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Have U.S. Gun Buyback Programs Misfired?

Abstract

Gun buyback programs (GBPs), which use public funds to purchase civilians' privately-owned firearms, aim to reduce gun violence. However, little is known about their effects on firearm-related crime or deaths. Using data from the National Incident Based Reporting System, we find no evidence that GBPs reduce gun crime. Given our estimated null findings, with 95 percent confidence, we can rule out decreases in firearm-related crime of greater than 1.3 percent during the year following a buyback. Using data from the National Vital Statistics System, we also find no evidence that GBPs reduce suicides or homicides where a firearm was involved.

Keywords: gun buyback program; gun violence; gun crime; firearm-related deaths

1. Introduction

"This bill authorizes the Department of Justice's Bureau of Justice Assistance (BJA) to make grants to states, local governments, or gun dealers to conduct gun buyback programs. The BJA may distribute smart prepaid cards for use by a state, local government, or gun dealer to compensate individuals who dispose of firearms."

- House Resolution (H.R.) 1259, *Safer Neighborhoods Gun Buyback Act of 2019* (2019)

"The people you're most worried about -- criminals -- they're either not going to turn in their guns, or if they do turn in their guns, they'll turn in some old broken-down guns, get some money for it, and buy a new gun."

- Professor Eugene Volokh, University of California-Los Angeles (2019)

There are 1.2 guns for every person in the United States, with the total number of firearms in circulation estimated to be over 393 million (Small Arms Survey 2015). Gun violence is the leading cause of death among young men ages 15 to 19 (Xu et al. 2016), and firearms are involved in 51 percent of completed suicides and 73 percent of all homicides (Xu et al. 2016; FBI UCR, 2016). The link between the supply of firearms and gun violence has been the subject of intense debate, both among policymakers (Spitzer 2015; Cook & Leitzel 1998) and in the economics of crime literature (Lott 2013; Lott & Mustard 1997; Donohue & Ayres 2009; Donohue et al. 2019). However, there is growing evidence that limiting access to firearms reduces gun violence, both among adults (Donohue et al. 2017) and minors (Anderson et al. 2019).

In an effort to reduce gun crime by limiting the supply of firearms in circulation, a number of U.S. cities have implemented gun buyback programs (GBPs). GBPs use public funds to purchase civilians' privately-owned firearms. The first GBP was launched in Baltimore, Maryland in 1974, when the city paid anyone who turned in a firearm to a local police station $50 ($259 in 2019 dollars), after which the gun was destroyed. There were no questions asked of those who turned in their guns and no limits were placed on the type of firearm that could be submitted to authorities (Parry 1974). In total, the GBP collected approximately 13,500 firearms, 8,400 of which were handguns, and cost taxpayers approximately $660,000 (Kansas

City Star 1992).[1] Reports suggested that those turning in firearms included those "afraid someone would use [the firearm] in anger" and those who feared their firearms "would be stolen" (Parry 1974). However, homicides and firearm-related assaults rose by over 50 percent following the Baltimore GBP, raising concerns among policymakers about its effectiveness (Parry 1974).

Following the Baltimore experiment, dozens of U.S. cities have held GBPs. While most have been funded by state and local governments (Mullin 2001), federal funding for such buybacks has been limited.[2] However, following mass shootings in El Paso, Texas and Dayton, Ohio in 2019, 12 congressmen co-sponsored H.R. 1279, the *Safer Neighborhoods Gun Buyback Act of 2019*, which would permit the U.S. Bureau of Justice Assistance to issue grants to state and local governments to fund GBPs.

Proponents of GBPs, including New York Governor Andrew Cuomo (2019), former President Clinton (2000), and several 2020 Democratic presidential candidates, argue that GBPs may be an important tool in the fight against gun crime and firearm-related violence.[3] Some proponents, including former Vice President Joe Biden and Senator Bernie Sanders, have called for a Federal GBP that specifically targets assault weapons (Hains 2019).[4] Opponents, including the National Rifle Association, argue that GBPs will do little to reduce gun crime because potential criminals are unlikely to participate in such programs and will waste taxpayers' dollars

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[1] A proposal put forth by the police commissioner for federal funding to continue the GBP was rejected by the federal Law Enforcement Assistance Administration, which argued a GBP would encourage the manufacturing of handguns and would be ineffective as long as a firearm can be purchased for less than $50. The LEAA statement went on to state, "As long as it is possible to buy a gun…for less than $50 and turn it in to the police department for $50, the profit motive is present and the law of economics indicates that if people can buy guns at a lower price and sell them at a higher price they will do so." (Parry 1974)

[2] An exception was during the period from 1999-2001, when President Bill Clinton approved $15 million for GBPs through the *Buyback America* program, funded by the Department of Housing and Urban Development's Public Housing Drug Elimination Program. *Buyback America* awarded $500,000 to each participating city with a goal of removing 300,000 firearms from the national supply. The program suggested cities offer approximately $50 for each firearm in either in the form of cash, food, gift certificates, toys or tickets to sporting events. However, this program was abandoned in the first year of the George W. Bush Administration with the announcement,

> "Gun buyback program initiatives are limited in their effectiveness as a strategy to combat violent and gun-related crime." (U.S. Department of Housing and Urban Development 2001).

[3] In 1999, President Bill Clinton enacted *Buyback America*, stating "Every gun turned in through a buyback program means potentially one less tragedy."

[4] During his aborted run for the 2020 Democratic presidential nomination, Beto O'Roarke supported a mandatory buyback of assault rifles as part of a comprehensive plan to curb gun violence (Bradner 2019).

(Ellis and Hicken 2015). In March 2020, the Michigan House of Representatives passed House Bill 5479, which would ban the use of state funds for local gun buybacks (Michigan Legislature 2020). Similar legislation has been introduced in Wyoming (Coulter 2020).

The impact of a GBP on firearm-related violence is *a priori* unclear. GBPs may reduce gun crime if marginal criminals who would otherwise commit firearm-related crime sell their firearms to local governments and eschew criminal activity. Moreover, GBPs may reduce gun crime if law-abiding individuals sell their firearms, reducing the supply of guns available for theft by would-be criminals. Finally, a reduction in the supply of firearms could reduce firearm-related suicides if such acts are impulsive and influenced by ease of firearm access at a time of high emotion (Barber and Miller 2014).

On the other hand, GBPs may fail to reduce gun violence for a number of reasons. First, if the price city governments are willing to pay gun owners is less than the value of the firearm for most sellers, a relatively small number of firearms may be collected. Second, if criminals believe law-abiding citizens (and potential victims) are relinquishing their firearms, then they may be more willing to commit gun crimes following a GBP (Lott 1998). Moreover, if GBPs induce gun owners to turn in older forearms that are not well-functioning (Kuhn et al. 2002; Levitt 2004), or the income gained from the sale of the firearm is used to purchase newer, more effective guns (Mullin 2001), gun violence could rise. Finally, repeated GBP programs may permanently lower the ownership cost of a firearm, also leading to an increase in newer firearm purchases (Mullin 2001).

While policymakers are fiercely debating whether to allow public funds to be used for GBPs, next to nothing is known about the effectiveness of prior GBPs in U.S. cities. This paper is the first to present evidence on this question. We highlight three key findings. First, using data from the 1991-2015 National Incident Based Reporting System (NIBRS), we find no evidence that GBPs are effective at deterring gun crime either in the short- or longer-run. The precision of our estimates is such that, with 95 percent confidence, we can rule out decreases in gun crime of 1.3 percent in the 12 months following a GBP and 2.3 percent a year or more after a GBP. Second, in the two months following a GBP, we detect a small *increase* in gun crimes with no corresponding change in non-gun crimes. This finding is consistent with a possible criminal response to perceptions about the likelihood of self-defense among law-abiding gun owners.

Finally, turning to data from the National Vital Statistics System (NVSS), we find no evidence that GBPs affected firearm-related suicides or homicides.

We conclude that GBPs are an ineffective policy strategy to reduce gun violence, a finding consistent with descriptive evidence that (i) firearm sales prices are set too low by cities to appreciably reduce the local supply of firearms (Reuter and Mouzos 2004), (ii) most GBP participants are drawn from populations with low crime risk (Planty and Truman 2013; Violano et al. 2014; Romero et al. 1998), and (iii) firearms sold in GBPs tend to be older and less well-functioning than the average firearm (Kuhn et al. 2002; Levitt 2004).

2. Background

2.1 Firearm Availability and Crime

The United States has more guns per capita than any country in the world. The estimated per capita supply of firearms in the United States is 128 percent higher than in its closest competitor, Yemen (Small Arms Survey 2015).[5] In 2015, 9.4 million firearms were manufactured domestically in the United States, a 71 percent increase from the 5.5 million manufactured in 2010 (Bureau of Alcohol, Tobacco, Firearms and Explosives 2016). Firearms account for 645 deaths and 1,565 emergency room visits per week, making firearm-related injuries among the five leading causes of death for individuals under the age of 65 (Fowler et al. 2015). Firearms are also present in more than half of all completed suicides (Xu et al. 2016).

Researchers attempting to estimate the relationship between the supply of firearms and gun crime have been limited by the lack of data on firearm ownership. However, a handful of studies have used proxies for ownership. For instance, Duggan (2001) uses changes in rates of gun magazine sales and National Rifle Association membership to proxy for firearm ownership at the county-level and finds that increases in firearm ownership are significantly positively related to changes in the homicide rate, driven by increases in gun-related murders. Lang (2016) uses data on state-level background checks as a proxy for new firearms purchases and finds that background checks are negatively related to property crime, though essentially unrelated to violent crime.

[5] The territory Falkland Islands have 9.3 more firearms per 100 civilians than Yemen's 52.8 but still much fewer than the United States' 120.5.

Other studies have used policy shocks that might affect the supply of firearms, including background checks (Sen and Panjamapirom 2012), longer waiting periods (Ludwig and Cook 2000), stricter safe storage laws (DeSimone et al. 2013; Grossman et al. 2005; Anderson and Sabia 2018; Anderson et al. 2019), trigger lock requirements (Shuster et al. 2000), and right-to-carry laws (Lott and Mustard 1997; Donohue and Ayers; Donohue et al. 2019). For the most part, these studies suggest that the supply of available firearms is positively related to crime. [6,7]

2.2 Gun Buyback Programs

National gun buyback programs achieved worldwide prominence in the mid-1990s with a massive gun buyback effort in Australia. On April 28, 1996, a psychologically disturbed 28-year-old man shot and killed 35 people and injured 28 others as part a mass shooting spree across Port Arthur, Australia (Associated Press 1996). The killer used an AR-15 assault rifle, a firearm not required to be registered in his home state of Tasmania (Bilowol & Davis 2007).

Twelve days after the mass shooting, the Australasian Police Ministers' Council enacted the National Firearms Agreement (NFA) with the goal of combatting gun violence and preventing further tragedies. This legislative package included prohibitions on gun ownership, including several categories of firearms deemed to be high-risk such as self-loading rifles and pump shotguns. To facilitate the removal of these firearms from circulation, Australia

[6] Sen and Panjamapiro (2012) find that stricter background checks are associated with a decline in homicide and suicide rates among those ages 55 and older but find no evidence of reductions in deaths for younger age groups. Grossman et al. (2005) find that tougher safe storage laws that require storing firearms unloaded, locked, and away from ammunition are associated with a decline in youth suicide and accidental injury. DeSimone et al. (2010) find that CAP laws, which impose criminal liability on owners who allow children unsupervised access to firearms, are associated with reductions in nonfatal gun injuries among children. Finally, Anderson et al. (2019) find that CAP laws reduce gun carrying and unsafe behaviors at school.

[7] The literature on shall issue laws is much more controversial than the other-mentioned laws. Lott and Mustard (1997) find that shall issue laws are associated with a 7.7 percent decrease in murders and a 5 to 7 percent decrease in rapes and aggravated assaults. The authors argue that by limiting the ability of law-abiding gun owners to obtain firearms for self-defense, criminals are more willing to engage in criminal acts (Lott 1998). However, this finding is quite controversial. Ayres and Donohue (2003) find evidence that the results found by Lott and Mustard (1997) are sensitive to functional form of the empirical specifications, years used for the analysis, and choice of controls. Donohue and Ayers (2009) continue to find no evidence that shall issue laws reduce crime with more years of data, and also show that right-to-carry laws are associated with an increase in aggravated assaults. Other studies, using the same data, have also found no evidence that shall issue laws reduce crime (Black and Naggin 1998; Kovandzic 2005; Durlauf et al. 2016). Finally, Donohue et al. (2019) uses both event-study analyses and synthetic control approaches and finds that right-to-carry laws were associated with increases in violent crime rates.

implemented one of the largest gun buyback in world history, and largest ever in terms of percentage of privately-owned firearms relinquished.[8]

In total, the GBP collected over 640,000 firearms, representing 20 percent of privately-owned firearms in Australia and cost taxpayers $230 million (Wintemute 2013).[9] In terms of firearms per capita, a comparable GBP in the United States would collect 78.6 million firearms (Small Arms Survey 2015). The NFA also expanded the Australian national firearm registry, which required potential gun owners to affirm a "legitimate need" for a firearm,[10] mandated a 28-day waiting period before purchase, implemented a minimum legal purchasing age of 18, and banned the ownership of several types of semi-automatic and self-loading firearms (Reuter and Mouzos 2004).

Early studies of Australia's NFA, based on time-series variation, have produced mixed findings (Reuter and Mouzos 2004; Chapman et al. 2006; Baker and McPhedran, 2007,2008; Neill and Leigh, 2008; Lee and Suardi, 2009). Leigh and Neill (2010) exploit state-level variation in the number of firearms bought back and, using a two-way fixed effects model, find that a 3,500 increase in firearms turned in per 100,000 population was associated with a 45 to 78 percent reduction in the firearm-related suicide rate. However, Chapman et al. (2016) find that the results reported by Leigh and Neill (2010) could also be detected for non-firearm related deaths, suggesting that the buybacks generated important spillovers unrelated to guns or that the research design employed failed to isolate the causal impact of the buyback.[11] Taylor and Li (2015) find that Australia's NFA led to decreases in armed robberies and attempted murders

[8] Brazil collected the largest number of guns with their GBP, with as many as 1.1 million firearms collected over a 6-year period. Policy changes in October 2003 preempted the GBP, which included increasing the minimum purchase age to 25 and increasing fines, penalties, and prison sentences for those in violation of these policies. By the conclusion of the first 6-month program that began on July 2004, a reported 450,000 firearms were collected.

[9] This number is the most widely-agreed upon estimate but is potentially a lower bound (Braga & Wintemute, 2013). It is interesting to note that Australia has no domestic firearm manufacturers and only imports 30,000 firearms per year (Neill and Leigh 2010).

[10] This occupational use, being an authorized member of target shooting club, or hunting with proof of permission from rural landowner, to pass a written exam, and participate in an accredited training course on firearm safety by a State-controlled firearm licensing body. Personal safety was not considered an adequate reason to own a firearm.

[11] Several other nations have implemented large-scale GBPs similar to Australia, but have not been widely studied. For example, Brazil collected 1,100,000 firearms between 2003 and 2009, the United Kingdom collected 162,000 firearms in 1996, and Argentina collected 105,000 firearms in 2007. To our knowledge, no studies have examined the impact of these national buybacks.

relative to sexual assaults, which they argue should be unaffected by changes in Australia's gun supply.

In the United States, gun buyback efforts have largely occurred at the city-level. Typically, these GBPs have destroyed 1,000 or fewer firearms, with city governments generally paying gun owners between $25 to $200 per firearm (Braga and Wintemute 2013).[12] Only two studies of which we are aware have studied the relationship between U.S. city GBPs and gun crime, each a case study of a particular city.

Callahan et al. (1994) examine a 1992 GBP in Seattle, Washington, which collected 1,171 firearms.[13] Using time-series variation, these authors find no evidence that the Seattle program was associated with a significant decline in gun crime or assault-related firearm injuries.[14] Braga and Wintemute (2013) studied Boston's *Operation Ceasefire*, a broader anti-crime effort that included a GBP. This 2006 buyback, which paid firearm owners $200 per weapon, collected 1,019 firearms, all of which were handguns. This GBP differed from the typical city buyback in that required participants to document Boston residency and specifically targeted high-crime areas for firearm drop-off points and advertising. These authors find that in the four years following *Operation Ceasefire*, there was a 30 percent decline in shootings (Braga and Wintemute 2013).

3. Data and Methods

3.1 National Incident-Based Reporting System Data

Our primary data source is the National Incident-Based Reporting System (NIBRS). Compiled by the Federal Bureau of Investigation (FBI), the NIBRS provides detailed information on criminal incidents, including offenders, victims, and circumstances of the crime. Approximately 29 percent of the U.S. population is covered by the NIBRS, a population that is

[12] Funds to pay for firearms could be collected from small businesses, financial institutions, and civilians (Callahan et al. 1994).

[13] Community leaders found funding from the state and urban civic leaders, financial institutions, and local small business owners with the goal to purchase 2,000 firearms for $100,000. This buyback collected 1,172 firearms, 95 percent of which were handguns, and 83 percent of which in working condition. The mean age of participants was 51.

[14] The magnitude of the decline was fairly substantial, however, reaching nearly 18 percent.

responsible for about 27 percent of all crime committed (FBI 2013). Law enforcement agencies that report to the NIBRS comprise more than one third of all agencies in the U.S.

Our main analysis sample consists of 36,516 law enforcement agency-year-months from 1991 to 2015. We restrict our sample to the 245 agencies that serve populations of at least 50,000 individuals. The NIBRS is very useful for our analyses because these data include information on whether a firearm was used in each criminal incident.[15]

Our main dependent variable is *Gun Crime*. *Gun Crime* is an agency-by-month count of criminal incidents involving a handgun, shotgun, automatic weapon, or long gun. As shown in Table 1, the average number of criminal incidents involving a firearm was 25.9. This represents approximately 2.9 percent of all crimes and 29.4 percent of all violent crimes. As shown in Appendix Table 1, the vast majority (71.4 percent) of firearm-related offenses were violent in nature (robberies and aggravated assault), with non-violent firearm-related crimes predominately consisting of weapon law violations (18.4 percent) and drug/narcotic offenses (5.6 percent).[16] The average number of non-firearm-related criminal incidents (*Non-Gun Crimes*) per month-year was 870.3. Also in Appendix Table 1, we show means of the dependent variable by gender, age, and race/ethnicity of the offender. Males, African Americans, and those under age 35 are disproportionately likely to be arrested in connection with a gun crime.

3.2 National Vital Statistics System (NVSS) Data

We supplement our detailed crime data with administrative death records from the National Vital Statistics System (NVSS). The NVSS, collected by the National Center for Health Statistics, consists of individual level death records by cause and county. The NVSS covers deaths for all U.S. residents. For our analysis we focus on firearm related homicide and firearm related suicide and use the years 1991 to 2015. We restrict our sample to counties that have at least one city with 50,000 population to ensure we have identified all enactment dates.

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[15] There are limitations of the NIBRS worthy of note. In contrast to the Uniform Crime Reports (UCR), geographic coverage of the NIBRS is much more limited. Only 15 states report all of their crime data through the NIBRS, though the NIBRS collects crime data from 37 states. The Midwest and North central regions have a large participation rate of policing agencies to the NIBRS while coverage in the West is fairly limited.

[16] We use definition the FBI definition for violent offenses that includes murder/non-negligent manslaughter, forcible sex offenses, robbery, and aggravated assault.

Our main dependent variables from these data are *Firearm Death, Firearm Homicide,* and *Firearm Suicide*. As shown in Table 1, the mean rate of firearm-related deaths was 2.4 per 10,000 county population, 54.9 percent of which were suicides and 45.1 percent of which were homicides.

There are several important advantages of the NVSS data. First, the data include every county in the United States, which allows us to expand the number of buybacks that contribute to identification and increase the external validity of our research design. Second, these data allow us to explore the impact of GBPs on completed suicides.

3.3 Gun Buyback Program Data

Data on GBPs were collected through searches of national, state and local media outlets, as well as city legislative histories. A GBP is defined as an event where gun owners could legally sell their firearms to their local law enforcement agencies, after which the firearms were destroyed. The price per firearm set by city governments typically ranged from $25 to $450, with the highest prices paid for self-loading rifles. Payments were typically made in cash, but occasionally made in the form of gift cards for gas and groceries. In some cases, a GBP required participants to redeem their reward within days following the GBP.

From 1991-2015, we identified 339 GBPs held in 277 cities (in 110 counties). We used public records to uncover the number of firearms sold in each GBP. Among these buybacks, the mean (median) buyback consisted of 397 (157) firearms, or 14 (4) firearms per 10,000 county population. The largest one-time buyback took place in St. Louis, Missouri on November 16, 1991, when 7,469 firearms were sold. Approximately 53 percent of these GBP cities had one buyback, 23 percent had two buybacks, and 25 had three or more buybacks. The city with the largest number of gun buybacks in our sample was Worcester, Massachusetts with 14 buybacks. Table 2 lists cities that identify variation in our NIBRS analysis.[17]

[17] Online Appendix 1 documents the source for each city gun buyback program.

3.4 Empirical Strategy: NIBRS

First, we use data from the 1991-2015 NIBRS to estimate the following regression equation via a Poisson model:[18]

$$GunCrime_{\alpha cst} = \kappa_{\alpha cst}\, Exp\,(\beta_0 + \beta_1 GBP\text{0to2}_{\alpha cst} + \beta_2 GBP\text{3to5}_{\alpha cst} + \beta_3 GBP\text{6to11}_{\alpha cst} + \beta_4 GBP\text{12More}_{\alpha cst} + \mathrm{X}'_{cst}\beta_5 + \mathrm{Z}'_{st}\beta_6 + \sigma_\alpha + \Psi_t + \varepsilon_{\alpha cst}) \qquad (1)$$

where α indexes law enforcement agency in county *c* in state *s* in month-by-year t.[19] *Gun Crime* measures criminal incidents involving a firearm and κ proxies exposure using agency level-population served.[20] Our key right hand-side variables are a set of dichotomous variables indicating time periods following a GBP: 0 to 2 months after (*GBP0to2*), 3 to 5 months after (*GBP3to5*), 6 to 11 months after (*GBP6to11*) and 12 or more months after (*GBP12More*). Additionally, we include agency-level fixed effects (σ_α) and month-by-year fixed effects (Ψ_t). The vector $\mathbf{X}'_{cst}$ includes controls for demographic, socioeconomic, and policy controls measured at the county and state levels. Demographic controls are measured at the county-level and include the percent of the population (i) with a Bachelor's degree or higher, (ii) ages 15-to-19, (iii) ages 20-to-29, (iv) that were male, and (v) that were White, Black or Hispanic.[21] Socioeconomic and political controls include county-level per capita income, the county unemployment rate, the larger of the state or Federal minimum wage, and an indicator for whether the Governor of the state was a member of the Democrat party. Finally, crime control

[18] See Osgood (2000) for the strength and weaknesses of Poisson models to analyze crime rates. OLS cannot be used because of two problems (i) precision of reporting is increasing in population size, violating the homogeneity of the error term, and (ii) crime rates are bounded by zero, leading to an abnormal error term. Poisson models deal with these problems by setting the variance as a function of the mean and uses only positive values. Additionally, Poisson models handle fixed effects well without suffering from the incidental parameters problem.

