event usually takes the form of a person being taken into custody by police because the police believe that it is probable that the person committed a crime. At the point of arrest, the person arrested is still a suspect since he or she is not yet officially charged with nor convicted of any crime. Thus, the arrest marks the beginning of a criminal justice process. If the person is convicted, that means that the court determined that the person is guilty of the crime for which he or she is charged. In other words, an arrest is not considered by the criminal justice system as the proof of a commission of crime, but a conviction is.

Since the record of arrest alone does not indicate that the person has committed a crime, laws that govern the criminal background checks by employers often restrict the use of arrest information. In many employment screening settings, employers are prohibited from asking

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1980. In order to minimize these problems while retaining a large enough sample size for the precision in the estimation of hazards, we focus here more narrowly on those with  $A_1$  in the 19-30 range.

<sup>13</sup> Violent crimes are designated to include robbery, aggravated assault, forcible rape, and simple assault. Murder and non negligent manslaughter are not included as  $C_1$  because special conditions are likely to apply to their redemption. Property crimes are designated to include burglary, larceny, motor vehicle theft, stolen property, forgery, fraud, and embezzlement. Drug crimes include both possession and sales of any controlled substance.

about an arrest record in the absence of a following conviction. The Equal Employment Opportunity Commission (EEOC) issued guidelines limiting the extent to which the record of an arrest that is not associated with a conviction can be used in hiring decisions. The guideline states that employers may not deny individuals based on an arrest record unless they can establish a justification that the conduct for which the person is arrested is “job related” and relatively recent (EEOC, 1990). The Fair Credit Reporting Act (FCRA) states that a vendor of criminal history records may not report arrest information that is older than 7 years (Hinton, 2004).

Because of the reasons above, the redemption times will be estimated based on those in the 1980 NY arrestee cohort who were convicted.<sup>14</sup> Of course, the initial sample will become smaller since many of our arrests were not followed by convictions. Table 2.1 shows the distribution of disposition types.<sup>15</sup> Table 2.2 provides the distribution of the sample by age and crime type at first arrest that led to conviction.

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<sup>14</sup> Some of those who were arrested in 1980 might have been convicted in 1981. For example, if an arrest occurred in December 1980, it is likely that the conviction that results from the arrest would occur in 1981. In our analysis, we do not make a distinction between the 1980 arrests that led to conviction in 1980 and the 1980 arrests that led to conviction in 1981.

<sup>15</sup> Major improvements in the quality of criminal history records came into effect in the 1990s through legislation such as the Brady Handgun Violence Prevention Act in 1993, and programs such as the National Criminal History Improvement Program in 1995. Through these initiatives, the federal government has been providing financial support to encourage the state repositories to ensure that the criminal history information is accurate and complete, where the completeness of the information means that a record of arrest needs to be accompanied by any relevant disposition information. As a result of these measures, it is expected that we will observe increased accuracy and completeness in the criminal history after 1990's. Based on our NY data, the percentage of known dispositions increased slightly from 82.9% in 1980 to 86.1% in 1990.

Table 2.1. Dispositions in NY in 1980 (for  $A_1 = 19-30$ )

Disposition	Frequency (%)
Conviction	15,948 (59.48%)
Non-conviction	6,266 (23.37%)
Unknown disposition	4,600 (17.16%)

Table 2.2. Initial Sample Size (convicted),  $n$  by Age at First Arrest ( $A_1$ ) and First Offense ( $C_1$ ) in 1980 <sup>16</sup>

$A_1$	$C_1$				
	Violent	Property	Drugs	Public Order	Others
$A_1 = 19,20$	971	2,510	546	824	522
$A_1 = 21-24$	1,066	2,558	729	904	641
$A_1 = 25-30$	871	1,945	627	716	518

## 2.4 Estimation Models of Survival and Rearrest Risk

We model time to recidivism using survival analysis, a statistical method developed to study the occurrence and the timing of events. Since the methods are flexible and generic, they have been used for studying a wide variety of events, such as deaths, marriages, cancer cures, unemployment, militarized disputes, earthquakes, equipment failures, and so on. Criminologists have long used the methods to study recidivism (e.g., Maltz, 1984; Schmidt and Witte, 1988; for review, see Chung et al., 1991). In general, we make most use of two statistics that are commonly used in survival analysis, survival function and hazard (rate).

<sup>16</sup>  $C_1$  refers to the type of offense for which the arrest was made, regardless of the conviction offense, which could possibly be different from the arrest offense.

### 2.4.1 Survival Function

Survival probability is the probability of surviving beyond time  $t$  without a subsequent arrest. We define  $T$  as the random variable that represents the time to the subsequent arrest. Survival probability,  $S(t)$ , is calculated as

$$S(t) = \Pr(T > t) = \frac{\text{\# of arrestees who have not had a new arrest by the end of time period } t}{\text{Total \# of arrestees in 1980}}$$

Survival probability can be translated intuitively to be the potential for redemption at time  $t$ . It can be used to describe the proportion of those who reach any specified redemption time.

### 2.4.2 Hazard

Hazard,  $h(t)$ , is defined as

$$h(t) = \lim_{\Delta t \downarrow 0} \frac{\Pr(t \leq T < t + \Delta t | T \geq t)}{\Delta t}.$$

Thus  $h(t)$  is the instantaneous rate of a new arrest at time  $t$ . For “small” values of  $\Delta t$ ,

$$\Pr(t \leq T < t + \Delta t | T \geq t) \approx h(t)\Delta t.$$

Thus the hazard can approximate the conditional probability of a new arrest at time  $t$ , given survival to  $t$  without an arrest (Hess et al., 1999, Wooldridge, 2002). This is the quantity employers and others would use to evaluate the offending risk of a person who has been revealed by the background check to have committed a crime  $t$  years ago and none since (Kurlychek, Brame, and Bushway, 2006).

In calculating  $h(t)$ , we count a new arrest (after their initial arrest in 1980) for any offense type.<sup>17,18</sup> Thus, for example, a new arrest is marked when a person whose first arrest occurred in

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<sup>17</sup> Throughout our analyses, we ignore all DUI arrests because DUI arrests are so prevalent that they might distort our results.

1980 (say, for burglary) is rearrested for any offense (burglary or otherwise). We estimate the hazard, given conditions at the first arrest, namely the arrestee's age  $A_1$  and the crime type  $C_1$  of the first arrest.

### 2.4.3 Smoothing Hazard

In order to reduce random fluctuations that prevent us from capturing the overall trend of the hazard, we smooth hazard estimates using kernel smoothing (Klein and Moeschberger, 2005; Wang, 2005).<sup>19</sup> The kernel-smoothed estimator of  $h(t)$  is a weighted average of crude hazards over the interval  $(t-b, t+b)$  where  $b$  is the bandwidth or window width that controls the smoothness of the hazard estimates. The weights are provided by kernel functions such as uniform, Gaussian, and Epanechnikov; the uniform kernel assigns an equal weight to the points in the interval, while other kernels assign more weights to the points closer to  $t$ . It's been shown that the choice of kernel does not affect the resulting hazard estimate (Hess et al., 1999). The Epanechnikov kernel is most commonly used, and it is used in our analysis.

Figure 2.1a displays the smoothed estimates of  $h(t)$  for  $A_1$  of 19-20 and of 25-30 for  $C_1$  of violent crimes. Figure 2.1b shows the estimates of  $h(t)$  for  $A_1 = 19-20$  for  $C_1 =$  violent, property, and drug crimes. As expected,  $h(t)$  varies with  $A_1$  and  $C_1$ . The hazard curves differ primarily in the first 10-12 years, with violent crimes tending to have the higher hazard than property and drug crimes. Also, a younger  $A_1$  is associated with a higher hazard. This is consistent with

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<sup>18</sup> In some cases, we find that an arrest is followed quickly by another arrest. We are concerned that what seems to be a new "arrest" might be related to the same crime event as the prior arrest (e.g., transfer to a different jurisdiction), so we count an arrest as a new arrest only if it occurs at least 30 days after the prior arrest.

<sup>19</sup> The particular algorithm that we use to produce hazard estimates is described in Müller and Wang (1994) and implemented in the function *muhaz* in the R statistical package.

general findings in criminology that younger starters persist longer in their criminal careers (Piquero et al., 2007).

Figure 2.1a. Hazard  $h(t)$ : Age 19-20, 25-30 Violent (convicted) in 1980

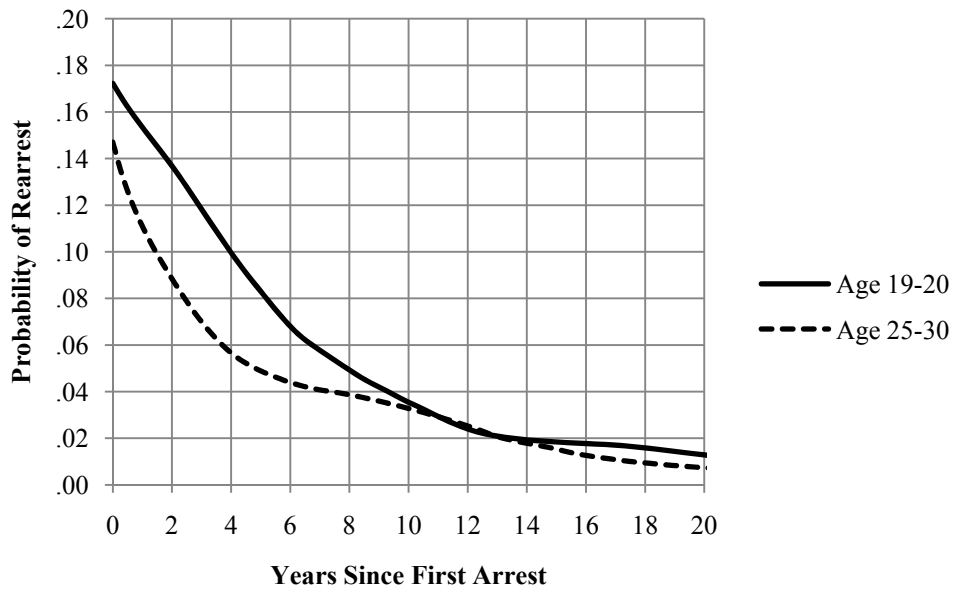
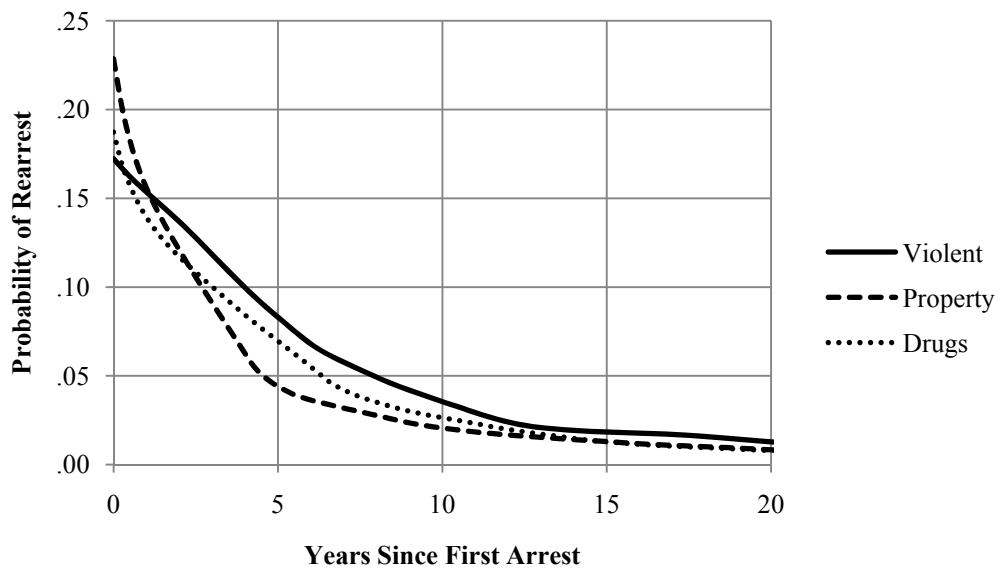


Figure 2.1b. Hazard  $h(t)$ : Age 19-20, Violent, Property, Drugs (convicted) in 1980



## 2.5 Incarceration

The length of crime-free time after 1980 is central to the estimation of redemption times. Thus, it is important that during crime-free time, the arrestees are not incarcerated and so are not at risk of being rearrested. Examining Figure 2.1b,  $h(t)$  for  $C_1 = \text{Violent}$  tends to be the highest except for the first 1-2 years. One could suspect that those with  $C_1 = \text{Violent}$  were disproportionately incarcerated and thus their risk of rearrest in the initial years is underestimated.

Among those who were sentenced, about 16% were sentenced to incarceration. Approximately 74% of the incarceration sentences are jail sentences with the maximum length = 1 year and the median = 60 days. Thus, a long period of incarceration after the first arrest is not likely for the current 1980 NY data. The rest (26%) of the incarceration sentences are indeterminate state prison sentences. Table 2.1 displays the frequencies of sentence types by  $C_1$  among those who were convicted. It is true that those who were arrested for violent crimes were more likely to be sentenced to incarceration (jail, prison) than those who were arrested for the other two crime types.

Table 2.3. Frequency of sentence types by  $C_1$  (row percentages in brackets)

$C_1$	Sentence				
	Jail*	Prison	Probation	Fine	Others**
Violent	276 (13.8)	242 (12.1)	468 (23.4)	502 (25.1)	512 (25.6)
Property	563 (11.6)	35 (.72)	1,043 (21.5)	1,893 (39.1)	1,311 (27.1)
Drugs	198 (13.1)	116 (7.7)	304 (20.2)	399 (26.4)	492 (32.6)

\* “Jail” includes sentences of jail alone and combined sentences of jail and probation.

\*\* “Others” contains mostly combinations of non-incarceration sentences.

Although our NY rap-sheet data contain information on incarceration sentences (type and length), since the data are from the police and courts, not from the correctional department, there is no

information regarding the dates of admission to, and release from, correctional facilities. This data limitation restricts our capability to fully account for the incarceration in our analysis.

In order to explore the possible effect of incarceration on hazard estimates, the hazard for  $A_1 = 19-20$ ,  $C_1 = \text{Violent}$  is estimated for those who were sentenced to incarceration, and it is compared with the hazard of all convicted individuals with the same  $A_1$  and  $C_1$ . The hazard for those who were sentenced to prison (“Prison” in Figure 2.2a) is lower for the first 3 years, suggesting the possibility that they were not at risk of rearrest during that time because they were incarcerated. On the other hand, those who were sentenced to jail (“Jail”) have a higher hazard. This could be due to a combination of two factors. First, unlike those who were sentenced to prison, the length of incarceration for those with a jail sentence might have been minimal. Second, they may have been given a jail sentence because they were judged to have a higher risk of recidivism.<sup>20</sup>

Since those who were sentenced to prison constitute a small fraction (12%) of the convictees, it is important to examine the potential magnitude of the incarceration effect on the hazard. The hazard ( $A_1 = 19-20$ ,  $C_1 = \text{Violent}$ ) is estimated after excluding those who were sentenced to incarceration. Figure 2.2b shows that if only those who were sentenced to prison are excluded (“No prison”), the hazard increases only very slightly, suggesting that accounting for prison incarceration would not change the hazard estimate by a meaningful amount. If those who were sentenced to jail are excluded (“No jail”), the hazard decreases somewhat, pointing to a possibility that the length of the jail sentence may have been short, and that a jail sentence was given to the particularly recidivism-prone population. The figure provides some evidence that

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<sup>20</sup> The hazards for those who were sentenced to prison (“Prison”) and those who were sentenced to jail (“Jail”) are more similar for the older first-time convictees ( $A_1 = 25-30$ ) than for the younger convictees ( $A_1 = 19-20$ ). Unlike the patterns we observe in Figure 2.2a for  $A_1 = 19-20$ , the “Prison” hazard for  $A_1 = 25-30$  is higher than the hazard for all convictees from the beginning.

without accounting for the length of prison incarceration for violent offenders, their hazard may be underestimated, but the magnitude of such bias seems small.

Figure 2.2a. Comparison of hazards by incarceration type ( $A_1 = 19-20$ ,  $C_1 = \text{Violent}$ )

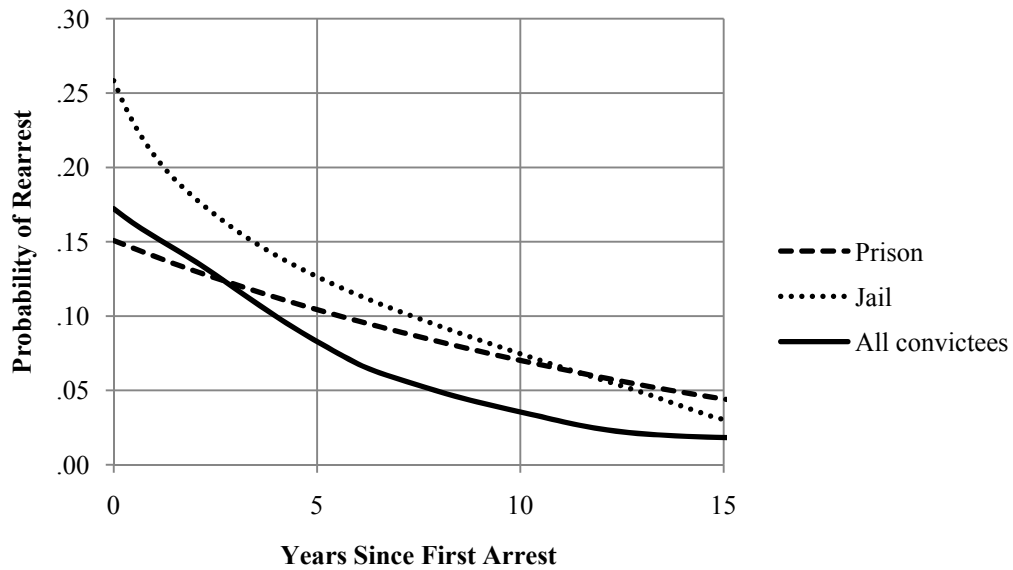
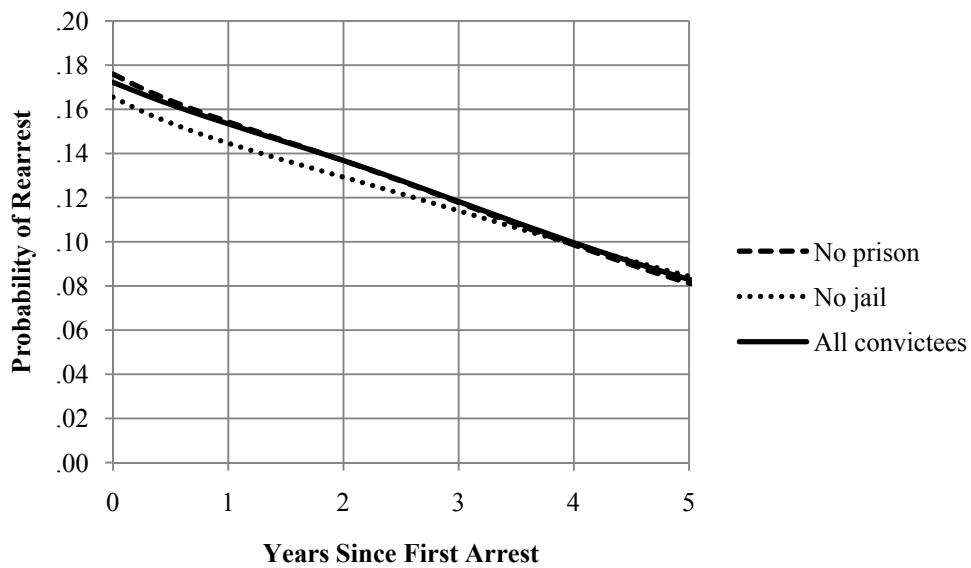


Figure 2.2b. Comparison of hazards by whether individuals were sentenced to incarceration or not ( $A_1 = 19-20$ ,  $C_1 = \text{Violent}$ )



## 2.6 $T^*$ : Comparison with the General Population

### 2.6.1: Approach

We are interested in finding  $T^*$ , the value of  $t$  where the risk of a new arrest matches the risk of arrest for the general population of the same age. The risk of arrest for the general population is represented by the age-crime curve whose horizontal axis is age ( $A$ ) and whose vertical axis is the age-specific arrest rate of people of age  $A$ , the ratio of the number of arrests of age  $A$  to the population of age  $A$ .

The value of the age-crime curve in year  $t$  after the first arrest of persons of  $A_1$  in 1980 is given by the number of arrests of people of age  $(A_1 + t)$  divided by the population of that age in 1980.<sup>21</sup> The sample cohort is from New York, so the age-crime curve as a comparison is also from New York. The number of arrests by age in New York is from the Uniform Crime Reports (Federal Bureau of Investigation, 1981–2001; National Consortium on Violence Research, April 10, 2008), and the population of New York State is from the census (U.S. Census Bureau, 1996, 2000, 2007).

### *Redundant-arrest correction for age-crime curves*

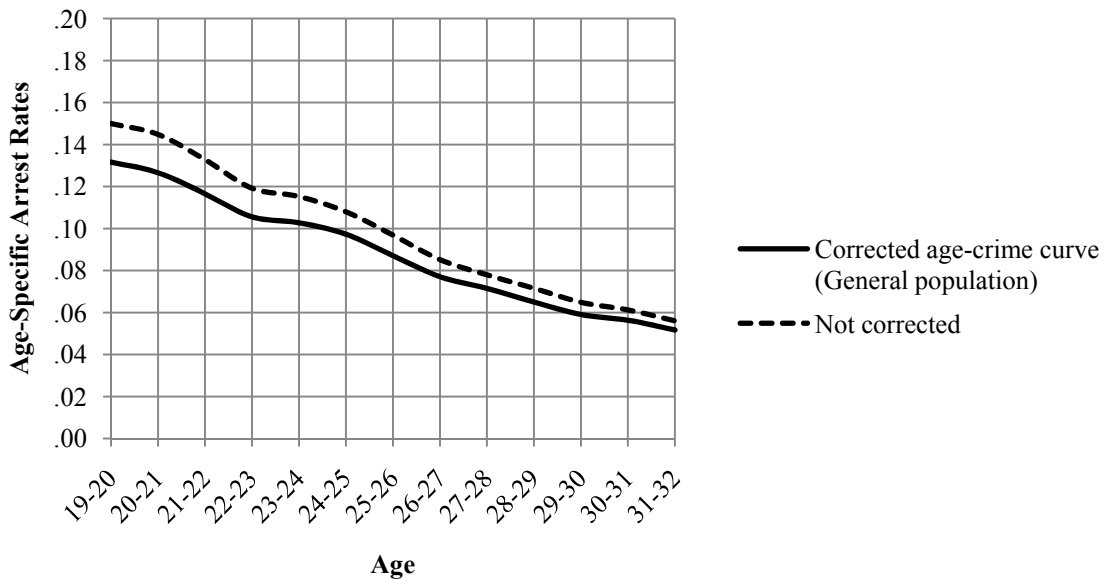
The number of arrests reported in the Uniform Crime Reports is greater than the number of individuals arrested because an individual can have multiple arrests in a year. As a result, the

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<sup>21</sup> An alternative to the conventional age-crime curve is a *progressive* age-crime curve where the age-specific arrest probability for those who were of age  $A_1$  in 1980 is calculated from the number of arrests and the population of age  $A_1$  in 1980, the number of arrests and the population of age  $(A_1 + 1)$  in 1981, those of age  $(A_1 + 2)$  in 1982, and so on. As a result of the way this progressive age-crime curve is constructed, it takes into account the period effect. Since the late 1980s through the early 1990s experienced a significant increase in the rate of violent crimes as well as drug crimes, the progressive age-crime curve for the 1980 cohort is relatively flat and does not show the monotonic decline with age, which is seen in conventional age-crime curves. As a result, the estimates of redemption times based on the progressive age-crime curve would be shorter than the estimates presented in this chapter.

age-crime curve that is based on the number of arrests is an overestimate of the probability of arrest for a member of the general population. In order to adjust for these redundant arrests, we first calculate the ratio of the number of arrestees to the number of arrests as a function of  $A_1$  in 1980, from the data of the 1980 NY arrestee cohort. We then estimate the number of arrestees by multiplying the  $A_1$ -specific ratio by the number of arrests from the UCR. In general, the ratio is smaller for younger ages (for example, for  $A_1 = 16$ , the ratio = .80, while for  $A_1 = 40$ , the ratio = .97), which is consistent with the fact that younger ages are associated with higher hazards and higher offending frequency. Figure 2.3 shows the corrected and uncorrected 1980 age-crime curves for  $A_1 = 19-20$ . By accounting for the redundant arrests, the correction lowers the age-crime curve by 8-13%. Here, we count arrests for any offense (except of course DUI) so that the range of offenses for which an arrest can be made for the general population is comparable with the range of offenses considered for a new arrest for redemption candidates.

Figure 2.3. Age-crime curve corrected for redundant arrests

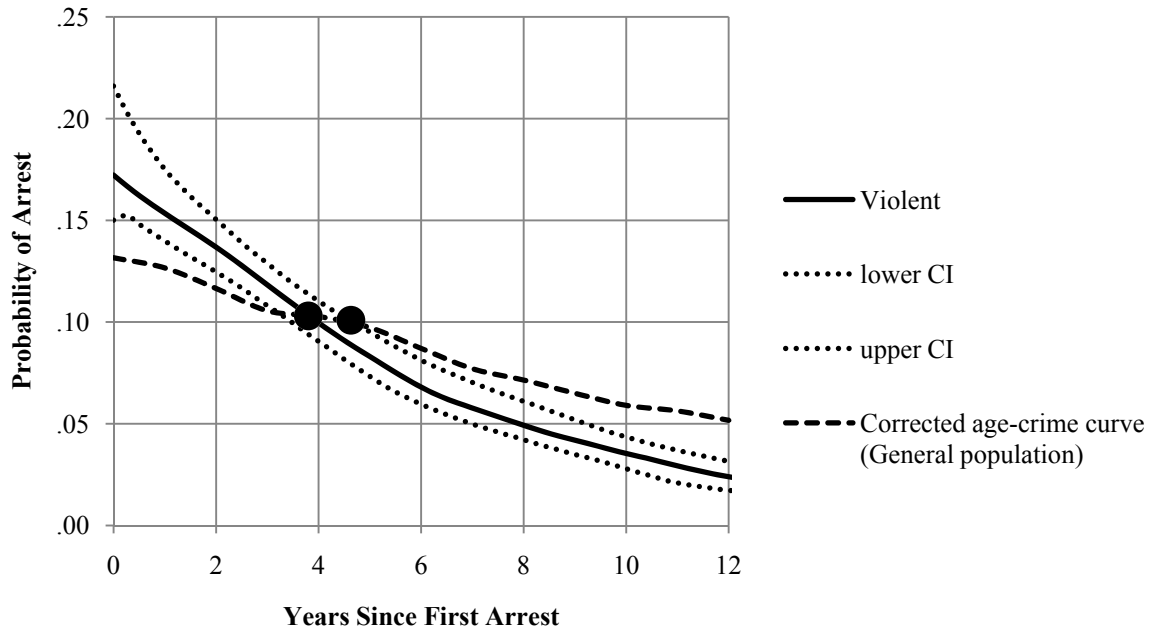


The hazard curve is expected to cross the age-crime curve at  $T^*$  years, because the age-crime curve includes among the larger population, those who recently offended and thus have a reasonably high risk of reoffending, as well as those who were never arrested. In contrast, the redemption candidates have been arrest-free for  $T^*$  years, during which time the risk, or hazard rate, should have fallen substantially and should have a reasonably steep negative slope.

### **2.6.2 Results**

Figure 2.4 shows the hazard curve for  $C_1 = \text{Violent}$ ,  $A_1 = 19-20$  along with the 95% confidence intervals, the corresponding corrected age-crime curve, and the resulting intersections,  $T^*$  (one with the hazard estimate and the other with the upper bound of the confidence intervals), which are represented by filled circles. The confidence intervals are based on the method of bootstrap, with the number of bootstrap samples,  $B = 2001$  (more detailed discussion about invoking the bootstrap method can be found in Appendix). The  $T^*$  values that are calculated based on the upper confidence bound provides more conservative estimates of redemption times.

Figure 2.4. Hazard  $h(t)$ : Age 19-20, Violent with confidence intervals, compared with the General Population



Tables 2.4a-2.4b show the values of  $T^*$  by offense type at first arrest ( $C_1 =$  Violent, Property, and Drugs) and age at first arrest ( $A_1 = 19-20$  and  $25-30$ ).<sup>22</sup> Although younger offenders have higher hazards (Fig. 2.1a), those who were arrested at younger ages are associated with somewhat smaller values of redemption times for those who were arrested for violent offenses in 1980: about 4 years for 19-20 year olds and about 5 years for 25-30 year olds. This difference from our earlier observations that redemption times are longer for younger  $A_1$ 's can be explained by the fact that the age-crime curve for the older offenders ( $A_1 = 25-30$ ) is much lower than the age-crime curve for the younger offenders ( $A_1 = 19-20$ ). Thus, although younger offenders have higher hazards than older offenders, it is also the case that their benchmark (the age-crime curve) is higher and this difference results in shorter estimated redemption times.

<sup>22</sup> The values of  $T^*$  are calculated as the intersection of the age-crime curve and the smoothed hazard by linear interpolation.

This phenomenon is illustrated in Figure 2.4. Younger offenders, those with  $A_1 = 19-20$ , reach their redemption time ( $T^*_y$ ), after staying clean for 4.8 years; this point is represented by the filled circle in the figure. Older offenders, those with  $A_1 = 25-30$ , reach their redemption time ( $T^*_o$ ) after staying clean for 7.9 years; this point is represented by the empty circle in the figure. It is important to recognize, however, that the resulting probability of arrest at the estimated redemption time is appreciably lower for the older  $A_1$  compared to the younger ones (about .05 compared to about .10).

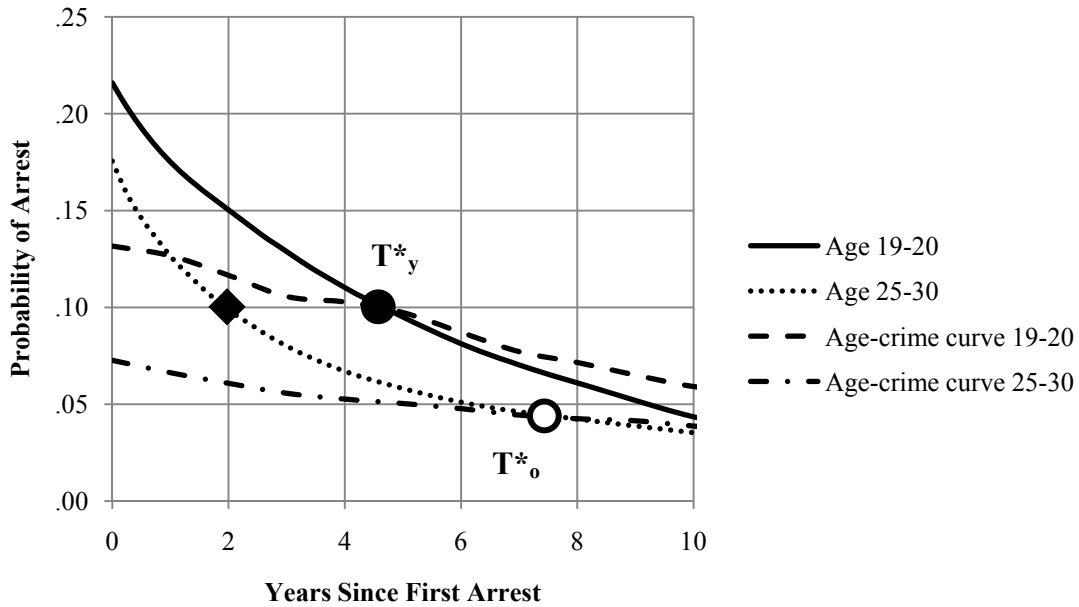
Table 2.4a. Values of  $T^*$  by  $C_1$  and  $A_1$  (arrest probability at  $T^*$  in brackets)

First Offense	Age at First Arrest	
	19-20	25-30
Violent	3.8 (.103)	4.7 (.051)
Property	2.2 (.114)	4.0 (.053)
Drugs	2.1 (.116)	4.5 (.051)

Table 2.4b. Values of  $T^*$  using the upper CI by  $C_1$  and  $A_1$  (arrest probability at  $T^*$  in brackets) in 1980

First Offense	Age at First Arrest	
	19-20	25-30
Violent	4.8 (.099)	7.9 (.043)
Property	2.6 (.110)	4.5 (.052)
Drugs	3.4 (.104)	6.1 (.047)

Figure 2.5. Hazard  $h(t)$  (the upper CI): Age 19-20, 25-30, Violent, compared with age-crime curves



It could be useful to consider these redemption times in the context of the age distribution of the job applicant pool. If the applicant pool consists of individuals whose initial conviction occurred at similar ages (homogenous in terms of  $A_1$ ), then the intersection between the hazard of those with a prior record and the age-crime curve of the corresponding age, provides a meaningful time point such that the risk of applicants who stayed clean up to that time should be considered sufficiently low compared to their counterparts in a similar applicant pool drawn from the general population. Thus, the redemption time  $T^*_y$  would be useful in selecting applicants with an acceptable risk if most applicants' conviction record is from when they were 19 or 20, and similarly the redemption time  $T^*_o$  would be useful if most applicants' conviction record is from when they were between 25 and 30.

