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7 *Prison Sentence Enhancements: The Case of Project Exile*

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 individuals 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 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 firearm offenses into federal court, where prison sentences are typically more severe than the ones found in most state systems.[1]

1 • The two-year budget for Project Safe Neighborhoods includes $24.3 million to hire 207 new distant U.S. attorneys devoted to prosecuting firearms cases, and $ 125 million in grants to local agencies to hire gun prosecutors (White House, press releases, 2001). 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.

The Bush administration's proposal to prosecute more firearm 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 George W. Bush.[2] 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, pointing out that homicides increased in Richmond in the last ten 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.

This chapter 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 ten 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 United States over diis period.

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 probably would 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 1980s and early 1990s also experienced the largest decreases during the late 1990s. Richmond happened to be among the handful of cities that experienced unusually large increases in homicide rates during the 1980s. Consequendy, 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

2. See, for example, Elaine Shannon, "Have Gun? Will Travel," *Time Magazine,* August 16, 1999, p. 154; "Remarks by the President on Project Safe Neighborhoods," White House, Office of the Press Secretary, May 14, 2001; Michael Janofsky, "New Program in Richmond Is Credited for Getting Handguns OffStreets," *New York Times,* February 10, 1999.

before 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 1990s 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 with 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, how much the decline in adult arrest rates exceeds the decline in juvenile arrest rates 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 uses 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 used standard panel data techniques that allowed 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.

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, drugs-gun cases, and domestic violence-gun cases, regardless of the number.[3] 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.[4]

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, and as we document below, most of the additional federal convictions under Exile appear to be FIP cases. Besides 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.[5] Both aspects of the program are thought to impose additional costs on offenders. In sum, the primary criminal-justice change introduced by Project Exile appears to be an increase in the prison penalties for carrying guns by those with prior felony convictions.

The potential public safety effects of programs such as Project Exile are suggested by previous empirical research on the incapacitadon 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 incapacitadon effects may be considerable for the current population of prisoners,[6] although the average effect may decline with expansions in the prison population if the rate of criminality declines for the marginal inmate.[7]

While disentangling the effects of deterrence from that of incapacitation is difficult, several studies suggest that the threat of punishment does seem to deter criminal behavior. For example, economists Ian Ayres and Steven Levitt find that areas where a larger fraction of cars are equipped with the antitheft radio

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3. U.S. Code Title 18, 922(g) (1); U.S. Code Tide 18, 924 (c). 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)J, although in practice federal prosecutors rarely take such cases, in part because the penalty for the first conviction of this offense is simply probation.

4. For a detailed description of Project Exile, see the summary statement available from the U.S. Attorney's Office for the Eastern District of Virginia.

5. Schiller (1998).

6. Levitt (1996).

7. Donohue and Siegelman (1998).

transmitter "Lojack" also experience lower rates of auto theft, consistent with a substantial deterrent effect on car thieves.[8] Additional support for the deterrence hypothesis comes from findings that areas with more police appear to experience lower rates of crime, and even by evidence that the presence of additional referees on the sporting field reduces the number of infractions.[9]

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 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.[10] 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 find tliat crime trends in states that implement sentence enhancements do not decline relative to other states.[11]

The most compelling effort to date to isolate the causal effects of sentence enhancements comes from Daniel Kessler and Steven Levitt, who analyze the effects of the enhancements introduced by Proposition 8 in California.[12] The authors examine how the difference in crime rates between offenses that are covered by Proposition 8 versus those that are not changes 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 United States during this period. The analysis thus isolates the effect of those factors, specific to California when 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 owing to die additional incapacitation effect that arises from incarcerating prisoners for a longer period.[13] However,

8. Ayres and Levitt (1998).

9. For studies on police presence and crime, see Marvell and Moody (1996); Levitt (1997); Corman and Mocan (2000). See also McCormick and Tollison (1984). For a more comprehensive review of the deterrence literature see Nagin (1998) and Levitt (2001).

10. McDowall, Loftin, and Wiersema (1992).

11. Marvell and Moody (1995).

12. Kessler and Levitt (1999).

13. 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 wert 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 m prison as a result of the new sentencing system.

because the analysis by Kessler and Levitt does not focus on gun crimes, 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 show that if Project Exile yielded crime reductions of the same magnitude as California's Proposition 8—15 to 20 percent in the steady state—then the program would be enormously cost effective.[14] 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 approximately $150 to $240 million to society. However, 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.[15]

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 now turn.

Homicide in Richmond during the 1990s

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 this evaluation, is the city's homicide rate.

Gun Homicide in Richmond

Figure 7-1 presents the trends in annual homicides, gun homicides, and non-gun homicides that are the foundation for the perceived success of Project Exile in Richmond.[16] The numbers offered as evidence of Exile's effectiveness by the

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14. Cook and Ludwig (2000, p. 128).

15. Greenwood and others (1994, 1996); Greenwood (2002).

16. 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 U.S. Census Bureau rather than the population data available in the SHR file, owing to some observed discrepancies for certain cities.

Figure *7-1. All Homicides, Gun Homicides and Other Homicides per 100,000 Residents in Richmond, Virginia, 1990-99*

Rate per 100,000 residents
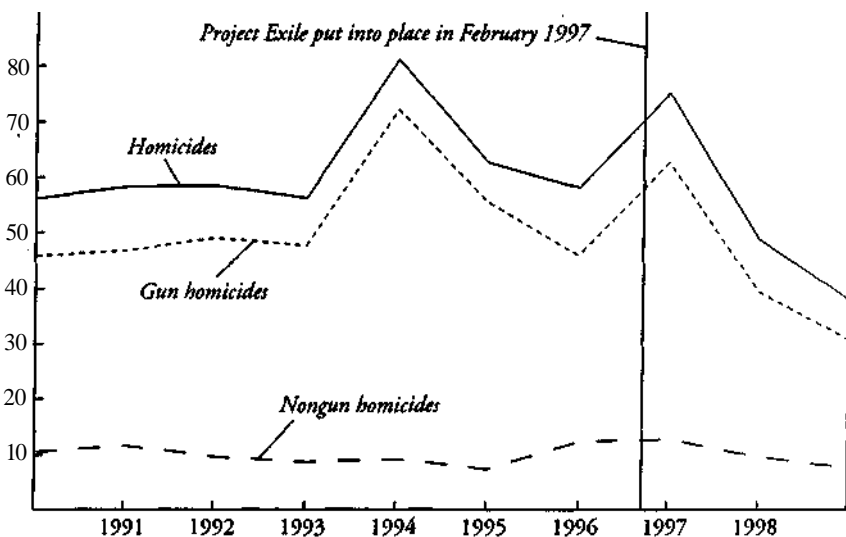
U.S. Attorney's Office for the Eastern District of Virginia and many others are calculated using the change in homicides occurring between 1997 and 1998. In this two-year period, the homicide rate declined by 35 percent while the gun homicide rate declined by nearly 40 percent, from 62 to 39 per 100,000.