[19] Within the NIBRS, agency and city are not interchangeable in all cases since the NIBRS accepts crime reports from university police, tribal police, state police, and other agencies which do not have a population attributed. Because we have restricted our population to agencies that serve a minimum of 50,000 people, none of these agencies are in our sample.

[20] The NIBRS compiles agency-level population at the year-level.

[21] Population distribution and per capita income come from the Surveillance, Epidemiology, and End Results (SEER), Educational attainment by state comes from the American Community Survey (ACS), and Unemployment Data comes from the Bureau of Labor Statistics (BLS).

policies are measured at the state-level and include indicators for whether the state has a shall issue law for concealed carry permits, a child access prevention (CAP) law with a reckless endangerment prosecutorial standard, a CAP law with a negligent storage prosecutorial standard, a stand your ground law, a law requiring a trigger lock for firearms sold, and a law requiring a minimum gun purchase age of 18.[22] In addition, we also include controls for police employees per capita, police expenditure per capita, and the number of firearm background checks per 100,000 population.

Identification of our key policy parameters of interest, β_1, β_2, β_3, and β_4 comes from 96 gun buybacks held across 42 cities with populations greater than 50,000. Table 1 lists each city's gun buyback program, the date of the initiative, and the number of guns sold, where such information was publicly available (in 81 percent of all cases). The geographic dispersion of all GBPs, including those not contributing to identification in the NIBRS, but which do contribute to identification in the NVSS-based analysis below, is shown in in Figure 1, with larger dots representing larger average gun buybacks, as measured by per-capita number of guns sold.

The credibility of our identification strategy relies on the parallel trends assumption being satisfied. We take a number of tacks to bolster the case for a causal interpretation of our estimated policy effects. First, to disentangle the effects of a GBP from jurisdiction-specific time-varying unobservables, we add controls for agency-specific linear time trends and census region-specific year effects to the right hand-side of the estimating equation:

$$GunCrime_{\alpha cst} = \kappa_{\alpha cst}\, Exp\,(\beta_0 + \beta_1 GBP0\text{to}2_{\alpha cst} + \beta_2 GBP3\text{to}5_{\alpha cst} + \beta_3 GBP6\text{to}11_{\alpha cst} + \beta_4 GBP12\mathrm{M}_{\alpha cst} + \mathrm{X}'_{cst}\beta_5 + \mathrm{Z}'_{st}\beta_6 + \sigma_\alpha + \sigma_\alpha * t + \Psi_t + \Theta_{ry} + \varepsilon_{\alpha cst}) \qquad (2)$$

where $\sigma_\alpha * t$ is an agency-specific linear time trend, r indexes one of the four census regions, y indexes the years 1991 to 2015, and Θ_{ry} is a census region-specific year effect. This approach is designed to control for unmeasured time shocks across law enforcement agencies and census regions.

[22] Background check counts come from the National Instant Criminal Background Check System (NICS), state CAP law policies are taken from Sabia and Anderson (2018), state policing expenditure are collected from the Bureau of Justice Statistics (BJS), county-level minimum wage data are collected from Vaghul and Zipperer (2016), and state shall issue laws, gun lock requirement laws, stand your ground laws, and minimum purchase age laws are collected from the Gifford Law Center.

Second, we conduct an event-study analysis by allowing the above β_1 through β_4 to vary over time before and after the gun buyback was held:

$$GunCrime_{\alpha cst} = \kappa_{\alpha cst} Exp(\varphi_0 + \left[\sum_{i=-12, i\neq -1}^{12} \varphi_i D_{\alpha cst}^i\right] + \mathrm{X}'_{cst}\beta_5 + \mathrm{Z}'_{st}\beta_6 + \sigma_\alpha + \sigma_\alpha * t + \Psi_t + \Theta_{ry} + \varepsilon_{\alpha cst}) \quad (3)$$

where $D_{\alpha cst}^i$ is a set of indicators set equal to 1 if a GBP occurred *i* months from period *t* and $G_{\alpha cst}^i$ is a set of indicators that are set equal to 1 if a GBP occurred *i* years from period t. For our monthly event-study analyses, we focus on the year before and after the buyback, with the reference period being the month prior to the GBP. We also present a longer-run event study where we examine the periods four years prior to through four years after the buyback. Note that because a single city may conduct multiple GBPs, our event-study framework accounts for multiple events (Sandler and Sandler 2014).[23] Common pre-treatment trends may lend credibility to the parallel trends assumption underlying our research design.

Third, we replace *Gun Crime* with *Non-Gun Crime* in equations (1) and (2). To the extent that GBPs have little effect on non-gun crime, estimates from such regressions could be interpreted as falsification tests. Detecting effects of GBPs on non-gun crime could suggest that the presence of a GBP is simply a marker of other unobserved crime trends, perhaps driven by unmeasured gun policies or macroeconomic trends. However, this is not a perfect falsification test. There may be general equilibrium effects through which GBPs affect non-gun crime. For instance, criminals may substitute toward other weapons in response to GBPs. Still, we would expect that GBPs should have a smaller effect on non-gun than gun crime. We also estimate a formal difference-in-difference-in-differences model to control for unobserved shocks that are common to gun and non-gun crime.

Finally, we explore spillover effects of GBPs. If buybacks have spillover effects to nearby jurisdictions without GBPs, and the effects are similar to that in "treatment" cities, this could bias estimated effects of GBPs toward zero. Thus, we explicitly model spillover effects of GBPs to neighboring jurisdictions by including controls in equations (1) and (2) for whether a GBP was held in another city within the same county or in a border county.

[23] In this framework the set of indicators, $D_{\alpha cst}^j$ and $G_{\alpha cst}^j$, are not individually mutually exclusive sets.

3.5 Synthetic Control Estimates

As noted above, there is substantial heterogeneity across city GBPs, including the size of the gun buyback program and characteristics of the affected population. While we experiment with interacting our gun buyback variables in equations (1) and (2) with indicators for the size of the buyback (i.e., number of guns sold), we can more flexibly address heterogeneous treatment effects via a synthetic control design (Abadie et al. 2010). Our donor pool for each buyback city is comprised of cities that did not enact a GBP over the period from 1991 to 2015. We generate each synthetic city by requiring pre-treatment rates of gun crime per 10,000 population to be similar in each pre-treatment year (Botosaru and Ferman 2019; Ferman and Pinto 2019). In unreported results available upon request, we also explored "matching" on every other pre-treatment year and on observable economic and gun policy variables described above. To conduct statistical inference, we assign a placebo GBP to each city in the donor pool (on the date of the treatment city GBP) and generate a *p*-value for the estimated treatment effect by ranking the treatment city's pre-post mean squared prediction error (MSPE) ratio to each donor city's pre-post MSPE (Abadie et al. 2010).

3.6 Empirical Strategy for NVSS

Finally, to explore the relationship between GBPs and firearm-related deaths, we conduct an event-study analysis comparable to equation (3a) above, using a Poisson specification:

$$FirearmDeaths_{cst} \ = \ \kappa_{cst} Exp(\varphi_0 + \left[\textstyle\sum_{i=-12, i\neq -1}^{12} \varphi_i D_{cst}^{i}\right] + \mathrm{X}'_{cst}\beta_5 + \mathrm{Z}'_{st}\beta_6 + \sigma_c + \sigma_c * t + \Psi_t + \Theta_{ry} + \varepsilon_{cst}) \qquad (5)$$

where $FirearmDeaths_{cst}$ measures firearm-related deaths in county *c* and state *s* at year *t*. A county is coded as having a gun buyback program if a city with a population greater than 50,000 held a buyback. We also disaggregate firearm-related deaths into homicides and suicides, as well as conduct an event-study analysis using up to four years before and four years after the buyback. Note that our main NVSS-based analysis is conducted at the county-level because only 35 percent of all deaths in the NVSS data include city identifiers. To the extent that the effects of city GBPs are very localized, a county-based analysis may be biased toward zero.

Supplemental analysis on the city-level subsample, available upon request, generates results on firearm-related deaths that are qualitatively similar to those reported below.

4. Results

Tables 3 through 11 present the main findings for this study. Standard errors are corrected for clustering at the city level (Bertrand et al. 2004).

4.1 Main Findings from NIBRS

Estimates of β_1 through β_4 from equation (1) are reported in Panel I of Table 3. Estimates from our most parsimonious specification, which includes controls for agency and month-by-year fixed effects, provide no evidence that GBPs are associated with reductions in gun crime, either during the first year following the buyback or in the years following. The inclusion of controls for demographic characteristics (column 2), socioeconomic and political controls (column 3), and other gun policies and anti-crime investments (column 4) does not change this finding. The stability of our estimated gun buyback program effects across specifications adds to our confidence that the timing of GBPs is exogenous to gun crime. The precision of our estimates is such that, with 95 percent confidence, we can rule out gun crime declines in the 12 months following a GBP of greater than 1.3 percent and gun crime declines of greater than 2.2 percent a year after a GBP is held. Moreover, during the first two months following the gun buyback, we find that a GBP is associated with an *increase* in incidents of firearm-related crime. The 7.7 percent increase in gun crime we detect in column (4) is relatively modest, suggesting at most, two additional gun crimes.[24]

In columns (5) and (6), we test the robustness of our findings to controls for spatial heterogeneity. We find that the estimated effects of GBPs on gun crime is not sensitive to added controls for agency-specific linear time trends (column 5) or census region-specific year effects (column 6).

Figure 2 presents the coefficient estimates from leads and lags the event-study analysis described in equation (3a). We find no evidence of differential pre-treatment trends in gun crime

[24] Using a wild cluster bootstrap approach to conduct inference (Cameron et al. 2015), we obtain a p-value of 0.153 for the period 0-2 months following the gun buyback. Estimated p-values for the remaining post-treatment windows range from 0.490 to 0.869.

in "treatment" and "comparison cities" in the months leading up to a city GBP. During the period 0 to 2 months following a GBP, we find evidence of a 4.9 to 6.8 percent increase in gun crime, followed by no change in gun crime after 2 months following a buyback. Figure 3 presents results from equation (3b), where we examine longer-run annual leads and lags. Again, the findings in the pre-treatment period are consistent with the common trends assumption. In the post-treatment period, we find a small increase in gun crime over the first year following a GBP, followed by longer-run null results.

In Panel II of Table 3, we present estimates of the effect of GBPs on non-gun crime. We find no evidence that a city GBP significantly affected the probability of a non-gun crime, either in the short- or longer-run. These results add to our confidence that the short-run positive gun crime effect we detect in Panel I of Table 3 is not driven by time-varying agency-specific unmeasured heterogeneity.

Difference-in-difference-in-differences estimates of the effect of GBPs on gun versus non-gun crime, shown in Table 4, control for jurisdiction-specific time-varying unobservables that may commonly affect gun and non-gun crime, such as increased investments in law enforcement community policing. Across the three specifications presented in Table 4, we show that GBPs are associated with a 6.9 percent increase in gun as compared to non-gun crime in the two months following a gun buyback. We find no change in gun versus non-gun crime thereafter. An event-study analysis, shown in Figure 4, confirms that this effect is not contaminated by differential pre-treatment crime trends.

4.3 Robustness Tests

In Table 5, we conduct a number of sensitivity checks on our main difference-in-difference (Panel I) and triple-difference (Panel II) specifications. Column (1) replicates estimates reported in column (6) of Table 4 (Panel I) and column (3) of Table 5 (Panel II).

In column (2), we restrict our buyback jurisdictions to those who implemented a GBP, thus identifying our effects entirely from differences in timing of enactment. Our findings are quantitatively similar to those shown in column (1).

In column (3), we require a strictly balanced panel of law enforcement agency-months and limit our sample to the 2005-2015 period, when electronic news sources in larger cities are likely to have information on city GBP. The pattern of results is similar to our main sample, with triple-difference estimates providing the strongest evidence for increases in gun crime in the first two months following the buyback, but no effect thereafter.

One concern with our "two-way fixed effects" estimates is that they may lead to biased estimates — and misleading diagnostic tests on pre-treatment trends—in the presence of dynamic treatment effects and the use of early-adopting GBP cities as controls for late-adopting GBP cities (Goodman-Bacon 2021).[25] In Figure 5, we present the results of an event-study analysis using the approach advanced by Callaway and Sant'Anna (2021). Using cities that never hold a GBP as the sole counterfactuals for our GBP-holding cities, we continue to find that pre-treatment crime trends are consistent with the common trends assumption. In the post-treatment period, we find no evidence that GBPs reduce gun crime (panel a) or non-gun crime (panel b) in the 24 months following its enactment.[26] We find a small, short-run (month of enactment) increase in gun relative to non-gun crime.

To test the sensitivity of our estimates to additional data quality controls, we drop agency-months where crime counts are greater than or less than two standard deviations from the city-specific mean (column 4). The estimates from these exercises are consistent with those in column (1).

In the final three columns of Table 5, we explore the sensitivity of results to weighting the regression by the population served by the law enforcement agency (column 5), adding controls for agency-specific quadratic time trends (column 6), and adding controls for census division-specific year effects (column 7). The pattern of results is consistent with our baseline estimates.[27]

[25] Of the 251 large cities in our sample, 207 never held a GBP.

[26] The event studies in Figure 5 are generated using a balanced panel from the sample described in column (3) of Table 5. Regression results are obtained from linear models, which are, at the time of this writing, the only regressions permitted in the available R package for this estimator. The dependent variable is equal to the inverse hyperbolic sign of gun (or non-gun) crimes, adjusting for agency population. In Appendix Figure 1 we show event-study analyses from two-way fixed effects models.

[27] Finally, in Appendix Table 2, we aggregate our data to the annual-level and estimate both Poisson and ordinary least squares estimates of the effect of GBPs on gun crime. Our results show no evidence of declines in gun crime following a GBP.

4.4 Jurisdictional Spillovers and Heterogeneous Treatment Effects

Could the null estimates we observe be driven by spillover effects of GBPs into nearby jurisdictions? In Table 6, we explore whether GBPs generate gun crime spillovers to (i) neighboring cities within the county, or (ii) cities in a bordering county. To that end, we generate two variables: *GBP in County*, set equal to 1 if a city within the county held a gun buyback and 0 otherwise, and *Border County GBP*, set equal to 1 if a city in a border county held a gun buyback and 0 otherwise. Note that in the creation of these variables, we also include GBPs held in neighboring cities even if those cities were not themselves included in the NIBRS. Our findings in Table 6 provide very little evidence of spillover effects of GBPs on gun crime in neighboring jurisdictions.

In Table 7, explore whether city GBPs had differential effects on violent (Panel I) as compared to non-violent (Panel II) gun crime. We find no evidence that GBPs significantly reduced violent or non-violent crime in either the short-run or longer-run. Rather, we find holding a GBP is associated with short-run increases in firearm-involved robberies (Panel I, column 2), weapons law violations (Panel II, column 2), drug violations (Panel II, column 3), vandalism (Panel II, column 4), and kidnapping (Panel II, column 5).[28]

We next examine whether the effects of GBPs on gun crime differ by age, gender, or race of the offender. The findings in Table 8 provide no support for the hypothesis that GBPs reduced gun crime for any demographic group. Instead, we find short-run increases in gun crime for those ages 18 to 23 (column 3), individuals over age 35 (column 5), males (column 6), females (column 7), and African Americans (column 9).

Finally, in Table 9, we examine whether our null results for the average city GBP masks heterogeneity in their effects by the number of guns bought back. We measure the size of the buyback by whether guns sold per capita (or raw number of guns sold) exceeded the (i) median city GBP (columns 1 and 2), (ii) 75th percentile of the gun sale distribution (columns 3 and 4), and (iii) 90th percentile of the gun sale distribution (columns 5 and 6). We find no evidence that relatively larger gun buyback efforts, in absolute or per capita terms, reduced gun crime in either the short- or long-run. Moreover, we find that the short-run increases in gun crime in the first two months following the gun buyback were driven by relatively larger GBPs.

[28] Our findings in Appendix Table 3, which include controls for spatial heterogeneity, produce a qualitatively similar pattern of results.

4.5 Synthetic Control Estimates

To most flexibly test for heterogeneity in the gun crime effects of city GBPs, we next turn to a synthetic control analysis (Abadie et al. 2010). Figure 6 shows synthetic control figures for each city GBP when we impose the requirement that each of six pre-treatment periods have comparable gun crime rates. Appendix Table 4 lists the donor cities that received positive weights for each treatment city and Table 10 shows estimated treatment effects for up to five years following a GBP. Also shown in Table 10 are p-values generated from permutation-type tests of how frequently one would expect to observe the estimated treatment effect of an actual GBP when randomly assigned a placebo (false) GBP in each donor city.[29] Note that there are a total of 211 cities that have never enacted a GBP in the donor pool.[30]

We highlight two key findings from the synthetic control analysis. First, across treatment cities, our synthetic procedure generates pre-intervention trends in gun crime that are similar across treatment and synthetic control jurisdictions. Second, we find very little evidence that GBPs are associated with significant post-treatment reductions in gun crime across GBPs. In 29 out of 38 cases, we can reject the hypothesis that GBPs affected gun crime following the first year of buyback. When p-values generated from permutation tests were less than .05, the estimated effect is *positive* in seven of these cases. Only for two Ohio cities — Cincinnati and Columbus — do we find evidence of significant declines in the rate of gun crime rate following a GBP. And in each case, the effect is rendered statistically indistinguishable from zero following the inclusion of a second year of post-treatment data.

4.6 Firearm-Related Homicides and Suicides

Finally, we use NVSS mortality data to estimate the impact of a gun GBP on county-level firearm-related deaths (column 1), which are then disaggregated into firearm-related suicides

[29] Appendix Figure 2 displays the results for placebo tests where we apply the synthetic control method to cities did not have a gun buyback during our sample period. We assign a placebo date to each donor state that is equivalent to the treatment city GBP date. The gray lines represent the gap in the gun crime rate associated with each placebo run.

[30] One GBP city, Battle Creek, did not report crime in December of 2013, we impute the average crime rate of previous months in that year for analysis. Three cities (Flint, Milwaukee, Springfield) did not have a full pretreatment year and thus could not be used for synthetic controls.

(column 2) and homicides (column 3).[31] The event-study coefficients reported in Table 11, along with those depicted in Figures 6 through 8, provide no evidence that GBPs reduce firearm-related deaths in the year following a GBP. There is also no evidence that firearm-related suicides and homicides declined in the the years following a GBP (Figures 9 and 10). Triple-difference estimates, shown in Appendix Figures 3 through 5, suggest no evidence that firearm-related deaths fell relative to non-firearm related deaths following a GBP.

Finally, when we explore whether firearm-related deaths were affected by larger gun buybacks (Table 12 and Appendix Table 5), we find little evidence that firearm-related deaths were differentially affected by the size of the buyback. Furthermore, event study analyses provide no evidence that larger GBPs reduce firearm-related suicides (Appendix Table 6) or homicides.

5. Discussion

Our findings provide strong evidence that GBPs of the last three decades did little to reduce gun crime or firearm-related violence. There are a number of explanations for this result. First, the number of firearms sold in a typical GBP is relatively modest, perhaps owed to a city buyback price of \$25 to \$450 per firearm. This price is often well below the cost of a new, or even used, firearm, which can easily exceed \$500 (Willis 2018). If a gun owner values his/her firearm at more than the city buyback price, perhaps because of its self-defense benefits or its usefulness in facilitating income-generating crime, the firearm will not be sold.

When compared to the number of licensed gun owners and firearm sales, it may not be surprising that GBPs have small effects on gun crime. For instance, the 2014 GBP in Somerville, Massachusetts collected 15 firearms. But just two years prior, 1,593 firearm permits were held by Somerville residents (Ouellette 2013). To take another example, a 2015 GBP in Worcester, Massachusetts collected 271 firearms. However, annual firearm sales at *The Gun Parlor*, a retail establishment in Worcester, exceeded 3,100 during this period (Gross 2018). Finally, GBPs in Gary, Indiana (2012), Indianapolis, Indiana (2006), and South Bend, Indiana (2007) netted 90 to 253 firearms per buyback. To put these numbers in context, there are approximately 17 registered firearms per 1,000 population in Indiana (World Population Review

[31] City-level identifiers are only available for 35 percent of all firearm-involved deaths. Supplementary analysis on this sample, shown in Appendix Table 2, produced a qualitatively similar pattern of results.

2020), or about 17,800 firearms in these buyback cities. Most buybacks have a very small effect on the local supply of firearms, which could be one reason for their ineffectiveness.

Second, most participants in buyback programs tend to be drawn from populations with relatively lower crime risk (Planty and Truman 2013; Violano et al. 2014; Romero et al. 1998). Case studies of GBPs find that the modal participant is white (81 percent), male (74 percent), and over the age of 55 (59 percent) (Violano et al. 2014). Those who participated in GBPs lived, on average, 19 miles from the city in more suburban and rural areas, and had a median household income of $65,731.

GBPs also do not appear to reduce the number of households without firearms. More than half of respondents who sold their firearms at a buyback had another firearm in their home (Kasper et al. 2017; Green et al. 2017; Violano et al. 2014).

In addition, firearms sold in buybacks do not appear to be those that would typically be used to commit gun crime. Approximately 25 percent of GBP participants reported that the firearms they sold were not in good working order (Romero et al. 1998). A study of a series of gun buybacks in Milwaukee, Wisconsin between 1994 and 1997 found that the types of firearms turned in were more likely to be older weapons with longer barrels and smaller magazine size (Kuhn et al. 2002). Such weapon traits are not commonly linked to firearm-related homicides and suicides (Planty and Truman 2013). These findings are consistent with adverse selection in firearm quality that one would expect to observe with a relatively low offer price and no price discrimination. Moreover, income gains to GBP participants selling low quality firearms could result in an increase the supply of properly functioning guns.

Finally, over half of all firearms turned in were not originally purchased by the gun owner, but instead were inherited or gifted (National Research Council 2005). To the extent that inherited or gifted firearms are less desired than firearms that are purchased with one's own income, this finding could suggest that GBPs are one way to transfer unwanted firearms.

Given that most buybacks are small in scope, involve poorly functioning firearms, and involve participants with low propensity for crime, how can we explain the short-run small crime increase that we observe following a GBP? This result is consistent with a number of hypotheses. First, as noted above, the marginal participant may have sold a poorly functioning firearm to obtain income used to purchase well-functioning firearms that may be used in or stolen for commission of gun crime.[32] Second, the GBP may induce some potential gun criminals to engage in more crime because they perceive that law-abiding citizens participating in GBP will be less likely to defend themselves with deadly force (Lott 1998). Third, some buybacks, particularly those that are repeated, may induce stocking up of firearms among those who (i) fear that repeated buybacks may lead to tighter compulsory gun control policies, or (ii) are responding to a reduction in the ownership cost of firearms and expect to sell the firearm back in the future (Mullin 2001).

