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Effectiveness of Denial of Handgun Purchase by Violent Misdemeanants

Final Report

Presented to the

National Institute of Justice

NIJ Grant 98-IJ-CX-0024

May 29, 2002

Violence Prevention Research Program

University of California, Davis

Garen J. Wintemute, M.D., M.P.H.

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Co-Investigators

FINAL REPORT

Approved By: _____

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ABSTRACT

Background: Federal and state laws prohibit the purchase of firearms by felons and certain others. Some states additionally prohibit the purchase of handguns by persons convicted of selected misdemeanor crimes, but most do not. California has denied handgun purchases by violent misdemeanants since 1991; the prohibition remains in effect for ten years following the conviction. Such policies enjoy widespread public support, but their effectiveness is unknown.

Description of Current Study: The present study is an evaluation of California's prohibition on the purchase of firearms by violent misdemeanants. The study uses a retrospective cohort design. We sought first to determine the risk factors for new criminal activity among violent misdemeanants who seek to purchase handguns. We then determined whether the denial of handgun purchase by violent misdemeanants affected their risk of arrest for new crimes, particularly gun and/or violent crimes.

The study population consisted of all persons 21-34 years of age who sought to purchase a handgun from a federally licensed firearm dealer in California during 1989-1991 and who had at least one conviction, in the preceding ten years, for a violent misdemeanor that became grounds for denial of handgun purchase in 1991. After exclusions, study cohorts consisted of 986 persons whose purchase applications were made in 1991 and were denied ("denied persons") and 787 persons whose purchase applications were made in 1989-1990, before the new law took effect, and were approved ("purchasers").

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The main outcome measures were the incidence and relative risk of first arrest for new gun and/or violent crimes and non-gun, nonviolent crimes over three years after actual or attempted handgun purchase. The Kaplan-Meier product limit method and Cox proportional hazards regression were used to assess difference in risk between the two study cohorts.

Results: Over three years following their actual or attempted handgun purchases, 546 (33.0%) of 1,654 subjects with follow-up were arrested for a new crime, including 296 (31.9%) of 927 denied persons and 250 (34.4%) of 727 purchasers. After adjusting for differences in age, sex, and prior criminal history characteristics, purchasers were more likely than denied persons to be arrested for new gun and/or violent crimes (Relative Hazard (RH), 1.29; 95% Confidence Interval (CI), 1.04-1.60), but not for non-gun, non-violent crimes (RH, 0.96; 95% CI, 0.78-1.19). In both groups, risk of arrest was also strongly related to age and number of convictions accrued prior to actual or attempted handgun purchase.

Conclusions: Denial of handgun purchase to violent misdemeanants is associated with a specific decrease in risk of arrest for new gun and/or violent crimes.

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INTRODUCTION

Although decreasing, rates of firearm violence remain high. In 2000, an estimated 544,000 firearm-related violent crimes were committed in the United States, including approximately 10,180 firearm homicides (FBI 2001; Rennison 2001). One widely accepted policy to prevent such violence is to prohibit the purchase and possession of firearms by persons believed to be at high risk for future criminal activity. The Gun Control Act of 1968 outlaws the purchase and possession of firearms by convicted felons, fugitives from justice, narcotics addicts, and certain others. More recent federal initiatives have extended these denial criteria to include persons convicted of misdemeanor domestic violence offenses and those subject to domestic violence restraining orders. By 2000, California and 17 other states had extended their criteria for denial of firearm purchase to include convictions for a number of violent misdemeanors and other offenses (RJIS 2001). Since the enactment of the Brady Handgun Violence Prevention Act in 1993, prospective handgun purchasers throughout the United States have been subject to a mandatory waiting period and background check. Many states had implemented such requirements earlier. This federal requirement for a criminal records background check of prospective handgun purchasers has been one of the major federal crime prevention initiatives of the past decade. It remains controversial and has been challenged in court. Criminal and mental health record background checks of prospective handgun purchasers now identify 150,000-200,000 prohibited persons per year, 42% of whom are not convicted felons (Bowling, Lauver et al. 2001).

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One scholar of the subject has noted that "an effective transfer-regulating scheme that prevents guns from going to dangerous people would be nearly as successful as a much more intrusive scheme targeted at current gun owners" (Cook, Molliconi et al. 1995). There is broad public support for such programs. There is also substantial support for expanding the current federal criteria for denial of firearm purchase. Results of a recent national survey indicate that, depending on the exact offense, 60-95% of the American public favor broadening the criteria for denial of firearm purchase to include persons convicted of selected misdemeanors (Johns Hopkins Center for Gun Policy and Research and National Opinion Research Center 1997).

However, the effectiveness of the denial of firearm purchase in reducing rates of criminal activity has never been established. There is great interest in measuring the effectiveness of denial policies; such information would have obvious and immediate public policy implications. We have previously completed a study of the effectiveness of denying handgun purchases by felons in California; denial was associated with a decrease in rates of recidivism that averaged 20-30% and was substantially higher for some groups (Wright, Wintemute et al. 1999). Scholars at a 1997 meeting of the Homicide Research Working Group, however, agreed that a nationwide evaluation of the Brady Act would be difficult, and perhaps impossible, to conduct adequately (Kleck 1997; Webster 1997).

We report here on a large-scale controlled assessment of the effect of denial of handgun purchase on rates of subsequent criminal activity among violent misdemeanants in California. In 1991, California's criteria for denial of handgun purchase were expanded to include prior convictions for any of a list of specified violent misdemeanors. The prohibition remained in place for ten years following the conviction.

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Our primary *a priori* hypothesis was that, in an analysis that adjusted for other known risk factors for future criminal activity, persons who were denied the purchase of a handgun in California in 1991 as a result of a conviction for selected violent misdemeanors would have rates of subsequent violent criminal activity that were significantly lower than those among misdemeanants who purchased handguns in 1989 or 1990, before the new criteria became operative. This effect, we proposed, would be greatest for those offenses involving firearms and/or violence.

At the same time, we assessed the independent effects of demographic factors and the nature and severity of prior criminal history on the subsequent rates of criminal activity among authorized purchasers of handguns and persons denied such purchases.

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BACKGROUND

The Problem of Firearm Violence

Rates of violent crime remain unacceptably high. In 2000 an estimated 544,000 firearm-related violent crimes were committed in the United States, including approximately 10,180 firearm homicides (FBI 2001; Rennison 2001). During 1992-1998, an average 27,700 persons each year suffered nonfatal assaultive gunshot wounds (Simon, Mercy et al. 2001). The aggregate cost of firearm violence has been estimated to be $100 billion per year (Cook and Ludwig 2000). The costs associated with firearm injuries themselves are substantial: an estimated $20 billion in lifetime costs for firearm injuries sustained in 1990, of which at least 80% are borne by public funds (Wintemute and Wright 1992; Max and Rice 1993).

Moreover, offenders armed with a firearm are substantially more likely to complete some violent crimes, particularly rape, than are offenders armed with other weapons (BJS 1986; Rand 1990; Rand 1995). Firearm use particularly appears to facilitate violent crime in which the perpetrator is a stranger to the victim; such crimes now constitute a majority of all violent crimes in the United States (Rennison 2001).

Firearms are not all at the same risk for use in violent crime. Handguns constitute approximately 40-45% of all firearms manufactured in the United State each year (Unpublished data, BATF) but are used in at least 80% of all violent crimes involving firearms (FBI 1996; Perkins, Klaus et al. 1996). Many policies intending to prevent firearm violence focus specifically on handguns.

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And crime guns tend to be newly, or recently, released into circulation. In 1999, the most recent year for which data are available, the median age of recovered crime guns was 5.7 years; for some frequently-recovered guns the median time from first sale to recovery was under three years (BATF 2000). By contrast, private gun owners report that they have owned nearly two-thirds of their guns for six years or more; the average time since acquisition is 12.8 years, and some portion of these guns were acquired used (Cook and Ludwig 1996). This suggests that policies seeking to prevent the flow of new guns into criminal hands might be particularly effective.

Research on Regulating the Purchase, Carrying, and Use of Firearms

Surprisingly little recent research has been conducted on illegal commerce in and use of firearms, considering the size of the problem itself and the number of policies that have been promulgated to address it. This section reviews the most pertinent studies.

One increasingly widespread policy is that of targeted street-level enforcement of laws forbidding the carrying of concealed weapons without permits. This policy has become widespread in part because of the favorable results of an evaluation of a pilot program in Kansas City (Sherman, Shaw et al. 1995). In that study, increased police patrols targeting firearm confiscation were associated with a modest increase in the number of firearms confiscated and a 49% decrease in the incidence of gun crimes. Similar changes were not seen in a control area. The evaluators concluded that both general and specific deterrence of gun carrying may have resulted from the increased police patrols.

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The impact of mandatory sentencing laws for gun crimes, a widely implemented and widely-supported strategy, has also been evaluated (McDowall, Loftin et al. 1992). McDowall and colleagues conducted six independent time series analyses in cities in four eastern states. Data for the individual cities did not provide consistent support for an effect of mandatory sentencing. Pooling the results from all six cities provided what the authors described as "exceptionally strong support" for an effect on homicide, but little effect on gun assault or robbery. Compatible results have been seen in evaluations of a Massachusetts law imposing *per se* enforcement and mandatory sentencing for the illegal carrying of concealed firearms (Beha 1977A)(Beha 1977B). However, in an analysis of nearly all such laws using a multiple time series design -- but, in what may be a significant design flaw, using all other states as controls for any one state -- Marvell and Moody found that such "laws produce any impact in no more than a few states and that there is little evidence that the laws generally reduced crime or increased prison populations" (Marvell and Moody 1995).

Several evaluations have recently been conducted of policies that seek to lower rates of violence by increasing, rather than decreasing, the percentage of the population that is armed while in public. These laws create a mandate for local law enforcement agencies to issue concealed carry permits to persons who request those permits and are legally able to purchase and own firearms.

Individual evaluations yielded results that were frequently interpreted as contradictory but which in fact suggest that shall issue policies had little, if any, effect on crime rates. The first such study examined effects of shall issue policies on homicide rates in five metropolitan areas in Florida, Mississippi, and Oregon (McDowall, Loftin et al. 1995). Homicides increased in four of

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the five sites and decreased in the other. One of the four increases and the one decrease were statistically significant. On average, homicides rose 25% after shall issue policies were adopted, but the authors cautioned that the variation between sites made this an unreliable result.

Another study, this one widely publicized, examined trends in county-level crime rates in ten states that adopted shall issue policies (Lott and Mustard 1997). There were decreases of 5 to 8% in most violent crimes and increases, which the authors considered to be compensatory, in property crimes. But when others examined data for individual states, they found neither consistent increases nor decreases. As with child access prevention laws, many of the critical results could not be reproduced with Florida removed from the analysis (Black and Nagin 1998). Criminologist Gary Kleck concluded that most likely "the declines in crime coinciding with relaxation of carry laws were largely attributable to other factors," and not to the laws themselves (Kleck 1997B, p 376).

A related study determined that the decrease in homicide in the postlaw period in states that adopted shall-issue policies consisted almost entirely of a decrease in juvenile homicides (Ludwig 1998). Homicide rates for adults may even have increased. The significance of this finding is that juveniles, who could not obtain concealed weapons permits under any circumstances, could not have been protected by more liberal access to these permits. This study also found wide variation across individual states.

The reason for the lack of a clear effect is now emerging. About 7% of adults — 3.4 million persons — carry firearms in public on a regular basis and for reasons not related to their work (Cook and Ludwig 1996). Of these, 22% carry every day and 10% carry at least one-half the time; some 900,000 people may be carrying firearms on their person on a typical day. In

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states that adopted shall issue policies, typically no more than 1 or 2% of the eligible population

requested a permit (Ludwig 1998), and a number of these new permittees probably carried

firearms already. It is doubtful that the frequency of weapon-carrying was significantly affected

by the adoption of shall issue statutes.

