GUNS, CRIME, AND ACADEMICS: SOME REFLECTIONS ON THE GUN CONTROL DEBATE

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ABSTRACT

This comment on Thomas Marvell's "The Impact of Banning Juvenile Gun Possession" analyzes Marvell's empirical findings and their policy implications for gun control legislation. While Marvell's article stresses the absence of any finding favorable to juvenile gun bans, this comment points out that the statistical results actually support the stronger finding that some of the juvenile gun bans are associated with a statistically significant increase in homicides nationwide. Under either finding, the juvenile gun bans are welfare reducing because of the inherently costly nature of conventional gun control legislation. The concluding discussion argues that the failure to draw appropriate policy conclusions from methodologically sound findings on controversial subjects such as gun control undercuts the value of academic research as compared with competing influences in the public debate.

THOMAS MARVELL's examination of the relationship between juvenile gun bans and homicide rates is interesting both for its particular findings and for its implications for the contemporary gun control debate. His work comes in two parts. His initial paper,[1] which I examine first, had as its primary finding that the wave of state and federal legislation in the 1980s and early 1990s, which criminalized the possession of handguns by minors, did not have its presumably intended effect of reducing gun homicides against either minors or the overall population. His published paper,[2] which I review in the Appendix, finds weaker results. Let me start with the initial paper.

In my view, Marvell's initial statistical results should be interpreted to support the stronger finding that at least some of these juvenile gun bans are associated with a statistically significant increase in gun homicide rates.

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[1] Thomas B. Marvell, The Impact of Banning Juvenile Gun Possession (paper presented at the American Enterprise Institute conference Guns, Crime, and Safety, December 10–11, 1999, Washington, D.C).

[2] Thomas B. Marvell, The Impact of Banning Juvenile Gun Possession, in this issue, at __.

Marvell seems to discount this secondary finding, which he characterizes as offering "slight support for the theory that the bans increase homicides because the juveniles appear more vulnerable." He draws no policy prescription from the evidence presented.

I.   INTRODUCTION

Thomas Marvell's empirical paper on the relationship between juvenile gun bans and homicide rates[3] is interesting both for its particular findings and for its implications for the contemporary gun control debate. Marvell's primary finding is that the wave of state and federal legislation in the 1980s and early 1990s, which criminalized the possession of handguns by minors, did not have its presumably intended effect of reducing gun homicides against either minors or the overall population. Indeed, his statistical results also support the stronger finding that at least some of these juvenile gun bans are associated with a statistically significant increase in gun homicide rates. Marvell seems to discount this secondary finding, which he characterizes as offering "slight support for the theory that the bans increase homicides because the juveniles appear more vulnerable."[4] He draws no policy prescription from the evidence presented.

In fact, the negative policy implications of his findings are unambiguous. If these laws have no effect one way or the other on crime rates, then they should be scrapped because of their other costs, which are strictly positive: they interfere with free choice, they impose punishment and deterrence costs without justification, and they consume public resources for no public benefit. But Marvell's findings suggest that the actual scenario is worse: the juvenile gun ban laws are positively correlated with the death by homicide of some hundreds of young people each year. By my conservative computations from Marvell's estimated coefficients, approximately 300–400 lives of young people are lost per year.[5] This result is consistent with the adverse-selection critique of gun control, which is that lawful defensive users will be disarmed before nonlawful users, thus producing a net increase in crime.[6] From this perspective, Marvell's evidence is consistent with the findings by John Lott and David Mustard[7] that defensive use is important to the impact of gun

[3] Marvell, *supra* note 1.

[4] *Id.* at 22.

[5] See Section II *infra.*

[6] See Bruce H. Kobayashi, Gun Control, Strict Liability, and Excise Taxes, in Taxing Choice: The Predatory Politics of Fiscal Discrimination 309 (William F. Shughart II ed. 1997); Nelson Lund, The Past and Future of an Individual's Right to Bear Arms, 31 Ga. L. Rev. 1 (1996), at nn.148–58 and accompanying text (tracing the argument back to Cesare Beccaria); and Daniel D. Polsby, The False Promise of Gun Control, 272(3) Atlantic Monthly 57 (1994).

[7] John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997). See also John R. Lott, Jr., More Guns, Less Crime: Understanding Crime and Gun Control Laws (2d ed. 2000).

supply policy on crime rates. What interests me is why Marvell seeks to discount this finding.