On the other hand, if the applicant pool consists of individuals whose initial conviction occurred at different ages (heterogeneous in terms of  $A_1$ ), then their  $A_1$  as well as the length of their time clean should both enter the employer's consideration. Since we noted earlier, and as

shown in Figure 2.4, the probability of rearrest at the estimated redemption time is appreciably lower for the older  $A_1$  compared to the younger ones (about .05 compared to about .10). If one wanted to focus on a particular probability of a new arrest (say, .10), then older offenders decline to that same level before younger offenders, as shown in Figure 2.5 by a diamond shape. Thus, if an employer is willing to accept the risk associated with 19-20 year olds who stayed clean for 4.8 years after their conviction, that same employer can accept that same risk with 25-30 year olds who have stayed clean for only 2 years, if he/she has that choice from a population with heterogeneous  $A_1$ 's. On the other hand, it is also clear from Figure 2.5 that he/she can halve their risk by selecting among the older offenders when they reach their  $T^*_o$  of 7.9 (represented by the open circle).

Within each of the  $A_1$  groups, using the upper confidence interval estimates of  $T^*$ , we find  $T^*$  to be largest for violent crimes, followed by drug crimes, and smallest for property crimes offenders. This is consistent with the rank of the hazard estimates for the three crime types (Fig. 2.1b).

Table 2.5 show the proportion of those who reached  $T^*$  as a function of  $A_1$  and  $C_1$ , which is an estimate of the survival function evaluated at  $T^*$ . For  $A_1 = 19-20$ , 55% remained arrest-free until  $T^*$  for violent offenders and over 70% for property offenders. Those who are arrested for property offenses tend to have a higher likelihood of reaching redemption times than those who are arrested for violent offenses or drug offenses.

Table 2.5. Proportion of those who reached  $T^*$  by  $C_1$  and  $A_1$  in 1980 (using the upper CI)

First Offense	Age at First Arrest	
	19-20	25-30
Violent	.55	.63
Property	.70	.68
Drugs	.67	.65

## 2.7 $T^{**}$ : Comparison with the “Never Arrested”

### 2.7.1 Approach

Our previous analysis estimated  $T^*$  as a point of redemption by comparing people with a prior record who have stayed clean with members of the general population of the same age. In contrast to  $T^*$ , which can be calculated as an intersection of two curves, comparison with the never arrested inherently involves more complex choices. Since the risk of rearrest for a redemption candidate might be expected to approach, but not quite cross, the risk of arrest for the never arrested, it becomes a matter of having to assess when the two curves are “close enough.”

#### *Approximating the Hazard of the Never Arrested*

Information about such individuals is not directly available in any repository-based data set that contains records of only those who have been arrested.<sup>23</sup> One approach to estimating the hazard of the never arrested involves using the 1980 age distribution of New York and the age

<sup>23</sup> Kurlychek, Brame, and Bushway (2006, 2007) pursued this issue using cohort data sets, but such data sets are often too limited for estimating hazard rates for the small fraction of individuals with a prior arrest who remain clean for a reasonable time.

distribution of 1980 first-time arrestees. The population of the never arrested at age  $A$  ( $P_{na}(A)$ ) is approximated as follows:<sup>24</sup>

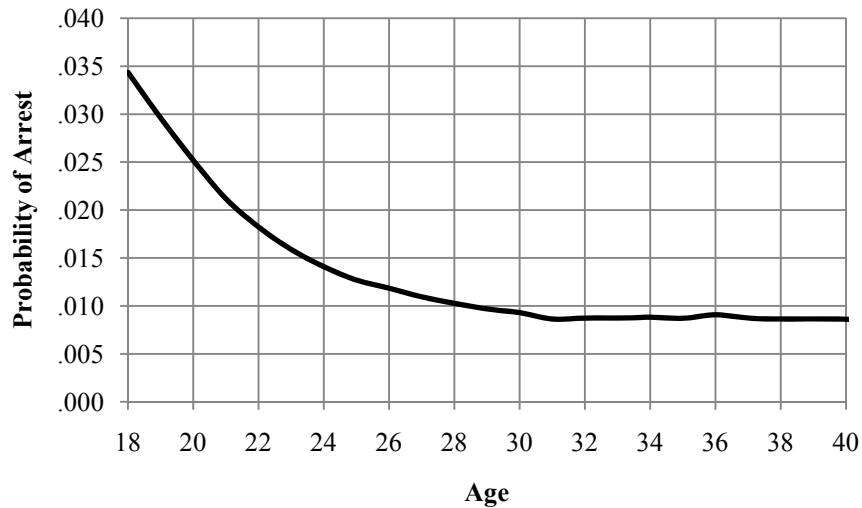
$$P_{na}(A) = \text{Population of NY of age } A \text{ in 1980} - \Sigma(\# \text{ of first-time arrestees in 1980 for all } A_1 < A).$$

As a result, the hazard of the never arrested at age  $A$  ( $h_{na}(A)$ ) is calculated as:

$$h_{na}(A) = \frac{\text{Numbre of first - time arrestees for } A_1 = A}{P_{na}(A)}$$

Figure 2.6 displays our estimate of  $h_{na}(A)$ .<sup>25</sup> It is evident that the younger ages are associated with higher risk of arrest, but even at age 18, the hazard is less than .04, which is clearly much lower than the risk of rearrest of those with a prior arrest. We can now compare the hazard of redemption candidates whose first arrest occurs at age  $A_1$ ,  $h(t)$ , with the hazard of the never arrested,  $h_{na}(t = A - A_1)$ .

Figure 2.6. Hazard of the never arrested,  $h_{na}(A)$



<sup>24</sup> We only consider arrests at adult ages in NY ( $A_1 \geq 16$ ).

<sup>25</sup> Considering the fact that the age-specific arrest rates increased during the period between 1980 and 1990 (see Figure 3.1a in Ch. 3), it is likely that the approximated  $h_{na}(A)$  could be underestimated.

### *Determining “Close Enough”*

We designate as  $T^{**}$  the point when the hazard of an individual with a criminal record,  $h(t)$ , is sufficiently close to that of one without. Figure 2.7 shows  $h(t)$  for  $A_1 = 19-20$  for  $C_1 =$  violent crimes and property crimes, as well as  $h_{na}(t)$ . We first note that  $h(t)$  declines considerably faster than  $h_{na}(t)$ . However, aside from random fluctuations,  $h(t)$  comes very close to  $h_{na}(t)$  but remains above it for a substantial length of time. Given these observations, our question is when the redemption candidate’s risk is deemed “close enough” to that of the never arrested.

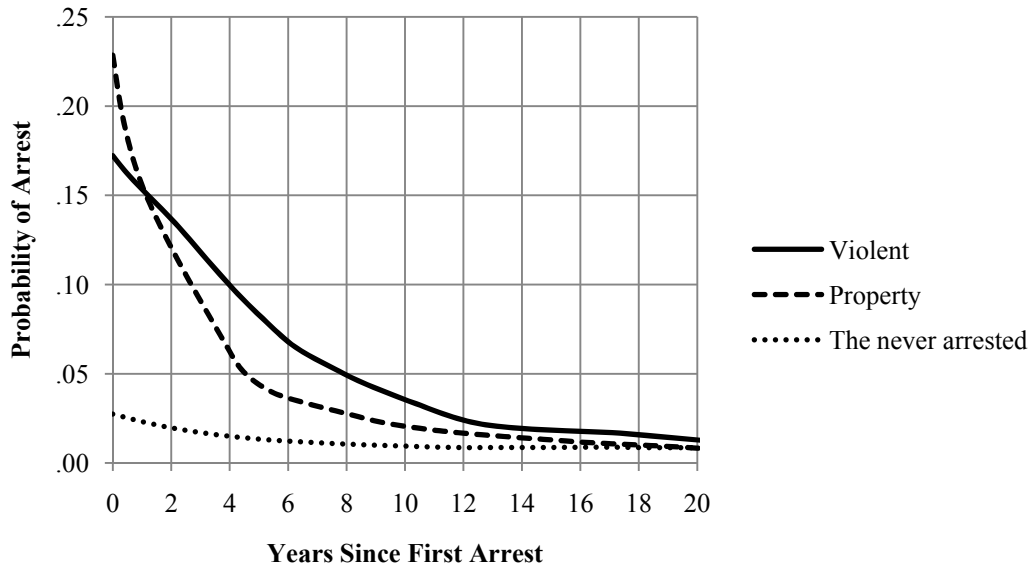
Our approach first invokes the use of  $\delta$ , a risk difference that an employer is willing to tolerate. The parameter  $\delta$  allows us to find a point estimate of a time when  $h(t)$  intersects the tolerable level of risk compared with  $h_{na}(t)$  (i.e., when  $h(t)$  falls below  $(h_{na}(t) + \delta)$ ).<sup>26</sup> Also, we invoke confidence intervals in order to account for the uncertainty in  $h(t)$ . Because as  $t$  increases, the number of those not yet rearrested declines, and so the uncertainty of  $h(t)$  increases as well. Thus, using confidence intervals is particularly appropriate for the estimates of  $T^{**}$ , which tend to be much larger than the  $T^*$  estimates. Using the hazard of the never arrested, we estimate  $T^{**}$  as the lowest value of  $t$  such that the *upper* bound of the confidence interval of  $h(t)$  becomes smaller than or equal to  $(h_{na}(t) + \delta)$  (statistical discussion of this approach can be found in the Appendix). The use of the upper bound makes the values of  $T^{**}$  a conservative estimate of redemption times.<sup>27</sup>

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<sup>26</sup> Alternatively, an employer can formulate the risk tolerance as a risk ratio (or a relative risk) of  $h(t)$  to  $h_{na}(t)$ .

<sup>27</sup> The conservative nature of  $T^{**}$  by the use of CI is aligned with the fact that those who need a stringent standard for redemption would use  $T^{**}$  instead of  $T^*$  for which CI is not used.

Figure 2.7. Comparison with the never arrested (Age 19-20 Violent, Property)



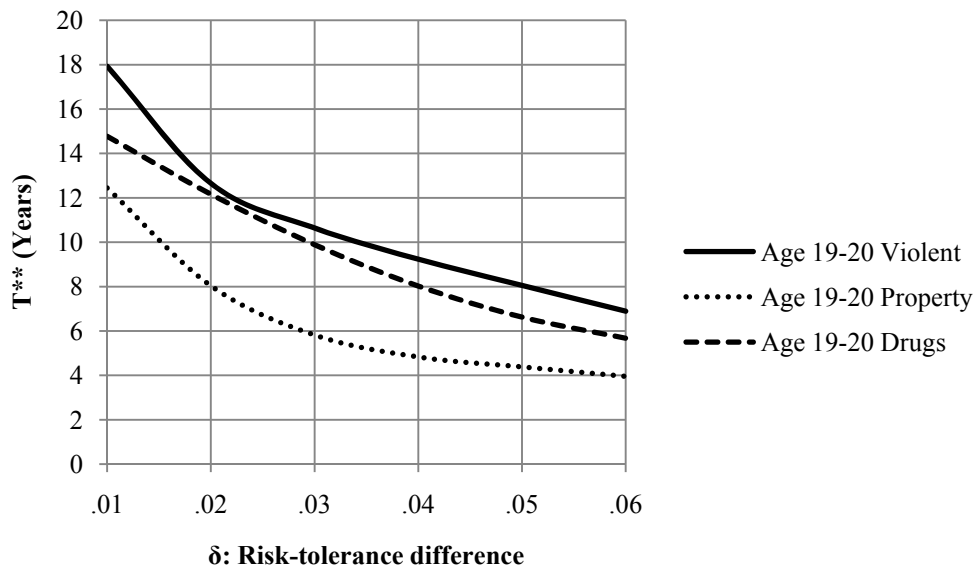
### 2.7.2 Results

Suppose that an employer can accept  $\delta = .03$ , whereby a redemption candidate's hazard can be as much as .03 higher than the hazard of a never-arrested person of the same age. Then we estimate  $T^{**} = 5.8$  for  $C_1 = \text{Property}$  and  $T^{**} = 10.6$  for  $C_1 = \text{Violent}$  (both for  $A_1 = 19-20$ ) using the 95 percent confidence interval. The more tolerant an employer is (larger value of  $\delta$ ), the shorter the redemption time (smaller value of  $T^{**}$ ). Figure 2.8a shows this tradeoff between  $\delta$  and  $T^{**}$  for three different crime types. Violent offenders have consistently higher values of  $T^{**}$  than drug offenders and property offenders, indicating that violent offenders need to stay clean longer for the same risk-tolerance difference. Figure 2.8b demonstrates that a younger  $A_1$  tends to be associated with a longer time necessary for violent offenders to be comparable with the never-arrested of the same age at a given tolerance level  $\delta$ .<sup>28</sup>

<sup>28</sup> Another approach to comparing redemption candidates with the never arrested is to recognize that the comparison need not be of two candidates of the same age. Because the hazard declines with age, younger never-arrested individuals may exist whose hazard is no less than that of an older individual with a prior arrest but who has stayed clean for a long period. It could also be the case that, based on some existing

For the employer who is more accepting of risk and willing to focus on the intersection of  $h(t)$  and  $\delta+h_{na}(t)$ , the values of  $T^{**}$  at the same value of  $\delta$  (.03) and  $A_1$  (19-20) would be 5.1 years for property and 9.3 years for violent compared with 5.8 years for property and 10.6 years for violent using the upper confidence interval approach above. The values of  $T^{**}$  based on the intersection of the hazards are lower than those for the conservative employer who wants high confidence that the candidate represents a low risk.

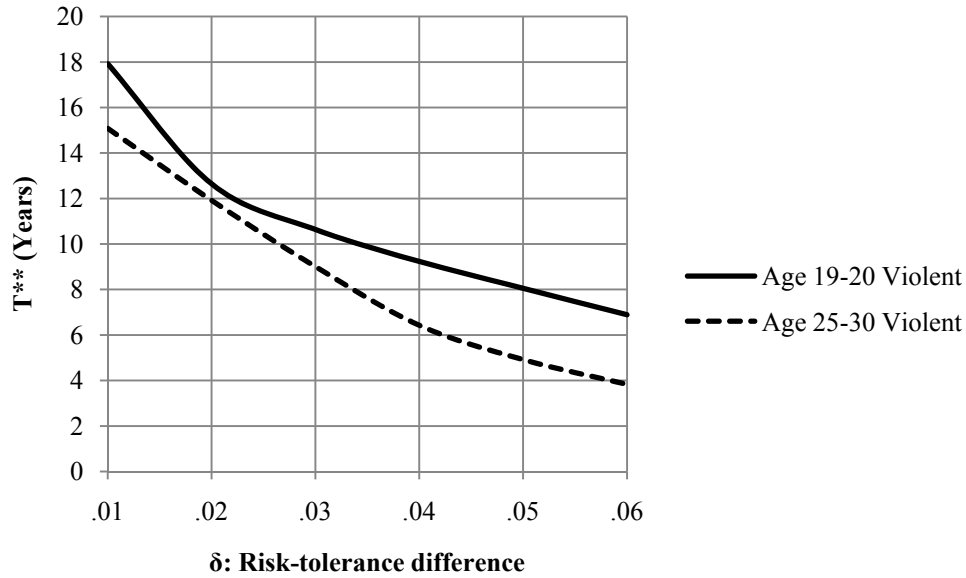
Figure 2.8a. Tradeoff between  $\delta$  and  $T^{**}$  (based on upper confidence interval of  $h(t)$ )




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base rates for workplace deviant behaviors (e.g., Bachman, 1994; Slora, 1989), some employers might have a specific risk level,  $\delta$ , below which the risk is tolerable or acceptable for the purpose at hand (e.g., a particular job position in a particular industry).

Figure 2.8b. Tradeoff between  $\delta$  and  $T^{**}$  (based on upper confidence interval of  $h(t)$ )



## 2.8 Out-Of-State Adjustment

In the previous sections,  $T^*$  was identified as the minimum duration of time clean in New York State for the recidivism probability to drop below the norm for New Yorkers of the same age. Also identified were approaches to estimating  $T^{**}$ , when the recidivism probability falls below any specified level compared to people who have never been arrested. It is possible, however that an individual who stayed clean in New York was arrested in another state. Thus, the estimates are lower bounds on  $T^*$  (and  $T^{**}$ ) and the associated recidivism probability. One study on the recidivism of released prisoners estimated that 7.6 percent of the released prisoners were rearrested out-of-state (Langan and Levin, 2002). Another finds that, among the prisoners who were released from eleven state prisons in 1983, roughly 10 percent of them have out-of-state arrests within three years of their release (Orsagh, 1992). The presence of geographic mobility has also been shown in a study that analyzes the effect of prisons in other states on crimes within

a state (Marvell and Moody, 1998). Geerken (1994) showed that not taking into account the extent of out-of-state arrests would bias the relationship between arrest rates and demographic variables such as age and race.

In order to address this concern about mobility, we approached the FBI, which maintains a national index of rap-sheet records in the Interstate Identification Index (III).<sup>29</sup> We presented them with identification information of the individuals who stayed clean in New York. They then provided information on their arrests elsewhere in the nation. By accounting for the out-of-state arrests, this adjustment raises the  $h(t)$  curve somewhat and so increases the value of  $T^*$  and  $T^{**}$ . The correction could be reasonably large for a state like New York, where the large fraction of offenders from New York City could easily commit offenses in a neighboring state.

### **2.8.1 Data**

We first select a sample of individuals from the original 1980 data and send it to the FBI, after which the FBI informs us of the national criminal-history records of the sampled individuals, which include the crime events that occur in New York as well as those that occur elsewhere.

We sent 2,514 individual identifiers (SID) to New York's Division of Criminal Justice Services. This sample consists of 20% of those who satisfy the following conditions: 1) the NY arrest in 1980 is unsealed, 2) the age at first arrest is between 16 and 26, 3) the 1980 crime type is either violent, property, drug or public order, and 4) there is no subsequent arrest in NY.<sup>30</sup> Out of

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<sup>29</sup> The access to the FBI national criminal records was contingent upon our grant from NIJ.

<sup>30</sup> We initially obtained a sample of national criminal records of about 260 individuals. About 20 percent of the sampled individuals submitted to the FBI have a second arrest after 5 years of their first arrest, according to the New York records. Their FBI records indicated that almost all of them indeed remained arrest free (both inside and outside of New York) until their second arrest in NY. This finding prompted us to request a larger sample of individuals who have no subsequent arrest in NY.

2,514, 1,969 had an FBI Number and were sent to the FBI to retrieve their national criminal records.<sup>31</sup> National criminal histories of 1,949 individuals were returned from the FBI.<sup>32</sup> Out of 1,949 individuals, 22.83% were rearrested after the 1980 arrest. 43 states had at least one first rearrest. The states with the most rearrests are Florida (20%), New Jersey (13%), New York (13%), California (6%), and Connecticut (6%).<sup>33</sup>

### 2.8.2 Approach

Using the national arrest histories from the FBI, the hazard estimate can account for the occurrence of the out-of-state arrests in the following two steps: first, suppose that a variable  $I_{\text{FBI}}$  is an indicator (0/1) of whether an individual, who appears to have no subsequent arrest after the 1980 arrest in the NY records, in fact has a post-1980 arrest. It can be modeled as a Bernoulli variable with the parameter,  $p$  = the probability of having a second arrest that is not recorded in NY (which could occur in NY or outside of NY). The value of  $p$  is given by the proportion of the FBI sample with a second arrest. From the data description above,  $p = .2283$ . Second, given  $I_{\text{FBI}} = 1$  (the second arrest is present), the time to the second arrest is simulated by a Weibull

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<sup>31</sup> Those who did not have an FBI number have presumably never been processed through the FBI system, possibly because the fingerprints, which are associated with criminal records that are submitted to the FBI, are not of sufficient quality to be entered into the system (U.S. Department of Justice, 2006).

<sup>32</sup> 1,697 are sent to us in a PDF format, which were first converted to a text file and then were reformatted in a spreadsheet to be ready for analysis, and 252 were in paper form, which were entered into a spreadsheet manually.

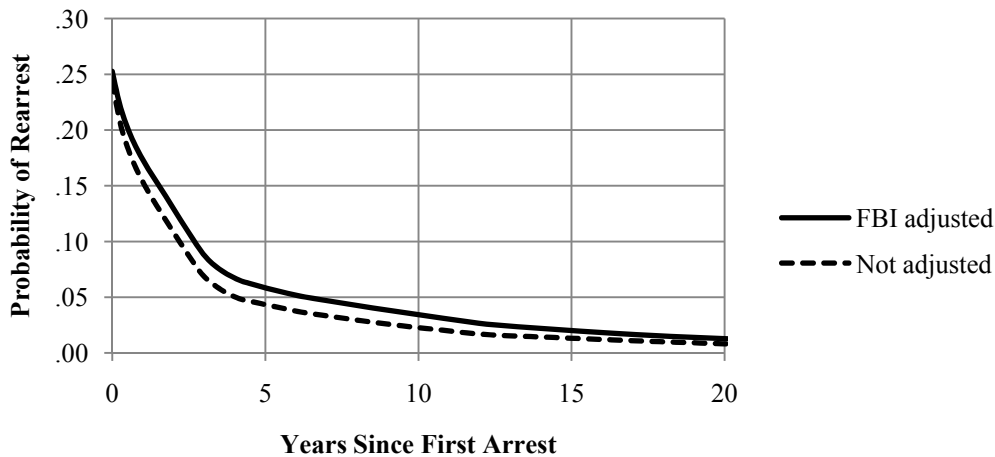
<sup>33</sup> Despite the fact that according to the NY records, the sample of individuals submitted to the FBI have no post-1980 records, the FBI records indicate that a sizable fraction of them do have second arrests in NY. In light of this information, the correction for the out-of-state arrests accounts for the NY arrests that are not included in the NY repository data.

probability density function that approximates the histogram of the time to the second arrest of the FBI sample who have a second arrest.<sup>34</sup>

### 2.8.3 Results

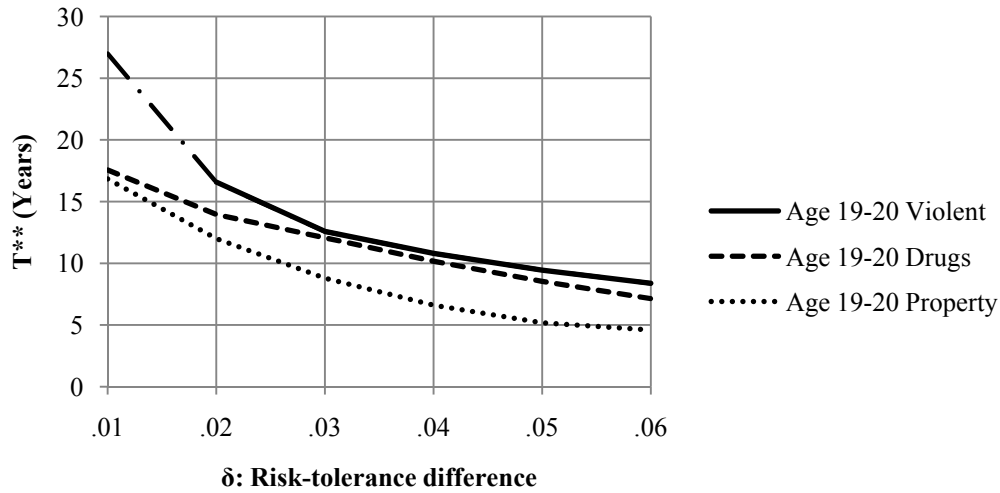
Using the above approach, 22.83% (2,974) of the 13,025 individuals, who appear to have no subsequent arrest after the 1980 arrest in the NY records, are estimated to have an out-of-state arrest. Figure 2.9 shows the hazards for  $A_1 = 19-30$  with and without the adjustment of out-of-state arrests. The ratio of the adjusted hazard to the non-adjusted hazard ranges from 1.04 to 1.69 (or the adjustment increases hazards by 4 to 69 %), generally increasing with the years since the first arrest. With the adjustments, the redemption times ( $T^{**}$ ) are re-estimated and summarized in Figure 2.10. For instance, the redemption time estimate for those with  $A_1 = 19-20$  and  $C_1 =$  Violent increases by about 2 years as a result of the adjustment.

Figure 2.9. Adjustment for the out-of-state arrests ( $A_1 = 19-30, C_1 = \text{Any}$ )



<sup>34</sup> The estimated scale parameter = 2637.795, and the shape parameter = .878282. A truncated Weibull function over the interval  $[0, 26.3]$  was used to simulate the time to the second. This is to accommodate the fact that the follow-up time in the FBI data is longer than the follow-up time in the NY data.

Figure 2.10. Tradeoff between  $\delta$  and  $T^{**}$  after adjusting for out-of-state arrests (based on upper confidence interval of  $h(t)$ )



Since there seems to be an appreciable effect of out-of-state arrests on hazards and redemption time estimates, the adjustment should be applied in estimating redemption times. It should be noted, however, that the effect of out-of-state arrests could be different across states. New York City is geographically very close to major cities in its neighboring states such as Newark, NJ and Philadelphia, PA. Thus, it was expected that taking into account the presence of out-of-state arrests would be important. The prevalence of out-of-state arrests might be lower for states like California where its major cities are relatively far from major cities in its neighboring states.

## 2.9 Mortality Adjustment

The hazard of a new arrest at time  $t$  is a probability of rearrest conditioned on having stayed crime free until  $t$ . The previous section considers the possibility that those who stayed clean in NY were rearrested in some other state. That was one way by which the hazard estimate needs to be adjusted. This section considers the possibility that some of those who appeared to stay clean

in NY were dead so were not at risk of being rearrested. Depending on the extent of mortality, the hazard estimate needs to be adjusted accordingly.

It is relatively well known that offenders have higher rates of mortality than non-offenders (Lattimore, Linster, and MacDonald, 1997; Laub and Vaillant, 2000; Nieuwbeerta and Piquero, 2008). The higher mortality among offenders can be largely due to the fact that characteristics of offenders tend to be the characteristics that can elevate mortality such as impulsive personality, living in disadvantageous neighborhoods, and poor self-care (Gottfredson and Hirschi, 1990; Laub and Vaillant, 2000; Piquero et al., 2005). Empirically, Nieuwbeerta and Piquero (2008) show that convicted criminals in Netherland have a higher risk of death due to natural causes (e.g., diseases) as well as a higher likelihood of death due to unnatural causes such as accidents, suicides and violent victimizations. Similarly, using the longitudinal data from the Glueck's classic study, Laub and Vaillant (2000) found that delinquency is strongly associated with a higher mortality of both natural and unnatural causes.

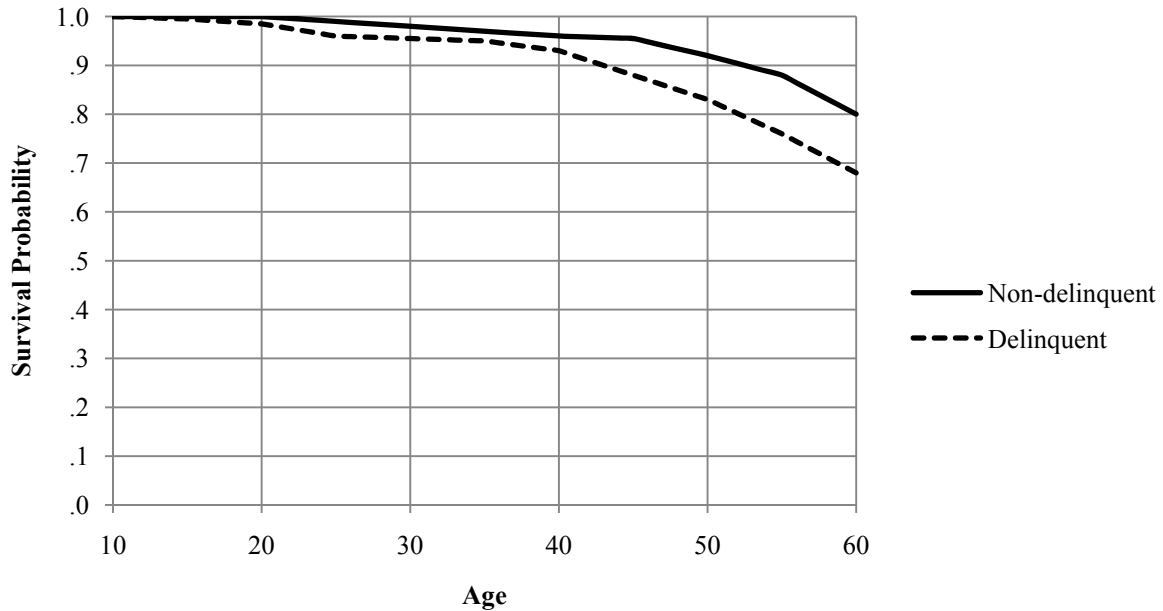
### **2.9.1 Approach**<sup>35</sup>

We first examine how much effect the mortality of the 1980 NY cohort would have on the hazard of a new arrest based on the New York 1979-81 life table (National Center for Health Statistics (NCHS), 1986) and the findings from Laub and Vaillant (2000). Laub and Vaillant (2000) provide survival probabilities (proportion still alive) of delinquent and non-delinquent boys in the 1940's. Those survival probabilities are shown in Figure 2.11.

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<sup>35</sup> It may be possible to directly estimate the mortality rate of the individual 1980 NY arrestees from the Death Master File, which is maintained by the Social Security Administration. However, since the issue of mortality is not central to the objective of this dissertation, we pursue the approach discussed above and use the published results.

Figure 2.11. Survival probabilities of Gluecks's delinquents and non-delinquents (taken from Laub and Vaillant (2000))



Since the mortality has surely improved in the 1980's compared with the 1940's, the mortality of the redemption candidates should reflect this fact. This can be accomplished in the following manner. First, we calculate the scale-up factor that reflects the heightened mortality (or the scale-down factor of the poorer survival) of the delinquents over the non-delinquents by taking the ratio of the survival probability of the delinquents to that of the non-delinquents. We apply the factor to the survival probability calculated from the 1979-81 general population (male) mortality rates that were published by the NCHS (NCHS, 1986) (Figure 2.12). We use the fact that the survival probability is related to the mortality rates by

$$S(t) = \prod_{t \leq t_j} [1 - h(t_j)], \quad j=1, 2, \dots \text{ where } t_1 < t_2 < \dots$$

The mortality rates of the redemption candidates are calculated from the scaled survival probability. Figure 2.13 shows the estimated mortality rates for the redemption candidates, along with the mortality rates of the general population from the NCHS.