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 crucial to any evaluation of the program's effects.

As seen in figure 7-2, the number of firearm prosecutions secured by the local U.S. Attorney's office for Richmond did not show any noticeable increase until 1998, when the number of such convictions more than tripled compared with the 1997 total (with most of the change accounted for by additional FIP convictions). In principle Exile may still have had some effect on crime in 1997 through an "announcement effect," in which the publicity surrounding the program changes the expectations that criminals have about the penalties for

Figure 7-2. *The Annual Number of Felon-in-Possession and Felony Gun-Use Convictions Prosecuted, Eastern District of Virginia, 1994—99ᵃ*

Number of convictions
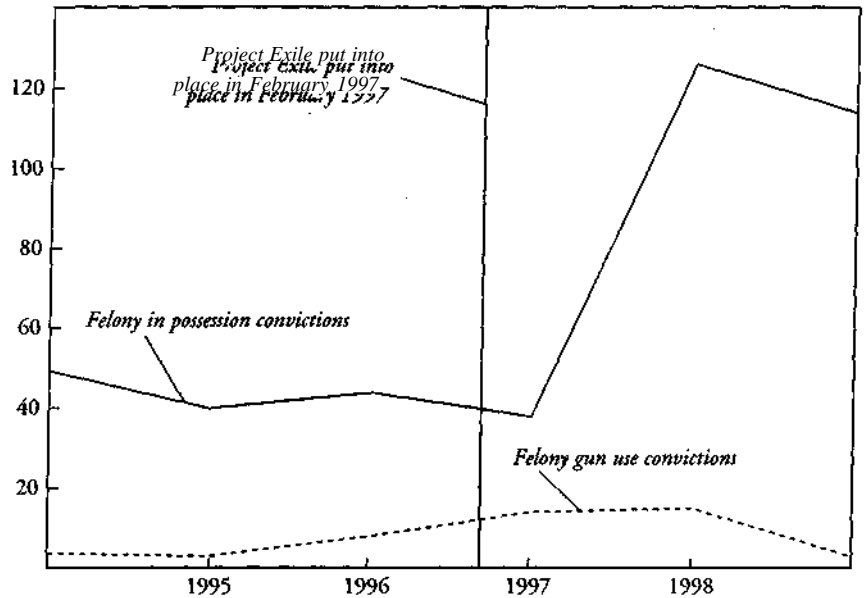
a. By the U.S. Attorney.

gun offenses. It is also possible that word about Project Exile spread among Richmond's criminal population following the initial wave of federal indict-ments (which may have occurred in 1997) rather than convictions (which seem to have started in 1998). However, we accept the argument that actual convic-tions 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 in full effect. The 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 7-1. The large year-to-year changes in homicide rates observed in Richmond suggest that much of the increase ob-served 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 fig-ure 7-1 seem to indicate that, the year 1997 aside, homicide rates in Richmond were trending downward even before the launch of Project Exile. To the ex-tent that the post-Exile declines simply reflect the continuation of trend, the

Table 7-1. *Change in the Gun Homicide Rate Compared with the Nongun Homicide Rate for Richmond during Project Exile*

Time and difference	Gun homicide rate	Nongun homicide rate	Gun minus nongun
1993-94	59.76	8.32	51.44
1995-96	50.55	9.19	41.36
1998-99	34.92	6.82	28.10
[1] Difference 1998-99-1995-96	-15.63	-2.36	-13.26
(percent change)	(-30.9)	(-25.7)	(-5.2)
[2] Difference: 1995-96- 1993-94	-9.22	0.87	-10.09
(percent change)	(-15.4)	(10.5)	(-25.9)
[1] — [2] (difference in percent changes)	-6.41	-3.23	-3.17
	(-15.5)	(-36.2)	(20.7)

Source: Tabulations based on extractions from FBI (1976-99).

raw numbers offered in support of the program are likely to overstate Exile's impact.

To address these concerns, table 7-1 presents a number of calculations based on the homicide rates displayed in figure 7-1. Since Project Exile is designed to deter the use and illegal possession of firearms, the table and the following discussion focus primarily on gun homicide rates. Because of the possibility of substitution from gun to nongun 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 similar findings when we focus on all homicides, gun and nongun together. To eliminate some of the year-to-year variation in gun homicides, the table presents the average annual firearm homicide rates for the three two-year periods 1993-94, 1995-96, and 1998-99. 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 1995-96 to 1998-99.

As seen in table 7-1, between 1995-96 and 1998-99 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 7-1 also shows that gun homicides declined by 15 percent from 1993-94 to 1995-96, 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 "difiference-in-difference" calculation (last row, table 7-1) suggests that the decline attributable to Exile above

and beyond the preexisting trend is approximately 6 gun homicides per 100,000 residents, or 15 percent.[17]

To be sure, it is impossible to assess whether the trend occurring earlier in the decade would have continued during the late 1990s in the absence of Project Exile. However, the following evidence suggests 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 1980s. 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).

Richmond versus Other Cities

Although these calculations are smaller than me effects claimed by program proponents, the declines in Richmond are still large. Any program mat can claim to reduce homicide rates by 15 to 30 percent is likely to be worth investing in and merits die attention of policymakers 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 7-1 are not uncommon.

Figures 7-3A and 7-3B begin to address these concerns by graphing the annual gun homicide rates in levels (figure 7-3A) and natural logs (figure 7-3B) for the period 1990 to 1999 for Richmond and for several groups of comparison cities. Besides gun homicide data for Richmond, each figure presents time series data for the ten 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

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17. 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 1995-96 and 1998-99 shows a smaller change of approximately 30 percent. To calculate an estimate of the percentage change under the assumption that the 1993—94 to 1995-96 change reflects an underlying trend, we would need to calculate what the murder rate would have been in 1998-99 in the absence of Project Exile by subtracting che earlier change from the 1995-96 murder rate. Based on this assumption, the murder rate during 1998-99 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 7-1 are considerably more modest.

