

Forthcoming, *Evaluating Gun Policy: Effects on Crime and Violence*, edited by Jens Ludwig and Philip J. Cook (November, 2002, Brookings Institution Press).

**Do Prison Sentence Enhancements Reduce Gun Crime?  
The Case of Project Exile**

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May 7, 2002

**Abstract**

This paper presents an analysis of the effects of enhancing prison penalties for gun carrying by diverting gun offenders from state to federal courts. We begin by presenting the first systematic evaluation of the widely-cited Project Exile in Richmond, Virginia. We argue that the decline in Richmond gun homicide rates surrounding the implementation of Project Exile was not unusual and that the observed decrease would have been likely to occur even in the absence of the program. This conclusion stems from the finding that cities with the largest increases in homicide rates during the 1980s and early 1990s also experienced the largest decreases during the late 1990s. We show that nearly all of the reduction in murder rates in Richmond following Project Exile may be attributed to the unusually large increase in gun homicides that occurred during the decade prior to Exile's implementation. Moreover, we find similar patterns for other types of crime. We then test for a more general relationship between federal firearm convictions and city-level homicide rates. Again, we find little statistically significant evidence of an impact.

We wish to thank Jordan Leiter, Robyn Thiemann and Eric Younger for assistance and Anthony Braga, Philip Cook, Jeff Fagan, Peter Greenwood, Steve Levitt, Lois Mock, Lawrence Sherman, and seminar participants at the Brookings Institution and the University of Pennsylvania for valuable comments. This research was prepared with support from the Annie E. Casey and Smith Richardson foundations as well as the National Institute of Justice; any errors and all opinions are those of the authors alone. Please direct comments to either [raphael@socrates.berkeley.edu](mailto:raphael@socrates.berkeley.edu) or [jludwig@brookings.edu](mailto:jludwig@brookings.edu).

## 1. Introduction

Unlike gun control, enhanced prison penalties for gun crimes enjoy widespread support from all sides of the U.S. gun policy debate. Prison sentence enhancements have the potential to reduce gun violence by incapacitating those who have been convicted of gun crimes and deterring such crimes in the future. Moreover, such a policy does not affect the ability of law-abiding adults to keep guns for self-defense or recreation.

Given this conceptual and political appeal, the Bush Administration has made enhanced prison penalties a centerpiece of its efforts to address the problem of gun violence. With a proposed budget of \$550 million over two years, one important objective of the Administration's "Project Safe Neighborhoods" is to enhance the penalties for gun crime by diverting those who have committed federal firearms offenses into federal court, where prison sentences are typically more severe than those found in most state systems.<sup>1</sup>

The Bush Administration's proposal to prosecute more firearms cases in the federal system is based on Richmond, Virginia's "Project Exile," first announced on February 28, 1997. This program has since been declared a dramatic success by observers from across the political spectrum including the National Rifle Association, Handgun Control, and Virginians Against Gun Violence, as well as news outlets such as the *New York Times* and the *Washington Post* and even President Bush.<sup>2</sup> These claims for Exile's success stem from the 40 percent reduction in gun homicides observed in Richmond from 1997 to 1998.

Despite this widespread acclaim, some skeptics have questioned the effectiveness of Project Exile due to the fact that homicides increased in Richmond in the last 10 months

of 1997 following the program's announcement. In fact, the Richmond homicide rate *increased* by 40 percent between 1996 and 1997. Despite these conflicting views of Project Exile's impact and the substantial policy interest in the program, surprisingly Project Exile has to date not been subject to a formal evaluation.

The present paper presents what we believe to be the first rigorous examination of the impact of Richmond's Project Exile on homicide and other crimes. We show that critiques of Exile focusing on the increase in homicide rates during the last 10 months of 1997 may be misplaced, given that the number of federal gun convictions in Richmond did not show any appreciable change between 1996 and 1997. At the same time, claims that Exile was successful based on the reduction between 1997 and 1998 in Richmond are also misguided, since Richmond had an unusually high murder rate in 1997 and, more generally, crime declined throughout the U.S. over this period.

In what follows, we argue that the reduction in Richmond's gun homicide rates surrounding the implementation of Project Exile was not unusual, and that almost all of the observed decrease is likely to have occurred even in the absence of the program. This conclusion is based on a very strong empirical regularity observed in city-level homicide rates: cities with the largest increases in homicide rates during the 1980's and early 1990's also experienced the largest decreases during the late 1990's. Richmond happened to be among the handful of cities that experienced unusually large increases in homicide rates during the '80s. Consequently, nearly all of the reduction in murder rates experienced by Richmond following Project Exile may be attributed to this large increase in gun homicides occurring prior to Exile's implementation. We also find nearly identical results for trends in other felony crimes.

In principle comparisons of crime trends across cities may yield misleading inferences about Exile's effects if unmeasured factors specific to Richmond would have driven the city's rates up even further in the late 1990's in the absence of the program. We address this potential omitted-variables problem in part by examining how the gap between adult and juvenile homicide arrest rates change in Richmond over time compared to other cities. Typically only adults are eligible for the "felon in possession" prosecutions that form the heart of the Exile intervention. Juveniles typically do not have prior felony records and should be largely unaffected by the program, thereby serving as a within-city control group against which one would compare adult homicide arrest rates. Since both adults and juveniles should be exposed to many of the same city-specific factors that affect local crime rates, the extent to which the decline in adult arrest rates exceeds the decline in juvenile arrest rate provides an alternative estimate of the impact of project Exile.

In fact, we find that adult homicide arrest rates increase relative to juvenile arrest rates in Richmond during the period surrounding the program's implementation. In contrast, adult arrest rates decline on average in relation to juvenile rates in other cities. These findings taken together call into question the empirical evidence commonly offered as evidence of Exile's impact.

We also present a more general analysis of the relationship between federal prosecutions of gun cases and gun homicide. This approach has the advantage of allowing data from the federal courts to identify the exact Exile "dose" experienced by Richmond and other cities that adopted Exile-like programs in each year. For the years 1994 through 1999, we matched information on the annual number of felon-in-possession

and felony-gun-use cases prosecuted by each U.S. Attorney's office to the cities corresponding to each U.S. Attorney district. We then use standard panel data techniques that allow us to control for unmeasured city fixed effects, and test for contemporaneous and lagged effects of the number of felons prosecuted in the federal system on city-level gun homicide rates. Consistent with our findings for Richmond's Project Exile, this analysis yields little evidence of a reduced-form relationship between the number of federal firearm prosecutions and city-level murder rates.

The remainder of the paper is organized as follows. In the next section we provide more information about Richmond's Project Exile and discuss the various ways in which the program may reduce gun violence. The third section presents descriptive comparisons of gun homicide trends in Richmond versus those observed in a number of comparison cities, while section four presents our formal evaluation of Exile's effects. The fifth section presents the more general reduced-form analysis of the effects of federal gun prosecutions on homicide. The sixth section discusses the policy implications of our findings in light of the limitations of the available evidence.

## **2. Project Exile: Design and Objectives**

The heart of the Project Exile program consists of the coordinated efforts of Richmond law enforcement and the regional U.S. Attorney's office to prosecute in federal courts all felon-in-possession of a firearm ("FIP") cases,<sup>3</sup> drugs/gun cases,<sup>4</sup> and domestic violence/gun cases, regardless of the number.<sup>5</sup> Exile also includes training for local law enforcement on federal statutes and search and seizure issues, a public relations campaign to increase community involvement in crime fighting, and a massive advertising campaign. The advertising campaign is intended to send the clear message of

zero tolerance for gun offenses and to inform potential offenders of the swift and certain federal sentence.<sup>6</sup>

Project Exile is effectively a sentence enhancement program since the federal penalties for these firearm offenses are more severe than those in effect in Virginia at the time Exile was announced in 1997. The disparity between the federal and state systems may be particularly dramatic for FIP convictions, for which the federal penalty is five years with no chance of early release. In addition to the differences in prison terms, gun offenders diverted into the federal system are denied bail at a higher rate than those handled in state courts, and serve time in a federal penitentiary that is likely to be located out of state.<sup>7</sup> Both aspects of the program are thought to impose additional costs on offenders.

The potential public safety effects of programs such as Project Exile are suggested by previous empirical research on the incapacitation and deterrent effects of incarceration more generally. Incapacitation occurs when individuals who would engage in criminal activity were they free to roam the streets are prevented from doing so because they are incarcerated. The best available research suggests that incapacitation effects may be considerable for the current population of prisoners,<sup>8</sup> although the average effect may decline with expansions in the prison population if the rate of criminality declines for the marginal inmate.<sup>9</sup>

While disentangling the effects of deterrence from that of incapacitation is difficult, a number of studies suggest that the threat of punishment does appear to deter criminal behavior. For example, economists Ian Ayres and Steven Levitt<sup>10</sup> find that areas where a larger fraction of cars are equipped with the anti-theft radio transmitter “Lojack” also

experience lower rates of auto theft, consistent with a substantial deterrent effect on car thieves. Additional support for the deterrence hypothesis comes from findings that areas with more police appear to experience lower rates of crime,<sup>11</sup> and even by evidence that the presence of additional referees on the sporting field reduces the number of infractions.<sup>12</sup>

Despite evidence for incapacitation and deterrence effects from imprisonment in general, the available evidence on the effects of specific sentence enhancement laws is more mixed. David McDowall, Colin Loftin and Brian Wiersema<sup>13</sup> use before/after comparisons and find that areas enacting sentence enhancements for firearm offenses experience a decline in gun crime following implementation of these laws. However in the absence of information about how crime changed in comparison areas that did not enact sentence enhancements, one cannot be sure that the observed crime reductions are attributable to the new sentencing policy. Consistent with this concern, Thomas Marvell and Carl Moody<sup>14</sup> find that crime trends in states that implement sentence enhancements do not decline relative to other states.

The most compelling effort to date to isolate the causal effects of sentence enhancements comes from Daniel Kessler and Steven Levitt,<sup>15</sup> who analyze the effects of the enhancements introduced by Proposition 8 in California. The authors examine how the difference in crime rates between offenses that are versus are not covered by Proposition 8 change over time before and after introduction of the new policy, and how this change over time compares to what was observed in the rest of the U.S. during this period. The analysis thus isolates the effect of those factors specific to California during the time Proposition 8 was introduced that affected only those crimes covered by the new

law. The authors find a short-term reduction in crimes covered by the enhancements of around 4 percent (presumably the result of the law's deterrent effect on criminals). Crime rates continue to decline over time due to the additional incapacitation effect that arises from incarcerating prisoners for a longer period of time.<sup>16</sup> However because the analysis by Kessler and Levitt does not focus on gun crimes specifically, the implications for Project Exile are somewhat unclear.