6. Conclusion

Advocates for GBPs argue that by reducing the supply of firearms, buybacks may be an important tool in tackling gun crime in the United States. Congressman Donald Payne (D-New Jersey), a lead sponsor of House Resolution 1279, *Safer Neighborhoods Gun Buyback Act of 2019*, which would permit Federal funding of GBPs through the BJA predicted, "there will be fewer guns in circulation, which will help reduce crime." (Payne 2019) On the other hand, North Dakota Representative Luke Simons, the sponsor of a bill that would ban GBPs in his state, argued "firearm buybacks do nothing to increase public safety and shouldn't be subsidized by taxpayer money". (MacPherson, 2019)

Over the last decade, over 100 U.S. cities have adopted GBPs with the hope of reducing gun crime. Using data from the 1991-2015 National Incident–Based Reporting System (NIBRS) and National Vital Statistics System (NVSS), this study is the first to comprehensively assess the impact of city GBPs on gun crime and firearm-related violence.

Our findings provide no evidence that GBPs are effective at deterring gun crime, firearm-related homicides, or firearm-related suicides in the short- or long-run. The precision of our estimates is such that, with 95 percent confidence, we can rule out gun crime declines of greater than 1.3 percent in the 12 months following a buyback and gun crime declines of greater than 2.3 percent more than one year after a buyback. These null findings are consistent with descriptive evidence that (i) firearm sales prices are set too low by cities to appreciably reduce the local supply of firearms (Reuter and Mouzos 2004), (ii) most GBP participants are drawn from

[32] While not a common occurrence, one GBP from Detroit in 2012 was faced with protesters who offered cash for guns adjacent to the GBP. In this case, we may actually see a GBP facilitating the exchange of firearms, albeit an illegal exchange since protestors presumably did not have a license to conduct such transactions.

populations with low crime risk (Planty and Truman 2013; Violano et al. 2014; Romero et al. 1998), and (iii) firearms sold in GBPs tend to be older and less well-functioning than the average firearm (Kuhn et al. 2002; Levitt 2004).

Moreover, we find some evidence of a small, short-run *increase* in gun crime in the two months following a GBP. This result is consistent with the notion that GBPs primarily target low-risk firearms that are more likely to deter crime than be used in the commission of a crime (Kuhn et al. 2002) and with the hypothesis that some criminals may be emboldened by their perception that victims will be less likely to defend themselves with deadly physical force (Lott 1998).

Our findings provide compelling evidence that prior U.S. city GBPs have been ineffective at deterring gun violence and have been an inefficient use of taxpayers' dollars. This conclusion suggests that alternative crime-fighting policies, such as child access prevention gun safe storage laws (Anderson et al. 2019) and stricter background checks (Gius 2015), are likely to be much more effective policy strategies to deter gun violence. Our findings also suggest that prior city GBPs have been poorly designed to achieve their policy objectives. In contrast, buyback programs that target high crime neighborhoods, offer higher rewards for firearms more likely to be used to commit violent gun crimes, and price discriminate across weapons of heterogeneous quality, may affect gun violence differently. However, the limited public appetite for large-scale government spending on voluntary gun confiscation, coupled with the inherent difficulties of targeting weapons used by criminals but not those used by those who would defend themselves, make gun buybacks an unlikely anti-gun crime-fighting tool.

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Figure 1: Gun Buyback Programs in Cities with Greater than 50,000 Population

Black dots indicate that a GBP occurred in this indicated city. Larger dots represent cities with more guns bought back per capita.

Figure 2: Event Study Analysis of Gun-Related Crimes, Short-Run

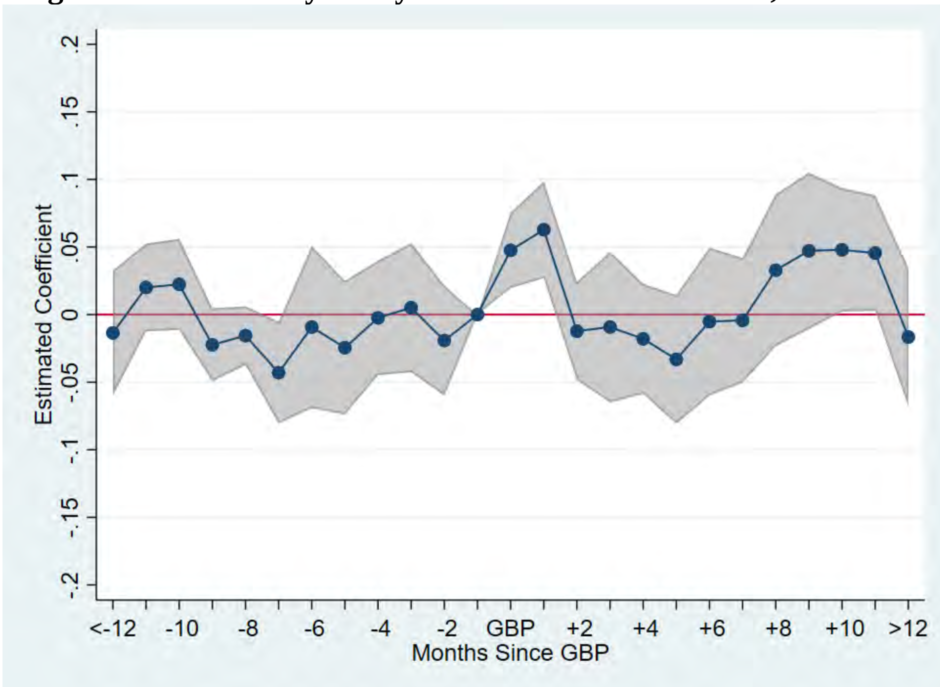
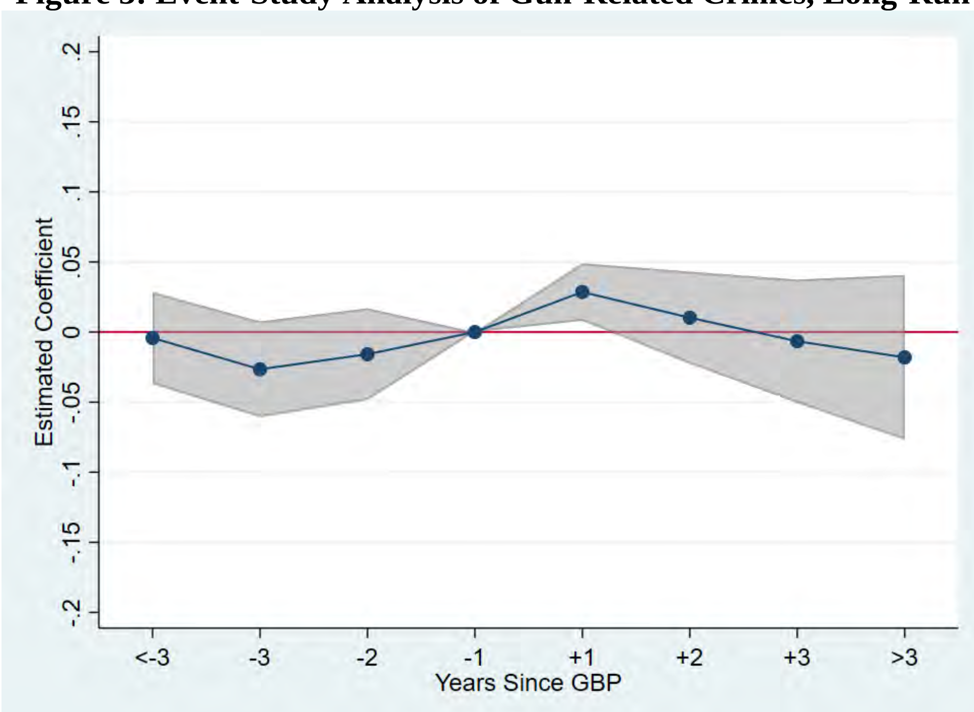
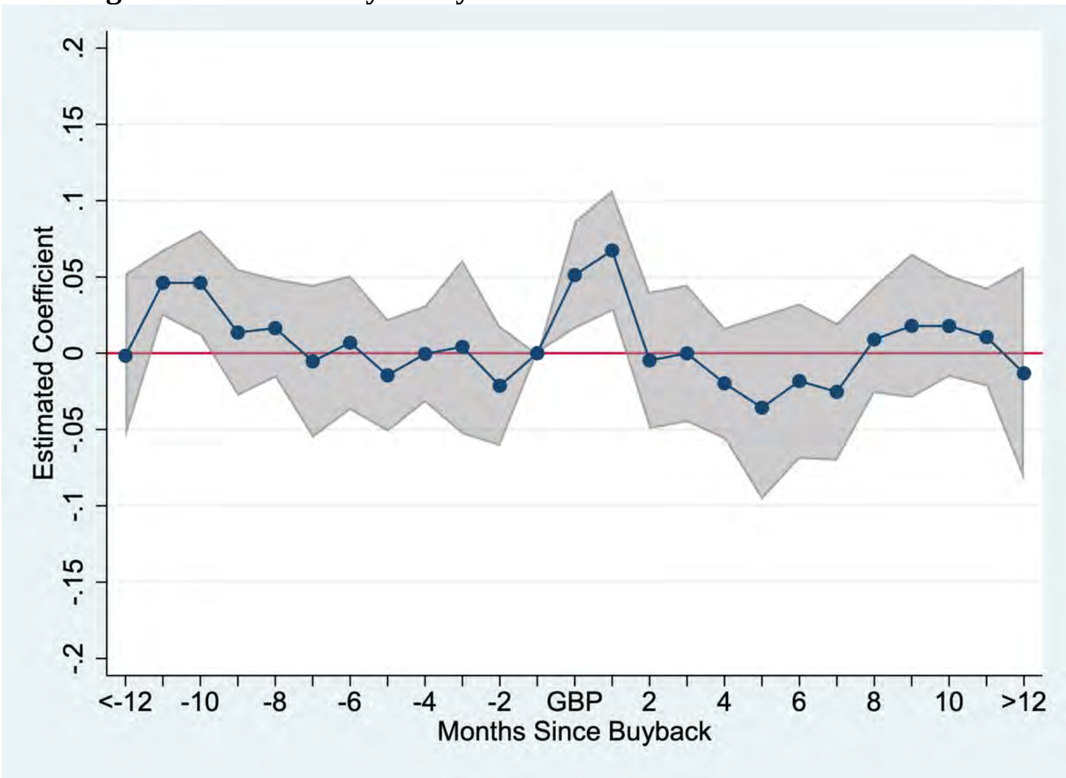


Figure 3: Event-Study Analysis of Gun-Related Crimes, Long-Run



Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the state-level.

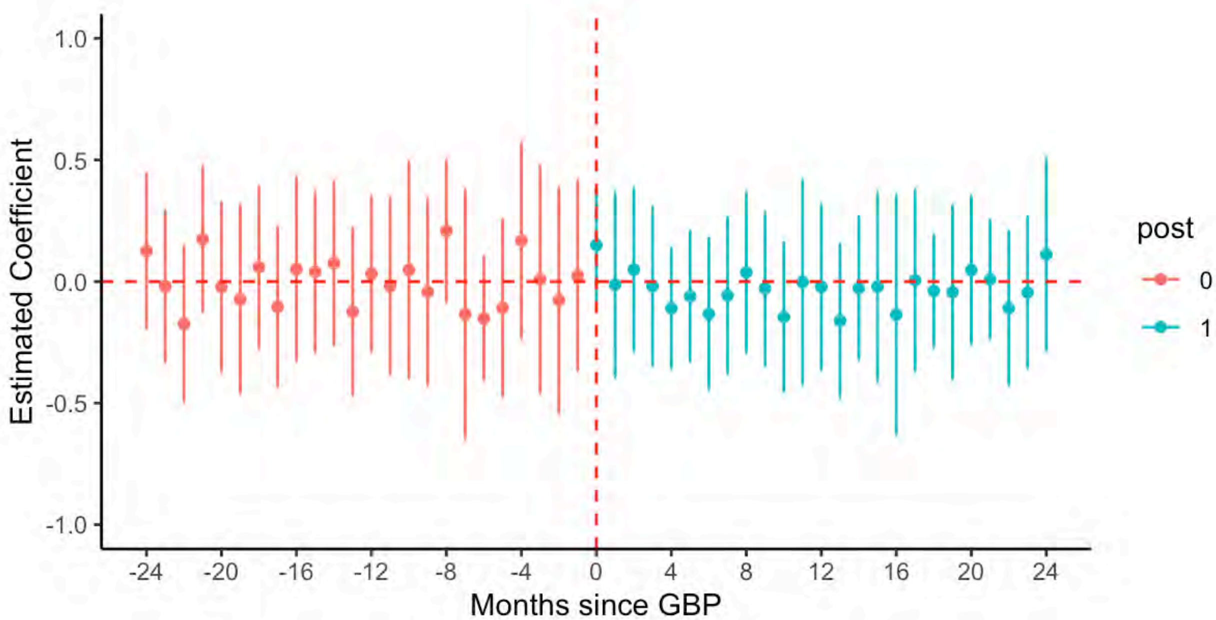
Figure 4: Event-Study Analysis of Gun Crimes vs Non-Gun Crimes



Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the state-level.

Figure 5: Callaway-Sant'Anna Event-Study Analysis of Effect of GBP on Crime, Using Cities without GBP as Counterfactual

(a) Gun Crime



(b) Non-Gun Crime

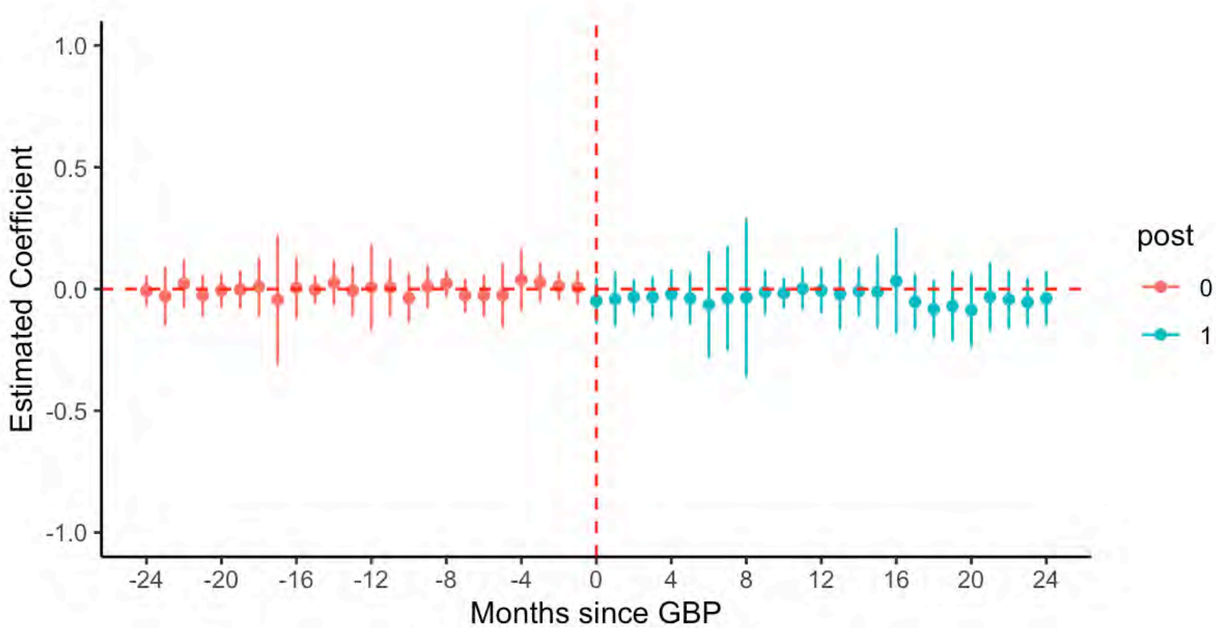
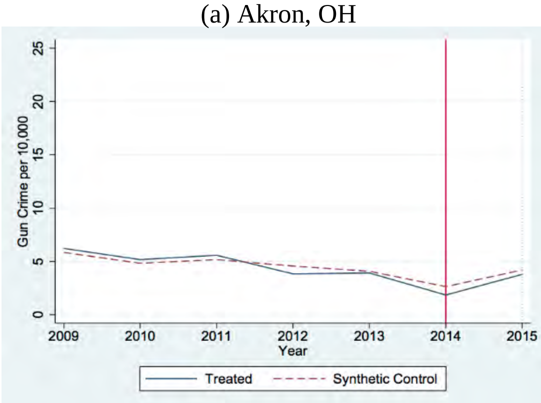
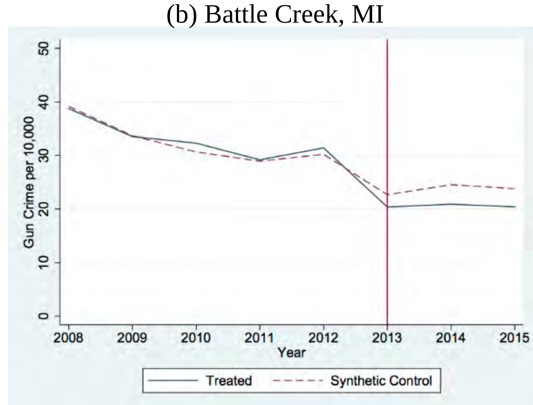


Figure 6: Synthetic Control Estimates of Effect of GBP on Gun Crime

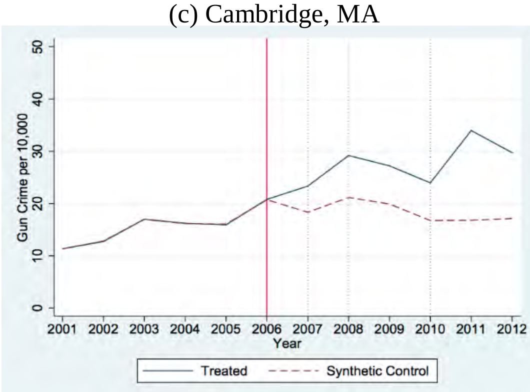
(a) Akron, OH



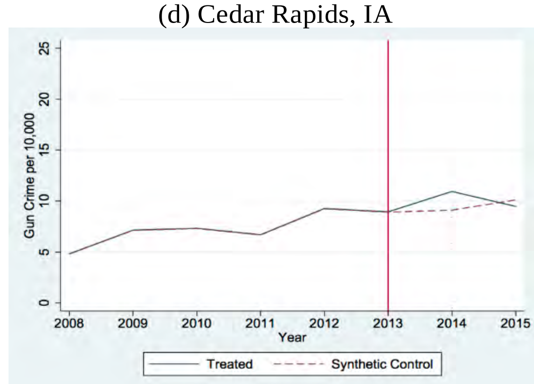
(b) Battle Creek, MI



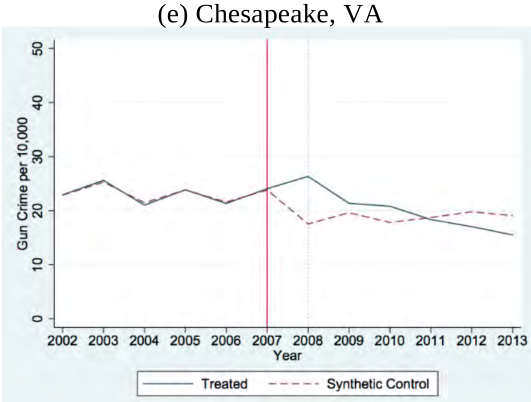
(c) Cambridge, MA



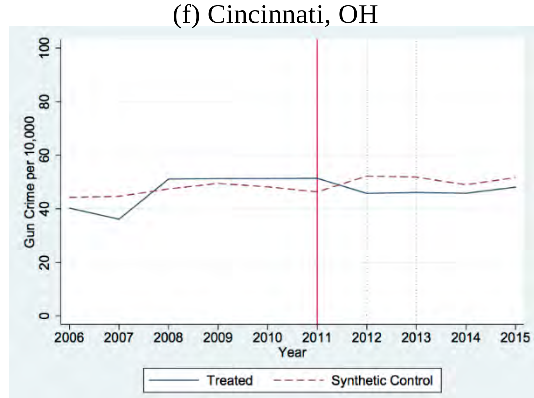
(d) Cedar Rapids, IA



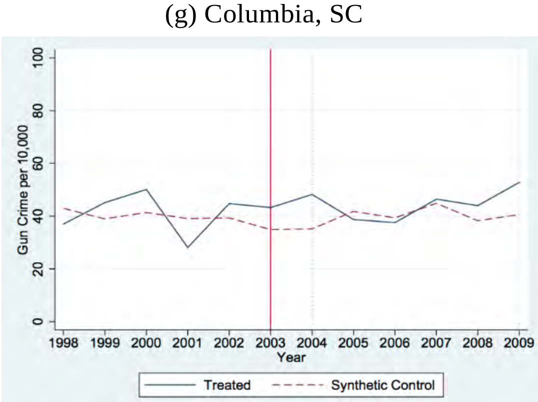
(e) Chesapeake, VA



(f) Cincinnati, OH



(g) Columbia, SC



(h) Columbus, OH

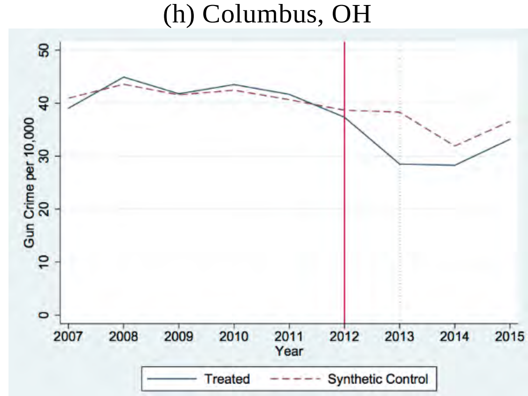
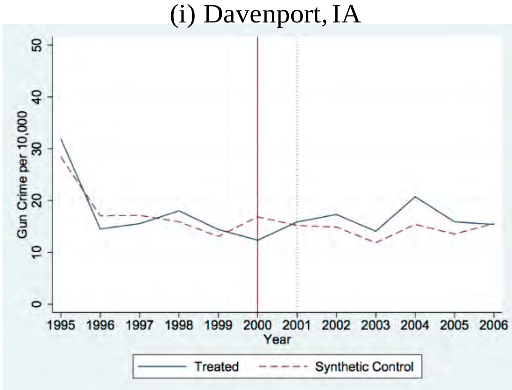
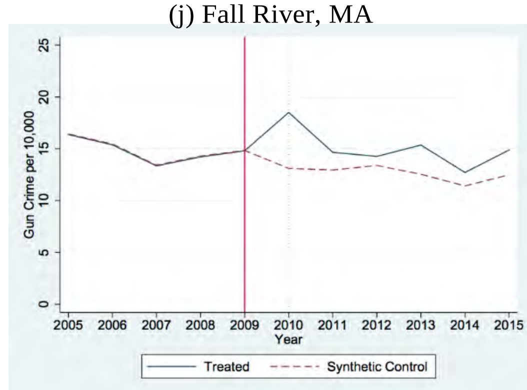


