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Reducing Gun Violence in America

Webster, Daniel W., Vernick, Jon S., Bloomberg, Michael R.

Published by Johns Hopkins University Press

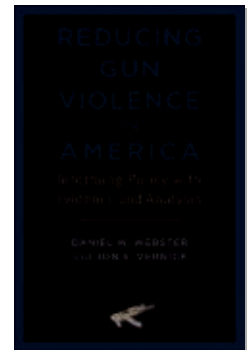
Webster, Daniel W., et al.

Reducing Gun Violence in America: Informing Policy with Evidence and Analysis.

Johns Hopkins University Press, 2013.

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The Limited Impact of the Brady Act

Evaluation and Implications

Philip J. Cook and Jens Ludwig

Federal firearms law divides the population into two groups: those prohibited from legally possessing a firearm due to their criminal record or certain other disqualifying conditions and everyone else. The vast majority of the adult public is allowed to acquire and possess all the firearms they want, thus preserving the personal right to “keep and bear arms” that has been established by recent U.S. Supreme Court rulings.¹ But that right, like all rights, has limits. People with serious criminal records or severe mental illness may reasonably be deemed at such high risk of misusing firearms that public-safety concerns take precedence over gun rights. While in practice it is impossible to keep all members of high-risk groups disarmed in a gun-rich environment, a selective prohibition may cause some reduction in gun misuse and save enough lives to be worthwhile.

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The effectiveness of this selective-prohibition approach may depend on how it is enforced. The two mechanisms in use to discourage disqualified people from obtaining guns are deterrence through the threat of criminal prosecution (“felon in possession” cases) and regulation of firearms transactions. The current regulatory framework was created by the Gun Control Act of 1968 (GCA), which required that those in the business of selling guns obtain a federal firearms license (FFL) and that interstate shipments of guns be limited to licensees. Anyone purchasing a gun from an FFL is required by the GCA to fill out a form 4473 stating that he or she did not have a felony conviction or other disqualifying condition, although under federal law dealers were not required to verify the information reported by the prospective buyer.

The GCA’s requirement was greatly strengthened by subsequent legislation, the Brady Handgun Violence Prevention Act, implemented in 1994. The Brady Act required that FFLs conduct a background check on would-be buyers—the buyer’s signature on a 4473 was no longer enough. This new regulation was enacted with high hopes of reducing gun violence, despite its limitations. Most gun crimes are committed with weapons that were not purchased from dealers, but rather acquired through off-the-books transactions. Such transactions are generally permitted and not regulated by the Brady Act. However, some disqualified individuals do attempt to buy guns from FFLs, and the *Brady* background checks have blocked over 2 million sales since the law was implemented (Bowling et al. 2010).

On March 2, 2000, President Bill Clinton declared at a news conference that “the Brady Bill is saving people’s lives and keeping guns out of the wrong hands,” a claim justified in part by the substantial number of people who had been denied handguns as a result of the law.² During the first five years of the Brady Act, 312,000 applications to purchase handguns from dealers (2.4% of the total) were denied due to a felony record or other disqualifying characteristic (Bowling et al. 2010). Other would-be buyers with criminal records may have been deterred from even attempting to buy a firearm. The logic is clear: Since guns are more lethal than knives and other likely substitutes, any reduction in criminal gun use due to *Brady* would likely translate into a net reduction in homicides (Zimring 1968, 1972).

The same year that President Clinton claimed success we published an evaluation of the Brady Act in the *Journal of the American Medical Association* (Ludwig and Cook 2000). Our conclusion was less positive—we found no evidence of a reduction in the homicide rate that could be attributed to

Brady. We also considered the possibility that *Brady* reduced the overall suicide rate, but found no discernible impact on that outcome either. In presenting these findings, we cautioned that our statistical method rested on certain untested (though in our judgment, reasonable) assumptions, and that our null results still left some room for the possibility that *Brady* had an effect, albeit small, and either positive or negative. Further, even if our null results are correct for the early years of *Brady*, they do not preclude the possibility that a different regulatory scheme might be more effective in achieving the purpose. Indeed, the Brady Act itself incorporated potentially important changes that were implemented in December 1998. While the initial “interim” phase, from 1994 to 1998, was limited to handgun purchases, the second “permanent” phase expanded the background check requirement to include purchasers of rifles and shotguns. Perhaps more importantly, the interim phase required a five-day waiting period from application to delivery of the handgun, while the permanent phase replaced the waiting period with a new system, known as the National Instant Criminal Background Check System (NICS). Our evaluation focused entirely on the interim phase.