Uncertainty about the effectiveness of sentence enhancements extends to whether such policies are cost-effective. Philip Cook and Jens Ludwig<sup>17</sup> show that if Project Exile yielded crime reductions of the same magnitude as California's Proposition 8 – on the order of 15 to 20 percent in the steady state – then the program would be enormously cost-effective. In this case the program's cost of \$40 million through the first two years would yield a reduction in gun violence that is worth on the order of \$150 to \$240 million to society. On the other hand, others have argued that the sentence enhancements generated by California's "Three-Strikes" law are no more cost-effective than social programs that reduce crime.<sup>18</sup>

Whether Project Exile reduces crime in practice, and whether any such crime reductions yield benefits in excess of the program's costs, are empirical questions, to which we turn in the next section.

### **3. Homicide in Richmond During the 1990's**

Since the announcement of Project Exile in February 1997, several indicators of criminal activity in Richmond have improved substantially. For example, there has been a decrease in the number of guns seized by the police, a pattern often interpreted as a decreased propensity to carry guns among felons and those engaged in activities that can

be prosecuted under Exile. The volume of crime tips from residents has also increased. But the outcome measure that has received the most attention, and also serves as the primary focus of our own evaluation, is the city's homicide rate.

#### **a. Gun homicide in Richmond**

Figure 1 presents the trends in annual homicides, gun homicides and non-gun homicides that have served as the foundation for the perceived success of Project Exile in Richmond.<sup>19</sup> The numbers offered as evidence of Exile's effectiveness by the U.S. Attorney's Office for the Eastern District of Virginia and many others are calculated using the change in homicides occurring between 1997 and 1998. In this two-year period, the homicide rate declined by 35 percent while the gun homicide rate declined by nearly 40 percent, from 62 to 39 per 100,000.

However, offering this two-year period as evidence of the program's impact is problematic for several reasons. First, the program was (at least officially) in place for most of 1997, a year in which the homicide rate increased dramatically over the previous year. Hence, one could easily choose the two-year period from 1996 to 1997 to estimate the program's impact and conclude that Exile drastically increased homicide rates. Deciding which year should be counted as the first "post-Exile" period is clearly crucial to any evaluation of the program's effects.

To answer this question, we turn to data on the number of gun offenses prosecuted by the local U.S. Attorney's office for Richmond. As seen in Figure 2, the number of actual firearm prosecutions secured by the local U.S. Attorney did not show any noticeable increase until 1998, when the number of such convictions more than tripled compared to the 1997 total. In principle Exile may still have had some effect on crime in 1997

through an “announcement effect,” in which the publicity surrounding the program serves to change the expectations that criminals have about the penalties for gun offenses. It is also possible that word about Project Exile spreads among Richmond’s criminal population following the initial wave of federal indictments (which may have occurred in 1997) rather than convictions (which seem to have started in 1998). However, we accept the argument that actual convictions under Exile are likely to be an important part of the program’s deterrent effect, and thus choose 1998 as the first year in which Exile is considered to be in full effect. Note that our decision to count 1998 as the first post-Exile year has the effect of *increasing* the chances that we find a program effect.

Even with 1998 chosen as the first “Exile” year, whether the program has been successful is not obvious from Figure 1. The large year-to-year changes in homicide rates observed in Richmond suggest that much of the increase observed in 1997 may reflect transitory factors that would have disappeared anyway. Using this unusual year as a base for calculating the change is bound to inflate the apparent impact of the program. Moreover the patterns in Figure 1 appear to indicate that, the year 1997 aside, homicide rates in Richmond were trending downwards even before the launch of Project Exile. To the extent that the post-Exile declines simply reflect the continuation of trend, the raw numbers offered in support of the program are likely to overstate Exile’s impact.

To address these concerns, Table 1 presents a number of calculations based on the homicide rates displayed in Figure 1. Since Project Exile is designed to deter the use and illegal possession of firearms, the table and the following discussion focuses primarily on gun homicide rates.<sup>20</sup> To eliminate some of the year-to-year variation in homicides, the table presents the average annual homicide rates for the three two-year periods 93/94,

95/96, and 98/99. To avoid the problems associated with 1997, we omit this year. We derive our baseline estimate of the effect of Project Exile by calculating the change in average annual gun homicide rates from 95/96 to 98/99.

As seen in Table 1, between 95/96 and 98/99 the gun homicide rate declined by 15.6 homicides per 100,000 residents. This is equivalent to a 31 percent decline in gun homicides (a figure somewhat smaller than the 40 percent decline between 1997 and 1998). However, Table 1 also shows that gun homicides declined by 15 percent from 93/94 to 95/96, before Exile went into effect. If we assume that this trend in Richmond's homicide rate would have continued even in the absence of Exile, then a "difference-in-difference" calculation (last row, Table 1) suggests that the decline attributable to Exile above and beyond the pre-existing trend is approximately 6 gun homicides per 100,000 residents, or 15 percent.<sup>21</sup>

To be sure, it is impossible to assess whether the trend occurring earlier in the decade would have continued during the late 90s in the absence of Project Exile. However, we will present evidence below suggesting that the declines observed for Richmond around the time of Exile were not unusual, given the large increase in Richmond's homicide rates during the 80s. For now, it is sufficient to say that more reasonable estimates of the potential impact of Project Exile on gun homicide rates range from around one-quarter to two-thirds of the program effects that have been claimed in the past and cited in popular press accounts (6 to 15 per 100,000 versus 23 per 100,000).

#### **b. Richmond vs. Other Cities**

While these calculations are smaller than the effects claimed by program proponents, the declines in Richmond are still quite large. Any program that can claim to reduce

homicide rates by 15 to 30 percent is likely to be worth investing in and merits the attention of both policy makers and researchers. Before concluding that the program has had such impacts, however, one should consider what happened to murder rates in other cities where Exile-type programs were not put into place. It is entirely possible that gun homicide rates declined uniformly across cities, which would indicate that the changes observed for Richmond are not unusual. Moreover, it may be that city-level variation in homicide rates is so great that changes in even two-year averages such as those depicted in Table 1 are not uncommon.

Figures 3A and 3B begin to address these concerns by graphing the annual gun homicide rates in levels (Figure 3A) and natural logs (Figure 3B) for the period 1990 to 1999 for Richmond and for several groups of comparison cities. In addition to murder rate data for Richmond, each figure presents time series data for the 10 cities with the highest average gun homicide rates during the 1990s, for other cities in Virginia and for cities located in those states that share a border with Virginia, for all cities in states on the eastern seaboard, and for cities from across the country that are about the same size as Richmond (defined as those with residential populations between 175,000 and 225,000).<sup>22</sup> The gun homicide rate levels presented in Figure 3A provide comparisons of the absolute change in gun homicide rates in Richmond versus the comparison groups. Changes in the natural log of homicide rates (graphed in Figure 3B) are indicative of the relative percentage changes in gun homicide rates for each series.<sup>23</sup>

Figure 3A and 3B clearly highlight the unusually high homicide rates that Richmond has suffered throughout the 1990s. Figure 3A also indicates that the decline in homicide rates in Richmond around the time of Exile also occurred to some extent in other cities.

However, the absolute drop in Richmond homicide rates appears to exceed the drop experienced in the other high murder rate cities and those observed for the other comparison groups of cities. On the other hand, Figure 3B shows that year-to-year movements in the natural log of gun homicide rates around the time of Project Exile's introduction are comparable in Richmond and the comparison cities. Hence, while the absolute change in Richmond gun homicide rates surrounding the implementation of Project Exile is distinct, the relative (or percentage) change in gun homicide rates is comparable to those observed in the various comparison groups of cities.

This last point can be seen more clearly in Table 2, which presents annual average gun homicide rates for Richmond and the four comparison groups of cities from Figures 3A and 3B. The first two columns present the average annual gun homicide rate for the periods 95/96 and 98/99, the third column presents the absolute change in gun homicide rates, while the final column presents the percentage change in gun homicide rates. The absolute changes in the third column indicate that the change in gun homicide rates for Richmond omitting 1997 (-15.63) was considerably larger than the comparable changes observed in all four comparison groups (ranging from -3.35 to -6.62). The relative changes in gun homicide rates, however, are comparable. While gun homicides declined by 31 percent, the declines in the comparison groups of cities range from 20 to 35 percent.

When we expand the set of comparison cities to include all cities with populations of 100,000 or more with complete data,<sup>24</sup> the findings are similar. Figure 4A shows the number of cities (shown on the vertical axis) that experienced changes in gun homicide rates of various magnitudes from 95/96 to 98/99 (given on the horizontal axis), ranging

from very large declines on the left to some modest increase in homicide over this period on the right. The graph shows that the change in gun homicide rates in Richmond around the time of Exile is larger in absolute value (that is, “more negative”) than what was observed in most cities during this period.

However as before the story is somewhat different when we focus on changes in the natural log of gun homicide rates (again, approximately equal to the percentage change in gun homicide rates), as seen in Figure 4B. Here, Richmond’s decline is still larger than average, but is less of an outlier when compared to the overall distribution of proportional changes. Put differently, the proportional change in Richmond’s gun homicide rate is within the bounds of variation observed among other cities.

Taken at face value, the patterns discussed above are not inconsistent with a real effect of Project Exile on the number of homicides committed with firearms. Despite the comparable proportional declines in gun homicide rates displayed in Figure 4B, the absolute reduction in gun homicide observed in Richmond was quite large and outside of the range of variation observed in other cities. However, we argue in the next section that the unusually large absolute declines observed in Richmond around the time of Exile are less “unusual” than the comparison in Figure 4A would seem to suggest.

#### **4. Reassessing Exile: Accounting for Initial City Conditions**

In the previous section we noted that two basic facts lend preliminary support for an effect of Project Exile on homicide rates. First, the pre-post absolute decline observed for Richmond was larger on average than the comparable changes for other cities. Second, the size of the decline was such that it exceeded the range of variation observed among other cities. While the same cannot be said for the proportional changes in gun homicide

rates, the absolute changes provide a provocative comparison. However, these simple comparisons fail to account for the crucial role of Richmond's initial conditions in predicting future changes in city homicide rates.

Specifically, in addition to suffering among the highest urban homicide rates in the nation, Richmond also experienced unusually large *increases* in homicide rates during the decade or so preceding the implementation of Project Exile. These initial conditions carry important implications for our evaluation because those cities with the highest homicide levels during the early 1990's, and with the largest increases in homicide prior to this period, also experienced the largest decreases during the late 1990s. This suggests that the pre-post Exile change in homicides observed for Richmond may largely have been a function of the run-up in homicide rates during earlier periods.

#### **a. The relationship between earlier and later changes in homicide rates**

Why might we expect an inverse relationship between changes in homicide rates during the late 90s and comparable changes occurring during the late 80's and early 90's? One possibility might be that the underlying factors causing the large increases in homicide rates during the 1980s such as the violence associated with the introduction of crack cocaine (Blumstein 1995) or changes in the age structure (Greenberg 1985, Grogger 1998) ran their course, and hence murder rates were bound to decline. Another possibility might be that the incapacitation effects associated with the massive increase in incarceration rates (see Levitt 1996) may have disproportionately affected areas (or cities) with high crime rates. A third source lies in the possibility that many homicide victims may themselves be among the population of potential perpetrators.<sup>25</sup> To the

extent that this is the case, a rash of homicides would be followed by a reduction in homicide rates, as the pool of likely offenders is reduced.