Figure 2.12. Survival probabilities of the 1979-81 general population (NCHS) and of the delinquents

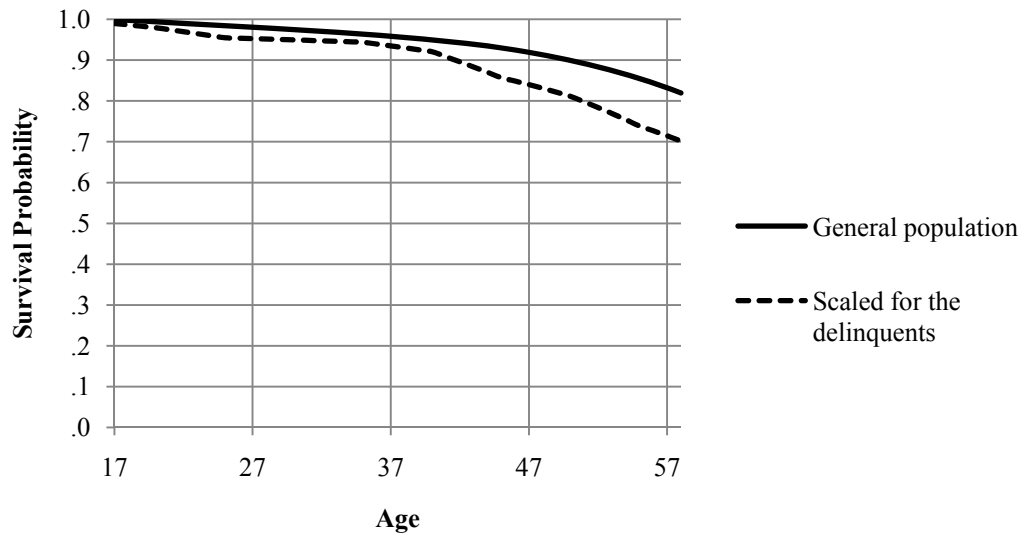
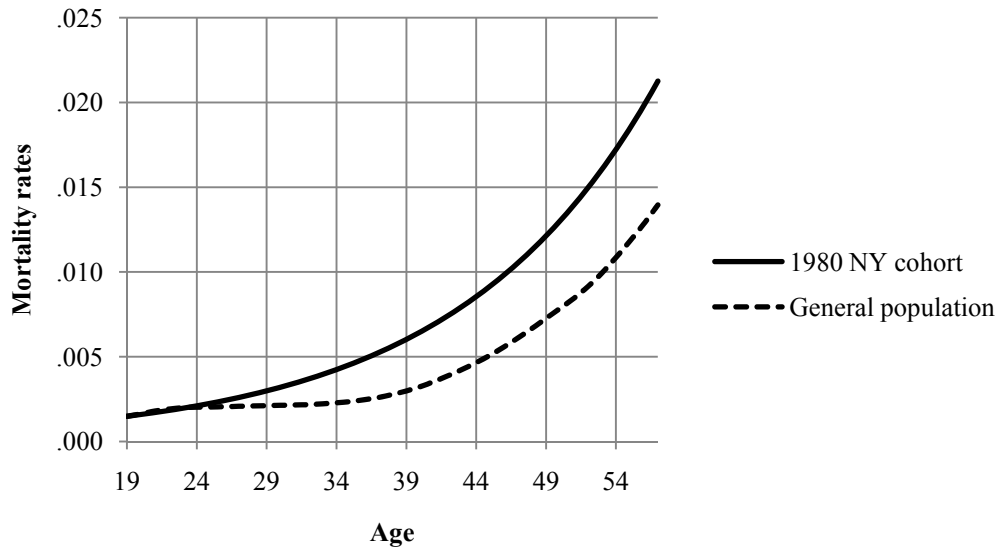


Figure 2.13. Mortality rates of the NY 1980 cohort and the general population



The mortality rates are applied to the redemption candidates who appear to have no second arrest, since those who have a second arrest were alive at least until the time of the second arrest. Suppose  $I_d(A_1, t)$  is an indicator (0/1) of whether a redemption candidate with the age in 1980 =  $A_1$  is dead at time  $t$ , the number of years since 1980. It can be modeled as a Bernoulli variable with the parameter,  $p_{A_1, t}$  = the probability of being dead at time  $t$  for  $A_1$ .  $p_{A_1, t}$  is given by the mortality rates ( $m(A_1, t)$ ) of the redemption candidates calculated above. For example, the indicator of whether a redemption candidate with  $A_1=19$  is dead in the first year,  $I_d(19, 0)$ , is equal to 1 with the probability,  $m(19, 0)$ . If  $I_d(19, 0) = 0$ ,  $I_d(19, 1) = 1$  with the probability of death in the second year,  $m(20, 1)$ . Similarly, this calculation is applied to  $A_1 = 19-30$  for  $t = 0-25$ .

## 2.9.2 Results

Using the approach above, 4.6% of the population is estimated to have died (13.0% of those who appear to have no second arrest). 3.3% of those whose  $A_1 = 19$  and 7.9% of  $A_1 = 30$  are estimated to have died. The hazards for  $A_1 = 19-30$  are shown in Figure 2.14a with the mortality experience of the 1980 NY cohort adjusted. Since the two curves are barely distinguishable, Figure 2.14b expands the y-axis by setting  $\max = .05$ . The adjustment increases the hazard by .1% to 16%, with the larger increases at later times. The difference between the two curves still seems to be practically negligible.<sup>36</sup> A possible reason for this is that mortality only affects the number of those who are at risk of being rearrested (risk set), and it does not change the number of those who are actually rearrested. This differentiates the adjustment for mortality from the adjustment for the out-of-state arrests, since the latter affects both the risk set and the number

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<sup>36</sup> It is true that the increases in the hazard are larger for those with older  $A_1$ 's, but the increases seem sufficiently small.

of rearrests, which could lead to a sizable change in the hazard estimates as seen in the previous section. Based on the analysis above, we are reasonably assured that mortality does not affect either the hazard or the redemption time estimates and thus shall not be incorporated in the subsequent analyses.

Figure 2.14a. Mortality adjustment ( $A_1 = 19-30$ ,  $C_1 = \text{Any}$ )

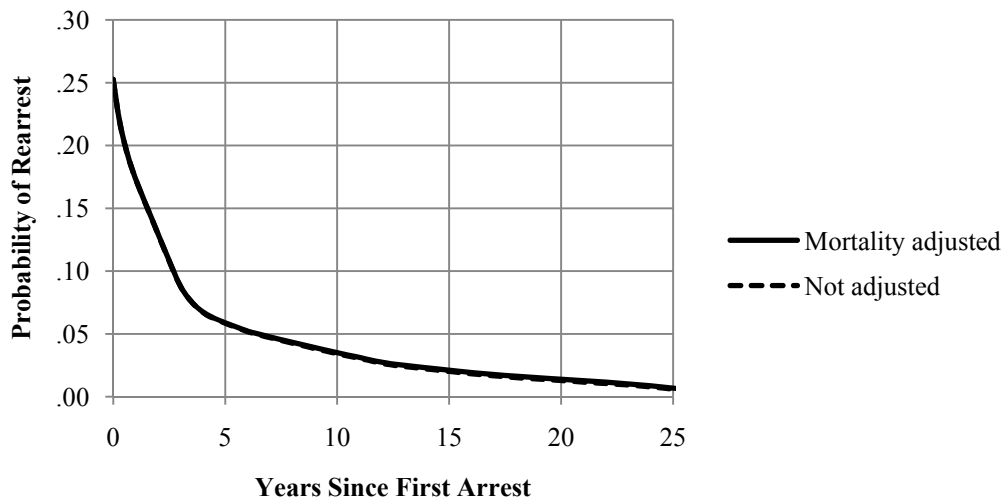
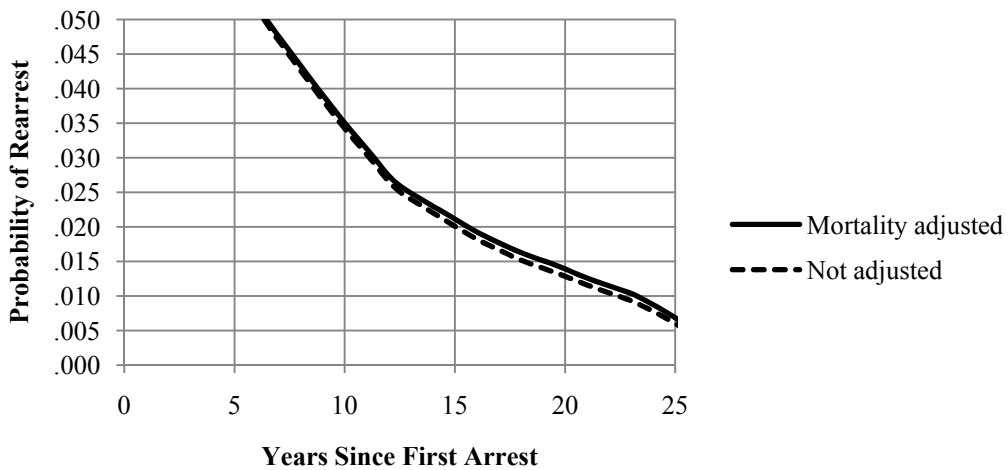


Figure 2.14b. Expanded y-axis ( $A_1 = 19-30$ ,  $C_1 = \text{Any}$ )



## **2.10 Summary**

This chapter discusses the approaches to estimating redemption times: how long it takes for an individual with a prior criminal record and no subsequent criminal involvement to be of no greater risk than appropriate benchmarks. Two benchmarks are discussed: 1) the risk of arrest for the general population, which is represented by the age-crime curve, and 2) the risk of arrest for those who have never been arrested. The risk of rearrest for those with a prior record is represented by hazard. The hazard is estimated using data on the arrest history of the 1980 arrestee cohort in New York. The redemption times were estimated as a function of age at first arrest and the crime type of the first arrest, and both factors seem to be important in understanding the risk of rearrest and the redemption times. The chapter also explored two possible adjustments to the redemption estimates to account for out-of-state arrests and mortality. We conclude that the adjustment for the arrests outside of New York is important, while the correction for mortality seems unnecessary.

## **CHAPTER 3: ROBUSTNESS RE SAMPLING YEARS AND JURISDICTIONS**

### **3.1 Robustness across Sampling Years**

The results in Chapter 2 are based on the analysis of the 1980 NY arrestee cohort, which consists of those who were arrested for the first time in 1980. Considering the dramatic swings in the levels of crime over the following 20 years, there is a possibility that the rearrest risk patterns of offenders first arrested in 1980 were different from those arrested more recently, so it is important that we test the robustness of the findings based on the 1980 NY arrest cohort. To the extent that there is stability in hazard patterns across sampling years, it would then be possible to aggregate the datasets from different years and achieve greater statistical power by estimating the hazard from the aggregated data. Employers must routinely consider applicants with a record of arrest that occurred, not necessarily in 1980, but in other years. The data aggregation is directly motivated by the employers' need for robust, generalized guidance on redemption times. It is also important that, if the hazard patterns are dissimilar across years, the guidance on redemption times should account for those differences.

#### **3.1.1 Changes in Crime Patterns over the Last Three Decades**

The period from the second half of the 1970s to the late 1990s is marked by dramatic changes in the levels of crime. The rate of violent crime started rising in the 1970s's, experienced its first peak around 1980, declined until the mid 1980s, then sharply increased to another peak in the early 1990's, and then dropped dramatically until 2000 (Bureau of Justice Statistics, 2010a). During the same period, the rate of property crime followed a similar pattern as that of violent

crime, but its ups and downs were much less dramatic (Bureau of Justice Statistics, 2010a).<sup>37</sup> The rate of arrests for drug crime has been in general on a steady increase with a sharp spike in the late 1980s (Bureau of Justice Statistics, 2010b).<sup>38</sup>

The rise and fall of the rate of violent crime during the period between the 1970s through the mid 1980s is largely attributed to the fact that the baby boomers entered and left the high crime ages (late teens to early 20s) during the period (Blumstein, Cohen, and Miller, 1980). The rise that started in the mid 1980s is most likely due to crack cocaine and the violence associated with its marketing (Blumstein 1995; Blumstein, Rivara, and Rosenfeld, 2000).

The growth of the crack markets might also be responsible for the simultaneous increase in robbery and the decrease in burglary as drug users who switched from burglary to robbery in need of quick money (Baumer et al. 1998). The striking drop in the second half of the 1990s until 2000s can be a result of many factors including the shift of drug preference among youths, increased incarceration, and changes in policing strategies (Blumstein and Wallman, 2006).

The escalation of the “war on drugs” in the early 1980s dramatically shifted the focus and funding of law enforcement to drug related crimes and introduced stringent laws and policies against drug offenses, exemplified by the Rockefeller drug laws in New York. As a result, the number of arrests for drug offenses almost tripled from 1980 to 1997 (Federal Bureau of Investigation, 1981-98).

In light of the shifts in crime rates over the last two decades, it is crucial to examine how robust our 1980 redemption estimates are against different sampling years. In order to carry out

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<sup>37</sup> The property crime rate experienced a mild peak around 1990, which is driven largely by non-burglary crimes (e.g., larceny). The burglary rate, after its peak around 1980, has been mostly steadily declining.

<sup>38</sup> The peak in the drug arrest rate is due to the heroin/cocaine arrest’s large increase in the 1980s and sharp decline around 1990.

the robustness test, in addition to the data for the 1980 NY arrestee cohort, we obtained similar criminal-history datasets for 1985 and 1990 NY arrestee cohorts.

Since we use rearrest in estimating  $h(t)$  of redemption candidates, it is instructive to examine the age-crime curves (age-specific arrest rates) for the three sampling years. Figure 3.1a depicts the age-crime curves for all offenses for 1980, 1985, 1990 in NY. Figures 3.1b-3.1d show the age-crime curves by crime types (violent, drug, and property offenses). These crime-type-specific age-crime curves are helpful to understand the differences in the prevalence of arrests during the period of 1980-1990. The arrest rate for violence in 1990 is 1.4-2.0 times higher than the arrest rates in 1980 and 1985 at all ages, while the 1980 and 1985 rates are close to each other. The arrest rate for drugs in 1990 is clearly much higher than the arrest rate in 1980, with the ratio of the 1990 rate to 1980 rate increasing with age, from 1.7 at age 16 to 5.7 at age 39. During the teenage years, the arrest rate for drugs in 1985 and 1990 are close to each other; whereas, during the 20's the rate of decrease for the arrest rate is slower in 1990 than in 1985. It is also important to note that the arrest rates for drugs peaks at different ages across three years. While the peak ages for the arrest rate for violence is around 17-18 for the three years, the peak ages for drugs are 18 in 1980, 21 in 1985, and 23 in 1990. As seen in Figure 3.1d, the arrest rate for property crimes in 1990 is on average 1.5 times higher than the arrest rates in 1980 and 1985, which are close to one another. It is clear from the disaggregated age-crime curves, that the difference in the overall age-crime curves across the three years is largely a result of increased arrest rates in violent and drug offenses. These differences indicate that it is very important to take into account of the crime types ( $C_1$ ) when we test robustness of the 1980 findings and examine the possible existence of a period effect in redemption times.

Figure 3.1a. NY Age-Crime Curves for 1980, 1985, and 1990 for all offenses

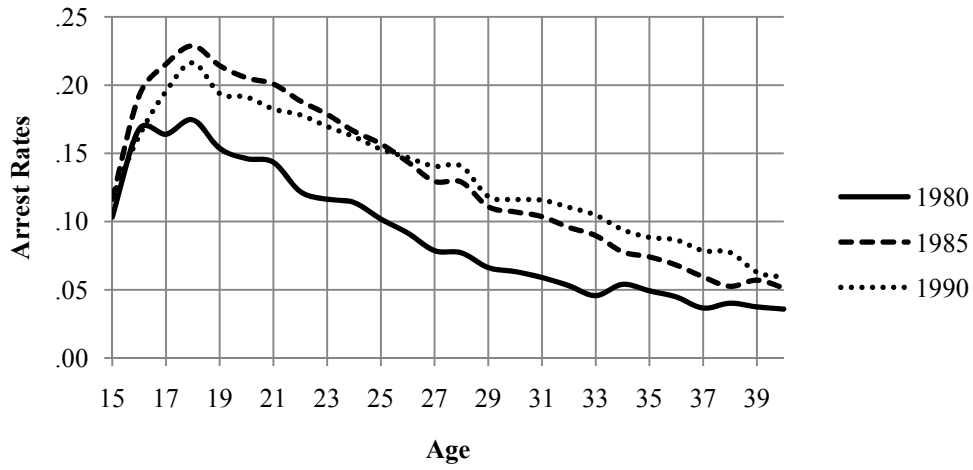


Figure 3.1b. NY Age-Crime Curves for violent offenses, 1980, 1985, and 1990

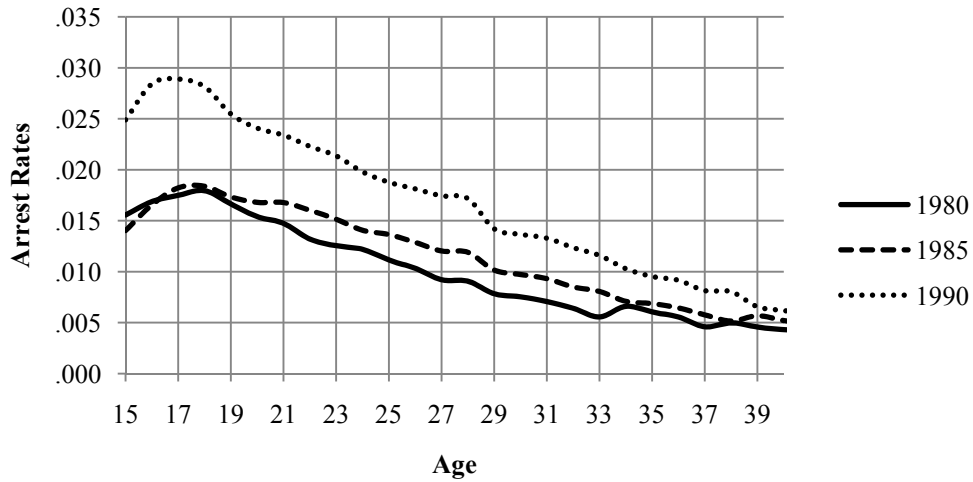


Figure 3.1c. NY Age-Crime Curves for drug offenses, 1980, 1985, and 1990

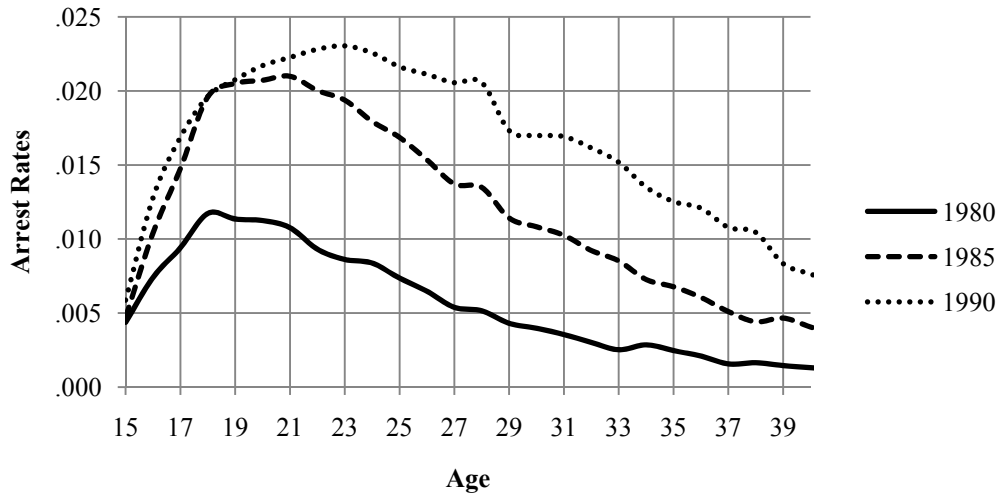
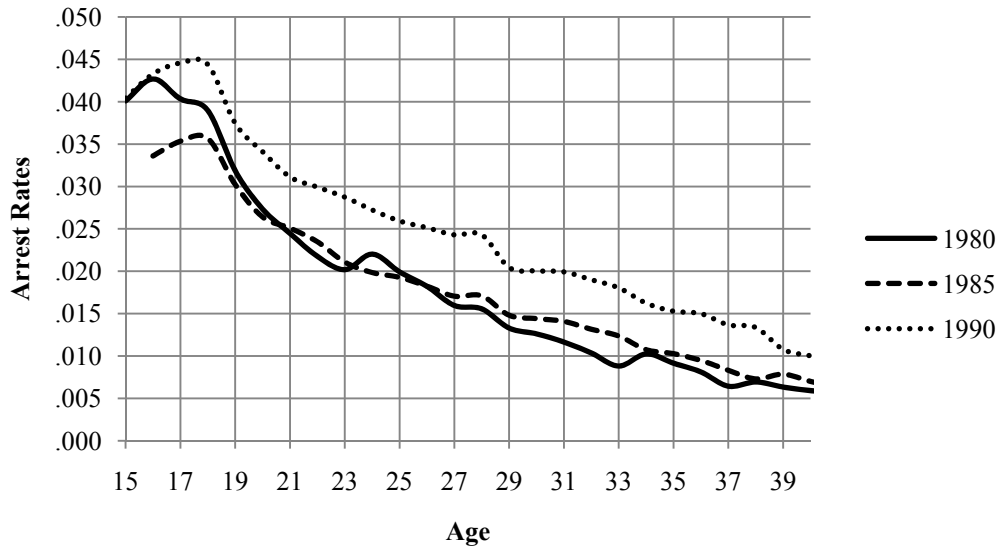


Figure 3.1d. NY Age-Crime Curves for property offenses, 1980, 1985, and 1990



### 3.1.2 Data

The data we used consist of the criminal history of three cohorts of first-time adult arrestees in 1980, 1985, and 1990 in New York State, with approximately 70,000, 63,000, and 65,000

individuals in the three cohorts respectively. Consistent with our analysis of the 1980 data, we focus on individuals whose age at first arrest is between 19 and 30 and who were convicted and whose crime type of arrest ( $C_1$ ) were categorized as violent, property, drug, and public-order crimes, and a remaining group of “others”.<sup>39</sup> Table 3.1 provides for each sampling year the distribution of the sample by age (three groups: 19-20, 21-24, and 25-30) and  $C_1$ . The difference between the total number of individuals for each of the three years in the table and the cohort sizes above is due to the fact that the table displays the distribution of those who were convicted, whose initial arrest record in 1980, 1985, and 1990 respectively is unsealed, and whose age at first arrest is between 19 and 30.<sup>40 41</sup> One can see that there are larger proportions of the convictees who were arrested for drug offenses in more recent years. Also, the convictees tend to be older in more recent years.<sup>42</sup>

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<sup>39</sup> Although we show the hazard estimates for only violent, property, and drugs, in a regression based analysis (i.e., Cox regression), all five categories are used.

<sup>40</sup> The reason to focus on the 19-30 age range is that the arrestees whose ages are between 16 and 18 are considered “youthful offenders” in NY and their criminal records are often sealed. The examination of national records from the FBI indicates that a number of those with older  $A_1$ 's (especially over 30) often had an adult arrest record prior to 1980 in NY, a recording anomaly that would disqualify them as “first-time arrestees” in 1980. In order to minimize these problems while retaining a large enough sample size for the precision in the estimation of hazards, we focus here more narrowly on those with  $A_1$  in the 19-30 range.

<sup>41</sup> The percentages of those convicted are 60% in 1980, 65% in 1985, and 71% in 1990. Eighteen percent of the 1980 arrest records, 24% of the 1985 arrest records, and 25% of the 1990 arrest records are sealed. Those with  $A_1 = 19-30$  constitute 43% of the 1980 arrestee cohort, 50% of the 1985 arrestee cohort, and 51% of the 1990 arrestee cohort.

<sup>42</sup> The arrest offenses are not necessarily the same as the conviction offenses. The conviction offenses are generally not available in the New York rap-sheet data.

Table 3.1 Initial sample size of those who were convicted, by age at first arrest ( $A_1$ ) and first arrest offense ( $C_1$ ) in 1980, 1985, and 1990 in NY (marginal % in brackets)

Year	$A_1$	Violent	Property	Drugs	Pub Ord	Others	Total
1980	19,20	971	2,510	546	824	522	5,373 (33.7)
	21-24	1,066	2,558	729	904	641	5,898 (37.0)
	25-30	871	1,945	627	716	518	4,677 (29.3)
	Total	2,908 (18.2)	7,013 (44.0)	1,902 (11.9)	2,444 (15.3)	1,681 (10.5)	15,948
1985	19,20	887	1,814	761	728	430	4,620 (27.6)
	21-24	1,154	2,090	1,390	1,097	620	6,351 (38.0)
	25-30	957	1,919	1,379	926	571	5,752 (34.4)
	Total	2,998 (17.9)	5,823 (34.8)	3,530 (21.2)	2,751 (16.5)	1,621 (9.7)	16,723
1990	19,20	931	1,820	1,089	745	423	5,008 (27.3)
	21-24	1,108	2,072	1,858	948	604	6,590 (36.0)
	25-30	1,058	1,923	2,266	874	608	6,729 (36.7)
	Total	3,097 (16.9)	5,815 (31.7)	5,213 (28.4)	2,567 (14.0)	1,635 (8.9)	18,327

### 3.1.3 Approaches and Results

#### *Approach 1: Comparison of hazard estimates across the three sampling years*

Estimating empirical hazards of a new arrest across the three sampling years and visually examining them allow us to understand the overall patterns of the hazards and to identify any important similarities and differences at different values of  $t$ .

Figure 3.2a shows the hazards for  $A_1 = 19-30$  who were convicted in 1980, 1985, and 1990 in NY. During the first year or two, the 1990 and 1985 hazards are higher than 1980, reflecting the higher arrest rates seen in Fig. 3.1a. They are still reasonably close to one another, especially after about 6.5 years. Figure 3.2b depicts the same hazards in logarithm, which allows

us to better observe the hazard differences at larger values of  $t$ . It shows more clearly the convergence after about 6.5 years, and it also shows some divergence after about 8.5 years, which we will investigate more closely next by looking at  $C_1$ -specific hazards. Nevertheless, the simple plots of the hazards suggest that the overall patterns of recidivism after the first few years are reasonably similar across the sampling years, especially when the redemption is most relevant.

Figure 3.2a. Hazards for the convicted across three sampling years (1980, 85, 90) in NY,  $A_1 = 19-30$

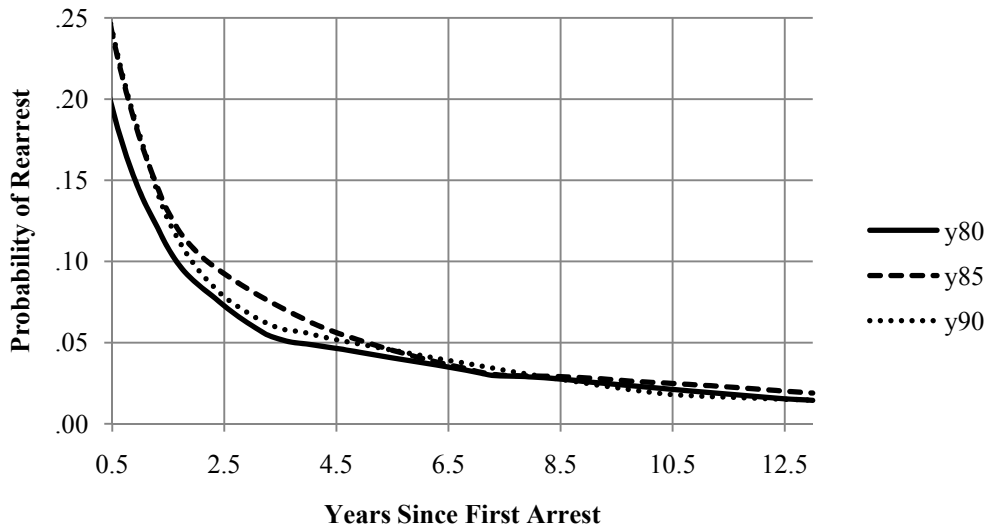
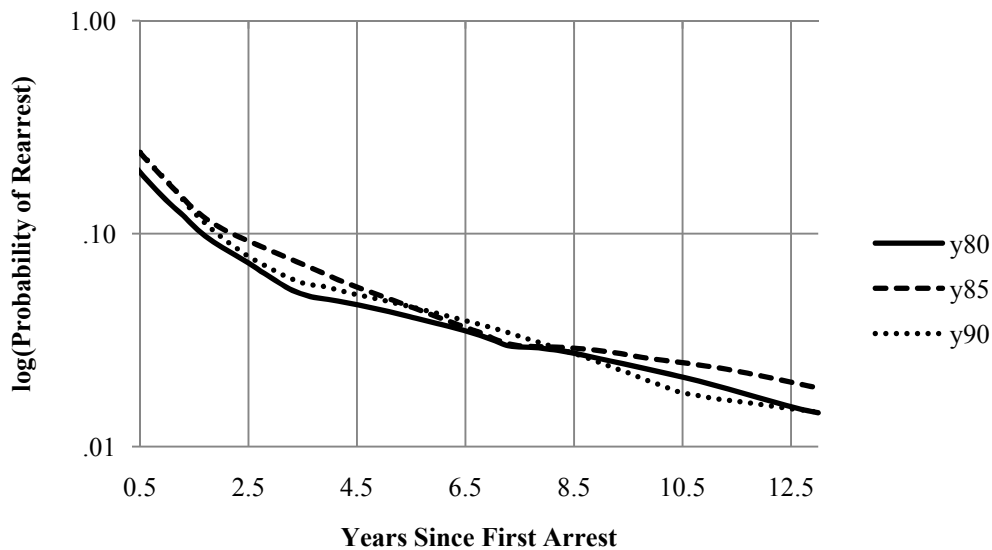


Figure 3.2b. Logarithm of hazards for the convicted across three sampling years (1980, 85, 90) in NY,  $A_1 = 19-30$



Figures 3.3a-3.3c show the hazards for the three sampling years for  $A_1 = 19-20$  for each of the three crime-type groupings,  $C_1 = \text{Violent, Property, and Drugs}$ .<sup>43</sup> Initially, for each of the three crime types, the 1980 hazard is consistently lower than the hazards for 1985 and 1990.<sup>44</sup> For  $C_1 = \text{Violent}$ , the three hazards cross at about  $t = 6.5$ . For  $C_1 = \text{Property}$ , the 1980 and 1990 hazards seem to follow one another closely after  $t = 2$ , while the 1985 hazard seems to be consistently higher than the other two.

For  $C_1 = \text{Drugs}$  (Fig. 3.3c), after the three hazards cross at about  $t = 5$ , the 1985 hazard goes below the other two until  $t = 10$  and then goes up rather steeply, surpassing the 1980 and

<sup>43</sup> The differences across the sampling years are larger once  $A_1$  and  $C_1$  are disaggregated, possibly in part because the disaggregated hazard estimates might be noisier with smaller samples that are used for the estimation (for example,  $n = 15,948$  was used for the estimation of the 1980 hazard for  $A_1 = 19-30$ ,  $C_1 = \text{Any offense}$  in Figure 15, and  $n = 971$  was used for the estimation of the 1980 hazard for  $A_1 = 19-20$ ,  $C_1 = \text{Violent}$ ).

<sup>44</sup> For  $C_1 = \text{Drugs}$ , the highest hazard (1990) is about 1.9 times higher than the lowest (1980), while for  $C_1 = \text{Property}$ , the highest (1985) is 1.3 times higher than the lowest (1980), which are quite consistent with the difference observed in the crime-type-specific age-crime curves for the three years. Thus, the early differences in the redemption candidates' hazards across the three sampling years reflect the differences in the prevalence of arrests in the three years.

1990 hazards. This abrupt increase in the 1985 hazard for drugs basically explains the fact that the 1985 aggregated hazard seems to depart from the other two in Fig. 3.2a-3.2b. It is possible that this is simply a data artifact that could be explained by the stochastic nature of the hazard. However, the trend of the drug arrest rates (Figure 3.4 from UCR arrest data) might provide an explanation for the seemingly anomalous pattern. Fig. 3.4 shows that after the peak in the late 80s (crack cocaine), the drug arrest rate experienced a gradual increase, mostly due to the increased arrests for marijuana. The drug arrest rate's peak in the late 80s, the trough around the early 90s, and the following increase could possibly have pushed the 1985 hazard upward.