A wide array of policies regulate the purchase of firearms. Recently, attention has

focused on the purchase of multiple firearms on a single occasion or within a short period of

time. ATF tracing data show that, among recently purchased and traced guns, those bought in

multiple purchases were particularly likely to have had an attempt made to obliterate their serial

numbers — a clear indication of criminal intent (BATF 2000). Weil and Knox recently

evaluated the effect on gun trafficking of a Virginia law that limited handgun purchases to one

per month (Weil and Knox 1996). The percentage of gun traces initiated in the so-called

northeast corridor states -- New York, New Jersey, Connecticut, Rhode Island, Massachusetts --

that identified guns as being transported from Virginia was 35% before the passage of the law

and 16% afterwards. The authors concluded that, in this case at least, regulating the rate of

handgun purchase had substantial beneficial effects on firearms trafficking.

A number of lines of research have suggested that limiting ease of access to firearms for

entire populations is associated with decreased rates of firearm violence. Sloan and colleagues,

for example, compared rates of homicide and other violent crimes in Seattle and Vancouver

(Sloan, Kellermann et al. 1988). They asserted that the two cities were similar with respect to

most risk factors for firearm violence, but differed substantially in the degree to which they

regulated sale and possession of handguns. They found selective increases in rates of firearm-

related violent crime in Seattle, as compared to Vancouver. For homicides, they demonstrated

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that this rate was specific to handgun crimes. Cook has reviewed a number of other studies on this point (Cook 1991).

Finally, recent studies have examined the effect of banning outright the purchase of specific classes of firearms. Loftin and colleagues studied the effect of restrictive licensing of handguns on homicide and suicide in the District of Columbia (Loftin, McDowall et al. 1991). Adoption of the law was associated with a 25% reduction in firearm homicide that became evident almost immediately. There was no compensatory increase in homicide by other means in the District of Columbia, nor were there similar reductions in firearm homicide in nearby Maryland or Virginia. Other observers have criticized this study for terminating follow-up in 1987, after which homicide again rose coincident with the appearance of crack cocaine.

A ban on specified assault-type firearms was enacted as part of the 1994 Crime Bill, and in 1998 the Clinton administration halted the manufacture and importation, but not sale, of large-capacity semiautomatic "copy cat" rifles that had been designed to avoid the prior bans on technical grounds (BATF 1998). The ban imposed by the 1994 Crime Bill has been evaluated by researchers at the Urban Institute (Roth and Koper 1997; Roth and Koper 1999). In the short run the ban appeared to have beneficial, but modest, effects. In the first year and a half after the ban became effective, trace requests to ATF fell by 20% for banned weapons but just 11% for other guns. There was no such decrease in traces in this period in those states where assault-type firearms had been banned earlier. In St. Louis and Boston, where all confiscated firearms were traced, traces for banned weapons fell 29% and 24%, respectively. The ban may have contributed to a 7% drop in firearm homicide from 1994 to 1995, but it was not clear at that

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time whether the decrease represented a downward trend or simple year-to-year variation. A re-evaluation over a longer time period is underway.

A number of jurisdictions acted to ban domestic production and sale of the poorly made, inexpensive handguns known as Saturday night specials. By 1997, four states had established a minimum melting point criterion for the metal used to produce gun frames; the inexpensive zinc alloy from which these guns are often made has a lower melting point than does high grade steel. In California, more than 40 cities and counties sought to eliminate Saturday night specials by outlawing the manufacture and sale of guns that failed to meet a series of design and materials criteria. Results varied, apparently as a result of variable monitoring and enforcement (Wintemute 2000A).

In 1989, Maryland created a Handgun Roster Board to develop a list of handguns that could legally be manufactured or sold in the state. The board was required to consider such characteristics as size, quality of materials, reliability, and suitability for sporting use, among others; no specific standards were set (Teret, Alexander et al. 1990). A preliminary evaluation of the impact of the Maryland law has been completed. As with assault-type weapons, there was a substantial increase in sales of non-approved guns prior to the law's effective date. Nonetheless, non-approved guns accounted for a progressively smaller percentage of crime guns confiscated by law enforcement agencies (Vernick, Webster et al. 1998). The effect of the ban on crime was unclear; crime rates did not fall appreciably faster in Maryland than in neighboring states without similar legislation (Webster, Vernick et al. 1998).

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Denial of Firearm Purchase

The Gun Control Act of 1968 specified classes of persons who were prohibited from purchasing or possessing firearms. Other classes have been added by subsequent legislation. Today, these classes include convicted felons, persons under felony indictment, persons convicted of domestic violence misdemeanors or subject to domestic violence restraining orders, illegal aliens, controlled substance addicts, persons adjudicated mentally ill, and others. This intervention seeks to be effective early in the chain of events leading to firearm violence, regulating the acquisition of firearms rather than their use.

The clear presumption behind this policy is that members of the prohibited classes are at unacceptable risk for future criminal activity involving firearms. In some cases this presumption is well supported. For example, a large body of evidence has established that persons with a prior history of criminal activity are more likely than persons without such a history to do crime in the future. Among many others, (Blumstein, Cohen et al. 1986; Tillman 1987; Tracy, Wolfgang et al. 1990; Greenberg 1991). In other cases the picture is less clear. Some commentators have suggested that these classes are over-inclusive, and that persons with mental illness and noncitizens are arguably at no greater risk for criminal activity than are others (Jacobs and Potter 1995).

It has also been argued that these criteria are not inclusive enough. No jurisdiction denies firearm purchase to all persons having a criminal history, and many thousands of persons with criminal histories legally purchase firearms every year. Given that a prior criminal history is a well established risk factor for future criminal activity, the possibility therefore exists that identifiable subgroups of authorized handgun purchasers are at increased risk for later criminal

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activity. This is not just a theoretical concern; one commentator had suggested that "a

considerable fraction of people who commit violent crimes are legally entitled to own guns"

(Cook and Blose 1981).

Our own research has established that, among legal purchasers of handguns in California,

those with a prior criminal history are at substantially increased risk for criminal activity after

handgun purchase (Wintemute, Drake et al. 1998). We undertook a retrospective cohort study of

5,923 authorized purchasers of handguns in California in 1977 who were younger than 50 years

of age, identified by random sample. These purchasers acquired their handguns long before

California law prohibited selected misdemeanants from purchasing handguns; all study subjects

passed mandatory criminal records background checks. Our main outcome measures were

incidence and relative risk (RR) of first charges for new criminal offenses after handgun

purchase. Follow-up to the end of the 15-year observation period or to death was available for

77.8% of study subjects and for a median 8.9 years for another 9.6%. Handgun purchasers with

at least one prior misdemeanor conviction were more than seven times as likely as those with no

prior criminal history to be charged with a new offense after handgun purchase (RR, 7.5; 95%

confidence interval [CI], 6.6-8.7). Among men, those with two or more prior convictions for

misdemeanor violence were at greatest risk for nonviolent firearm-related offenses such as

weapon carrying (RR, 11.7; 95% CI, 6.8-20.0), violent offenses generally (RR, 10.4; 95% CI,

6.9-15.8), and Violent Crime Index offenses (murder or non-negligent manslaughter, forcible

rape, robbery, or aggravated assault) (RR, 15.1; 95% CI, 9.4-24.3). However, even handgun

purchasers with only one prior misdemeanor conviction and no convictions for offenses

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involving firearms or violence were nearly five times as likely as those with no prior criminal

history to be charged with new offenses involving firearms or violence.

As a practical matter, the enforcement of a policy to deny firearm purchase to specified

classes of persons has been contingent upon the enactment of mandatory background checks for

persons seeking to purchase firearms. At the national level, this became possible only in 1994

following the enactment of the Brady Handgun Violence Prevention Act. The Brady Act

required a five-day waiting period prior to handgun purchase, and initially also required a

designated state or local chief law enforcement officer to conduct a criminal records background

check. The latter requirement was declared unconstitutional by the Supreme Court in June 1997.

Most chief law enforcement officers continued to perform background checks on a voluntary

basis.

By 2000, when The Brady Act had been in operation for seven years, all states and federal

agencies together had screened a total of 30 million applications to purchase guns and had issued

689,000 denials. In 2000, 42% of denials were for reasons other than felony conviction or

pending indictment (Bowling, Lauver et al. 2001).

Procedures for screening handgun purchasers in the states operating under Brady Act

procedures were reconfigured in November 1998. Both the waiting period and the background

checks conducted by state or local law enforcement agencies were replaced by a National Instant

Check System (NICS) administered by the FBI. During NICS' first year of operation, nearly

90% of background checks were completed within two hours of application; 72% were

completed within 30 seconds. Difficult checks could take several days, however, and the law

allowed dealers to release firearms to purchasers after three business days, whether or not the

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background checks were completed. By the end of 1999, 3,353 prohibited persons, most of them felons, had acquired firearms in this manner; just 442 had surrendered their guns. Federal law enforcement experts have suggested that this problem would largely be eliminated if the waiting period for firearm purchases were lengthened (FBI 2000; GAO 2000).

California has required the recording of all sales of firearms on a Dealer's Record of Sale (DROS) form since 1917. Background checks have been conducted since the late 1960s following standardized procedures. There has been a mandatory waiting period to allow the background check to be conducted, which was shortened from 15 to ten days in 1997, after our study period. In 1991, the background check requirement was extended to include sales between private parties. In addition, the criteria for denial of firearm purchase were expanded to include prior convictions for a number of violent misdemeanors. The most important of these were misdemeanor assault and battery, brandishing a firearm, and discharging a firearm. A complete listing is in Table 1.

At the time this study was undertaken, California procedures were as follows: The prospective purchaser and the selling dealer completed a DROS form. A copy was forwarded to the California Department of Justice (CDOJ) in Sacramento; another was sent to the chief law enforcement officer of the jurisdiction in which the subject resided. CDOJ personnel searched the state's criminal history and mental health records databases for records pertaining to this applicant, using a sophisticated Soundex matching system. They also queried national databases for records maintained in other states. If records were identified, they were reviewed for disqualifying events. If incomplete information existed, such as arrests without dispositions, contact was made with the appropriate agencies; many of these contacts were with agencies in

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other states. Additional information was obtained from mental health personnel and others as needed.

If dealers do not receive a negative report within the allotted time, the sale is consummated. With some variation from year to year, 1.5-2.5% of sales are denied. Under California law, as distinct from federal law, sales that are put on "delay" status by CDOJ screeners may not be consummated when the waiting period ends, but only after CDOJ has obtained the information needed to make a final determination of the prospective purchaser's eligibility. Additional sales (well under 1%) are therefore denied initially and later permitted, sometimes after the passage of weeks to months, when this critical missing information becomes available.

Incapacitation is the principal mechanism by which denial of firearm purchase is thought to lower crime rates: such policies are intended to deprive high-risk persons of access to firearms, and thereby reduce their capacity for committing violent crimes. The effectiveness of these policies might therefore be expected to vary directly with the importance of firearm use in affecting completion rates. Thus, the impact of these laws should be greatest for gun and/or violent crime. In the case of homicide, the weapons effect is very substantial (Cook 1991). This also appears to be true for robbery (Rand 1995). One might hypothesize an additional deterrent effect, particularly in a legal environment such as California's that includes "three strikes" or similar legislation. A prospective firearm purchaser would be aware that his or her criminal history is known to the Justice Department. This might deter some potential offenders from incurring further "strikes." However, a number of critics have questioned whether these laws have any substantial deterrence effect, and one analysis has associated them with a substantial

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increase in homicide, both immediately and over the long term (Marvell and Moody 2001).

It is possible that the main effect of such policies in much of the United States is simply to deter ineligible persons from acquiring firearms from licensed firearms dealers, leaving them free to acquire firearms by other methods instead. Cook and colleagues have defined two markets for firearms: a primary market consisting of sales made by holders of federal firearms licenses and a secondary market consisting of all other gun sales, licit or illicit (Cook, Molliconi et al. 1995). Cook and Ludwig estimate approximately a 60:40 ratio in sales between the primary and secondary markets. And they note that, "the secondary market will look increasingly attractive as the regulations governing the primary market become more restrictive" (Cook, Molliconi et al. 1995, pg 71). There is evidence to support this position. In the 1991 Survey of State Prison Inmates, half of those who purchased their most recent handgun from an illegal source stated that they had not bought the weapon from a retail store because of concerns about a background check (BJS 1994).