In this essay we provide a summary of our evaluation, discussing its strengths and limitations, and then go on to consider two questions that are vital to the current debate: (1) What are the most important limitations of the current selective prohibition system?; and (2) How could this general approach be strengthened?

Background and Findings

James Brady, press secretary to President Reagan, was shot during an assassination attempt against the president in March 1981. Together with his wife, Sarah, Brady became a leader of the gun control movement, and through Handgun Control, Inc. worked for seven years to achieve passage of what became known as the Brady Handgun Violence Prevention Act. The first set of provisions was implemented in February 1994, requiring that FFLs conduct a background check and wait for five business days before transferring a handgun to a customer. Only 32 states were directly affected by these provisions, because the other 18 states and the District of Columbia already met the minimum requirements of the Act. In effect these provisions created a sort of natural experiment, with 32 states in the “change” or “treatment” condition, and the 18 no-change states serving as “controls.” Our evaluation took

advantage of this experiment-like setting to estimate the causal effect of the Brady Act on certain outcomes.

Our main outcome measure was the homicide rate from the Vital Statistics records. While other types of crime are also of interest, the data on homicides are more detailed and far more accurate than for the other violent crimes, such as robbery and assault. (The main limitation of the Vital Statistics data for our purposes is the lack of information on perpetrators.) We also analyzed the effects of the *Brady* regulations on suicide. The focus of our analysis for both homicide and suicide was on adult victims, and in particular for those 21 years of age and older. The primary rationale for this age limitation is that the Brady Act would logically have little or no effect on access to guns by those under 21; federal law sets 21 as the minimum age to purchase a handgun from an FFL, and the age of the customer was subject to check even before *Brady* by a requirement that he or she show identification. Of course, limiting the homicide outcome to adult victims does not provide exactly what we would like to have, namely rates of homicide *committed* by those age 21 or over; *Brady* regulations are aimed at the potential perpetrators rather than the victims. But in practice teenage killers select teenage victims, and few homicide victims aged 21 years or over are shot by perpetrators under 21 years of age (Cook and Laub 1998). It turns out that limiting the analysis to adults is not only logical given the nature of the intervention, it also enhances the validity of our evaluation method, since it helps avoid potential biases introduced by the volatility of juvenile homicides during our sample period that was associated with the rise and fall in crack-market activity (Blumstein 1995; Cork 1999).

The importance of having a control group for evaluating the effect of *Brady* on the “treatment” states’ homicide rates is that other factors were at play, and homicide rates were dropping nationwide in the 1990s. In particular, the national homicide rate dropped by 34% from 1990 to 1998. Most of the crime drop during the 1990s (which was by no means limited to homicide) has been attributed to causes that are unrelated to changes in firearm regulations. Among the factors that have been suggested to explain the crime drop of the 1990s are increased imprisonment and spending on police, the waning of the crack cocaine “epidemic” that began in the mid-1980s, and, more controversially, the legalization of abortion in the early 1970s (Blumstein and Wallman 2000; Levitt 2004; Cook and Laub 2002). In any event, an evaluation of the Brady Act based only on the trend in homicide rates in

the *Brady* treatment states would mistakenly attribute to the Brady Act the effects of all of the other forces that were driving crime rates down over the 1990s.

Our assumption that the 18 states that were not directly affected by the Brady Act provide a valid control group is supported by the remarkable similarity in pre-*Brady* trends in adult homicide rates. Evidently other causal factors did exert similar impacts on the *Brady* treatment and control states. Thus if the trends in homicide rates (and especially gun homicide rates) had diverged between the two groups after *Brady*, then it would be plausible to attribute that divergence to the new regulations introduced by the Act. Our evaluation approach is further supported by the fact that the law in question was exogenous to the individual states—there is no “self-selection” problem here, as might arise if we were evaluating laws that were changed by the act of individual state legislatures (perhaps in response to state-specific changes in crime).