Regardless of the underlying causes, the implication of this empirical regularity for evaluating the impact of Project Exile is clear: to some degree the decline in homicide rates observed in Richmond was to be expected. Hence, a careful evaluation of the program's impact requires taking into account earlier changes in the homicide rates.

Figures 5 and 6 provide more formal evidence of the relationship between later and earlier changes in city-level homicide rates during the 1980's and 1990's. Figure 5A plots each city's change in overall homicide rates from 95/96 to 98/99 (vertical axis) against the city's change over the prior decade from (85/86 to 95/96 on the horizontal axis). Figure 5B shows the comparable scatter plot for the same changes in gun homicide rates. Each figure includes a fitted linear regression lines (along with the estimated equation) that summarize the overall relationships between the homicide changes across time periods. In addition, the Richmond data point is explicitly identified.

For Richmond, the figures also provide the standardized residual from each regression, defined as the fitted residual (the differences between the actual pre-post Exile change in homicide rates and the change predicted by the regression line) divided by the standard error of the regression (the summary measure of the amount of variation around the regression line observed for the sample). A negative residual for Richmond that is large (in absolute value) relative to the regression's standard error (for example, at least twice the standard error) would provide evidence of an effect of Project Exile.

Both scatter plots in Figure 5 provide strong evidence of a negative relationship between earlier and later changes in homicide rates – i.e., areas that experience larger

increases in homicide rates initially go on to experience larger reductions thereafter. This relationship is expressed more formally by the fact that the regression coefficient relating previous homicide changes to later homicide changes is in both figures negative and highly statistically significant.<sup>26</sup>

In light of these findings, Richmond's experience during the 1990's does not appear to be unusual. In Figure 5A, the Richmond data point lies below the regression line, indicating that the decline in overall homicide rates for Richmond was large even after accounting for the increase occurring during the prior decade. However, the relationship summarized by the regression line predicts that Richmond should have experienced a decline in overall homicide rates of 13.1 (roughly 80 percent of the decline of 16.9 actually observed). Concerning the residual decline of 3.8, this is well within the bounds of variability around the regression line observed for the sample of cities. This is evidenced by the fact that the standardized residual is equal to  $-1$ .

Concerning the change in gun homicide rates, Figure 5B tells a similar story. Based on the fitted regression line, the increase of approximately 22 gun homicides per 100,000 experienced by Richmond during the late 1980s and early 1990s is predicted to cause a pre-post Exile decrease in gun homicide rates of 13.3. This is roughly 85 percent of the observed decline of 15.6. Moreover, the residual decline of 2.3 is less than the standard error of the regression (as is evidenced by the small standardized residual). Hence, Figures 5A and 5B indicate that effectively all of the pre-post Exile decline in Richmond gun homicide rates is attributable to the pre-Exile run-up in homicide rates.<sup>27</sup> The residual declines that remain after adjusting for the pre-Exile increases in homicide rates are not sufficiently large to conclude that the program had any effect whatsoever.

Figures 6A and 6B present comparable results where we use the change in the natural log of homicide rates rather than the change in the homicide rate levels to construct the scatter plots and estimate the regressions. Again, both figures provide strong evidence of a negative relationship between earlier and later change in homicide rates (as is evidenced by the significant negative coefficients on the change for the prior decade). In addition, both regressions indicate that the proportional change in homicide rates observed for Richmond are fully explained by the large proportional increases in homicide rates occurring prior to Exile's implementation. In fact, the regression lines in these figures predict that Richmond would have experienced even *larger* proportional declines in homicide without Project Exile than those that were actually observed (as is evidenced by the fact that in each scatter plot the Richmond data point lies above the regression line). Hence, the results in Figure 6 confirm the findings in Figure 5.

To summarize, the large increase in homicide rates occurring during the late 80's in Richmond coupled with the inverse relationship between earlier and later changes in homicide rates observed among other U.S. cities casts serious doubt on the validity of previous claims about the effects of Project Exile. Adjusting the decline in Richmond's homicide rates for the increase in murder rates during the 1980s leaves little residual decline in need of explanation.

#### **b. Some Tests of Robustness**

In what follows we examine the sensitivity of our findings and conclude that our results are fairly robust to methodological changes.

Perhaps the most obvious contention that one might raise in response to the results presented above concerns the fact that we have included Richmond in the sample

used to estimate the regression model. Since Richmond had a larger pre-Exile increase in homicide rates and experienced a large decline, perhaps the inclusion of this observation is causing us to over-estimate the relationship between earlier and later changes in homicide rates and thus understate the effects of Exile. To assess whether this criticism is important, we re-ran the regressions for the changes in the level of gun homicides with Richmond omitted and then calculated a fitted residual for Richmond based on these alternative regression results. Again, nearly all of the decline observed for Richmond is explained by the prior increase in homicide rates. Moreover, the residual decline is small relative to the regression standard error, indicating no evidence of an impact of Project Exile.

For our analysis of the proportional changes in homicide rates (Figure 6), this criticism does not apply since the Richmond data point lies *above* the regression line. This indicates that omitting this observation would actually increase the slope coefficients (that is to say, make it more negative) and cause Project Exile to appear even less effective than it does in our figures.

A related criticism concerns the fact that in 1998 and 1999 several cities that are included in our sample implemented Exile-type programs. If these cities had unusually large increases in homicide rates prior to Exile's introduction that were then substantially reduced by the program, then part of the estimated relationship between earlier and later changes in homicide rates may reflect the impact of Exile programs. Again, this would lead us to attribute too much of the decline in Richmond to the prior increase in homicide rates and understate the impact of Project Exile.

To examine whether those cities that implemented an Exile-like program during our sample period experienced larger reductions in homicide than other cities, we re-estimated the regression models included in Figures 5 and 6 with an additional dummy variable equal to one if the city has implemented a sentence enhancement program for gun crimes added to the model specification (results not shown).<sup>28</sup> In all cases the point estimate on the Exile variable is positive and not statistically significant, consistent with the idea that these programs have no measurable effect on homicide.<sup>29</sup>

Some might argue that despite the popular attention devoted to homicides as the outcome measure of choice for Project Exile, given the relative infrequency of homicides (even in a relatively violent city such as Richmond) it is more realistic to expect a program impact on other, more frequent types of crime. To address this concern, we replicated the analyses shown above using annual county-level crime and arrest data from the FBI's Uniform Crime Reports (UCR) system. Here, we restrict the sample of counties to those containing each of the cities in our sample analyzed above.<sup>30</sup> Because of the well-known problems associated with the UCR, including variation across areas and time in victim reporting to police and police reporting of crime data to the FBI, our UCR findings are far from definitive. Nevertheless, investigating the potential impact on other UCR "index" crimes provides a good robustness check to our analysis of murder rates.

Table 3 presents our analysis using the UCR crime data. Panel A of the table presents three columns of information for each of the eight UCR index crimes: the first column present the relative change in crime rates (98/99 – 96/95) for Richmond relative to the other counties in our sample, the second column presents the standard deviation of

these changes for all counties, and the final column presents the ratio of the relative changes in the first column to the standard deviations presented in second. As can be seen, incidence rates dropped for all crimes in Richmond relative to other cities with the exception of arson and auto theft. However, (with the exception of homicide) relative to the standard deviation of changes in crime rates observed in other cities, these changes are not large.

Panel B of Table 3 presents comparable regression-adjusted results for each of the crimes. The first column presents the fitted residual from Richmond from a regression of the change in the crime rate on the change in crime rates from 85/86 to 95/96. The second column provides the regression standard error for each model while the third column presents the fitted residual for Richmond divided by the regression standard error (which we refer to as the standardized residual). For all crimes, the observed residual is small relative to the standard error of the regression. Hence, there is little evidence in Table 3 of an impact of Project Exile on any of the part I felonies recorded by the FBI.

The most important concern with our analysis is whether we are able to distinguish the effects of Project Exile from those of other unmeasured factors that drive crime trends over time at the local level. Our comparison of Richmond homicide trends to those of other cities is intended to address this concern. However, it may be the case that such comparisons are invalid due to unobserved differences among cities in policing, age structure, and other factors that are likely to influence homicide rates.

An alternative way to identify the effects of unmeasured factors would be to define a control group of offenders *within the City of Richmond* who are not subjected to the stipulations of Project Exile. With such a control group, one could compare offense

rates (measured for example by homicide arrests) for this within-Richmond control group to offense rates for criminals subjected to Exile's provisions. We pursue this strategy by comparing changes in juvenile homicide arrests (pre-post Exile) to change in adult homicide arrests using county-level arrest data. The logic behind this comparison is based on the observation that juveniles will in general be ineligible for federal prosecution under Project Exile, since very few juveniles have been convicted of a felony in the adult criminal justice system. In other words, illegal gun possession by teens will not be eligible for federal prosecution under the "felon in possession" statute that seems to account for the large majority of Richmond's federal gun convictions (Figure 2).

Table 4 presents the results of these tabulations.<sup>31</sup> Tabulations of changes in homicide arrests per 100,000 residents are presented for all counties with population exceeding 100,000, Richmond, and all large counties excluding Richmond. The first column presents changes in all murder arrests. The second and third column presents changes in adult murder arrests and juvenile murder arrests, respectively. Finally, the last column presents the difference between the change in adult murder arrests and the change in juvenile murder arrests.

The disaggregated arrest data from the UCR indicate that adult homicide arrests actually increased relative to juvenile homicide arrest in Richmond, pre-post Project Exile. Moreover, adult homicide arrests declined on average during the late 1990's for adults relative to juveniles for other counties with populations of 100,000 or more. Taken together these findings are not consistent with the idea that some intervention specific to Richmond – such as Project Exile – served to reduce crime rates among adults relative to juveniles.

## **5. Testing for an Impact of Federal Prosecutions Using Panel Data**

While the program implemented in Richmond entailed a host of efforts ranging from community outreach to new officer training, the main provision of Project Exile is the unlimited prosecution of those found to be in violation of federal gun laws by the regional U.S. Attorney's office. An alternative way to test the impact of this intervention is to directly examine the effects on homicide from increases in the number of federal firearm convictions secured by the local U.S. Attorney's office.

Relating federal gun convictions to homicide rates directly has several advantages. First, this strategy recognizes that the Exile "dose" that Richmond experiences is not the same during every year of the program's existence. The most notable example in this regard is the similarity of federal gun convictions between 1997 and 1996 in Richmond, even though Project Exile was officially announced in February 1997.

A second advantage of defining federal gun cases as the "treatment" of interest is that we can directly control for the fact that other cities may have copied Richmond and implemented Exile-style programs during the late 1990's; to the extent that these cities replicated Richmond's Project Exile faithfully, they will also experience an increase in federal firearm convictions. More generally, Project Exile is not the only or even the first concerted effort by law enforcement to use federal prosecutions of gun cases. In the early 1990s, the federal program Project Triggerlock was introduced to systematically prosecute in federal courts violent offenders that use firearms during drug offenses and certain violent offenses. The number of federal prosecutions under this program were curtailed somewhat by a 1995 Supreme Court decision ruling that defendants in drugs/guns cases must have actively used the firearm in committing the offense to be

charged in a federal court.<sup>32</sup> Nonetheless, a certain amount of activity of this sort is observed in all regional U.S. Attorney's offices.

