Can an Informative Letter Reduce Gun Crime and Be Cost-Effective?

A Study of Los Angeles

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Preface

The National Institute of Justice provided a grant to the RAND Corporation's Justice Policy Program to identify the cost-effectiveness of a gun letter program used in Los Angeles, California, to reduce gun crime and gun trafficking. This report provides estimates of the effects of the program on citywide levels of homicide, robbery, and aggravated assault with a firearm. We also demonstrate how much the program cost the city and provide a discussion of the costs and benefits trade-offs to the Los Angeles District Attorney's Office running the program, as well as to wider society. Although the focus of this study is on Los Angeles, other large cities may find the results of interest as well.

The findings of this study are based on two types of statistical analyses to identify the impact of the letter program, which involved sending letters to individuals purchasing a new handgun from a licensed dealer regarding their responsibilities as gun owners and the steps they are required to take should they transfer the weapon to another person. For the cost analysis, we used a bottom-up, cost accounting approach to determine the staff time spent on the program and any minor equipment expenses. Using literature on the social and economic benefits of crime prevention and the benefit-cost ratios of other crime reduction programs, we conclude with recommendations for research and evaluation and a discussion about the trade-offs for policymakers deciding whether to implement a similar program.

RAND Justice Policy

The research reported here was conducted in the RAND Justice Policy Program, which spans both criminal and civil justice system issues with such topics as public safety, effective policing, police-community relations, drug policy and enforcement, corrections policy, use of technology in law enforcement, tort reform, catastrophe and mass-injury compensation, court resourcing, and insurance regulation. Program research is supported by government agencies, foundations, and the private sector.

This program is part of RAND Justice, Infrastructure, and Environment, a division of the RAND Corporation dedicated to improving policy- and decisionmaking in a wide range of policy domains, including civil and criminal justice, infrastructure protection and homeland security, transportation and energy policy, and environmental and natural resource policy.

Questions or comments about this report should be sent to the project leader, Priscillia Hunt (Priscillia_Hunt@rand.org). For more information about RAND Justice Policy, see www.rand.org/jie/justice-policy or contact the director at justice@rand.org.

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Overall Study Motivation

Between 2012 and 2015, the number of homicides committed with a firearm increased 12 percent to 12,979 across the 50 U.S. states, Washington, D.C., Guam, and the Virgin Islands (Centers for Disease Control and Prevention, undated-b). Some estimates would put the economic cost associated to this number of firearm homicides at $112.3 billion in 2015 alone, in terms of the psychological, social, and economic loss and damages (Heaton, 2010). In addition to fatalities, firearm violence resulted in more than 20,000 hospital admissions (Centers for Disease Control and Prevention, undated-a), with more than 460,000 nonfatal criminal firearm victimizations in 2014 (Truman and Langton, 2015). Given the magnitude of these damages, it is vital that criminal justice agencies develop programs that reduce the risk of these crimes. Furthermore, local, state, and federal governments have limited resources, so considering the benefit-cost of a program to society compared with other programs can be one way of deciding whether to select a program.

Study Intervention Background

In 2007, the Los Angeles City Attorney's Office implemented a pilot study of a gun letter intervention to deter straw purchases and other illegal transfers of firearms (such as gifting to a prohibited possessor), and to increase reporting of lost or stolen firearms (Ridgeway et al., 2008). The intervention involved sending a letter to handgun purchasers during their ten-day waiting periods between purchase and taking possession of the firearm, which advised the purchaser that the new weapon was registered to them and that failure to properly record any transfer or loss of the weapon with California's Department of Justice could result in the owners' liability for any future misuse of the gun. Because studies show that many people who use a gun in a crime (1) do so with a gun recently purchased from a licensed dealer but (2) acquired the gun through the secondary market or by a straw purchase, the idea of the letter program was to deter legal buyers from transferring the weapon to a prohibited possessor who may use the gun in a crime.

A pilot randomized controlled trial of the letter program was conducted in two neighborhoods of the city of Los Angeles from May 2007 to September 2008 (Ridgeway et al., 2011). Results of the trial indicated people who received the letter were twice as likely to report their gun stolen as those who did not receive the letter. Although no difference between groups was found regarding rates at which guns were recovered at crime scenes, the study follow-up period may have been too short to detect any such effects.

Present Study

Five years after the pilot study, the letter program was fully implemented citywide from January 1, 2013, through September 1, 2015. Letters were sent to all handgun purchasers residing in city of Los Angeles zip codes during the ten-day waiting period. Our study expanded on previous findings of the pilot study by examining the effect of the letters on firearm violence citywide and by conducting a benefit-cost assessment of the letter program.

We used two statistical approaches to assess the effect on firearm violence. First, we used monthly data on the number of reported firearm violence offenses (homicide, robbery, and aggravated assault) from October 2008 to December 2015 and tested whether the introduction of the letter in 2013 fundamentally changed the pattern of firearm violence in Los Angeles. Second, we used the monthly rates of firearm violence in Los Angeles before the letter program (2001 through 2012) and matched Los Angeles to a control group with similar trends in firearm violence. Using this match, we tested whether there was any difference in firearm violence rates between Los Angeles and the control group after the letter program was implemented (2013 and 2014). A number of robustness checks were performed to test the sensitivity of results.

Finally, we calculated the cost of implementing the program over a period for which we have data (March 7, 2014, through September 1, 2015), and compared the costs of the program with the societal benefits of firearm crimes prevented. We also compared the benefit-cost ratio of this program to other criminal justice programs for adults.

Findings

We were unable to detect a reliable citywide effect for the letter program on homicides, robberies, or aggravated assaults with a firearm. The cost of the program was approximately $1.13 to $1.85 per letter, or $145 to $428 per day worked on the program. Considering the cost of the program relative to the societal benefits of preventing victimization, if the program prevents one homicide, one aggravated assault, or two robberies, then the program achieves a net benefit to society. While a statistical analysis would not have been able to identify the prevention of one or two crimes at a citywide level regardless of the true effects, it is also unclear whether the letter program can indeed prevent any firearm violence.

Policy Implications

Attorney's Offices make decisions about what programs to fund given the effectiveness of programs and limited budgets and staff time. To that end, the letter program was a relatively low-cost program. Given the way that the Los Angeles City Attorney's Office staffed and operated the program by the end, the opportunity costs were also minimized in such a way that made the program feasible along with the other work that staff needed to perform. However, although it was low cost, this program appeared to have effects on firearms crime too minor to detect using

our statistical procedures. If an Attorney's Office has the resources, there are more costly programs that have demonstrated better cost-effectiveness than the letter program (we note some of these in the results section of this report). If, however, an Attorney's Office is specifically looking for lower-cost programs (approximately $55,000 per year operating cost, not including start-up costs), the letter program appears to be a sensible option.

Another important implication of this study is data-related. It is essential to have data on the guns affected by the program and a group of comparison guns to more definitively determine whether firearm-related strategies are cost-effective. For example, if we had information on the specific guns used in firearm violence as a result of straw purchases, then we could determine whether the letter intervention made it less likely that the affected guns were used in this manner. We were not able to access these data through the Bureau of Alcohol, Tobacco, Firearms and Explosives or California's Department of Justice for purposes of this study.

Acknowledgments

Abbreviations

ATF	Bureau of Alcohol, Tobacco, Firearms and Explosives
CBT	cognitive behavioral therapy
DROS	dealer record of sale
FBI	Federal Bureau of Investigation
ITSA	Interrupted Time Series Analysis
SCM	synthetic control method

1. Introduction

Background

In 2005, an interagency working group of California law enforcement officials and crime researchers entered a partnership to design interventions to reduce gun violence in Los Angeles. At the time, and for the past two decades, Los Angeles County accounted for 44 to 55 percent of all firearm homicides in California.[1] Because many guns used in crimes were purchased within the past three to five years but by someone other than the person committing the crime (Cook and Braga, 2001), one of the partnerships' objectives was reducing the transfer of legally purchased weapons to individuals likely to commit crimes. With the support of California's Attorney General, the Los Angeles City Attorney agreed to prioritize the prosecution of illegal transfers of handguns in the city. California laws required that nearly all such transfers between private parties be conducted through a federally licensed gun dealer, where a record of the transfer would be generated and recorded at the California Department of Justice (California Penal Code § 27545, 2011). By publicizing this new priority for prosecution, the Los Angeles City Attorney's Office and partner agencies hoped to facilitate investigations of crimes committed with firearms that had been improperly transferred and to deter straw purchases, whereby legal purchasers of firearms buy them on behalf of those prohibited from purchasing them.

To evaluate the effectiveness of the intervention, the RAND Corporation and the Los Angeles City Attorney's Office initially publicized the program by mail to new handgun purchasers in two high-crime neighborhoods of Los Angeles. The letter for this pilot project was sent to a random subset of individuals making new handgun purchases during the ten-day waiting periods between purchases and taking possession of the firearms. It advised the purchaser that the dealer record of sale (DROS) for the new weapon was in his or her name, and that failure to properly record any transfer of the weapon with California's Department of Justice is a crime. The letter further emphasized that whenever an improperly transferred gun is found at a crime scene, the Los Angeles City Attorney's Office would prosecute the original owner.

A pilot randomized controlled trial of the letter program was conducted in two high-crime neighborhoods of Los Angeles between May 2007 and September 2008 (Ridgeway et al., 2011). Results of the trial indicated that people who received the letter were more likely to report a gun stolen. Given the relatively high rate of guns reported stolen in the neighborhoods studied, the finding may reflect an increased reporting of true gun thefts, of false reporting in an effort to

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[1] Analysis by the authors performed on the Federal Bureau of Investigation (FBI) Supplemental Homicide reports (FBI and U.S. Department of Justice, 2014b).

"separate the gun buyer from future misuse of the gun," or both (Ridgeway et al., 2011). The authors did not find statistically significant differences in the rates at which guns were later recovered at crime scenes, although the short follow-up of at most two years may have been insufficient for any such differences to be observed.

Present Study

Five years after the pilot project, Los Angeles launched a citywide gun letter program. Letters were sent to all handgun purchasers residing in zip codes within Los Angeles from January 1, 2013, through September 1, 2015. Once a purchase is made in California, a DROS form for each attempted handgun purchase is sent to the state's Bureau of Firearms. It is the state's receipt of this form that starts the ten-day waiting period. The Los Angeles City Attorney's Office tracked DROS forms for residents in city of Los Angeles zip codes. Based on DROS forms, letters were sent to those living within the Los Angeles boundary and to individuals residing outside of Los Angeles if they lived in a zip code that extends outside the boundary.