A distinct concern in evaluating the effects of the Brady Act is that the new law may have reduced gun running from the treatment to control states, in which case comparing the two groups of states might understate the overall effects of the law (Weil 1997). The concern here is that homicide rates in the “control” states were in fact affected by the intervention. Some support for this concern comes from ATF trace data in Chicago showing that the fraction of crime guns in the city that could be traced to the *Brady* treatment states declined dramatically following implementation of the law (Cook and Braga 2001). However, the proportion of homicides in Chicago committed with guns did not change over this period, despite the substantial changes in gun-trafficking patterns (Cook and Ludwig 2003). One explanation of these results is that traffickers were able to substitute in-state sources for out-of-state sources at little extra cost. If correct, they suggest that while *Brady* did affect trafficking to the control states, the effect was not of much consequence for gun availability to those at risk of violence in those states.

Here are the specifics of our quantitative evaluation. We utilize a “difference in difference” approach that compares the pre- and post-*Brady* changes for the treatment and control groups. The econometric technique is panel regression analysis utilizing specification (1) below, where Y_{it} represents a mortality measure for state (i) in period (t), and X_{it} represents a set of control variables.³ The model includes separate dichotomous indicator variables for each state, d_p , to

capture unmeasured state-specific “fixed effects” that cause the level of violence to differ across states, a set of year indicator variables, g_t , that capture changes in the overall rate of violence in the U.S. conditional on the observed covariates, and the indicator variable T_{it} that is equal to 1 in the treatment states following implementation of the Brady Act and equal to 0 otherwise. From Vital Statistics data, we had available four years of post-*Brady* data (1994 to 1997). For comparability, we define the pre-*Brady* period as the four years prior to the law’s implementation (1990 to 1993).

$$Y_{it} = b_0 + b_1 X_{it} + b_2 T_{it} + d_i + g_t + e_{it} \quad (1)$$

Since state-specific fixed effects are included in the model, the key coefficient of interest (b_2) reflects the difference between the treatment and control states in the change in violence rates from the pre- to post-*Brady* periods. The coefficient b_2 captures any one-time shift in the rate of gun violence in the treatment versus control states around the time of the Brady Act, and should be negative if *Brady* reduced gun violence.

Equation (1) was estimated via weighted least squares, a technique that corrects for heteroskedasticity in the stochastic term by pre-multiplying the dependent and explanatory variables by the square root of the state’s population. We calculated Huber-White standard errors to adjust for the non-independence of observations from the same state.

The findings from this regression analysis are summarized in Table 2.1. We find no statistically discernible difference in homicide trends between the *Brady* (treatment) and non-*Brady* (control) states among people aged 21 and older. While our point estimates are negative, they are even more negative for non-gun homicide than for gun homicide (and in every case statistically insignificant). In this pattern of results we see no case for a causal effect of *Brady*. The 95% confidence interval for one version of our estimates ranges from an increase of 8% to a reduction of 13%.⁴

Of course the Brady Act may have affected outcomes other than homicide. In particular, the waiting period required during phase one of *Brady* may have slowed handgun acquisition by some people experiencing a suicidal impulse. As shown in Table 2.1, our analysis of suicide rates found some evidence that *Brady* may have reduced gun suicide rates among people aged 55 and older. However, these gains were at least partially offset by an increase in non-gun suicides (perhaps due to weapon substitution), so whether waiting periods reduced overall suicides among this age group is unclear.