Figure 7-3A. *Gun Homicides per 100,000 Residents in Richmond and Other Regions*[a]

Rate per 100,000 residents
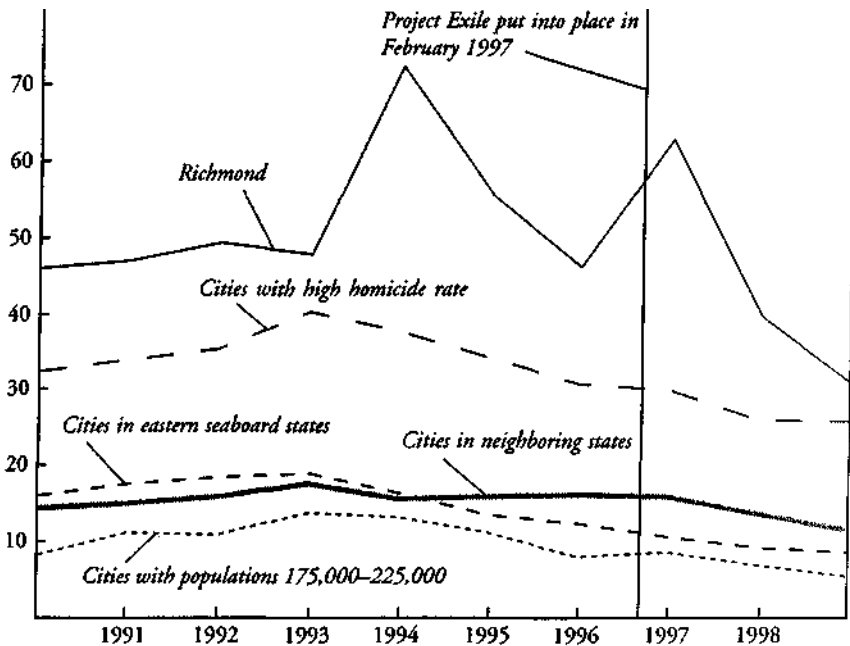
a. "Other regions" means other high-crime cities, cities in states surrounding Virginia, cities on the eastern seaboard, and cities with comparable populations.

those with residential populations between 175,000 and 225,000).[18] The gun homicide rate levels presented in figure 7-3A 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 7-3B) are indicative of the relative percentage changes in gun homicide rates for each series.[19]

Figures 7-3A and 7-3B clearly highlight the unusually high homicide rates that Richmond has suffered throughout the 1990s. Figure 7-3A also indicates that the decline in homicide rates in Richmond around the time of Exile also occurred to some extent in other cities. On one hand, the absolute drop in Richmond homicide rates appears to exceed the drop experienced in the other high murder rate cities and diose observed for the other comparison groups of cities. On the

18. Richmond is omitted from all comparison groups. Between 1990 and 1999, the population of Richmond varied between approximately 203,000 and 190,000 residents.

19. The change in the natural log of a variable is approximately equal to the percentage change in the variable.

Figure 7-3B. *The Natural Log of Gun Homicides per 100,000 Residents in Richmond and Other Regions* [a]

Rate per 100,000 residents
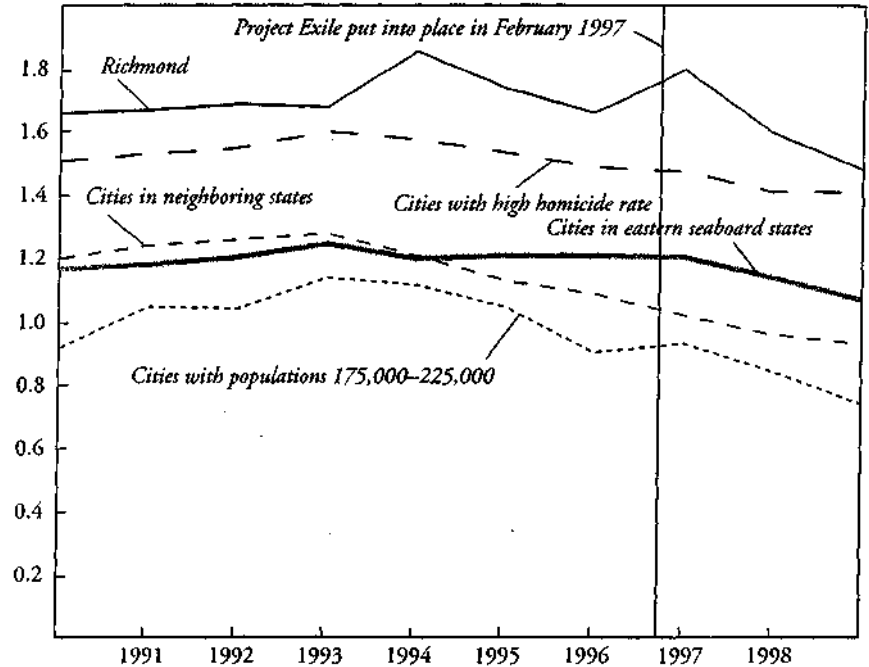
a. "Other regions" means other high-crime cities, cities in states surrounding Virginia, cities on the eastern seaboard, and cities with comparable populations.

other hand, figure 7-3B 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 die various comparison groups of cities.

This last point can be seen more clearly in table 7-2, which presents annual average gun homicide rates for Richmond and the four comparison groups of cities from figures 7-3A and 7-3B. The first two columns present the average annual gun homicide rate for the periods 1995-96 and 1998-99, 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

Table 7-2. *Change in Gun Homicides Rates in Richmond and Other Regions, 1995-96 to 1998-99*[a]

| Group of cities | Gun homicide rate | | Change | change |
	1995-96	1998-99		
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

Source: Tabulations based on extractions from FBI (1976-99). The city groups correspond to those used in constructing figures 7-3A and 7-3B.

a. "Other regions" means other high-crime cities, cities in states surrounding Virginia, cities on the eastern seaboard, and cities with comparable populations, 1995-96 to 1998-99.

observed in all four comparison groups (ranging from —3.35 to —6.62). The relative changes in gun homicide rates, however, are comparable. Although 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, the findings are similar.[20] Figure 7-4A shows the number of cities (shown on the vertical axis) that experienced changes in gun homicide rates of various magnitudes from 1995-96 to 1998-99 (given on the horizontal axis), ranging from very large declines on die left to some modest increase in homicides 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 7-4B. Here, Richmond's decline is still larger than average but is less of an outlier when compared with 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

20. Over this period, Richmond's population ranged between approximately 190,000 and 200,000. We dropped the city of Gary, Indiana, because the 1996 homicide total from the SHR was approximately one-half 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.