The letter explained the duty of handgun owners to conduct any sale or transfer through a licensed dealer, and that failure to do so could lead to prosecution (see the letter in Figure A.1 in the Appendix). In addition, the letter emphasized other criminal penalties that could be brought against gun owners who knowingly transferred, sold, or lent a firearm to anyone prohibited from possessing one, including someone with a known mental disorder. Furthermore, it emphasized that if a child gained access to the handgun and injured himself, herself, or others, the gun owner was subject to criminal prosecution.

Our study expanded on the previous pilot study by investigating the effect of the letter program on firearm violence citywide and by estimating the costs and benefits of the program to society. Between 2001 and 2014, Los Angeles had on average comparable levels of firearm crime to several cities across the United States. Among California cities with a population of over 100,000, Los Angeles had the fifth highest rate of total crimes with a firearm after Stockton, San Bernardino, Inglewood, and Pomona (FBI and U.S. Department of Justice, 2014a and 2014b). Nationally, compared with large cities (population of 500,000 or more), Los Angeles ranked 20th in firearm violence with rates similar to Albuquerque, New Mexico; Phoenix and Tucson, Arizona; and Las Vegas, Nevada (Figure 1.1).

Figure 1.1. Top 25 Large Cities for Total Crime Committed with a Firearm, 2001–2014 Annual Mean
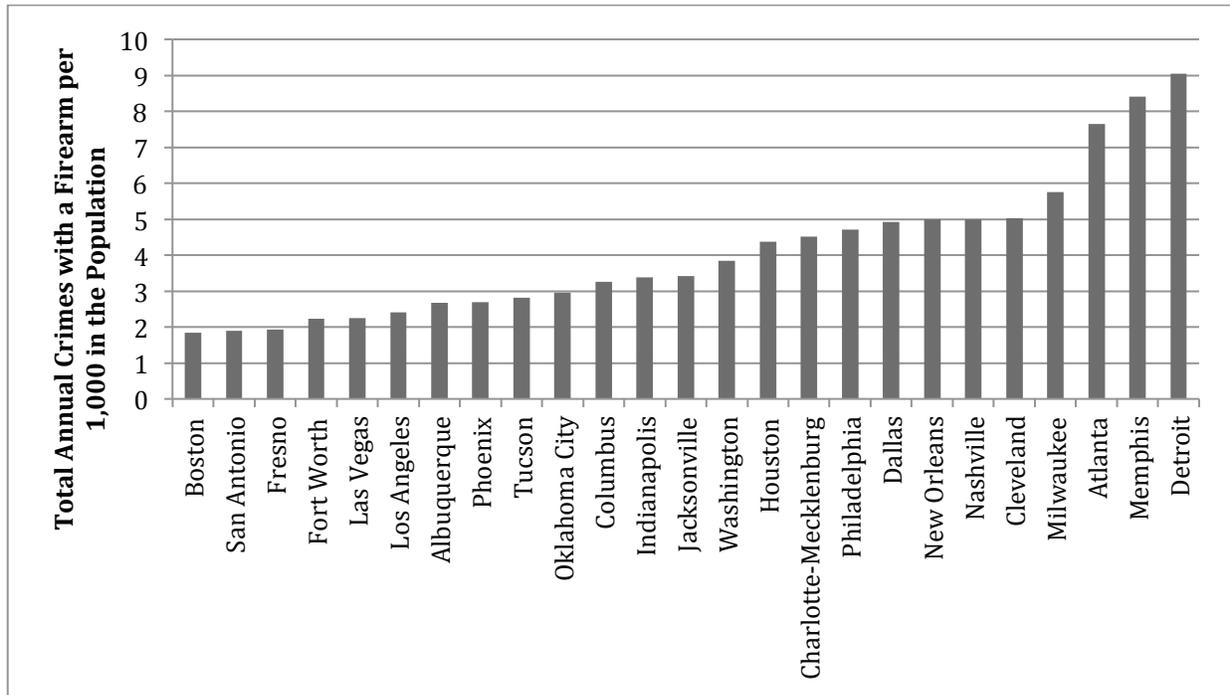
SOURCE: FBI and U.S. Department of Justice (2014a and 2014b).
NOTE: Large cities are considered to be those with a population of 500,000 or more in at least one year since 1980.

How Can a Letter Reduce Gun Crime?

Between 2012 and 2015, the number of homicides committed with a firearm increased 12 percent to 12,979 across the 50 U.S. states, Washington, D.C., Guam, and the Virgin Islands (Centers for Disease Control and Prevention, undated-b). In addition to fatalities, firearm violence resulted in more than 20,000 hospital admissions (Centers for Disease Control and Prevention, undated-a), with more than 460,000 nonfatal criminal firearm victimizations in 2014 (Truman and Langton, 2015). Research suggests that one important stream of illegal guns to criminals in targeted areas involve legal purchasers who engage in one or two straw purchases to provide guns to someone with a disqualifying criminal record (Ridgeway et al., 2008). In the vast majority of firearm crimes, offenders have not purchased their weapons themselves. Instead, approximately 70 percent of state correctional inmates who committed crimes with a gun obtained their weapons from family members, friends and associates, or in unregulated street transactions (Cook, Parker, and Pollack, 2015). At the same time, many studies have found that guns used in crimes were manufactured relatively recently (Cook and Braga, 2001; Kennedy, Piehl, and Braga, 1996; Pierce et al., 2001; Wachtel, 1998). This implies that the legal first purchasers of firearms used in crimes were not typically the ones who will commit crimes with them. Instead, guns that later turn up at crime scenes are transferred shortly after their legal purchase to others who commit the crimes, or who themselves quickly transfer the weapon again.

Furthermore, guns used in crimes in Los Angeles tend to have been purchased in the city. This evidence was identified during the pilot study when, as a separate research task for a Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) Southern California Regional crime-gun center, a software-based tool was developed to identify patterns of potential firearm trafficking (Ridgeway et al., 2008). One finding from the tool was that when the firearm possessor was not the original purchaser, the geographic distance between the two was quite small. Specifically,

> [i]n more than one-third of all such instances in the 77th Street study area, the original legal purchaser resided within 4.5 miles of 77th Street. Based on its investigative experience, the interagency working group suggested that the local nature of the market was driven by prohibited possessors who were having local friends or family members conduct straw purchases for them. (Ridgeway et al., 2008, pg. 10)

Consequently, a strategy that reduces the likelihood of a new purchase by a legal gun buyer being transferred to a disqualified person could lead to reductions in crime with a firearm within the city.

One strategy used by local, state, and federal governments to induce changes in behavior is to send letters appealing to the sense of harm in the community generated by the unwanted behavior (Blumenthal, Christian, and Slemrod, 2001). Research has demonstrated that letters tend to induce changes in behavior for some groups of a target population, but the effects tend to depend on the content of the letter. For example, a tax letter experiment in Minnesota in 1994 had no effect on aggregate behavior, but there were certain subgroups that responded to the letter depending on specifics of the language used (Blumenthal, Christian, and Slemrod, 2001). Some people in the treatment group received a letter with a sentence describing how the community suffers when people do not pay their taxes, while others received a letter with sentence about how most people pay their taxes (i.e., there were two "treatment arms" and one control group). The results were surprising: People paying a mortgage increased tax compliance if they received the letter about the community suffering, but those mortgage payers receiving the letter about the high proportion of compliers were less compliant. Similarly, a letter experiment aimed at reducing the inappropriate provision of Schedule II controlled substances by "overprescribing" providers had no overall impact on physicians, although the authors indicated that repeatedly contacting and altering the language to focus on penalties rather than focusing on a comparison to peers may lead to changes in prescribing behavior (Sacarny et al., 2016). Indeed, a letter program to adult probationers designed to increase victim restitution payments included these features—repeated letters and a reminder about penalties—and resulted in statistically significant increases in payment (Lurigio and Davis, 1990). Like the other letter programs, the letter affected some probationer groups more than others: People with jobs and a shorter criminal history (e.g., fewer prior arrests) were more likely to increase restitution payments than other probationers.

Summary

This study evaluates the effectiveness and cost of an intervention designed to reduce firearm violence by reducing straw purchases, in which a legal purchaser of firearms acquires one on behalf of a prohibited possessor, and by increasing reporting of guns that are lost or stolen (Braga et al., 2002). The intervention consisted of a single letter sent to all purchasers of handguns in the city of Los Angeles during the ten-day waiting period between purchase and acquisition of the gun. The letter advised handgun purchasers that their new weapons were registered to them, and that failure to properly record any transfer of the weapon with California's Bureau of Firearms could result in the owners' liability for any future misuse of the gun. The letter further described dangers associated with improper storage and use of firearms. The letter intervention was modeled on many government programs using letters as a low-cost way to improve compliance with laws and regulations, and to encourage healthier and safer behaviors (Wenzel, 2006).

There are two key mechanisms by which the letter program could result in less firearm violence. Studies show that people who use a gun in a crime have recently acquired the gun (Cook and Braga, 2001; Kennedy et al., 1996; Pierce et al., 2001; Wachtel, 1998) from a family member, friend, or black market suppliers (ATF, 2002; Webster et al., 2013). As such, the letter could increase the likelihood of legal buyers reporting guns lost or stolen and/or deter legal buyers from making an illegal transfer of their weapons to someone residing in the city who might be a prohibited possessor or who might not want a record of the transfer linking him or her to the gun, either of which could elevate the risk of the gun being used in a crime. This report presents an evaluation of the effect of the letter on city-level firearm violence and the costs and benefits of the program.

2. Data on the Los Angeles Citywide Gun Letter Program

Although the first full month of the program was January 2013, the Los Angeles City Attorney's Office systematically organized aggregated data daily between March 7, 2014, and September 1, 2015, which we present here. Aggregated data were available on the number of attempted and denied purchases, the number of letters sent each working day, and the number of letters returned by the post office as undeliverable for residents with Los Angeles zip codes. As shown in Table 1, there were approximately 125 attempted handgun purchases[2] per day by Los Angeles residents[3] during this period, with one to two purchases denied per day on average (or 1.2 percent). On average, approximately 218 letters were sent per working day and approximately three (or 1.5 percent) per day were returned as undeliverable.