Figure 5, Continued

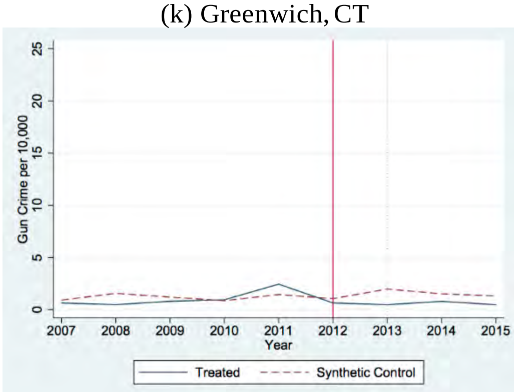
(i) Davenport, IA



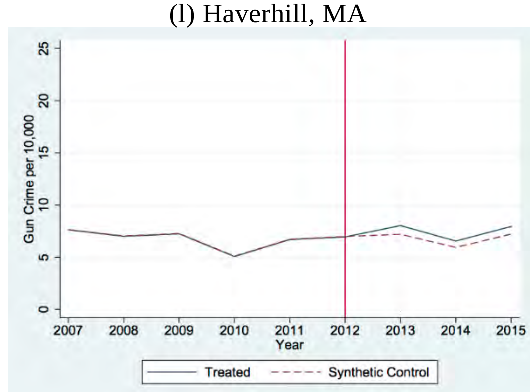
(j) Fall River, MA



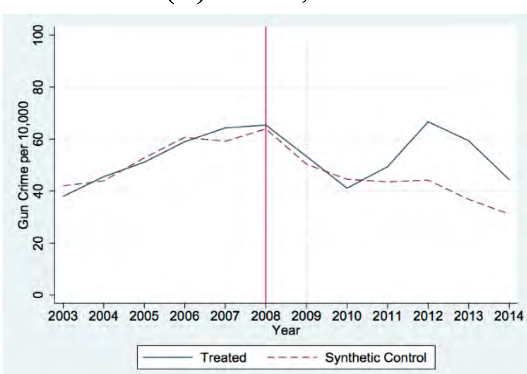
(k) Greenwich, CT



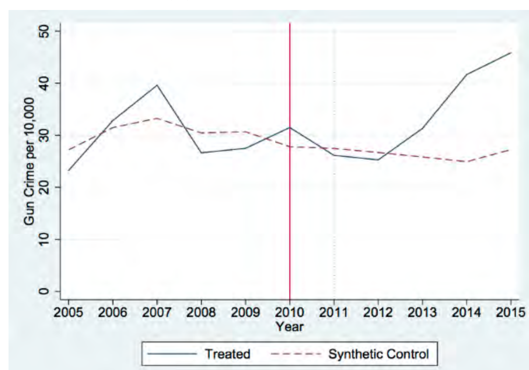
(l) Haverhill, MA



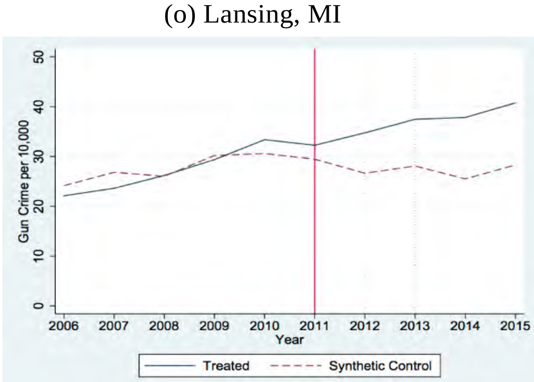
(m) Jackson, TN



(n) Kalamazoo, MI



(o) Lansing, MI



(p) Lowell, MA

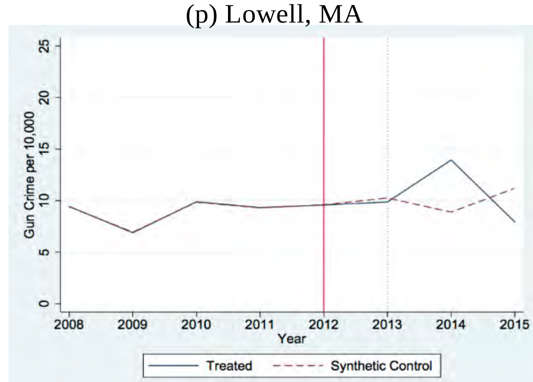
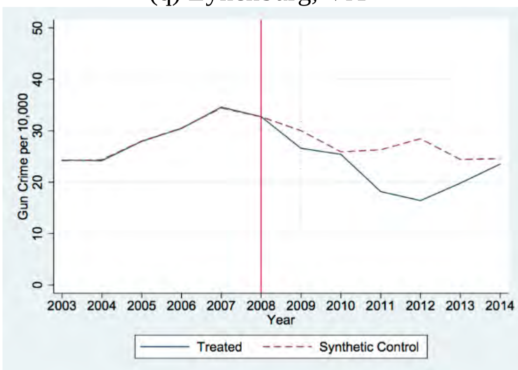
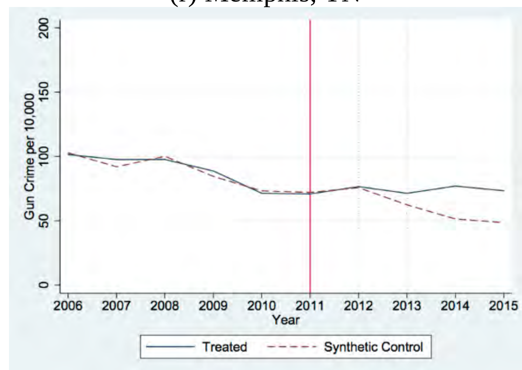


Figure 5, Continued

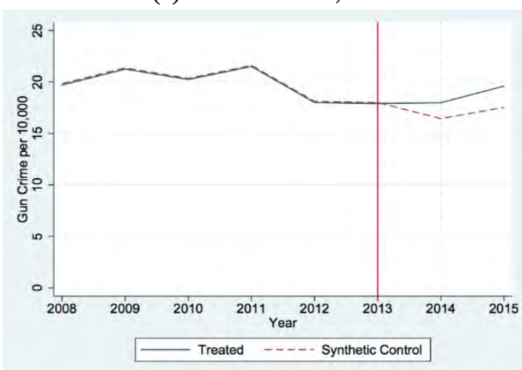
(q) Lynchburg, VA



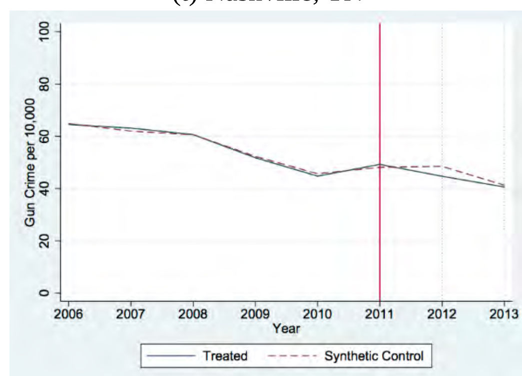
(r) Memphis, TN



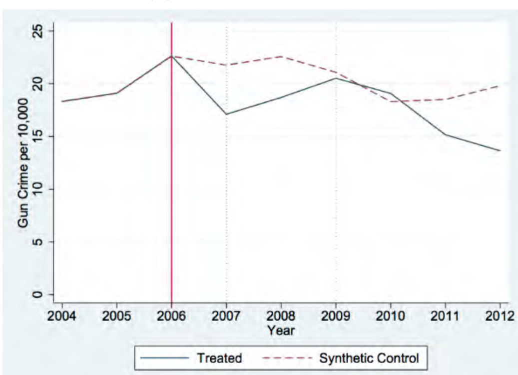
(s) Murfreesboro, TN



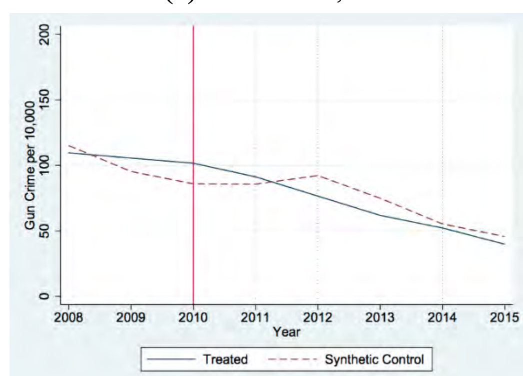
(t) Nashville, TN



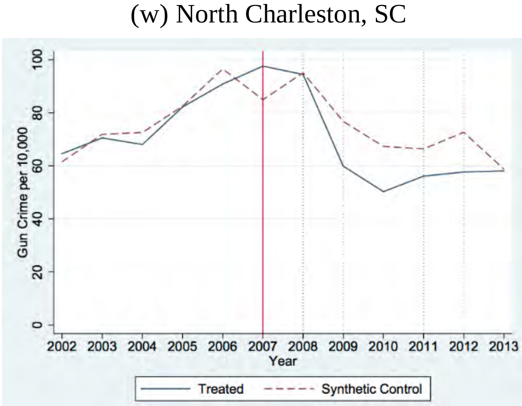
(u) New Bedford, MA



(v) New Haven, CT



(w) North Charleston, SC



(x) Norwalk, CT

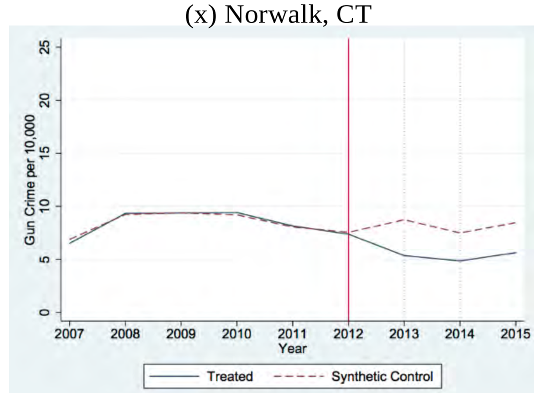
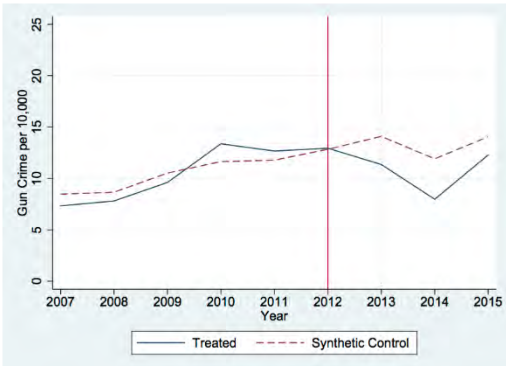
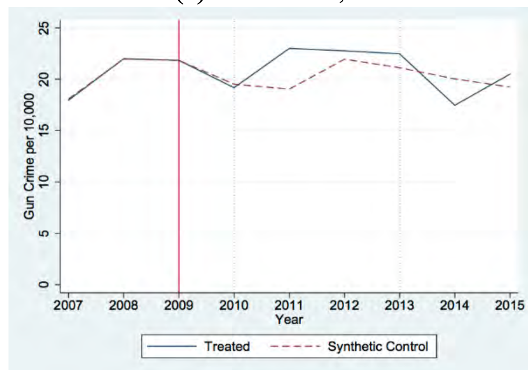


Figure 5, Continued

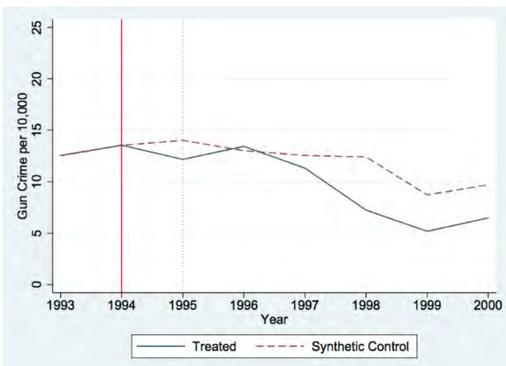
(y) Pawtucket, RI



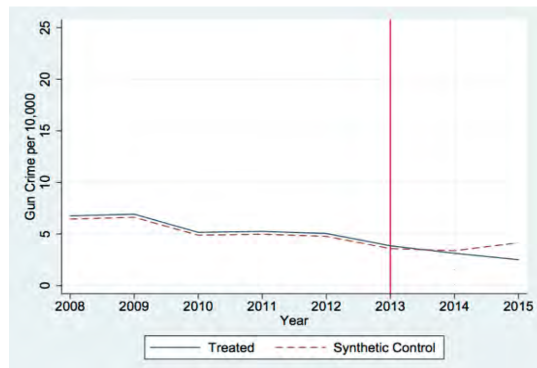
(z) Providence, RI



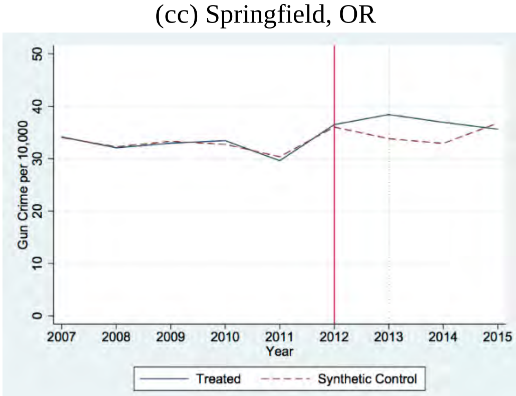
(aa) Pocatello, ID



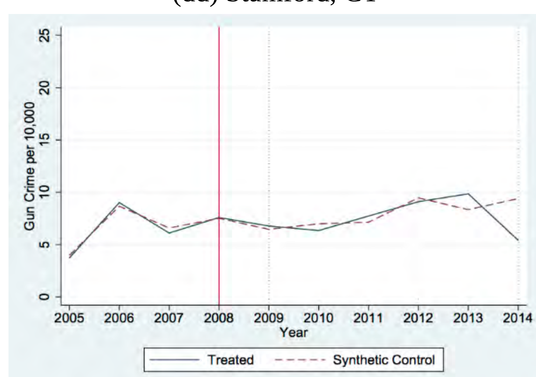
(bb) Sommerville, MA



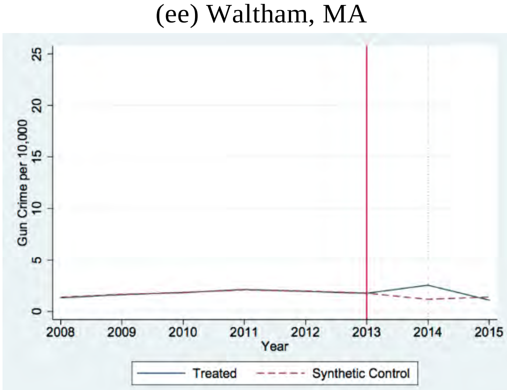
(cc) Springfield, OR



(dd) Stamford, CT



(ee) Waltham, MA



(ff) Warwick, RI

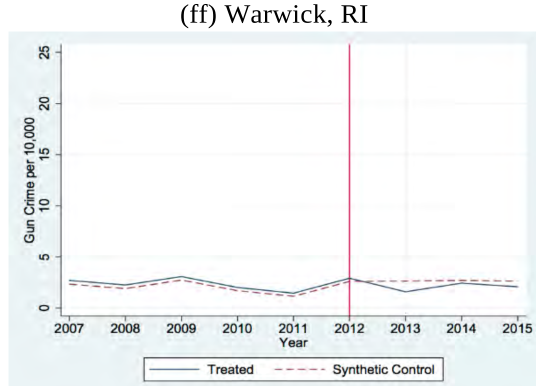
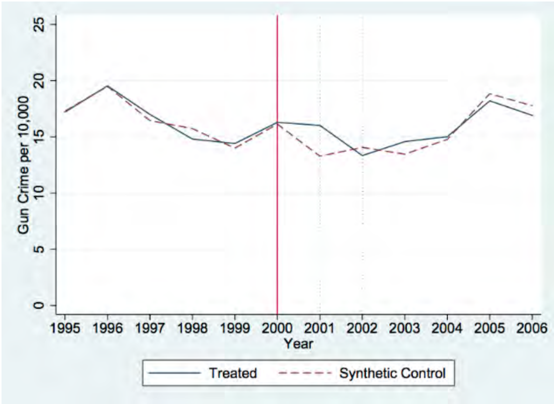
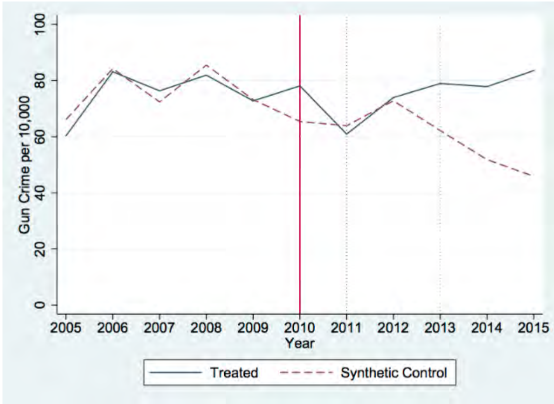


Figure 5, Continued

(gg) Waterloo, IA



(hh) Wilmington, DE



(ii) Worcester, MA

Figure 6: Event-Study Analysis of Effect of GBP on Firearm-Related Deaths, NVSS

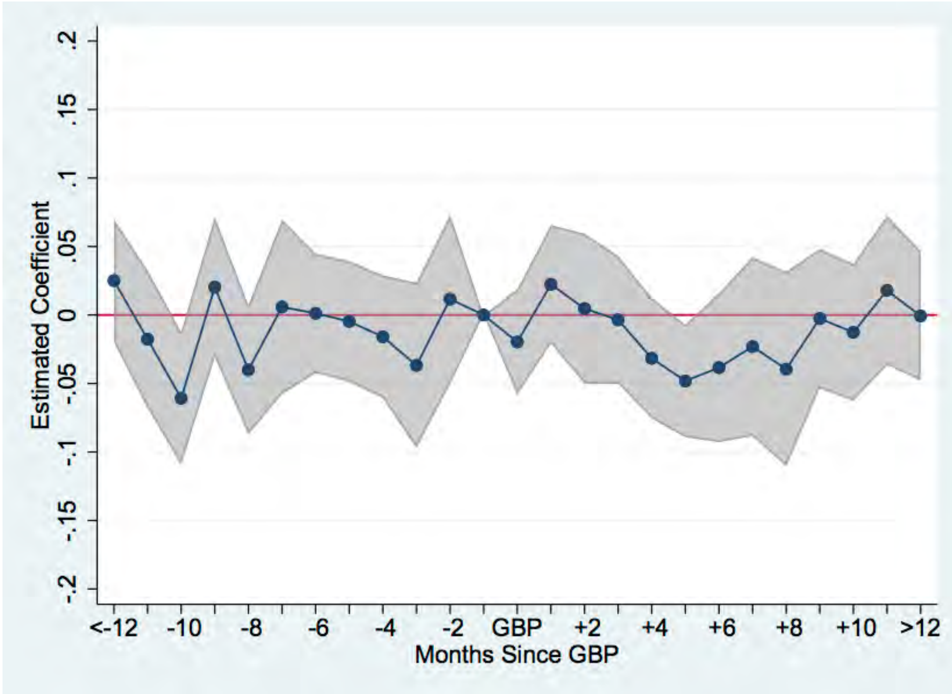
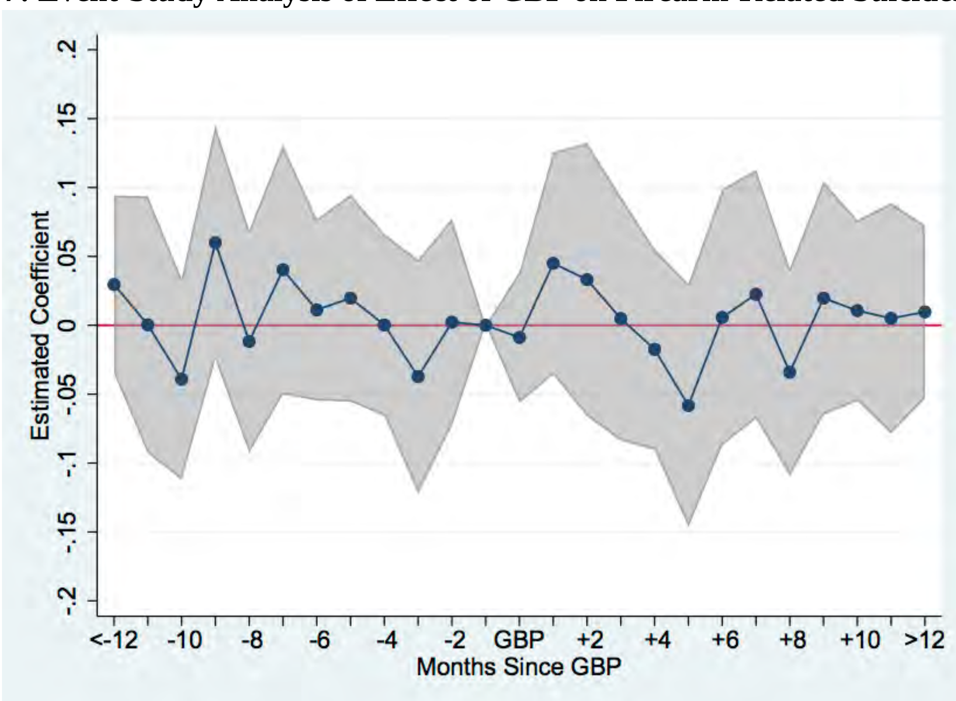


Figure 7: Event-Study Analysis of Effect of GBP on Firearm-Related Suicides, NVSS



Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include socioeconomic, demographic, & policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. Standard errors are clustered at the state-level.