Figure 7- *4A. Histogram of the City-Level Changes in Gun Homicide Rates,
1995-96 to   1998-99*
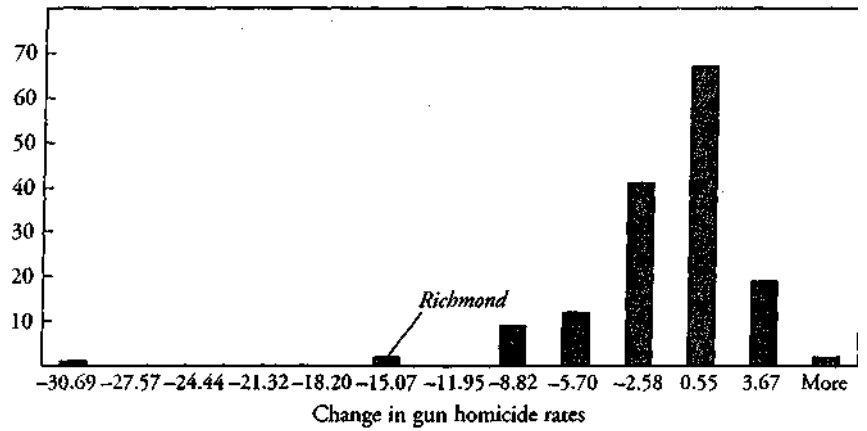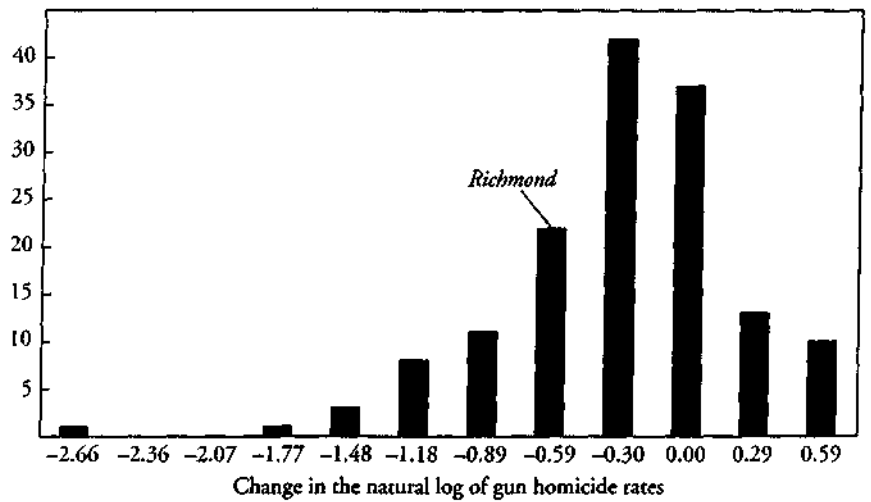
Number of cities

Figure 7- 4B. *Histogram of the Change in the Natural Log of City-Level Gun
Homicide Rates, 1995-96 to 1998-99*

Number of cities

in figure 7-4B, the absolute reduction in gun homicides observed in Richmond was quite large and outside of the range of variation observed in other cities. However, we argue that the unusually large absolute declines observed in Richmond around the time of Exile are less "unusual" than the comparison in figure 7-4A seems to suggest.

Reassessing Exile: Accounting for Initial City Conditions

As already noted, 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.

Besides 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 1990s, 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.

The Relationship between Earlier and Later Changes in Homicide Rates

Why might we expect an inverse relationship between decreases in homicide rates during the late 1990s and increases occurring during the late 1980s and early 1990s? 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 ran their course, and hence murder rates were bound to decline.[21] Another possibility might be that the incapacitation effects associated with the massive increase in incarceration rates may have disproportionately affected areas (or cities) with high crime rates.[22] A third source lies in the possibility that many homicide victims may themselves be

21. Blumstein (1995a).
22. Levitt (1996).

among the population of potential perpetrators.[23] 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 7-5A and 7-5B provide more formal evidence of the relationship between later and earlier changes in city-level homicide rates during the 1980s and 1990s. Figure *7-5A* plots each city's change in gun homicide rates from 1995-96 to 1998—99 (vertical axis) against the city's change over the prior decade, 1985-86 to 1995-96, on the horizontal axis. Figure 7-5B shows the comparable scatter plot for the same changes in the natural logarithm of gun homicide rates. Each figure includes 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 7-5 provide strong evidence of a negative relationship between earlier and later changes in homicide rates—that is, 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.[24]

In light of these findings, Richmond's experience during the 1990s does not appear to be unusual. As seen in figure 7-5A, based on the fitted regression line the increase of approximately 22 gun homicides per 100,000 residents experienced by Richmond during the late 1980s and early 1990s is predicted to cause

23. There seems to be considerable overlap between the populations of potential offenders and victims: the large majority of both groups have prior criminal records. See Kennedy, Piehl, and Braga (1996); McGonigal and others (1993); Schwab and others (1999); Kates and Polsby (2000).

24. The slope coefficient in figure 7-5A indicates that a one-unit increase in overall homicide rates between 1985-86 and 1995-96 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 7-5B is 0.53.

Figure 7- 5A. *Scatter Plot of the Pre-Post Exile Change in Gun Homicide Rates, Against the Change during the Prior Decade*

Change in gun homicide rates, 1998-99 - 1995-96
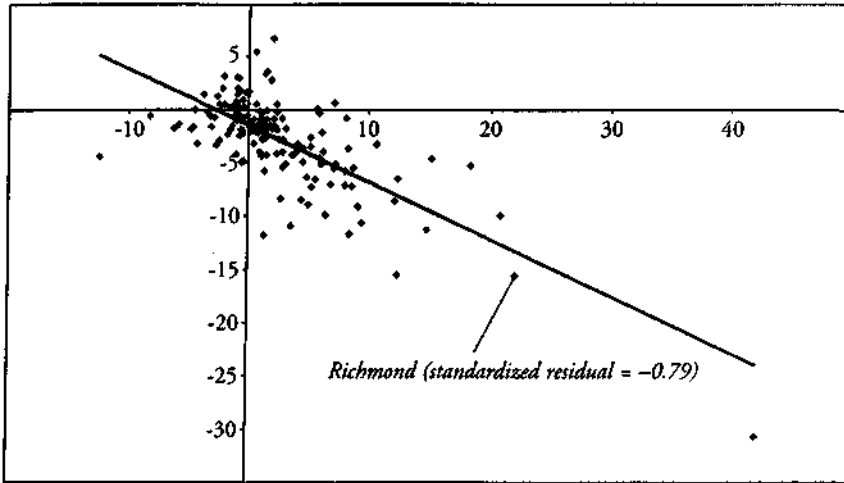
Change in gun homicide rates, 1995-96 - 1985-86