Table 2.1. Summary Statistics of Handgun Purchases and Letters, March 2014–September 2015

Purchase and Program Letter Metrics	Mean	Standard Deviation
Handgun purchases per day	124.9	45.0
Handgun purchases denied per day	1.5	1.6
Letters sent per working day	217.7	173.6
Letters returned per working day	3.3	
Number of purchase days	544	
Number of working days[a]	310	

[a] On average, approximately four days per week were worked on the program, accounting for holidays, weekends, vacations, and any days not worked on the letter program.

In terms of the trend in purchases over time, Figure 2.1 shows that the number of attempted purchases each day was relatively stable. However, there was a large spike in purchases on November 28, 2014, the day after Thanksgiving (also known as "Black Friday" and the largest shopping day of the year) when 666 handguns were purchased. This was followed by a steady increase until Christmas, when the number of attempted purchases returned to stable levels. Although this was only a subset of days during the citywide letter program,[4] it demonstrated the scale of handgun purchases in Los Angeles and the purchasing pattern during the intervention.

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[2] Attempted purchases refer to every purchase, including those that are denied during the ten-day waiting period.

[3] More specifically, we are referring to people living in a zip code in the city of Los Angeles.

[4] Ideally, we could observe and test purchasing patterns before and after the intervention, but these data were collected during the intervention only.

Figure 2.1. Number of Daily Attempted Handgun Purchases by Los Angeles Residents (March 7, 2014–September 1, 2015)
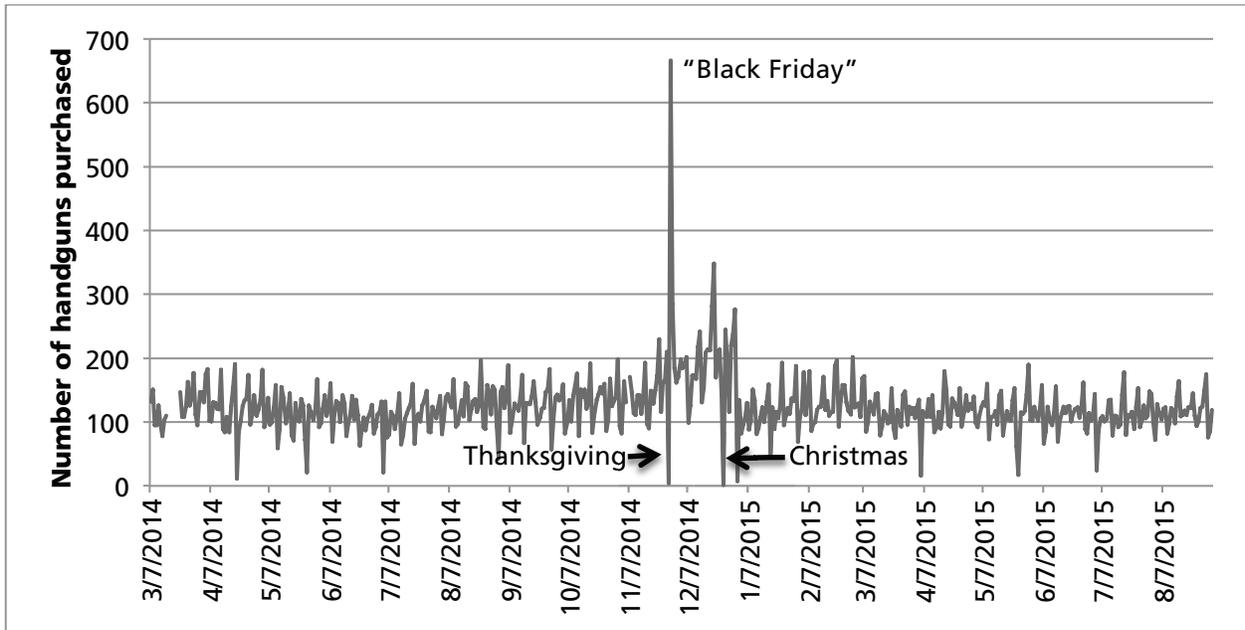
NOTE: The six days of missing data in March 2014 is due to a technical issue.

3. Gun Crime in the City of Los Angeles

In the city of Los Angeles, between January 2001 and December 2015, an average of 755 violent crimes with a firearm (murder, robbery, and aggravated assault) were reported to police per month (FBI and U.S. Department of Justice, 2014b and 2014a). The bulk of these offenses, 96.5 percent, were robberies (359.5 per month) and aggravated assaults (369.23 per month).

Figure 3.1 shows the total monthly firearm crime rate in Los Angeles decreased between 2002 and 2013 but leveled off or increased slightly in 2014. The intervention was fully under way in January 2013 (vertical line), which appears to be before the increase. One possibility is that the letter program slowed the decline of firearm crime. Another possibility is that the growth in firearm crime in 2014 and 2015 would have been even greater without the letter. Our analyses examine whether the introduction of the gun letter intervention affected the trend in gun crimes in Los Angeles, for better or worse.

Figure 3.1. Trend in Total Crimes per Month Committed with a Firearm per 1,000 in the Population of Los Angeles, 2001–2015
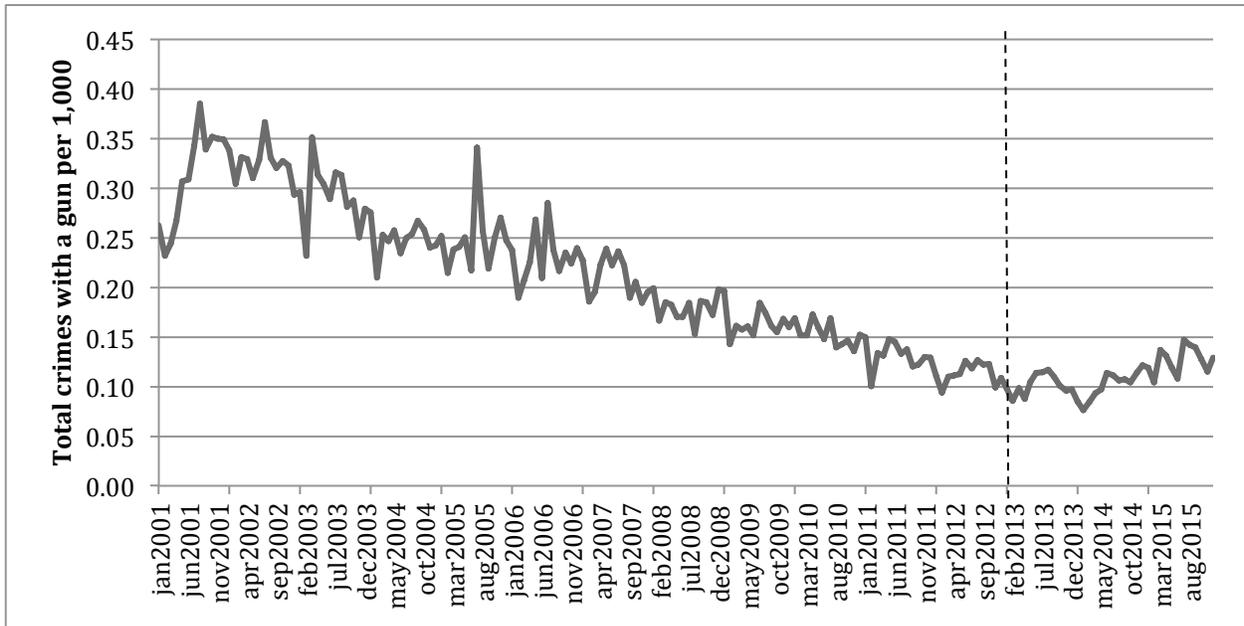
NOTE: Dashed vertical line signifies start of the letter program.

4. How We Studied Effects on Gun Crime and Program Costs

Effect on Crime Methodology

This impact study differs from the earlier pilot study of letters and gun crime (Ridgeway et al., 2011) by focusing on the wider city-level implementation and by using the full scope of publically available data on types of reported gun crime.[5] Our study used city-level data on homicides in the FBI's Supplementary Homicide Reports (FBI and U.S. Department of Justice, 2014b) and reported robberies and aggravated assaults with a firearm in the Uniform Crime Reports (FBI and U.S. Department of Justice, 2014a). We used an interrupted time-series analysis (ITSA) and synthetic control approach exploiting the timing and location of the letter program to identify its effect on gun crime rates (full methodological description in the Appendix).

First, we conducted an ITSA with only a single-unit group to test whether the letter program "interrupted" the gun crime time series in Los Angeles. We used a monthly data set starting in October 2008 and ending in December 2015, and tested whether there was an interruption to the time series starting in January 2013 (when the letter program began). While there were available data between 2001 and September 2008, we did not use months prior to October 2008 because of concerns that the prior pilot study might affect our analysis. And, we extended the analysis beyond the date of the last letter sent (September 2015) because the intervention could have had lasting effect through December 2015.

Next, we estimated the difference in gun crime between Los Angeles and a synthetic control. We constructed the synthetic control using a weighted set of U.S. cities with populations over 500,000 in at least one year since 1980 (control group) on a defined set of preintervention outcomes. The synthetic control method (SCM) used monthly data from 2001 to 2014, where the preintervention matching period was from January 2001 to December 2012. Unlike the ITSA, we used data prior to the 2007–2008 pilot study because we were matching Los Angeles to a control group of other cities. The pilot does not affect the results because by construction, the comparison cities had similar gun crime rates to Los Angeles during the pilot period; therefore, the pilot cannot affect results in this approach. We studied only through December 2014 because monthly gun crime data were not available for other cities (or agencies) when we started this study. However, these data were available for Los Angeles, which allowed us to conduct the ITSA through 2015. Most studies using a synthetic control approach examine the effects of health policies (Abadie, Diamond, and Hainmueller, 2010; Bauhoff, 2014; Kreif et al., 2015) or economic policies and outcomes, (Billmeier and Nannicini, 2013; Coffman and Noy, 2012;

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[5] The pilot study relied on guns recovered, and it can take some time to recover a gun that was previously used in a crime.

Hope, 2016), but there is increasingly more criminal justice–related literature using the approach (Saunders et al., 2015; Wright, 2016).