II. MARVELL'S RESEARCH AND CONCLUSIONS

Marvell's empirical research applies the new standards of gun supply policy research pioneered by Lott and Mustard to these "juvenile gun bans." Marvell measures the correlation between those laws and rates of both gun homicide and nongun homicide, for the population in general and for certain bands of the youthful population, through multiple time-series regression analysis on a state-by-state basis, covering 45–50 states and victims aged 18–29, depending on available data. He reports results both as national aggregates and by state.[8]

On the basis of the national aggregates, Marvell concludes that "there is no evidence that juvenile gun possession bans, taken as a whole, reduce gun homicides or total homicides."[9] This overly cautious statement might be read to imply the absence of significant regression results. But in fact, the nationally aggregated results (tables 3–5) consistently reflect significant coefficients for one of three categories of juvenile gun bans studied ("1994 state laws") in a regression against gun homicide rates. The coefficients are positive, which means that those juvenile gun bans are associated with a significant increase in gun homicide rates, ranging in magnitude from a 7.8 percent increase for victims aged 15–19, to a 6.9 percent increase for victims aged 15–24, and to a 2.6 percent increase for all age groups combined. These national aggregates contain no significant negative coefficient for any of the experimental variables. Thus, the conventional statistical result of Marvell's study is consistent with the Lott-Mustard findings that gun supply is inversely related to crime rates, though Marvell's findings are more narrowly focused on gun homicide rates as influenced by juvenile gun bans.

Marvell's results are fairly robust to several changes in specification,[10]

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[8] Marvell's state-by-state results (Marvell, *supra* note 1, table 5) reflect an interesting diversity in effects across states, which suggests that this topic was inappropriate for the uniform federal legislation enacted by Congress. On the other hand, the state-level results are the only ones reported by Marvell that include significant crime-reduction effects from juvenile gun bans. While overwhelmed by crime-increasing effects in the national aggregates, these findings do suggest the need for further research on what local conditions, other than random chance, produced the difference in particular states. However, Marvell's research does not isolate any such variables.

[9] *Id.* at 6.

[10] Marvell reports one alternative specification in which the significant results for 1994 state laws are extinguished by redefining the variables to combine the 1994 state and federal law dummies into a single variable (table 8). Given his general findings of positive and significant coefficients on the 1994 state law dummy and negative but insignificant coefficients on the 1994 federal law dummy (tables 3–5), it is not surprising that the combination of the two nets out to insignificance, although it remains positive for the two regressions involving youthful victims. However, I fail to see how a reduction in the number of explanatory variables improves the specification in this context, as it dilutes the effects of both categories in the regression

including explicit consideration of other contemporaneous crime control leg-
islation, such as the Brady law, the "three strikes" sentencing laws, and the
concealed-carry laws. As Marvell notes, these respecifications had "supris-
ingly little impact on the results."[11] Marvell's analysis also responds to the
suggestion of simultaneity between cause and effect of the juvenile gun ban
laws, that is, that rising crime rates caused the laws to be enacted[12] such that
the crime-increasing effect shown by the regressions masks an underlying
upward trend. Marvell's findings reject the simultaneity hypothesis.[13] Thus,
Marvell's work displays a commendably thorough methodological approach
to the problem.

Nevertheless, Marvell discounts his findings of the crime-increasing effects
of the juvenile gun bans, as noted above. Why? He offers two reasons, neither
of which is persuasive.

First, Marvell comments on the disparity of results between the gun hom-
icides, which reflect the results noted above, and nongun homicides, which
consistently show no significant effects, positive or negative.[14] He remarks
that this contrast seems inconsistent with the view that homicide rates in-
creased because the gun control law denies lawful access to defensive tech-
nologies because "the corollary that the increase should be greatest for non-
gun homicides is not substantiated."[15]

I doubt that this contrast has any bearing on the validity of the findings
for gun homicides. It could simply be an artifact of Marvell's experimental
design. The experimental hypothesis was that juvenile gun bans were intended
to reduce gun homicide rates, and Marvell's study was designed to isolate
that effect. Had he not isolated gun homicides by design, he might have been
criticized for "diluting" the effect (positive or negative) of the gun control

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and forces both to a single fixed effect. This is particularly puzzling in light of the fact that
state-by-state results (table 6) show a good deal of interstate variability in the effects of juvenile
gun bans and no evidence that I can detect that the state variations are "due to chance" (*id.*
at 17). In my view, the logical next step should be to add further variables to explain the
interstate variations in the state-level regressions. However, Marvell did not follow this path.