Table 2.1 Effects of the Brady Act on homicide and suicide changes from pre- to post-Brady period in treatment relative to control states (Standard-error estimates in parentheses)

	Victims aged 21 and older	Victims aged 55 and older
Homicide (rate per 100,000)	-0.36 (0.64)	-0.09 (0.27)
Gun homicide rate	-0.14 (0.52)	0.05 (0.10)
Non-gun homicide rate	-0.22 (0.15)	-0.14 (0.20)
% homicides committed with gun	1.1 (1.0)	3.3 (2.4)
Suicide (rate per 100,000)	-0.12 (0.27)	-0.54 (0.37)
Gun suicide rate	-0.21 (0.19)	-0.92** (0.25)
Non-gun suicide rate	0.09 (0.13)	0.38* (0.20)
% suicides committed with gun	-0.3 (0.5)	-2.2** (0.9)

Source: Cook and Ludwig (2003). The original results reported in Ludwig and Cook (2000) were based on a data set with several minor errors which we subsequently corrected.

Note: The pre-Brady period is defined as 1990 to 1993 and post-Brady period as 1994 to 1997. Regressions are calculated by estimating equation (2) in text using state population as weights to adjust for heteroskedasticity.

**Statistically different from zero at the 5% p-value

*Statistically different from zero at the 10% p-value

How do we reconcile our findings of no detectable impacts on homicide with administrative records on the numbers of people denied handguns as a result of Brady background check requirements? About 2.4% of potential handgun buyers were denied handguns during the interim phase of the Brady Act as a result of background checks (Bowling et al. 2010). One explanation is that the type of person who is disqualified from legally buying a gun but shops at an FFL anyway tends to be at relatively low risk for misusing a gun (compared with other disqualified individuals). Data from California show that individuals who were denied purchase of a handgun due to a felony record have 23% fewer violent-crime arrests than those who have been arrested but not convicted for a felony, and thus were able to successfully purchase a handgun from an FFL (Wright, Wintemute and Rivara 1999). Yet the follow-up arrest rates for both groups are fairly low, and only around 3% of violent-crime arrests are for homicide (Wright and Wintemute 1999). Projecting the California data to the nation suggests that those 312,000 convicted felons who were denied a handgun in Brady states in the interim phase (from 1994 to 1998) would have committed about 60 fewer homicides as a result.

Discussion

Suppose that our null findings are correct and that the first phase of the Brady Act had little or no impact on homicide or suicide rates. What are the likely explanations, and what can we conclude about the possibility of saving lives through the Gun Control Act's ban on gun possession by certain high-risk groups?

The most prominent of the likely explanations is simply that by limiting the background-check requirement to sales by FFLs, the Brady Act's background-check requirement had no direct effect on the vast majority of transactions that provide criminals with guns. Surveys of prisoners in the 1980s show that only one-fifth obtained their guns directly from a licensed gun dealer (Wright & Rossi, 1994), even though at that time dealers in most states were not required to conduct background checks to verify the buyer's eligibility.⁵ Most crime guns are obtained from people who are not licensed FFLs through private transactions that are largely unregulated under existing federal law—that is, these crime guns are obtained in the off-the-books secondary gun market.

While this “private sale loophole” is the most compelling explanation for limited impact of the Brady Act, there are several other considerations that should be taken into account. First, a majority of adults who end up using a gun in crime are not disqualified from possessing a gun. Cook, Ludwig and Braga (2005) find that nearly three in five homicide offenders in Illinois in 2001 did not have a felony conviction within the 10 years prior to the homicide. Not that they had spotless records—only one-quarter of homicide offenders had not been arrested at least once during the 10 years prior to the homicide. Expanding the crime-related disqualification criteria to include, say, conviction of any violent misdemeanor (rather than the current disqualification, which is limited to felonies and misdemeanor domestic violence) could help in this respect.

Second, even if a disqualified person did seek to buy guns from an FFL after *Brady*, there is a good chance of success, simply because the relevant records are often incomplete or difficult to access. In recognition of this problem Congress established the National Criminal History Improvement Program (NCHIP) to provide grants and technical assistance to the states to improve the quality and immediate accessibility of criminal history records and related information. This federal investment resulted in an 83% increase in

the criminal records accessible for background checks by 2003 (Ramker 2006), thereby increasing the chance that a disqualified person would be identified as such through the NICS process. NCHIP has continued to provide modest funding for improving records and was supplemented in 2007 by a new program focused on assisting states to incorporate mental health records in the NICS system. A few states have made large gains in this respect, but most do not yet have a reliable system in place for submitting relevant records on severe mental illness or drug abuse (Mayors Against Illegal Guns 2011).