Program Cost Methodology

A second major component of this study focused on the costs and benefits to taxpayers of operating a letter program. The activities driving costs followed the process shown in Figure 4.1. The initiation of costs took place when the Los Angeles City Attorney's Office ran the daily gun report, which is used to identify the contact details needed for the letters. We included the costs of activities throughout the process until the sending of letters, as well as picking up returned letters and responding to calls about the program. For each activity throughout the process, we interviewed staff to determine the direct costs (labor time spent and the equipment used). Additionally, we obtained documentation of indirect costs (overhead expenses) associated to the Los Angeles City Attorney's Office (Simpson and Simpson, 2015). The information on direct and indirect costs was used to calculate the cost per day and per letter. While the development costs to set up a program can be a significant amount of time and take resources away from a City Attorney's Office, our research was not under way until many months later and we cannot reliably identify the planning resources used for this program.

Figure 4.1. Process of Letter Program Included in Cost Analysis
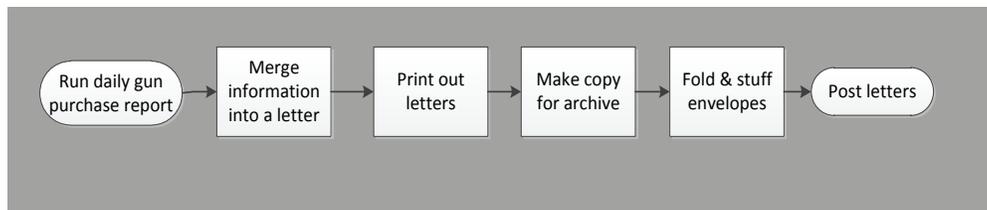
NOTE: We also include handling letters returned to sender and responding to calls about the program.

There are two general approaches to processing information, a top-down and a bottom-up approach, that are used in a variety of fields including examining policy implementation (Sabatier, 1986). In estimating the cost of a program, a top-down approach breaks down total expenditures into a relevant unit (e.g., total labor and equipment costs) using shares that could be attributed to costs of the program. A bottom-up approach to estimating the cost of a program identifies each of the resources expended to produce the output of the program (e.g., number of hours spent sending letters), applies a relevant monetary value (e.g., hourly pay by job type), and aggregates them to generate the additional cost of providing services of the program. Because we had data on the resources used daily for the letter program, this study employed the latter approach. While the first full month of implementation of the program was January 2013, full documentation needed for this benefit-cost study started in March 2014, and thus we begin the analysis then.

Applying a bottom-up approach to calculate costs means that we used the process map shown above and (1) identified each of the resources expended for the implementation activities of the letter program (e.g., total number of hours spent for each person working the program), (2) applied a relevant monetary value (e.g., hourly pay by job type, overhead rate), and then (3) aggregated the costs to generate the additional cost of providing the letter-delivering program. Our cost estimates included: labor costs of those involved in the letter program, overhead costs, and the direct equipment costs (e.g., paper, envelopes). It is important to note that the Los Angeles City Attorney's Office used in-house contracted services and resources. There were no additional costs to taxpayers in the form of higher taxes to hire someone or purchase specific software, for example. Therefore, results of this cost study can be understood as the amount of resources shifted away from other work they would have been doing toward this firearm letter program.

We conducted two interviews with the Los Angeles City Attorney's Office to identify the resources used in each activity. These interviews allowed us to identify the time spent by anyone involved in the program. There were three different stages of implementation or cost structures during this study that could be important to policymakers for planning a similar program, so we present costs separately for each. Additionally, within each stage, there are two features that define a stage: automation of the letter production and contracting in-house clerical services to stuff and post envelopes. For the letter production, the process for adding names and addresses to letters and envelopes was initially performed manually. Once the process was automated with a mail merge,[6] far less time was spent preparing the letters. A relatively higher-ranked employee, rather than clerical staff, prepared the envelopes (e.g., stuffing the letters). Eventually, this task was outsourced to in-house contractors. We calculated the cost of the three implementation stages separately:

- **Stage 1** (Low Automation, Clerical Work Not Contracted; March 7 to March 15, 2014) refers to the costs when a Los Angeles City Attorney's Office employee performed all tasks, including pulling gun-specific data for the letter, preparing addresses and names for the letters and envelopes, stuffing envelopes, and sending the letters.
- **Stage 2** (High Automation, Clerical Work Not Contracted; March 18, 2014, to June 5, 2015) involves the period when a Los Angeles City Attorney's Office employee used a mail merge to prepare letters, stuff envelopes, and send the letters.
- **Stage 3** (High Automation, Clerical Contracted; June 6, 2015, to September 1, 2015) involves a Los Angeles City Attorney's Office employee preparing the letters using the mail merge; and the stuffing of envelopes and mailing of the letters was transferred to an in-house contractor, rather than a Los Angeles City Attorney's Office salaried employee performing this portion of the work.

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[6] A *mail merge* is an automatic addition of names and addresses from a database to letters and envelopes to facilitate sending mail to many addresses.

Regarding the calculation of labor costs, interviews indicated there was one person who performed all the necessary tasks. The person conducting all the tasks was relatively higher ranked, so no senior management or oversight was needed (we later discuss the reasons for and implications of this program staffing). We applied the average hourly pay (including benefits) in 2013 for government paralegal and legal assistants in the Los Angeles–Long Beach–Santa Ana area of $21.61 (Bureau of Labor Statistics, 2014). This was then multiplied by the overhead rate of the Los Angeles City Attorney's Office, 26 percent, to take into account any administrative and supply costs associated with the program, such as utilities, computers, software, telephone, and others. (Simpson and Simpson, 2015).

Regarding direct equipment costs, the interview revealed a number of supplies used specifically for the intervention. These included paper, envelopes, printer ink, and postage. Although the supply costs were not readily available for each of these items, we were advised that paper and envelopes were bought in bulk, so we used estimates online of $0.06 per page and $0.02 per envelope (Office Depot, undated). The cost of ink per page, $0.10, is based on estimates of the price of black ink for a page in the literature (Riofrio, 2010). We use the standard price for a stamp for a first-class letter in 2014, $0.49. For Stage 3 (High Automation/Clerical Work Contracted), we use the fixed contract amount per day of $66. Another cost was identified—a dedicated telephone line—for people to call in with any questions or concerns about the program. While we could not determine with certainty how much the extra line cost, it is also potentially not a useful cost to include.[7] We do provide a more in-depth discussion later about the impact of the telephone line on staff (due to concerned calls).

Since the number of purchases varied from day to day, we calculate the cost for days when there were fewer letters sent (10th percentile number of letters, *Low*), days with the typical number of letters (mean number of letters, *Usual*), and days when there were more letters than usual (90th percentile number of letters, *High*). Formally, for each stage, we estimate the cost per letter as the following:

$$\textit{Cost per letter per stage} = \frac{\textit{(Hours spent per day in each stage*\$21.61)*(1+0.26)}}{\textit{Number of letters sent per day in each stage}} + (\$0.67).$$

We also multiply this cost per letter by the average number of letters per day in each stage to generate a cost per day.

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[7] It is not legally necessary and the calls that were received were more relevant for police than prosecutors.

5. Costs of the Gun Letter Intervention

The previous chapter describes three distinct cost stages in which the costs may have varied per day and per letter. Furthermore, as previously discussed, the number of purchases varied from day to day, so we calculated the Low, Usual, and High costs for each stage. Our approach of breaking down the costs into Low, Usual, and High for each stage allows us to provide an estimated range with enough detail that would allow decisionmakers to plan resource needs.

Time Resources Spent

Figure 5.1 shows that the time spent for the key personnel during the first stage was seven to 11 hours per day, which then fell to 3.5 to six hours and 2.5 to four hours for the second and third stages, respectively. During low days, the total time spent ranged from 2.5 hours to seven hours across stages. The time spent increases from 4.5 to 11 hours across stages when there were relatively more letters sent (High). Dividing the number of hours spent by the number of letters sent, the typical amount of time spent per letter ranges from slightly less than one minute (Stage 3) to just over two minutes (Stage 1). In other words, the implementation process of the first stage took more than double the time per letter than the final process.

Figure 5.1. Daily Hours Spent by City Attorney's Office on the Letter Program
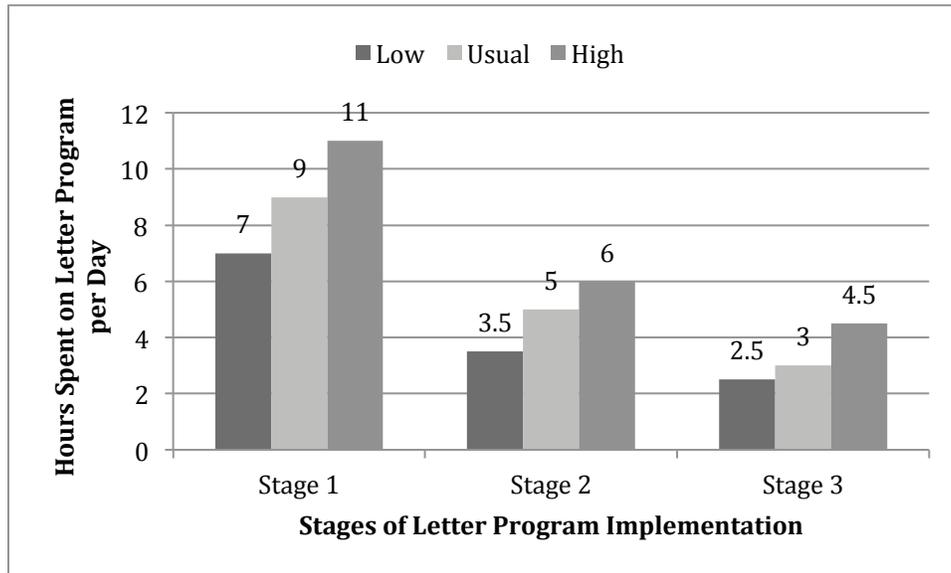
NOTE: Stage1: Low Automation/Clerical Work Not Contracted; Stage 2: High Automation/Clerical Work Not Contracted; Stage 3: High Automation/Clerical Contracted.

Costs per Day and per Letter

Results in Table 5.1 show that the cost of the program per day ranges from approximately $145 to $428 per day worked, depending on the stage and number of letters sent. Stage 1 was the most costly because this was the most labor-intensive stage. The change from Stage 1 to Stage 2 was the addition of a mail merge, which resulted in a 29-percent to 47-percent decrease in costs (depending on the number of letters sent). Then, contracting out the task of stuffing envelopes and posting letters that occurred between Stage 2 and Stage 3 resulted in cost increases of 4 percent to 28 percent. As noted earlier, this allowed the Los Angeles City Attorney's Office staff to spend more time on other activities (e.g., cases). Therefore, Stage 3 was probably more cost-effective to taxpayers and a better use of Los Angeles City Attorney's Office resources.