Note: Change (1998-99 - 1995-96) = - 1.48 - 0.3 * change (1995-96 - 1985-86);
(-5.76) (-13.15)
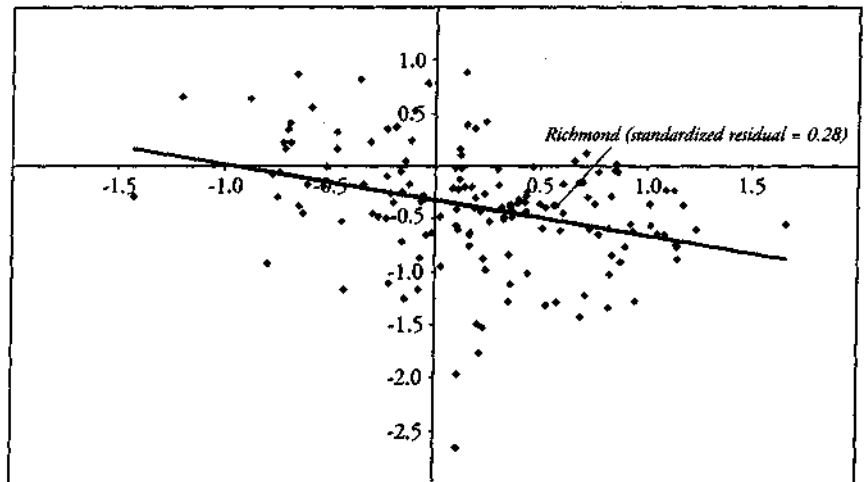
$R^2 = 0.53$. *t* statistics in parentheses.

a decline in gun homicide rates following Project Exile of 13.3 per 100,000— 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).

Figure 7-5 B shows that when we focus on the log of the gun homicide rate, the proportional declines in gun homicide rates in Richmond following Exile are fully explained by the large proportional increases that occurred before Exile's implementation. In fact, the regression lines in figure 7-5 B predict that Richmond would have experienced an even *larger* proportional decline in homicide without Project Exile (as is evidenced by the fact that the Richmond data point lies above the regression line). We obtain similar findings when we focus on the overall homicide rate rather than restricting our attention to gun homicides only, or calculate the regression lines weighting each data point by the city's mid-1990s population.

To summarize, the large increase in homicide rates occurring during the late 1980s 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. Ad-

Figure 7- 5B. *Scatter Plot of the Pre-Post Exile Change in the Natural Log of Gun Homicide Rates, Against the Change during the Prior Decade*

Change in natural log of gun homicide rates, 1998-99 - 1995-96

Change in natural log of gun homicide rates, 1995-96 - 1985-86

Note: Change (1998-99 - 1995-96) = -0.32 - 0.34 * change (1995-96 - 1985-86);
(-7.14) (-4.44)

$R^2 = 0.35$. *t* statistics in parentheses.

justing the decline in Richmond's homicide rates for the increase in murder rates during the 1980s leaves little residual decline in need of explanation.

Some Tests of Robustness

Perhaps the most obvious concern that one might raise in response to the results just presented is that we have included Richmond in the sample used to esti-mate the regression model. Since Richmond had a larger pre-Exile increase in homicide rates and experienced a large decline, perhaps the inclusion of this ob-servation is causing us to overestimate 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 reran 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 regres-sion standard error, indicating no evidence of an impact of Project Exile.

For our analysis of the proportional changes in gun homicide rates (fig-ure 7-5B), 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 before 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 figure 7-5 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).[25] 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.[26]

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 common 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.[27] Because of the well-known problems associated with the UCR, including variation across areas and time in victim reporting to police

25. Cities that implemented Exile-type programs during our sample (besides 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.

26. 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.

27. 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.

Table 7-3. *Pre-Post Project Exile Changes in FBI Index of Crimes Compared with Changes in All Other Cities[a]*

Crime	Richmond residual[b]	Regression standard error[c]	Standardized residual[d]
Murder	-1.85	2.48	-0.75
Rape	-9.49	8.81	-1.08
Robbery	-67.51	80.47	-0.84
Assault	-104.04	104.34	-0.99
Burglary	-258.51	159.02	-1.62
Larceny	-259.79	417.27	-0.62
Auto theft	234.46	177.99	1.32
Arson	17.83	27.52	0.65

Source: Data come from county-level tabulations of FBI (1976-99).

a. Adjusted for pre-Project Exile changes in crime rates.

b. These figures are the fitted residuals for Richmond from a regression of the 1998-99 - 1995-96 change in the rate of the given crime on the 1995-96 - 1985-86 change.

c. These figures are the estimates of the regression standard error from regressions of the 1998—99 — 1995-96 change in the rate of the given crime on the 1995—96 — 1985-86 change.

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

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 7-3 presents our analysis using the UCR crime data. The table presents three columns of information for each of the eight UCR index crimes. The first column presents the fitted residual from Richmond from a regression of the change in the crime rate from 1995—96 to 1998—99 (before and after Exile) on the change in crime rates from 1985-86 to 1995-96. 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 7-3 of an impact of Project Exile on any of the "part I" (serious) 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, such comparisons may be invalid because of unobserved differences among cities in policing, age structure, and other factors likely to influence homicide rates.

Table 7-4. *Change in Homicide Arrests, Richmond and Other Large Counties, by Age, from 1995-96 to 1998*

Item	Change in all murder arrests	Change in adult murder arrest	Juvenile murder arrests	Adult change — juvenile change
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

Source: Data come from county-level tabulations of FBI (1976-99).

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 who are subject to Exile's provisions. We pursue this strategy by comparing changes in juvenile homicide arrests (pre-post Exile) to changes 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 7-2).

Table *7-4* presents the results of these calculations.[28] 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 present 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 arrests in Richmond, pre-post Project Exile. Moreover, adult homicide arrests declined on average during the late 1990s for adults relative to juveniles for other counties with populations

28. In this instance, we use the change in arrests 1995-96 to 1998 as the key dependent variable rather than the change from 1995-96 to 1998-99 because arrest data by age are not yet available for 1999.

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—reduced crime rates among adults relative to juveniles.

Testing for an Impact of Federal Prosecutions Using Panel Data

Although 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 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 1990s; 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 was curtailed somewhat by a 1995 Supreme Court decision ruling that defendants in drug-gun cases must have actively used the firearm in committing the offense to be charged in a federal court.[29] 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

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29. The case *Bailey* v. *the United States* along with a discussion of trends in federal firearm prosecutions is discussed in Scalia (2000).