To implement this analysis we assembled information on the number and type of convictions secured for each of the local U.S. Attorney's offices throughout the country for each year from 1994 to 1999. We focus initially on measuring changes in the number of felon in possession of a firearm (FIP) convictions, since many observers believe that increases in the penalty for this offense is the real innovation of Richmond's Project Exile. However, we also replicate our analysis using cases where the defendant was convicted of any federal firearms charge. For each of the cities in the analytic sample used in the previous section we identify the local U.S. Attorney's office that is responsible for prosecuting federal cases in that area.

Table 5 presents the results of regressing the number of gun homicides per 100,000 residents (or the first difference of this variable) against the number of FIP convictions per 100,000 city residents (or their first difference).<sup>33</sup> Panel A presents four regressions that use the level (as opposed to the change) in each city's homicide rate as the outcome measure of interest. The first two specifications regress the gun homicide rate on the contemporaneous number of convictions while the second two specifications regress the gun homicide rate on the numbers of convictions lagged by one year. Regression results are presented with and without year dummies. Since the FIP and gun-use conviction rates are the same for cities located within the same U.S. Attorney jurisdiction, the assumed error structure for all models includes district-level variance components. This ensures that we do not under-estimate the standard errors of the slope coefficients due to within-district correlation of the regression error terms.

The results in Panel A indicate a statistically significant (at the one percent level) negative relationship between the number of FIP convictions and gun homicide rates. This relationship is evident in both the models that enter the contemporaneous conviction rate (regressions (1) and (2)) and the lagged conviction rate (regressions (3) and (4)). In all four regressions, the coefficient on the number of gun-use convictions is either the wrong sign and significant or insignificant. Hence, these preliminary regression results provide support for an effect of FIP convictions but not for an effect of gun-use convictions.

One problem with the models in Panel A, however, is that they ignore the possibility that some cities may always have higher or lower gun homicide rates compared to other cities for reasons that are not explicitly modeled by our regression model. This may be a particular concern for our application because the regression model does not include any additional covariates to control for differences across cities in police resources, poverty, racial or age composition of the population or other potentially relevant factors. One way to overcome this problem is to only compare the change in gun homicides over time across cities, which eliminates the influence of unmeasured city fixed effects that cause some areas to have persistently higher or lower gun homicide rates year after year. This is achieved by first-differencing all variables and re-estimating the model by regressing the one-year changes in homicide rates on the one-year changes in conviction rates.

Panel B of Table 5 shows that the first-difference models provide little support for the idea that additional federal FIP convictions, or gun convictions more generally, have a statistically significant negative relationship to city homicide rates. This finding is robust to whether we regress the change in gun homicide rates against the contemporaneous or

lagged change in FIP or other gun convictions, and to whether we control for period effects by including year dummies in the model.

As a further check we re-estimate the first-difference model where the dependent variable is the change in the two year averages of gun homicide rate four years apart –i.e., 99/98 minus 94/95 -- and the explanatory variables are the comparable changes in the FIP and gun use conviction rates. Using these longer time periods provides a good robustness check for several reasons. First, there is greater variation in the FIP and gun-use conviction rates over the longer period, a fact that increases the likelihood of identifying an effect. Second, looking at changes over a longer time period will yield an estimate of the relationship that should be less sensitive to the lag specification.<sup>34</sup>

These alternative results are presented graphically in Figures 7A and 7B. Replicating our analysis using this “long difference” approach yields qualitatively similar results to those presented in Panel B of Table 5, where the relationship between FIP or other gun convictions and homicides are near zero and not statistically significant.

The results presented in this and the previous section taken together do not provide support for the idea that Project Exile had any detectable effect on homicide rates in Richmond or in the larger set of cities that began to divert eligible gun offenders from state to federal courts.

## **5. Conclusion**

The widespread enthusiasm for Richmond’s Project Exile (and other programs designed to enhance prison terms for gun crime) is understandable: the program enjoys political support from all sides in America’s contentious debate about gun control, and a superficial examination of the data suggests that Exile may have had a dramatic impact

on gun homicides. Setting aside the troublesome data from 1997 in Richmond, when Exile was ostensibly in effect even though there was no increase in federal gun convictions, the declines from 95/96 to 98/99 in gun homicide rates in Richmond are larger than those observed in most other cities of comparable size (although the proportional decline in Richmond is less remarkable by comparison).

However, the impressive declines in gun homicide rates in Richmond around the time of Project Exile can be almost entirely explained by the fact that the city had unusually large increases in gun homicides through the mid-1990's, and that cities with larger-than-average increases in gun homicide rates subsequently experience unusually large declines. This finding appears to be robust to a variety of ways to analyze the available data, including decisions about whether to measure changes in homicide rates in actual or proportional terms or to define the treatment as the existence of an Exile-type program or instead as the actual number of federal firearms convictions that are secured.

Our results also hold when we define the outcome measure of interest as the difference in homicide or other arrest rates between adults and juveniles. Since Exile's design makes the program applicable primarily to adult offenders, program impacts should be concentrated among adults. On the other hand other unmeasured local factors may affect both adult and juvenile arrest rates, and so focusing on the difference between adult and juvenile trends in Richmond versus other cities helps isolate Exile's effects from those of other confounding variables.

One potential qualification to our empirical methodology concerns the possibility that the mean-reversion observed in city-level homicide trends may in and of itself be driven by efficacious local policy responses. Specifically, if localities implement creative and

effective policies to reduce homicide only when homicide levels cross some unacceptable threshold, then the larger reductions in homicide observed among high-homicide cities may be the result of the policy responses of those cities. Hence, the large drop observed for Richmond may be attributable to the aggressive federal prosecution of FIPs, while the declines observed for cities like Boston or New York may be attributable to alternative policy response, such as community policing programs or crack downs on “quality-of-life” infractions. If this were the case, the effective policies of other localities would serve to empirically mask the effectiveness of Project Exile in Richmond.

However, if this were the case, one would still expect to see an impact of FIP convictions on homicide rates among cities that implemented a variety of other crime-fighting programs. For example an increase in FIP prosecutions in federal courts in New York City should yield additional crime reductions above and beyond those produced by the city’s changes in policing practices. We were unable to find such an effect in our panel data analysis. Moreover, we have argued that the Exile intervention in Richmond focuses largely on adult offenders. Presumably whatever criminal justice or other interventions that are implemented in other jurisdictions around 1997 are not so narrowly targeted. In this case we would expect to observe a greater decline in adult offenses compared to juveniles over time in Richmond than in other cities; however the best available data are not consistent with this expectation.

To be sure, our finding that Project Exile did not drive the decline in Richmond gun homicides during the late 1990s begs the question of what exactly was responsible for the observed decline. While we cannot answer this question decisively, taking a more detailed look at the age and racial composition of gun homicide victims during the later

1990s may shed some light on the issue. Tabulations from the U.S. Vital Statistics indicate that for the 95/96 to 98/99 time period, gun homicide victimization rates for young African-American males in Richmond declined considerably while comparable rates for the rest of the population declined modestly.<sup>35</sup> To be specific, the two-year gun homicide rate for black males 10 to 24 years of age declined from 276 per 100,000 in the 95/96 period to 206 per 100,000 in 98/99.<sup>36</sup> The comparable figures for all other population groups are 39 and 30. In addition, the large increase in homicide rates over the decade prior to Exile's implementation was driven to a large decrease by increased victimization rates among young black males. Moreover, the large spike in homicide during the year 1997 is due entirely to a spike in the young black male homicide rate.

These patterns are quite similar to the patterns of increase and decline in high homicide rate cities documented by Philip Cook and John Laub, who demonstrate that most of the increase in homicides during the 1980's and 1990's is due to gun homicides committed against and by young black males.<sup>37</sup> The leading explanation for this substantial increase in gun violence is the diffusion of crack markets, which in turn is thought to have increased the demand for guns among the young people involved in these markets in order to enforce drug transactions.<sup>38</sup> The magnitude of these changes and the symmetries between the homicide increase and decline suggest that changes in the involvement of youth and guns in drug markets is a natural starting point for any explanation of crime patterns in the 1990's in Richmond and other American cities.

One larger lesson from our analysis of Richmond's Project Exile is the apparent tendency of the public to judge any criminal justice intervention implemented during a period of increasing crime as a failure, while symmetrically judging those launched

during the peak or downside of a crime cycle as a success. Given that policymakers, news reporters and voters all seem to employ this heuristic device in evaluating government programs, Project Exile would appear to highlight the enduring maxim that it is often better to be lucky than good.

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<sup>1</sup> The two-year budget for Project Safe Neighborhoods includes \$24.3 million to hire 207 new Assistant United States Attorneys devoted to prosecuting firearms cases, as well as \$125 million in grants to local agencies to hire gun prosecutors ([www.whitehouse.gov/news/releases/2001/05/20010514-2.html](http://www.whitehouse.gov/news/releases/2001/05/20010514-2.html) accessed on February 18, 2002). See also “Bush Pitches \$550 Million Fight Against Gun Crime,” CNN, May 14, 2001; “Ashcroft Says Justice to Target Illegal Firearms,” David Morgan, Reuters, August 27, 2001.

<sup>2</sup> See for example “Have Gun? Will Travel,” by Elaine Shannon, *Time Magazine*, August 16, 1999, 154(7); and “Remarks by the President on Project Safe Neighborhood,” The White House, Office of the Press Secretary, May 14, 2001.

<sup>3</sup> U.S. Code Title 18, 922(g) (1).

<sup>4</sup> U.S. Code Title 18, 924 (c).

<sup>5</sup> In principle the local U.S. Attorney for Richmond also has the option of prosecuting those who sell a handgun or ammunition to juveniles [U.S. Code Title 18, 924 (x)] although in practice federal prosecutors rarely take such cases, in part because the penalty for the first conviction of this offense is simply probation.

<sup>6</sup> For a detailed description of Project Exile, see the summary statement of the U.S. Attorney’s Office for the Eastern District of Virginia (<http://www.vahv.org/Exile/Richmond/PE-R005.html>).

<sup>7</sup> Schiller (1998).

<sup>8</sup> Levitt (1996).

<sup>9</sup> Donohue and Siegleman (1998).

<sup>10</sup> Ayres and Levitt (1998).

<sup>11</sup> Marvell and Moody (1996), Levitt (1997), Corman and Mocan (2000).

<sup>12</sup> McCormick and Tollison (1984). For a more comprehensive review of the deterrence literature see Nagin (1998) and Levitt (2001).

<sup>13</sup> McDowall, Loftin, and Wiersema (1992).

<sup>14</sup> Marvell and Moody (1995).

<sup>15</sup> Kessler and Levitt (1999).