The cost of the program was approximately $1.13 to $1.85 per letter. We find no variation in the cost per letter in Stage 1. This is because the labor resources are the same per letter, and there are no economies of scale. That is, the amount of work for staff was the same for each letter during this stage, so while sending more letters increases the total day cost from $304 to $478, the cost per letter does not increase or decrease. The cost-reduction benefits of the mail merge are observed in Stage 2, with a cost of $161 to $327 as more letters went out.

Comparing the results of Stage 2 and Stage 3, we can further understand the trade-offs in resources used for a letter program. On the one hand, while the cost per letter for Stage 2 and Stage 3 are similar when fewer letters are sent (Low), the change in cost increases at a higher rate for Stage 3 than Stage 2 when more letters are sent out (Usual and High). This is largely driven by the fact that Stage 3 includes a fixed outsourcing cost, so when there are fewer letters sent, the outsourcing cost per letter is relatively high. On the other hand, this study does not include the opportunity costs, and although the costs per letter of Stage 3 are higher when fewer letters are sent, we might expect that it is a better use of resources not to have Los Angeles City Attorney's Office employees stuffing envelopes or posting letters, but rather working on other job duties. A more detailed discussion of the opportunity costs and trade-offs is presented in a subsequent section.

Table 5.1. Costs of Operating Letter Program

	Low (Fewer Letters Sent)	Usual (Average Letters Sent)	High (More Letters Sent)
Cost per day			
Stage 1	$304	$390	$478
Stage 2	$161	$266	$327
Stage 3	$197	$266	$395
Cost per letter			
Stage 1	$1.58	$1.58	$1.58
Stage 2	$1.45	$1.21	$1.18
Stage 3	$1.85	$1.33	$1.13

NOTE: Stage1 = Low Automation/Clerical Work Not Contracted; Stage 2 = High Automation/Clerical Work Not Contracted; Stage 3 = High Automation/Clerical Contracted.

Nonmonetary Program Costs and Benefits Also Matter

Although not legally required, the Los Angeles City Attorney's Office set up a phone line to address questions and concerns from the public. There were members of the public, mostly buyers but sometimes family members or city residents, concerned by the letter and the Los Angeles City Attorney's Office received a good deal of phone calls. For example, one reason for concerned calls described by the Los Angeles City Attorney's Office was related to zip codes and jurisdiction. Individuals were concerned about whether the Los Angeles City Attorney's Office had jurisdiction to send them a letter when part of their zip codes were within Los Angeles but they do not reside in Los Angeles. In addition to nonmonetary costs to some members of the public, these calls also affected the people who had to handle these calls.

In some cases, callers indicated they benefited from the letters in unexpected ways. For example, the Los Angeles City Attorney's Office reported that an unrecorded number of calls came from individuals who only learned that someone in the household had purchased a gun when the letter arrived. While some of these people were concerned about safety in their household, others were concerned about the health of a (suicidal) family member. As such, the letters may have had an unintended effect of facilitating interventions that could have reduced violence or self-harm. At the same time, however, the individuals who opened another person's mail may have violated the privacy of gun buyers in these cases. We are not able to quantify these costs and benefits to the telephone line for the Los Angeles City Attorney's Office and the public, but it is clearly an important aspect to consider.

Opportunity Costs of the Program

While a key staff member was working on the letter program, he or she could not work on other tasks in the Los Angeles City Attorney's Office. Because we were not able to identify the alternative tasks, and thus net benefit, postponed because of the letter program, we describe them here. During Stage 1 of the program, there was a substantial amount of time spent on clerical activities, as well as oversight or management of the program. The activities this staff member would have otherwise performed had to be shifted to other staff members or completed on days when there were fewer letters, which may have caused delays and caused staff to be overworked (which is costly). Given the seniority of the staff member working on the letter program, the other activities postponed were potentially high-value activities. In Stage 2, this staff member was able to spend far less time on clerical work, although some of his or her time was still spent on clerical activities. This means that the opportunity cost fell because this staff member had more time to work on his or her other important activities. Finally, in Stage 3, the staff member had even less clerical work to do because it was contracted. Although the costs of Stage 2 and Stage 3 were similar, the relatively senior staff member only spent program time on oversight and management, and opportunity costs were minimized in Stage 3.

6. Lessons on the Impacts of the Letter Program

The Letter's Effects on Gun Crime Are Uncertain

We started by using the ITSA method to test whether there were significant changes in the rate of gun crime at the time the letters were sent (for all results, see the Appendix). Theoretically, we might expect that the letters would have only a short-term effect on behavior (i.e., the letter is no longer salient months later). For example, because of the receipt of the letter, some individuals may be discouraged from transferring the handgun to a prohibited possessor, but they might reconsider later when the letter's message is no longer as easily recalled. Under this short-term effect model, we might expect the effects of the program on crime to be fairly constant over time: A steady state might quickly be achieved of new buyers who are currently influenced by the letter. Alternatively, if the letter has long-term effects, the effect of the program on violent crime might be expected to build gradually as more and more handgun buyers receive the letter and behavior is influenced by it. We tested both the long-term and the short-term models for the letter's effect on crimes in the city with alternative ITSA specifications.

For the short-term model, we found an immediate decline in robberies committed with firearms after implementation of the letter program, but no such changes in aggravated assaults or homicides with a firearm (Table A.2 in the Appendix). Although robberies committed with firearms declined around the same time as the start of the letter intervention, other contemporaneous influences may have had a greater effect than the letter program. Because we cannot rule out this possibility, we do not consider this finding particularly strong or compelling evidence for the effectiveness of the program.

Using the long-term model, we found no statistically significant effect on robberies committed with firearms, but a statistically significant decrease of homicides with a handgun and aggravated assault with a firearm. The fact that these results are so sensitive to the model specification is concerning and an indication that results are not robust.

We then used the stronger SCM of comparing Los Angeles with a weighted group of U.S. cities with populations over 500,000. The synthetic control should have similar gun crime trends to Los Angeles but does not get the letter intervention, which would yield a viable comparison group. Despite a variety of different model specifications, we could not get a high-quality match between Los Angeles and the synthetic control, which was needed to identify the effect of the letter. To illustrate this, Figure 6.1 shows total crimes with a gun for Los Angeles and the optimal synthetic control we identified, with a red line drawn at the time of the intervention. For this to be considered a good match, the black solid line and the black dashed line should be very close to each other. There is virtually no guidance in the literature on what is considered "close,"

so it is not possible to determine whether this is a good match. It is clear, however, that the crime trend in Los Angeles and the synthetic control diverges before the letter program takes effect, as the two lines are not equivalent to each other. Because we do not have a good fit between Los Angeles and the synthetic control in the preintervention period, any results of the effect of the letter would be statistically biased and we do not conduct inference analyses.

Figure 6.1. Trend in Total Rate of Crimes Committed with a Gun for Los Angeles and Synthetic Control, January 2001–December 2014
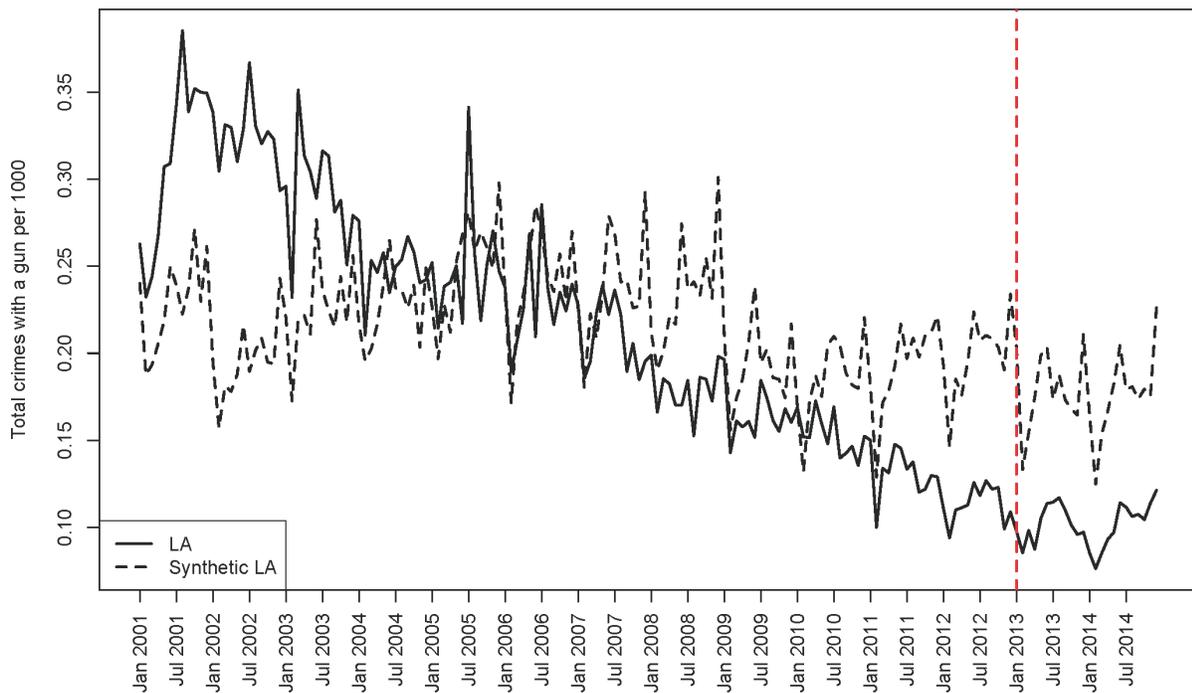
Studies Should Analyze Impacts at the Individual, Not City, Level

Using two statistical approaches and a series of robustness checks, we were unable to detect a reliable citywide effect for the letter program on homicides, robberies, or aggravated assaults with a firearm (see Appendix for statistical results). While it may be the case that the program indeed had no effect on gun crime, it is also possible that we simply do not have the statistical power required to detect the effect on aggregate behavior. On the one hand, if the number of gun crimes committed by people who obtained a handgun through an initially legal purchase at a licensed dealer during the intervention period is a small fraction of all gun crimes, the size of the effect of the letter needed to be very large (i.e., prevent every single transfer that would eventually result in a gun crime) for the analyses to identify the effect at an aggregate level after less than two years of issuing letters. On the other hand, research has demonstrated that a low number of households purchase a relatively large proportion of sold firearms (Hepburn et al.,

2007). If this is the case, then the average of nearly 125 handgun purchases per day by residents of Los Angeles could be composed largely of repeat buyers. This would imply that the letters actually went to a substantial proportion of the firearm-purchasing population, in which case we have a real result of no statistically significant effect.