Table 7-5. *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*

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

Note: The dependent variable is the year-to-year change in the gun homicide rate. 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 "in possession" variable is the number of cases per 100,000 city residents in which one of the primary offenses of the felon charged in the regional office is possession of a firearm. The "used a firearm" variable is the number of cases per 100,000 in which one of the primary offenses involves the use of a firearm. Changes in these variables and the dependent variable refer to one-year single differences. Lagged variables refer to one-period lagged explanatory variables. Our data set refers to cases handled by regional federal attorneys and covers the period from 1994 to 1999. For specifications 1 and 2, the year 1994 is omitted by necessity. For the specifications 3 and 4, the sample is restricted to observations between 1995 and 1999. Standard errors are in parentheses.

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 with cases in which the defendant was convicted of any federal firearm charge. For each of the cities in the analytic sample used in the previous section we identify the local U.S. Attorney's office responsible for prosecuting federal cases in that area.

Table 7-5 presents the results of regressing the year-to-year change in the number of gun homicides per 100,000 residents against the year-to-year change in the number of FIP convictions per 100,000 city residents (or their first difference).[30] Our focus on changes in gun homicides helps account for the influence of unmeasured city fixed effects that cause some areas to have persistently higher or lower gun homicide rates year after year. The first two columns regress the change in the gun homicide rate on the contemporaneous change in the number of convictions, while the last two regressions relate changes in gun homicides to last year's change in convictions. 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

30. We also estimated these models taking the log of dependent and explanatory variables. The results are similar to those presented in table 7-5.

error structure for all models includes district-level variance components. This ensures that we do not underestimate the standard errors of the slope coefficients owing to within-district correlation of the regression error terms.

Table 7-5 shows that rhese regression 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 reestimate the model where the dependent variable is the change in the two-year averages of gun homicide rate four years apart—that is, 1999-98 minus 1994-95—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.[31]

This alternative approach yields qualitatively similar results to those presented in table 7-5. Again, the relationship between FIP or other gun convictions and homicides is near zero and not statistically significant.

The results presented now and in 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.

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 1995-96 to 1998-99 in gun homicide rates in Richmond are larger than those observed

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31. Over a longer period, whether the proper lag is one, two, or three periods (and so on) 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.

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-1990s, and that cities with larger-than-average increases in gun homicide rates subsequently experience unusually large declines. This finding is 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 firearm convictions 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 responses, 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 true, 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.

Possibly, however, the effect of Richmond's Project Exile is partially obscured by increased prison penalties for gun carrying and other offenses that are being imposed through state courts across the country. What we can say at the very least, however, is that federal prosecutions of such crimes does not appear to be substantially more effective than state prosecutions during this time period.

To be sure, our finding that Project Exile did not drive the decline in Richmond gun homicides following the program's launch begs the question of what exactly was responsible for the observed decline. One natural starting point for any explanation of the decline in criminal violence during the 1990s rests with the cause of the substantial increase in homicides starting in the mid-1980s, most of which was driven by gun homicides committed against and by young minority males.[32] The leading explanation for the crime surge of the 1980s is the growing involvement in crack distribution by young minority youth, who increasingly turned to firearms to enforce property rights in drug-market transactions.[33] Support for this explanation comes in part from the similarities in timing between the introduction of crack in cities and increases in youth homicides.[34]

The crack hypothesis would lead us to predict a substantial increase in homicides starting in the mid- or late 1980s for Richmond, since more than half of the city's population is African American, which is consistent with the homicide patterns presented here.[35] And Richmond's experience does not look very different from that of the rest of the country when we focus on homicides to young black males. From 1985—86 to 1995—96 the gun homicide rate for African American males between the ages of 15 and 34 increased by 72 percent (from *87* to 150 per 100,000), compared with an increase of 50 percent for the same group in the United States as a whole (35 to 53 per 100,000). The subsequent decline from 1995-96 to 1998-99 is smaller in proportional terms in Richmond compared with the United States as a whole (16 versus 25 percent).[36] Some change in the involvement of youth and guns in crack markets is thus an obvious candidate to explain the decline in gun homicide during the 1990s in Richmond

32. Cook and Laub (1998).
33. Blumscein (1995a).
34. Cork (1999); Grogger and Willis (2000).
35. U.S. Bureau of the Census (1998, p. *49).*
36. It should be noted, however, that the decline from 1995-96 to 1998-99 among all other population groups excluding African American males 15 to 34 years old were larger in Richmond than in the rest of the country (40 versus 25 percent), perhaps partly because the homicide rate among this population increased by 39 percent from 1985—86 to 1995—96 in Richmond, while the rate declined by 5 percent in the rest of the country.

and elsewhere, although a complete explanation must account for the fact that nongun homicides also declined substantially over this period.[37]

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 judging those efforts launched during the peak or downside of a crime cycle as a success. This heuristic device is particularly unfortunate in the case of government interventions directed at crime and gun violence, problems that impose enormous costs on society. Of course, it may be worthwhile to implement interventions that have quite modest effects on gun violence. While our analysis confidently rules out the possibility that Project Exile achieved the dramatic reductions in gun violence that have been claimed in the past, more modest (and perhaps more realistic) program effects are unlikely to be detected by the methods used here. Moreover, such modest effects are equally unlikely to be noticed by the policymakers, news reporters, and voters who focus on simple and dramatic changes over time as the benchmark of a program's success.

COMMENT BY
Steven D. Levitt

In this excellent chapter, Steven Raphael and Jens Ludwig analyze the effects of an innovative antigun criminal justice intervention known as Project Exile. Initiated in Richmond, Virginia, in 1997, the program's centerpiece is stepped-up prosecution of felon-in-possession-of-a firearm (FIP) cases. The program also included an advertising campaign stressing zero tolerance toward gun offenses. Proponents cite a 40 percent reduction in gun homicides in Richmond as evidence of its success. Based on this initial success, the budget of the program (under the new name Project Safe Neighborhoods) has been expanded tenfold.

In light of this apparent success, it is perhaps surprising that Raphael and Ludwig come to the conclusion that Project Exile, in reality, seemed to have no impact on crime in Richmond. The arguments they present are simple. First, the 40 percent decline in gun homicides between 1997 and 1998 is greatly exaggerated because the 1997 gun homicide rate was itself an aberration—up 30 percent from 1996 even though the trend in Richmond and elsewhere has been persistently downward. The blip up in 1997 is particularly problematic for proponents of Project Exile given that the program went into effect in February

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37. Cook and Laub (2001).

1997, and thus this spike in gun homicides occurred postprogram implementation. Second, crime rates were falling everywhere and falling more steeply in high-crime places. Thus, when Richmond is compared with other cities that did not have Project Exile, Richmond does not seem to be an outlier in the crime reductions it experienced. Subject to the inherent limitations of this sort of case study approach, I find the arguments presented in the Raphael and Ludwig chapter convincing. Furthermore, the authors were kind enough to make their data available to me. I found their results remarkably robust to a wide range of alternative specifications.