Figure 8: Event-Study Analysis of Effect of GBP on Firearm-Related Homicides, NVSS

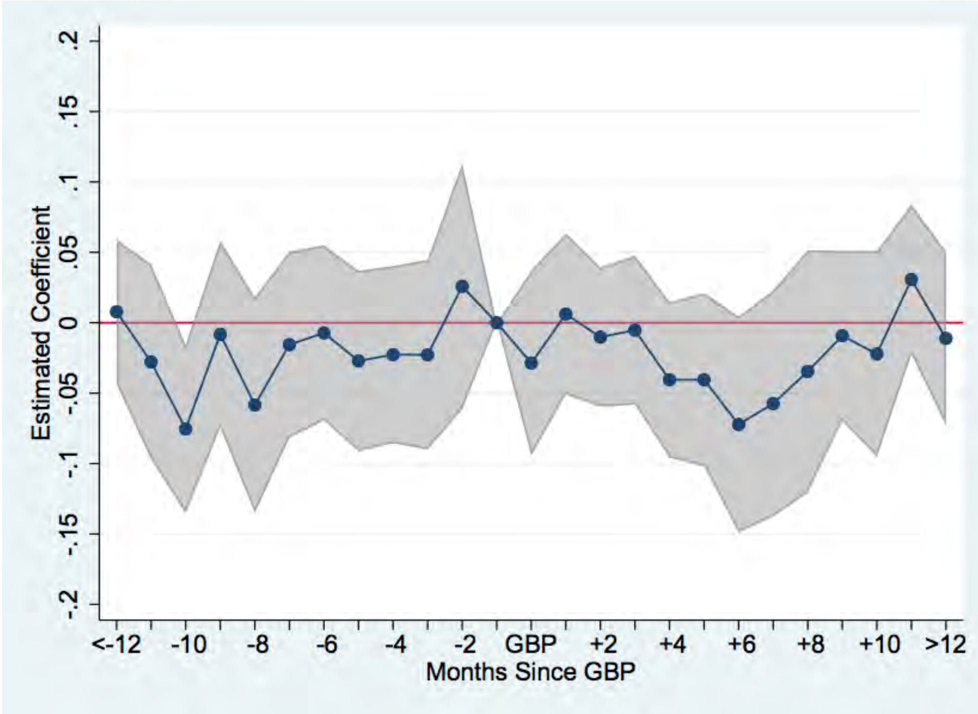
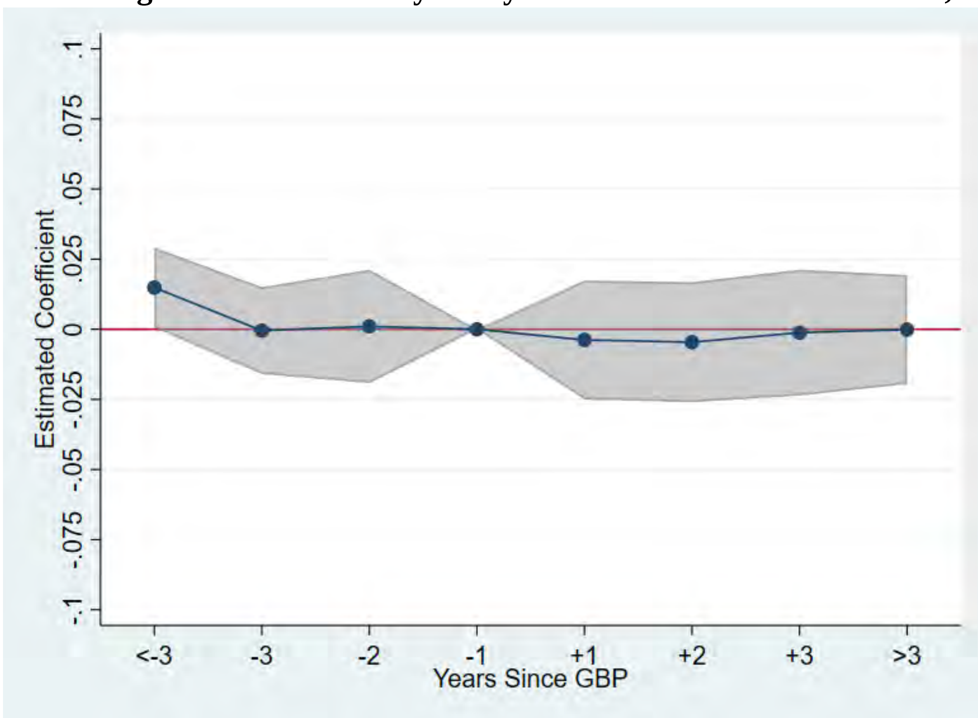
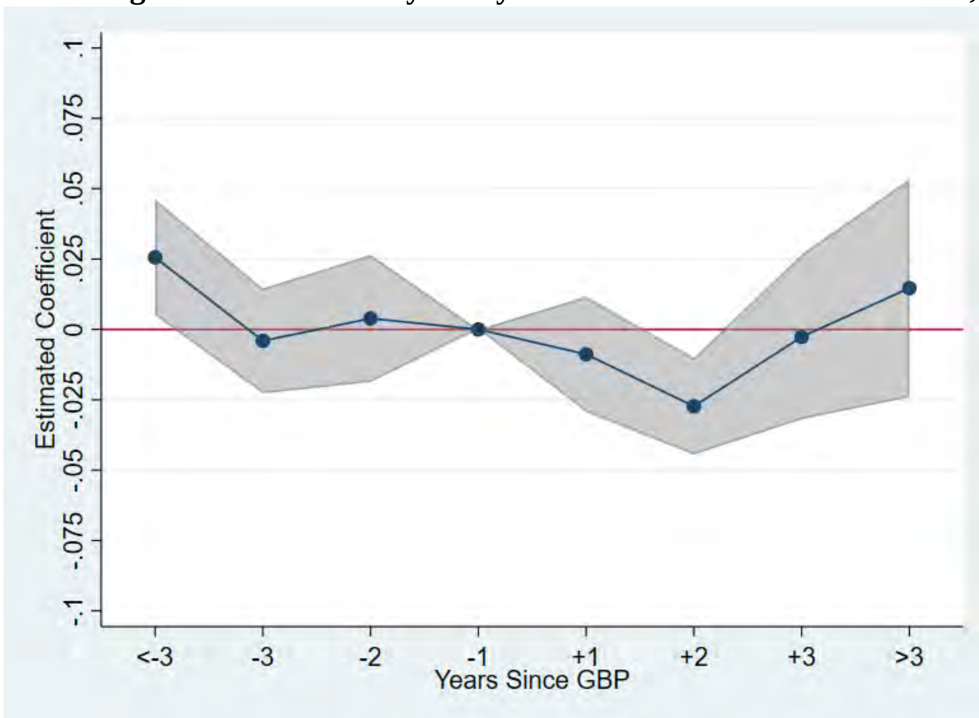


Figure 9: Longer-Run Event-Study Analysis of Firearm-Related Suicides, NVSS



Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the state-level.

Figure 10: Longer-Run Event-Study Analysis of Firearm-Related Homicides, NVSS



Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the state-level.

Table 1: Descriptive Statistics, 1991-2015

	Mean	SD	Min	Max
Dependent Variables, NIBRS				
Gun Crime Count	25.879	68.140	0.00	1, 020.00
Gun Crime Count, Non-Violent	7.399	16.674	0.00	328.00
Gun Crime Count, Violent	18.480	53.282	0.00	849.00
Non-Gun Crime Count	870.266	1,208.272	0.00	10,940.00
Non-Gun Crime Count, Non-Violent	825.931	1, 139.367	0.00	9, 961.00
Non-Gun Crime Count, Violent	44.336	77.038	0.00	1, 255.00
Dependent Variables, NVSS				
Firearm-Related Deaths	2.370	6.036	0.00	219.00
Firearm-Related Homicide	1.068	4.260	0.00	175.00
Firearm-Related Suicide	1.302	2.363	0.00	58.00
Demographic Controls				
Population % Age 15-19[a]	0.072	0.010	0.03	0.12
Population % Age 20-29[a]	0.149	0.035	0.08	0.30
Population % White[a]	0.734	0.141	0.14	0.98
Population % Black[a]	0.132	0.133	0.00	0.58
Population % Hispanic[a]	0.092	0.088	0.00	0.82
Population % Male[a]	0.490	0.009	0.46	0.52
Percentage College Graduates[b]	0.293	0.066	0.14	0.46
Socioeconomic and Political Controls				
Per Capita Income ($2015)[b]	45,799.965	13,576.492	22,745.69	114,636.08
Unemployment Rate[c]	5.868	2.494	0.80	28.30
Minimum Wage ($2015)[d]	7.504	0.945	5.90	15.24
Democrat Governor[e]	0.432	0.495	0.00	1.00
Crime Control Policy Controls				
Background Checks per 100,000 Population[f]	5,012.952	5,317.951	0.00	72,737.94
Stand Your Ground Law[g]	0.335	0.471	0.00	1.00
Shall Issue Law[g]	0.757	0.429	0.00	1.00
Gun Lock Required for Sale[g]	0.330	0.470	0.00	1.00
Negligent Child Access Prevention Law[h]	0.305	0.460	0.00	1.00
Reckless Child Access Prevention Law[h]	0.248	0.432	0.00	1.00
Police Expenditure per 100,000 ($2015)[i]	263.838	45.656	108.87	396.32
Police Officers per 100,000 Population[i]	2.147	0.425	1.25	3.67
State/Federal Minimum Age Law 18[g]	0.993	0.084	0.00	1.00

Notes: NIBRS sample includes all reporting agencies with population mean greater than or equal to 50,000. NVSS sample includes all counties with at least one city with 50,000 population.

<u>Observation Level:</u>

State: State/Fed Min Age Law, Shall Issue Law, Background Checks per 100,000, Gun Lock Required, CAP Law, Police Expenditure, Police Expenditure per Capita, Stand Your Ground, Percent College Grad, Democratic Governor.

County: Age 15-19, Age 20-29, Population % Hispanic, Population % Black, Population % White, Population % Male, Per Capita Income (2015$), Minimum Wage (Average), Unemployment Rate

Sources:

[a] Surveillance, Epidemiology, and End Results (SEER).
[b] American Community Survey (ACS)
[c] Bureau of Labor Statistics (BLS)
[d] Vaghul and Zipperer (2016)
[e] Ballotpedia
[f] National Instant Criminal Background Check System (NICS)
[g] Giffords Law Center
[h] Sabia and Anderson (2018)
[i] Bureau of Justice Statistics (BJS)

Table 2: Dates of GBPs in NIBRS Larger Cities, 1991-2015

City	Date of GBP Initiative	Guns Bought	City	Date of GBP Initiative	Guns Bought
Akron, OH	December 12, 2007	950	New Bedford, MA	June 6, 2007	18
	November 11, 2008	650		October 10, 2009	41
	November 11, 2010	*NR*	New Haven, CT	December 12, 2006	235
Cambridge, MA	June 6, 2015	55		December 12, 2007	100
Cedar Rapids, IA	September 9, 2014	34		December 12, 2011	60
Chesapeake, VA	December 12, 2008	309		December 12, 2012	65
Cincinatti, OH	January 1, 2012	50		June 6, 2014	106
	January 1, 2012	135	North Charleston, SC	December 12, 2008	245
Clevelend, OH	November 11, 2007	41		December 12, 2009	127
	November 11, 2008	*NR*		January 1, 2011	*NR*
	November 11, 2010	*NR*		March 3, 2012	135
	September 9, 2011	706	Norwalk, CT	February 2, 2013	18
	October 10, 2012	298		June 6, 2014	20
	June 6, 2013	*NR*	Pawtucket, RI	June 6, 2013	50
	September 9, 2014	270	Pocatello, ID	April 4, 1995	22
	August 8, 2015	150	Providence, RI	June 6, 2010	*NR*
Columbia, SC	October 10, 1994	*NR*		April 4, 2013	186
	November 11, 1994	*NR*	Pueblo, CO	December 12, 2012	7
	December 12, 2004	300	Sommerville, MA	August 8, 2014	15
	February 2, 2009	*NR*	Springfield, MA	March 3, 2013	333
Columbus, OH	June 6, 2013	352	Springfield, OR	October 10, 2007	500
Davenport, IA	September 9, 2001	450		March 3, 2013	330
Denver, CO	December 12, 2008	15	Stamford, CT	January 1, 2009	56
Detroit, MI	July 7, 2006	*NR*		December 12, 2012	54
	September 9, 2010	*NR*		March 3, 2014	32
	December 12, 2010	*NR*		September 9, 2014	*NR*
	August 8, 2012	365		October 10, 2014	*NR*
	May 5, 2013	*NR*	Waltham, MA	September 9, 2014	46
	November 11, 2013	24	Warwick, RI	April 4, 2013	186
Fall River, MA	December 12, 2010	115	Waterloo, IA	December 12, 2001	100
Flint, MI	April 4, 2000	1000		April 4, 2002	*NR*
	March 3, 2007	*NR*		June 6, 2011	34
	June 6, 2009	178	Wilmington, DE	December 12, 2011	2040
Greenwich, CT	February 2, 2013	11		August 8, 2013	67
Haverhill, MA	May 5, 2013	27	Worcester, MA	December 12, 2002	250
Jackson, TN	December 12, 2009	*NR*		December 12, 2003	244
Kalamazoo, MI	July 7, 2011	99		December 12, 2004	305

City	Date of GBP Initiative	Guns Bought	City	Date of GBP Initiative	Guns Bought
Lansing, MI	August 8, 2012	99	Worcester, MA	December 12, 2005	206
	February 2, 2013	100		December 12, 2006	271
Lowell, MA	November 11, 2013	36		December 12, 2007	217
Lynchburg, VA	July 7, 2009	12		December 12, 2008	127
Memphis, TN	September 9, 2012	497		December 12, 2009	241
Memphis, TN	September 9, 2013	588		December 12, 2010	195
Milwaukee, WI	July 7, 2005	200		December 12, 2011	113
	May 5, 2014	353		December 12, 2012	142
Murfreesboro, TN	April 4, 2014	6		December 12, 2013	85
Nashville, TN	November 11, 2012	91		December 12, 2014	149
	June 6, 2013	62		December 12, 2015	271

Notes: *NR* identifies GBPs were the number of guns bought was not publicly reported.

Table 3: Poisson Estimates of the Effect of a GBP on Gun-Related Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Months Following GBP:			**Panel I: Gun-Related Crime**			
0 to 2 Months	0.078**	0.070*	0.071*	0.077**	0.073**	0.068**
	(0.029)	(0.029)	(0.028)	(0.025)	(0.024)	(0.024)
3 to 5 Months	-0.006	-0.013	-0.011	0.003	-0.010	-0.007
	(0.023)	(0.027)	(0.027)	(0.026)	(0.038)	(0.031)
6 to 11 Months	0.037	0.029	0.031	0.043*	0.031	0.035
	(0.024)	(0.026)	(0.026)	(0.021)	(0.033)	(0.026)
≥ 12 Months	0.001	0.003	0.004	0.000	-0.007	0.008
	(0.012)	(0.010)	(0.010)	(0.010)	(0.032)	(0.026)
			Panel II: Non-Gun Related Crime			
0 to 2 Months	0.010	0.008	0.009	0.009	-0.004	-0.008
	(0.017)	(0.017)	(0.017)	(0.016)	(0.017)	(0.014)
3 to 5 Months	0.005	0.003	0.004	0.006	-0.009	-0.008
	(0.020)	(0.021)	(0.021)	(0.021)	(0.024)	(0.022)
6 to 11 Months	0.030	0.029	0.030	0.033	0.020	0.021
	(0.019)	(0.020)	(0.020)	(0.021)	(0.023)	(0.020)
≥ 12 Months	0.007	0.010	0.009	0.011	0.020	0.027
	(0.008)	(0.007)	(0.008)	(0.008)	(0.015)	(0.015)
Observations	36,516	36,516	36,516	36,516	36,516	36,516
Demographic Controls?	-	Yes	Yes	Yes	Yes	Yes
Socioeconomic Controls?	-	-	Yes	Yes	Yes	Yes
Crime Control Policy?	-	-	-	Yes	Yes	Yes
Agency-Specific Linear Time Trends?	-	-	-	-	Yes	Yes
Region-Specific Year Effects?	-	-	-	-	-	Yes

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime Control Policy controls are listed in Table 1.

Table 4: "Triple Difference" Poisson Estimates of the Effect of GBPs on Gun versus Non-Gun Crimes

	(1)	(2)	(3)
Months Following GBP:			
0 to 2 Months	0.010	0.009	-0.008
	(0.017)	(0.016)	(0.014)
0 to 2 Months * Gun Crime	0.068**	0.069**	0.069*
	(0.027)	(0.026)	(0.028)
3 to 5 Months	0.005	0.006	-0.007
	(0.02)	(0.021)	(0.022)
3 to 5 Months * Gun Crime	-0.011	-0.010	-0.014
	(0.018)	(0.018)	(0.019)
6 to 11 Months	0.03	0.033	0.022
	(0.019)	(0.021)	(0.02)
6 to 11 Months * Gun Crime	0.007	0.008	0.002
	(0.017)	(0.016)	(0.017)
12 Months	0.007	0.011	0.026
	(0.008)	(0.008)	(0.015)
12 Months * Gun Crime	-0.006	-0.004	-0.014
	(0.015)	(0.015)	(0.015)
Observations	73,032	73,032	73,032
Controls	-	Yes	Yes
Agency Specific Time Trends	-	-	Yes
Region Specific Year Effects	-	-	Yes

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 5: Sensitivity Tests of Estimated Effects of GBPs on Gun Crimes

	Baseline Specification	Sample: Only Treated Cities	Strongly Balanced Panel 2005-2015	Dropping Obs with ± 2 S.D.	Weighted by Population	Agency-Specific Quadratic Time Trends	Census Division Specific Year Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Months Following GBP:	**Panel I: Difference-in-Differences Estimates**						
0 to 2 Months	0.068**	0.068***	0.081***	0.049*	0.076***	0.081***	0.070**
	(0.024)	(0.019)	(0.024)	(0.020)	(0.020)	(0.020)	(0.025)
3 to 5 Months	-0.007	-0.010	0.038	-0.008	0.013	0.001	-0.003
	(0.031)	(0.029)	(0.026)	(0.027)	(0.023)	(0.027)	(0.029)
6 to 11 Months	0.035	0.021	0.078***	0.033	0.045	0.047	0.035
	(0.026)	(0.028)	(0.022)	(0.026)	(0.026)	(0.025)	(0.024)
≥ 12 Months	0.008	0.001	0.058	-0.003	0.004	0.035	0.002
	(0.026)	(0.035)	(0.030)	(0.024)	(0.029)	(0.029)	(0.027)
Observations	36,516	7,424	18,612	34,925	36,516	36,516	36,516
	Panel II: Difference-in-Difference-in-Differences Estimates						
0 to 2 Months * Gun	0.069*	0.061**	0.050*	0.041	0.057*	0.067*	0.065*
	(0.028)	(0.023)	(0.025)	(0.029)	(0.024)	(0.028)	(0.028)
3 to 5 Months * Gun	-0.014	-0.023	-0.016	-0.022	-0.021	-0.018	-0.017
	(0.019)	(0.017)	(0.018)	(0.018)	(0.013)	(0.019)	(0.019)
6 to 11 Months * Gun	0.002	-0.013	0.006	0.005	-0.007	-0.001	0.002
	(0.017)	(0.018)	(0.013)	(0.016)	(0.015)	(0.016)	(0.016)
≥ 12 Months * Gun	-0.014	-0.031	-0.028	-0.012	-0.033	-0.014	-0.011
	(0.015)	(0.032)	(0.019)	(0.014)	(0.020)	(0.016)	(0.015)
Observations	73,032	14,848	37,224	69,850	73,032	73,032	73,032

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 6: Examination of Spillover Effects of GBPs

	(1)	(2)	(3)
0 to 2 Months			
City GBP	0.070**	0.071**	0.070**
	(0.024)	(0.024)	(0.026)
GBP in County		0.048	0.045
		(0.042)	(0.042)
Border County GBP			0.001
			(0.027)
3 to 5 Months			
City GBP	-0.007	-0.005	-0.005
	(0.030)	(0.031)	(0.031)
GBP in County		-0.015	-0.015
		(0.044)	(0.043)
Border County GBP			0.005
			(0.030)
6 to 11 Months			
City GBP	0.034	0.036	0.036
	(0.026)	(0.026)	(0.027)
GBP in County		0.026	0.024
		(0.046)	(0.045)
Border County GBP			-0.003
			(0.029)
≥12 Months			
City GBP	0.001	0.004	-0.002
	(0.027)	(0.028)	(0.028)
GBP in County		0.033	0.027
		(0.046)	(0.045)
Border County GBP			-0.028
			(0.026)
Observations	36,516	36,516	36,516
Controls?	Yes	Yes	Yes
Region-Specific Year Effects?	Yes	Yes	Yes
Agency-Specific Linear Time Trends?	Yes	Yes	Yes

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 7. Poisson Estimates of GBPs on Gun Crime

Panel I: Violent Gun Crime

	All	Robbery	Aggravated Assault	Murder/ Manslaughter	Rape/Sexual Assault
0 to 2 Months	0.056*	0.070*	0.030	0.005	-0.020
	(0.026)	(0.033)	(0.023)	(0.055)	(0.105)
3 to 5 Months	-0.022	-0.039	0.003	0.002	-0.014
	(0.022)	(0.022)	(0.031)	(0.040)	(0.074)
6 to 11 Months	0.028	0.026	0.044	0.022	-0.002
	(0.019)	(0.018)	(0.025)	(0.029)	(0.101)
≥ 12 Months	0.007	0.008	0.014	0.001	-0.004
	(0.014)	(0.016)	(0.018)	(0.022)	(0.040)
Observations	36,516	36,504	36,516	35,546	33,759
Mean DV:	18.48	9.587	8.281	0.493	0.214

Panel II: Non-Violent Gun Crime

	All	Weapon Law Violations	Drug/Narcotic Violations	Destruction/ Vand. of Property	Kidnapping/ Abduction	Simple Assault
0 to 2 Months	0.065*	0.065*	0.056*	0.091**	0.158**	0.040
	(0.025)	(0.030)	(0.022)	(0.030)	(0.057)	(0.036)
3 to 5 Months	-0.009	-0.039	0.034	0.011	0.081	-0.025
	(0.027)	(0.035)	(0.028)	(0.049)	(0.069)	(0.053)
6 to 11 Months	0.044	0.028	0.079**	0.020	0.087	-0.013
	(0.025)	(0.029)	(0.025)	(0.042)	(0.066)	(0.049)
≥ 12 Months	0.025	0.011	0.062*	-0.057	-0.016	-0.004
	(0.031)	(0.035)	(0.030)	(0.032)	(0.044)	(0.040)
Observations	36,516	36,504	36,516	35,546	33,759	36516
Mean DV:	7.399	8.421	1.835	0.967	0.405	0.318

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controlsare listed in Table 1. Buybacks are coded using partial months (e.g. a buyback on September 15th for 0 to 2 Months Following will take the value .5 in September, 1 in October, and .5 in November).