Figure 3.3a. Hazards for the convicted across three sampling years (1980, 85, 90) in NY,  $A_1 = 19-20$ ,  $C_1 = \text{Violent}$

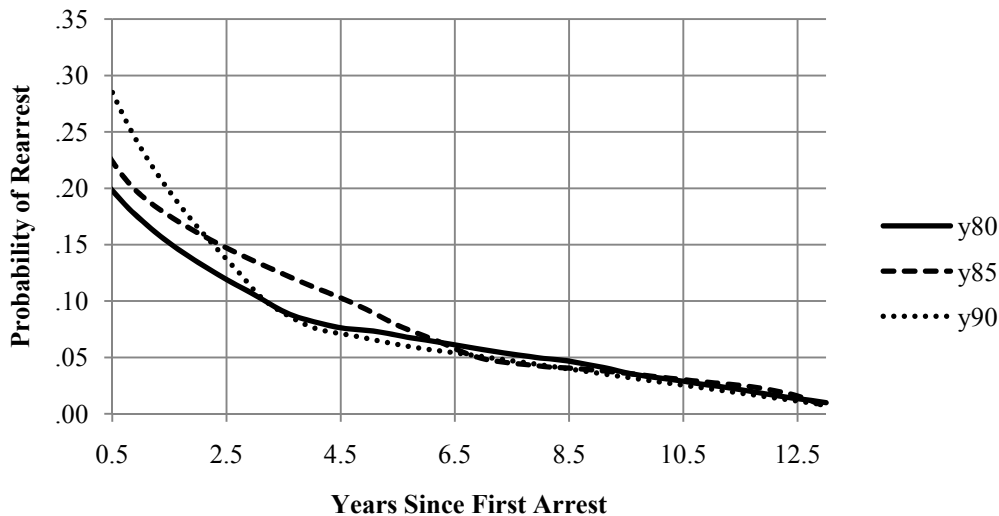


Figure 3.3b. Hazards for the convicted across three sampling years (1980, 85, 90) in NY,  $A_1 = 19-20$ ,  $C_1 = \text{Property}$

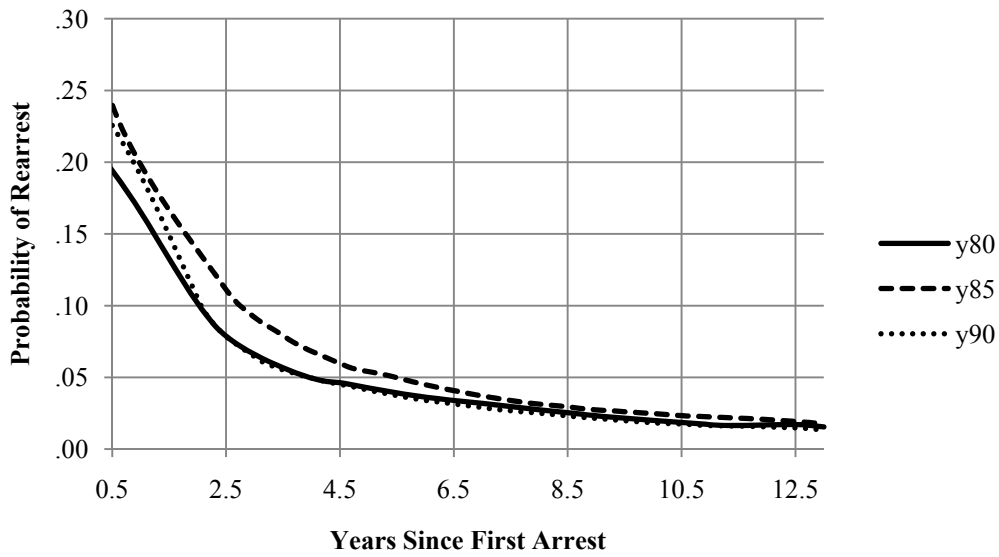


Figure 3.3c. Hazards for the convicted across three sampling years (1980, 85, 90) in NY,  $A_1 = 19-20$ ,  $C_1 = \text{Drugs}$

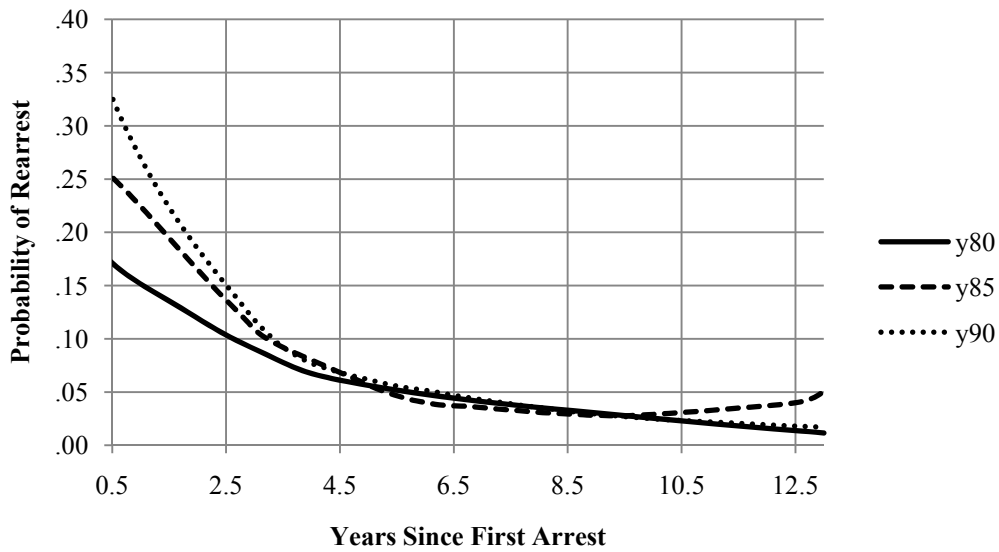
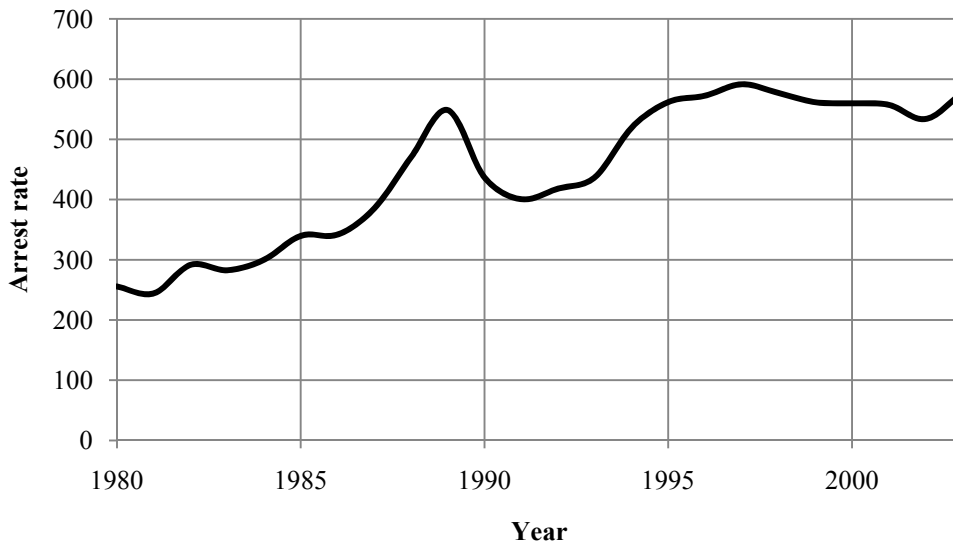


Figure 3.4. Drug arrest rates (per 100,000 population), 1980-2003



The hazard estimates are visually very close, especially after  $t = 6.5$ , but it would be desirable to introduce further statistical procedures to appreciate the degree to which they differ as a result of statistical variation and to generate more precise estimates of their proximity, and the regions where they are close and where they are different. To address this, we introduce log-rank tests and the estimation of time-varying effects of sampling years based on Cox regression models.

***Approach 2: Log-rank tests*** <sup>45</sup>

There are several statistical tests in which the proximity of hazards across the three sampling years can be examined. The first is to test a hypothesis that the hazards of a new arrest that are generated from the three years, are equal against the alternative that at least one of them is different for some  $t$ :

$$H_0 : h_{1980}(t) = h_{1985}(t) = h_{1990}(t) \text{ for all } t$$

$$H_1 : \text{at least one of the } h(t)\text{'s is different for some } t$$

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<sup>45</sup> Much of this section is drawn from Klein and Moeschberger (2005).

To the extent that the hazards are statistically indistinguishable, the redemption is robust against different sampling years.

The construction of a test statistic is as follows. Let  $t_1 < t_2 < \dots < t_D$  be the distinct rearrest times in the sample pooled over the three years. At time  $t_i$  we observe  $d_{ij}$  rearrests in the  $j$ -th year out of  $Y_{ij}$  individuals at risk of a new arrest, where  $j=1980, 1985, 1990$ . Let  $d_i = \sum_j d_{ij}$  and  $Y_i = \sum_j Y_{ij}$  be the number of rearrests and the number at risk in the combined sample at time  $t_i$ ,  $i=1, \dots, D$ . The test of  $H_0$  is based on the weighted comparisons of the estimated hazard of the  $j$ -th population under the null and alternative hypotheses. Under the null hypothesis, an estimator of the expected hazard is the pooled sample estimator of the hazard  $d_i / Y_i$ . Using only data from the  $j$ -th sample, the estimator of the hazard is  $d_{ij} / Y_{ij}$ . Let  $W_j(t)$  be a positive weight function. The test of  $H_0$  is based on the statistic:

$$Z_j = \sum_{i=1}^D W_j(t_i) \left\{ \frac{d_{ij}}{Y_{ij}} - \frac{d_i}{Y_i} \right\}, j = 1980, 1985, 1990.$$

Most commonly,  $W_j(t_i) = Y_{ij} W(t_i)$ . With this weight functions

$$Z_j = \sum_{i=1}^D W(t_i) \left[ d_{ij} - Y_{ij} \left( \frac{d_i}{Y_i} \right) \right].$$

The variance of  $Z_j$  is given by

$$\hat{\sigma}_{jj} = \sum_i W(t_i)^2 \frac{Y_{ij}}{Y_i} \left( 1 - \frac{Y_{ij}}{Y_i} \right) \left( \frac{Y_i - d_i}{Y_i - 1} \right) d_i$$

and the covariance of  $Z_j$  and  $Z_g$  is

$$\hat{\sigma}_{jg} = - \sum_i W(t_i)^2 \frac{Y_{ij}}{Y_i} \frac{Y_{ig}}{Y_i} \left( \frac{Y_i - d_i}{Y_i - 1} \right) d_i, g \neq j.$$

Test statistic:  $\chi^2 = (Z_{1985}, Z_{1990}) \hat{\Sigma}^{-1} {}_{2 \times 2} (Z_{1985}, Z_{1990})^t$  has a chi-squared distribution with  $df=2$  under  $H_0$  ( $\hat{\Sigma}^{-1} {}_{2 \times 2}$  is the estimated variance-covariance matrix). A most common weight function is  $W(t)=1$  for all  $t$ , which leads to the log-rank test. Other choices of the weight function correspond to different test statistics. They include Gehan's (Gehan-Wilcoxon), Tarone-Ware, Peto-Peto (Peto-Prentice), modified Peto-Peto, and Fleming-Harrington. The Log-rank test gives weight uniformly across time, while other weight functions apply weight to either earlier or later times. Most notably, Fleming-Harrington test (Fleming and Harrington, 1981) has two parameters  $p$  and  $q$  that can vary to determine where the most weight is applied. The Fleming-Harrington weight function is give by

$$W_{p,q}(t_i) = \hat{S}(t_{i-1})^p [1 - \hat{S}(t_{i-1})]^q$$

where  $\hat{S}(t)$  is the estimated survival function. Because we are most interested in detecting potential differences at later times where redemption is most relevant, the Fleming-Harrington test may be the most statistically powerful one. In order to take into account  $A_1$  and  $C_1$ , the above tests are carried out by stratifying them by these two factors.

The results of the (weighted) log-rank tests suggest that at least one of the hazards from the three sampling years is statistically different. In order to explore the possible differences in hazards between the pairs of the sampling years, pairwise tests [(1980, 1985), (1980, 1990), (1985, 1990)] were performed. The results indicate that the 1985 hazard is significantly different from the other two years, while the 1980 and 1990 hazards are found to be similar when more weight is being given to the later failure times for the Fleming-Harrington test ( $p = 0, q = 2$ ).

***Approach 3: Time-varying effects of sampling years based on Cox models***

Although the above tests can provide a general sense of whether the hazards from different sampling years are distinguishable over the entire follow up, it is not clear whether any period effect (and cohort effect) changes over time. Moreover, if the effect of sampling years diminishes, it is of most interest to know *when* the effect practically disappears.

One way to examine visually the possibly diminishing effect of sampling year is to use smoothed scaled Schoenfeld residuals (Schoenfeld, 1982) from Cox's proportional hazards model (Cox, 1972). For simplicity, let us consider a Cox model with a single covariate  $x$ :

$$h(t | x) = h_0(t) \exp(\beta x).$$

The fundamental assumption of the Cox model is that the hazard ratio of two individuals is constant *in time*, thus the hazard rates are proportional. If we look at two individuals with covariate values  $x_1$  and  $x_2$ , the ratio of their hazards is

$$\text{hazard ratio} = \frac{h(t | x = x_1)}{h(t | x = x_2)} = \frac{h_0(t) \exp(\beta x_1)}{h_0(t) \exp(\beta x_2)} = \exp[\beta(x_1 - x_2)].$$

In the case a of binary covariate (i.e.,  $x_1 = 1$  and  $x_2 = 0$ ), the hazard ratio is  $\exp(\beta)$ . Thus, the hazard ratio,  $h(t | x = 1) / h(t | x = 0)$ , can be estimated by exponentiating the parameter estimate from the Cox regression,  $\hat{\beta}$ .

Since we are interested in examining the possibility that the effect of sampling years could vary with time clean, we include interactions between sampling year dummies (y85, y90) and time in the Cox regression model (Klein and Moeschberger, 2005). In order to control for  $A_1$  and  $C_1$ , we fit the Cox model stratified by  $A_1$  and  $C_1$ .<sup>46</sup> The hazard ratio estimates with confidence intervals can be plotted against time to examine whether the effect of sampling year changes as arrest-free time increases. The redemption times are deemed robust across sampling

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<sup>46</sup> We use 1980 (i.e., y80) as the reference year.

years if the hazards converge (or the hazard ratio becomes unity) in around the redemption times.<sup>47</sup>

Figures 3.5a-3.5b show the estimated hazard ratios,  $(y_{85}/y_{80})$  and  $(y_{90}/y_{80})$ , with confidence intervals using the Cox model with the interactions between the sampling year dummies and time (2-year time intervals). In Fig. 3.5a, the 1985 hazard gradually approaches the 1980 hazard, and remains relatively close to it (i.e., the hazard ratio remains close to 1.0). Although between  $t = 10$  and 14, the 1985 hazard estimate goes above the 1980 hazard, which we observe also in Fig. 3.2b, the confidence intervals indicate that the ratio of 1985-1980 hazard during  $t = 10-14$  is only marginally different from unity. The 99% confidence intervals mostly contain unity after  $t = 6$ , which provides evidence of convergence between the 1985 and 1980 hazards. In Fig. 3.5b, the 1990 hazard quickly falls toward that of 1980, and stays quite close. The 1990 hazard point estimate is lower than the 1980 hazard for the later years, but the confidence intervals contain unity. Thus, once the uncertainty of the hazard is taken into account, the difference we observe between the 1990 and 1980 hazards in Fig. 3.2b is statistically not significant.

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<sup>47</sup> We determine whether the hazards from different sampling years are deemed robust by examining how often the confidence intervals of the hazard ratio contain unity. In statistical inference terms, this is equivalent to setting a null hypothesis that the hazards are the same. We retain the null hypothesis if the data do not provide sufficient evidence to reject it. This logic applies to the robustness test of hazards across states in the following section.

Figure 3.5a. Hazard ratio estimates (y85/y80) with confidence intervals

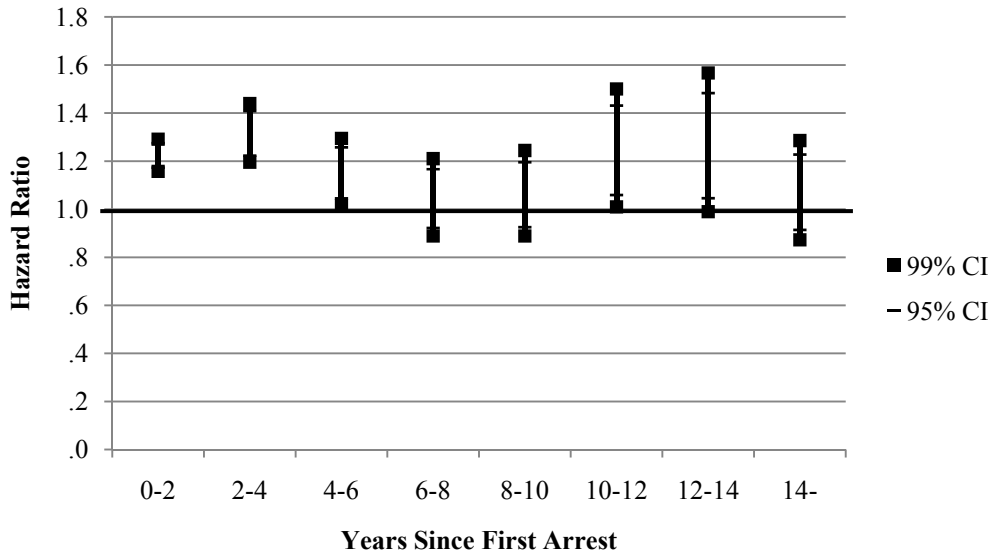
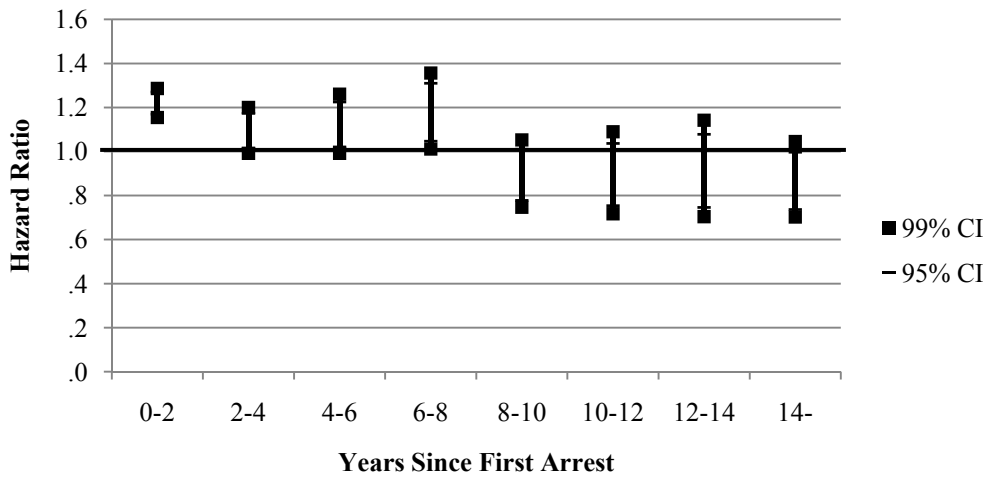


Figure 3.5b. Hazard ratio estimates (y90/y80) with confidence intervals



### 3.1.4 Robustness of Redemption Times

In addition to testing the robustness of hazard patterns, it is important to examine how robust the estimates of redemption times are against variation in sampling years. The estimation of

redemption times requires benchmarks, which determine when the hazards are sufficiently low so that a person with a prior criminal record is considered redeemed. In Ch. 2, the choice of benchmarks was relatively straightforward since the redemption candidates have their first arrest in the same year (1980). Determining appropriate benchmarks for redemption candidates who have their first arrests in different years involves more complex choices.

One approach is to use age-crime curves from years that correspond to the years of redemption candidates' first arrests. Taking this approach, redemption times are estimated at time points when the 1980 redemption candidate's hazard crosses the 1980 age-crime curve, and the 1985 redemption candidate's hazard crosses the 1985 age-crime curve, and so forth. Another approach is to use more general age-crime curves such as the average age-crime curve over the sampling years (80, 85, and 90). With this approach, redemption times are estimated at time points when the hazards from different sampling years cross the general age-crime curve. Yet another approach is to set a risk threshold in terms of probability of arrest, say .1, which is the probability of arrest at  $T^*$  for the 1980 cohort, discussed in Ch. 2, or .03, which is the benchmark probability of arrest for the never arrested when  $\delta = .02$ , and then to estimate redemption times when the hazards fall below the threshold. This last approach has the virtue of the benchmark being not directly influenced by period effects, and we have shown some evidence that the hazards from different sampling years are reasonably close to one another, whereas the benchmarks (age-crime curves) from different years could be very different (see Fig. 3.1a). Table 3.2 shows the redemption time estimates for  $A_1 = 19-20$ ,  $C_1 = \text{Violent, Property, and Drugs}$  using the threshold of .1 and .03. Within about 1.5 years from one another, the hazards from three different sampling years fall below .1. Similarly, within 2 years from one another, the hazards from the three years fall below .03.

Table 3.2 also reports the average and standard deviation of redemption time estimates for each of the  $C_1$ 's. The small standard deviations (consistently 1 or less) highlight the relative similarity of the estimates across the three years, and given the similarity, we are reasonably confident that redemption would be reached in NY at about the average values, regardless of which of the three years the prior crime was committed.

Table 3.2. Values of  $T^*$  for  $A_1 = 19-20$ ,  $C_1 = \text{Violent, Property, Drugs}$  by sampling years, using the upper CI with the thresholds of .1 and .03

$C_1$	Year	Thresholds (probability of a new arrest)	
		.1	.03
Violent	1980	4.66	12.33
	1985	5.00	12.40
	1990	3.73	10.78
Average		4.5	11.8
Std. Dev.		.7	.9
Property	1980	2.94	8.16
	1985	3.14	9.70
	1990	2.44	7.67
Average		2.8	8.5
Std. Dev.		.4	1.1
Drugs	1980	3.67	11.87
	1985	3.90	9.49*
	1990	3.81	10.33
Average		3.8	11.1
Std. Dev.		.1	1.1

\* Although the hazard for  $C_1 = \text{Drugs}$  for the 1980 cohort does not fall below .03 because it begins to increase around  $t = 10$ , at  $t = 9.49$  the hazard reaches its lowest value .036, which is sufficiently close to .03.

## 3.2 Robustness across States

We can perform similar robustness tests with data from different states. There is a possibility that conditions in New York, from which our 1980 data came, are different from other states. It is likely that various factors that may affect arrest rates such as policing policies and labor market conditions differ from one state to another, and so it is desirable that we test the robustness across states of the hazard patterns and of the estimates of  $T^*$  and  $T^{**}$  findings presented in Ch. 2. To the extent that we find similar patterns, that would be very encouraging in terms of the generalizability of our results.

### 3.2.1 Data

We have received and processed rap-sheet data that are similar to the NY data from two additional states, Florida (FL) and Illinois (IL). The main features of the data from the two states are the same as the NY data; they both contain information about the arrests such as dates and crime types of the arrests and the demographic information about the arrestees such as the date of birth, gender, and race. Our comparison focuses on conviction events for 1980 arrest cohorts aged 19 to 30 in the three states. The distribution of dispositions in the three states is shown in Table 3.3. The fact that the percentage convicted varies considerably across the states suggests a possibility that the disposition (court) processes and thus the characteristics of the convicted in the three states could be different.<sup>48</sup> In order to assure that the difference or lack thereof among

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<sup>48</sup> The ways in which court dispositions are categorized differ across states. That could contribute to the different proportions of arrestees who were convicted. In addition to the fact that employers are usually allowed to consider only conviction records, the rationale for focusing our attention on those who were convicted lies in our efforts to make the three states comparable in terms of the extent to which the data contain the recidivism events. In Florida, those who are convicted are not eligible for sealing of their criminal records (the conviction record and the record of any subsequent arrest/conviction); thus, we should be able to capture any subsequent arrests of the convictees. In Illinois, we were told that sealing in

the states is not completely driven by differences in the disposition process, the across-state comparison will be based on the convicted as well as those who were arrested, who include the convicted, the non-convicted, and those with unknown dispositions.

Table 3.3. Dispositions in NY, FL, and IL in 1980 (for  $A_1 = 19-30$ )

	Disposition	Frequency (%)
NY	Conviction	15,948 (59.48%)
	Non-conviction	6,266 (23.37%)
	Unknown disposition	4,600 (17.16%)
	Total number of arrestees	26,814
FL	Conviction	13,812 (26.53%)
	Non-conviction	23,411 (44.96%)
	Unknown disposition	14,843 (28.51%)
	Total number of arrestees	52,066
IL	Conviction	8,537 (19.10%)
	Non-conviction	23,098 (51.67%)
	Unknown disposition	13,065 (29.23%)
	Total number of arrestees	44,700

### 3.2.2 Approach and Results

The approach will be similar to the ones discussed in the examination of robustness across sampling years. We first compare the hazard estimates across the three states and investigate further whether the hazard ratio of different states becomes statistically indistinguishable from

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the face of conviction is very unlikely, and the situation is similar in New York. Thus, these divergent policies regarding sealing encouraged us to focus specifically on those who were convicted in 1980.

unity using the estimates of the interactions between the dummy variables for the states (FL and IL) and time from Cox regression models.<sup>49</sup>

***Approach 1: Comparison of hazard estimates across the three sampling years***

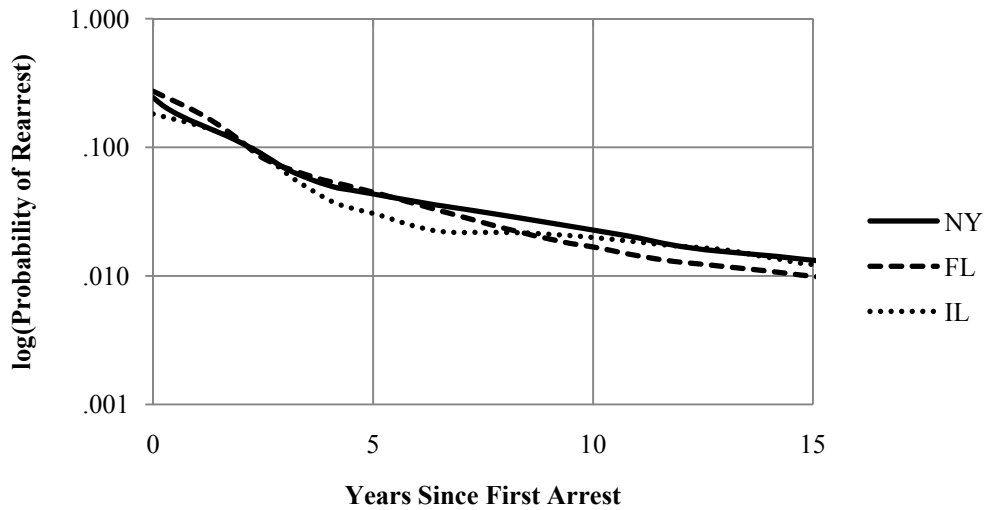
Figure 3.6a presents the hazards for the three states. It is clear that the FL cohort have a higher initial recidivism risk, but that all three converge very quickly so that the hazards at about  $t = 2.5$  are almost the same. Then the hazard for IL drops somewhat below the other two for  $t$  about 4-8 and the three seem to be very close after  $t > 8$ . The log-transformed hazards in Figure 3.6b show more clearly that the IL hazard stays below the NY and FL hazards for about 5 years in the middle and that after about  $t = 8$  years, the FL hazard is lower than the other two.

Figure 3.6a.  $h(t)$  of NY, FL, and IL for those convicted in 1980 ( $A_1 = 19-30$ )



<sup>49</sup> NY is the reference state.

Figure 3.6b. Logarithm of  $h(t)$  of NY, FL, and IL for those convicted in 1980 ( $A_1 = 19-30$ )



Figures 3.7a-3.7b compare the hazards and log-transformed hazards for those who were arrested in each of the three states. They show that the NY and FL arrestee cohorts are very similar, while the hazard for the IL arrestee cohort is lower than the other two states until about  $t = 10$ . In order to develop better estimates of their proximity, we also examine the hazard ratios using the interaction terms between state dummies and time.

Figure 3.7a.  $h(t)$  of NY, FL, and IL for those who were arrested in 1980 ( $A_1 = 19-30$ )

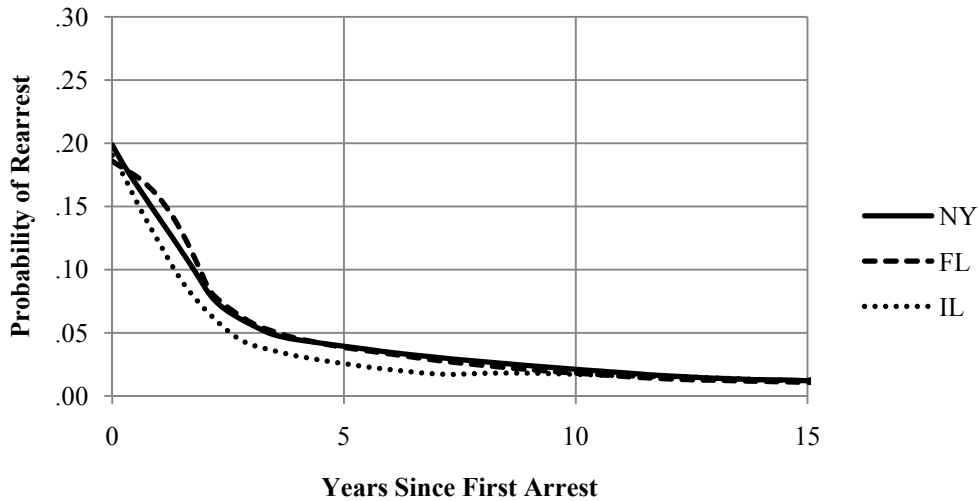


Figure 3.7b. Logarithm of  $h(t)$  of NY, FL, and IL for those who were arrested in 1980 ( $A_1 = 19-30$ )



**Approach 2: Log-rank tests**

The results of the (weighted) log-rank tests suggest that at least one of the three states is statistically different. In order to further examine the possible differences that could exist between the pairs of the states, the pairwise weighted log-rank tests were performed. The tests show that the NY and FL hazards are statistically similar when the values of the parameter  $q$  are

approximately between .7 and 1.7 (slightly more weight is given to later rearrests). NY and FL are found to be statistically different when either heavier weight ( $p = 0, q = 2$ ), or less weight ( $p = 1, q = 0$ ) is given to later rearrests. This is consistent with our observation that NY and FL might be different at early and later times. The results of the test of difference between NY and IL show that the hazards of the two states are similar at earlier and later times.

### ***Approach 3: Time-varying effects of sampling years based on Cox models***

The hazard ratio estimates based on the Cox model with the state-dummy-time interactions are shown in Figure 3.8a-3.8b. In relation to the NY hazard, the hazard of FL is higher than NY initially, crosses NY within 5-7 years, stays somewhat lower than NY for a while, and approaches or crosses NY after 14 years.

The IL hazard seems to cross the NY hazard faster than FL hazard, which can be seen in the log-transformed hazards in Figure 3.6b as well. The IL and NY hazards seem to converge within 10 years after the initial arrest.

Besides the fact that the confidence intervals are narrower for a given confidence level due to larger samples sizes, the hazard ratio estimates based on the arrestee cohorts (Figure 3.9a-3.9b) are generally similar to the results based on the convictee cohorts. When the specifics are examined by using the criminal history of arrestee cohorts, the ratio of FL to NY seems to change less with time clean and is closer to unity than the ratio based on conviction cohorts. Together with the observation from Figure 3.7b that the FL hazard is more similar to the hazards for the other two states when the hazard is based on arrests, it is possible that the process of conviction in FL could be different than that of NY and IL. On the other hand, the finding that

the hazard of IL is lower than the hazard of NY or FL holds whether the observation is based on arrests or convictions.