In this discussion, I focus on two primary issues. First, I offer further context as to why one might expect Project Exile to represent a very sensible approach to criminal justice. Second, I provide some rough calculations of how large the impact of Project Exile on crime might have been expected to be based on existing estimates of the relationship between crime and punishment. The answer, it appears, is that even if Project Exile were far more effective than the standard criminal justice approaches, its expected impact on crime would be small enough to be undetectable with the methods employed in this chapter.

The form that Project Exile takes reflects die collision of two of the defining elements of the U.S. criminal justice experience in the past three decades: the incarceration boom and the explosion of gun violence by youth in the early 1990s. Since the early 1970s, an explosion in incarceration has occurred in the United States. The number of Americans behind bars today is seven times as high as three decades earlier. Gun violence—especially by youth—soared in the late 1980s and early 1990s. The homicide rate for young black males tripled between 1985 and 1992, for instance. All of this increase was concentrated in gun homicides.

In light of those two features—large increases in incarceration and gun homicides—the premise of Project Exile appears sound. Although there is strong evidence that this increase in incarceration has reduced crime, the cost has been substantial, with annual government expenditures on courts and corrections currently running more than $50 billion annually.[38] Furthermore, it seems reasonable to assume that as the prison system expands, the marginal benefit to society of locking up one more criminal is declining. If the criminal justice system is at all good at doing its job, the millionth criminal locked up should pose a threat to society that is a lot smaller than the first criminal. That being the case, a general expansion of the prison population does not seem desirable from a cost-benefit perspective, even to people like me who believe prisons work. Instead, selectively targeting resources toward hard-core, gun-toting violent of-

38. Marvell and Moody (1994); Levitt (1996).

fenders who impose the greatest costs on society might plausibly represent a more sensible plan of attack, and this is precisely the logic underlying Project Exile. There is some scholarly work suggesting that such targeted programs might work, based on sentence enhancements in California and a youth anti-crime initiative in Boston.[39]

Although I am convinced by the authors' analysis that Richmond's crime experience was not out of the ordinary, one might question whether Project Exile, even if successful, would be expected to affect crime sufficiently to be detected in aggregate crime statistics. The centerpiece of Project Exile was an increase in federal FIP convictions. After the program was initiated, there were roughly eighty additional FIP convictions a year. Those convicted on these charges might well have been convicted on other charges in absence of Project Exile. Thus the eighty convictions do not imply an extra eighty offenders off the streets each year in the period analyzed. As the evidence suggests in the analysis of California sentence enhancements, only a small fraction of the total impact of the laws is felt in the first few years (the only period for which Raphael and Ludwig have data).

Even putting aside that issue, is the increase in punishment implied by an extra eighty convictions enough tliat one would predict an observable change in the crime rate? In my own past research I find some of the largest estimated impact of incarceration on crime.[40] Each additional prisoner is associated with a reduction of .004 homicides annually (this effect captures deterrence and incapacitation). Thus, if the eighty convictions through Project Exile were of typical criminals, the predicted decline in homicide would be .32! Even if those targeted by Project Exile were ten times more likely to commit murder than the marginal prisoner, we would only expect a reduction of three homicides—a decline that would simply be lost in die noise of the estimation.

Another way of determining the expected impact of Project Exile on crime is to calculate the increase in expected punishment implied by the extra prosecutions. There are roughly 3,000 violent crimes and 15,000 property crimes reported each year in Richmond. The average clearance rate in large cities is 45 percent for violent crime and 15 percent for property crime. If these averages hold in Richmond, there are roughly 3,500 arrests for violent and property crimes a year in that city. On average in the United States, about 25 percent of violent arrests lead to imprisonment. For property crime arrests the corresponding figure is 10 percent. Thus about 500 Richmond residents are sentenced to prison each year for these crimes. If one includes drug-related and

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39. Kessler and Levitt (1999); Braga and others (2001).
40. Levitt (1996).

other offenses, I estimate die number imprisoned as about 850 each year. If on average these individuals expect to serve about 2.5 years each in prison, this translates into a total punishment of roughly 2,100 person-years of time behind bars handed out each year in Richmond. If the eighty Project Exile convictions represent new punishments (as opposed to substituting for other punishment), and each Project Exile conviction represents an increase of three years in prison, then Project Exile accounts for 240 extra person-years of imprisonment, or roughly a 10 percent increase in the total punishment in Richmond. A reasonable estimate of the elasticity of crime with respect to expected punishment is -.25.[41] Thus one might expect a 2.5 percent reduction in crime in Richmond as a result of Project Exile.

That would translate into a reduction of two to three homicides a year, about 70 violent crimes and 350 property crimes. Given the inherent volatility in city crime rates, such small changes would be imperceptible in the data.

COMMENT BY
Peter Greenwood

Steven Raphael and Jens Ludwig have demonstrated fairly conclusively that one of the more popular strategies developed during the past decade to reduce firearm violence is a bust. It has no impact. It does not work.

The authors have restrained themselves admirably from poking holes in the theoretical underpinnings of the basic model. They concede that the evidence is not entirely negative on deterrence effects and yes, of course, they agree that there is surely a significant incapacitation effect when former felons who are found carrying a gun are locked up. They make Project Exile sound like a plausible and well-thought-out concept and help the reader understand several ways in which it might work. Well done on the objectivity scale.

But the numbers tell the tale. That which climbs the highest is going to eventually fall down the most as well—regression to the mean. There is no evidence of impacts on adults compared with impact on juveniles, who, theoretically, are not affected by the program.

What do you think is going to happen now that these findings are out? Is the president going to withdraw his endorsement of the program? Is Attorney General John Ashcroft going to tell his assistants to take or hold back all that extra funding ($550 million) his agency has set aside to replicate the program in

41. See Levitt (1996, 1997).

other jurisdictions? Will they shut down or redesign the program in Richmond? I think not.

Project JExile was developed by the U.S. Attorney's Office in Richmond, Virginia, as its contribution to local efforts to reduce gun violence. The primary focus of the program is a coordinated effort by Richmond law enforcement agencies and the U.S. Attorney's Office to prosecute, in federal court, all felon-in-possession-of-a-firearm (FIP) cases, drug-gun cases, and domestic violence with gun cases, where federal sentencing guidelines call for longer sentence terms than local laws. For felons-in-possession-of-a-firearm federal sentencing means an effective sentence enhancement of about five years, with no chance of early parole and less likelihood of making bail. Project Exile proponents and supporters argue that the much tougher sentences handed out by the federal system for simple felon-in-possession cases should deter some ex-felons from carrying firearms, and thus the likelihood that they will engage in gun violence.