Although not available to us, a better approach than analyzing aggregate, city-level data would have been to conduct tests that are more sensitive to the effect of the letter intervention. This could be done in a number of ways if the necessary data were available. First, we would recommend an analysis based on gun-level data (where the unit of observation is each gun purchase), which would allow for a statistically stronger evaluation of whether the letter has an impact on the likelihood a gun is reported lost, stolen, or recovered by police. Moreover, researchers could also use these data to test for whether there are differences by subgroups based on characteristics of the gun purchase (e.g., make, model, day of week). A second recommendation would be to study the data over a longer period with a long follow-up for testing the effects on crime, because it takes time for a gun to be used and to be recovered. Lastly, a potentially fruitful avenue of research would be to interview gun dealers and buyers to get to their observations of the effects of the letter.

The City of Los Angeles May Be Very Different from Other Cities

The key criterion for credibility of a synthetic control analysis is a good (if not perfect) fit between the treated unit (Los Angeles) and the synthetic control. As described earlier, it was not possible to achieve a good fit in our analysis. We tried a number of different ideas to improve the matching (adding cities in California of population 250,000 to 500,000, changing the variables used for the matching, changing the frequency of the data, using a natural logarithm outcome variable), but our robustness checks in the end always show that we cannot have confidence in the results of comparing our control group with Los Angeles (see the Appendix). This raises an interesting question: Why? We can see that the mean values of all the matching variables are similar between Los Angeles and a synthetic control (see Table A.4 in the Appendix), yet the two groups are not a good fit (Figure 6.1). The most likely explanation is that the means between two groups are similar but the underlying distributions of the variables are different. In other words, we can see that crime was falling fairly dramatically in Los Angeles in the preintervention period (Figure 3.1), and it may be that most cities do not have firearm crime rates high enough to experience such a downward trend. If no other cities experienced a similar downward trend in firearm crime of the magnitude seen in Los Angeles, then this would lead to poor matches between Los Angeles and a mix of other cities. While the SCM can match the overall *mean* in the preintervention period, the matches may be poor in any given year because crime rates for the synthetic control are lower than Los Angeles (when crime rate is high in Los Angeles) or higher than Los Angeles (when the crime rate is low in Los Angeles).

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In conducting the additional analyses to improve the match, we discovered a gap in research regarding if and how to improve matches. Therefore, we are conducting a study to support researchers using SCM in settings in which it may be difficult or impossible to identify a perfect match, with an application to firearm data.

Studies Should Take into Account Where Guns Are Transferred

One limitation of our methodology is that people could have transferred guns to individuals who committed gun crimes outside of Los Angeles, thus contaminating our control group. Such a situation would mean that the letter could have lowered gun crime rates in both Los Angeles and the synthetic control, which is why we found no statistically significant difference between the two. That said, using their Firearms Trace Pattern Analysis workstation software tool, Ridgeway et al. (2008) indicate that when the firearm possessor was not the original purchaser, the geographic distance between the two was quite small. In more than one-third of all such instances, the original legal purchaser resided within 4.5 miles of the neighborhood boundary of the crime. Based on its investigative experience, the interagency working group suggested that the local nature of the market was driven by prohibited possessors who were having local friends or family members conduct straw purchases for them (Ridgeway et al., 2011). Therefore, it is unlikely that transferring guns outside Los Angeles would be a large driver of our results.

7. Can the Program Benefits Outweigh the Costs?

This study did not detect a great effect on the level of gun crime in the city of Los Angeles. However, even an effect too minor to detect using our methods might justify the gun letter program. Given that the cost of the program is fairly low, it is possible to see how even minor effects could easily result in the benefits outweighing the costs to the community. To demonstrate this, we use our finding for the usual cost per day in Stage 3 ($266 per day) and multiply by the number of working days on the program in the year (shown earlier to be 208 days per year) to generate the total cost of the program per year of $55,328. When we compare this cost with the benefits to society of crime prevention (Table 7.1), it is clear that if this letter prevents one homicide or suicide, then it provides net positive benefits to society (benefit-cost ratio of $156.33:$1). Perhaps more interesting is the implication regarding the crimes of robbery or aggravated assault, as the costs and benefits are slightly better than break-even if the letter program prevents one robbery ($67,277 benefit versus $55,328 cost). Therefore, the program would need to prevent at least one robbery per year to generate a positive return on investment (if it prevents no other crimes).

Because the program is implemented through a City Attorney's Office, there may be interest in the implications for the judicial and legal system in particular. If we focus only on the judicial and legal system costs, we can see that at least two homicides per year need to be prevented, which would be a 0.5-percent decrease in homicides in Los Angeles, to ensure a positive net value of the program. For robberies, because the benefit in the California judicial and legal system for preventing one reported robbery is between $1,044 and $2,176 on average, the program would need to prevent 25 to 53 reported robberies per year (if the program prevents no other crimes and we consider no other benefits to society). That would be a 1-percent reduction in the annual number of reported robberies in Los Angeles.

To compare the benefit-cost potential of this program with other programs, we examined the benefit-cost ratios of adult criminal justice programs using the Washington State Institute for Public Policy Benefit-Cost Tool (Washington State Institute for Public Policy, 2016). The top adult (nonprison) program in terms of the greatest benefit-cost ratio and low cost per person is cognitive behavioral therapy (CBT) for moderate- to high-risk offenders, with a ratio of $24.19:$1 (benefits of $10,483 in crime prevention and costs of $433 per participant on average in Washington state). Put differently, for every $1 spent on CBT, society receives $24.19 in benefits. Given that the Los Angeles City Attorney's Office letter program cost $55,328 per year, the program would be equivalent to the CBT program if there were a $1,338,384 benefit to society per year on average in crime reduction (i.e., we would achieve the same $24.19 benefit-cost ratio). Using the findings shown in Table 7.1, this amounts to the prevention of approximately 19 robberies, 15 aggravated assaults, or fewer than one homicide per year.

There are two aspects of these results to consider. First, an implication of this result is that the letter program is not as useful when comparing the trade-offs to other programs, such as CBT, because it is not as effective as other programs. Second, the letter program costs far less than other programs and can be delivered to every gun purchaser (from a gun seller with a Federal Firearms License) for low cost. The letter program costs less than $2 per person, whereas CBT, the lowest-cost program available (with a net benefit to society), costs $433 per person. This is a very important result to consider because programs like CBT may not be available to Attorney's Offices given the costs. For approximately $55,000, an Attorney's Office was able to deliver information to all purchasers in a year. Far fewer people would be "treated" with other programs.

Table 7.1. Benefits of Reducing Crime, by Crime Type

Crime	Judicial/Legal System Cost per Crime, California	Economic and Social Cost per Crime to Victims and Society, Nationally
Homicide	$42,897–$78,698	$8,649,216
Robbery	$1,044–$2,176	$67,277
Aggravated assault	$2,573–$4,847	$87,238

SOURCE: Judicial System Cost: Hunt, Anderson, and Saunders (2016); Economic and Social Costs: Heaton (2010).

Appendix

Letter

Figure A.1. Example Letter

Office of the Attorney General for the State of California
City of Los Angeles
Los Angeles Police Department

200 North Main Street, Suite 966, Los Angeles, California 90012
(213) 978-8100

March 17, 2014

DROS #

Purchaser Name
Purchaser Address
Purchaser City, State & Zip Code

Dear Purchaser:

As you are aware, gun violence is a serious problem both within the City of Los Angeles and our country. Nationwide, thousands of our fellow Americans, including children, are killed or seriously injured each year by firearms. It is therefore the hope and intent of the City and the Attorney General to pursue measures that help reduce those deaths and injuries.

To that end, City prosecutors, in conjunction with the California Attorney General and Los Angeles Police Department, are engaged in a program to remind gun purchasers of their legal responsibilities as gun owners.

Records show that you have recently purchased a firearm. It is important that everyone does their part to handle and store firearms in a safe manner, including keeping them out of the hands of children, criminals, and others who may not be authorized to own or possess such a firearm.

In the event you decide to sell or give your gun to another person, both parties must first complete a "Dealer Record of Sale" (DROS) form at any federally-licensed gun dealer. Please remember that, with very few exceptions, it is a crime to transfer a firearm to any person without first completing the DROS form. Additionally, it is a crime to knowingly sell, give or allow possession of a firearm to a person with a known mental disorder. Furthermore, should a child obtain access to your firearm and injure him/herself or another person, you could be subject to criminal prosecution. City prosecutors are also authorized to bring an eviction action against the tenants residing at a property at which certain unlawful conduct takes place, including the illegal possession, use, sale, furnishing or giving away of a firearm.

You should also be aware that in the event the police recover a firearm that has been involved in a crime, City prosecutors can prosecute its previous owner for a misdemeanor, if that owner failed to complete the "Dealer Record of Sale" form.

Please help make Los Angeles a safer community by preventing your gun from falling into the wrong hands. Thank you for your compliance with these very important obligations and responsibilities.

Any inquiries may be directed to the City prosecutor at (213) 978-8192.