Table 8: Heterogeneity in Effect of GBPs on Gun Crime by Characteristics of Arrestees

	Full	Age Group				Gender		Race	
	Sample	<18	18-23	24-35	>35	Males	Females	White	Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
0 to 2 Months	0.070**	-0.003	0.062*	0.043	0.095**	0.062**	0.074*	0.046	0.069**
	(0.024)	(0.030)	(0.030)	(0.026)	(0.034)	(0.023)	(0.032)	(0.046)	(0.023)
3 to 5 Months	-0.007	-0.005	0.011	-0.009	-0.004	-0.015	0.000	-0.065	-0.005
	(0.030)	(0.033)	(0.033)	(0.029)	(0.021)	(0.027)	(0.027)	(0.038)	(0.029)
6 to 11 Months	0.034	0.040	0.035	0.051	0.031	0.035	0.064	0.053	0.027
	(0.026)	(0.044)	(0.029)	(0.026)	(0.037)	(0.025)	(0.034)	(0.041)	(0.027)
>12 Months	0.001	0.030	0.000	0.021	-0.030	-0.008	-0.005	0.003	-0.010
	(0.027)	(0.031)	(0.029)	(0.021)	(0.016)	(0.024)	(0.025)	(0.019)	(0.027)
Observations	36,516	36,516	36,516	36,516	36,516	36,516	36,516	36,516	36,516
Mean DV	25.88	2.64	9.05	6.08	3.81	20.92	2.02	5.13	16.25

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 9: Heterogeneity in Effect of GBPs on Gun Crime by Size of Gun Buyback

Size of GBP	Median		75th Percentile		90th Percentile	
	Nominal	Per Capita	Nominal	Per Capita	Nominal	Per Capita
	(1)	(2)	(3)	(4)	(5)	(6)
0 to 2 Months for						
Larger	0.090***	0.093***	0.108***	0.100***	0.108***	0.106***
	(0.024)	(0.025)	(0.022)	(0.024)	(0.023)	(0.023)
Smaller	0.045	-0.002	0.143	-0.003	0.039	0.070
	(0.058)	(0.087)	(0.077)	(0.078)	(0.110)	(0.055)
Unknown	-0.010	-0.012	-0.077	0.009	0.020	-0.020
	(0.049)	(0.065)	(0.078)	(0.075)	(0.106)	(0.047)
3 to 5 Months for						
Larger	0.053	0.040	0.068	0.059	0.064	0.062
	(0.040)	(0.036)	(0.038)	(0.038)	(0.038)	(0.038)
Smaller	-0.004	-0.120	0.118	-0.128	0.117	-0.049
	(0.070)	(0.081)	(0.106)	(0.083)	(0.163)	(0.054)
Unknown	-0.058	-0.032	-0.135	-0.001	-0.138	-0.004
	(0.060)	(0.060)	(0.101)	(0.073)	(0.160)	(0.050)
6 to 11 Months for						
Larger	0.076	0.078*	0.091*	0.096*	0.087*	0.087*
	(0.040)	(0.038)	(0.037)	(0.039)	(0.037)	(0.037)
Smaller	0.073	-0.013	0.223*	-0.018	0.211	0.005
	(0.068)	(0.072)	(0.102)	(0.058)	(0.126)	(0.058)
Unknown	-0.063	-0.035	-0.175	-0.017	-0.175	-0.002
	(0.063)	(0.049)	(0.097)	(0.046)	(0.126)	(0.049)
≥ 12 Months for						
Larger	0.068	0.059	0.086	0.085	0.073	0.078
	(0.061)	(0.060)	(0.057)	(0.058)	(0.060)	(0.062)
Smaller	-0.030	-0.103	0.156	0.007	0.177	0.001
	(0.078)	(0.098)	(0.104)	(0.075)	(0.128)	(0.075)
Unknown	-0.048	-0.035	-0.149	-0.042	-0.171	-0.027
	(0.069)	(0.067)	(0.095)	(0.067)	(0.123)	(0.073)
Observations	36,516	36,516	36,516	36,516	36,516	36,516

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 10: Synthetic Control Estimates, by GBP City and Post-Treatment Period

	Years Since Initial GBP					
City	0	1	2	3	4	5+
Akron, OH[1,3]	5.00***	8.05*	7.30*	7.21*	17.17***	12.6***
	[0.000]	[0.024]	[0.024]	[0.024]	[0.000]	[0.000]
Battle Creek, MI	0.13	0.26				
	[0.745]	[0.824]				
Cambridge, MA	-0.41	-0.64				
	[0.785]	[0.692]				
Cedar Rapids, IA	1.83	-0.64				
	[0.308]	[0.692]				
Chesapeake, VA	8.81*	1.75	3.01	-0.37	-2.75	-3.56
	[0.030]	[0.455]	[0.227]	[0.788]	[0.227]	[0.227]
Cincinnati, OH[1]	-6.49*	-5.71	-3.19	-3.58		
	[0.034]	[0.069]	[0.241]	[0.284]		
Cleveland, OH	13.32***	-1.8	10.66*	11.42**	17.79***	14.36*
	[0.00]	[0.459]	[0.031]	[0.010]	[0.000]	[0.020]
Columbia, SC[5]	13.03***	-3.07	-1.81	1.57	5.72	12.18
	[0.029]	[0.257]	[0.571]	[0.571]	[0.286]	[0.114]
Columbus, OH	-9.78**	-3.63	-3.39			
	[0.008]	[0.151]	[0.303]			
Davenport, IA	0.67	2.43	2.21	5.33	2.35	-0.16
	[0.588]	[0.176]	[0.235]	[0.059]	[0.529]	[0.824]
Denver, CO	-1.65	-3.98	-3.9	-1.7	0.13	-1.21
	[0.378]	[0.134]	[0.122]	[0.451]	[0.976]	[0.707]
Fall River, MA	5.39***	1.71	0.87	2.82	1.30	2.40
	[0.000]	[0.333]	[0.593]	[0.222]	[0.704]	[0.333]
Greenwich, CT	-1.50	-0.72	-0.84			
	[0.383]	[0.652]	[0.757]			
Haverhill, MA	0.83	0.61	0.73			
	[0.571]	[0.750]	[0.750]			
Jackson, TN	3.00	-3.42	5.82	22.44***	22.56***	13.24*
	[0.227]	[0.216]	[0.125]	[0.000]	[0.000]	[0.023]
Kalamazoo, MI	-1.32	-1.42	5.48	16.74*	18.60**	
	[0.453]	[0.443]	[0.085]	[0.019]	[0.009]	
Lansing, MI1	8.12*	9.38*	12.33*	12.46*		
	[0.026]	[0.026]	[0.017]	[0.043]		
Lowell, MA	-0.39	5.05	-3.26			
	[0.828]	[0.057]	[0.230]			
Lynchburg, VA	-3.43	-0.48	-8.12*	-12.03***	-4.61	-1.06
	[0.097]	[0.790]	[0.048]	[0.000]	[0.161]	[0.613]
Memphis, TN[1]	0.84	8.95*	25.60***	24.76***		
	[0.638]	[0.026]	[0.000]	[0.000]		
Murfreesboro, TN	1.54	2.07				
	[0.412]	[0.265]				
Nashville, TN[1]	-3.79	-0.69				

	Years Since Initial GBP					
City	0	1	2	3	4	5+
	[0.096]	[0.670]				
New Bedford, MA[2]	-4.66	-3.88	-0.57	0.78	-3.35	-6.16
	[0.163]	[0.188]	[0.825]	[0.738]	[0.238]	[0.100]
New Haven, CT[1,5]	5.54	-15.66*	-13.05*	-3.1	-5.85	
	[0.054]	[0.015]	[0.015]	[0.323]	[0.154]	
North Charleston, SC[1,3,4]	-0.63	-16.86*	-17.04*	-10.25*	-14.96*	-0.62
	[0.75]	[0.013]	[0.013]	[0.038]	[0.013]	[0.838]
Norwalk, CT[1]	-3.37	-2.63	-2.83			
	[0.087]	[0.184]	[0.282]			
Pawtucket, RI	-2.74	-3.93	-1.78			
	[0.179]	[0.128]	[0.479]			
Pocatello, ID	-1.85	0.42				
	[0.333]	[1.00]				
Providence, RI[3]	-0.34	3.97	0.82	1.35	-2.57	1.26
	[0.843]	[0.093]	[0.657]	[0.519]	[0.324]	[0.611]
Pueblo, CO	14.13***	14.99***	3.4	6.67*		
	[0.000]	[0.000]	[0.105]	[0.026]		
Sommerville, MA	-0.25	-1.64				
	[0.875]	[0.354]				
Springfield, MA	4.6	4.03	-1.15			
	[0.061]	[0.122]	[0.687]			
Stamford, CT[3,5]	0.32	-0.64	0.59	-0.37	1.51	-3.98
	[0.854]	[0.728]	[0.689]	[0.893]	[0.573]	[0.204]
Waltham, MA	1.37	-0.3				
	[0.329]	[0.871]				
Warwick, RI	-1.05	-0.26	-0.55			
	[0.564]	[0.897]	[0.821]			
Waterloo, IA[1]	2.74***	-0.73	1.12	0.24	-0.64	-0.85
	[0.000]	[0.571]	[0.429]	[0.929]	[0.857]	[0.643]
Wilmington, DE[2]	-2.98	1.15	16.75***	25.97***	37.65***	
	[0.189]	[0.509]	[0.000]	[0.000]	[0.000]	
Worcester, IA[1,2,3,4,5]	0.78	5.09	3.88	2.51	2.44	1.62
	[0.684]	[0.105]	[0.158]	[0.316]	[0.263]	[0.421]

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

^{n} An additional GBP occurred n year(s) after initial GBP

Notes: Measuring gun crime rate per 10,000 population. Up to 6 years of pretreatment data was used to match treated cities to the donor pool using annual gun crime rate per 100,000. Pseudo p-values are reported in brackets using the proportion of placebo agencies with a post treatment root mean squared error (RMSE) greater than the treated city for each given year.

Table 11: Estimated Effect of GBPs on Gun Related Deaths

	Total Deaths	Suicide	Homicide
	(1)	(2)	(3)
≥ 12 Months Before	0.001	0.001	-0.019
	(0.016)	(0.018)	(0.016)
6 to 11 Months Before	-0.018	0.008	-0.041**
	(0.012)	(0.022)	-0.015
3 to 5 Months Before	-0.026	-0.010	-0.036*
	(0.017)	(0.025)	(0.017)
1 to 2 Months Before	-	-	-
0 to 2 Months After	0.007	0.032	-0.016
	(0.012)	(0.024)	(0.018)
3 to 5 Months After	-0.029	-0.015	-0.044**
	(0.017)	(0.026)	(0.015)
6 to 11 Months After	-0.031	-0.009	-0.050*
	(0.016)	(0.021)	(0.021)
≥ 12 Months After	-0.014	0.002	-0.041*
	(0.017)	(0.021)	(0.018)
Observations	272,386	272,386	272,386

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include county fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: County population. Standard errors clustered at the county-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Table 12: Heterogeneity in Effect of GBPs on Total Firearm Related Deaths by Size of Gun Buyback

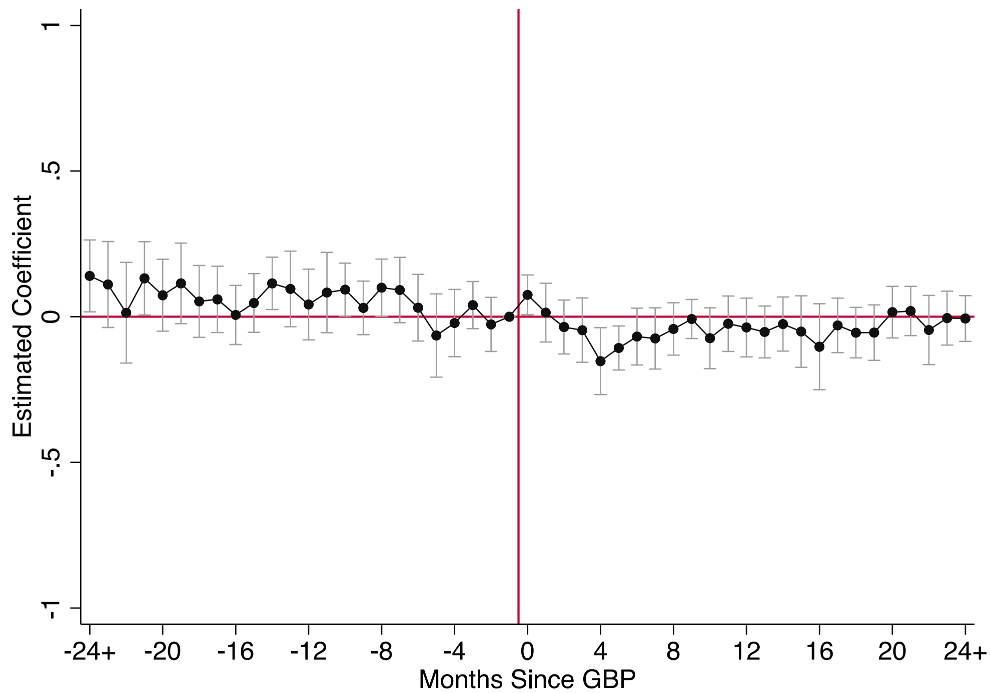
Size of GBP	Median		75th Percentile		90th Percentile	
	Nominal	Per Capita	Nominal	Per Capita	Nominal	Per Capita
	(1)	(2)	(3)	(4)	(5)	(6)
0 to 2 Months for						
Larger	0.035	0.034	0.057*	0.012	0.018	-0.024
	(0.022)	(0.046)	(0.027)	(0.070)	(0.034)	(0.114)
Smaller	-0.039	0.015	-0.027	0.018	0.013	0.016
	(0.039)	(0.033)	(0.031)	(0.025)	(0.021)	(0.019)
Unknown	0.042**	0.044**	0.041**	0.048***	0.047**	0.048***
	(0.016)	(0.016)	(0.015)	(0.014)	(0.016)	(0.014)
3 to 5 Months for						
Larger	0.007	-0.034	0.025	0.015	0.026	0.004
	(0.026)	(0.030)	(0.026)	(0.042)	(0.027)	(0.096)
Smaller	-0.038	0.021	-0.033	-0.009	-0.021	-0.008
	(0.033)	(0.040)	(0.027)	(0.033)	(0.026)	(0.027)
Unknown	-0.017	-0.025	-0.015	0.015	-0.015	-0.014
	(0.021)	(0.021)	(0.022)	(0.042)	(0.022)	(0.021)
6 to 11 Months for						
Larger	-0.036	-0.023	-0.019	-0.053	-0.060	0.004
	(0.031)	(0.042)	(0.039)	(0.049)	(0.063)	(0.096)
Smaller	-0.023	-0.006	-0.037	-0.009	-0.019	-0.008
	(0.029)	(0.030)	(0.022)	(0.020)	(0.020)	(0.027)
Unknown	0.001	-0.007	-0.000	-0.004	0.001	-0.002
	(0.015)	(0.016)	(0.015)	(0.015)	(0.016)	(0.014)
≥ 12 Months for						
Larger	0.004	0.035	0.029	-0.016	0.017	-0.061
	(0.032)	(0.034)	(0.026)	(0.035)	(0.033)	(0.069)
Smaller	-0.064*	-0.045	-0.058*	-0.017	-0.039	-0.019
	(0.029)	(0.038)	(0.030)	(0.029)	(0.025)	(0.028)
Unknown	0.023	0.038	0.025	0.033	0.026	0.031
	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)
Observations	272,386	272,386	272,386	272,386	272,386	272,386

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

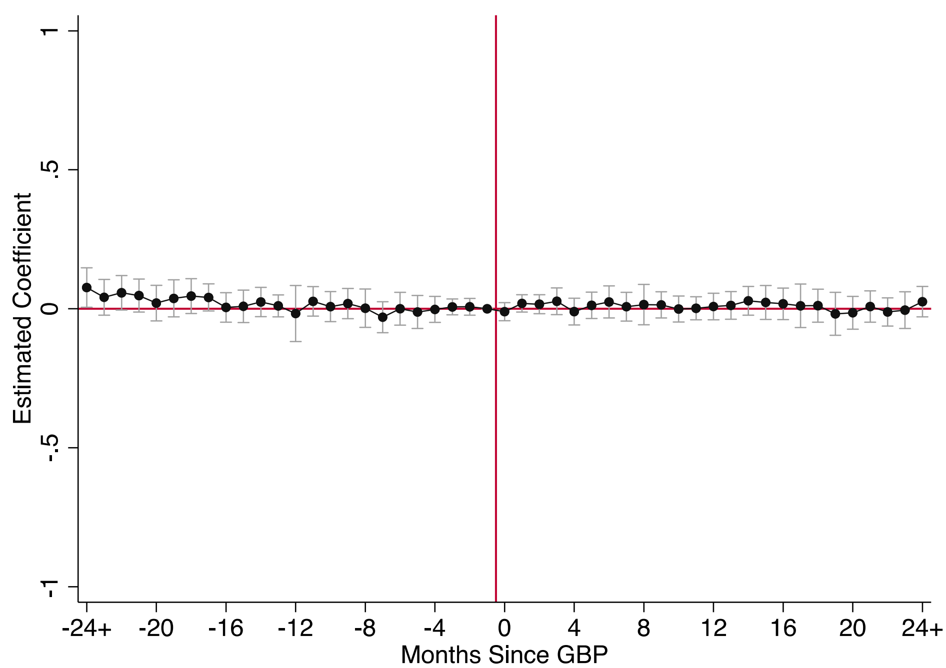
Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include 12 or more months lead, 6 to 11 month lead, and 3 to 5 month lead for each size of the gun buyback, as well as county fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: County population. Standard errors clustered at the county-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Appendix Figure 1: Two-Way Fixed Effects Event-Study Analysis of Effect of GBP on Inverse Hyperbolic Sine of Crime, Controlling for Population

(a) Gun Crime

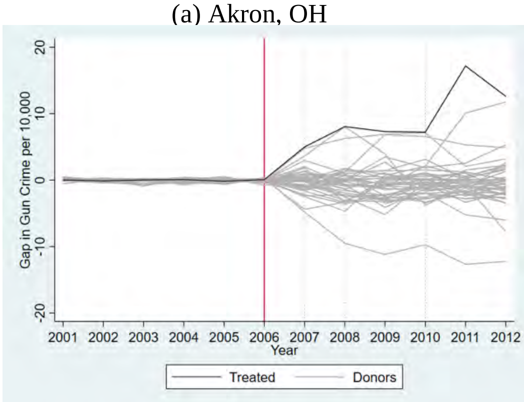


(b) Non-Gun Crime

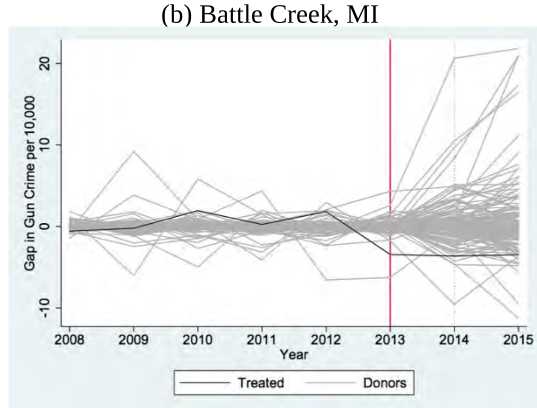


Appendix Figure 2: Gun Crime Rate Gaps in Treatment and Placebo Gaps in Donor Cities (I)