Waiting period and background check policies, in that they only affect sales by licensed dealers, clearly are targeted at the primary market. However, the primary market may be of more importance, even for high risk purchasers, than is commonly supposed. In that same 1991 survey, those who used a handgun in the offense leading to their incarceration were as likely to have purchased that firearm from a licensed dealer as from "the black market, a drug dealer, or a fence" (Beck, Gilliard et al. 1993). And "sales by licensed firearm dealers" has a broader meaning in some jurisdictions than others. California and several other states have effectively outlawed the secondary market, requiring that almost all transfers of firearms between private parties be routed through a licensed dealer so that a background check could be conducted. The

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California law took effect in 1991, at the same time that the state's broader denial criteria became

operative. This might be expected to enhance the effect of expanded denial criteria, by making it

more difficult for prohibited persons to make illegal purchases. But enforcement is problematic.

California maintains a computerized archive of all transfers of handguns that are conducted by

FFLs. Based on the Cook and Ludwig estimate, we would expect perhaps 40% of these records

to indicate that they concerned private party transfers facilitated by FFLs. But in actuality, fewer

than 10% of the records so signify.

Critics have suggested that easy access to the secondary firearms market renders waiting

period/background check programs ineffective. Jacobs and Potter, for example, argue that the

regulatory goals of such policies far exceed their regulatory capacity and that their chief effect is

to create pressure for straw purchases and purchases in the secondary market. They consider

such policies to be nothing more than "a sop to the widespread fear of crime" (Jacobs and Potter

1995).

However, Cook and colleagues have argued that the effect of denial policies should not

be considered in isolation (Cook, Molliconi et al. 1995). They may work synergistically with

enhanced sentences for and enforcement of illegal possession statutes that make acquisitions in

the secondary market less attractive. Their incapacitative effect could be enhanced by extending

their scope, as has been done in California by requiring all private party sales to be routed

through a licensed dealer.

Moreover, criticism such as that of Jacobs and Potter must be seen as speculation in the

absence of data on whether these policies produce their intended final effect: reducing rates of

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criminal activity among those whose primary-market handgun purchases are denied. It is that outcome that the present proposal addresses.

We have already conducted a small-scale evaluation of the denial of firearm purchases by felons (Wright, Wintemute et al. 1999). We examined a sample of persons who were denied handgun purchase in California in 1977 on the basis of a prior felony conviction and a sample of those whose handgun purchases were approved although they had a prior felony arrest (this group had no felony convictions or other disqualifying events.) Subjects were followed for three years. In multivariate analysis, the arrestees whose purchases were allowed were at greater risk for offenses involving a gun (Relative Risk (RR)= 1.2, 95% Confidence Interval (CI), 1.1-1.4) or violence (RR= 1.2, 95% CI, 1.1-1.4). Among those having only a single prior arrest for an offense involving weapons or violence, those whose handgun purchases were approved appeared to be at substantially increased risk for a new gun offense (RR= 2.7, 95% CI, 0.4-19.5) or violent offense (RR= 3.9, 95% CI, 0.6-28.3); the small sample sizes limited the power of the analysis.

These findings suggest that, even among serious offenders, denial of handgun purchase may lower rates of expected criminal activity for offenses involving firearms or violence by 20-30% and much more for some subgroups. Additional preliminary evidence comes from McDowall and colleagues' study of Florida's "shall issue" statute. They observed a decline in homicide rates in that state beginning only several years after its "shall issue" statute was enacted and roughly contemporaneous to its adoption of a waiting period and background check requirement (McDowall, Loftin et al. 1995).

In 2000, after the study we report on here had been largely completed, Ludwig and Cook published an evaluation of the Brady Handgun Violence Prevention Act that compared homicide

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trends in states where the act led to new screening programs for gun purchasers with trends in

homicide in states that had pre-existing screening programs. They found no significant

difference in homicide rates in the two groups in states, and their findings have been widely

interpreted as demonstrating that Brady has been ineffective.

It can be argued, however, that the outcome of their study was determined by the method

chosen. The law is designed to affect the behavior of a very small part of the likely population at

risk. Using a population-wide outcome measure, rather than one pertaining to those directly

affected, means that a real effect may well be overlooked. A population-wide outcome measure

would be appropriate an intervention that impacts an entire population, but that is not what gun

purchaser screening programs do. Consider as an analogy a vaccine trial, in which an

intervention is taken to prevent an adverse outcome. The proper assessment of that vaccine is in

its effect on those vaccinated, as compared to others; population-based results would not be

accepted. In the case of Brady, the number of persons affected is small enough that even a

complete and permanent elimination of homicide risk in that affected population would probably

not be reflected in any discernible change in population-wide homicide rates (Wintemute

2000C).

Long-Term Studies of Criminal Behavior

In this study, the independent effect of the denial of legal purchase of a handgun on

subsequent rates of criminal activity among identified persons at risk, not entire populations, is

the primary subject of interest. We therefore very briefly review here selected longitudinal

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studies of criminal behavior. To our knowledge, no studies other than our own have been conducted on criminal behavior among legal purchasers of firearms.

The importance of gender as a risk factor both for initial arrest and for recidivism has been well established (Blumstein, Cohen et al. 1986). Race/ethnicity is also related to substantial differences in rates of first arrest but generally not to rates of recidivism (Blumstein and Graddy 1982; Blumstein, Cohen et al. 1986; Tracy, Wolfgang et al. 1990; Greenberg 1991). When race/ethnicity is taken into account, the effect of socioeconomic status appears to be relatively minor and inconsistent (Tittle and Meier 1990; Visher, Lattimore et al. 1991). The number of prior offenses is also strongly correlated with the likelihood of new offending (Tillman 1987; Greenberg 1991).

Previous longitudinal studies have used a variety of measures of criminal behavior. One such measure is self report, which is not available to us. Studies making use of records have variably relied on arrest, conviction, violations of probation or parole, and others. As one of our study cohorts has no prior criminal history, only arrest and conviction are applicable to all subgroups of our study population. Each has strengths and drawbacks. The use of arrest alone creates the possibility of misclassification on the basis of false positives, or Type 1 errors. The use of arrest is widespread, however (Belkin, Blumstein et al. 1973; Blumstein and Graddy 1982; Tillman 1987; Beck and Shipley 1989). Crimes rates estimated from documented arrest histories are similar to those derived from self report data (Blumstein and Cohen 1979). Sole reliance on conviction, even assuming that dispositions are always available, creates a high likelihood of a Type 2 error, or misclassification based on false negatives. The majority of felony arrests do not result in felony convictions, even when there is substantial evidence of guilt. Many other causes

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for nonconviction exist (Maltz 1984). Our own prior longitudinal studies have used data for both

arrests and convictions. We have found that results based on conviction are quite similar to those

based on arrest (Wintemute, Drake et al. 1998).

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METHODS

Overview

This is a historical cohort study. Subjects are identified and classified as to their characteristics as of a certain point in the past and followed forward in time, toward the present.

We have taken the critical exposure in this study to be the legal purchase of a handgun. Our primary study cohort, the denied cohort, is by this definition the unexposed cohort: persons who were denied the purchase of a handgun in 1991 because of a prior conviction for a violent misdemeanor within the preceding ten years. This was the first year in which such convictions were grounds for denial. Our comparison co hort, the exposed or purchaser cohort, is made up of persons whose applications to purchase handguns in 1989 or 1990 were approved and whose criminal records at that time contained a conviction within the preceding ten years for an offense which would have been disqualifying had they sought to purchase handguns in 1991.

Subjects were followed for three years from the date 15 days after the date on their application for handgun purchase. This is the earliest date on which handgun acquisition could have occurred given the length of California's mandatory waiting period at the time. The outcomes of major interest were rates and relative risks of arrest and conviction for new offenses, particularly those involving firearms, other weapons, and/or interpersonal violence. Arrests and convictions for other offenses were also examined to assess the specificity of any observed effect with denial of handgun purchase.

Because offenses occurring in other states were likely not to appear on California's criminal records, only those subsequent offenses occurring in California were identified as

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outcome events. To establish that study subjects remained at risk for such events, records

linkage procedures that we have refined in previous studies were used to verify that subjects'

continuing residence in California.

Data Sources

Dealer's Record of Sale File: Since the early 1970s, selected data elements from

California's Dealer's Records of Sale (DROS) forms for all approved handgun purchases have

been computerized. The files for 1989 and 1990 were used as the sampling frame for our control

or purchaser cohort. If the CDOJ background check identifies a criminal record for a person

whose handgun purchase is eventually approved, that person's unique Criminal Identification and

Information (CII) number is added to the computerized record of that approved purchase. Thus,

it is possible to identify prior to sampling those persons who have a criminal history at the time

of their approved handgun purchase.

The computerized record also includes the unique record number for the Dealer's Record

of Sale form; this number is used by CDOJ as the identifier for that particular handgun purchase.

Not all of the data elements on the DROS form are entered into the automated file. However,

originals or microfilm copies of the reports are retained by CDOJ. These were made accessible

to us.

Prohibited Persons File: Since 1989, a computer file of elements of all applications that

are denied has also been maintained. This file contains personal identifiers, the unique Dealer's

Record of Sale number for the denied purchase, the CII number for all persons having a criminal

history, and the reason for denial. For those denied as a result of prior criminal activity, the

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computer file includes the specific offense for which a conviction that resulted in the denial. CDOJ provided us with a copy of this file for 1991, which we used to identify all persons whose applications for handgun purchase were denied as a result of prior violent misdemeanor convictions.

Longitudinal File: California's Adult Criminal Justice Statistical System Longitudinal Database was created to allow batch sorting of subjects with criminal histories for research purposes (CDOJ, 1985). It contains complete identifier data, including the unique CII number, and salient criminal history transaction data on all persons whose adult criminal history records began in 1974 or subsequently. Thus, it contains these data for all persons who reached the age of 18 on January 1, 1974 or later (and would therefore have been 35 years of age or younger in 1990). Records in the longitudinal file may be sorted and retrieved by any of the automated variables and nested sorts can be performed. Thus, the file can be used to produce a list of all persons with criminal histories in California who have selected demographic and or criminal history characteristics.

From this file, CDOJ provided us a registry of all persons who reached 18 years of age on or after January 1, 1974 who, in 1990 or earlier, had been convicted of one of those violent misdemeanor offenses that became grounds for denial of handgun purchase in California in 1991.

Criminal History System: The Criminal History System (CHS) contains data on all adults arrested in California. These criminal records include extensive personal identifier information to maximize the possibility that a newly arrested person will be linked to his prior criminal record. In a trial run involving several hundred handgun purchasers known by us to have criminal histories, we verified a 100% "hit" rate.

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The quality and completeness of data in CHS are high. In the late 1980s, other researchers established that felony dispositions were available in at least 80% of cases in California, compared with only 40-60% nationally (Orsagh 1989). In 1991 we performed a pilot review of several hundred rapsheets to validate the data quality and establish our abstracting procedures. This review determined that nearly 80% of *all* dispositions, whether felony or misdemeanor, were available. Consequent to that time a backlog of the entry of new criminal justice transactions into CHS developed (BJS 1995). That backlog has since been cleared. The criminal history records we obtained for this study show arrests that occurred within a few weeks of our request for the records.

Since the early 1970s, CHS has been subject to an episodic records purge designed to remove inactive records. Records become eligible for removal following specified criteria; mandatory retention periods are related to the nature and severity of an individual's criminal history. No offense involving weapons or interpersonal violence may be purged, and no record containing any such offense can be purged before the subject reaches age 70. Records for persons whose handgun purchase is denied are maintained until the subject's 100th birthday (CDOJ 1990). As a result, the purge process has had minimal impact on our ability to obtain records for study subjects.

Cohort Assembly

Last name and date of birth were used to identify tentative matches between persons listed in the 1989-1990 handgun purchaser data and persons recorded in our extract of the longitudinal file as having violent misdemeanor convictions by 1990. All tentative matches were

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confirmed by manual records review. Criminal records for all subjects in both cohorts were

reviewed to verify that each had a disqualifying violent misdemeanor conviction within ten years

of actual or attempted handgun purchase.

We identified 1,099 persons under age 35 whose handgun purchases had been denied for

a prior violent misdemeanor conviction in 1991, and 877 persons under age 35 who had

purchased handguns in 1989 or 1990 and within the preceding ten years had been convicted of a

violent misdemeanor that became grounds for denial in 1991. We excluded 23 persons from the

denied cohort who appeared to have been denied in error: 22 whose convictions were more than

ten years prior to the date of their handgun purchase applications and one whose conviction was

for a crime that did not constitute grounds for denial. Another 90 persons purchased handguns in

1989 or 1990 and then were denied when they attempted to purchase handguns in 1991.