Data

Table A.1. Definition and Sources of Data

Crime	Description	Data Source
Reported crime with a firearm		
Murder with a firearm	Total number of known non-negligent murder and justifiable homicide events committed with a handgun, rifle, shotgun, or any other type of gun or firearm	FBI and U.S. Department of Justice, 2014b
Murder with a handgun	Total number of known non-negligent murder and justifiable homicide events committed with a handgun	FBI and U.S. Department of Justice, 2014b
Robbery with a firearm	Total number of known robbery offenses that were committed with any type of firearm; unlike burglaries, robberies presume a victim who was hurt or threatened during the theft	FBI and U.S. Department of Justice, 2014a
Aggravated assault with a firearm	Total number of known aggravated assault offenses that were committed with any type of firearm; examples include attempted murder and threatening the victim	FBI and U.S. Department of Justice, 2014a
Reported crime		
Homicide	Total number of known non-negligent murder and justifiable homicide events reported to police	FBI and U.S. Department of Justice, 2014b
Manslaughter	Total number of known murders that were found to be as a result of involuntary manslaughter	FBI and U.S. Department of Justice, 2014b
Aggravated assault	Total number of known aggravated assault; aggravated assault is distinguished from regular assault by whether there is a weapon involved	FBI and U.S. Department of Justice, 2014a
Robbery	Total number of known aggravated assault or robbery offenses	FBI and U.S. Department of Justice, 2014a

Description of Methodologies

This study applied two approaches—an ITSA and SCM. Beyond the differences in these empirical models, there are two important differences to point out. First, the two methods in this report used different data sources. The ITSA used a monthly data set—October 2008 to December 2015—for Los Angeles only. We did not use months prior to October 2008 because of concerns that the prior pilot test may affect our analysis. The SCM used monthly data from January 2001 to December 2014 for Los Angeles and all other U.S. cities with populations over 500,000 (in at least one year since 1980). While 2015 data were available for Los Angeles, they were not available for all cities with populations over 500,000, which was why 2015 data were

used in the ITSA and not the SCM. We argue that the pilot study does not contaminate this analysis because the comparison cities, by construction of the model, are outside of Los Angeles with similar gun crime rates during the pilot period. Second, the primary ITSA models the outcome as a count (negative binomial), while the SCM models the outcome as a rate (crime count per 1,000 population). This is because SCM only exists for linear models, not negative binomial models. For all SCM analyses in this study, the package Synth in R Version 3.3.2, based on the Abadie, Hainmeuller, and Diamond (2010) method, was used.

Interrupted Time-Series Analysis

We started by using an ITSA with only Los Angeles to understand whether there were any changes in the average crime rates before and after the letter program; and if so, whether there was an abrupt and lasting change on the crime rate and/or gradual effect over time. Using the ITSA method, we conducted three types of analyses. First, we applied a traditional ITSA to test the effect of the intervention on the level of the crime rate after the intervention (using a pre-post dummy variable) and on the trend of the crime rate (interacting a pre-post dummy variable with a variable representing the time since the start of the data) (Linden, 2015). We estimated a linear regression with the outcome of number of crimes per 1,000 in the population, and included a first-order autocorrelation term and seasonality indicator (Model I).

However, because (1) there was evidence of some biases associated with this model (Davidson and MacKinnon, 1992) and (2) recent simulation work (Schell and Griffin, 2017) has shown potential issues with bias, type I error rates, and power with this model (specifically the use of the rate as the outcome), we also tested another approach of estimating the effect on the changes in crime rates using the count outcome and first differences. Specifically, our outcome is a count variable and is estimated using a negative binomial regression. Our primary regression model assumes the following form (Huitema and Mckean, 2000; Linden, 2015):

$$Y_t = \alpha + \delta M_t + \theta D_t + \beta X_t + \mu A_t + \varepsilon_t, \tag{1}$$

where Y_t is the count of a particular type of crime with a gun (robbery, assault, murder, or total crime) in Los Angeles at each month t (=October 2008, …, December 2015), M_t is the time from the first period of the study (=1,…, 87), D_t is a dummy variable representing the intervention coded using effect-level or first-differences coding (=0 in pre-intervention periods, 1 during the month of the intervention, 0 after the intervention), X_t is a set of monthly dummy indicators (one for each month of January through December) to account for seasonality, and A_t is an autocorrelation term equal to the log-transformed crime count in the month immediately prior to t to account for persistence in crime rates. Typically, the lagged outcome variable is labeled Y_{t-1}; however, because the lagged term must be log-transformed in the negative binomial model, we represent the term with an A. The model is fit using a negative binomial regression with an offset term equal to the log of the population in month t. For more on this specification, see also

Agresti (2002). Of particular interest is the statistical significance and sign of θ, which represents the change in the crime rate level immediately following launch of the letter program.

We then conducted a third analysis in which we allowed the program to affect not only the overall level of crime, but the crime trends as well. Formally, D_t becomes a continuous variable representing the fraction of the first year in which the new gun purchasing population is exposed to the letter (=0 in preintervention periods, 1/12 during the first 12 months of the intervention, 0 after the intervention). This allows for an increasing fraction of the gun owner population to be exposed to the letter. Furthermore, because it could be months before a gun might be used in a crime, the effect of the letter might occur months after initial receipt of the letter and thus affect the crime trend.

Results of ITSA

The results for the intervention effect coefficient for each examined outcome are shown in Table A.2. Note that the main body of this report mentioned our preferred specification of Model II and Model III. Regarding Model I, results indicated there were reductions in nearly all types of crimes, and yet an upward shift in the slope of crime over time. However, this approach is known to downwardly bias estimates (Davidson and MacKinnon, 1992) and recent simulation work has shown this approach can lead to spurious findings (Schell and Griffin, 2017), so it is unclear that this is a reliable result.

The difference models found results of a similar direction. Model II indicated that there was an immediate effect in the change of robberies with a firearm. When taking into consideration that it may have taken one year for the entire relevant population to receive treatment, Model III results indicated there might have been a decrease in homicides with a handgun and assaults with a firearm. Note that because Models II and III were estimated using a negative binomial model, the significant effect observed can be interpreted as a multiplicative change. For example, in Model II, the result for robberies was a change in the rate of *exp(0-.2344)=0.79* at the time of intervention compared with before the intervention. In other words, the average rate of robberies committed with a firearm after the intervention was 0.79 times less than prior to the intervention.

Table A.2. Impact of Letter Program on Los Angeles Time Series

| Crime	Model I: Level Model		Models II: Differences Model	Models III: Differences Model
	Effect on Level	Effect on Trend	Intervention Effect in First Month	Intervention Effect in First Year
Homicides with other firearm	−0.0011 (0.0016)	0.000 (0.000)	0.0166 (0.2815)	−0.8088 (0.9985)
Homicides with a handgun	−0.0045[b] (0.0016)	0.0001 (0.000)	−0.2038 (0.466)	−4.236[a] (1.6883)
Robberies with a firearm	−0.0517[b] (0.0103)	0.001[b] (0.0002)	−0.2344 [a] (0.1161)	−0.4755 (0.4259)
Assaults with a firearm	−0.0589[b] (0.0121)	0.001[b] (0.0002)	−0.0199 (0.1221)	−1.12[a] (0.4626)
Total crimes with a firearm	−0.0984[b] (0.0203)	0.0017[b] (0.0003)	−0.1063 (0.0921)	−0.6156 (0.3479)

NOTE: Standard errors in parentheses. Model I outcome is a rate; Models II and III outcomes are counts.
[a] $p<0.05$.
[b] $p<0.01$.

To visually demonstrate the implications of our results, in Figure A.2, we use statistically significant results from Model II and plot the predicted monthly *change* (difference in expected log counts comparing current month with previous month) in robberies committed with a firearm with the letter program (solid line) compared with the change in robberies committed with a firearm predicted without the letter program (dotted line). As Figure A.2 demonstrates, there was an effect on the monthly change in the first month of the letter—there was a greater reduction in robberies committed with a firearm with the letter intervention than if there had not been a letter program. The overlapping of the two lines for the rest of the postintervention period is due to our specification of the intervention effect in Model I being at the month of the intervention initiation (i.e., 00001000 described above with equation [1]). We remind the reader that this is a first-differences result, which means that because there was the initial drop in crime because of the letter, the number of robberies committed with a firearm remained lower in every month after the initial launch of the letter. However, because of how we constructed Model I, there were no changes in the trends.

**Figure A.2. Illustration of Model II Result for Robberies with a Firearm
(October 2008–December 2015)**
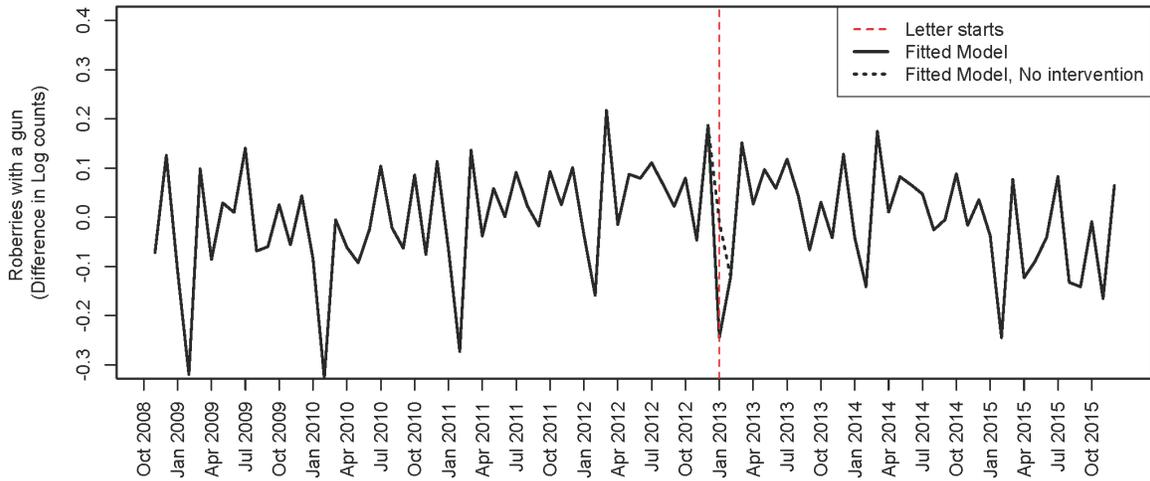
Synthetic Control for Comparative Case Studies

Although we do not identify any other criminal justice–related policy in Los Angeles at the same time the letters were introduced, we cannot rule out that an event outside the intervention may bias the ITSA treatment effect estimates (Linden, 2017). A more robust evaluation approach would be to use a comparison group as the counterfactual and thus remove changes unrelated to treatment in the time series observed in both the control and treatment group (Rubin, 1974, 2005). Given we have one treated unit (Los Angeles) and a number of potential control units (other large cities in California or outside of California), we apply the SCM procedure in Abadie, Diamond, and Hainmueller (2010) to generate a composite comparison group. This approach is a novel alternative to a more traditional difference-in-differences approach, which would require the crime trends in Los Angeles to be parallel to crime trends in the control cities prior to the intervention, as this assumption does not hold in our data.