<sup>16</sup> Since those criminals whose sentences are enhanced by Proposition 8 would have been incarcerated in any case, the longer prison sentences caused by the new California sentencing system exert a deterrent but not an additional incapacitation effect on crime in the short run. The only incapacitation effects from the new law occur when those prisoners sentenced under Proposition 8 have served out the sentence that they would have received under the old regime and spend the additional years in prison as a result of the new sentencing system.

<sup>17</sup> Cook and Ludwig (2000, p.128).

<sup>18</sup> Greenwood et. al. (1994, 1996), Greenwood (2002).

<sup>19</sup> We calculate homicide rates by weapon using the Combined Supplemental Homicide Reports (SHR) Files covering the period from 1976 to 1999. To aggregate Federal Bureau of Investigation Organized Reporting Units up the level of individual cities, we use a special cross-walk file that maps the link between law enforcement districts and Federal Information Processing Standards geographic boundaries. To calculate, homicide rates, we use place level population data from the US Census Bureau rather than the population data available in the SHR file, due to some observed discrepancies for certain cities.

<sup>20</sup> Because of the possibility of substitution from gun to non-gun violence, a reduction in gun homicide is a necessary but not sufficient condition for a program impact on the overall homicide rate. However, given that the large majority of homicides are committed with guns, we find quite similar findings when we focus on all homicides, gun and non-gun together.

<sup>21</sup> In the U.S. Attorney’s assessment of the percentage impact of Project Exile on murder rates, it is stated that the program caused a nearly 40 percent decline in homicide rates. The averaged change between 95/96 and 98/99 shows a smaller change of approximately 30 percent. To calculate an estimate of the percent change under the assumption that the 93/94 to 95/96 change reflects an underlying trend, we would need to calculate what the murder rate would have been in 98/99 in the absence of Project Exile by subtracting the earlier change from the 95/96 murder rate. Based on this assumption, the murder rate during 98/99 would have been 41.33 in the absence of Project Exile. The estimated effect of a 6.4 per 100,000 decline in gun homicide rates constitutes 15.5 percent of this base. Hence, as with the absolute changes, the implicit percentage changes implied by the alternative estimates in Table 1 are considerably more modest.

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<sup>22</sup> Richmond is omitted from all comparison groups. Between 1990 and 1999, the population of Richmond varied between approximately 203,000 and 190,000 residents.

<sup>23</sup> The change in the natural log of a variable is approximately equal to the percentage change in the variable.

<sup>24</sup> Over this time period, Richmond's population ranged between approximately 190,000 and 200,000. We dropped the city of Gary Indiana due to the fact that the 1996 homicide total from the SHR was approximately ½ the number of homicides reported in published Uniform Crime Report, thus creating the false impression of a sharp increase in homicide rates during the late 1990s for this city.

<sup>25</sup> There appears to be considerable overlap between the populations of potential offenders and victims: the large majority of both groups have prior criminal records (Kennedy, Piehl and Braga, 1996, McGonigal et al., 1993, Schwab et al., 1999, Kates and Polsby, 2000).

<sup>26</sup> The slope coefficient in Figure 5A indicates that one unit increase in overall homicide rates between 85/86 and 95/96 is associated with a 0.48 decrease in gun homicide rates during the pre-post Exile period, while the comparable figure for gun homicide in Figure 5B is 0.53.

<sup>27</sup> We also estimated the models in Figures 5A through 5B weighting the regression by the mid 90's population of each city. Doing so does not alter our conclusions.

<sup>28</sup> Cities that implemented Exile-type programs during our sample (in addition to Richmond) include Oakland, Buffalo, Norfolk, Rochester, Atlanta, Chicago, Philadelphia, Pittsburgh, and Kansas City. One complication with identifying the list of Exile-type cities is that many cities call their programs "Exile" although they do not follow the same practice as in Richmond of diverting gun offenders from state to federal courts. At the same time some cities that do substantially increase the number of diversions may have different names for their programs that we may have missed. We address this problem in the next section by using data from local U.S. Attorney offices on the number of gun convictions per year to identify the Exile "dose" that each city experiences.

<sup>29</sup> Another way to test this hypothesis is to calculate the ratio of each city's fitted residual to the city's actual or imputed error standard deviation; ratios that are negative and larger than two in absolute value are consistent with an effect of Exile to reduce homicide. None of the cities that we identify as having Exile-type programs had such residuals.

<sup>30</sup> Since there are several instances in our city sample where more than one city is in a single county, this county-level data set has fewer observations (131 to be exact) than our city-level data set. The city of Richmond is separately identifiable in the county data.

<sup>31</sup> In this instance, we use the change in arrests 95/96 to 98 as the key dependent variable rather than the change from 95/96 to 98/99 due to the fact that arrest data by age is not yet available for 1999.

<sup>32</sup> The case *Bailey vs. the United States* along with a discussion of trends in federal firearm prosecutions is discussed in Scalia (2000).

<sup>33</sup> We also estimated these models taking the log of dependent and explanatory variables. The results are similar to those presented in Table 5.

<sup>34</sup> Over a longer time period, whether the proper lag is one, two or three periods (etc.) is less relevant, since the impact of the trend in the explanatory variable should have ample time to exert an impact on the dependent variable.

<sup>35</sup> We calculated these figures using mortality data and the data extraction system CDC Wonder, administered by the U.S. Center for Disease Control and Analysis. We accessed this data interface on May 6, 2002 at <http://www.cdc.gov>.

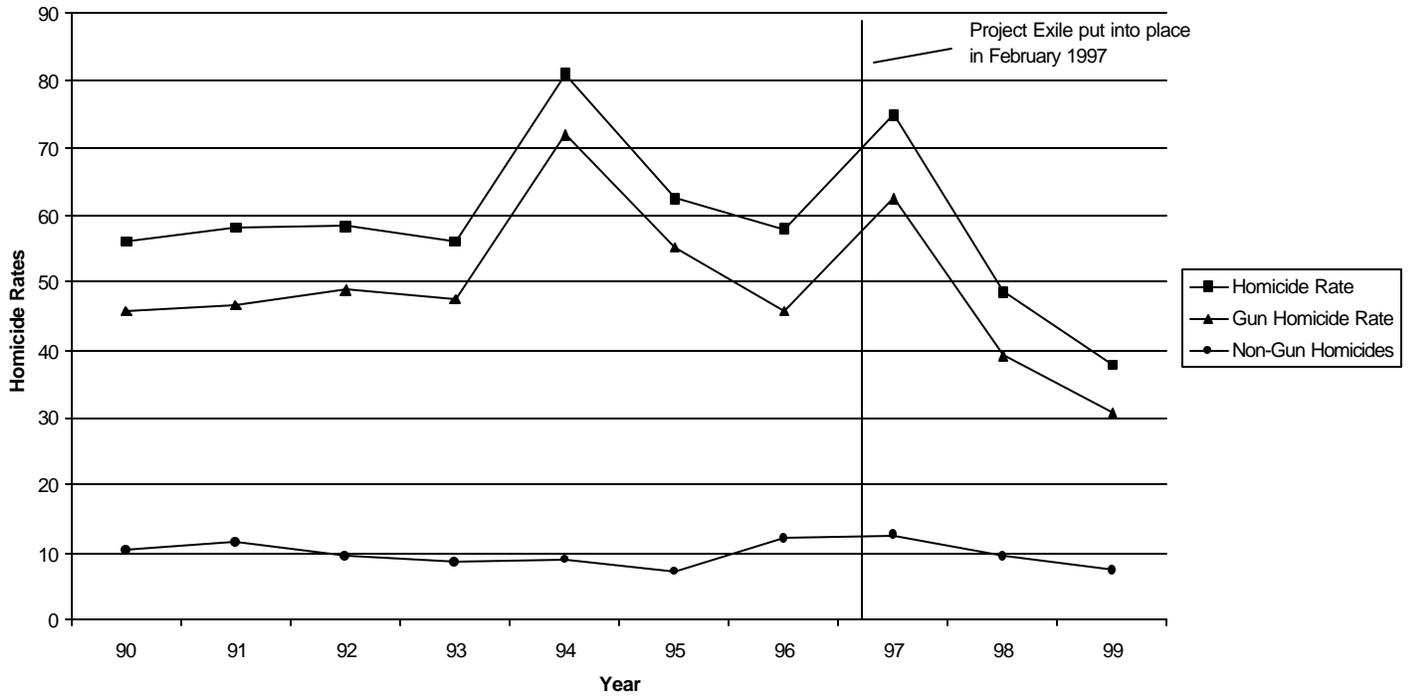
<sup>36</sup> For the four years used in these comparisons, black males between 10 and 24 year of age constituted roughly 6 percent of Richmond's population while accounting for 42 percent of gun homicide victims.

<sup>37</sup> Cook and Laub (2001).

<sup>38</sup> Blumstein (1995); Cork (1999).