Would that it were so. The 40 percent drop in gun homicides in the year following Project Exile's actual, as opposed to official, implementation certainly suggests that it may be so. The U.S. Attorney, the National Rifle Association, HandGun Control, Inc., the Bush administration, and Virginians Against Gun Violence all claim that it is so. Raphael and Ludwig have some bad news for them.

After comparing trends in Richmond's homicide rate with those in other similar cities, they find that the decline experienced in Richmond is no greater than what would have been expected without any intervention. They also find that juvenile and adult gun homicide rates declined at about the same rate, not showing any effects from Exile, which only applies to adults. Replicating Project Exile seems to be a waste of money.

Now wait a minute. What are the characteristics of felons who have been prosecuted by the federal government as part of this project? Are they really "bad news" characters who obviously need to be taken off the street, or are they only marginal offenders whose primary characteristic is being dumb? As the body of criminal career research has shown, there are lots of the latter type around. It would have been nice to have the social history and prior records on those individuals selected for federal prosecution, compared with those who were not. In fact it would be extremely useful to see a breakdown of the prior records of all those individuals arrested for gun homicide in Richmond during the years under study. The fraction of homicides committed by gun-wielding parolees would seem to set an upper limit on the number of homicides that Project Exile could prevent and is the kind of analysis engaged in by the Boston Gun Project before deciding what the intervention should be. Remember, it's ready, aim, fire—not ready, fire, aim.

The authors do not give us this information. It might help some people better understand why the project did not appear to have any effect. Then again, crime control strategies that primarily involve tough sentencing enhancements for some designated group of offenders believed to represent a high-risk to society seldom deliver their promised punch. A situation the mandatory sentencing crowd finds hard to stomach.

What Happens Next

If one believes the results of the Raphael and Ludwig study it is time to "call' off the dogs," "call in the sheep," and find some other program for the Justice Department to franchise this year.

Unfortunately, not everyone who needs to get this word will do so. Many who do will choose to ignore it. Other U.S. Attorneys will be encouraged and influenced by Department of Justice grants to implement similar programs. Many local law enforcement officials will lobby their U.S. Attorneys to mount similar efforts, and numerous legislators will propose state laws that provide the same outcome—much longer sentences for any felon found in possession of a gun.

The saga of Project Exile reads amazingly like the prison baseball game being played in California—three strikes and you're out. Many people's first reaction to any type of crime threat is to propose longer sentences for those who seem to represent the highest risk. In California, it was violent crimes by repeat offenders (remember Richard Allen Davis—the killer of little Polly Klaas?), hence three strikes.[42] In Richmond, it was gun homicides, hence Project Exile. In neither instance did the proponents do much homework to see if the proposed program was appropriately targeted and could be expected to work.

Both programs were implemented in response to rising crime rates, and both benefited from the nationwide decline in crime rates that followed. None of the proponents had any question about whether their program would work. Both sets of proponents claimed success at the first sign of any positive results and continued to claim success even after the statisticians announced there were no impacts to cheer. The end result is that millions of hard-to-come-by crime prevention dollars are being squandered on correctional facilities that seem to have minimal effects, and thousands of defendants are serving unusually long terms because somebody came up with a theory and a good bumper sticker title that captured the public's fancy.

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42. Richard Allen Davis, a repeat offender, was convicted of the 1993 kidnapping and murder of 12-year-old Polly Klaas and was sentenced to death by the Santa Clara County Superior Court.

Why is this so? How can it happen? Why does scientific evidence carry so little weight in this field? A perfectly conducted study can usually be demolished by the opinion of one ill-informed police officer or prosecutor.

In California, very few legislators have been willing to speak out against three strikes in the seven years since it was passed. This in spite of numerous media stories about cookie thieves and pizza bandits getting life terms. Finally, this past November, the Ninth Circuit struck down a third strike sentence of fifty-years-to-life for stealing $140 worth of videotapes as cruel and unusual punishment.

Police officials continue to be in favor of the law. It has been the prosecutors, those traditional defenders of the bastions of liberalism, who have stepped forward to say enough is enough. The current incumbent, Steve Cooley, defeated Gil Garcetti for the office of Los Angeles District Attorney on a platform of only filing third-strike enhancements for serious and violent offenses. Many other California district attorneys have adopted similar policies. Here at last we find individuals with the wisdom, integrity, and political clout to face down the slogan mongers and demagogues.

The value of a Project Exile approach may depend on the quality of the selectivity with which it is applied. If they use it only against felons scoring high on some objective risk prediction scale, then it might be worthwhile. But as a normal response to every single ex-con found in possession—it will cost a great deal of money that could be better spent on early prevention, delinquency programs, and drug treatment, with a much greater payoff in reduced crime. In fact governmental investments in appropriate early childhood and delinquency prevention programs have been shown to produce future savings that exceed their costs.[43]

One of the questions that needs to be asked about Project Exile is whether it made any sense as a pilot project back in 1996 or whenever people started kicking around the idea of finding creative ways of increasing the penalties for illegal gun possession. The selective use of federal prosecution for "felon-in-possession" is one of the key levers in the "pulling levers" strategy of the nationally acclaimed Boston Gun project. The Boston Gun project was also developed in 1996.[44]

What was our state of knowledge in 1996 when Exile was developed? The first place to look is the University of Maryland Preventing Crime Report, which was produced in the fall of 1996. Surprisingly, the topic of increasing penalties for offenders is not even mentioned. In Doris MacKenzie's chapter we learn that

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43. Karoly and others (1998); Karoly and others (1998); Aos and others (2001).
44. Kennedy and others (2001).

incapacitation of chronic offenders does work, but the chapter emphasized that specific deterrence (shock probation, scared straight) does not. Neither do boot camps.[45]

Another source would be *Crime,* published in 1994, in which Al Blumstein wrote the chapter on prisons. "While the threat of a lengthy prison sentence is undoubtedly very effective at deterring white collar crimes that tend to be committed by middle class individuals, they are probably far less effective in deterring the crimes committed by underclass individuals, who are the primary occupants of prisons, and for whom the increment of pain associated with prison time may be far less severe than it would be for those ensconced in a comfortable job. "[46]

Raphael and Ludwig address an important policy issue, using the best methodology available. They have achieved dear and unambiguous results. Let's hope their study gets the attention it deserves.

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45. Sherman and others (1997, chap. 9, pp. 1-75).
46. Blumstein (1995b, p. 417).

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