The set of weights, W, defines the synthetic control by weighting each city making up the group, where the weights are based on data from 12 years prior to the intervention. Once units are matched with the weights, the characteristics of Los Angeles and synthetic control Los Angeles should be statistically comparable or "balanced." Balance in multiple preperiods for the outcome implies that the preintervention trends for the treatment and control districts are similar; thus addressing the parallel trends assumption of a difference-in-differences estimator. The algorithm of Abadie, Diamond, and Hainmueller (2010) produces weights that minimize imbalances between the synthetic and treatment cities on a specified set of covariate and outcome variables. Specifically, if we consider X_1 as a vector of preintervention characteristics of the exposed area and X_0 as the vector for untreated areas, a vector W is chosen to minimize $||X_1 - WX_0||$. Weights are restricted to be nonnegative and sum to one. Solving for the optimal

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weights involves an iterative numeric search that increases or decreases the value of *W* that offers the greatest decrease in the objective function. That is, the weights are manipulated so that at each step the weighted comparison cities look more and more like the treatment city.

We focused our analyses on cities in the United States with populations over 500,000 in at least one year since 1980. The data are for law enforcement agencies serving these city jurisdictions. This selection criterion of jurisdictions with population over 500,000 reduced missing data bias associated with smaller agencies. The 36 cities were Albuquerque, New Mexico; Atlanta, Georgia; Austin, Texas; Baltimore, Maryland; Boston, Massachusetts; Charlotte-Mecklenburg, North Carolina; Chicago, Illinois; Cleveland and Columbus, Ohio; Dallas, Texas; Denver, Colorado; Detroit, Michigan; El Paso and Fort Worth, Texas; Fresno, California; Honolulu, Hawaii; Houston, Texas; Indianapolis, Indiana; Jacksonville, Florida; Las Vegas, Nevada; Memphis, Tennessee; Milwaukee, Wisconsin; Nashville, Tennessee; New Orleans, Louisiana; New York City, New York; Oklahoma City, Oklahoma; Philadelphia, Pennsylvania; Phoenix, Arizona; Portland, Oregon; San Antonio, Texas; San Diego, San Francisco, and San Jose, California; Seattle, Washington; Tucson, Arizona; and Washington, D.C.

Given that subsequent research indicated that matching solely on preintervention outcomes provides a more efficient match in situations where overfitting is not a concern (Kaul et al., 2016), we matched based on the reported crime rate for homicide with a handgun, homicide with other firearm, aggravated assaults with a firearm, robberies with a firearm, and total crime with a firearm. Therefore, our matches included 720 variables (144 months × 5 variables).

Results of the Matching

First, in Table A.3, we show the estimated weights by city and outcome examined. While the weights vary slightly depending on the outcome, generally Milwaukee, Memphis, New Orleans, San Diego, and New York are frequent large contributors as measured by a positive weight. For those familiar with synthetic control weights, the subgroup of weights slightly greater than zero is perhaps surprising (instead of zeros for most cities and positive weights for a small subgroup of cities). We offer one explanation for this based on analyses we conducted using annual data.[8] For the annual analysis, we conducted SCM matching only on the preintervention outcomes for reasons described earlier. The weights assigned to cities in the annual analysis were similar to that shown in the main chapters of this report using monthly data; some weights were slightly above zero, and the rest of the weights were nontrivial. Because we also had city-level socioeconomic and demographic characteristics available at the annual level, we also conducted a synthetic control matching using preintervention outcomes *and* preintervention covariates in the annual analysis. Interestingly, the weights were unlike when only using preintervention outcomes and more similar to other reports: Weights were either zero or a small subset of cities received positive weights.

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[8] For clarity in reading this report, we do not present our findings from the annual analyses with and without covariates. Results are similar to those presented here and can be made available by authors upon request.

Table A.3. Synthetic Control Weights by City

City	Homicides with a Firearm	Homicides with a Handgun	Robberies with a Gun	Assaults with a Gun	Total Crimes with a Gun
Albuquerque	0.009	0.009	0.01	0.011	0.01
Atlanta	0.011	0.010	0.01	0.011	0.010
Austin	0.014	0.014	0.015	0.013	0.015
Baltimore	0.010	0.011	0.009	0.008	0.009
Boston	0.009	0.009	0.009	0.008	0.009
Charlotte-Mecklenburg	0.013	0.013	0.013	0.014	0.014
Cleveland	0.010	0.010	0.008	0.008	0.008
Columbus	0.009	0.009	0.009	0.009	0.009
Chicago	0.012	0.012	0.013	0.011	0.012
Dallas	0.018	0.018	0.024	0.020	0.023
Denver	0.009	0	0.008	0.008	0.008
Detroit	0	0	0.083	0.114	0.092
El Paso	0.012	0.012	0.013	0.013	0.013
Fort Worth	0.012	0.012	0.014	0.014	0.014
Fresno	0.018	0.019	0.024	0.021	0.023
Honolulu	0.012	0.012	0.006	0.004	0.005
Houston	0.011	0.012	0.012	0.012	0.012
Indianapolis	0.005	0.005	0.004	0.004	0.004
Jacksonville	0.009	0.010	0.008	0.009	0.009
Las Vegas	0.018	0.019	0	0	0.001
Memphis	0.056	0.051	0.162	0.163	0.164
Milwaukee	0.165	0.163	0.228	0.238	0.230
Nashville	0.019	0.020	0.016	0.014	0.016
New Orleans	0.140	0.138	0.045	0.037	0.042
New York	0.123	0.121	0.025	0.022	0.023
Oklahoma City	0.017	0.018	0.014	0.012	0.013
Philadelphia	0.013	0.012	0.017	0.016	0.017
Phoenix	0.019	0.02	0.018	0.016	0.017
Portland	0.005	0.005	0.005	0.006	0.005
San Antonio	0.026	0.030	0.052	0.044	0.050
San Diego	0.113	0.117	0.050	0.048	0.048
San Francisco	0.010	0.01	0.012	0.012	0.012
San Jose	0.017	0.018	0.014	0.014	0.014
Seattle	0.028	0.031	0.042	0.036	0.041
Tucson	0.018	0.018	0.006	0.009	0.007
Washington, D.C.	0.010	0.011	0.002	0	0

Once the weights, *W*, are obtained, the adequacy of the control cities to create a "synthetic Los Angeles" can be examined visually by plotting the preintervention outcome in the city of Los Angeles versus the weighted synthetic Los Angeles preintervention outcome over time. If these plotted lines are very close, this indicates a good match. The intervention effect is then defined as the difference between the city of Los Angeles and the synthetic control, at the time of intervention and beyond. This difference between the city of Los Angeles and the synthetic control is referred to as a "gap." Plotting the gap over time is referred to as a *gap plot*. An ideal gap plot would have a line drawn at zero for every time point before the intervention, and then a positive or negative gap at the time of intervention and beyond (if the intervention was effective). We show this result in Figure 6.1 of this report, which shows that the crime trend in Los Angeles and the synthetic group differ both before and after the letter program.

A similar way to examine this is by visually inspecting the gap plot, shown in Figure A.3 for total rate of crime committed with a gun. For this to be considered a good match, the black solid line would be at or very close to 0 for all time points before the intervention (red line). As we can see, the gap between Los Angeles and synthetic Los Angeles is not close to zero. Again, this is also the case for each crime type—homicide, aggravated assault, and robberies with a firearm.

Figure A.3. Difference Between City of Los Angeles and Synthetic Control for Total Crime Rate with a Gun (January 2001–December 2014)
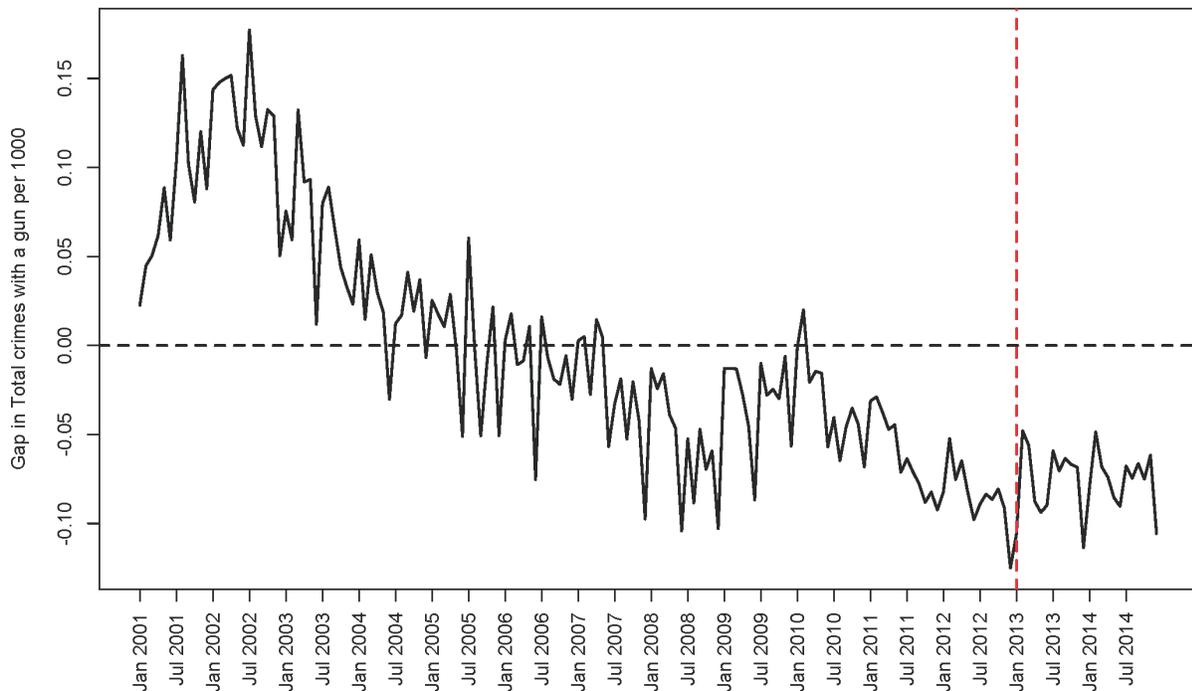
Another way to examine matching adequacy is to compare the weighted mean of the synthetic control with the parallel value in Los Angeles for each matching variable, averaged over the preintervention time period. For all matching variables, these means are almost exactly matched (Table A.4).

Table A.4. Variable Means for Los Angeles and Synthetic Control

Crime	City of Los Angeles	Synthetic Control
Reported crime per capita for		
Homicides with a firearm	0.008	0.008
Homicides with a handgun	0.007	0.007
Robberies with a firearm	0.105	0.105
Assaults with a firearm	0.105	0.105
Total crimes with a firearm	0.218	0.218

Results on Gun Crime and Inference

Because the SCM failed to identify a sufficient control group, any results are statistically biased and unreliable. Thus, we do not present these results.

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