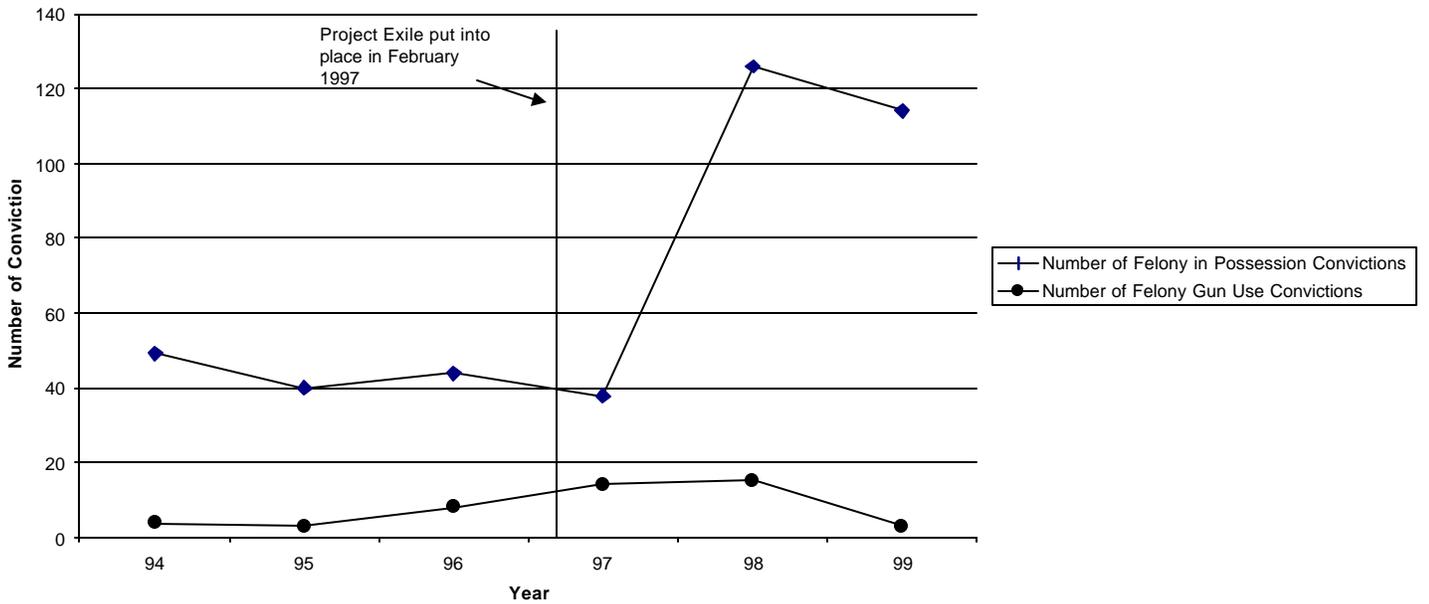
**Figure 1**

**All Homicides, Gun Homicides, and Other Homicides per 100,000 Residents in Richmond Virginia, 1990-1999**



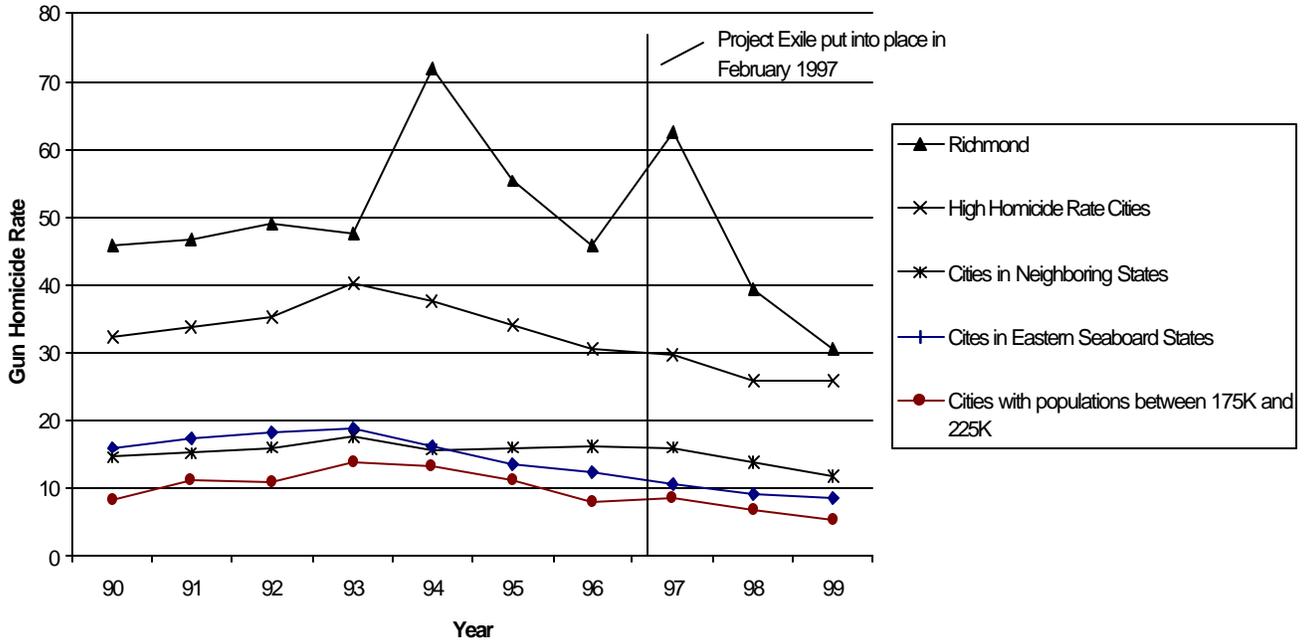
**Figure 2**

**The Annual Number of Felon-In-Possession and Felony-Gun-Use Convictions Prosecuted by the U.S. Attorney's Office for the Eastern District of Virginia, 1994 through 1999**



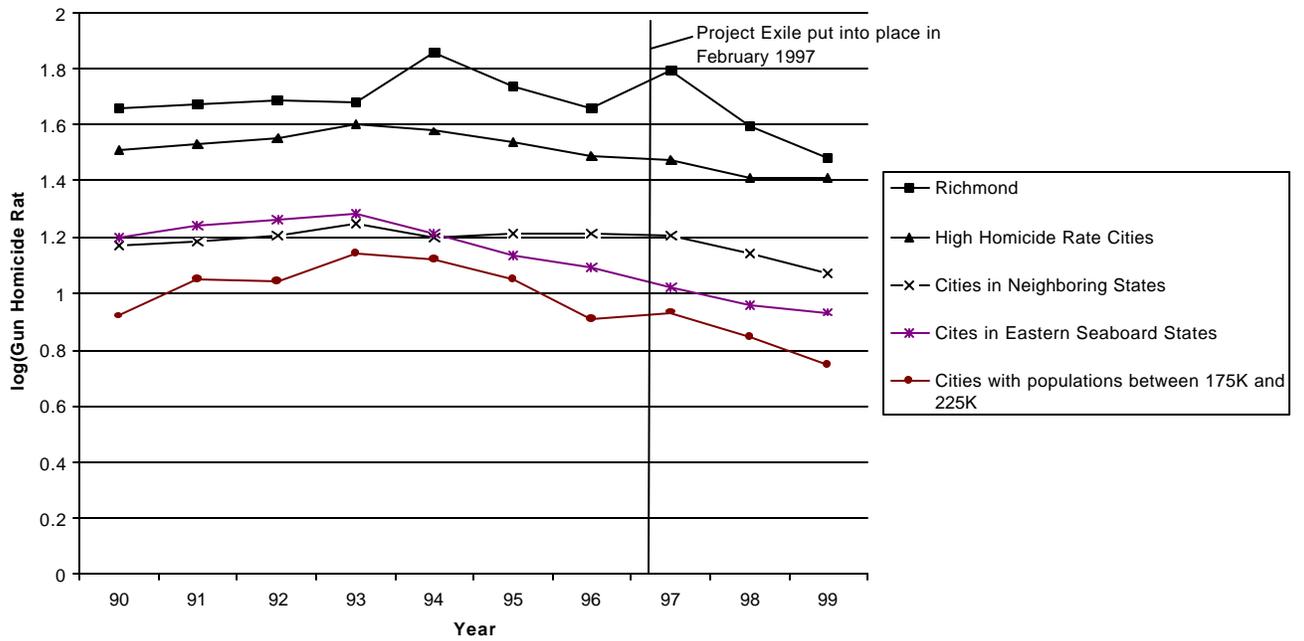
**Figure 3A**

**Gun Homicides per 100,000 residents in Richmond, other High Crime Cities, Cities in States Surrounding Virginia, and Cities on the Eastern Seaboard**

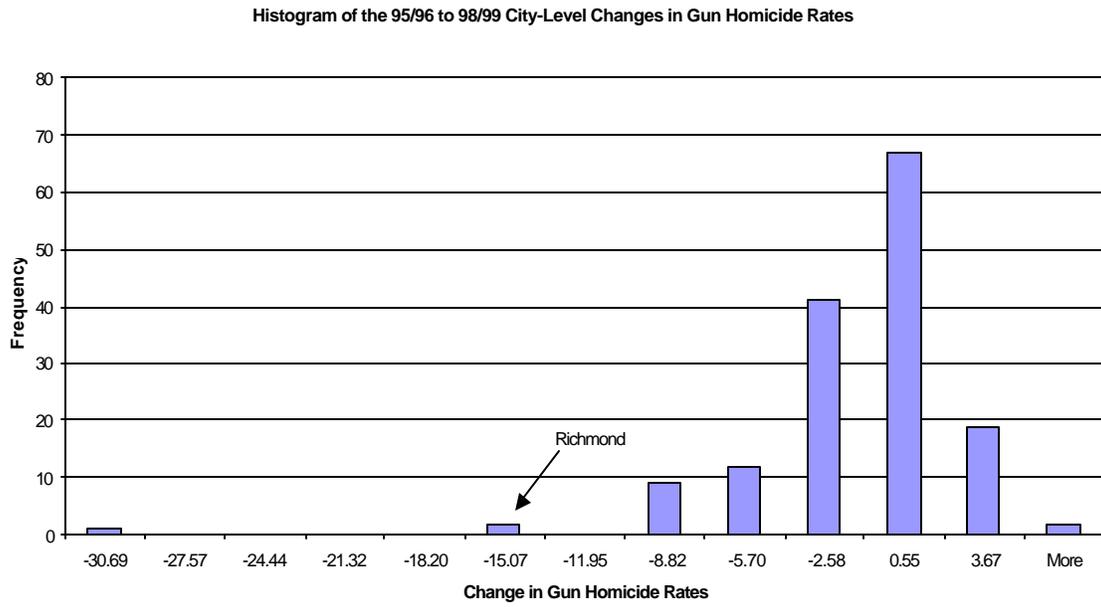


**Figure 3B**

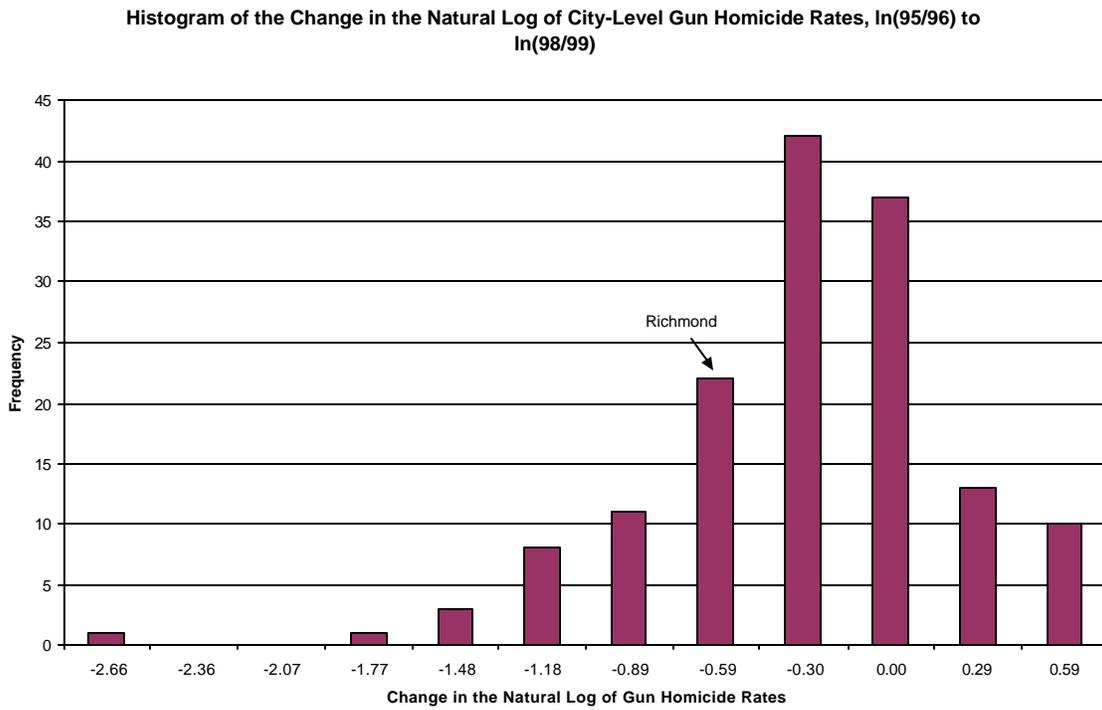
**The Natural Log of Gun Homicides per 100,000 Residents in Richmond, Other High Crime Cities, Cities in States Surrounding Virginia, Cities on the Eastern Seaboard, and Cities with Comparable Populations**