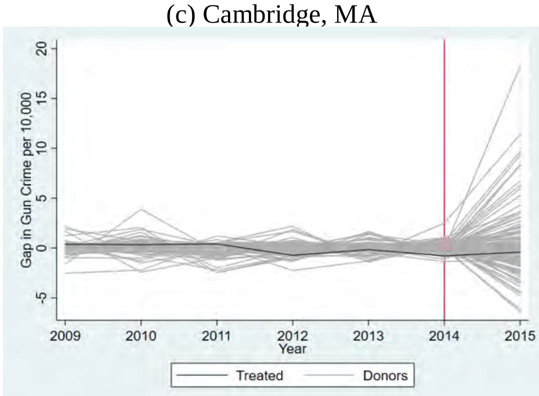
(a) Akron, OH



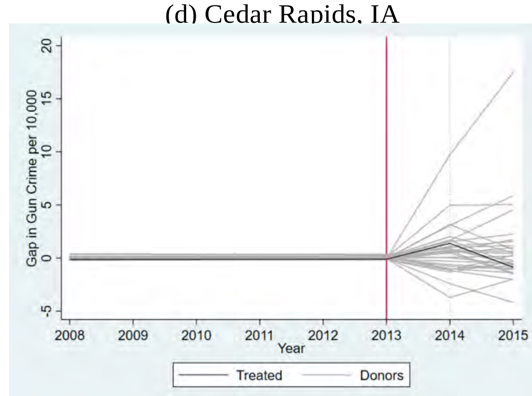
(b) Battle Creek, MI



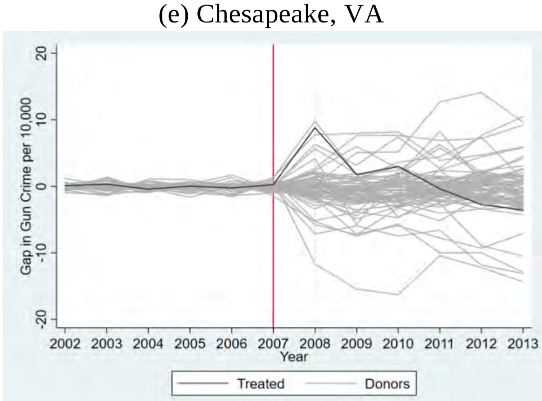
(c) Cambridge, MA



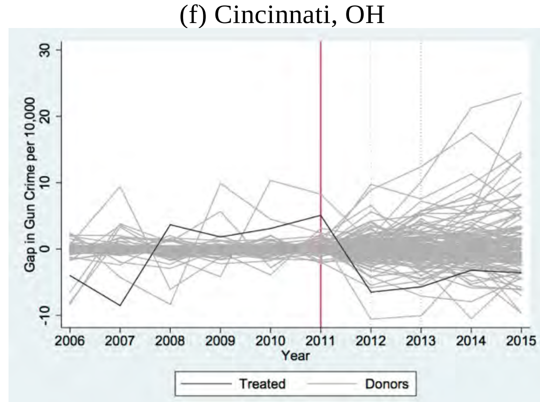
(d) Cedar Rapids, IA



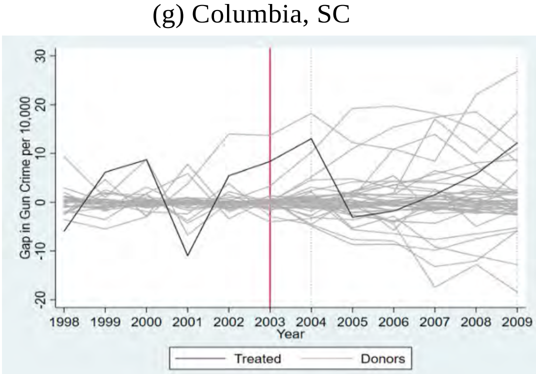
(e) Chesapeake, VA



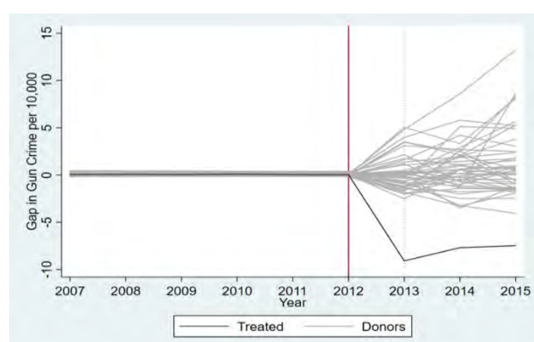
(f) Cincinnati, OH



(g) Columbia, SC

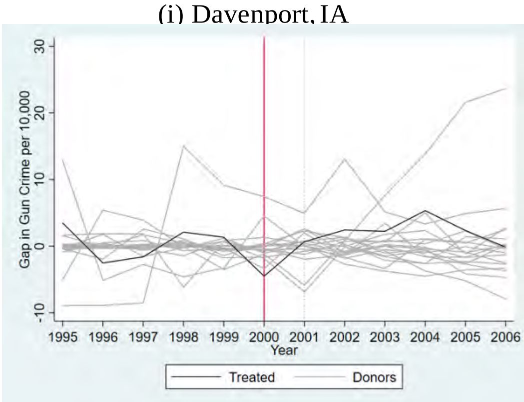


(h) Columbus, OH

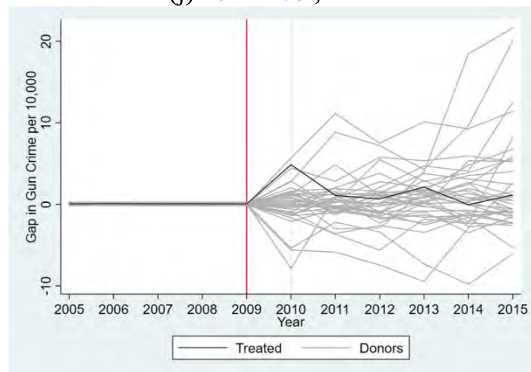


Appendix Figure 2, Continued

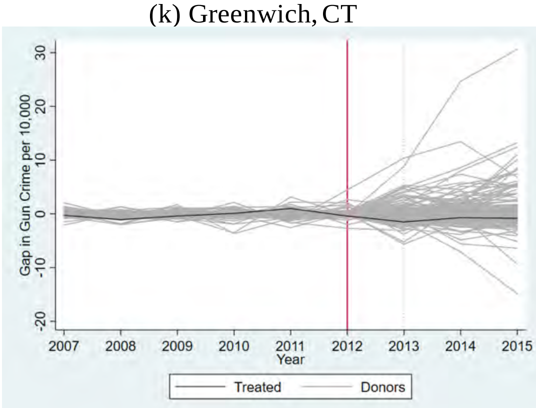
(i) Davenport, IA



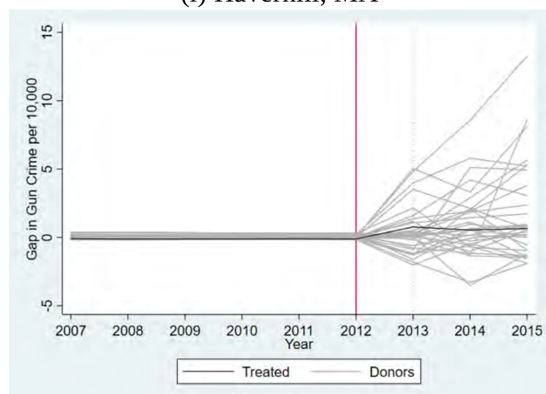
(j) Fall River, MA



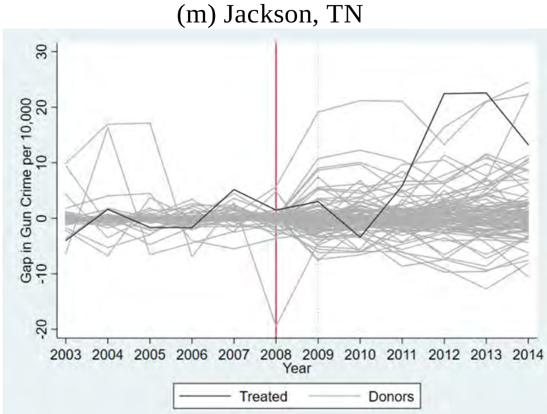
(k) Greenwich, CT



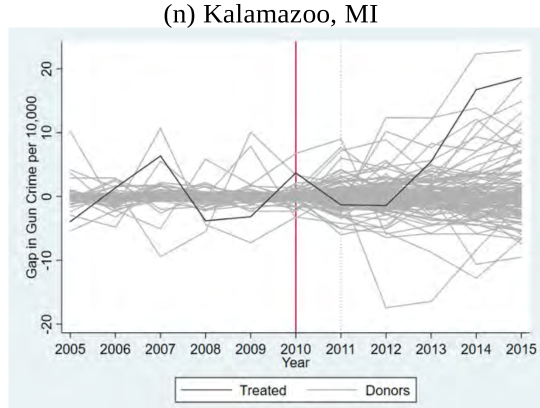
(l) Haverhill, MA



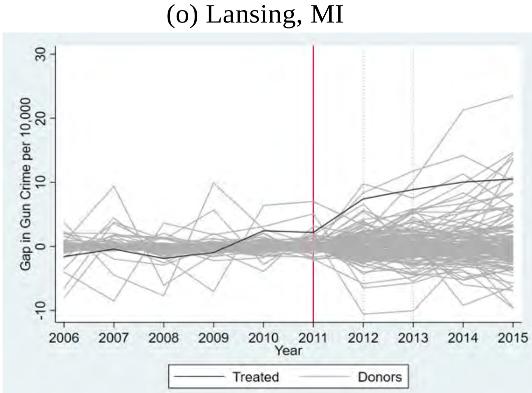
(m) Jackson, TN



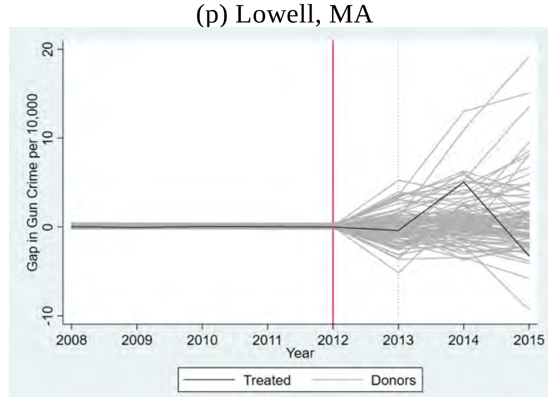
(n) Kalamazoo, MI



(o) Lansing, MI

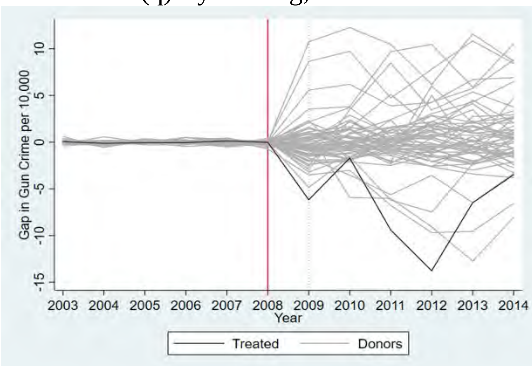


(p) Lowell, MA

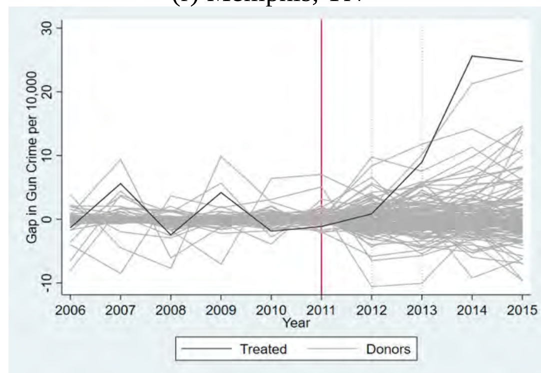


Appendix Figure 2, Continued

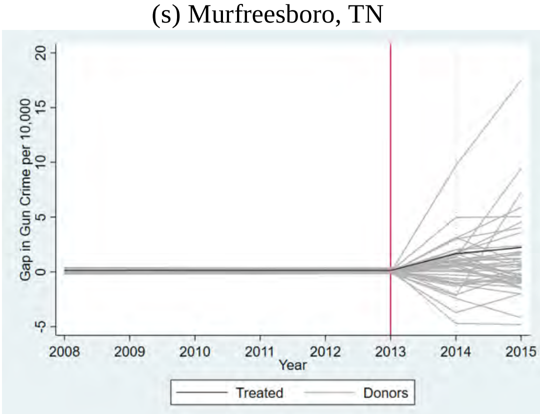
(q) Lynchburg, VA



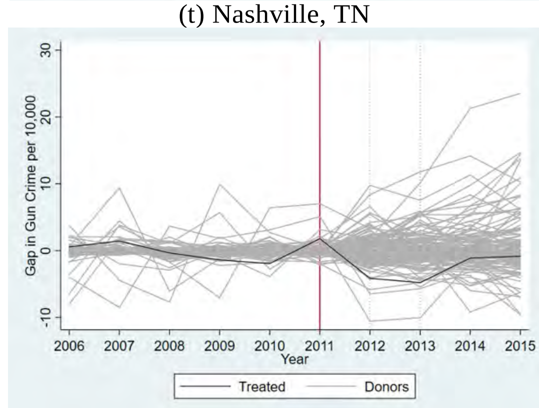
(r) Memphis, TN



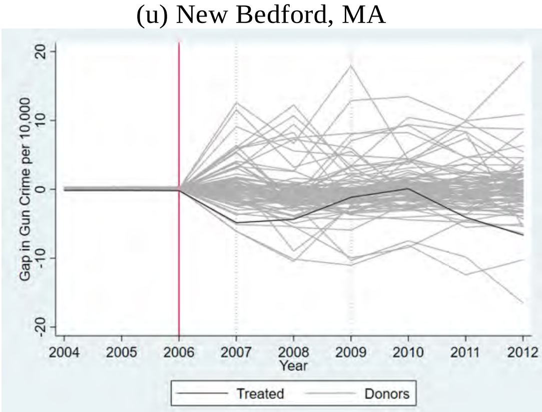
(s) Murfreesboro, TN



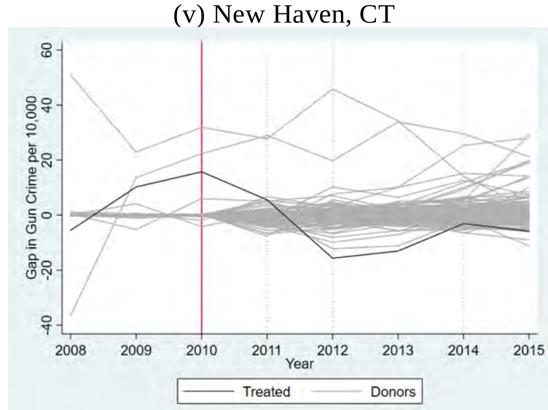
(t) Nashville, TN



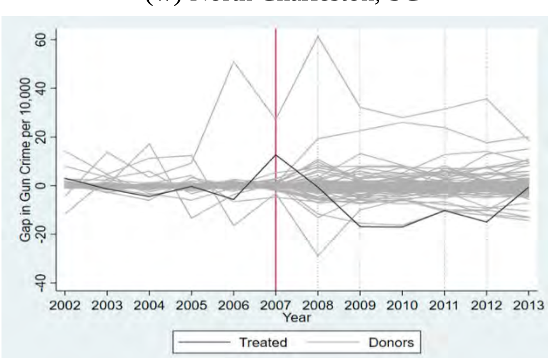
(u) New Bedford, MA



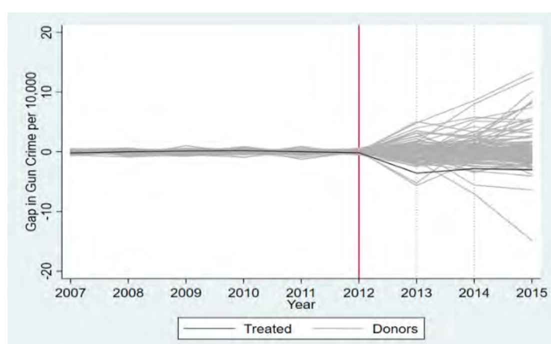
(v) New Haven, CT



(w) North Charleston, SC

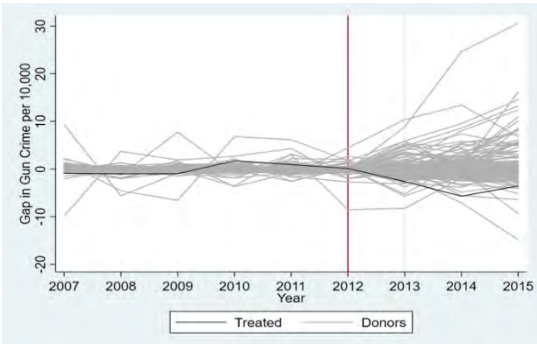


(x) Norwalk, CT

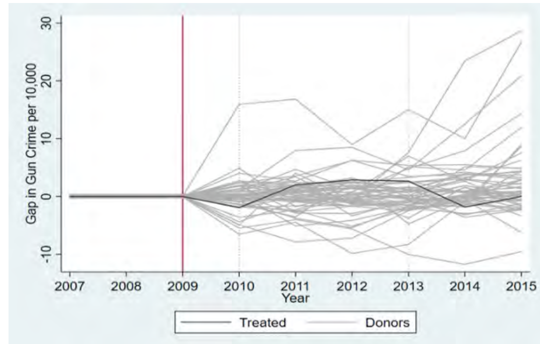


Appendix Figure 2, Continued

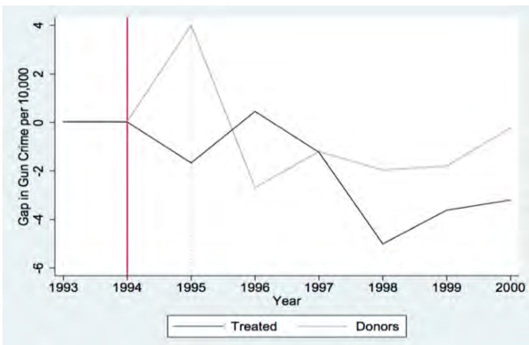
(y) Pawtucket, RI



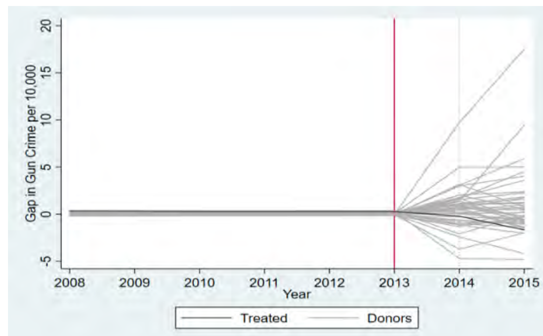
(z) Providence, RI



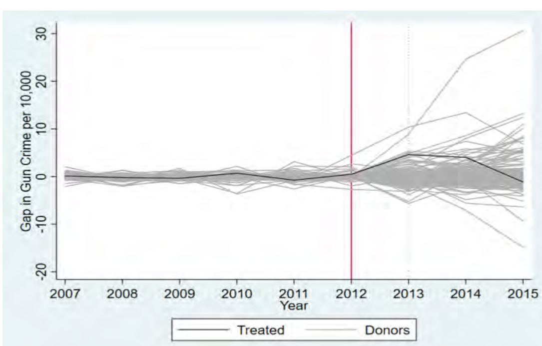
(aa) Pocatello, ID



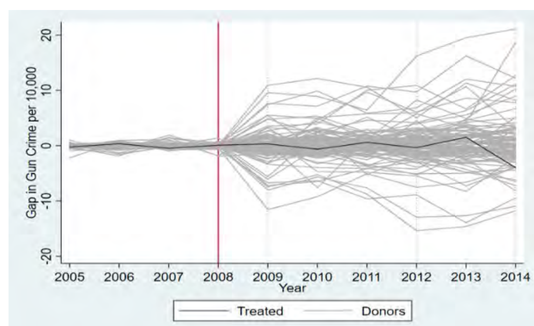
(bb) Sommerville, MA



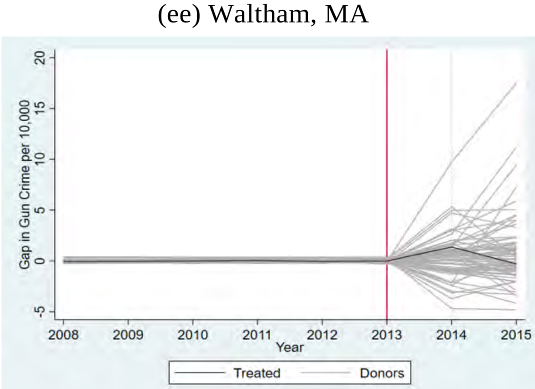
(cc) Springfield, OR



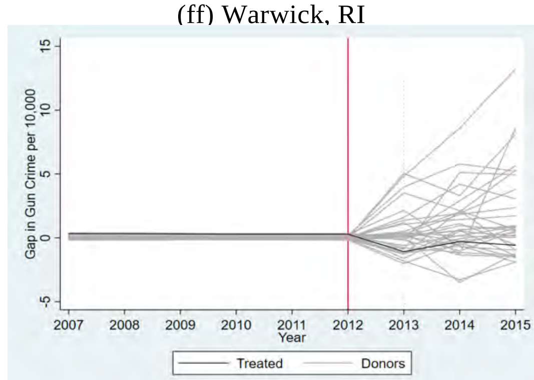
(dd) Stamford, CT



(ee) Waltham, MA

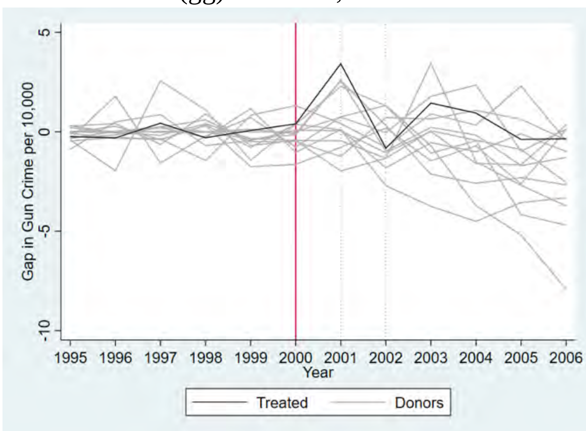


(ff) Warwick, RI

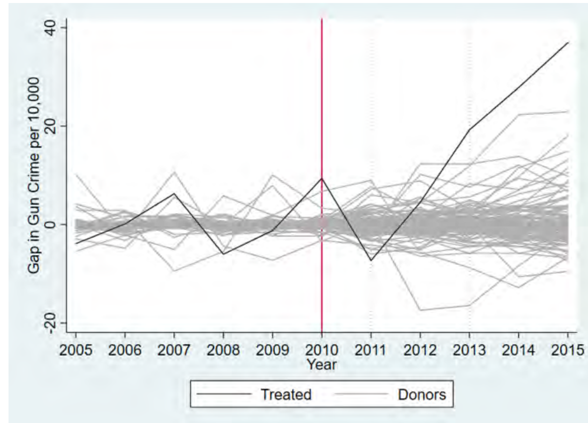


Appendix Figure 2, Continued

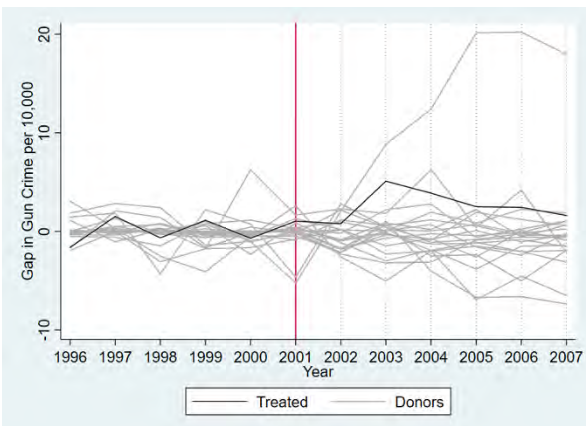
(gg) Waterloo, IA



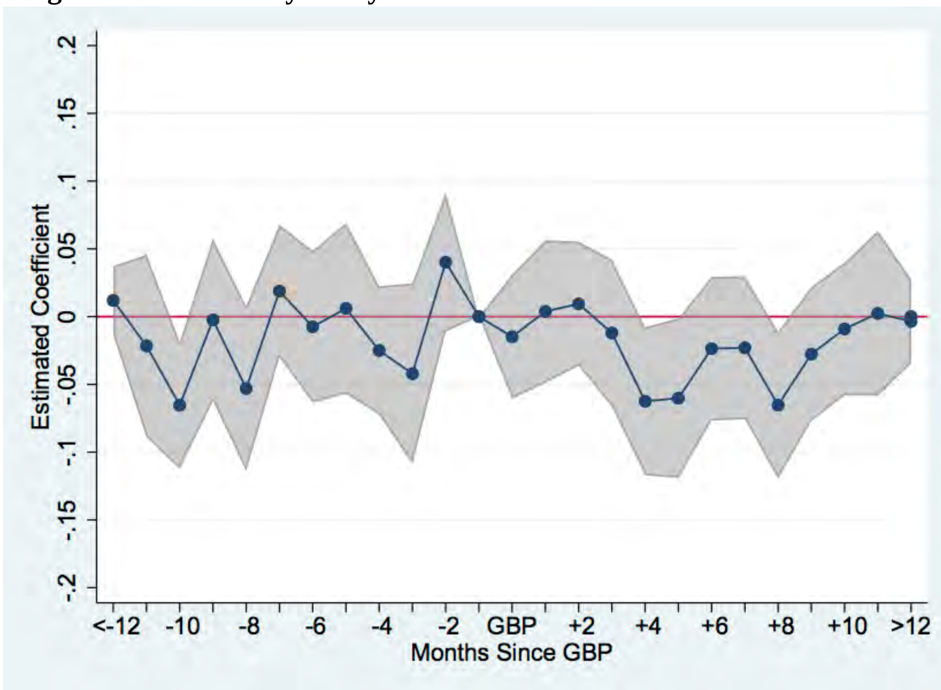
(hh) Wilmington, DE



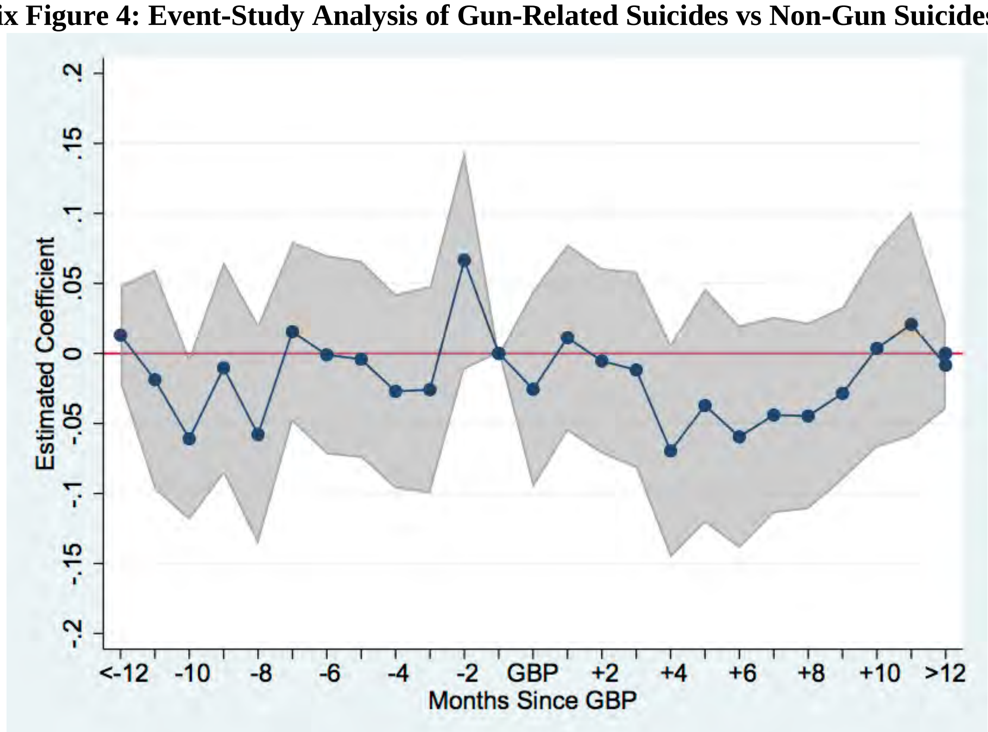
(ii) Worcester, MA



Appendix Figure 3: Event-Study Analysis of Gun-Related Deaths vs Non-Gun Deaths (NVSS)