Preliminary analyses performed with these persons included and excluded yielded nearly

identical results, and they were therefore excluded.

Power calculations were based on results from our prior studies. We found that a

previously arrested cohort of successful gun purchasers under 50 years of age and having a prior

criminal history would experience approximately a 40% incidence of arrest for all offenses and a

20% incidence of arrest for violent crimes or less serious weapons offenses over a defined period

of follow-up, with most first arrests occurring within a few years of the onset of follow-up.

Recidivism for younger offenders will be higher (Beck and Shipley 1989), and these power

calculations are therefore conservative.

The sample size requirements were derived from data presented by Breslow and Day

(1987 pg 283), and Kahn and Sempos (1989). We predicted that our cohort sizes would be

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sufficient to detect relatively small changes in risk with sufficient power. For the outcome arrest for any offense, we would be able to detect a relative risk of between 1.2 and 1.3 in the purchaser cohort, equivalent to a risk reduction of 15-25% in the denied group. For the outcome arrest for an offense involving violence or weapons we would be able to detect a relative risk of between 1.3 and 1.5 in the purchaser cohort, equivalent to a risk reduction of 25-33% in the denied cohort.

Data Acquisition and Management

Dealer's Record of Sale and criminal history records were obtained for members of both study cohorts. Project staff reviewed the records to confirm a match between the study subject and the record supplied.

Data were entered and cleaned by three-member teams. In the case of the DROS records, two team members independently entered each record into computer files. These databases were compared by computer and discrepancies were then resolved by a third team member who consulted the original record.

Similar, but more complex, procedures were used for criminal history data. All data staff were trained by CDOJ's records technicians in criminal history interpretation. Two team members independently abstracted each rapsheet onto a standardized paper form. These forms were compared for obvious discrepancies by a third team member who reconciled them while making reference to the original record. For ambiguous cases the principal investigator was consulted. The paper record was then computerized by two team members working independently, such that there were two separate files for each record. The two files were compared by computer, and all discrepancies were again resolved by the third member of the

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team, with consultation by the principal investigator and others as needed. While they were labor intensive, these procedures minimized both abstracting and data entry error.

Data entry was performed in Foxpro for Windows, using specialized screens developed by us. Data comparison was performed in SAS. We used the OCA number, a unique number identifying a specific Dealer's Record of Sale form and thus a specific application for handgun purchase, as our linking identifier for data assembled from multiple sources. The number was added to the rapsheet database as records were key entered.

The following variables, listed here by data source, were abstracted:

From Dealer's Record of Sale Forms/ Data Tape:

Personal Data: Name, Date of Birth, Driver's License number, Criminal Information and Identification number (if present), Social security number (if present), Other identifying number (if present), Sex, Race, Occupation, Local address, Permanent address

Transaction Data: OCA number (unique transaction identifier for this purchase only), Date of transaction, Dealer name, Dealer address, Private sale (yes/no)

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From the Prohibited Persons File:

Denial type (Felony conviction, misdemeanor conviction, restraining

order, mental health, under age, etc.) , Specific denial offense (e.g. 245 PC

for aggravated assault), Out of state offense (Y/N), Denial date

From criminal history rapsheets (in addition to identifiers):

Nature of action, Date, Statute violated (Section, Paragraph, Statute Code),

Data source (arrest report, court report, probation or custody report)

The nature of action variable on criminal history rapsheets was coded as follows to allow for

detailed specification:

TRANSACTION CATEGORY	ACTION TYPE AND CODE
Charges	Arrest/Cite
	New charge(filed during criminal justice proceedings)
	Arrest--Released-Detention only
	Additional/eXtra charges
Convictions	Conviction, level of offense unspecified
	Felony
	Kid (Juvenile) Convictions
	Misdemeanor

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Commitments	DiaGnostic & Narcotics
Applications	Law enforcement, other security
	Concealed Weapon
Other	. RegiZtration, Deceased

Crimes were grouped into the following discrete classes: non-gun, nonviolent crimes

(e.g., petty theft, driving under the influence of alcohol); nonviolent gun crimes (e.g., carrying a

concealed firearm in a public place); and violent crimes (e.g., simple and aggravated assault,

robbery, murder).

Our initial intent had been to categorize all crimes as to whether they had involved a gun,

violence, both, or neither. This would have permitted us the strongest possible analysis of the

effect of the nature of prior offenses on subjects' risk of recidivism, and of the specificity of any

effect of the policy we were evaluating. Unfortunately, California's criminal records did not

reliably distinguish between violent crimes that involved guns and those that did not. This was

particularly important with regard to such offenses as aggravated assault, which may or may not

involve a firearm. The state's Penal Code contained separate subparagraphs indicating firearm

involvement or its absence but the rapsheets frequently omitted this level of coding. Our records

review established that, in the period prior to actual or attempted handgun purchase, convictions

for nonviolent gun crimes made up only 4.4% of convictions for all crimes involving guns,

violence, or both guns and violence. We therefore defined the main outcome event for the study

as the first arrest for a new gun and/or violent crime. Additional analyses provided separate

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results for non-gun, nonviolent crimes; nonviolent gun crimes; violent crimes; and all crimes combined.

Verification of At-Risk Status

The follow-up period began 15 days following application for handgun purchase – the first day on which legal acquisition of the handgun could have occurred, if permitted – and ended three years later. Our surveillance for criminal events after handgun purchase was limited to those occurring in California as information on offenses occurring elsewhere was not available. We employed a series of procedures developed by us in earlier research to verify that study subjects remained in California and at risk for outcome events. These procedures relied on data other than records of outcome events, to avoid outcome bias. Following standard procedure for longitudinal studies, follow-up for subjects who could not be independently determined to be at risk throughout the study period was censored as of their last known date of residence in the state.

Our procedures were as follows. Subject identifiers, including a driver's license number when available, were first provided to the state Department of Motor Vehicles for linkage to their driver's license files. As our period of follow-up ended no later than December 31, 1994, nearly all subjects wishing to maintain an active driver's license would have renewed that license after the end of the study period and before our records requests were made in 1999. Our data included a driver's license number for over 90% of all subjects. Subjects were considered to have remained California residents until the date of their most recent license renewal.

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For subjects for whom further data was needed, we queried registries maintained by credit

agencies and telephone listings. We also queried the California Master Mortality File and social

security-derived mortality registries available on the World Wide Web. Finally, a hand search

was made of telephone books and registries of property owners available from the California

State Library.

Subjects for whom no independent confirmation of continued residence in California

could be obtained were excluded from outcome analyses. However, to allow for an estimate of

the possible bias introduced by lack of follow-up, data on new arrests were also collected for

these subjects and were tabulated for comparison purposes.

Analysis

We originally conducted an analysis that was very similar to that which we had developed

and used in prior similar studies. Outcome rates were calculated as incidence density rates using

person years at-risk for the denominators and the number of events for numerators (Kleinbaum,

Kupper et al. 1982). Rates were standardized by stratification, and relative risks estimated by

calculating the ratio of rates. Probabilities and confidence limits were calculated using statistics

programs for the comparison of two Poisson distributed rates (Breslow and Day 1987).

Outcome rates were additionally analyzed by Poisson regression (Frome and Checkoway

1985), which allowed more thorough consideration of risk patterns and interactions between risk

factors. One set of regressions addressed entire study cohorts. In those regressions, the main

effect (explanatory) variables included cohort membership, gender, race/ethnicity, and severity of

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criminal history prior to purchase. Separate analyses were performed for each of the outcomes of interest. Two way interactions were tested.

On review, however, we found that risk differentials were time-dependent and determined to reanalyze the data using survival analysis techniques. Reviewers of an earlier version of this report also suggested this modification. In this second analysis, the probability of experiencing a first new arrest was estimated by the Kaplan–Meier method (Kaplan and Meier 1958). The significance of differences in probabilities was assessed by the log-rank statistic.

Cox proportional hazards regression was used to calculate univariate and adjusted relative hazards and 95% confidence intervals (Cox 1972). A model including age, sex, race, and number of prior criminal convictions was used to estimate adjusted relative hazards. Time since actual or attempted handgun purchase was measured in days. Other continuous variables were stratified: age, 21-24, 25-29, and 30-34 years; prior convictions for any crime: one, two, three, and four or more; prior convictions for gun and/or violent crimes: one, two, and three or more. Subjects for whom the number of prior convictions could not be determined (12 persons in the case of prior convictions for any crime, 21 persons for prior gun and/or violent crime convictions) were excluded from multivariate analyses; all were denied persons.

The addition of terms for interactions between study cohort and age, study cohort and number of prior convictions, and age and number of prior convictions did not improve the fit of the model; none were included in the final model. Similarly, inclusion of measures of the elapsed time between the most recent prior conviction for any crime and for any gun and/or violent crime did not improve the fit of the model, and these were not retained. Reliance on the

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proportional hazards assumption was validated by plotting Schoenberg residuals for individual

covariates against time (Hosmer and Lemeshow 1999).

The primary regression analysis examined risk for experiencing a first arrest. A

conditional, total time recurrent-events model was developed to study effects as additional arrests

occurred and as time since actual or attempted purchase increased. In the recurrent events

analysis an overall effect estimate was generated for each covariate (Prentice, Williams et al.

1981; Hosmer and Lemeshow 1999; Kelly and Lim 2000).

The significance of differences between subjects with and without independent follow-up

was estimated using the chi-squared statistic.

All tests of significance were two-sided, with a P value of <0.05 considered to indicate

statistical significance. SAS software was used for all analyses (PC-SAS, Version 8, SAS

Institute, Cary, NC).

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RESULTS

After exclusions, the study cohorts were made up of 986 persons who were denied the purchase of a handgun in 1991 ("denied persons") and 787 persons who purchased a handgun in 1989 or 1990 ("purchasers"). The demographic and prior criminal history characteristics of the two cohorts were very similar; 23.1% of denied persons and 27.2% of purchasers had been convicted of more than one violent misdemeanor that had become grounds for denial of handgun purchase in 1991 (Table 2).

Independent evidence of subjects' continued residence in California for the entire three-year follow-up period was available for 83.9% of denied persons and 84.6% of purchasers. Another 10.1% of denied persons and 7.8% of purchasers were confirmed as alive and in the state for part of the follow-up period (median 1.7 years for both groups). No follow-up information was available for 119 subjects. Absence of follow-up was not related to subjects' study cohort (7.6% (n=60) for purchasers and 6.0% (n=59) for denied persons, P=0.172), sex (P=0.564), age group (P=0.892) or number of prior convictions for any crime (P=0.084) or gun and/or violent crimes (P=0.295).

Over three years following their actual or attempted handgun purchases, 546 (33.0%) of 1,654 subjects with follow-up were arrested for a new crime, including 296 (31.9%) of 927 denied persons and 250 (34.4%) of 727 purchasers (Table 2). Purchasers were more likely than denied persons to be arrested for a new gun and/or violent crime (23.9% and 20.1% respectively, log-rank P=0.048)(Figure 1a), but not for a new non-gun, non-violent crime (21.3% and 22.8%, respectively, log-rank P=0.461)(Figure 1b).

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Among the 119 subjects with no follow-up, purchasers were more likely than denied

persons to experience a new arrest for any crime, (46.7% and 28.8%, respectively, P=0.044), a

non-gun, nonviolent crime (33.3% and 23.7%, respectively, P=0.245), and a gun and/or violent

crime (31.7% and 22.0%, respectively, P=0.235). Among purchasers, the crude incidence of

arrest was substantially higher for those without follow-up than for those with follow-up

available -- by an absolute 12.3% for any crime, 12.0% for non-gun, non-violent crimes, and

7.8% for gun and/or violent crimes. For denied subjects, these absolute differences were much

smaller and, in the case of arrest for any crime, reversed. The crude incidence of arrest among

denied persons without follow-up, as compared to those with follow-up, was 3.1% lower for any

crime, 0.9% higher for non-gun, non-violent crimes, and 1.9% higher for gun and/or violent

crimes.