In sum, the limitations of the current system for screening firearms buyers to prevent gun crimes include, in order of importance, the private sales loophole, the fact that a large share of gun criminals are not disqualified, and the incomplete coverage of the databases utilized in the NICS. The same limitations apply if the screening system is intended to prevent gun suicides, although for suicides the relative importance of these three changes differs: those at risk of suicide may be more likely to obtain guns from FFLs (in which case the private-sales loophole would be less important) but much less likely to be disqualified under current standards.⁶

There has been considerable interest in closing the private-sales loophole by simply requiring that all gun sales, whether in the primary or secondary market, be subject to background checks. California has instituted such a system for firearms transactions, which must go through an FFL who then charges a fee for conducting the background check. Such a system, were it to be enforceable, would make it more difficult for disqualified people to obtain a gun. The fundamental question is how to enforce such a system. California requires that handguns be registered to their owner, which is useful in holding owners accountable for the disposition of their handguns. Even without a registration requirement, a universal background check system could be enforced in a variety of ways, including law-enforcement oversight of gun shows and undercover “buy and bust” operations by the police. Whether the California system is successful in reducing gun violence has not been established (but see Webster, Vernick, and Bulzacchelli 2009).

While the prospects are dim for decisive victories against gun violence through modest improvements in the regulation of gun transfers, the stakes are very high. Even just a one percent reduction in gun homicides and suicides would amount to over 300 lives saved—enough to justify a billion-dollar program by the usual reckoning of the value of life. The findings from our evaluation of the Brady Act certainly do not rule out the possibility that it saved

several times that many lives during each of the early years, and hence was worthwhile. Neither our evaluation method nor any other that we know of would be precise enough to detect such a proportionally small effect.

ACKNOWLEDGMENTS

The original research reported in this essay was supported by a grant from the Joyce Foundation. The authors thank Bob Malme, Chris Clark, Heath Einstein, Meghan McNally, and Esperanza Ross for valuable research assistance, and Roseanna Ander, Steve Hargarten, David Hemenway, Arthur Kellermann, Debby Leff, Willard Manning, James Mercy, John Mullahy, William Schwab, Daniel Webster, Garen Wintemute, and Mona Wright for useful comments. All opinions and any errors are the authors'.

NOTES

1. *District of Columbia v. Heller* (554 US 570 (2008)) established a personal right to keep a handgun in the home for self-defense purposes. *McDonald v. Chicago* (561 US 3025 (2010)) extended this right beyond federal jurisdiction to encompass state and local governments.

2. Brady Campaign to Prevent Gun Violence, 'Saving Lives by Taking Guns Out of Crime: The Drop in Gun-Related Crime Deaths Since Enactment of the Brady Law,' Executive Summary, downloaded from <http://www.bradycenter.org/xshare/Facts/brady-law-drop-in-crime.pdf>

3. In our reported specification, we controlled for state-level changes in the following factors that may influence rates of crime and violence: consumption of alcohol per capita (measured in gallons of ethanol), percentage of the population living in metropolitan areas, percentage of the population living below the official poverty line and income level per worker (in 1998 constant dollars) percentage who are African American, and the percentage of the population falling into 7 different age groups.

4. In this version we used the log form of the dependent variable in each of the regressions. The results using other specifications are similar.

5. For a more recent estimate of the percent of crime guns obtained directly from an FFL, see the essay by Webster, Vernick, McGinty, and Alcorn (in this volume).

6. In a personal communication dated January 14, 2013, Mallory O'Brien, Director of the Milwaukee Homicide Review Commission, reports evidence that suicides, unlike violent criminals, are quite likely to obtain their guns directly from an FFL. "From January 1, 2010 to December 31, 2012, firearms were recovered from 59 suicide victims in the City of Milwaukee. ATF eTrace data was used to determine: first purchaser, time to event and firearm type. ATF was able to successfully trace firearms

for 52 of the victims. In 31 (60%) cases the suicide victim purchased the firearm from a licensed firearm dealer. Ten of these victims who purchased the firearm from an FFL used the weapon within a year of the event.”

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