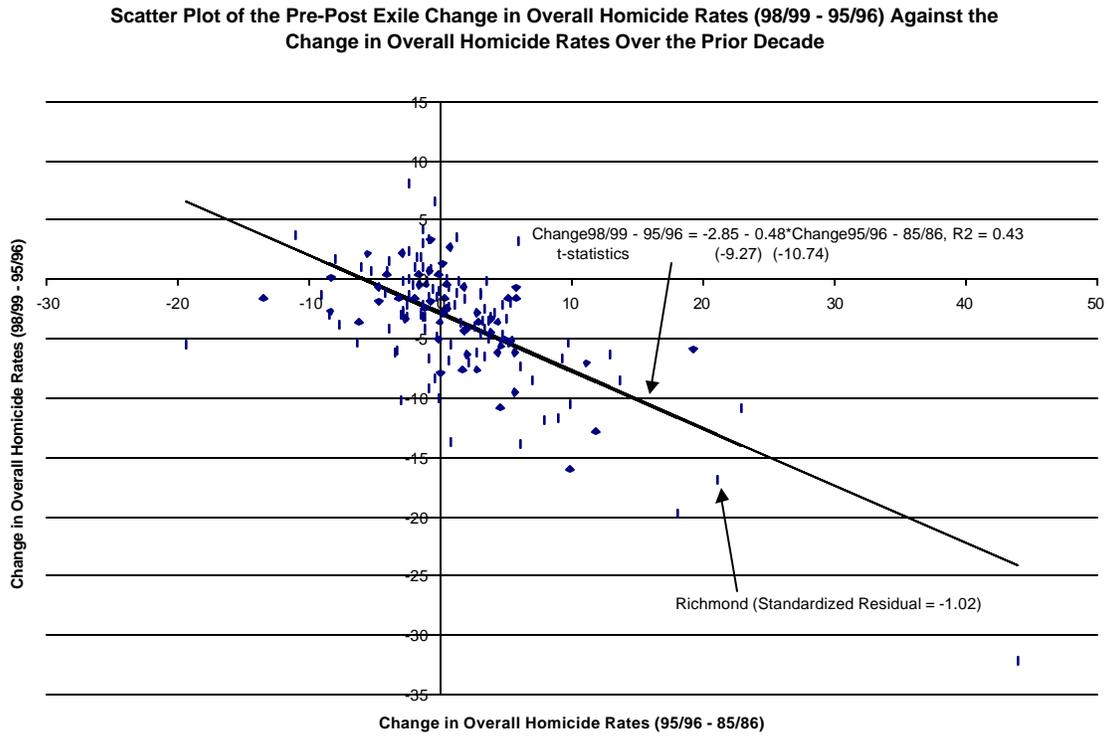
**Figure 4A**



**Figure 4B**



**Figure 5A**



**Figure 5B**

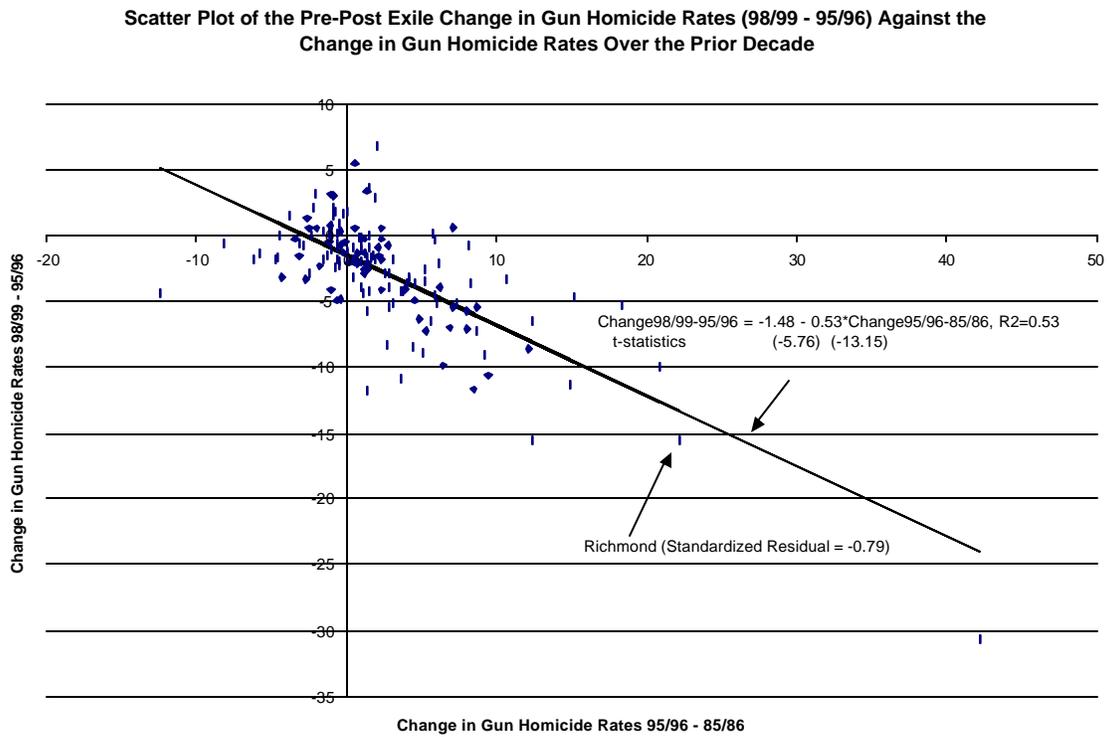


Figure 6A

Scatter Plot of the Pre-Post Exile Change in the Natural Log of Overall Homicide Rates ( $\ln(98/99) - \ln(95/96)$ ) Against the Change in the Natural Log of Overall Homicide Rates Over the Prior Decade

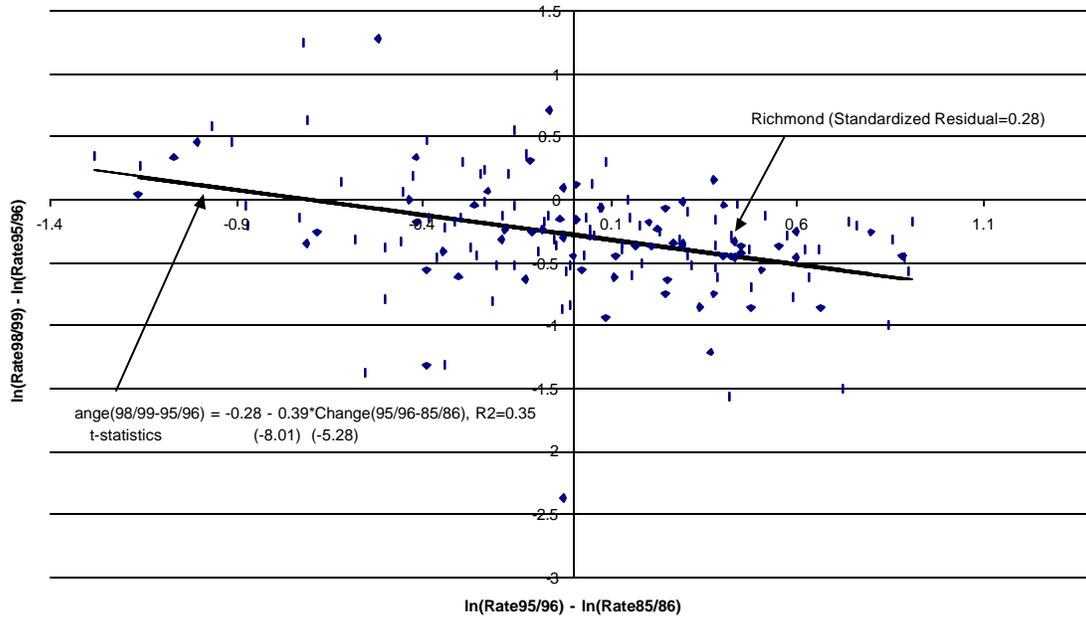
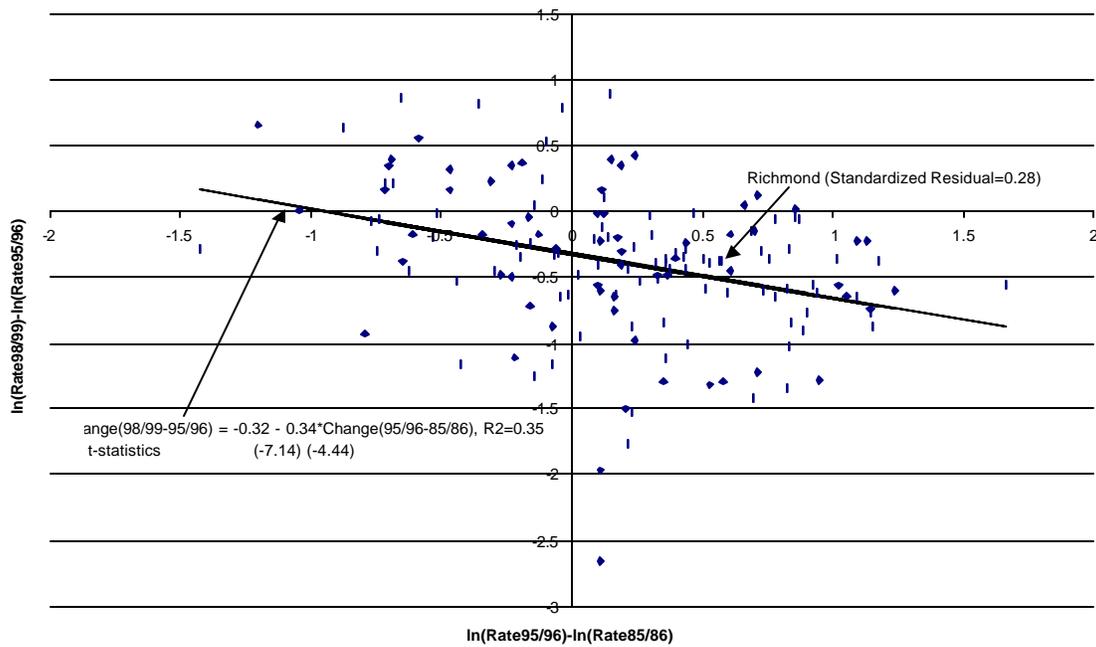