The results of univariate analysis are presented in Table 3. Crude first-arrest rates for

new gun and/or violent crimes were 9.9/100 person-years (py) for purchasers and 8.0/100 py for

denied persons (Relative Hazard (RH), 1.23; 95% Confidence Interval (CI), 1.00-1.52). There

was no significant difference between the two groups in risk of arrest for non-gun, nonviolent

crimes. Among purchasers the arrest rate for gun and/or violent crimes exceeded that for non-

gun, nonviolent crimes; among denied persons the opposite was true. When both denied persons

and purchasers were considered together, males were at increased risk of arrest for gun and/or

violent crimes; risk of arrest for all crime categories was strongly related to age (Table 2, Figure

2) and number of prior criminal convictions (Table 3, Figure 3).

These results were generally confirmed in multivariate analysis (Table 4). Purchasers

remained more likely than denied persons to be arrested for new gun and/or violent crimes (RH,

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1.29; 95% CI, 1.04-1.60) but not for non-gun, non-violent crimes (RH, 0.96; 95% CI, 0.78-1.19).

Adjusted risk of first arrest for all crime types decreased by more than 50% as age increased.

Risk of arrest increased for all crime types with the number of prior convictions for any crime,

but an increasing number of prior convictions for gun and/or violent crimes was associated only

with an increased risk of arrest for new crimes of that type.

When nonviolent gun crimes and violent crimes were considered separately, results were

similar to those for all gun and/or violent crimes considered together. After adjustment,

purchasers were more likely than denied persons to be arrested for both violent crimes (RH, 1.24;

95% CI, 0.98-1.58) and nonviolent gun crimes (RH, 1.46; 95% CI, 0.98-2.17). For both study

cohorts combined, subjects age 30-34 were substantially less likely than those ages 21-24 to be

arrested for either violent crimes (RH, 0.49; 95% CI, 0.36-0.67) or nonviolent gun crimes (RH,

0.36, 95% CI; 0.21-0.62). Subjects with three or more prior convictions for a gun and/or violent

crime were more likely than were subjects with one such conviction to be arrested for a violent

crime (RH, 1.57; 95% CI, 0.97-2.54), but not a nonviolent gun crime (RH, 1.04; 95% CI, 0.38-

2.83).

There was relatively little variation across age and prior criminal history strata in the

increased risk of arrest for gun and/or violent crimes associated with handgun purchase (Table 5).

The increase in risk was modest, and not statistically significant, in many instances.

Over the entire period of follow-up, and including both first and subsequent arrests, the

crude arrest rate for gun and/or violent crimes was 10.6/100 py for handgun purchasers and

9.5/100 py for denied persons; rates for non-gun, non-violent crimes were 11.8/100 py and

12.8/100 py, respectively. After adjustment, purchasers were at slightly greater risk of arrest for

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gun and/or violent crimes (RH, 1.12; 95% CI, 0.93-1.35) but not for non-gun, non-violent crimes

(RH, 0.96; 95% CI, 0.81-1.14). Among subjects who were arrested for gun and/or violent crimes

following actual or attempted handgun purchase, denied persons were slightly more likely than

purchasers to be arrested more than once for such crimes (25.6% and 24.0% respectively,

P=0.120).

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COMMENTS

In this population of violent misdemeanants who sought to purchase handguns, risk for subsequent criminal activity was high. One person in three was arrested for a new crime at least once within three years of purchasing a handgun; more than one in five were arrested at least once for a new crime involving guns and/or violence. Risk of arrest was directly related to the number of prior convictions subjects had acquired and inversely related to age, relationships that have been documented previously (Blumstein and Cohen 1979; Blumstein, Cohen et al. 1986; Farrington 1987; Tillman 1987; Visher, Lattimore et al. 1991; Wintemute, Drake et al. 1998).

Aggressive efforts to lower the incidence of new crimes among violent misdemeanants appear to be well founded. This may particularly be the case among younger misdemeanants and those with multiple prior convictions, who appear to be at highest risk. However, precisely because of their established pattern of criminal activity, repeat offenders may be less responsive than other misdemeanants to many interventions.

Such interventions operate by one or both of the mechanisms of deterrence and incapacitation. Denial of handgun purchase can be seen as potentially operating by both: deterrence, in that it stigmatizes the behavior of handgun purchase by prohibited persons, and incapacitation, in that it also prevents that purchase, at least from licensed and regulated firearm retailers.

As such, it will be incompletely effective. While some misdemeanants may be susceptible to the level of control embodied in such a policy, others will not. Assuming (erroneously) the existence of entirely complete and up-to-date registries of prohibited persons, no misdemeanants

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would be able to purchase guns from licensed retailers; some would not try. But others might falsify their identification, employ a surrogate or straw purchaser, or -- and perhaps most commonly -- purchase guns from unlicensed and unregulated private vendors. Nationwide, perhaps 40% of all firearm transfers involved these vendors (Cook and Ludwig 1996). While licensed retailers must identify prospective purchasers, initiate background checks, and keep records, unlicensed vendors need see no identification, cannot initiate background checks, and need not keep records (BATF 1999).

Nonetheless, denial of handgun purchase was associated with a moderate decrease in risk of arrest for new gun and/or violent crimes, even when gender, age and prior criminal history were taken into account. Several aspects of our findings suggest that this is a causal association. First, it is specific: denial of handgun purchase had no impact on risk for non-gun, nonviolent crimes. Second, it is plausible: reduced access to guns in a high risk population could be expected to reduce their risk of committing new gun and/or violent crimes, but not other crimes. Third, it is consistent: denial of handgun purchase was associated with a reduced risk for gun and/or violent crimes across the ranges of both age and severity of subjects' prior criminal activity. The magnitude of the effect, furthermore, is similar to that seen in an earlier study of the effectiveness of prohibiting handgun purchases by felons (Wright, Wintemute et al. 1999).

Not surprisingly, denial of handgun purchase appears to have its greatest effect in reducing risk for a first arrest for a gun and/or violent crime. Its effectiveness may diminish as time since actual or attempted handgun purchase increases and among subjects who have already incurred new arrests for gun and/or violent crimes.

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Three attributes of this study suggest that our findings may have minimized the effect of denial of handgun purchase. First, our study compared persons denied in the first year of the new law to those whose purchases were approved in the two prior years. It can plausibly be argued that those who attempted to purchase guns immediately after it became illegal for them to do so -- and the adoption of the law was widely publicized -- demonstrated a continued willingness to violate laws concerning the possession and use of firearms. It reasonably follows from this that such persons would also be at increased risk for committing gun crimes. Nonetheless, our denied persons manifested a lower risk of crimes involving guns or violence.

Second, as a commentator on an earlier version of this study has noted (Blackman 2001), background crime rates were varying at this time; this raises the possibility that a period effect could account for our results. By simple inspection, as Blackman reports, violent crime rose about 9% in the three years following the approved purchases in our comparison cohort, and fell 7% during the three years following the denials. But this comparison is misleading. If one directly compares the crime rate for the first year of follow-up for the approved purchasers to the crime rate for the first year of follow-up for the denied persons, then compares the respective second years, and then compares the respective third years, a very different pattern emerges. California's violent crime rate was higher during each of the first two years of follow-up for persons denied the purchase of a handgun then during the comparable years of follow-up for those whose purchases were approved. The denied persons were nonetheless at lower risk of arrest for gun and/or violent crimes.

The third concerns the 6.7% of study subjects for whom we were unable to obtain independent follow-up. The proportion of subjects without follow-up was not related to study

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cohort assignment (or any other hypothesized risk factor). Among these subjects, purchasers were more likely than denied persons to be arrested for new crimes — by much larger margins than those seen among subjects for whom follow-up was available. Moreover, loss to follow-up was associated with an absolute increase in incidence of first arrest for all types of crime among purchasers, but not among denied persons; including results for persons without follow-up would have raised the incidence of arrest in the former group, but not the latter. These findings suggest that excluding subjects without follow-up has caused us to underestimate both the risk of arrest for new crimes among handgun purchasers and the effects of denial of purchase.

Our findings are subject to several limitations. The small size of the study population limited our statistical power to detect relative risks that were below approximately 1.25, or higher for subgroup analyses. When relative risks are below 1.5, results should be interpreted with caution regardless of the size of the study population due to the potential impact of unmeasured factors.

Rising crime rates may account in part for the puzzling finding that the number of violent misdemeanants seeking to purchase handguns in 1991 was greater than that for 1989 and 1990 combined. Violent crime rates are closely linked to demand for handguns (Wintemute 2000B), and handgun sales in California rose annually between 1986 and 1993. It is also possible that the upsurge in attempted purchases in 1991 represented a misinformed effort on the part of newly-ineligible persons to purchase handguns before the new law was enforced, rather than deliberate attempts to make illegal purchases as discussed above. Accelerated gun sales in anticipation of possible restrictions have been observed previously (Roth and Koper 1997).

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Because the criminal records data were not sufficiently specific, we were unable to

categorize crimes systematically as involving guns, violence, both, or neither. We were therefore

unable to study the specific effect of California's denial policy on risk of arrest for violent gun

crimes. We were, however, able to separate nonviolent gun crimes from violent crimes; the

results were very similar to those for all gun and/or violent crimes considered together.

It could be argued that the prevention of nonviolent gun crimes, particularly the illegal

carrying of a concealed firearm in public, should not be an objective of policies that deny

handgun purchases by persons believed to be at high risk of committing gun violence. We would

disagree; illegal gun carrying is a necessary precursor to much violent gun crime, and controlled

experiments have shown that law enforcement efforts to interdict illegal carrying have had

substantial effects on the incidence of gun violence (Sherman, Shaw et al. 1995; OJJDP 1999).

As in other states, information regarding juvenile offenses is frequently missing from the

criminal records. As a result, we are to some degree undercounting offenses prior to handgun

purchase. However, it is an important aspect of this study that we are relying on data as they are

now routinely gathered and maintained by law enforcement agencies.

Finally, this is a single state study, and no two states have adopted the same expanded

denial criteria. New Jersey, for example, denies the purchase of a handgun to "any person who

has been convicted of a crime" (RJIS 2000). Replications in several states would provide a more

general estimate of the effectiveness of denial of handgun purchase.

Critics of programs to screen prospective purchasers of firearms and deny purchases by

prohibited persons have suggested that they are unlikely to be effective, describing them in one

case as a "sop to the widespread fear of crime" (Jacobs and Potter 1995). They have argued that

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persons with criminal intent who are prevented from buying guns in the legal market will simply

acquire them illegally. However, the formal, legal gun market is an important source of guns for

purchasers with criminal intent. Among state prison inmates who were incarcerated for a crime

involving a handgun, that handgun was as likely to have come from a gun store as from an

obviously illegal supplier (Beck, Gilliard et al. 1993). And aggressive law enforcement has

begun to disrupt the operations of the illegal gun market (Wintemute 2000B). Denial of legal

access to handguns may have even greater impact now, as illegal access becomes more difficult,

than during our study period.

We note that a recent evaluation of the impact of the Brady Handgun Violence Prevention

Act, the federally-mandated waiting period and background check for handgun purchases, did not

detect an effect on criminal violence (Ludwig and Cook 2000). That evaluation measured

changes in state-level homicide rates from 1994-1998. During those years, however, so few

persons were denied the purchase of handguns that their expected 20-25% reduction in risk of

committing gun and/or violent crimes (Wright, Wintemute et al. 1999) could not have produced a

measurable effect on homicide rates (Ludwig and Cook 2000; Wintemute 2000C).

The evidence presented here suggests that denying the purchase of handguns by violent

misdemeanants is an effective means of preventing gun-related and violent crime in a high risk

population. However, there are substantial logistic considerations to be addressed before such a

policy could be implemented nationwide. No federal registry of violent misdemeanants exists,

and it may be difficult to compile one (Tien and Rich 1990; OTA 1991). Such a registry would

need to be updated on a continuing basis to prevent newly-ineligible persons from purchasing

handguns. As discussed earlier, under the present National Instant Check System (NICS), more

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than 3,353 prohibited persons, most of them felons, had inadvertently been permitted to purchase

firearms by the end of 1999; their background checks had not been completed within the 72

hours allowed by NICS (GAO 2000). This risk could be minimized by reinstituting a waiting

period to allow all background checks to be completed.