Figure 3.8a. Hazard ratio estimates (FL/NY) with confidence intervals

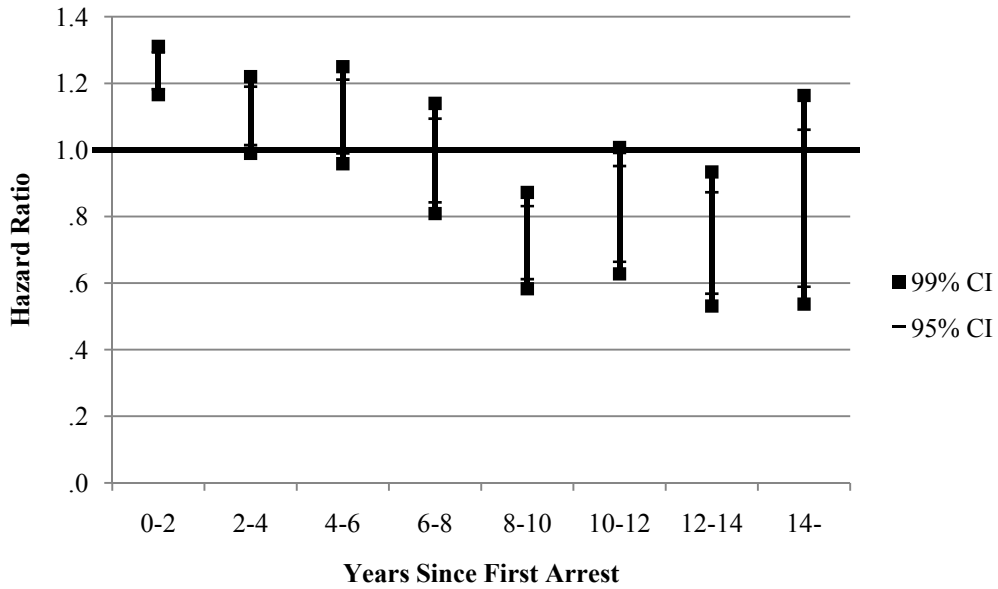


Figure 3.8b. Hazard ratio estimates (IL/NY) with confidence intervals

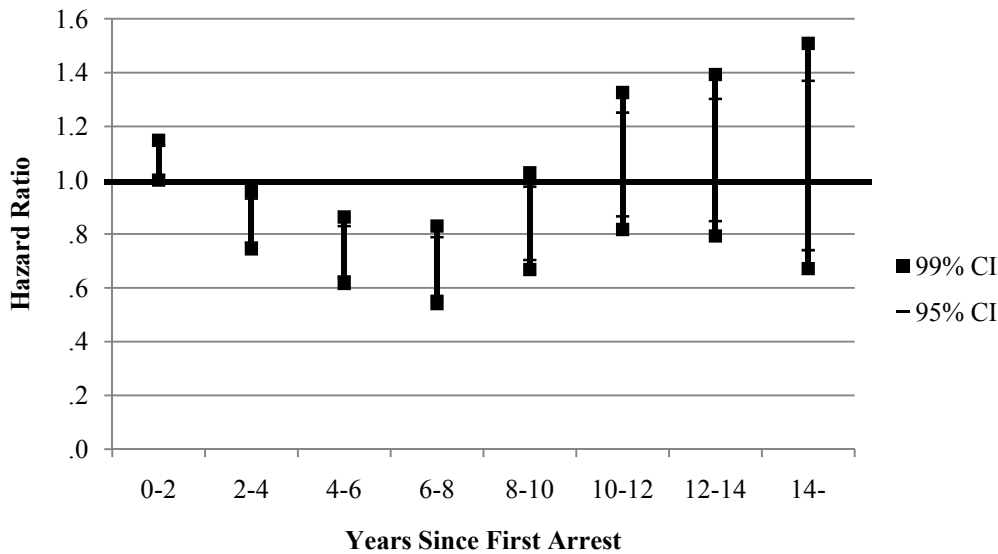


Figure 3.9a. Hazard ratio estimates (FL/NY) with confidence intervals based on arrest data

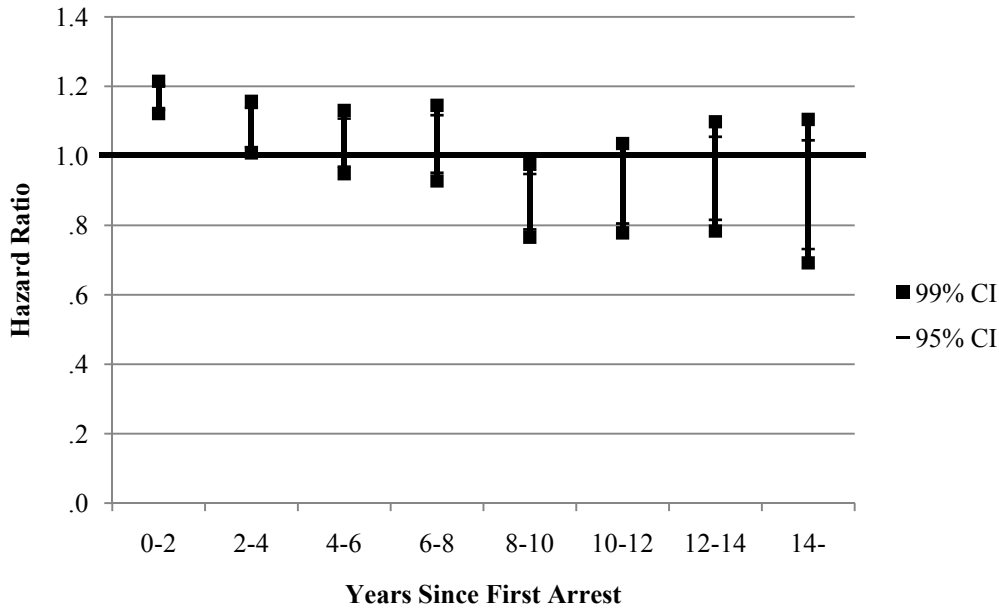
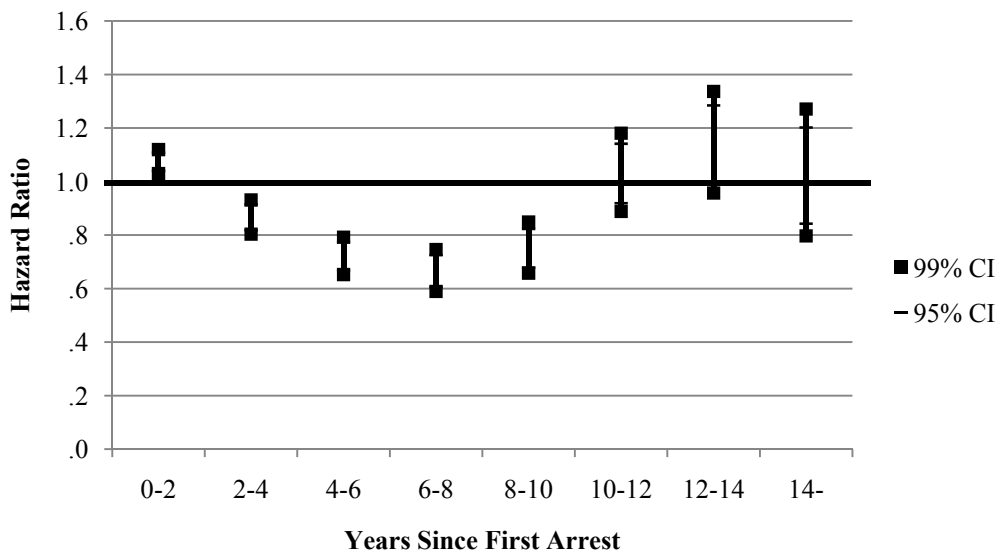


Figure 3.9b. Hazard ratio estimates (IL/NY) with confidence intervals based on arrest data

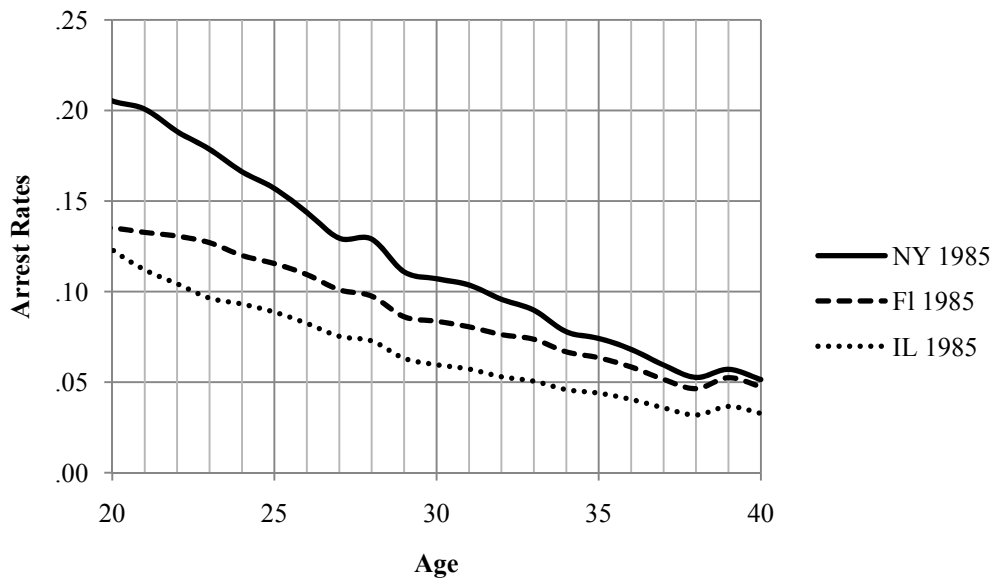


We can begin to explain the patterns of differences between the hazards of the three states by looking at the age-crime curves from the three states. Figures 3.10a-3.10b show the age-crime

curves of NY, FL, and IL in 1985 and 1992, 5 years and 12 years after their initial arrest or conviction in 1980.<sup>50</sup> Those who were 19-20 years old in 1980 are 24-25 years old in 1985. The arrest rate for 24-25 year olds in IL is the lowest (Figure 3.10a). Those 19-20 year olds become 31-32 year olds in 1992. The state with the lowest arrest rate for 31-32 is now FL, which is somewhat lower than IL. This switch of FL and IL are consistent with the patterns of hazards we observe in Figure 3.6b.

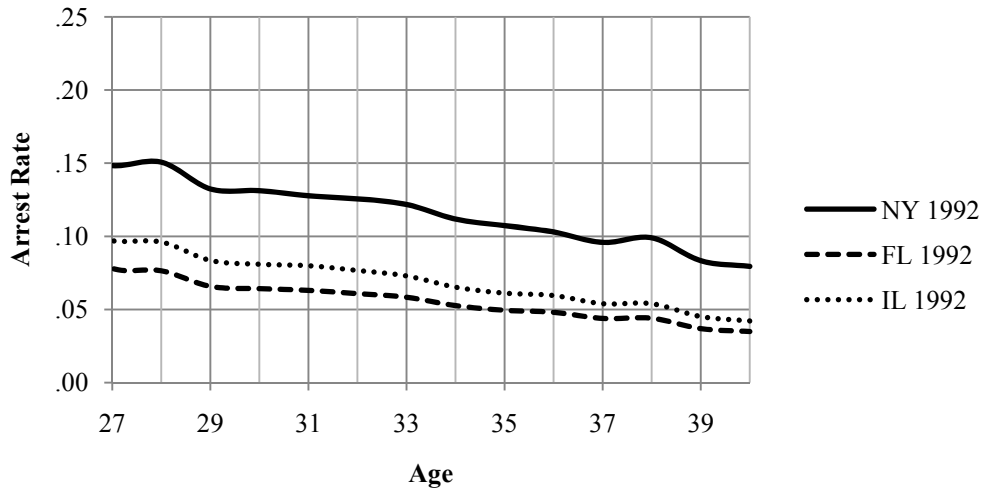
This finding suggests that the arrest prevalence (represented by age-crime curves) in different states is useful in understanding the long-term patterns of recidivism for those who stay clean for a long period of time. It is important to note that the magnitude is very different between the hazard for redemption candidates and the age-crime curves (for example, the hazards at  $t = 12$  for FL and IL are in the range of .013-.017, whereas the arrest rates at ages 31-32 in FL and IL are in the range of .06-.08).

Figure 3.10a. 1985 Age-Crime Curves for NY, FL, and IL



<sup>50</sup> We were unable to construct 1990 age-crime curves due to the fact that the 1990 IL UCR data from the National Consortium on Violence Research Data Center seem anomalous.

Figure 3.10b. 1992 Age-Crime Curves for NY, FL, and IL



### 3.3.3 Robustness of Redemption Times

Finally, it is important to examine how much the difference in the hazards across states affects the estimates of redemption times. For the reasons discussed in the robustness of redemption times across sampling years, the choice of benchmarks to estimate redemption times could require considering different approaches. One way is to use the age-crime curves from the different states. Another approach is to apply one universal benchmark to all states. A natural choice of such a universal benchmark is the national age-crime curve. As discussed above in the context of robustness across sampling years, setting a risk threshold would be useful here. As shown in Table 3.4a, using the value of .1, which is the probability of arrest at  $T^*$  for the 1980 NY cohort, as the threshold, the hazards of those who were convicted in the three states ( $A_1 = 19-20$ ,  $C_1 = \text{Violent, Property, Drugs}$ ) fall below the threshold on average after 5.5 years for Violent and 3.5 years for Property and Drugs. Especially, for  $C_1 = \text{Drugs}$ , the redemption time estimates are very close. For the .03 threshold, the redemption times are on average 13.5 years for Violent, 9 years for Property, and 11 years for Drugs. There is more variation in the redemption times across the three states. This is partially due to the fact that the estimation of

redemption times uses the upper confidence intervals. The sizes of the samples that are used to produce the confidence intervals are different across the states, and these sample size differences affect the widths of the confidence intervals (especially at later times), and in turn affect the estimates of redemption times. Table 3.4b shows redemption times that are similar to Table 3.4a, but uses those who were arrested instead of those who were convicted. Because of the larger samples sizes based on the arrests, the variation in the estimated redemption times across the states is smaller.

There is larger variation in the estimates of redemption times across the states than across sampling years, indicated by larger standard deviations in Table 3.4a. Yet, except for  $C_1 = \text{Drugs}$ , the estimates from the three states are on average within 2 years, which provides reasonable evidence for the robustness of the estimates.

Table 3.4a. Values of  $T^*$  for  $A_1 = 19-20$ ,  $C_1 = \text{Violent, Property, Drugs (convictees)}$  by states, using the upper CI with the thresholds of .1 and .03

$C_1$	State	Thresholds (probability of a new arrest)	
		.1	.03
Violent	NY	4.66	12.33
	FL	6.99	13.89
	IL	5.56	15.00
Average		5.7	13.7
Std. Dev.		1.2	1.3
Property	NY	2.94	8.16
	FL	3.76	7.99
	IL	3.68	11.31
Average		3.5	9.2
Std. Dev.		.5	1.9
Drugs	NY	3.67	11.87
	FL	3.57	7.58
	IL	3.54	14.13
Average		3.6	11.2
Std. Dev.		.1	3.3

Table 3.4b. Values of  $T^*$  for  $A_1 = 19-20$ ,  $C_1 = \text{Violent, Property, Drugs (arrestees)}$  by states, using the upper CI with the thresholds of .1 and .03

$C_1$	State	Thresholds (probability of a new arrest)	
		.1	.03
Violent	NY	3.81	11.10
	FL	4.75	10.60
	IL	3.71	12.32
Average		4.1	11.3
Std. Dev.		.6	.9
Property	NY	2.59	7.45
	FL	2.90	8.16
	IL	2.86	6.28
Average		2.8	7.3
Std. Dev.		.2	.9
Drugs	NY	2.82	10.13
	FL	2.43	6.81
	IL	2.66	6.59
Average		2.6	7.8
Std. Dev.		.2	2.0

### 3.4 Summary

The redemption time estimates produced in Ch. 2 are based on the data from the 1980 NY arrestee cohort. In order to develop empirical evidence that can apply beyond the particular data, it is necessary to test the robustness of the estimates to key conditions that generate the data. In this chapter, the robustness of redemption time estimates were tested in terms of two conditions: sampling year and jurisdiction (state), using data from two additional sampling years (1985 and 1990) and data from two additional states (Florida and Illinois).

Despite major shifts in the levels of arrest rates during the period of 1980 through 1990, the patterns of recidivism risk across the three sampling years are found to be similar. In estimating redemption times across sampling years and across states, two threshold probabilities (.1 and .03) of incurring a second arrest are used. The higher threshold (.1) represents the approximate probability of arrest at  $T^*$  for the 1980 NY cohort, and the lower threshold (.03) represents the approximate probability of arrest at  $T^{**}$  (for the risk tolerance  $\delta = .02$ ) for the same cohort. For the higher threshold probability (.1), the average estimates of redemption times of the three sampling years are about 5 years, 4 years, and 3 years for  $C_1 = \text{Violent, Drugs, and Property}$  respectively. For the lower threshold probability (.03), the averages are about 12 years, 11 years, and 9 years for the three  $C_1$ 's. These estimates are robust, to the degree that the estimates are on average within a year of the estimate from each of the sampling years.

The risk patterns and the associated estimates of redemption times vary more across the states than across sampling years, but they appear to converge after 10 years. However, even that variation may not be operationally significant since the estimates across the three states differ by an average of only two years except for drugs. For drugs, the estimates differ, on average, just over 3 years. For the higher threshold probability (.1), the average estimates of redemption times of the three years are about 6 years, 4 years, and 4 years for  $C_1 = \text{Violent, Drugs, and Property}$  respectively. For the lower threshold probability (.03), the averages are about 14 years, 11 years, and 9 years for the three  $C_1$ 's.

Lastly, the table below displays the range of redemption time estimates by  $C_1$  for the two threshold probabilities. The estimates for  $C_1 = \text{Violent}$  tends to be the largest, the estimates for  $C_1 = \text{Property}$  tend to be the smallest, and the estimates for  $C_1 = \text{Drugs}$  are in between the other two.

Table 3.5. Range of redemption time estimates (years) based on the estimates across three sampling years and three states

C <sub>1</sub>	Thresholds (probability of a new arrest)	
	.1	.03
Violent	4-7	11-15
Property	2-4	8-11
Drugs	4	10-14

## **CHAPTER 4: RACE: PREVALENCE AND LONGER-TERM RECIDIVISM RISK**

### **4.1 Concern over the Role of Race in Criminal Background Checking**

The issue of redemption is particularly important for African-Americans compared to whites. It is widely recognized that their arrest experience is considerably greater than that of whites, and therefore it is reasonable to assume that this higher prevalence of arrest would lead to further handicaps, beyond those based on racial discrimination alone. This difference has been shown rather dramatically by Pager (2003) in her experimental audit study, which shows that a white job applicant with a criminal record was 3.4 times more likely to receive a call back from an employer than a black applicant with a criminal record, whereas a white applicant with no criminal record was 2.4 times more likely to be called back than a similarly situated black applicant.<sup>51</sup> The difference in the likelihoods implies a considerable additional disadvantage that African-Americans with criminal records face in employment opportunities.

The concern about this racial difference in criminal-history background has been an important focus of the Equal Employment Opportunity Commission (EEOC), which is committed to enhancing employment opportunities for minorities, and especially African-Americans. The EEOC issued a guideline in 1990 that employers' decisions to screen out job applicants with criminal records could cause a disparate impact of race or ethnicity under Title VII of the Civil Rights Act of 1964 (EEOC, 1990). Title VII prohibits employers from denying employment to job applicants based on their race, sex, religion, or national origin. Individuals with criminal records are not part of the "protected classes" under Title VII. However, given the racial/ethnic disparity in the rates of having criminal records, blacks, Hispanics, and other

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<sup>51</sup> These race-based differences combine to give rise to her oft-cited observation that a white applicant with a criminal record is more likely to receive a call-back than a black with no criminal record.

racial/ethnic minorities could be affected adversely by the employers' screening out those with criminal records.

In recent years, the EEOC has stepped up its efforts to challenge employers' criminal background checking practices on the grounds that employers must demonstrate "business necessity" which is that the prior criminal records are relevant to the nature of the job positions. For example, in 2008, the EEOC filed a lawsuit against Peoplemark, Inc. (*EEOC v Peoplemark, Inc.*), alleging that they unlawfully denied employment to those with criminal records, thereby having a disparate impact on African-American applicants. The EEOC also sued a corporate events company, Freeman (*EEOC v Freeman*) in 2009 for similar allegations about the unlawful use of criminal records in violation of Title VII.

#### **4.2 Relative Arrest Experience of African-Americans and Whites**

It is widely recognized that blacks experience higher arrest rates than do whites. This disparity is represented by a black-to-white arrest rate ratio. This ratio  $R$  is calculated for each offense type by the reported ratios of black-arrests-to-black population divided by the ratio of white-arrests-to-white populations:

$$R = B / W \text{ Arrest - Rate Ratio} = \frac{A_B / P_B}{A_W / P_W}$$

where:

$R$  = black-to-white arrest rate ratio

$A_B$  and  $A_W$  = number of arrests of blacks and whites respectively

$P_B$  and  $P_W$  = population of blacks and whites respectively.

The data for the arrest numbers are available for each year in the Uniform Crime Reports (UCR) published by the FBI (2009).<sup>52, 53</sup> Table 4.1 presents the B/W arrest- rate ratios for 2008 for the variety of crime types enumerated in the UCR.<sup>54, 55</sup> The crime types are listed in the order of their ratios and are grouped as high ( $R > 4$ ), medium ( $3 < R < 4$ ), moderate ( $1 < R < 3$ ), and reverse ( $R < 1$ ).

The highest ratios are for robbery ( $R = 8.4$ ) and murder ( $R = 6.5$ ) (probably the two offenses seen as most serious) as well as weapons offenses ( $R = 4.6$ , primarily carrying unlicensed weapons) and involvement in prostitution ( $R = 4.6$ ). The reverse ratios occur for liquor-law violations ( $R = .85$ , primarily involving juveniles in possession of alcoholic beverages) and driving under the influence of alcohol ( $R = .71$ ).

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<sup>52</sup> These measures of relative arrest prevalence are based on national arrest numbers based on the reports of police departments to the UCR. The number of departments reporting represent only about 75% of the total US population, and to the extent that they might be a less-than-representative sample, that could lead to some distortions in the values of  $R$ .

<sup>53</sup> In the UCR arrest data, there are four categories for race: White, Black, American Indian or Alaskan Native, and Asian or Pacific Islander. In the Census population data, in addition to a question on race, there is a separate binary question on Hispanic origin. To be consistent with  $A_W$  and  $A_B$ ,  $P_W$  and  $P_B$  both include Hispanic and non-Hispanic,

<sup>54</sup> We do not include in Table 4.1 crime types that are applicable only to juveniles (curfew and loitering violations and runaways) and crime types for which fewer than 10,000 arrests were reported: suspicion ( $R = 6.83$ ) and gambling ( $R = 21$ ), which had the largest value of  $R$ .

<sup>55</sup> The values of  $R$  for 2007 are very close to those for 2008.

Table 4.1. B/W arrest-rate ratios in 2008

	Offense	B/W arrest-rate ratios
High (> 4)	Robbery	8.43
	Murder	6.49
	Weapons	4.57
	Prostitution	4.56
Medium (3-4)	Motor vehicle theft	3.96
	Vagrancy	3.93
	Stolen property	3.68
	Drug abuse	3.39
	Aggravated assault	3.35
	Disorderly conduct	3.34
	Embezzlement	3.22
	Other assaults	3.07
Low (1-3)	Forcible rape	3.07
	Burglary	2.92
	All other offenses (except traffic)	2.89
	Offenses against the family and children	2.88
	Fraud	2.84
	Forgery and counterfeiting	2.82
	Larceny-theft	2.67
	Other sex offenses	2.03
	Vandalism	1.81
	Arson	1.78
Reverse (< 1)	Drunkenness	1.13
	Liquor laws	.85
	Driving under the influence	.71

Blacks tend to be from lower socioeconomic status (SES) groups disproportionately more than whites, and it is clear that there can be a strong interaction between the SES and likelihood of involvement in many of the property- or income-related crimes. It is also the case that differences in police patrol patterns, which are more densely located in minority and high-crime

areas, could be important factors affecting differential involvement in crimes like vagrancy and disorderly conduct (whites might confine their disorderliness to their homes and backyards whereas blacks without those refuges are more likely to do so in the street where they are visible to patrolling police).

### **4.3 Long-Term Patterns of Recidivism by African-Americans and Whites**

The differences in arrest rates between blacks and whites displayed in Table 4.1 represent the racial difference in the prevalence of arrests, how commonly an arrest occurs in each of the two populations. The prevalence difference is explained by the fact that blacks are more likely than whites to penetrate the participation “filter” between the general population and those who participate in crimes (Blumstein and Cohen, 1987; Blumstein et al., 1986). It is clear that there are important differences between the two races in their participation in the various kinds of criminal activity – or at least in their likelihood of being apprehended for doing so. However, it is estimated that there is much less difference between blacks and whites in the arrest frequency of those who have already been identified as being criminally active (i.e., those who passed through the participation filter) (Blumstein and Cohen, 1987). This suggests that among those who have been identified as being criminally active, race may play less of a role in predicting their propensity to commit another crime.

Studies of recidivism provide additional insight into this phenomenon. Recidivism studies of released prisoners conducted by the Bureau of Justice Statistics (BJS) have shown that there is racial disparity in the recidivism rates; blacks are more likely, but only somewhat so, to recidivate (Beck and Shipley, 1997; Langan and Levin, 2002). Among the sample of prisoners released in 1983, the rearrest rate within 3 years is about 8 percentage points higher (67%

compared to 59%) for blacks than whites (Beck and Shipley, 1997). Among a similar sample of prisoners released in 1994, blacks' rearrest rate within 3 years was 10 percentage points higher (73% compared to 63%) than whites' rearrest rate (Langan and Levin, 2002).

It is important to recognize that the race difference in recidivism rates among the released prisoners is much smaller than the race difference in the prevalence rates. Applying the idea of a "filter" posited by Blumstein and Cohen (1987), the released prisoners were more similar in terms of their propensity to commit a new crime regardless of race because they all passed through the participation filters of arrest and conviction, and they were all given an incarceration sentence.<sup>56</sup>

It is quite possible that the large prevalence difference between blacks and whites could play a role in shaping the employers' perception of applicants' risk of future crime. However, in the context of redemption, where job applicants with a prior record have stayed clean for a substantial length of time, we might anticipate that the racial difference in the recidivism risk, which employers should be concerned about, will be less than the difference in arrest prevalence. Employers should be able to make more informed evaluations regarding the risk associated with white and black applicants if they had the information about the racial difference in the risk of recidivism *conditional on the length of time clean*.

The evidence of racial difference in recidivism rates by the BJS studies is short term, since their follow-up time was limited to 3 years, and there is little known about the extent to which the racial difference in recidivism rates persists in the long run. Thus, it is important to investigate the possibility that the risk of recidivism for blacks with a prior record will be greater than that of whites, but that the difference between the two will be much less than the difference

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<sup>56</sup> It is interesting to note that the BJS recidivism studies also show that the race difference in rearrest rates decline with the number of prior arrests. Thus, the more prior arrests the released prisoners have, the less important race is as a predictor of recidivism.

in their prevalence. Also, among those who stay clean for a considerable length of time after their first arrest or conviction, the racial difference in recidivism probability could be smaller not only than the difference in arrest prevalence, but even in the hazard shortly after their prior arrest.

#### 4.4 Data

We use the criminal history data of our cohort of first-time arrestees in 1980 in New York State. In addition to the age at first arrest ( $A_1$ ) and the crime type of the first arrest ( $C_1$ ), we now consider race differences. The NY data record four race categories: white, black, Hispanic, and other, but in order to examine the most relevant racial differences in recidivism and redemption, we focus on only white and black offenders.<sup>57</sup>

In order to maintain sample sizes large enough for more precise statistical estimation, in this chapter, we base our analyses on all the arrestees, including those who were not necessarily convicted for their first crime. The conviction probabilities are very similar between blacks and whites, as shown in Table 4.2, which depicts the fraction of arrestees who were convicted for each of the five  $C_1$ 's and for all crime types. For property offenses, whites are only slightly more likely to be convicted than blacks, and vice versa for drug offenses and public order offenses; however, overall, there is not much difference between whites and blacks in their probability of being convicted after having been arrested. Table 4.3 provides the distribution of the 1980 arrestee sample by crime type at first arrest.

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<sup>57</sup> In the data we received from the NY state repository, there is one column for race (white, black, Hispanic, other) and another column for "Ever Hispanic". Among the 1980 NY arrestees ( $A_1 = 19-30$ ), 11% are recorded as Hispanic in the race column. In order to focus on the contrast between white and black, we do not treat those whose race is recorded as Hispanic. 96% of black arrestees and 97% of white arrestees are recorded as Hispanic in the "Ever Hispanic" column.

Table 4.2. Percent of arrestees who were convicted in 1980 <sup>58</sup>

Race	C <sub>1</sub> *					
	Violent	Property	Drugs	Public Order	Others	All
white	64.6%	75.6%	72.1%	70.5%	68.4%	71.5%
black	64.4%	71.2%	76.9%	74.8%	66.6%	70.6%

\* Violent crimes are designated to include robbery, aggravated assault, forcible rape, and simple assault. Property crimes are designated to include burglary, larceny, motor vehicle theft, stolen property, forgery, fraud, and embezzlement. Drug crimes include both possession and sales of any controlled substance. The category “All” is for all crime types except DUI.

Table 4.3. Initial Sample Size (arrested), by First Offense (C<sub>1</sub>) in 1980\*

Race	C <sub>1</sub>					
	Violent	Property	Drugs	Public Order	Others	All
White	3,053 (18.0)	7,268 (42.9)	1,904 (11.25)	2,375 (14.0)	2,324 (13.7)	16,924 (71.3)
Black	1,556 (22.8)	2,801 (41.1)	827 (12.1)	1,125 (16.5)	508 (7.5)	6,817 (28.7)

\* The row % for the five C<sub>1</sub>'s and the marginal % for C<sub>1</sub> = All are in brackets.

## 4.5 Approach and Results

We are interested in contrasting the relative risk of arrest between blacks and whites in the general population and then comparing that to the relative risk of arrest between blacks and whites in the population of those with a prior criminal arrest who have stayed clean since the arrest.

### 4.5.1 Relative Arrest Experience of African-Americans and Whites

<sup>58</sup> C<sub>1</sub> refers to the type of offense for which the arrest was made, regardless of the conviction offense, which could possibly be different from the arrest offense.

The relative experience of arrest between blacks and whites in the general population is represented by the black-to-white arrest-rate ratio (prevalence ratio),  $R$ , discussed earlier. The  $R$  values are calculated as the number of arrests of blacks and whites in New York State for the violent, property, and drug offenses from the UCR reports, each divided by their respective NY populations. We estimated these values for 1985, 1990, and 1995, representing 5-year intervals for the 1980 arrestee cohort. These values of  $R$  are tabulated in Table 4.4. We note that the prevalence ratios are reasonably close for the three sampling years, and that they show a slight decline over that interval. We also note that the ratios are appreciably larger for violence (4.7) than for property (3.3), and that violence and drugs (4.4) are reasonably close. This suggests that although the racial disparity in arrest prevalence seems to be declining over time, it is still the case that arrest is about 4 times more common for blacks than for whites in the NY general population.

Table 4.4. Black-to-white Arrest Prevalence Ratios for Violent, Property, Drugs in 1985, 1990, 1995

Year	$C_1$			
	Violent	Property	Drugs	All
1985	5.0	3.5	4.7	5.0
1990	4.7	3.0	4.6	3.9
1995	4.3	3.3	4.1	3.5
Average	4.7	3.3	4.4	4.1

#### 4.5.2 Relative Rearrest Experience of African-Americans and Whites

As a contrast to the prevalence of arrests, we now turn to examine the hazard of a rearrest,  $h(t)$ , which is the probability of a new arrest at a time  $t$ , given staying clean (i.e. without any subsequent arrests) until  $t$ . We first estimate  $h(t)$  separately for blacks and whites for three  $C_1$ 's

(Violent, Property, Drugs), shown in Figures 4.1a-4.1c. For the three crime types, blacks have consistently higher hazards than whites. Initially, the ratios of hazards for blacks to whites are higher for  $C_1 = \text{Violent}$  and  $C_1 = \text{Drugs}$  than for  $C_1 = \text{Property}$ . This is consistent with what we found above (Table 4.4) in the arrest prevalence ratios for the three crime types: the black-to-white arrest prevalence ratio is higher for violent offenses and drug offenses, than for property offenses. But, most strikingly, for drug offenses, the initial hazard for blacks (at  $t = 0$ ) is more than 3 times the hazard for whites; this results from the fact that drugs represents blacks' highest hazard and whites' lowest. This result is also supported by the direct estimation of the black-to-white hazard ratio. As discussed in Ch. 3, we can estimate the hazard ratios by race and its standard errors by including interactions between the indicator variable for blacks (Black) and time in the Cox regression model. The hazard ratio estimates with confidence intervals can be plotted against time to examine how that ratio changes with arrest-free time.