[11] *Id.* at 19. The only change reported by Marvell is to reduce the size of the "all-ages"
(table 5) coefficient on 1994 state laws from .026 to .018 and to reduce its *t*-statistic from 2.59
to 1.75 (*id.* at 19). This change could reflect a loss of resolution from the additional control
variables in the all-ages regression, where the crime-increasing effects of the juvenile gun bans
presumably would be the weakest. As no comparable change is reported for the youthful victim
regressions, this respecification would not appear to affect the basic findings of additional
deaths from the juvenile gun bans. As is explained below, most or all of the additional deaths
associated with the juvenile gun bans are in the youthful population.

[12] This suggestion had been stressed by Jens Ludwig in his critique of the Lott-Mustard
study. Jens Ludwig, Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State
Panel Data, 18 Int'l Rev. L. & Econ. 239 (1998).

[13] Marvell, *supra* note 1, at 20–21.

[14] I note in passing that in two of the three instances of significant positive coefficients in
the gun homicide regression there also were positive (albeit insignificant) coefficients in the
nongun homicide regression. In the third instance (all ages combined) the coefficient was zero.

[15] Marvell, *supra* note 1, at 14.

laws. He then used the same specification for both the gun homicide and nongun homicide regressions as a control measure. The fit generally was much better for gun homicides than nongun homicides.[16] These statistics suggest that nongun homicides are more strongly explained by variables omitted from Marvell's regression equation. If this is true, then it is not necessarily true that the marginal effects of the laws on nongun homicide rates would have any fixed relationship with the effects on gun homicide rates. Nor is there a necessary logical linkage here: nongun homicides involve a wide range of killing methods that vary in their susceptibility to defensive measures. There could easily be substitution within the nongun category (for example, toward poison or stealth and away from frontal attacks).

Second, Marvell's main point is that if the juvenile gun bans increased crime, that "impact would apply only to" juvenile crime.[17] His apparent rationale is that most juvenile victims are attacked by other juveniles, whereas his results show similar impact on both "murders by juveniles and those by adults."[18] I believe that this comment misinterprets the results and again does not follow logically.

Marvell's paper does not report its results in terms of adult victimizations only; only all-age victimization is presented. The all-age regression displays the same sign and roughly the same significance level but is a different magnitude—approximately one-third the magnitude observed in the regressions on victimizations in the 15–19- and 15–24-year-old age groups.[19] General statistical sources show that, during the period studied, the 15–24-year-old age group represented about one-fourth to one-third of known homicide victims.[20] This ratio is fairly close to the ratio of the all-ages coefficient of .026 to the 15–24-year-old age group coefficient of .069, which is .37.

Furthermore, as Marvell himself points out (figure 1), gun homicides account for a higher percentage of all homicides in the younger age groups than in older age groups. According to his summary data (table 2), the average mix was 74.4 percent guns in the 15–19-year-old age group and 72.3 percent

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[16] The nongun regressions had much lower R^2- and F-statistics, with the exception of a relatively high R^2 in nongun homicide for all ages. However, in that case, the Chow test yielded significant results, which suggests structural differences between the gun and nongun regressions. The insignificant Chow statistics on the other two equation pairs (on victims aged 15–19 and 15–24) may reflect nothing other than the mix of gun versus nongun homicides in those age groups. It is not a reason to doubt the significant results in the gun homicide regressions for those age groups.

[17] Marvell, *supra* note 1, at 23.

[18] *Id.* I assume in the latter quotation that Marvell means to say murders "of" juveniles and murders "of" adults, as his dependent variables are victimization rates.

[19] The analysis here sets aside Marvell's alternative specification that includes control variables for other contemporaneous laws (see text at note 11 *supra*). These actually do support his postulated "corollary" by producing no change in the juvenile victimization regressions but a weaker result in the "all-ages" regression.