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Table 1:   Violent Misdemeanors That Became Grounds for Denial of Firearm

Purchase in California in 1991

- Possession of a deadly weapon with the intent to intimidate a witness. (Penal Code, § 136.5.)
- Threatening witnesses, victims, or informants while committing another misdemeanor. (Penal Code, § 140.)
- Unauthorized possession of a weapon in a courtroom, courthouse or court building, or at a public meeting. (Penal Code, § 171 (b).)
- Bringing into or possessing a loaded firearm within the state capitol, legislative offices, etc. (Penal Code, § 171 (c).)
- Taking into or possessing loaded firearms within the governor's mansion or residence or other constitutional officer, etc. (Penal Code, § 171 (d).)
- Assault. (Penal Code, § 241.)
- Battery. (Penal Code, § 243.)
- Assault with a stun gun or taser weapon. (Penal Code, § 244.5.)
- Assault with a deadly weapon or instrument, by any means likely to produce great bodily injury or with a stun gun or taser on a school employee engaged in performance of duties. (Penal Code, § 245.5.)
- Discharging a firearm in a grossly negligent manner. (Penal Code, § 246.3.)
- Shooting at an unoccupied aircraft, motor vehicle, or uninhabited building or dwelling house. (Penal Code, § 247.)
- Drawing, exhibiting, or using any deadly weapon other than a firearm. (Penal Code, § 417(a)(1).)
- Drawing or exhibiting, selling, manufacturing, or distributing firearm replicas or imitations. (Penal Code, § 417.2.)
- Bringing into or possessing firearms upon or within public schools and grounds. (Penal Code, § 626.9.)
- Driver of any vehicle who knowingly permits another person to discharge a firearm from the vehicle or any person who willfully and maliciously discharges a firearm from a motor vehicle. (Penal Code, § 12034(b)(d).)
- Person or corporation who sells any concealable firearm to any minor. (Penal Code, § 12100 (a).)
- Possession of ammunition designed to penetrate metal or armor. (Penal Code, § 12320.)
- Carrying a concealed or loaded firearm or other deadly weapon or wearing a peace officer uniform while picketing. (Penal Code, § 12590.)

Source: California Firearms Laws 1991. Sacramento: California Department of Justice, 1991.

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Table 2:   Demographic and Prior Criminal History Characteristics of Violent

Misdemeanants Who Applied to Purchase Handguns in California*

Characteristic		Purchase Denied, 1991 (n= 986)	Purchase Approved, 1989-1990 (n= 787)
Sex, n (%)			
	Male	945 (95.8)	757 (96.2)
	Female	41 (4.2)	30 (3.8)
Age, n (%)			
	21-24	234 (23.7)	172 (21.9)
	25-29	411 (41.7)	360 (45.7)
	30-34	341 (34.6)	255 (32.4)
Race/Ethnicity, n (%)			
	White	455 (46.1)	370 (47.0)
	Black	157 (15.9)	99 (12.6)
	Hispanic	296 (30.0)	228 (29.0)
	Asian/Other	48 (4.9)	35 (4.4)
	Missing/Unknown	30 (3.0)	55 (7.0)
Number of Prior Convictions[†]			
Any Crime, n (%)			
	1	504 (51.7)	382 (48.5)
	2	253 (26.0)	196 (24.9)
	3	102 (10.5)	111 (14.1)
	4+	115 (11.8)	98 (12.5)

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Characteristic	Purchase Denied, 1991 (n= 986)	Purchase Approved, 1989-1990 (n= 787)
Gun and/or Violent Crime, n (%)		
1	737 (76.4)	573 (72.8)
2	163 (16.9)	161 (20.5)
3+	65 (6.7)	53 (6.7)

*   Percentages may not add to 100.0 due to rounding.

†   Because records were incomplete, the number of convictions for any crime

was unknown for 12 denied persons, and the number of convictions for gun or

violent crimes was unknown for 21 denied persons. Percentages are of subjects

for whom the number of convictions was known.

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Table 3. Incidence and Crude Relative Hazard of First Arrest for New Crimes among Violent Misdemeanants Who Applied to Purchase Handguns*

Characteristic	Subjects	Any Crime			Gun and/or Violent Crime			Non-Gun, Non-Violent Crime		
		Number (%) Arrested	Events per 100 py	Crude RH (95% CI)	Number (%) Arrested	Events per 100 py	Crude RH (95% CI)	Number (%) Arrested	Events per 100 py	Crude RH (95% CI)
All Subjects	1,654	546 (33.0)	14.7		360 (21.8)	8.8		366 (22.1)	9.0	
Purchase Status										
Denied	927	296 (31.9)	14.1	1	186 (20.1)	8.0	1	211 (22.8)	9.3	1
Approved	727	250 (34.4)	15.5	1.10 (0.93-1.30)	174 (23.9)	9.9	1.23 (1.00-1.52)	155 (21.3)	8.6	0.93 (0.75-1.14)
Sex										
Female	65	21 (32.3)	14.3	1	11 (16.9)	6.6	1	15 (23.1)	9.5	1
Male	1,589	525 (33.0)	14.7	1.02 (0.66-1.58)	349 (22.0)	8.9	1.34 (0.74-2.45)	351 (22.1)	9.0	0.94 (0.56-1.58)
Age										
21-24	377	163 (43.2)	21.0	1	108 (28.6)	12.3	1	117 (31.0)	13.3	1
25-29	719	234 (32.5)	14.4	0.70 (0.57-0.85)	152 (21.1)	8.5	0.70 (0.55-0.89)	152 (21.1)	8.6	0.65 (0.51-0.83)
30-34	558	149 (26.7)	11.3	0.55 (0.44-0.69)	100 (17.9)	7.1	0.58 (0.44-0.76)	97 (17.4)	6.8	0.52 (0.40-0.68)

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Characteristic	Subjects	Any Crime			Gun and/or Violent Crime			Non-Gun, Non-Violent Crime		
		Number (%) Arrested	Events per 100 py	Crude RH (95% CI)	Number (%) Arrested	Events per 100 py	Crude RH (95% CI)	Number (%) Arrested	Events per 100 py	Crude RH (95% CI)
Prior Convictions										
Any Crime										
1	815	209 (25.6)	10.8	1	144 (17.7)	7.0	1	126 (15.5)	6.0	1
2	429	147 (34.3)	15.2	1.40 (1.14-1.73)	90 (21.0)	8.4	1.19 (0.92-1.55)	104 (24.2)	9.9	1.65 (1.27-2.14)
3	200	87 (43.5)	21.0	1.90 (1.48-2.44)	57 (28.5)	12.1	1.70 (1.25-2.31)	58 (29.0)	12.2	2.01 (1.47-2.75)
4+	198	95 (48.0)	25.4	2.26 (1.77-2.88)	63 (31.8)	14.1	1.97 (1.47-2.65)	73 (36.9)	17.2	2.79 (2.09-3.73)
Gun and/or Violent Crime										
1	1,217	359 (29.5)	12.7	1	230 (18.9)	7.5	1	241 (19.8)	7.9	1
2	302	123 (40.7)	19.6	1.50 (1.23-1.85)	86 (28.5)	12.3	1.60 (1.25-2.05)	81 (26.8)	11.4	1.43 (1.11-1.84)
3+	115	53 (46.1)	23.9	1.81 (1.36-2.42)	37 (32.2)	14.1	1.84 (1.30-2.60)	36 (31.3)	14.0	1.74 (1.23-2.47)

* Limited to subjects for whom follow-up independent of new criminal activity was available. Subjects were excluded when number of prior convictions was not precisely known (n=12 for any convictions, n=20 for gun and/or violent convictions).

PY denotes person-years; RH, relative hazard; CI, confidence interval.

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Table 4. Adjusted Relative Hazard of First Arrest for New Crimes among Violent

Misdemeanants Who Applied to Purchase Handguns*

Characteristic	Any Crime	Gun and/or Violent Crime	Non-Gun, Non-Violent Crime
	Adjusted RH (95% CI)	Adjusted RH (95% CI)	Adjusted RH (95% CI)
Purchase status			
Denied	1	1	1
Approved	1.15 (0.97-1.37)	1.29 (1.04-1.60)	0.96 (0.78-1.19)
Age			
21-24	1	1	1
25-29	0.70 (0.57-0.86)	0.70 (0.54-0.90)	0.64 (0.50-0.82)
30-34	0.46 (0.37-0.59)	0.48 (0.36-0.64)	0.44 (0.33-0.59)
Prior Convictions			
Any Crime			
1	1	1	1
2	1.36 (1.08-1.72)	1.01 (0.74-1.37)	1.71 (1.29-2.27)
3	1.99 (1.47-2.69)	1.52 (1.04-2.23)	2.47 (1.72-3.54)
4+	2.40 (1.76-3.28)	1.77 (1.19-2.63)	3.47 (2.43-4.96)
Gun and/or Violent Crime			
1	1	1	1
2	1.06 (0.82-1.36)	1.39 (1.01-1.91)	0.85 (0.64-1.15)
3+	1.04 (0.73-1.49)	1.28 (0.82-2.00)	0.84 (0.55-1.28)

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*   Limited to subjects for whom follow-up independent of new criminal activity was available. Subjects were excluded when number of prior convictions was not precisely known (n=12 for any convictions, n=20 for gun and/or violent convictions). Relative hazards are adjusted for sex and all variables in the table. RH denotes relative hazard; CI, confidence interval.

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Table 5. Adjusted Relative Hazard of First Arrest for New Crimes, for Handgun Purchasers as Compared to Denied Persons, among

Violent Misdemeanants Who Applied to Purchase Handguns*

Characteristic	Any Crime			Gun and/or Violent Crime			Non-Gun, Non-Violent Crime		
	Events per 100 py		Adjusted RH (95% CI)	Events per 100 py		Adjusted RH (95% CI)	Events per 100 py		Adjusted RH (95% CI)
	Purchase Approved	Purchase Denied		Purchase Approved	Purchase Denied		Purchase Approved	Purchase Denied	
Age									
21-24	22.1	20.2	1.13 (0.82-1.56)	14.4	10.9	1.37 (0.92-2.03)	12.8	13.6	1.02 (0.69-1.50)
25-29	14.2	14.7	1.04 (0.80-1.36)	8.5	8.6	1.06 (0.76-1.48)	8.0	9.1	0.93 (0.67-1.29)
30-34	13.4	9.8	1.38 (0.98-1.94)	9.1	5.7	1.64 (1.07-2.51)	6.6	6.9	0.96 (0.63-1.46)
Prior Convictions									
Any Crime									
1	11.6	10.1	1.20 (0.91-1.60)	7.8	6.4	1.26 (0.89-1.78)	5.6	6.3	0.97 (0.67-1.39)
2	15.2	15.2	0.98 (0.70-1.37)	9.4	7.7	1.18 (0.77-1.83)	9.1	10.6	0.85 (0.57-1.28)
3	23.1	19.0	1.27 (0.82-1.96)	12.2	11.9	1.12 (0.65-1.93)	13.5	11.1	1.33 (0.79-2.25)
4+	27.2	24.0	1.27 (0.83-1.96)	18.8	10.9	1.80 (1.05-3.09)	15.6	18.6	0.90 (0.55-1.47)

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Characteristic	Any Crime			Gun and/or Violent Crime			Non-Gun, Non-Violent Crime		
	Events per 100 py		Adjusted RH (95% CI)	Events per 100 py		Adjusted RH (95% CI)	Events per 100 py		Adjusted RH (95% CI)
	Purchase Approved	Purchase Denied		Purchase Approved	Purchase Denied		Purchase Approved	Purchase Denied	
Gun and/or Violent Crime									
1	13.6	12.0	1.18 (0.95-1.47)	8.7	6.6	1.38 (1.05-1.81)	7.4	8.2	0.97 (0.74-1.26)
2	20.8	18.5	1.15 (0.80-1.65)	13.6	11.1	1.29 (0.84-2.00)	11.9	11.0	1.12 (0.72-1.75)
3+	22.8	24.9	1.24 (0.70-2.21)	12.9	15.1	0.93 (0.47-1.84)	11.6	16.2	0.84 (0.42-1.68)

- Limited to subjects for whom follow-up independent of new criminal activity was available. Subjects were excluded when number of prior convictions was not precisely known (n=12 for any convictions, n=20 for gun and/or violent convictions). Relative hazards are adjusted for sex and all variables in the table. PY denotes person-years; RH, relative hazard; CI, confidence interval.