Figure 4.1a.  $h(t)$  for black and white arrestees,  $C_1 = \text{Violent}$

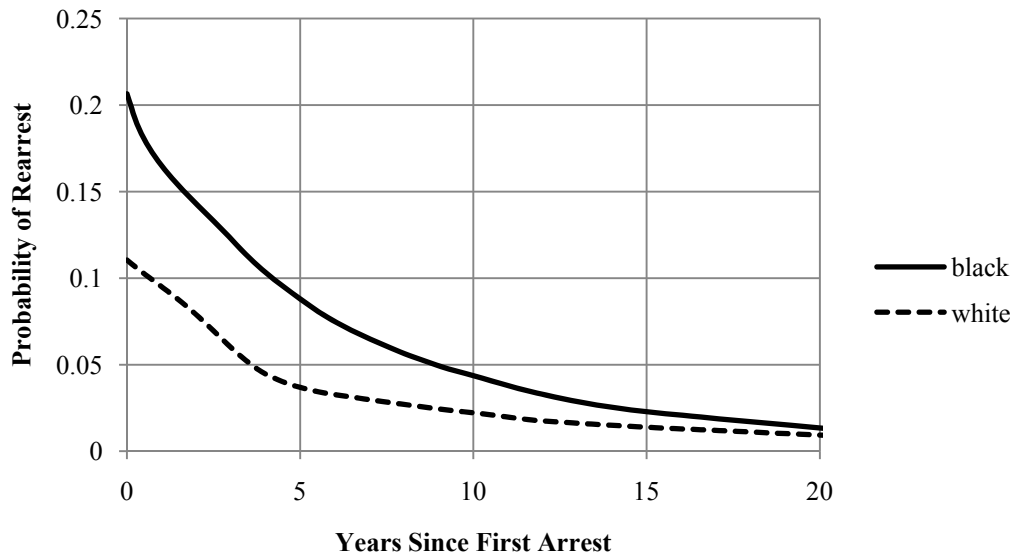


Figure 4.1b.  $h(t)$  for black and white arrestees,  $C_1 = \text{Property}$

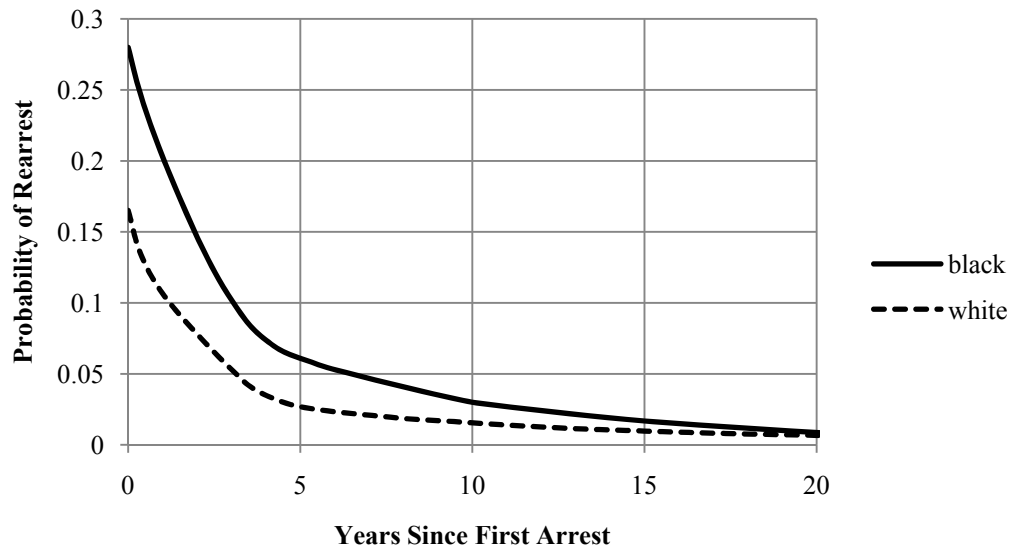
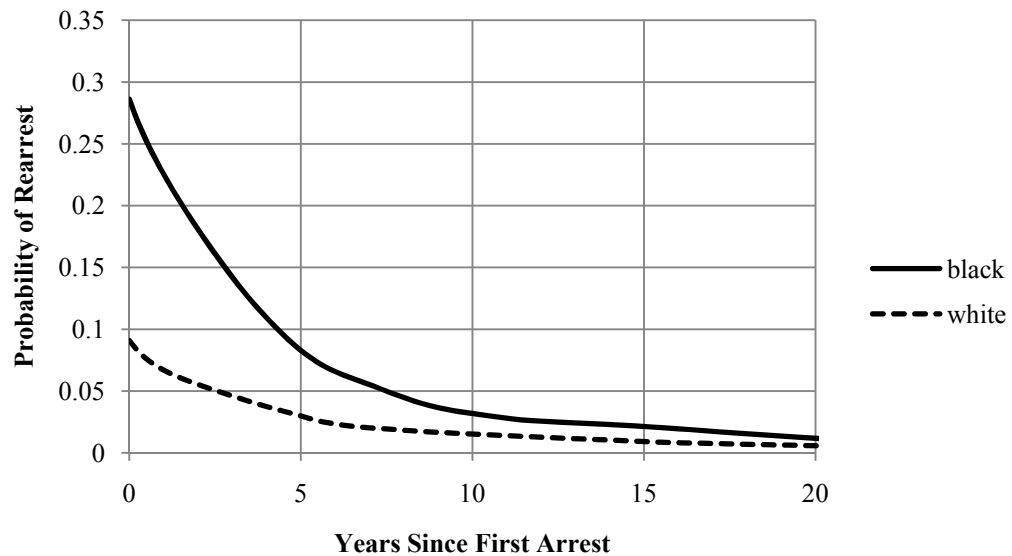


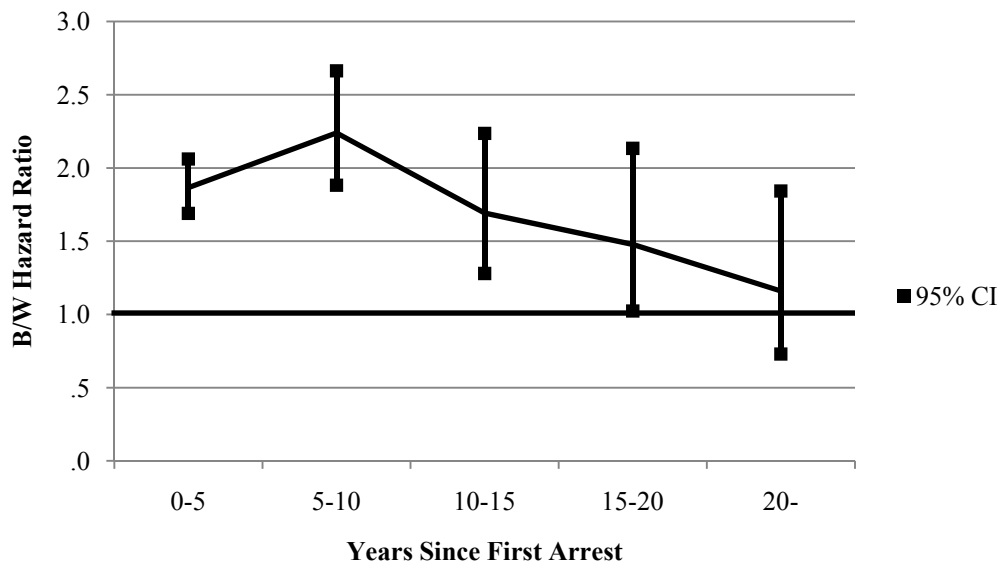
Figure 4.1c.  $h(t)$  for black and white arrestees,  $C_1 = \text{Drugs}$



Figures 4.2a-4.2c show the estimated hazard ratios,  $(B/W)$  for  $C_1 = \text{Violent}$ ,  $\text{Property}$ , and  $\text{Drugs}$ , with confidence intervals using the Cox model with the interactions between Black and time (5-

year time intervals).<sup>59</sup> The hazard ratios for  $C_1 = \text{Violent}$  and Property increase slightly for the first 10 years, and after that the ratios decline steadily toward 1. In contrast, for  $C_1 = \text{Drugs}$ , the ratio shows a different pattern. First, it is much higher than the other two crime types: the initial ratio for drugs is about 3.5, while for violent and property offenses, the ratios are about 2. Second, the Drug ratio gradually declines in the first 15 years, but it is still over 2 during that period. After  $t = 15$ , the ratio seems to increase somewhat, but since the confidence intervals are very wide, it could well be that it doesn't change much and remains at about 2. Table 4.5 indicates the values of the hazard ratio for the 1980 arrestee cohort at the five-year points. Again, it highlights the overall downward trend for the hazard ratios as those with a prior stay clean. For drug offenders, however, their ratio increases at  $t = 15-20$ .

Figure 4.2a. Hazard ratio estimates (B/W) with confidence intervals,  $C_1 = \text{Violent}$



<sup>59</sup> Confidence interval for the hazard ratio is based on the exponentiated endpoints of the confidence interval for the original coefficient of the Cox model. This is preferable to an alternative way, which is based on the standard error of the hazard ratio directly, because this alternative method can lead to negative values of the confidence intervals. Both methods are asymptotically equivalent (Klein and Moeschberger, 2003)

Figure 4.2b. Hazard ratio estimates (B/W) with confidence intervals,  $C_1 = \text{Property}$

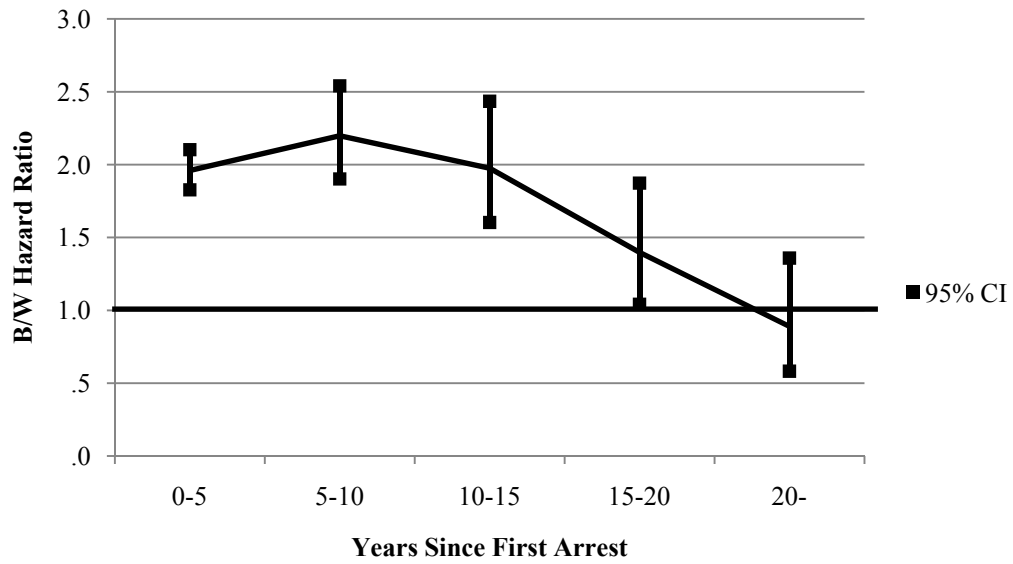


Figure 4.2c. Hazard ratio estimates (B/W) with confidence intervals,  $C_1 = \text{Drugs}$

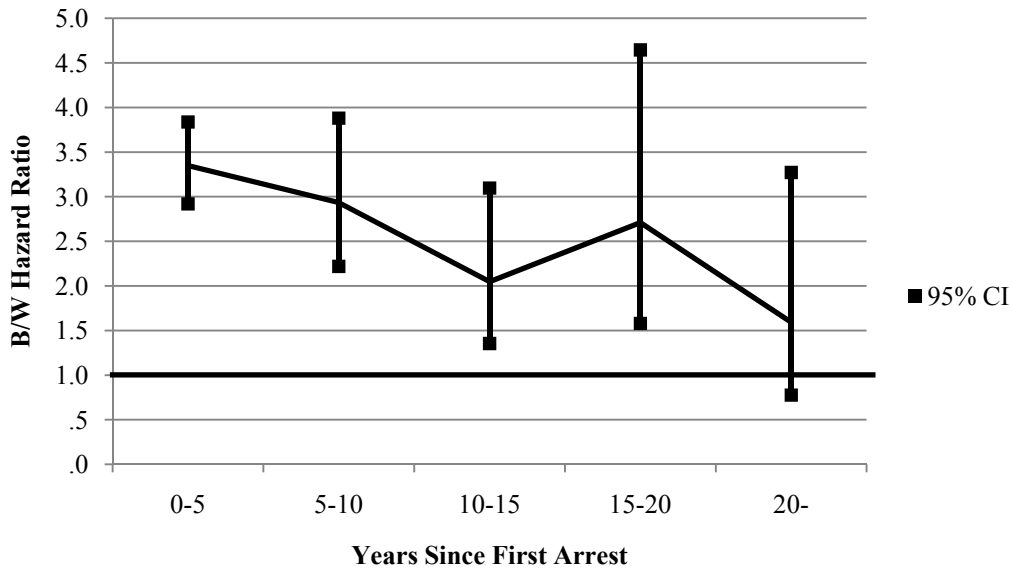


Table 4.5. Black-to-white hazard ratios for  $C_1 =$  Violent, Property, Drugs, All at 5 year intervals for the 1980 Cohort

t	$C_1$			
	Violent	Property	Drugs	All
0-5	1.9	2.0	3.3	2.1
5-10	2.2	2.2	2.9	2.2
10-15	1.7	2.0	2.0	1.8
15-20	1.5	1.4	2.7	1.5

The analysis of the survival probabilities for whites and blacks shown in Figures 4.3a-4.3c also illustrates the point that the risk of recidivism for blacks becomes similar to the risk for whites. It is clear that the survival probabilities for blacks are substantially lower than for whites: at  $t = 20$  about 20% lower for  $C_1 =$  Violent and Property and about 35% lower for  $C_1 =$  Drugs. However, these large differences are mostly due to the differences that occur in the first 10 years and the fact that the survival probabilities for blacks fall much faster than for whites in that period. This is consistent with the relatively large black-to-white hazard ratio in that period. While hazard function is informative about the instantaneous rearrest risk at  $t$ , survival probability, which is  $1 - F(t)$ , the cumulative distribution function, is informative about the probability of rearrest in a certain time interval. Table 4.6 shows the proportions of blacks and whites being rearrested in the first 10 years and after 10 years, which are calculated by the differences of survival probabilities,  $S(t = 0) - S(t = 10)$  and  $S(t = 10) - S(t = 25)$ , respectively. In the first 10 years, much larger proportions of blacks experience rearrests than whites. On the other hand, after 10 years, there is virtually no difference between whites and blacks in their probabilities of being rearrested. This suggests that although blacks may have a higher hazard than whites at  $t = 10$ ,

blacks who stay clean for 10 years have the same probability as whites of ever being rearrested in the future.<sup>60</sup>

Figure 4.3a. Survival probabilities,  $C_1 = \text{Violent}$

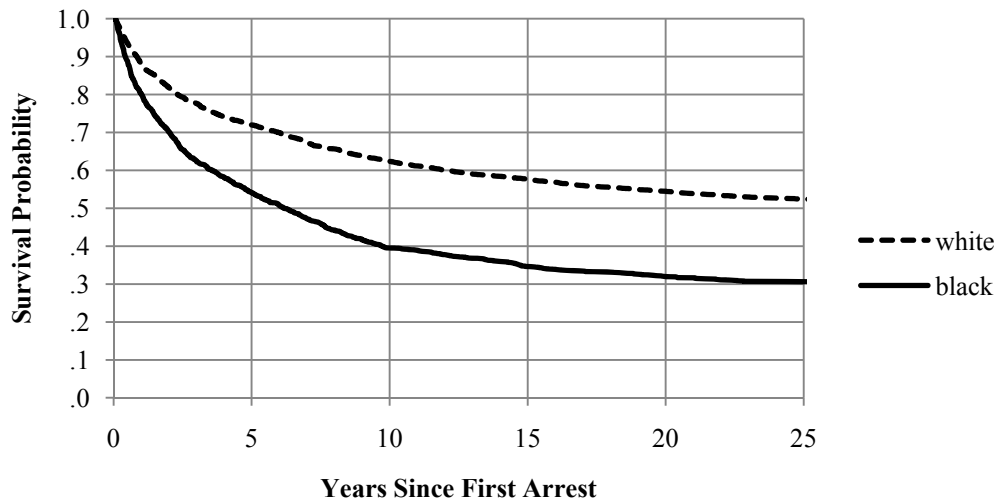
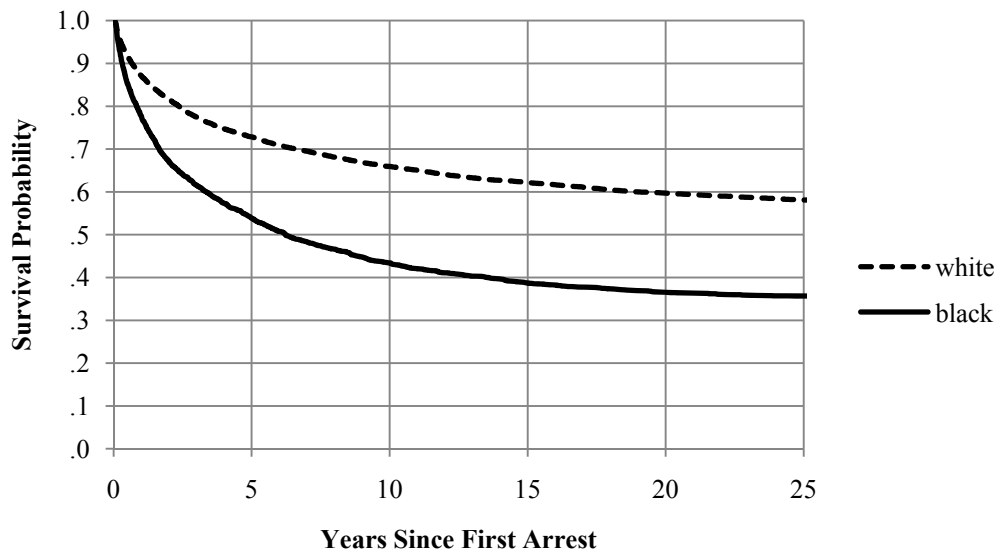


Figure 4.3b. Survival probabilities,  $C_1 = \text{Property}$



<sup>60</sup> It assumes that after staying clean for 25 years, very few would experience rearrests.

Figure 4.3c. Survival probabilities,  $C_1 = \text{Drugs}$

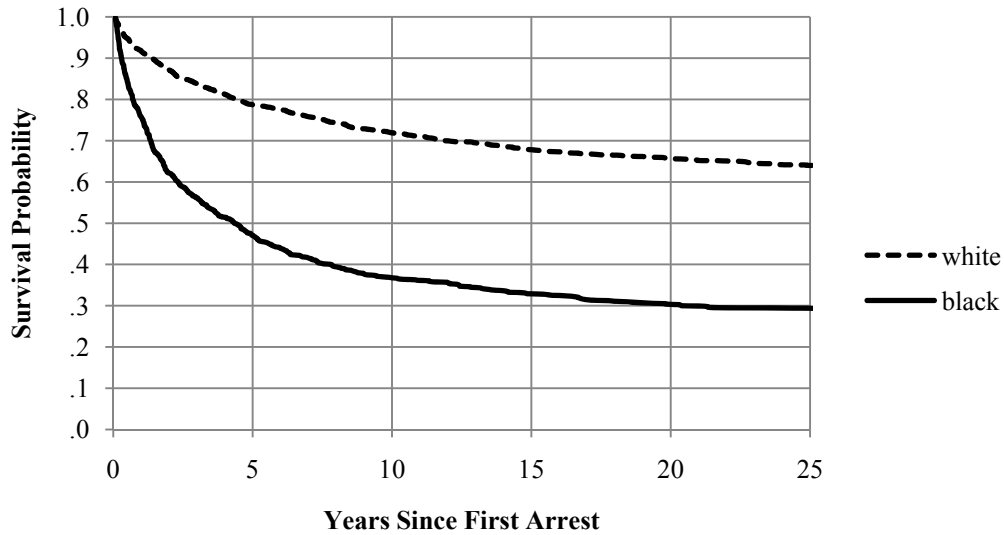


Table 4.6. Proportions rearrested between  $t = 0$  and 10 and between  $t = 10$  and 25

		$C_1$		
		Violent	Property	Drugs
$1 - S(t = 10)$	white	.38	.34	.28
	black	.61	.57	.63
$S(10) - S(25)$	white	.10	.08	.08
	black	.09	.08	.08

### 4.5.3 The Effect of “The Crack Epidemic”

#### *Disproportionate impact of “the crack epidemic” on African-Americans*

Despite the overall trend of decreasing black-to-white hazard, it is important to explore further the possible explanations for the fact that blacks have a higher hazard than whites for more than 10 years, and the hazard for the 1980 drug arrestees seems particularly high. One possible

interpretation of this large black-to-white difference could be that during the mid to late 1980s, which the 1980 cohort arrestees went through, the “crack epidemic” swept through African-American neighborhoods in major cities, and in New York City in particular.<sup>61</sup> The crack market and the aggressive policing that followed might have put the relatively few African-Americans who were arrested for drugs in 1980 before the introduction of crack, in a particularly vulnerable situation for recidivism.<sup>62, 63</sup> The introduction of crack and the drug war resulted in a large racial disparity in the arrest rates for drug offenses in the late 1980s (Blumstein, 1995). As shown in Figure 4.4, which is based on the UCR national arrest data, since 1980, the drug arrests for blacks increased rather sharply, while drug arrests for whites, remained reasonably stable (National Consortium on Violence Research, May 25, 2010). The arrest rates for blacks rose to 4-5 times that of whites in the late 1980s.<sup>64</sup>

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<sup>61</sup> In New York City in particular, crack cocaine began to be distributed in 1984, and its market grew considerably in 1985-86, mostly in minority neighborhoods (Johnson, Golub, and Dunlap, 2000).

<sup>62</sup> In our 1980 cohort, 827 blacks were arrested for drugs. The number of first-time black drug offenders increased to 1,400 in the 1985 cohort and over 2,300 in the 1990 cohort.

<sup>63</sup> As a response to the growing crack problem in the city, the New York Police Department launched Tactical Narcotics Teams (TNT) in 1988, reassigned about one-fourth of the department to the teams, and began mass drug arrests (Johnson, Golub, and Dunlap, 2000), and the open street transactions made the sellers particularly vulnerable to the arrests. Since African-American sellers of crack tend to operate in streets whereas white sellers of powder cocaine operate indoors, the “war on drugs” would disproportionately arrest African-Americans.

<sup>64</sup> The disproportionate impact of the drug war on African-Americans has profound lasting effects. Collateral consequences of acquiring criminal records can limit the access to services and opportunities that are essential for offenders to reintegrate into society (Travis, 2002). Compared to other types of crimes, drug offenders are often subjected to additional layers of collateral consequences. For example, those with drug convictions may be denied access to public housing or be ineligible for other housing assistance programs (e.g., Section 8). According to a report by the Government Accountability Office (2005), 15 large public housing agencies reported that about 5 percent of applications for admission were denied because of drug-related convictions. People with certain drug convictions are also ineligible for Temporary Aid to Needy Families and Food Stamps, federally-funded health care programs, and federal student loans.

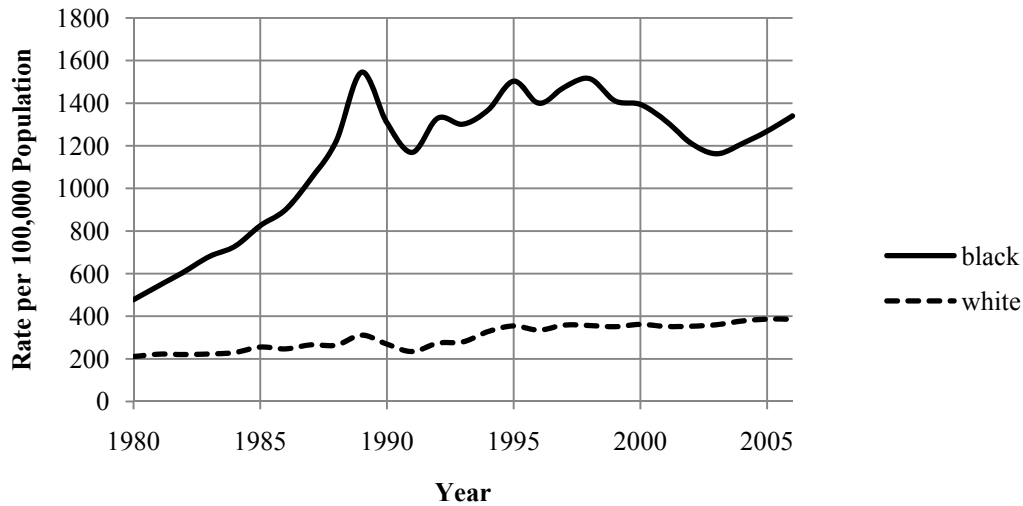
Since 1990, intense policing forced the crack market to move from outdoor curbside locations to closed-door locations (Johnson, Dunlap, and Tourigny, 2000). At the same time, in the early 1990s, the drug of choice for the youths began to shift from crack to marijuana, which could be largely attributed to a growing realization of the negative impact of crack on its users (Johnson, Dunlap, and Tourigny, 2000). This transition in the market location and the drug of choice possibly contributed to the end of the previous sharp rise to a peak in 1989, and then a leveling until about 2000, as seen in Figure 4.4. However, those changes did not lead to any significant closing of the gap between whites and blacks. As seen in Figure 4.4, even as marijuana replaced crack as the drug of choice, African-Americans continued to be disproportionately arrested for drug offenses.<sup>65, 66</sup>

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<sup>65</sup> At least in New York City, the use of marijuana became the most common misdemeanor arrest (15% of all NYC adult arrests) by 2000. Golub et al. (2007) reports that in 2000, the black-to-white ratio of misdemeanor marijuana arrest rates in NYC was about 6. For marijuana sale, the ratio was over 26. Although these numbers are limited to NYC, they provide some evidence for the continuing racial disproportionality in drug arrests into the 1990s.

<sup>66</sup> It is informative to follow over time the fraction of total drug rearrests for the 1980 black arrestees which are for marijuana. Among the drug rearrests, 56% are marijuana in the period  $0 < t \leq 5$ , 14% in  $5 < t \leq 10$ , 4% in  $10 < t \leq 15$ , and 21% in  $t > 15$ . Thus, it appears that marijuana was the main drug type for their rearrests until 1985, non-marijuana (likely most are crack) was the major drug for the next 10 years, and marijuana returned as the more common drug after the mid-90s.

Figure 4.4. Drug arrest rates for blacks and whites (national)



One way to understand the impact of racial disproportionality in drug arrests on the recidivism risk of whites and blacks in our 1980 NY cohort is to investigate the crime types for which the rearrest is made ( $C_2$ ). We have been treating recidivism as the rearrest for any crime. However, considering the possibility of the differential impact of the drug arrests on whites and blacks, examining the distribution of  $C_2$  may help us understand why blacks have a higher hazard.

Tables 4.7a-4.7d show the crime switch matrices, which display the combination of crime type of first arrest (the rows) and the probability of different crime types of second arrest (the columns), for those who have a second arrest and stay clean for the first 5 years, 5 to 10 years, 10 to 15 years and more than 15 years.<sup>67</sup> This allows us to examine what proportion of those who were arrested for each of the five  $C_1$  categories in 1980 were rearrested for the same crime category, or for a different category. The values in the diagonals of the matrix represent the

<sup>67</sup> The crime-switch matrices inform about the probability of switching from the crime type of the first arrest to different crime types of the second arrest. It is important to note that the matrices take no account for the crime types of the third and later arrests for those who have more than two arrests. In this sense, the information that the matrices contain is consistent with the hazard, which is the risk of having a second arrest.

proportion recidivating to the same crime type as their first arrest. The values in the off-diagonals represent the proportion committing different crime types than their first one. The last row is the average of the probabilities of each  $C_2$ . Since an important concern is the repetition of the initial crime type ( $C_1$ ), the last column contains the ratio of the diagonal values (the probability of repeating  $C_1$ ) to the average of the off-diagonal values.

For both whites and blacks who were rearrested within the first 5 years (Table 4.7a), the diagonal values (in bold in the table) tend to be higher than off-diagonal values, indicating a propensity to repeat the first crime type in the second arrest, and especially so for property crimes. It is important to note that for  $C_1 = \text{Drugs}$ , the diagonals are appreciably larger for blacks than whites, which could suggest the growing influence of the crack market on African-Americans of our 1980 cohort.<sup>68</sup>

For the next interval,  $5 < t \leq 10$  (Table 4.7b), the overall propensity to repeat the same crimes is lower for both whites and blacks, reflecting a weaker connection between  $C_1$  and  $C_2$ , undoubtedly a result of the longer interval. More importantly, a clear pattern emerging from Table 4.7b is that blacks' recidivism to drug offenses is much more salient than for whites. The average of the conditional probabilities of drug rearrest (average of the values in the column Drugs) is 28.9% for blacks and 17.5% for whites. Similar 1.5:1 disparities are seen in Table 4.7c (for  $10 < t \leq 15$ ) and 4.7d (for  $t > 15$ ).

The fact that the drug offenses are such a dominant crime type for the rearrest of black offenders who stay clean for 10 years is clearly in accord with the explanation that African-Americans were disproportionately caught up in the intensive drug battles of the 1980s and 1990s.

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<sup>68</sup> The diagonals are larger for blacks than whites also for  $C_1 = \text{Public Order}$ , which could reflect different patrol patterns.

Table 4.7a. Crime switch matrix for arrestees  $0 < t \leq 5$

		C <sub>2</sub>					Diag/Avg of Off-Diags
C <sub>1</sub>		Violent	Property	Drugs	Public Order	Others	
White	Violent	<b>37.5</b>	23.6	8.2	15.6	15.1	2.4
	Property	13.3	<b>57.4</b>	7.0	11.6	10.7	5.4
	Drugs	14.4	26.2	<b>35.9</b>	12.9	10.6	2.2
	Public Order	22.1	23.3	9.5	<b>33.1</b>	12.0	2.0
	Others	18.3	32.4	8.5	14.8	<b>26.1</b>	1.4
	Avg		21.1	32.6	13.8	17.6	14.9
Black	Violent	<b>37.0</b>	28.4	12.2	11.8	10.6	2.3
	Property	20.1	<b>53.0</b>	9.1	10.7	7.2	4.5
	Drugs	15.1	16.9	<b>52.5</b>	11.4	4.1	4.4
	Public Order	19.3	19.8	9.9	<b>45.5</b>	5.5	3.3
	Others	24.4	36.1	12.6	11.7	<b>15.2</b>	.7
	Avg		23.2	30.8	19.3	18.2	8.5

Table 4.7b. Crime switch matrix for arrestees  $5 < t \leq 10$

		C <sub>2</sub>					Diag/Avg of Off-Diags
C <sub>1</sub>		Violent	Property	Drugs	Public Order	Others	
White	Violent	<b>33.7</b>	20.8	15.3	16.3	14.0	2.0
	Property	15.2	<b>44.9</b>	12.2	11.0	16.8	3.3
	Drugs	20.2	29.5	<b>27.9</b>	9.3	13.2	1.5
	Public Order	36.0	18.8	15.2	<b>15.7</b>	14.2	.7
	Others	19.7	24.9	16.8	8.7	<b>30.1</b>	1.7
	Avg		25.0	27.8	17.5	12.2	17.7
Black	Violent	<b>41.0</b>	20.3	23.4	8.4	7.1	2.8
	Property	19.5	<b>38.7</b>	25.0	10.6	6.2	2.5
	Drugs	17.9	25.0	<b>46.4</b>	6.0	4.8	3.5
	Public Order	27.3	23.2	27.3	<b>19.2</b>	3.0	1.0
	Others	31.5	22.2	22.2	3.7	<b>20.4</b>	1.0
	Avg		27.4	25.9	28.9	9.6	8.3

Table 4.7c. Crime switch matrix for arrestees  $10 < t \leq 15$

		C <sub>2</sub>					Diag/Avg of Off-Diags
C <sub>1</sub>		Violent	Property	Drugs	Public Order	Others	
White	Violent	<b>24.3</b>	25.0	13.9	16.0	20.8	1.3
	Property	12.1	<b>45.6</b>	8.5	12.9	21.0	3.4
	Drugs	21.5	17.7	<b>26.6</b>	11.4	22.8	1.4
	Public Order	32.6	24.7	16.9	<b>10.1</b>	15.7	.4
	Others	15.2	34.3	14.1	9.1	<b>27.3</b>	1.5
	Avg	21.1	29.5	16.0	11.9	21.5	1.6
Black	Violent	<b>31.6</b>	29.0	27.6	7.9	4.0	1.8
	Property	14.4	<b>39.4</b>	29.6	6.1	10.6	2.6
	Drugs	24.2	18.2	<b>27.3</b>	12.1	18.2	1.5
	Public Order	20.0	22.9	34.3	<b>14.3</b>	8.6	.7
	Others	28.0	48.0	8.0	8.0	<b>8.0</b>	.3
	Avg	23.6	31.5	25.4	9.7	9.9	1.4

Table 4.7d. Crime switch matrix for arrestees  $15 < t$

		C <sub>2</sub>					Diag/Avg of Off-Diags
C <sub>1</sub>		Violent	Property	Drugs	Public Order	Others	
White	Violent	<b>32.5</b>	17.8	15.4	10.7	23.7	1.9
	Property	18.1	<b>32.0</b>	12.3	9.1	28.5	1.9
	Drugs	23.0	16.2	<b>29.7</b>	8.1	23.0	1.7
	Public Order	17.1	15.9	18.3	<b>9.8</b>	39.0	.4
	Others	25.4	14.0	8.8	11.4	<b>40.4</b>	2.7
	Avg	23.2	19.2	16.9	9.8	30.9	1.7
Black	Violent	<b>33.9</b>	26.2	23.1	7.7	9.2	2.0
	Property	25.6	<b>29.1</b>	37.2	3.5	4.7	1.6
	Drugs	22.6	16.1	<b>41.9</b>	6.5	12.9	2.9
	Public Order	27.6	13.8	27.6	<b>13.8</b>	17.2	.6
	Others	31.6	36.8	26.3	.00	<b>5.3</b>	.2
	Avg	28.3	24.4	31.2	6.3	9.9	1.5

If this period-specific drug involvement and enforcement in the 1980s are an important factor in explaining the observation that the probability of being rearrested for drug offenses is larger for blacks than for whites, it is possible that the disparity in the hazards between blacks and whites might not be as much as what we saw in Figures 4.2a-4.2c once we focus our attention on non-drug recidivism risk. Focusing on recidivism for crimes other than drugs may be reasonable, given that employers are most likely to be concerned about violence (assaults, rapes, etc.) against customers and co-workers, or property crimes, which could involve stealing property or money from the business entity.