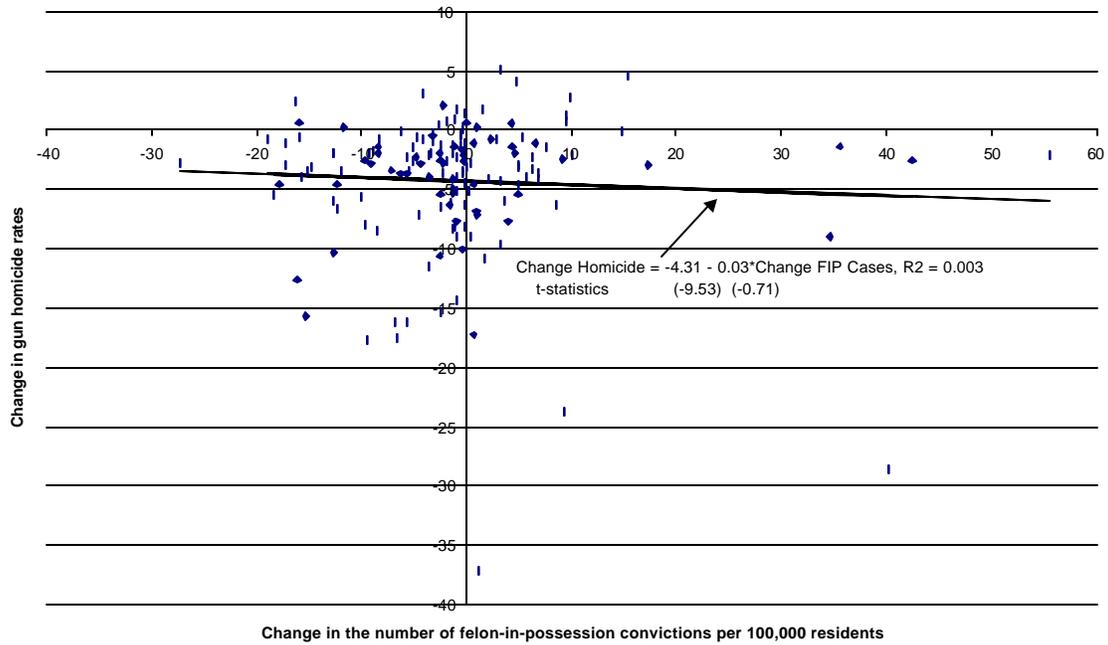
Figure 6B

Scatter Plot of the Pre-Post Exile Change in the Natural Log of Gun Homicide Rates ( $\ln(98/99) - \ln(95/96)$ ) Against the Change in the Natural Log of Gun Homicide Rates Over the Prior Decade



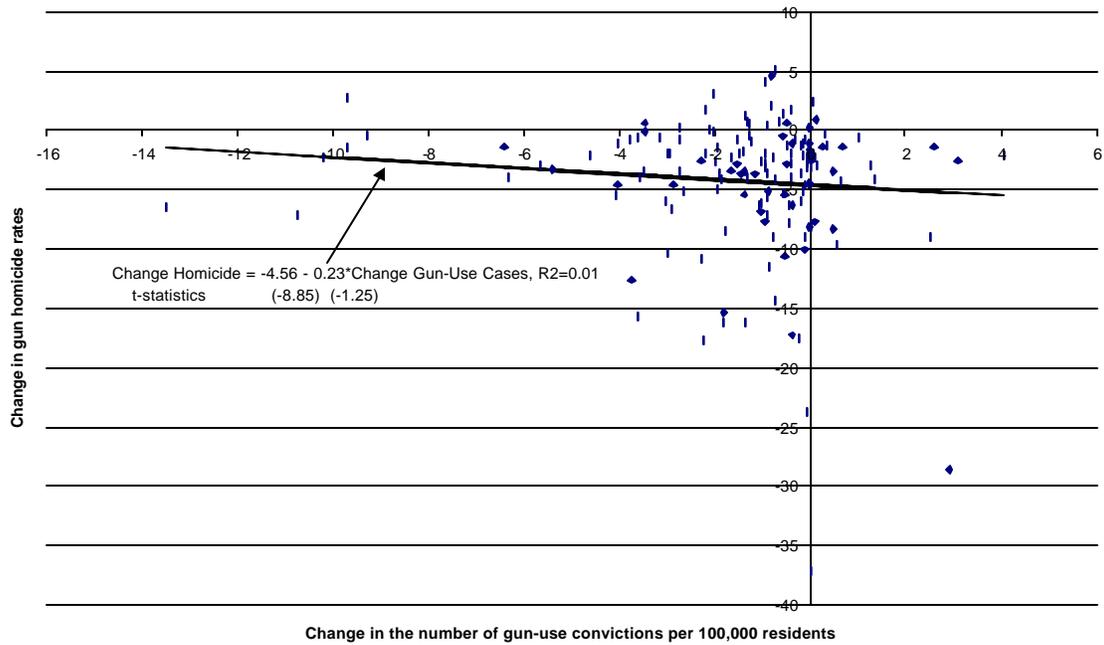
**Figure 7A**

**Scatter Plot of the 94/95 to 98/99 Change in Gun Homicide Rates Against the 94/95 to 98/99 Change in the Number of Federal Felon-in-Possession Prosecutions per 100,000 Residents**



**Figure 7B**

**Scatter Plot of the 94/95 to 98/99 Change in Gun Homicide Rates Against the 94/95 to 98/99 Change in the Number of Federal Gun-Use Convictions per 100,000 Residents**



**Table 1**  
**Change in the Gun Homicide Rate Relative to the Non-Gun Homicide Rate for**  
**Richmond During the Time Period Surrounding the Introduction and Implementation of**  
**Project Exile**

	Gun Homicide Rate	Non-Gun Homicide Rate	Gun Minus Non-Gun
Time period			
93/94	59.76	8.32	51.44
95/96	50.55	9.19	41.36
98/99	34.92	6.82	28.10
[1] Diff: 98/99 –	-15.63	-2.36	-13.26
95/96	(-30.9%)	(-25.7%)	(-5.2%)
(% Change)			
[2] Diff: 95/96 –	-9.22	0.87	-10.09
93/94	(-15.4%)	(10.5%)	(-25.9%)
(% change)			
[1] - [2]	-6.41	-3.23	-3.17
(Difference in	(-15.5%)	(-36.2%)	(20.7%)
Percent Changes)			

Tabulations based on extractions from the Uniform Crime Reports Supplementary Homicide Reports, 1976-1999.

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**Table 2****Change in Gun Homicides Rates in Richmond, Other High Crime Cities, Cities in States Surrounding Virginia, Cities on the Eastern Seaboard, and Cities with Comparable Populations, 95/96 to 98/99**

Group of Cities	Gun homicide rate, 95/96	Gun homicide rate, 98/99	Change	% Change
Richmond	50.55	34.92	-15.63	-31%
High homicide	32.50	25.88	-6.62	-20%
Neighboring states	16.24	12.80	-3.44	-21%
Eastern seaboard	12.92	8.85	-4.07	-32%
Comparable populations	9.63	6.28	-3.35	-35%

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Tabulations based on extractions from the Uniform Crime Reports Supplementary Homicide Reports, 1976-1999. The city groups correspond to those used in constructing Figures 3A and 3B.

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**Table 3****Pre-Post Exile Changes in FBI Index Crimes Relative to Changes in All other Cities, Unadjusted and Adjusted for Pre-Exile Changes in Crime Rates****Panel A: Changes Unadjusted for Base Crime Levels**

	Change in Richmond (98-95/96) - average change in all other counties	Standard deviation of changes for all counties	Ratio of relative change for Richmond to the standard deviation for all counties
Change in murder	-11.69	4.03	-2.90
Change in rape	-10.09	8.80	-1.15
Change in robbery	-100.89	81.76	-1.23
Change in assault	-133.27	107.74	-1.23
Change in burglary	-268.02	158.54	-1.69
Change in larceny	-335.89	420.28	-0.79
Change in auto theft	203.87	178.76	1.14
Change in arson	16.63	27.42	0.60

**Panel B: Regression Adjusted Changes**

	Richmond Residual <sup>a</sup>	Regression Standard Error <sup>b</sup>	Standardized Residual <sup>c</sup>
Change in murder	-1.85	2.48	-0.75
Change in rape	-9.49	8.81	-1.08
Change in robbery	-67.51	80.47	-0.84
Change in assault	-104.04	104.34	-0.99
Change in burglary	-258.51	159.02	-1.62
Change in larceny	-259.79	417.27	-0.62
Change in auto theft	234.46	177.99	1.32
Change in arson	17.83	27.52	0.65

Data come from county-level tabulations of the FBI Uniform Crime Reports.

a. These figures are the fitted residuals for Richmond from a regression of the 98/99-95/96 change in the given crime rate on the 95/96-85/86 change.

b. These figures are the estimates of the regression standard error from regressions of the 98/99-95/96 change in the given crime rate on the 95/96-85/86 change.

c. The standardized residual is calculated by dividing the fitted residual for Richmond by the regression standard error.

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**Table 4**  
**Change in Homicide Arrests by Age for Richmond and Other Large Counties, 98 – 95/96**

	Change in All Murder Arrests	Change in Adult Murder Arrest	Juvenile Murder Arrests	Change Adult - Change Juvenile
All Counties	-0.83	-0.74	-0.14	-0.60
Richmond	10.53	9.77	0.25	9.52
All Counties less Richmond	-0.83	-0.74	-0.14	-0.60

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Data come from county-level tabulations of the Uniform Crime Reports.

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**Table 5****Panel Data Regression Models of the Percent Change in Gun Murder Rates Caused by a Percent Change in the Number of Felon-in-Possession Cases Handled by Regional Federal Attorneys****Panel A: Dependent Variables Equals the Gun Homicide Rates**

	(1)	(2)	(3)	(4)
In possession	-0.131 (0.023)	-0.115 (0.023)	-	-
Uses a firearm	0.255 (0.124)	-0.041 (0.128)	-	-
In possession, lagged	-	-	-0.134 (0.026)	-0.126 (0.025)
Used a firearm, lagged	-	-	0.089 (0.130)	-0.073 (0.131)
Year dummies	No	Yes	No	Yes

**Panel B: Dependent Variable Equals the Year-to-Year Change in the Gun Homicide Rate**

	(1)	(2)	(3)	(4)
Change, in possession	-0.013 (0.021)	-0.012 (0.021)	-	-
Change, uses a firearm	-0.029 (0.063)	0.005 (0.064)	-	-
Change, in possession lagged	-	-	0.007 (0.023)	0.003 (0.023)
Change, used a firearm lagged	-	-	-0.058 (0.060)	-0.093 (0.067)
Year dummies	No	Yes	No	Yes

Standard errors are in parentheses. All models include a constant term. In each model, the assumed error structure is a variance-components model with random effects for each district. The  $\Delta$  in possession variable is the number of cases per 100,000 city residents where one of the primary offenses of the felon charged in the regional office is possession of a firearm. The  $\Delta$  Used a firearm variable are the number of cases per 100,000 where one of the primary offenses involves the use of a firearm. Changes in these variables and the dependent variable in Panel B refer to one year single differences. Lagged variables refer to one-period lagged explanatory variables. Our data set covers the period from 1994 to 1999. For level models with a lag and for specifications (1) and (2) in panel B, the year 1994 is omitted by necessity. For the specification (3) and (4) in Panel B, the sample is restricted to observations between 1995 and 1999.