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Figure 1A. Kaplan–Meier Event Curves for New Arrests for Gun and/or Violent Crimes, by Whether a Handgun Purchase Was Approved or Denied
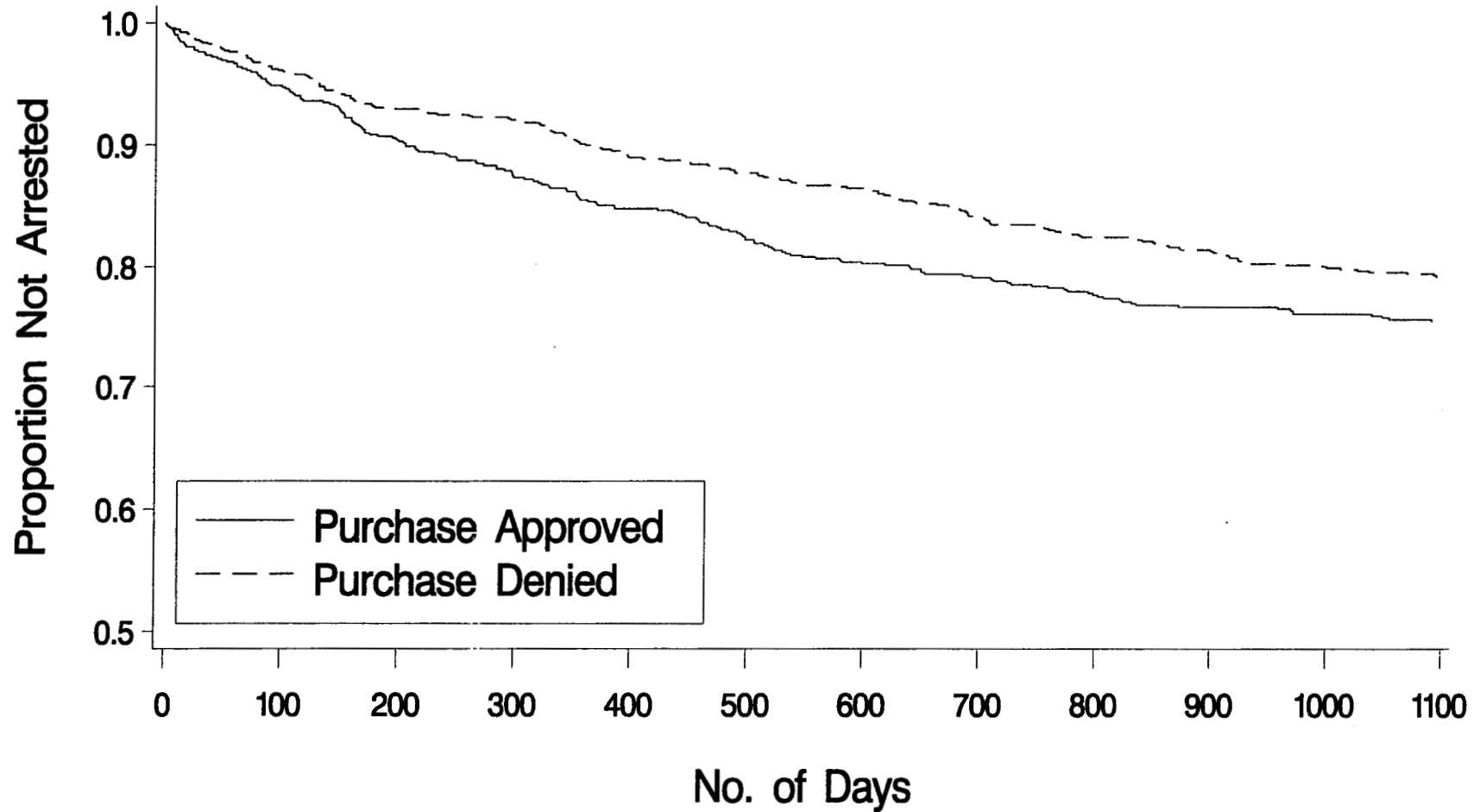
Figure 1B. Kaplan–Meier Event Curves for New Arrests for Nongun, Nonviolent Crimes, by Whether a Handgun Purchase Was Approved or Denied
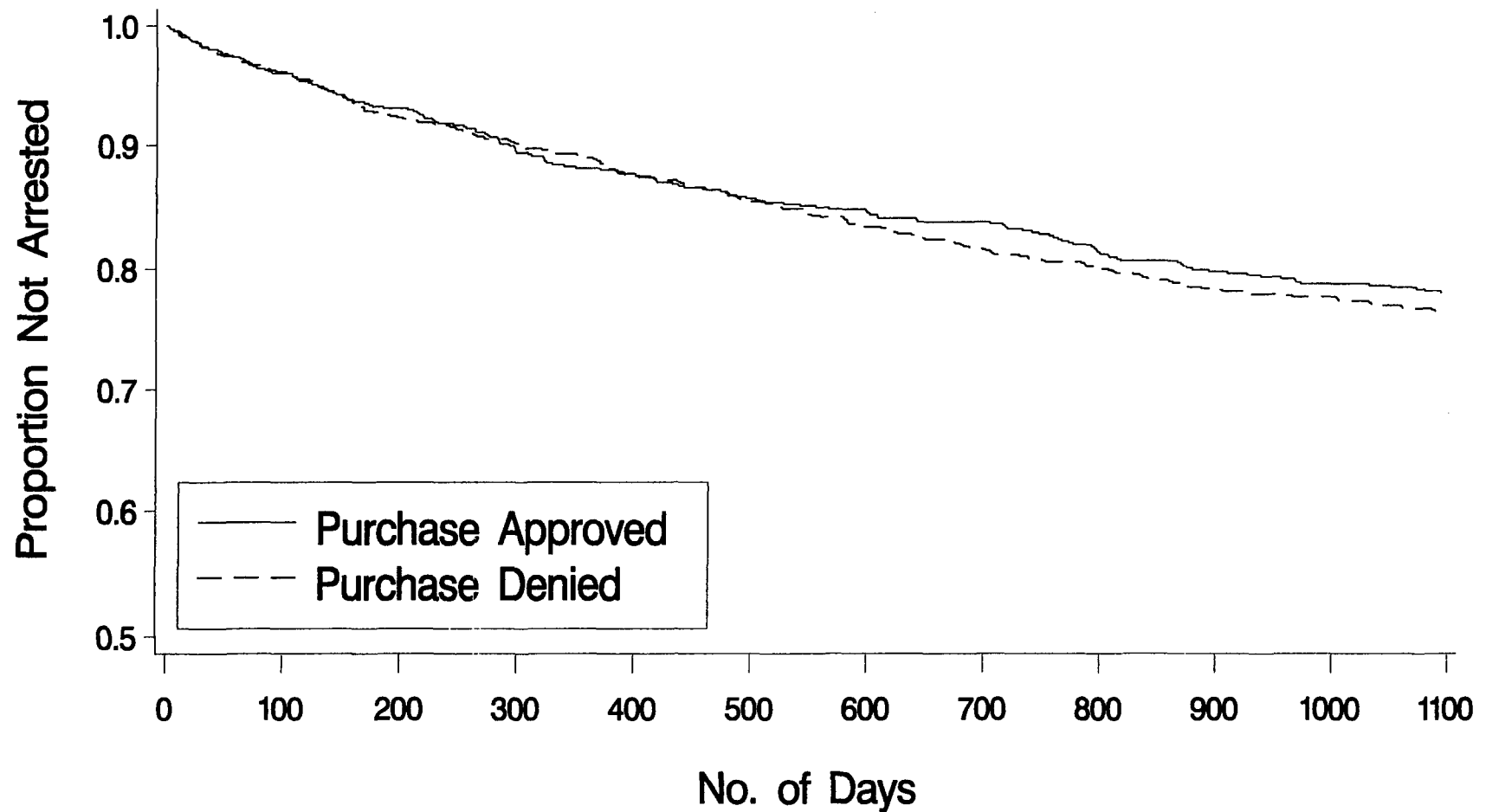
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Figure 2A. Kaplan–Meier Event Curves for New Arrests for Gun and/or Violent Crimes Among Approved Handgun Purchasers, by Age
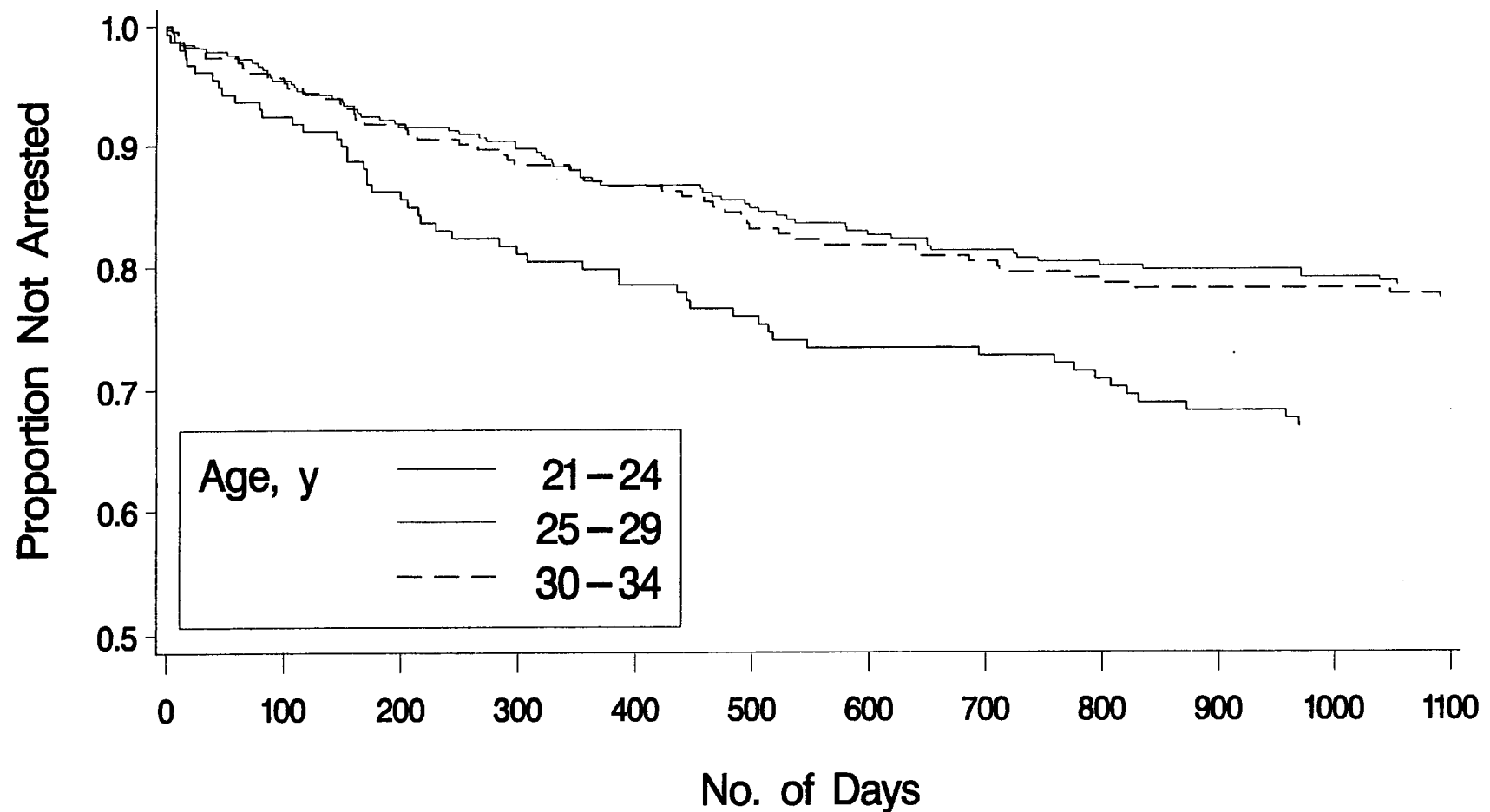

Legend:

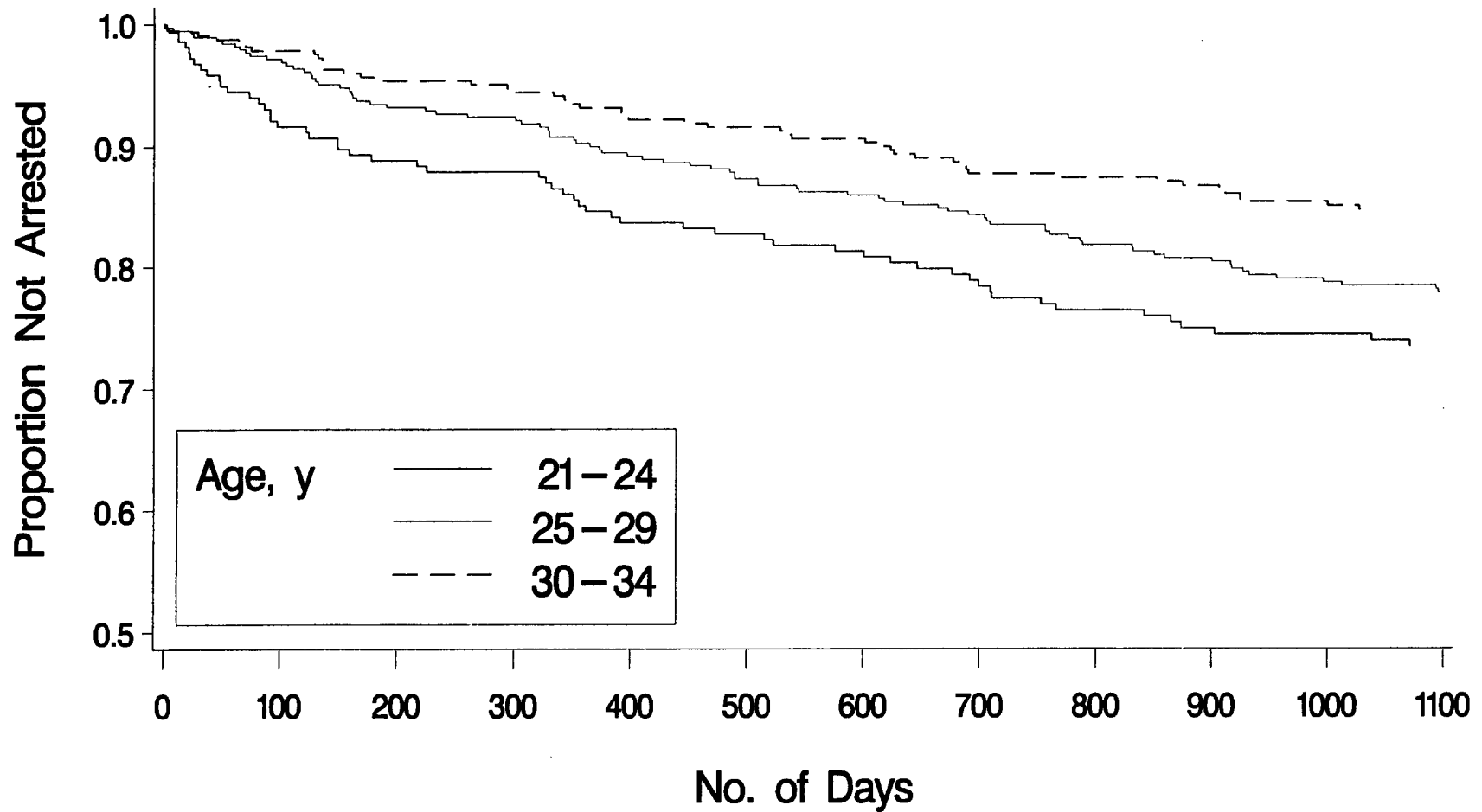Age, y
— 21–24
— 25–29
– – – 30–34

Y-axis: Proportion Not Arrested (0.5 to 1.0)

X-axis: No. of Days (0 to 1100)

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Figure 2B. Kaplan–Meier Event Curves for New Arrests for Gun and/or Violent Crimes Among Denied Handgun Purchasers, by Age

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Figure 3A. Kaplan–Meier Event Curves for New Arrests for Gun and/or Violent Crimes, Among Approved Handgun Purchasers, by Number of Prior Convictions for Any Crime
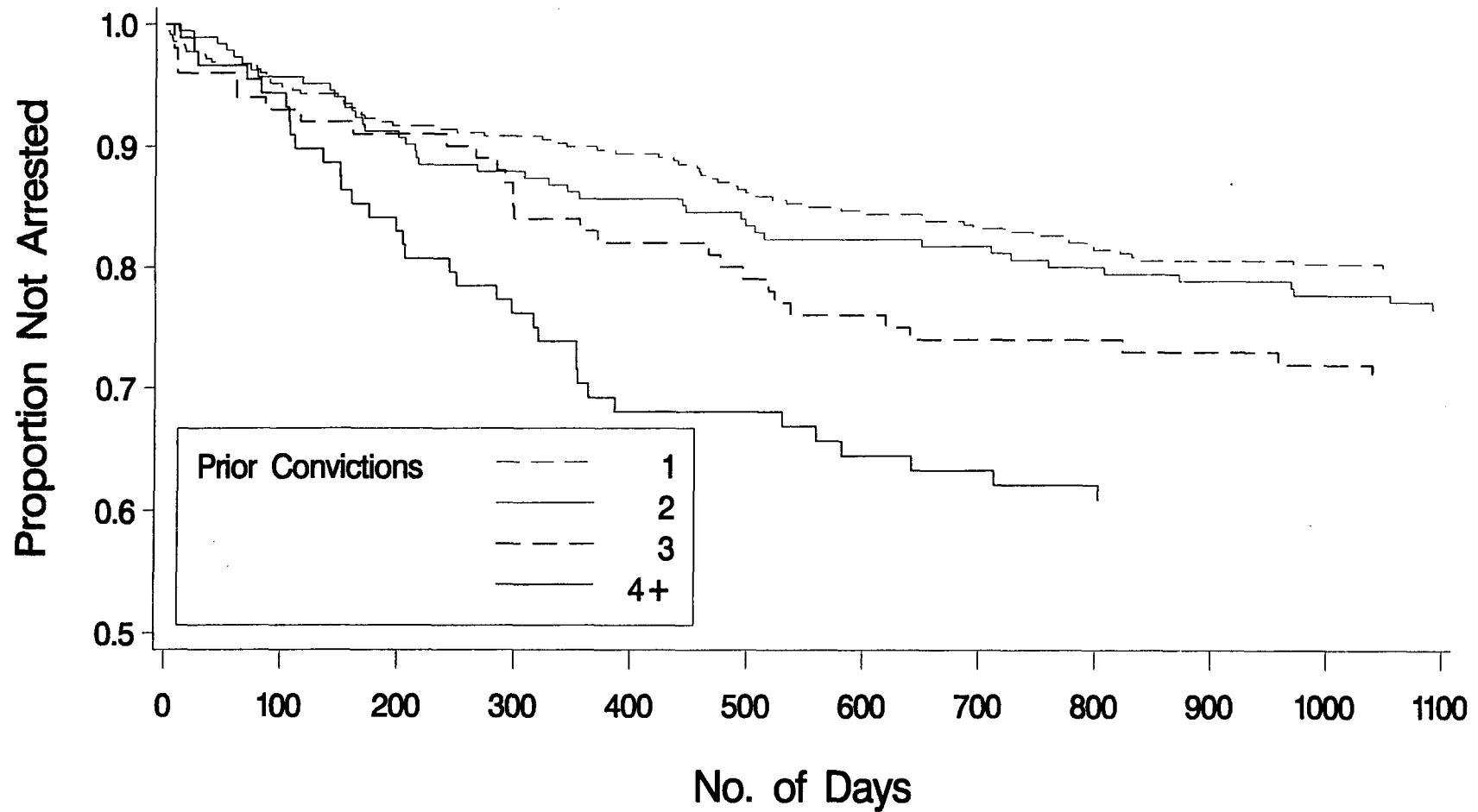

Proportion Not Arrested (y-axis: 0.5 to 1.0)

No. of Days (x-axis: 0 to 1100)

Prior Convictions
- – – – – 1
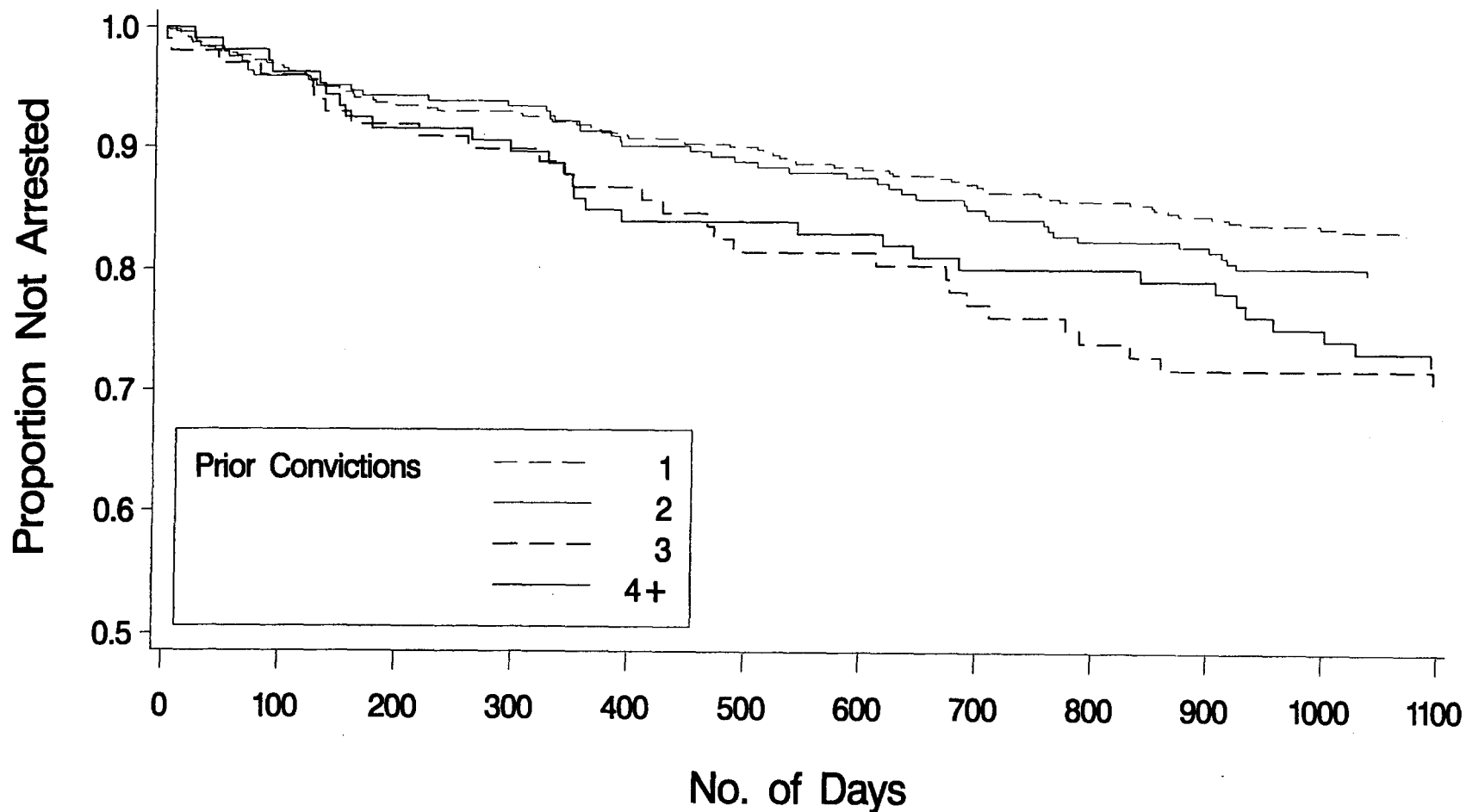- ———— 2
- – – – 3
- ———— 4+

Figure 3B. Kaplan–Meier Event Curves for New Arrests for Gun and/or Violent Crimes, Among Denied Handgun Purchasers, by Number of Prior Convictions for Any Crime

Legend:

Prior Convictions
- 1
- 2
- 3
- 4+

Y-axis: Proportion Not Arrested

X-axis: No. of Days