Figures 4.5a-4.5b show the black-to-white ratio of hazards for non-drug offenses for  $C_1 =$  Violent, Property.<sup>69</sup> Compared to Figures 4.2a-4.2c, the ratio declines and approaches unity faster, especially for  $C_1 =$  Property. This indicates that at least part of the explanation for the blacks' higher hazard is due to the period effect of the 1980s when blacks were disproportionately arrested for drugs. After taking into account that period effect, those blacks who succeed in remaining arrest free for over 10-15 years are much more similar to whites in terms of their future recidivism risk.

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<sup>69</sup> The confidence intervals of the hazard ratio for  $C_1 =$  Drugs are too wide to make reasonable interpretations.

Figure 4.5a. Hazard ratio estimates (B/W) for non-drug offenses with confidence intervals,  $C_1 = \text{Violent}$

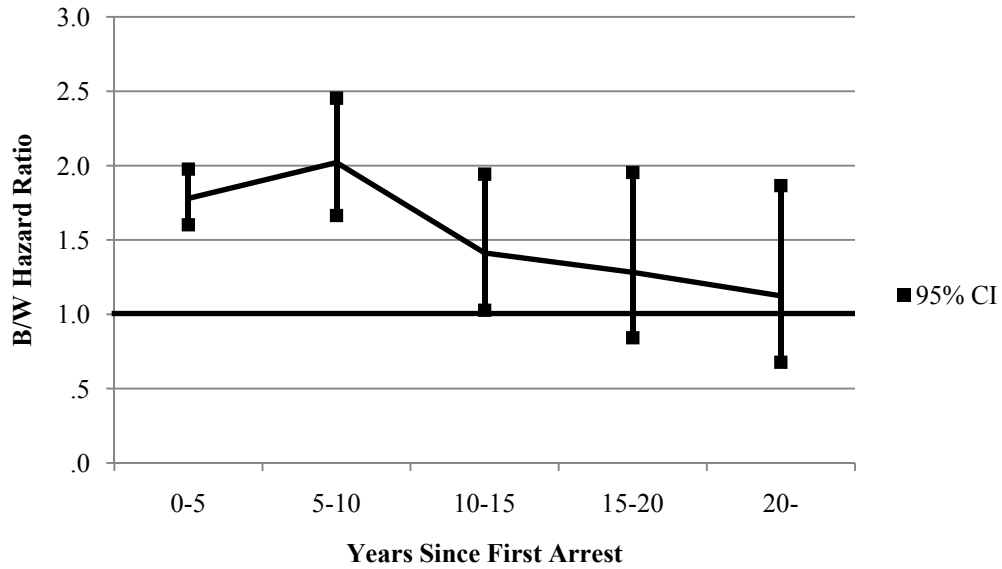
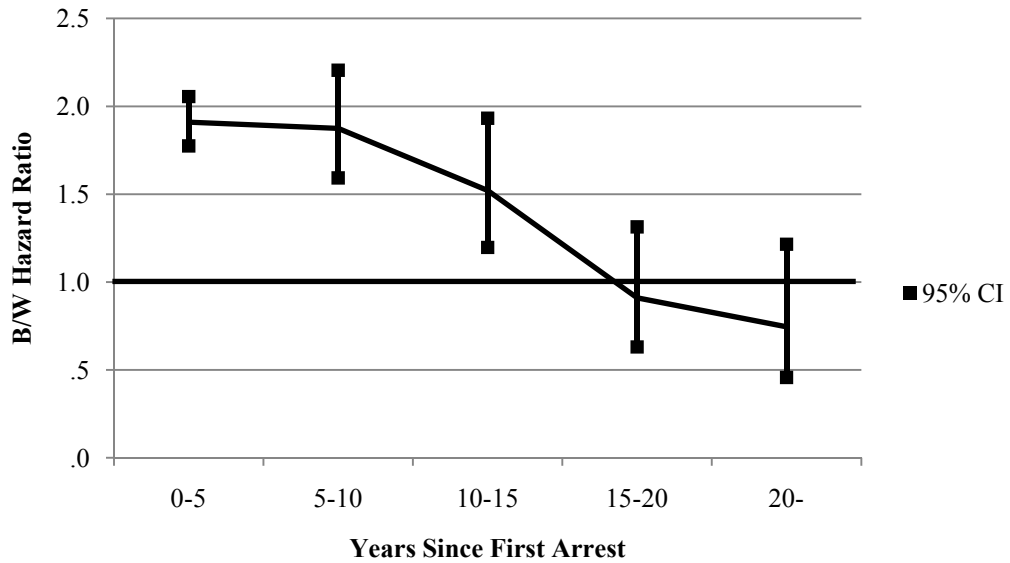


Figure 4.5b. Hazard ratio estimates (B/W) for non-drug offenses with confidence intervals,  $C_1 = \text{Property}$



#### 4.5.4 Comparison of Prevalence and Hazard Ratios

It is important to compare the black-to-white hazard ratios with the prevalence ratios shown in Table 4.4. First, it is seen that the hazard ratios are appreciably smaller than the prevalence ratios. This undoubtedly reflects the difference of the two concepts: prevalence represents the fraction of people drawn from the total population who are arrested, whereas hazard represents the fraction of people with a prior record who are arrested. Thus, they represent the selection from two different base populations – prevalence: total population, hazard: those with a prior who have stayed clean so far. Thus, the selection differences associated with arrest could account for the fact that the black-to-white hazard ratios are roughly half the comparable prevalence ratios.

It is also important to consider the fact that the blacks' hazard becomes similar to whites' over time, as evidenced by the declining black-to-white hazard ratio. Thus, as the arrest-free period increases, the difference between the prevalence ratio and the hazard ratio becomes larger. This could be due to a combination of many factors, but it can be speculated that staying clean for a substantial length of time is an important indication that those with a prior criminal record have made efforts to turn their life around and succeed in integrating into society by committing themselves to a stable marriage, gaining stable employment, and distancing themselves from environments that are highly susceptible to involvement in crime. Thus, those who stay clean for a long time are of comparably low risk of recidivism *regardless of race* because those are the ones who most surely managed to straighten their lives.<sup>70</sup>

These results have important implications for employers and policy makers, such as the EEOC. There is reason to suspect that employers' perceived risk of future crime, posed by white

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<sup>70</sup> Evident in the large difference in survival probabilities, only a relatively smaller fraction of African-Americans with a prior arrest manage to stay clean. However, given that African-Americans are faced with disproportionate socioeconomic disadvantages and employment discrimination, those African-Americans who survive without subsequent involvement in crime for a long time might well have had to overcome more obstacles than their white counterparts.

and black applicants, is shaped by the racial difference in arrest prevalence. Arrest prevalence can be calculated using publicly available data from the UCR and the Census. It is also true that racial difference in arrest prevalence is often the statistic that is used to highlight blacks' higher likelihood of engaging in crime (or being involved in the criminal justice system). The results suggest that employers should be aware that the racial difference in arrest prevalence does not accurately reflect the risk difference of white and black applicants whose crime occurred long ago.

The possibility exists that employment discrimination against black applicants, which may well be caused by the employers' use of prevalence difference, further diminishes blacks' employment opportunities. Given this possibility and the finding that whites and blacks who stay crime free for a long period of time have a similar risk of future crime, it is important to develop empirically supported policies to encourage providing equal employment opportunities to people with stale criminal records, regardless of race. The difference in the risks at some point diminishes appreciably, a point not recognized in short-time recidivism studies.

## **CHAPTER 5: EMPLOYERS' CONCERN ABOUT C<sub>2</sub>**

### **5.1 Employers' Liability Concerns and Lack of Guidelines on Redemption**

Employers are increasingly aware of the risk of liability for negligent hiring (Hahn, 1991; Harris and Keller, 2005; Jacobs and Crepet, 2008; Holzer, Raphael, and Stoll, 2004). Negligent hiring occurs when an employer fails to screen an employee who caused injury given some prior indication that that the employee is likely to cause such injury (Scott, 1987). Many commercial vendors of criminal records strongly advertise the need for pre-employment criminal background checking and caution employers that failure to conduct such checks is likely to result in considerable financial, as well as reputational, cost to the employers (Jacobs and Crepet 2008).<sup>71</sup> Given the growing availability of criminal background-checking services to employers, screening job applicants based on criminal history records has become a common practice for a wide range of employers, not limited to the employers that deal with job positions that involve security-sensitive responsibilities or serve vulnerable populations (Bureau of Justice Statistics, 2001).

Employers and criminal background checking providers recognize the positive relationship between past criminal conduct and future criminal involvement; a robust finding in criminology literature (Brame, Bushway, and Paternoster, 2003; Nagin and Paternoster, 2000). While studies seem to support employers who would avoid hiring anyone with a criminal-history record, the employers' decision to exclude a potential employee with a criminal record has legal bounds, and a blanket exclusion based solely on the presence of a criminal record is often prohibited.

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<sup>71</sup> See, for example, one such vendor, Kroll, <http://www.infolinkscreening.com/Infolink/Background/BackgroundScreening.aspx>.

Title VII of the Civil Rights Act of 1964 prohibits employers from denying employment to job applicants based on their race, sex, religion, or national origin. The Equal Employment Opportunity Commission (EEOC) has determined that refusing to hire applicants based on their criminal record may violate Title VII because the employers' use of criminal records will have a "disparate impact" on the protected groups under Title VII (Harris and Keller, 2005; Legal Action Center n.d.). The EEOC stated that employers may base their hiring decision on the presence of criminal records only if they can demonstrate "business necessity" (EEOC, 1987). In order for employers to establish a business necessity defense, they need to take into account the following three factors: 1) the nature and gravity of the offense, 2) the time that has passed since the conviction or completion of the sentence, and 3) the nature of the job held or sought (EEOC, 1987). Chapter 2 directly addresses the second factor, the time limit on the relevance of criminal records. The third factor poses a challenge to employers in determining how the nature of the job position is related to the nature and the level of the risk of crime that the job applicants with criminal records are likely to cause. As Harris and Keller (2005) point out, the assumption underlying the third factor is that the offense type of the prior crime event, which employers can know by running background checks, has a predictive relationship with the type of crime that the employers are concerned about.

## **5.2 Employer's Concern about Particular Crime Types**

Ch. 2 examined the hazard of a new arrest for any crime type. In reality, however, many employers are concerned about a job applicant's risk of committing a particular type of crime rather than committing any crime. For example, employers who are looking for someone to work as a cashier are probably concerned mostly about an applicant's risk of committing a property

crime, whereas if they are looking for someone to drive a paratransit bus for the elderly, the crime of most concern is more likely to be a violent crime.

Suppose there is a job position that is sensitive to the risk of violent crimes ( $C_2 = \text{Violent}$ ), and the employer has a pool of job applicants with criminal records of a variety of crime types ( $C_1$ ). Then, the employer would be interested in comparing the risk of committing a violent crime among the applicants depending on their individual  $C_1$ 's.

When evaluating applicants with criminal records, employers consider the applicants' risk of future crime, but they can only observe the record of crimes that already occurred. Thus, it is important to consider the crime types of both the first arrest ( $C_1$ ) and the second arrest ( $C_2$ ) and probabilistically quantify which  $C_1$  (known to employers) is likely to lead to which  $C_2$  (unknown to employers).

### **5.3 Data**

We continue to use the criminal history data of our cohort of first-time arrestees in 1980 in New York State. In addition to the age at first arrest ( $A_1$ ) and the crime type of the first arrest ( $C_1$ ), we now consider possible crime types of a second arrest ( $C_2$ ). As in the previous chapters, we focus on the arrestees who are also convicted and consider the range of  $A_1$  between 19 and 30 and five crime types (Violent, Property, Drugs, Public Order, and Others) for  $C_1$  and  $C_2$ .

### **5.4 Approaches and Results**

#### **5.4.1 Crime Switch Matrix**

As used in Ch. 4, crime-switch matrices in Tables 5.1 display the combination of crime type of first arrest (the rows) and the probability of different crime types of second arrest (the columns,

including the possibility of no second arrest) for  $A_1 = 19-20$ .<sup>72</sup> We consider three such matrices by the timing of their second arrest: G1) those who have a second arrest within the first 5 years (Table 5.1a), G2) those who stay clean in the first 5 years and have a second arrest between 5 and 10 years (Table 5.1b), and G3) those who stay clean for the first 10 years and have a second arrest after 10 years or who do not have a second arrest for the entire observation period ( $> 25$  years) (Table 5.1c). By this construction, each one of the convictees belongs to one and only one of the disjoint groups (G1, G2, G3) depending on the timing of their second arrest. We also consider similar crime switch matrices for  $A_1 = 25-30$  in Tables 5.2. By comparing the two  $A_1$  groups, we examine how the relationship between  $C_1$  and  $C_2$  could depend on  $A_1$ .

The last column contains the ratio of the diagonal values (the probability of repeating  $C_1$ ) to the average of the off-diagonal values for some  $C_2$ , i.e., excluding the column of “No rearrest” in Table 5.1c.

For  $A_1 = 19-20$ , it is not surprising that G1 (Table 5.1a) shows a relatively strong propensity to recidivate to the same crime type because the interval between the first and second arrests is rather short.<sup>73</sup> Among the five crime types, those with  $C_1 = \text{Property}$  show the strongest propensity to repeat the same crime for the second arrest, 4 times more likely than the average of committing the other four crime types. Despite the assumed association between drug offenders and violence, which in part provided the justification to enact the “tough on drugs” legislation

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<sup>72</sup> The entries in the crime-switch matrix are  $P_{ij}$  representing the probability that someone who is arrested for a crime of type  $i$  in 1980 will commit a crime of type  $j$  in his/her second arrest.

<sup>73</sup> As discussed in Ch. 2, in some cases, we find that an arrest is followed quickly by another arrest. We are concerned that what seems to be a new “arrest” might be related to the same crime event as the prior arrest (e.g., transfer to a different jurisdiction), so we count an arrest as a new arrest only if it occurs at least 30 days after the prior arrest.

(e.g., mandatory minimum sentences), first-time drug offenders' probability of recidivating to violence is low, the lowest among the five  $C_1$ 's in G1.

The propensity to commit the same crime types tends to be lower for G2 than G1 as the time between the first and second arrests increases. One exception is for those with  $C_1 = \text{Drugs}$ , who show a somewhat stronger inclination to be rearrested for drugs than in G1. This could be due to the growing crack market during the period of mid to late 80s. Those who have a history of involvement in drugs ( $C_1 = \text{Drugs}$ ) could have been particularly vulnerable to re-engaging in and being arrested for drugs despite the fact that they remained arrest free for at least 5 years.

In Table 5.1c, similar patterns are found in the crime switch matrix of G3. It is important to first recognize that the probability of no subsequent arrest for those who stayed clean for at least 10 years is rather high (80-85%).<sup>74</sup> The propensity to commit the same crime types is relatively weak, again except for drug offenders who still show a strong propensity to be rearrested for the same crime type (close to 3 times more likely than the average probability of being rearrested for any other crime types).

For those with  $A_1 = 25-30$ , their probability of having no subsequent arrest is slightly larger than the younger counterparts, as shown in Tables 5.2b-5.2c. Their propensity to commit the same crime types tends to be larger than the younger offenders for  $C_1 = \text{Violent and Property}$ . It is interesting that older offenders who are initially arrested for violent or property offenses are more likely to return to the same crime than younger offenders. It could be speculated that older offenders might specialize in one type of crimes. Among those with  $C_1 = \text{Drugs}$  who stay clean at least 5 years, older offenders do not show as strong a tendency to repeat a drug offense as younger offenders. Again, considering the period where there is an increasing effect of the crack

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<sup>74</sup> For those who stay clean longer than 5 years, the probability of no subsequent arrest is just over 70% except for  $C_1 = \text{Violent}$ , which is 61%. Thus, staying clean for an additional 5 years is associated with about a 10 percentage-point increase in the probability of no subsequent arrest.

market, older drug offenders might have been less involved in crack than their younger counterparts (Blumstein, 1995).

Table 5.1a. Crime switch matrix for G1 ( $0 < t \leq 5$ ),  $A_1 = 19-20$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others	
Violent (439)	35.8	30.1	10.3	12.5	11.4	2.2
Property (958)	19.1	51.8	8.7	12.8	7.6	4.3
Drugs (218)	15.6	24.3	43.6	12.8	3.7	3.1
Public Order (383)	18.3	19.6	11.0	42.3	8.9	2.9
Others (189)	20.6	28.0	13.8	16.9	20.6	1.0
						Avg = 2.7

Table 5.1b. Crime switch matrix for G2 ( $5 < t \leq 10$ ),  $A_1 = 19-20$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others	
Violent (126)	32.5	17.5	19.8	18.3	11.9	1.9
Property (217)	19.4	32.7	19.4	14.3	14.3	1.9
Drugs (53)	13.2	22.6	47.2	9.4	7.6	3.6
Public Order (70)	30.0	15.7	20.0	20.0	14.3	1.0
Others (45)	24.4	13.3	17.8	15.6	28.9	1.6
						Avg = 2.0

Table 5.1c. Crime switch matrix for G3 ( $t > 10$ ),  $A_1 = 19-20$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					No rearrest	Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others		
Violent (406)	6.4	3.9	4.4	3.0	3.0	79.3	1.8
Property (1335)	3.2	4.3	2.9	1.3	3.4	84.9	1.6
Drugs (275)	3.3	1.5	6.6	0.7	3.6	84.4	2.9
Public Order (371)	4.9	3.2	1.9	1.4	2.7	86.0	.4
Others (288)	3.1	4.5	2.1	1.7	5.2	83.3	1.8
							Avg = 1.7

Table 5.2a. Crime switch matrix for G1 ( $0 < t \leq 5$ ),  $A_1 = 25-30$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others	
Violent (267)	36.3	25.1	9.7	16.9	12.0	2.3
Property (652)	12.3	61.5	8.1	9.5	8.6	6.4
Drugs (207)	10.1	17.9	52.7	11.6	7.7	4.5
Public Order (248)	13.7	15.7	8.1	55.2	7.3	4.9
Others (143)	20.3	30.1	12.6	8.4	28.7	1.6
						Avg = 3.9

Table 5.2 b. Crime switch matrix for G2 ( $5 < t \leq 10$ ),  $A_1 = 25-30$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others	
Violent (91)	30.8	19.8	22.0	14.3	13.2	1.8
Property (152)	13.2	46.1	17.8	10.5	12.5	3.4
Drugs (52)	19.2	23.1	44.2	5.8	7.7	3.2
Public Order (65)	30.8	24.6	20.0	21.5	3.1	1.1
Others (43)	18.6	30.2	14.0	7.0	30.2	1.7
						Avg = 2.2

Table 5.2c. Crime switch matrix for G3 ( $t > 10$ ),  $A_1 = 25-30$  ( $n$  is in brackets)

C <sub>1</sub>	C <sub>2</sub>					No rearrest	Diag/Avg of Off-Diags
	Violent	Property	Drugs	Public Order	Others		
Violent (513)	6.0	2.9	2.7	2.3	1.6	84.4	2.5
Property (1141)	2.4	5.7	2.1	1.3	2.5	86.1	2.8
Drugs (368)	1.9	2.7	5.4	1.6	2.7	85.6	2.4
Public Order (403)	3.5	1.0	2.7	0.7	3.5	88.6	.3
Others (332)	3.6	4.8	0.9	1.2	3.9	85.5	1.5
							Avg = 1.9

### 5.4.2 Crime Type-Specific Hazard

While a crime-switch matrix is informative in characterizing the crime types to which the 1980 arrestees recidivate, and by breaking it into three G's, the matrices inform us about the temporal patterns of C<sub>1</sub>-C<sub>2</sub> interactions, we are interested in more explicitly examining how the risk of recidivism to certain C<sub>2</sub>'s changes over time. In order to estimate the hazard for recidivism to a particular crime type, we used type-specific hazard (Allison, 1995; Kalbfleisch and Prentice, 2002). Type-specific (or sometimes called "cause-specific") hazard approximates the conditional probability of having a new arrest for a particular crime type at time  $t$  given surviving without a new arrest until time  $t$ . In effect, we remove the observation from the risk set of all other types of events once one type of event occurs.<sup>75</sup> In the context of our study, if an individual is first rearrested for a particular C<sub>2</sub> crime type, we ignore any risk of rearrest for all the other crime types.

Figure 5.1a shows the hazards of rearrest for a violent offense for each of four C<sub>1</sub>'s. First, we notice that the hazard of rearrest for violence is much lower (less than a half) than the hazard

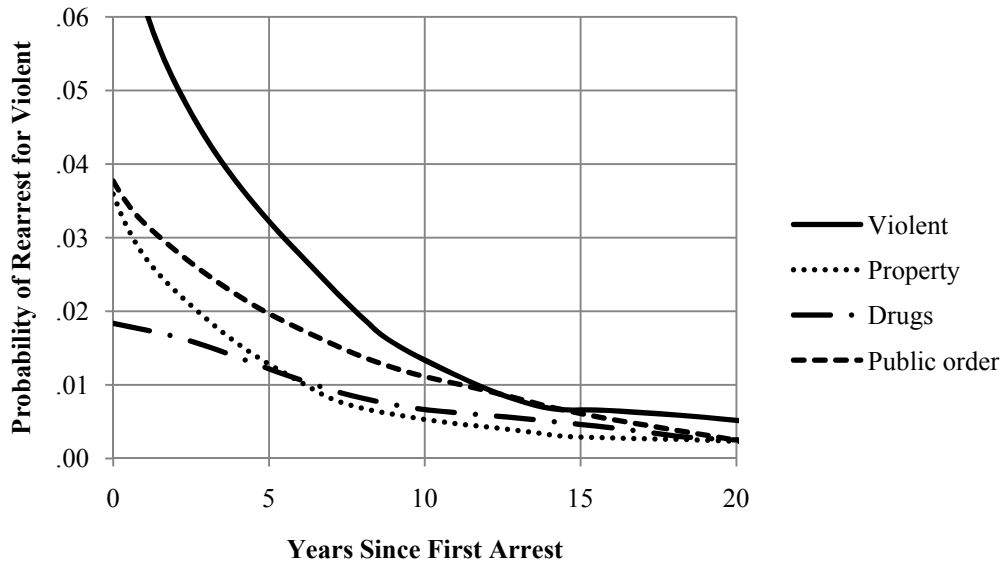
<sup>75</sup> The concept of type-specific hazard has been used in the competing risks analysis in criminology (e.g., Escarela, Francis, and Soothill (2000); Lattimore, Visher, and Linster (1995))

of rearrest for any crime, which we estimated in Ch.2. Thus, employers should be aware that the risk of future crime by a potential employee with a prior record of a particular crime ( $C_1$ ) is less if they are only concerned about a selective set of crimes such as only violent crimes.

As implied by the crime switch matrixes above, those whose first arrest in 1980 was for violence tend to have a higher risk of violence for about 10 years than those whose first arrest was for the other four crimes. This suggests that for the employers who are particularly concerned about the potential employee's risk of violence, a prior record of violence, which is as old as 10 years, indicates at most a probability of .015 if  $C_1 = \text{Violent}$  and 20% less, or .012, if  $C_1 = \text{Property}$ .

It was shown earlier in the discussion of crime switch matrices that the probability of violent recidivism for drug offenders is relatively low. The hazard curves show consistent evidence that the hazard of rearrest for violence is lowest for  $C_1 = \text{Drugs}$ , less than half the hazard for  $C_1 = \text{Violent}$ . Thus, there does not seem to be evidence to support particularly severe penalties for drug offenders based on the claim that drug offenders have a high likelihood of engaging in violence.

Figure 5.1a.  $h(t)$  of  $C_2 = \text{Violent}$ ,  $A_1 = 19-20$ , for Four  $C_1$ 's



Similarly, as shown in Figure 5.1b, those with  $C_1 = \text{Property}$  have a higher hazard of rearrest for property offenses than those with other  $C_1$ 's. In contrast to Violent, those with  $C_1 = \text{Violent}$  have the highest hazard for  $C_2 = \text{Violent}$  for about 10 years, the hazards for those with  $C_1 = \text{Property}$  seem to converge to the same rearrest probability as the other four crime types at about  $t = 5$ . Thus, a prior record of a property offense seems to lose its relevance in predicting a subsequent property crime faster than a prior violent crime in predicting a subsequent future violent crime.

Figure 5.1b.  $h(t)$  of  $C_2 = \text{Property}$ ,  $A_1 = 19-20$ , for 4  $C_1$ 's



Figures 5.2a-5.2b show the same hazards (for  $C_2 = \text{Violent}$  and  $\text{Property}$ ) for  $A_1 = 25-30$ . Again, for  $C_2 = \text{Violent}$ , a prior record of violence is associated with a higher risk of violent recidivism than the prior records of other crime types. However, the magnitude of the risk of rearrest for violence is about half that of those with  $A_1 = 19-20$ , which is consistent with the results in Ch. 2 that older ages at first arrest is associated with lower hazards of rearrest.

For  $C_2 = \text{Property}$ , a prior record of property crimes is not only associated with the highest hazard, but its magnitude is about the same as its younger counterparts ( $A_1 = 19-20$ ). Considering the general tendency that those with older  $A_1$ 's have lower hazards, this result could suggest a particularly strong propensity of recidivating to the same crime for older property offenders. This is consistent with the finding from the crime switch matrix for  $A_1 = 25-30$  (Tables 5.2).

Figure 5.2a.  $h(t)$  of  $C_2 = \text{Violent}$ ,  $A_1 = 25-30$ , for 4  $C_1$ 's

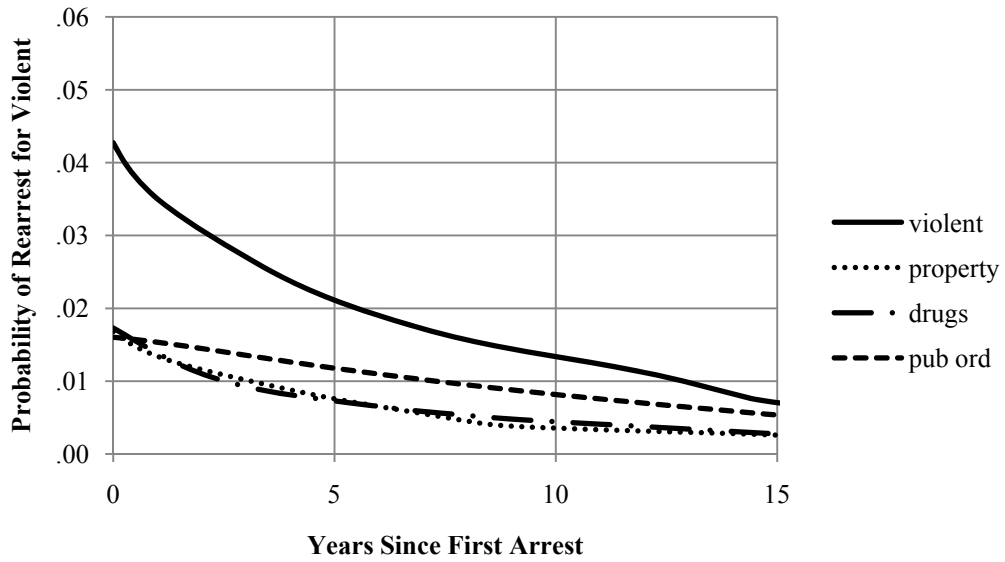
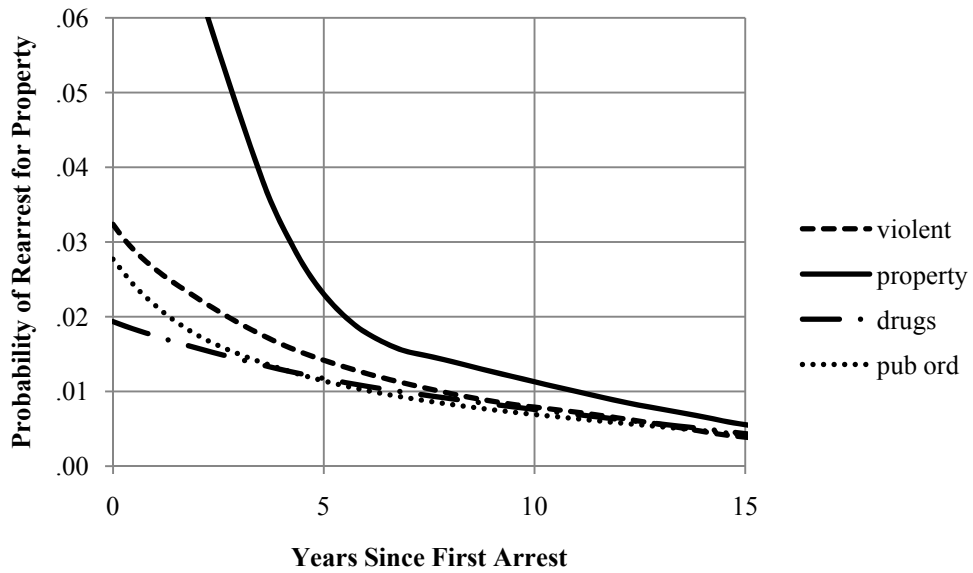


Figure 5.2b.  $h(t)$  of  $C_2 = \text{Property}$ ,  $A_1 = 25-30$ , for 4  $C_1$ 's



### 5.4.3 $C_2$ -specific Benchmarks and Redemption Time Estimates

In order to estimate redemption times  $T^*$  as a function of  $C_1$ - $C_2$  combinations, we need appropriate age-crime curves. Since we are interested in the  $C_2$ -specific benchmark arrest risk, the age-crime curves are constructed for each particular crime type  $C_2$ .

Figure 5.3 compares the age-crime curve covering all crime types (except DUI)<sup>76</sup> and the  $C_2$ -specific age-crime curves (for Violent and Property).<sup>77, 78</sup> Clearly, the likelihood of arrest for violent and property offenses in the general population is much smaller than the likelihood of arrest for all crimes.<sup>79</sup> This has important implications for estimating redemption times because if the hazards were stable, lowering benchmarks for redemption would increase the length of time to redemption.

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<sup>76</sup> This is the same age-crime curve that was used to estimate redemption times in Ch. 2.

<sup>77</sup> The age-crime curves are corrected for the fact that the number of arrests from the UCR is greater than the number of arrestees. More detailed discussion about this correction can be found in Ch. 2.

<sup>78</sup> Since it is reasonable to believe that the crime types of primary concern for employers are violent and property offenses, we focus on these two crime types as  $C_2$  in the discussion of redemption times.

<sup>79</sup> About 40-50% of non-DUI arrests in NY are categorized as “All Other Offenses,” which include all violations of state or local laws that are not part of the FBI’s Part I or Part II offenses.

Figure 5.3. Age-crime curves for Overall, Violent, Property

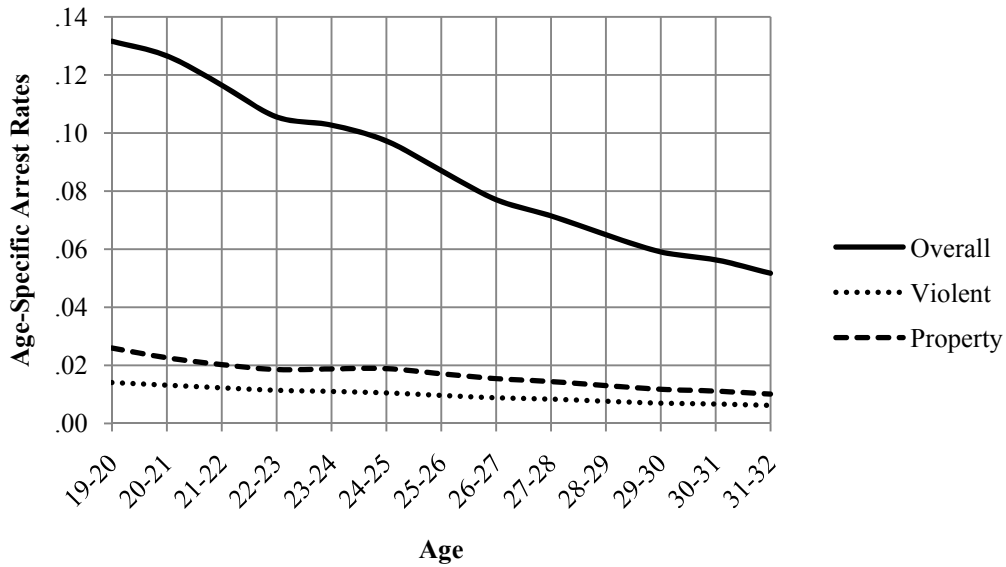


Table 5.3 shows the redemption time estimates  $T^*$  for various  $C_1$ 's for  $C_2 = \text{Violent}$  and for  $C_2 = \text{Property}$  for  $A_1 = 19-20$  and  $25-30$ . The redemption times for  $A_1 = 19-20$  are estimated as time points when the  $C_2$ -specific hazards intersect the  $C_2$ -specific age-crime curves. Although younger offenders ( $A_1 = 25-30$ ) tend to have higher hazards, the age-crime curve for the older offenders is much lower than the age-crime curve for the younger offenders ( $A_1 = 19-20$ ). This difference could result in appreciably longer estimated redemption times for older offenders. Thus, we estimate  $C_2$ -specific redemption times for  $A_1 = 25-30$  as time points when the  $C_2$ -specific hazards fall below the probability of rearrest at the estimated redemption for  $A_1 = 19-20$ . This translates to the following situation: if an employer, who is concerned about  $C_2 = \text{Violent}$ , is willing to accept the risk associated with 19-20 year olds with  $C_1 = \text{Property}$  who stayed clean for 6.5 years after their arrest, that same employer can accept that same risk with 25-30 year olds who have stayed clean for 3.6 years.