[20] Sourcebook of Criminal Justice Statistics—1995, table 3.128 (1996).

guns in the 15–24-year-old group, versus 64.15 percent guns for all ages combined. Alternatively, an examination of the 1994 homicides by firearm victimizations by age shows that the 15–24-year-old age group represents 37.6 percent of all gun homicides, which is almost exactly equal to the ratio of the coefficients.[21]

Thus, the only effect isolated here could be the weighting of an impact on juvenile crime by overall victim demographics. But that technical point to one side, it is not necessarily true that a crime-increasing effect of juvenile gun bans would only affect the number of juvenile victims. Juveniles can defend adults, just as adults can defend juveniles.

For these reasons, I see no basis for disregarding or discounting Marvell's statistically significant results of increased homicide rates from the juvenile gun bans. The magnitude of the effect is not trivial and is in the range of a 7 percent increase in gun homicide rates in the youthful age bands. Marvell's nationwide point estimates, even after combining all three groups of laws studied, imply an estimate of 300–400 fatalities annually attributable to the juvenile gun bans,[22] where virtually all of those additional fatalities are youthful victims. At the low end of that estimate, this implies a cumulative loss of some 2,000 young lives since 1994[23] that are attributable to these juvenile gun bans (see Table 1). Of course, Marvell's regressions, like all estimates, embody a margin of error. However, at conventional levels of significance, Marvell's results must be interpreted to show that the number of additional deaths correlated with juvenile gun bans is significantly different from zero.

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[21] *Id.* at table 3.126, enumerates 1994 homicides by age groupings and weapon used. Under the conservative assumption that 15- and 16-year-old victims represent only half of the total homicides by firearm within the 13–16-year-old age band, total gun homicides by firearm in the 15–24-year-old age group were 5,809 in 1994, which is 37.6 percent of the 1994 total of 15,456 gun homicides for all ages.

[22] Using the 1994 homicide data reported in the Sourcebook for Criminal Justice Statistics, *supra* note 20 (table 3.126), total homicides by firearm were 15,456 for all ages and 5,809 in the 15–24 age group. Applying Marvell's point estimates of .026 and .069, respectively, both produce estimates of about 400 annual deaths associated with the juvenile gun bans. Even after netting these significant coefficients against the insignificant coefficients (both positive and negative) for the other two law types, the coefficients are .022 and .068, respectively, leading to an estimated 340 and 395 deaths, respectively.

[23] Since 1994, homicide rates (like all other rates of traditional crimes) have declined continuously for all age groups, including the youthful age groups studied by Marvell. See James Alan Fox & Marianne D. Zawitz, Homicide Trends in the United States (2000), which discusses homicide victimization by age (available at http://www.ojp.usdoj.gov/bjs/homicide). Their homicide rates (per 100,000 of population) by age for 1994–98 are shown in my Table 1. Furthermore, this declining trend applies to both gun and nongun homicides, in all age groups. See Fox & Zawitz, *supra,* for discussion of homicides by weapon type and age of offender. While, as Marvell notes, the mix of gun versus nongun homicides may have changed over time, the overall rate of homicide declined cumulatively by up to 45 percent between 1994 and 1998 (in the case of victims aged 14–17).

TABLE 1

HOMICIDE RATES BY AGE, 1994–98 (per 100,000 of Population)

Year	<14	14–17	18–24	25–34	35–49	≥50
1994	2.0	11.2	23.6	15.4	8.9	3.8
1995	1.9	11.0	21.5	13.9	8.2	3.8
1996	1.9	9.1	19.5	12.3	7.7	3.4
1997	1.7	7.3	19.1	11.4	6.8	3.2
1998	1.7	6.2	17.4	10.6	6.5	2.8

III. DISCUSSION

No matter which interpretation of Marvell's evidence is offered, the juvenile gun bans are a public policy disaster. His study also underscores the inherent differences among types of gun control laws. Unlike the concealed-carry laws studied by Lott and Mustard, which are self-financing and compatible with individual choice, a conventional gun control law of the type studied by Marvell is inherently costly in both material and human terms. Even if there is strictly no impact on crime rates, positive or negative, a conventional gun control law reduces welfare by interfering with individuals' voluntary decisions on the costs and benefits of armed defense. In addition to deterring such welfare-increasing decisions, conventional gun control law embodies the out-of-pocket costs of police, prosecutors, courts, and prisons, plus the punishment costs imposed on violators or accused violators. Juvenile prohibitions can lead to the lifetime label of "juvenile delinquent" on the basis of an innocuous or even beneficial act. At this point, these laws should be presumed to be bad unless they can be shown to do affirmative good, which any reading of Marvell's data precludes, at least on the basis of nationwide results.