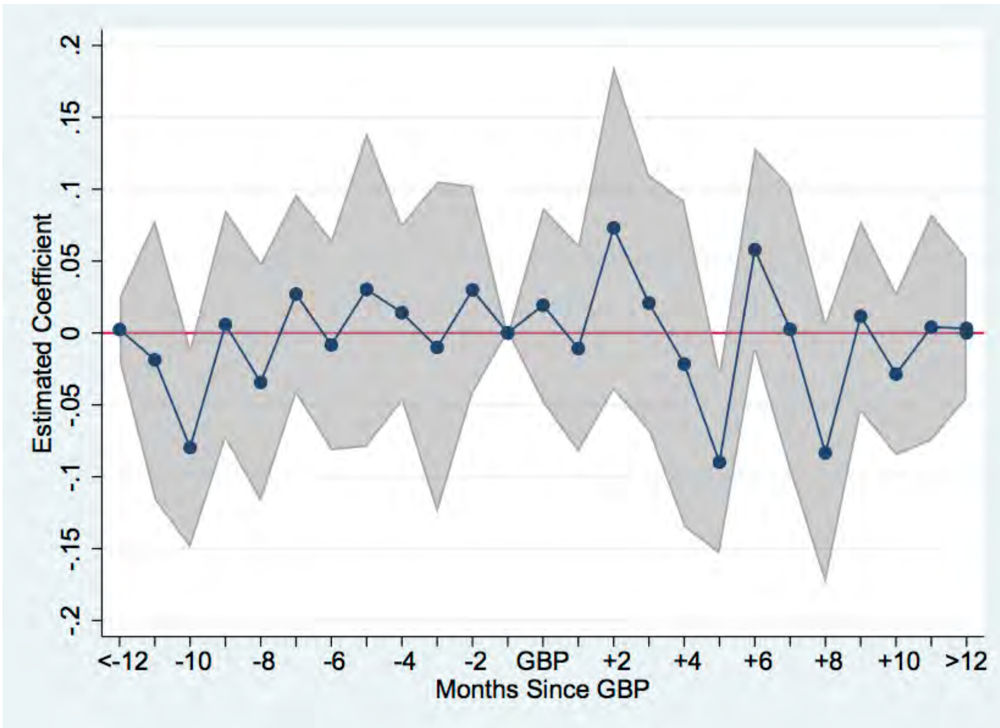


Appendix Figure 4: Event-Study Analysis of Gun-Related Suicides vs Non-Gun Suicides (NVSS)



Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the county-level.

Appendix Figure 5: Event-Study Analysis of Gun-Related Homicides vs Non-Gun Homicides (NVSS)



Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include all socioeconomic, demographic, and policy controls as well as region-specific time effects and agency-specific linear time trends. The gray area represents 95% confidence interval. The standard errors are clustered at the county-level.

Appendix Table 1: Means of Counts of Gun Crimes, by Violent and Non-Violent Gun Crime, Race/Ethnicity, Gender, and Age

Crime that Involved Firearm	Full	Hispanic	White	Black	Male	Female	Age ≤ 17	Age 18 to 23	Age 25 to 34	Age > 35
Violent										
Robbery	9.587	0.108	1.201	6.937	8.201	0.486	1.037	3.479	2.478	0.662
Aggravated Assault	8.281	0.213	1.761	4.739	6.223	0.781	0.738	2.172	2.245	1.352
Murder/Non-Negligent Manslaughter	0.493	0.016	0.076	0.281	0.350	0.031	0.039	0.152	0.132	0.068
Forcible Rape	0.149	0.002	0.028	0.105	0.139	0.003	0.008	0.034	0.056	0.029
Forcible Sodomy	0.029	0.000	0.005	0.021	0.027	0.001	0.002	0.007	0.012	0.006
Forcible Fondling	0.026	0.001	0.006	0.016	0.023	0.001	0.003	0.007	0.007	0.005
Sexual Assault with An Object	0.009	0.000	0.003	0.004	0.008	0.001	0.001	0.002	0.003	0.002
Nonviolent										
Weapon Law Violations	8.421	0.430	2.237	4.896	6.869	0.800	0.968	2.774	2.686	1.420
Drug/Narcotic Violations	1.835	0.148	0.571	1.308	1.768	0.339	0.170	0.836	0.891	0.373
Destruction/Damage/Vandalism of Property	0.967	0.024	0.167	0.391	0.548	0.068	0.088	0.233	0.189	0.074
Kidnapping/Abduction	0.405	0.013	0.091	0.277	0.368	0.034	0.025	0.127	0.156	0.066
Simple Assault	0.318	0.020	0.106	0.202	0.296	0.059	0.039	0.115	0.133	0.080
Stolen Property Offenses	0.181	0.018	0.056	0.126	0.176	0.025	0.033	0.086	0.072	0.027
Intimidation	0.146	0.007	0.053	0.086	0.131	0.019	0.018	0.047	0.049	0.038
Drug Equipment Violations	0.475	0.047	0.243	0.245	0.451	0.130	0.033	0.187	0.247	0.138
Burglary/Breaking and Entering	0.391	0.013	0.080	0.271	0.350	0.032	0.042	0.148	0.129	0.042
All other Larceny	0.096	0.006	0.032	0.056	0.085	0.014	0.014	0.034	0.034	0.016
Justifiable Homicide	0.027	0.000	0.015	0.012	0.026	0.002	0.000	0.004	0.013	0.011
Motor Vehicle Theft	0.081	0.003	0.018	0.054	0.071	0.008	0.013	0.033	0.025	0.008
Shoplifting	0.030	0.002	0.013	0.016	0.027	0.007	0.006	0.011	0.011	0.006
False Pretenses/Swindle/Confidence Game	0.030	0.001	0.007	0.024	0.029	0.005	0.005	0.014	0.012	0.006
Counterfeiting/Forgery	0.025	0.004	0.010	0.015	0.024	0.004	0.002	0.011	0.012	0.006
Theft From Motor Vehicle	0.034	0.002	0.013	0.017	0.029	0.003	0.005	0.013	0.011	0.004
Impersonation	0.025	0.003	0.010	0.015	0.024	0.004	0.004	0.009	0.011	0.005
Theft From Building	0.022	0.001	0.008	0.012	0.019	0.003	0.004	0.006	0.008	0.004
Extortion/Blackmailing	0.010	0.000	0.002	0.005	0.007	0.001	0.001	0.003	0.003	0.002
All Gun Crime:	25.88	0.71	5.13	16.25	20.92	2.02	2.64	8.23	7.33	3.42

Notes: Hispanic is not mutually exclusive, includes white Hispanic, black Hispanic, and other Hispanic. Note: Not all nonviolent crimes are shown, only the 20 most common for the full sample

Appendix Table 2: Estimates of the Effect of a GBP on Gun-Related Crime, Using Annual Data, OLS and Poisson Models

	Poisson	OLS	OLS	OLS
	Count of Gun Crime	Count of Gun Crime	Gun Crime rate per 10,000	Log Gun Crime rate per 10,000
Years Following GBP:	(1)	(2)	(3)	(4)
0 to 1 Years	0.038*	-76.501	-0.544	0.013
	(0.019)	(47.658)	(0.965)	(0.032)
2 to 3 Years	0.018	-108.876	-0.523	-0.012
	(0.020)	(80.869)	(1.191)	(0.025)
4 to 5 Years	-0.034	-125.990	-0.513	-0.005
	(0.030)	(89.849)	(1.182)	(0.038)
> 5 Years	-0.007	-8.430	0.148	0.022
	(0.024)	(39.768)	(0.633)	(0.028)
Observations	2,968	2,968	2,968	2,962
Mean DV:	313.55	313.55	17.94	-0.05

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Year data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and year fixed effects. Poisson regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses. Linear time trends are at the annual level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Appendix Table 3: Poisson Estimates of the Effect of GBP, Splitting Violent and Non-Violent Crime

	Violent			Non-Violent		
	All	Robbery	Aggravated Assault	All	Weapon Law Violations	Drug/Narcotic Violations
	(1)	(2)	(3)	(4)	(5)	(6)
0 to 2 Months	0.065*	0.065*	0.056*	0.091**	0.158**	0.040
	(0.025)	(0.030)	(0.022)	(0.030)	(0.057)	(0.036)
3 to 5 Months	-0.009	-0.039	0.034	0.011	0.081	-0.025
	(0.027)	(0.035)	(0.028)	(0.049)	(0.069)	(0.053)
6 to 11 Months	0.044	0.028	0.079**	0.020	0.087	-0.013
	(0.025)	(0.029)	(0.025)	(0.042)	(0.066)	(0.049)
≥ 12 Months	0.025	0.011	0.062*	-0.057	-0.016	-0.004
	(0.031)	(0.035)	(0.030)	(0.032)	(0.044)	(0.040)

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: Agency-by-Month data for regressions are drawn from the 1991 to 2015 National Incident-Based Reporting System (NIBRS). All regressions include agency fixed effects and month-by-year fixed effects. All columns include socioeconomic controls, demographic controls, and gun policy and crime controls listed in Table 1, as well as region-specific year effects and agency-specific time trends. Regressions are estimated via poisson with exposure proxy: Agency population. Standard errors clustered at the city-level are shown in parentheses.

Appendix Table 4: Donor Cities that Received Positive Weights

City	Weight	City	Weight
Akron, OH		Battle Creek, MI	
Lawrence, KS	0.499	Tyler, TX	0.436
Southfield, MI	0.181	Clarksville, TN	0.35
Peabody, MA	0.115	North Little Rock, AR	0.129
Hamden, CT	0.101	Saginaw, MI	0.06
Saginaw, MI	0.082	Youngstown, OH	0.024
Cambridge, MA		Cedar Rapids, IA	
Lawrence, KS	0.499	Missoula, MT	0.338
Southfield, MI	0.181	Dubuque, IA	0.245
Peabody, MA	0.115	Medford, OR	0.08
Hamden, CT	0.101	Brookline, MA	0.066
Saginaw, MI	0.082	Owensboro, KY	0.04
Chesapeake, VA		Cincinnati, OH	
Johnson City, TN	0.391	Chattanooga, TN	0.77
Amarillo, TX	0.314	Rockford, IL	0.23
Layton, UT	0.207		
Chattanooga, TN	0.064		
Greenville, SC	0.024		
Columbia, SC		Columbus, OH	
Greenville, SC	0.856	Brockton, MA	0.413
Dayton, OH	0.144	Rockford, IL	0.251
		Lynn, MA	0.224
		Saginaw, MI	0.111
Davenport, IA		Fall River, MA	
Des Moines, IA	0.485	Sterling Heights, MI	0.336
Nampa, ID	0.449	Youngstown, OH	0.08
Council Bluffs, IA	0.043	Greeley, CO	0.056
Provo, UT	0.023	Redford Township, MI	0.038
		Conroe, TX	0.021
Greenwich, CT		Haverhill, MA	
Fairfield, CT	0.868	Wyoming, MI	0.225
Novi, MI	0.132	Royal Oak, MI	0.128
		Denton, TX	0.115
		Fairfield, CT	0.115
		Cranston, RI	0.102
Jackson, TN		Kalamazoo, MI	
Knoxville, TN	0.411	Knoxville, TN	0.365
Norfolk, VA	0.373	Brockton, MA	0.268
Saginaw, MI	0.215	Amarillo, TX	0.211
		Mount Pleasant, SC	0.117
		Dearborn, MI	0.039

Lansing, MI	
Brockton, MA	0.707
Tyler, TX	0.152
Rockford, IL	0.1
Rapid City, SD	0.041
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Lowell, MA	
Bend, OR	0.344
Owensboro, KY	0.25
Novi, MI	0.197
Hoover, AL	0.125
Rockford, IL	0.084

Lynchburg, VA	
Dearborn Heights, MI	0.259
Greenville, SC	0.22
Knoxville, TN	0.17
Grand Rapids, MI	0.145
Rock Hill, SC	0.104

Memphis, TN	
Saginaw, MI	0.479
Richmond, VA	0.331
North Little Rock, AR	0.189I

Murfreesboro, TN	
Ogden, UT	0.322
Hamden, CTI	0.1
Waterford Township, MI	0.085
Little Rock, AR	0.068
North Little Rock, AR	0.047

Nashville, TN	
Richmond, VA	0.413
Charleston, WV	0.269
Saginaw, MI	0.11
Suffolk, VA	0.097
Norfolk, VA	0.09

New Bedford, MA	
Ames, IA	0.37
Flower Mound, TX	0.244
Missoula, MT	0.186
Grand Forks, ND	0.084
St. George, UT	0.06

New Haven, CT	
Saginaw, MI	0.882
Little Rock, AR	0.118

North Charleston, SC	
Saginaw, MI	0.56
Greenville, SC	0.173
Norfolk, VA	0.134
Richmond, VA	0.126
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Norwalk, CT	
Sterling Heights, MI	0.578
Lynn, MA	0.209
Livonia, MI	0.141
Brockton, MA	0.07
Taylor, MI	0.003

Pawtucket, RI	
Livonia, MI	0.344
Redford Township, MI	0.317
Rapid City, SD	0.305
Rockford, IL	0.034

Providence, RI	
Tyler, TX	0.934
Chattanooga, TN	0.056
North Little Rock, AR	0.01
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Pocatello, ID	
Provo, UT	0.409
Des Moines, IN	0.116
West Jordan, UT	0.069
Boise, ID	0.065
Sioux City, IA	0.061

Sommerville, MA	
Corvallis, OR	0.391
Brookline, MA	0.235
Victoria, TX	0.152
West Hartford, CT	0.074
Flower Mound, TX	0.05

Springfield, OR	
Medford, OR	0.451
Chattanooga, TN	0.321
Rockford, IL	0.118
Richmond, VA	0.11

Stamford, CT	
Sioux Falls, SD	0.711
Bismarck, ND	0.171
Redford Township, MI	0.104
Saginaw, MI	0.015

Waltham, MA	
Fairfield, CT	0.316
Plymouth, MA	0.259
Brookline, MA	0.252
Shelby Township, MI	0.133
Novi, MI	0.038
Waterloo, IA	
Fargo, ND	0.434
Greenville, SC	0.222
Charleston, SC	0.159
Saint George, UT	0.125
Plymouth, MA	0.057
Worcester, MA	
Des Moines, IA	0.447
Sandy, UT	0.298
Council Bluffs, IA	0.122
Nampa, ID	0.077
Sioux City, IA	0.056

Warwick, RI	
Ames, IA	0.37
Flower Mound, TX	0.244
Missoula, MT	0.186
Grand Forks, ND	0.084
St. George, UT	0.06
Wilmington, DE	
Chattanooga, TN	0.517
Saginaw, MI	0.483

Appendix Table 5: Heterogeneity in Effect of GBPs on Firearm Related Suicide by Size of Gun Buyback

Size of GBP	Median		75th Percentile		90th Percentile	
	Nominal	Per Capita	Nominal	Per Capita	Nominal	Per Capita
	(1)	(2)	(3)	(4)	(5)	(6)
0 to 2 Months for						
Larger	0.048	0.054	0.058	0.040	-0.031	0.182
	(0.027)	(0.047)	(0.030)	(0.064)	(0.027)	(0.137)
Smaller	-0.035	0.004	-0.014	0.013	0.034	0.012
	(0.039)	(0.037)	(0.037)	(0.029)	(0.034)	(0.028)
Unknown	0.055	0.056	0.053	0.057*	0.058*	0.059*
	(0.030)	(0.029)	(0.029)	(0.027)	(0.026)	(0.027)
3 to 5 Months for						
Larger	0.047	-0.017	0.042	0.019	0.048	0.052
	(0.033)	(0.040)	(0.035)	(0.056)	(0.029)	(0.143)
Smaller	-0.002	0.055	0.017	0.027	0.016	0.026
	(0.043)	(0.044)	(0.042)	(0.039)	(0.035)	(0.036)
Unknown	-0.046*	-0.052	-0.046*	-0.042	-0.047*	-0.043
	(0.023)	(0.027)	(0.023)	(0.022)	(0.022)	(0.022)
6 to 11 Months for						
Larger	0.037	0.027	0.049	0.016	-0.021	0.047
	(0.026)	(0.038)	(0.032)	(0.052)	(0.030)	(0.129)
Smaller	-0.013	0.022	-0.004	0.023	0.024	0.018
	(0.025)	(0.029)	(0.023)	(0.023)	(0.023)	(0.022)
Unknown	-0.020	-0.018	-0.019	-0.017	-0.015	-0.016
	(0.025)	(0.024)	(0.025)	(0.024)	(0.025)	(0.024)
≥ 12 Months for						
Larger	0.054*	0.054	0.043	0.021	0.040*	0.061
	(0.025)	(0.029)	(0.023)	(0.034)	(0.019)	(0.093)
Smaller	-0.032	0.003	-0.001	0.020	0.008	0.015
	(0.024)	(0.024)	(0.021)	(0.021)	(0.020)	(0.020)
Unknown	-0.006	0.004	-0.002	0.001	-0.002	0.002
	(0.025)	(0.021)	(0.023)	(0.021)	(0.021)	(0.021)
Observations	272,386	272,386	272,386	272,386	272,386	272,386

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include 12 or more months lead, 6 to 11 month lead, and 3 to 5 month lead for each size of the gun buyback as well as county fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: County population. Standard errors clustered at the county-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.

Appendix Table 6: Heterogeneity in Effect of GBPs on Firearm Related Homicide by Size of Gun Buyback

Size of GBP	Median		75th Percentile		90th Percentile	
	Nominal	Per Capita	Nominal	Per Capita	Nominal	Per Capita
	(1)	(2)	(3)	(4)	(5)	(6)
0 to 2 Months for						
Larger	0.008	0.005	0.032	-0.020	0.012	-0.126
	(0.028)	(0.065)	(0.039)	(0.098)	(0.046)	(0.118)
Smaller	-0.050	0.017	-0.043	0.013	-0.010	0.006
	(0.053)	(0.034)	(0.041)	(0.027)	(0.028)	(0.019)
Unknown	0.022	0.022	0.022	0.030	0.029	0.028
	(0.026)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)
3 to 5 Months for						
Larger	-0.032	-0.047	-0.009	0.018	-0.021	-0.001
	(0.027)	(0.046)	(0.031)	(0.049)	(0.037)	(0.112)
Smaller	-0.072	-0.009	-0.076*	-0.042	-0.051	-0.043
	(0.041)	(0.043)	(0.031)	(0.030)	(0.030)	(0.024)
Unknown	0.001	-0.010	0.004	0.001	0.004	0.003
	(0.043)	(0.037)	(0.042)	(0.040)	(0.042)	(0.039)
6 to 11 Months for						
Larger	-0.097*	-0.060	-0.085	-0.091	-0.111	0.032
	(0.044)	(0.058)	(0.051)	(0.058)	(0.086)	(0.084)
Smaller	-0.031	-0.026	-0.064	-0.034	-0.056	-0.072*
	(0.051)	(0.033)	(0.035)	(0.025)	(0.030)	(0.036)
Unknown	0.015	-0.003	0.012	0.002	0.010	0.006
	(0.024)	(0.023)	(0.022)	(0.023)	(0.024)	(0.021)
≥ 12 Months for						
Larger	-0.044	0.014	-0.003	-0.033	-0.027	-0.081
	(0.034)	(0.043)	(0.026)	(0.043)	(0.035)	(0.080)
Smaller	-0.119*	-0.102*	-0.128**	-0.068	-0.091*	-0.071*
	(0.053)	(0.050)	(0.046)	(0.037)	(0.040)	(0.034)
Unknown	0.042	0.064*	0.044	0.060*	0.044	0.053
	(0.027)	(0.030)	(0.027)	(0.029)	(0.029)	(0.029)
Observations	272,386	272,386	272,386	272,386	272,386	272,386

*** Significant at 0.1% level **Significant at 1% level * Significant at 5% level

Notes: County-by-Month data for regressions are drawn from the 1991 to 2015 National Vital Statistics System (NVSS). All regressions include 12 or more months lead, 6 to 11 month lead, and 3 to 5 month lead for each size of the gun buyback as well as county fixed effects and month-by-year fixed effects. Regressions are estimated via poisson with exposure proxy: county population. Standard errors clustered at the county-level are shown in parentheses. Linear time trends are at the monthly level. Demographic, Socioeconomic, and Crime control policy controls are listed in Table 1.