First, when  $C_1 \neq C_2$ , the redemption estimates are larger for younger  $A_1$ 's, which is consistent with the fact that the hazards for younger offenders tend to be higher than the hazards

for older offenders. However, it is important to recognize that when  $C_1 = C_2$ , the redemption estimates for older offenders are similar to or even slightly higher than those for younger offenders. This result has an important implication: in contrast to the redemption estimates in the context of  $C_2 = \text{Any}$ , which tend to be shorter for older  $A_1$ 's for all  $C_1$ 's, when the concerning crime ( $C_2$ ) is the same as the crime type of the prior record, the redemption estimates are similar across the two  $A_1$  groups.

Although the hazard for  $C_2 = \text{Property}$  is highest for  $C_1 = \text{Property}$ , the redemption times are similar across four different  $C_1$ 's (the average is about 5 years) for  $A_1 = 19-20$ , because the hazards for the different  $C_1$ 's converge relatively early. In contrast, for  $A_1 = 25-30$ ,  $C_1 = \text{Property}$  is associated with a longer redemption time than the other  $C_1$ 's. For  $C_2 = \text{Violent}$ ,  $C_1 = \text{Violent}$  is clearly an indication of longer redemption times for both categories of  $A_1$ .

Table 5.3. Values of  $T^*$  by  $C_1$ ,  $A_1$ , and  $C_2$

$C_2$	$C_1$	$A_1$	
		19-20	25-30
Violent	Violent	10.8*	11.5
	Property	6.5	3.6
	Drugs	7.6	3.6
	Public Order	8.5*	3.6
Property	Violent	5.4	3.3
	Property	5.1	5.8
	Drugs	5.2	< 1
	Public Order	4.1	1.6

\* The values are estimated using  $\delta = .005$ , since the hazards do not cross the age-crime curves.

Table 5.4 shows the values of redemption times estimated at the intersection of the upper confidence intervals of the hazards based on bootstrap samples and the age-crime curves. These estimates account for the hazard estimates' different levels of uncertainty across combinations of

$A_1$  and  $C_1$ . The important patterns of redemption times are similar to the patterns based on point estimates above. When  $C_1 \neq C_2$ , the redemption estimates are larger for younger  $A_1$ 's, whereas when  $C_1 = C_2$ , the redemption estimates for older offenders are similar to those for younger offenders.

Table 5.4 Values of  $T^*$  by  $C_1$ ,  $A_1$ , and  $C_2$  (using the upper CI and  $\delta = .005$ )

$C_2$	$C_1$	$A_1$	
		19-20	25-30
Violent	Violent	14.0	13.3
	Property	4.6	1.5
	Drugs	5.2	2.1
	Public Order	12.6	8.9
Property	Violent	6.1	3.3
	Property	5.2	5.7
	Drugs	4.8	1.3
	Public Order	4.2	1.6

Employers who are willing to hire individuals with a prior criminal record have been aware that not all crimes by potential employees are equal. Some are concerned about violence because the job position involves interactions with vulnerable populations, but they might be tolerant about employees' disorderly conducts outside of the workplace. In addition, the EEOC has recently been increasing its pressure on employers to demonstrate how relevant an employee's prior record is to the nature of a job position. Yet, there has been little known about the extent to which a prior record of particular crime types is predictive of the risk of particular crimes. The results in this chapter provide some guidance on that issue. Employers should be informed that if they are primarily concerned about violence or property crimes, a prior record of violence and property crime respectively is associated with the highest risk of recidivism and the longest

redemption times. This connection between the prior crime type and the crime type of recidivism does not seem to vary with the ages at which the prior record is obtained. Thus, when the prior crime type is the same as the crime type of concern, employers may be advised to expect the risk of recidivism to be similar between those who have their prior at different ages. Employers should also recognize that the risk of rearrest for a particular crime type is very low after  $t > 10$ , much lower than the risk of rearrest for any crime type. For example, the hazard of rearrest for any crime type at  $t = 10$  for  $A_1 = 19-20$  and  $C_1 = \text{Violent}$  is about .05, whereas the hazard of rearrest for violent offenses at  $t = 10$  for the same  $A_1$  and  $C_1$  is close to .01.

## **CHAPTER 6: POLICY IMPLEMENTATION**

### **6.1 Early Background**

The idea of redemption or a second chance is not new. In the 1960s and the 1970s, law reformers advocated for a legal system that limits the various negative consequences of carrying a criminal record and helps facilitate the reintegration of ex-offenders into the society (Love, 2003). In 1956, the Attorney General of the United States called the National Conference on Parole, which was sponsored jointly by the United States Board of Parole and the National Council on Crime and Delinquency. The conference recommended the abolition of laws that restrict the rights of those with criminal records and the adoption of laws to expunge the record of conviction and disposition (Bell, 1957; Love, 2003). The National Council on Crime and Delinquency (NCCD) published a model statute that provides discretionary power to the judge of the sentencing court to annul convictions and in effect authorizes an individual with an annulled conviction to say “in testifying or in filling out applications of various kinds, that he had not been convicted” (NCCD, 1962).

In the same year as the NCCD released the model act, the American Law Institute (ALI) proposed the Model Penal Code. In contrast to the NCCD proposal, the ALI’s proposal provided a more nuanced approach to restoring the rights and status of ex-offenders (Love, 2003). In the proposal, the sentencing court would be empowered to remove the disqualification disability as a result of the conviction after the sentence is complete, and “vacate” the judgment of conviction after 5 years of good behavior (ALI, 1985). It is notable that the proposal does not justify the

removal of the conviction record but in spirit acknowledges the rehabilitation process by specifying a required length of good behavior.<sup>80</sup>

After the reform movements in the '60s through the '70s came the two decades of retribution- focused, “tough on crime” policies. Many barriers were erected to prevent those with criminal records from pursuing employment opportunities, while limiting the means available for them to restore their rights and status. The computerized criminal background checks and the increasing concern of negligent-hiring liability among employers brought about a situation where criminal records can be a lifetime handicap.

The findings and approaches that are presented in this dissertation will be of considerable value in enhancing redemption opportunities and consequent employment opportunities for individuals who made a mistake in the past but have since lived a lawful life. Despite good intentions, past reforms that are designed to relieve the handicapping effect of criminal records suffer from a limitation of lack of empirical support. For example, to the best of our knowledge the ALI's 5-year period of good behavior to vacate a conviction is not based on reliable empirical evidence. The knowledge of redemption times along with the consideration of various conditions put forth (age, crime type, race, etc.) could provide the empirical evidence to formulate reasonable redemption policies.<sup>81</sup> Discussed below are ways in which the information

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<sup>80</sup> An order to vacate the judgment of conviction “does not preclude proof of the conviction as evidence of the commission of the crime” (ALI, 1985: 259).

<sup>81</sup> The current political climate seems more supportive of such empirical evidence: for example George W. Bush's “second chance” in his 2004 State of the Union Address (<http://www.gpoaccess.gov/sou/index.html>), the Second Chance Act of 2007 was signed into law in 2008 ([http://reentrypolicy.org/announcements/bush\\_sign\\_SCA](http://reentrypolicy.org/announcements/bush_sign_SCA)), and President Obama's agenda includes “Reduce Crime Recidivism by Providing Ex-Offender Supports” ([http://www.whitehouse.gov/agenda/urban\\_policy/](http://www.whitehouse.gov/agenda/urban_policy/)).

about redemption times could be used by various pertinent parties to facilitate the redemption process.

## **6.2 Users of Criminal Records**

### **Employers**

Employers who run background checks on job applicants could be given a brief document informing them of the diminished value of records older than redemption times for risk assessment purposes.<sup>82</sup> Since employers have a strong concern about liability suits, a statute could protect them from such due-diligence vulnerability in case they hire someone whose last arrest was longer ago than redemption times.<sup>83</sup> This would be a relief for employers who are otherwise willing to hire individuals with criminal records, and would add to the existing incentives such as Work Opportunity Tax Credit (WOTC) and Federal Bonding Program (FBP) (U.S. Department of Labor, 2010).

Such liability-protection statutes could also be applicable to employers that ask applicants about their criminal background, but would limit their inquiries to criminal involvements that are not yet as old as redemption times.

As the EEOC and state and local civil rights agencies aggressively pursue the complaints of disparate impact of background screening based on criminal records, employers should

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<sup>82</sup> Users of background checks should base their decision not only on the information about criminal history but also on information about other important factors (such as employment history, marriage, and educational attainment), and especially actions taken by the redemption candidate since the last criminal-history event.

<sup>83</sup> Although such legal protections would most likely be welcomed by employers, their concern over possible damage to the organization's reputation would not be eliminated (Fahey, Roberts, and Engel, 2006).

evaluate the three factors of business necessity: 1) the nature and gravity of the offense, 2) how old the conviction is, and 3) how the type of offense committed is relevant to the nature of the job. Consideration of redemption times would satisfy the second factor. In order to meet the third factor, employers should understand the relationship between the crime type of first crime event and the crime type of a possible second arrest. Employers would be better informed by knowing that for violence and property offenses, which are of primary concern to many jobs, a prior criminal record of the same type (violence and property respectively) elevates the risk of recidivism. This knowledge should allow employers to be more selective about the type of criminal record that they should be concerned about.

It is possible that the racial difference in arrest prevalence could affect the way in which employers assess the risk of job applicants and could explain why employers view African-American applicants unfavorably (Pager, 2003). Given the possibility, it is important for employers to be aware that the prevalence difference between whites and blacks does not accurately reflect the racial difference in recidivism for those who have a prior record. Employers should come to realize that the racial difference in recidivism risk is not only much lower than the difference in arrest prevalence, but also diminishes as the age of the criminal record increases.

### **Pardon Boards**

The governor of each state is empowered to grant a pardon as an act of clemency and forgiveness. Most typically, a pardons board reviews relevant information about the individual seeking clemency and makes a recommendation to the governor. Although the length of the law-abiding period is often considered one of the most important factors in pardon applications, it is not clear

whether pardon boards have reliable guidelines as to how long a law-abiding period is long enough for the individual to be deemed appropriate for pardon.<sup>84</sup> Despite the fact that pardons are hard to obtain, especially for the poor, pardons have a significant restorative effect that signals that the pardoned individual is rehabilitated (Love, 2003, 2010).

### **6.3 Distributors of Criminal Records**

#### **State Repositories**

State record repositories could adopt a policy not to disseminate criminal record information older than redemption times. This could apply specifically to the states that make their criminal-history information publicly available on the Internet.<sup>85</sup> States are clearly moving in the direction of making individual criminal records more publicly accessible (Jacobs, 2006). However, given the lasting consequence of disseminated records on a large number of individuals, finding means to limit the dissemination would be a realistic approach to the problem.<sup>86</sup> The state could adopt a policy to seal repository records of events older than redemption times in response to a request

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<sup>84</sup> For example, in Pennsylvania, the Board of Pardons (2005: 1) publicly states that the length of time free of crime after the offense is one of the best indicators of rehabilitation that the applicant can demonstrate.

<sup>85</sup> Samuels and Mukamal (2004) report that 28 states allow Internet access to criminal records.

<sup>86</sup> Some employers might “statistically discriminate” based on correlating individual characteristics of a job applicant with generic covariates of criminal activity such as race and ethnicity. Presence of statistical discrimination implies that if the access to criminal records is limited, employers would infer the potential employee’s probability of having a criminal record based on the accessible information about the person’s characteristics such as race. Thus, those who belong to groups that are associated with a high probability of having criminal records but do not themselves have records would be unfairly discriminated against. As a result, limiting employers’ access to criminal records could possibly have an adverse consequence for those without criminal records (Bushway, 2004; Finlay, 2009; Freeman, 2008; Holzer et al., 2006; Pager, 2003; Raphael, 2006).

from a non-criminal justice agency. Such sealed records could still be accessible for criminal justice purposes. A more aggressive approach would be to expunge the stale records.

Even though these judicial procedures tend to be more accessible and reliable than pardon, the popularity of sealing and expungement peaked in the 1970s and has severely declined since then in most jurisdictions (Love, 2003, 2006). Moreover, Love (2003, 2006) reports that there is no one standard in terms of what it means to have a record sealed, expunged, set aside, vacated, or annulled. A record being expunged does not necessarily mean that the record is literally destroyed; rather, the expunged records “almost always remain available for use by law enforcement agencies and the courts, and in some states they may be accessible to other public agencies and even to private investigative services hired to perform criminal background checks for employers” (Love, 2003). Furthermore, critics of sealing and expungement argue that the concealment of records and the denying of past wrongdoing are institutionalized deception and are not compatible with the pursuit of truth, which is the foundation of a legal system (Franklin and Johnsen, 1980; Kogon and Loughery, 1970).

Despite these criticisms, concealment and denial of criminal records after some “rehabilitation period” are common in many countries. For instance, in the UK, according to the Rehabilitation of Offenders Act 1974, those who are convicted of certain crimes, after specified rehabilitation periods, are treated as though the crime never happened, and are not obligated to reveal the record when asked at employment settings.<sup>87, 88</sup>

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<sup>87</sup> The Rehabilitation of Offenders Act of 1974 followed a report called *Living It Down: The Problem of Old Convictions*, which is a report of a committee chaired by Lord Gardiner (1972). The report shows that the longer a convicted person remains crime free, the less likely that the person will commit another crime.

<sup>88</sup> For more on the sealing and expungement of criminal records in the European Union, see Loucks, Lyner, and Sullivan (1998). There are also similar systems of sealing and expungement of criminal records in countries such as Canada and Australia (Lam and Harcourt, 2003; Ruddell and Winfree, 2006).

## **Commercial Vendors**

Because many employers rely on background-check services provided by commercial vendors of criminal records, if states seal or expunge stale records, this should be accompanied by a process of requiring those old records also to be erased from commercial databases.<sup>89</sup>

### **6.4 Certificate of Rehabilitation**

The main criticisms of sealing and expungement include the compromise of governmental transparency as well as the possible adverse effect on non-offenders because of statistical discrimination. Certificates of rehabilitation and other similar means can circumvent the problem. Certificates of rehabilitation are designed to remove certain collateral consequences for eligible ex-offenders and can potentially enhance their employment prospects.<sup>90</sup> The certificates reward good behavior of ex-offenders by explicitly acknowledging them as being rehabilitated rather than erasing the record of their contact with the criminal justice system. Thus, these certificates are similar to pardons in spirit but are relatively more accessible than pardons. Currently, only a handful of states issue such certificates (Love and Frazier, 2006; Samuels and Mukamal, 2004),

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<sup>89</sup> Given the considerable discrepancy between the records from official sources (state repositories) and the records from commercial databases (Bushway et al., 2007), it is important that any update (i.e., sealing or expungement) that takes place on the official records is reflected on the records in the commercial sources. Jacobs and Crepet (2008) highlight the difficulty in forcing vendors to make such changes because their right to access the criminal records would be protected by the First Amendment of the Constitution.

<sup>90</sup> Criminal history records are regarded as “negative credentials” or a scarlet letter “A” (Nagin, 1998; Kurlychek, Brame, and Bushway, 2006) that signify “social stigma and generalized assumptions of untrustworthiness or undesirability” (Pager, 2007: 33; see also Jacobs, 2006 and Jacobs and Crepet, 2008), whereas certificates of rehabilitation attempt to emphasize the progress made by the ex-offender. Bushway et al. (2007) mention that it is conceivable for the government to devise some score (like a credit score) that indicates the risk of offending, which can be affected by positive factors such as the length of crime-free time, completion of a drug treatment program, and vocational training, as well as negative factors such as committing another crime (for a similar approach, see Freeman, 2008).

but they could be used more widely by taking advantage of the empirical evidence of redemption times.<sup>91</sup>

## **6.5 Ban the “Box”**

Another alternative to sealing and expungement of records is so-called “ban the box” legislation. The “box” refers to a question on job applications that asks prospective employee whether they have ever been convicted of a crime. This type of legislation removes the question about criminal history from the initial job application, although employers can ask about criminal records at later stages of hiring process, and evaluate the risk in light of redemption times. Ban the box’s primary desired effect is to allow applicants’ qualifications and experience to be evaluated before criminal records, which are salient but not necessarily always relevant, enter the hiring decision-making process. This could benefit both applicants and employers. For applicants, their qualifications and experience would receive a fair share of employers’ consideration, and also their employment prospect could improve if they have a chance to interact and build rapport with the employer (Pager et al., 2009). For employers, this would allow them to select the most qualified individuals for the job and would also reduce the likelihood of discrimination lawsuits for being engaged in a blanket exclusion of people with criminal records. So far, the movement to “ban the box” have been largely limited to employment for city and state governments (Henry and Jacobs, 2007; National Employment Law Project, 2008), but the movement is growing. Already, states, such as Minnesota, New Mexico, and most recently Massachusetts have passed legislation to ban the box (Levenson, 2010). Similarly, cities such as San Francisco, Chicago, and Memphis have such legislation (Maki, 2010).

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<sup>91</sup> Bushway and Sweeten (2007) discuss policy implications regarding the diminished value of old criminal records in the context of collateral consequences.

## **CHAPTER 7: SYNTHESIS: CONCLUSIONS, CONTRIBUTIONS, AND FUTURE WORK**

### **7.1 Summary and Conclusions**

As information technology has increased the accessibility of criminal-history records, and concern for negligent hiring lawsuits has grown, there is no doubt that criminal background checking has become an important part of the hiring process for most employers. We have begun to understand the consequences of widespread criminal background checking through numerous stories covered in media about people being denied a job or losing a job, not because of a recent involvement in crime, but because of a criminal record from long ago. We have known from many studies on recidivism that the risk of recidivism declines monotonically as the time since the last crime lapses. However, there is little known empirically about how long it takes for the recidivism risk to decline to a level that is sufficiently low, that is the length of time the process of “redemption” requires. This dissertation provides some guidance on redemption by providing approaches to estimating redemption time, examining the robustness of estimates, and addressing several important, policy-relevant aspects of redemption such as race and the crime type of concern to employers.

We first introduce approaches to estimating redemption time. The risk of recidivism for those with a prior criminal record (“redemption candidates”) is represented by hazard. The hazard is estimated using data of arrest history of all individuals who were arrested for the first time in 1980 in NY. This large dataset with more than 25 years of follow-up provides an ideal opportunity to study recidivism risk for a time span longer than recidivism studies have traditionally dealt with. In order to determine the level of recidivism risk that is deemed sufficiently low, two benchmarks were considered: 1) the risk of arrest for the general population

of the same age, and 2) the risk of arrest for those without a prior record. For the first benchmark, redemption time is estimated as the time point when the hazard of redemption candidates has declined sufficiently to cross the age-crime curve, which is the age-specific arrest rate for the general population. For the second benchmark, the redemption time is estimated as the time point when the hazard for redemption candidates is considered “close enough” to the benchmark, since the hazard does not cross the benchmark. The determination of “close enough” invokes the use of confidence intervals, which are based on bootstrap samples, and a risk-tolerance parameter, which represents how much higher risk an employer is willing to accept for those with a prior record, over those without.

The redemption times were estimated as a function of age at first arrest and the crime type of the first arrest (violence, property, drugs). The results show that violence is associated with higher hazards and larger redemption times. While younger ages are associated with higher hazards, the results show that the redemption times also depend on the choice of benchmarks.

Our original data were limited to arrests that occurred in New York State. In order to account for the arrests that occur outside of New York, a sample of national criminal records from the FBI was used. Based on the national records, over 20% of those who appear to have no subsequent arrests in NY after 1980 were found to have arrests elsewhere. The geographical location of New York State, and specifically the proximity of New York City to large cities in the neighboring states may have contributed to the relatively high prevalence of out-of-state arrests.

The magnitude of change between the estimated hazards and redemption times before and after the correction for the out-of-state arrests seem to warrant such a correction. Another candidate for correction could be based on a higher mortality rate for an offender population than

a non-offender population; the results suggest that there does not seem to be a substantively meaningful difference in the hazard estimates before and after the correction.

Examination of robustness of estimates established earlier in the dissertation is important if those estimates are to be used for possible policy implementation. The period between 1980 and 1990 is characterized by changes in the levels of arrest rates; thus, the estimates may be sensitive to the sampling years from which the arrest cohort is drawn. Similarly, considering that there is variation across states in terms of factors such as law enforcement policies and labor market conditions that might affect recidivism, it is possible that the estimates may be sensitive to the state from which the arrest cohort is drawn. The robustness with regard to sampling years and jurisdictions was tested using NY data from three different sampling years and additional data from Florida and Illinois. The results of the comparison across sampling years provide evidence for reasonable convergence in hazards across sampling years and that the redemption time estimates are robust. The results regarding the across-state comparison, on the other hand, show somewhat slower convergence of hazards and larger variation in the redemption time estimates, but are still reasonably close.

In estimating redemption times across sampling years and across states, we used two benchmark probabilities (.1 and .03) of incurring a second arrest. The higher benchmark (.1) represents the approximate probability of arrest at  $T^*$  for the 1980 NY cohort, and the lower benchmark (.03) represents the approximate probability of arrest at  $T^{**}$  (for the risk tolerance  $\delta = .02$ ) for the same cohort. The table below presents the range of redemption times by  $C_1$  for the two benchmark probabilities. The estimates for  $C_1 = \text{Violent}$  tends to be the largest, the estimates for  $C_1 = \text{Property}$  tend to be the smallest, and the estimates for  $C_1 = \text{Drugs}$  are somewhere in between the other two.

Table 7.1 Range of redemption time estimates (years) based on the estimates across three sampling years and three states

C <sub>1</sub>	Thresholds (probability of a new arrest)	
	.1	.03
Violent	4-7	11-15
Property	2-4	8-11
Drugs	4	10-14

Because of racial differences in arrest rates, employers' use of criminal background checking as a screening tool is of great concern to the Equal Employment Opportunity Commission (EEOC). The arrest prevalence for blacks is more than four times higher than the arrest prevalence for whites. In contrast, the risk of rearrest for blacks is about twice the risk of rearrest for whites shortly after their first arrest, so that racial rearrest risk ratio is about half the arrest-prevalence ratio. Furthermore, the rearrest risk ratio declines as the length of time clean increases and approaches unity after about 15 years. It is important that employers recognize that the arrest prevalence difference does not provide a meaningful estimate of risks posed by white and black applicants with a criminal record, and after a long period of time clean, their risks become similar.

The EEOC has ruled that in order for employers to use criminal records to screen job applicants, they need to demonstrate that the criminal record is "job related". That suggests that employers would need to understand whether the type of prior crime is a meaningful indicator of the type of crime that is of most concern in the context of what the job entails. In order to address this issue, we use crime-switch matrices to examine the probabilities of having a second arrest for a particular crime type, given the type of crime for the first arrest. We further analyze crime-type-specific hazards and estimate redemption times for different crime types depending on

which crime is of most concern to employers. The results show that in general the type of prior crime tends to be related to a higher risk of rearrest for the same crime. The results provide support for employers, who are often concerned about a particular type of crime, to evaluate the predictive value of the prior record.

## **7.2 Contributions**

This dissertation provides the first explicit empirical estimates of redemption times, which are based on large criminal-history data from state repositories. It also provides a comprehensive approach to understanding redemption by addressing many aspects of redemption that are important in developing relevant policies, which include the employers' risk tolerance, employers' concern about particular crimes, robustness of the estimates, and demographic characteristics of those with a prior record (age and race).

The preliminary results based on the dissertation have been published in *Criminology* in 2009 (Blumstein and Nakamura, 2009), and the findings received attention from the media, academic community, practitioners, policy makers, and organizations that represent the interests of commercial data brokers.

Since the policy implications and implementations are an important part of this dissertation, the preliminary results have been presented at numerous conferences, not only for the academic audience, but also for policy makers and practitioners, particularly those who are involved in corrections and re-entry efforts. At the National Institute of Justice conference in 2009, Attorney General, Eric H. Holder mentioned the research on redemption during his keynote address, describing it as "potentially transformative". One notable example of the contribution to the policy arena is a policy guideline proposed by the Pennsylvania Human

Relations Commission (PHRC), titled “The Disparate Impact Discrimination Implications of a Denial of Employment Based on a Criminal Record” (2009).<sup>92, 93</sup> The proposed guideline cites the aforementioned article in *Criminology* to argue that employers do not have grounds to claim business necessity if the criminal record is older than a certain number of years.

The last several years has seen a strong trend toward the recognition of problems that stem from excluding people with criminal records from employment, regardless of how long ago the crime occurred. The “ban the box” legislation has been increasingly adopted by major cities (e.g., San Francisco, Chicago, Austin) and states (Minnesota, New Mexico, Massachusetts) to remove the criminal record question from initial job applications, while maintaining employers’ rights to ask about, and consider, the record at later stages of the hiring process. The EEOC has been increasing its efforts to challenge the appropriateness of employer’s screening of people with criminal records, and as a response, the professional association for human resources management, and legal professionals in employment and labor relations have been advising employers to be aware of the laws governing background screening and to be selective about the job positions for which background checking is required.<sup>94</sup>

The understanding of redemption, the risk of recidivism falling to some acceptable level after staying clean for a substantial length of time, is consistent with the above trend, and the idea of redemption is essential to the policies that address the problems associated with the criminal background checking. For this reason, although the research on redemption has already

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<sup>92</sup> The PHRC enforces Pennsylvania’s anti-discrimination laws. Its eleven commissioners are appointed by the Governor and confirmed by the Senate. The commission is independent and nonpartisan, with no more than six commissioners from one political party.

<sup>93</sup> The nature of reactions to this type of proposal could depend on the interests and perspectives of the organization. Human resource management organizations and providers of background checking services tend to object to the idea of limiting the access to criminal records.

<sup>94</sup> See, for example, Slagle (2010) and Deschenaux (2010).

had some impact on policies such as the guideline by the PHRC, it is important to pursue the research in directions that can further inform the policymaking process.

### **7.3 Future Work - Prior Criminal History**

The estimates of redemption shown in this dissertation are based on the length of time since the “first” arrest. In this sense, we only address redemption for *first-time* offenders. Although such first-time offenders can be viewed as most deserving of redemption, it is possible to extend the concept of redemption to people with more than one prior criminal event. Employers also routinely receive applications from individuals with multiple arrests or convictions who have stayed clean a reasonable length of time. How do the redemption estimates vary with the number of prior crime events?

Research on recidivism of released prisoners informs us that the presence of prior incarceration increases the likelihood of recidivism (Beck and Shipley, 1997; Harer, 1994; Hoffman and Stone-Meierhoefer, 1979; Kitchener, Schmidt, and Glaser, 1977; Langan and Levin, 2002). In general, criminal history is identified as one of the most powerful predictors of recidivism (Gendreau, Little, and Goggin, 1996). In evaluating the effectiveness of after-prison reentry programs, Rosenfeld (2008) points out an important but often ignored distinction among released prisoners, comparing first-timers, those who were released from prison for the first time and veterans, those who have a prior incarceration experience. The first-timers and the veterans are different in that the veterans have a higher chance of recidivism than the first-timers. Ezell (2007) empirically shows that the increased number of adult arrest charges is associated with a higher risk of reoffending, and more importantly that the duration of time since the last arrest is associated with a reduction in the reoffending risk, with the number of charges being fixed. As

with first-time arrestees, this suggests the possibility that a long arrest-free duration should compensate for the effect of a number of prior arrest charges.

Despite the abundance of evidence that suggests that the relatively short-term recidivism risk increases with the number of prior records, there is no empirical evidence about the relationship between redemption times and the number of prior records. Since redemption times are determined by the long-term recidivism risk and the benchmark risk level, it is unknown how well the findings in recidivism studies hold for the estimates of redemption times.

In order to test the degree to which the redemption process varies with the number of prior arrests, we can estimate redemption times as a function of the number of prior arrests,  $k$ , and  $T$  is now a conditional time, which is a time since the  $k$ -th arrest.

The previous approach simplifies the estimation of redemption times by ignoring the variation in the rates of the previous arrests (i.e., two arrests in two years, two arrests in three years, and the like). That means that among the individuals with say, three arrests, the estimation does not differentiate those who have had the two arrests in a relatively short period of time (a high rate) and those who have had the two arrests with relatively long interval of time clean (a low rate). The rate can be estimated as the yearly offending rate,  $\lambda$  (Blumstein and Cohen, 1979).

Another approach to incorporating the heterogeneity in the patterns of prior arrests employs group-based trajectory models (Nagin, 2005). Using this approach, we can identify groups of individuals following approximately the same arrest trajectory during a specific period of time. Based on the estimates of the posterior probability of group membership, individuals can be assigned to a trajectory group which they are most likely to belong to. Given the group membership, it is possible to examine redemption times conditioned on the group membership.

We have shown in Ch. 2 that most arrestees from the 1980 cohort were not incarcerated as a result of their first crime because it was their first arrest as adults. Thus, it was not necessary to account for the length of incarceration time in estimating the hazard of a new arrest. As we pursue the consideration of those with multiple prior arrests, it is more likely that they would be incarcerated for a reasonable length of time following their last arrest. Unfortunately, data from most state repositories do not have accurate information on time served. It would be desirable to obtain a dataset from a state repository that can allow us to link the arrest-history data with information about time served, which is often maintained by the state's correctional department.

## APPENDIX: ESTIMATION OF $T^{**}$

This approach of estimating  $T^{**}$  is equivalent to carrying out a hypothesis test where the null hypothesis states that the difference between  $h(t)$  and  $h_{na}(t)$  is greater than  $\delta$ . The alternative hypothesis is that the difference is less than or equal to  $\delta$ . Thus, it is of the following form:

$$H_0 : h(t) > \delta + h_{na}(t) \text{ vs. } H_1 : h(t) \leq \delta + h_{na}(t)$$

We would reject the null hypothesis at  $t = T^{**}$ , where the *upper* confidence interval of  $h(t)$  first intersects  $(h_{na}(t) + \delta)$ . This approach is motivated by the literature on (bio)equivalence tests where the studies want to show that the effectiveness of new treatments (drugs, vaccines, diagnoses, etc.) is no worse than the standard, existing treatment by a specified margin (e.g., Barker et al., 2001; Westlake, 1976).

Our approach is different from the more familiar approach of determining whether  $h(t)$  is “close enough” to  $h_{na}(t)$  by carrying out a hypothesis test with the null hypothesis stating  $h(t)$  is equal to  $h_{na}(t)$  and concluding that  $h(t)$  is “statistically indistinguishable” from  $h_{na}(t)$  when we fail to reject the null hypothesis. This corresponds to constructing confidence intervals around  $h(t)$  and denoting  $T^{**}$  as the intersection of the *lower* bound of the confidence interval of  $h(t)$  with  $h_{na}(t)$ . However, smaller sample sizes inevitably make confidence intervals wider, which reflects the larger uncertainty of the estimates. If  $T^{**}$  were estimated using the lower bound of the confidence interval of  $h(t)$ , then wider confidence intervals would lead inappropriately to smaller values of  $T^{**}$ , possibly producing unreasonable values of  $T^{**}$  less than  $T^*$ . By introducing  $\delta$  and using the upper confidence interval, our approach circumvents this shortcoming. The conventional standard error of  $h(t)$  can be calculated by the formula  $\sqrt{[h(t) * (1 - h(t))] / n(t)}$ .

However, this formula relies on the asymptotic normality of the estimate of  $h(t)$ . Because the sample sizes defining  $h(t)$  become small when  $t$  is large, the standard errors calculated by the formula above are questionable. Moreover, in this case, the symmetric confidence intervals can include negative lower endpoints, which are a problem of “overshoot” (Newcombe, 1998); because we are trying to estimate the proportion of those who are rearrested at  $t$ , those estimates have to be bounded between 0 and 1, and so cannot go negative. The standard confidence interval of a proportion (often referred to as the Wald interval) is also known to show erratic behaviors in terms of the coverage probability, regardless of sample sizes and the values of  $h(t)$  (Brown, Cai, and DasGupta, 2001). Given the limitations of the Wald interval for  $h(t)$ , we use the statistical method of “bootstrap.” The bootstrap provides a reliable method to estimate the uncertainty of an estimator via resampling, without relying on the asymptotic properties of the estimator. We used the bootstrap intervals for  $h(t)$ , with the number of bootstrap samples,  $B = 2001$  (Efron, 1987; Efron and Tibshirani, 1993, Wu, 1989). Confidence intervals can be constructed using methods other than the bootstrap (Brown, Cai, and DasGupta, 2001; Newcombe, 1998).

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