In my judgment, Marvell's results leave no room for academic agnosticism in the name of dispassionate and detached analysis. When empirical research undermines conventional policy decisions on emotional and contested issues, such as gun control, it should narrow the range of acceptable policy options. If that does not happen, then we should look to public choice explanations for why legislatures resist the best available evidence of social consequences. Perhaps law enforcement groups wish to retain a monopoly on the protective use of firearms.

Whatever the actual explanation, academic researchers should not shrink from drawing the appropriate policy conclusions from their studies simply because the subject is controversial. To do so is to risk debasing the currency of academic research and abandoning controversial topics to other influences (such as journalists, publicists, lobbyists, and the like) that will move into the empty niche. If that outcome can happen with gun control, then it can happen elsewhere.

APPENDIX

COMMENT ON THE PUBLISHED VERSION

The body of my article comments on Marvell's failure to take account of his significant statistical findings in the 2000 manuscript indicating that juvenile gun control costs some hundreds of lives per year. The main difference in substance between the 2000 manuscript and Marvell's article as now published is the dilution of the statistical significance of those findings through a respecification of the regression model, without any mention of the previous findings' existence.[24] Accordingly, in Table A1, I report both the original and revised findings regarding the experimental variables representing the three types of juvenile gun control laws studied, in terms of their effects on gun homicide rates.

Obviously, these new regressions represent a poorer fit with the data than the 2000 findings. While 1 year of data has been added, the specifications also have been changed by dropping out two demographic control variables that appeared in the 2000 model: one for percentage of African-American population (which in all cases had a negative effect on gun homicide rates) and a second for the percentage of

TABLE A1

MARVELL'S ORIGINAL AND REVISED FINDINGS

	ORIGINAL VERSION (2000)[a]		PUBLISHED VERSION[b]	
	Coefficient	t	Coefficient	t
Ages 15–19 regression:				
Early state laws	.028	1.058	.000	.008
1994 state laws	.078	2.244	.172	1.787
Federal law only	−.033	1.198	−.045	0.582
Regression adjusted R^2	.92		.90	
F-statistic for three law types	3.49 (.02)		1.59 (.19)	
Ages 15–24 regression:				
Early state laws	.034	1.690	.−.000	.007
1994 state laws	.069	2.608	.129	1.757
Federal law only	−.035	1.625	−.079	1.324
Regression adjusted R^2	.95		.91	
F-statistic for three law types	5.59 (.001)		2.44 (.06)	
All-ages regression:				
Early state laws	.011	1.665	−.002	.080
1994 state laws	.026	2.588	.060	1.659
Federal law only	−.015	1.806	−.084	2.786
Regression adjusted R^2	.96		.95	
F-statistic for three law types	6.26 (.001)		5.55 (.001)	

[a] Thomas B. Marvell, The Impact of Banning Juvenile Gun Possession (paper presented at the American Enterprise Institute conference Guns, Crime, and Safety, December 10–11, 1999, Washington, D.C.), tables 3–5.

[b] Thomas B. Marvell, The Impact of Banning Juvenile Gun Possession, in this issue, at __, tables 2–4.

[24] I was unaware that Marvell's paper had undergone major changes in substance until I received final proofs of both papers. The changes in the content of the Marvell paper are so marked that the only practical alternative to withdrawing my article entirely was to add this Appendix summarizing the changes and reporting the original findings, which are omitted from the published version.

metropolitan population (which had a positive effect on gun homicide rates in two of the three regressions). As an apparent result of these changes, most of the experimental variables have larger coefficients but lower t-statistics. Even if the new results are taken at face value, there are unexplained discrepancies between the all-ages result and the results for youthful victims, in that the only favorable net effect on crime rates is produced in the category of older victims, in exchange for large increases in juvenile-victim homicides by gun, ranging up to a 17 percent increase for the most youthful category of victims aged 15–19.

There is no reason given in the Marvell article for the specification changes, and no acknowledgment of the previous significant findings, which are omissions worthy of criticism in themselves. Otherwise, the appearance of these changes during the editorial process illustrates the prescience of my previous normative comments: that the failure of academic researchers to take their own work seriously has and will debase the currency of academic research in the public debate.