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ABSTRACT

Gun buyback programs (GBPs), which use public funds to purchase civilians' privately-owned firearms, aim to reduce gun violence. However, next to nothing 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 1.1 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. These results call into question the efficacy of city gun buyback programs in their current form.

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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; Luca et al. 2017) and minors (Anderson et al. 2021).

In an effort to limit 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 firearms were turned in by individuals who were "afraid someone would use [the firearm] in anger" or 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, including a flurry of buybacks in 2021. For instance, from April-May 2021, GBPs were held in Reading, PA (Rearden 2021); Albany, Georgia (Godley 2021); Canton, Ohio (Goshay 2021); Philadelphia, Pennsylvania. (Chinchilla 2021); Birmingham, Alabama (Birmingham PD 2021); Rochester, New York (Ly 2021); and Albuquerque, New Mexico (Associated Press 2021), with more planned in June and beyond (Daily Freeman Staff 2021; Milian 2021).[2] GBPs have generally been funded by government dollars at the state and local, rather than federal, level (Mullin 2001).[3] 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. While this legislation was not further pursued by Congressional Democrats during the Trump presidency, the May 2021 introduction of H.R. 3143 to establish a federally funded gun buyback program suggests that this issue has continued salience (Congresss.gov 2021).

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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] In addition, the possibility of expanding gun buybacks in New York City (NYC) took center stage in the 2021 Democratic primary race for NYC mayor. During a debate on May 13, 2021, Democratic candidate Eric Adams claimed, "Gun buybacks don't work to get rid of the illegal guns we need to eliminate" (New York Post 2021). On the other hand, Democratic candidate Kathryn Garcia suggested expanding GBPs by increasing the trade-in value for surrendered firearms from $200 to $2000 per firearm (Rubinstein et al. 2021).

[3] 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 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).

Proponents of GBPs, including New York Governor Andrew Cuomo (2019), former President Bill Clinton (2000), and current President Joe Biden, argue that GBPs may be an important tool in the fight against gun crime and firearm-related violence.[4] Some proponents, including President Biden and Senator Bernie Sanders, have called for a federal GBP that specifically targets assault weapons (Hains 2019).[5] Opponents, including the National Rifle Association (NRA), 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 (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 potential 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 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, 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 cost of owning a firearm, also leading to an increase in newer firearm purchases (Mullin 2001).[6]

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

[5] 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).

[6] Additionally, some buybacks, particularly those that are repeated, could induce a stockpiling of guns among those who fear that repeated buybacks may lead to stricter gun control policies (Mullin 2001).

While policymakers are fiercely debating whether to implement GBPs, little is known about their effectiveness. This paper is the first to present credible causal estimates on the effects of GBPs in the United States. 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 1.2 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).[7] 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).

[7] 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.

Researchers attempting to estimate the relationship between the supply of firearms and gun crime have been limited by a lack of data on firearm ownership, and only a few studies have identified reliable proxies. For instance, Duggan (2001) uses changes in rates of gun magazine sales and NRA membership to proxy for firearm ownership at the county level and finds that increases in firearm ownership are significantly and 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 firearm purchases and finds that background checks are negatively related to property crime, though essentially unrelated to violent crime.

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. 2021), trigger lock requirements (Shuster et al. 2000), and right-to-carry laws (Lott and Mustard 1997; Donohue and Ayers 2009; Donohue et al. 2019). In general, these studies suggest that the supply of available firearms is positively related to crime. [8, 9]

2.2. Gun Buyback Programs

Gun buyback programs achieved worldwide prominence in the mid-1990s with a massive 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).

[8] Sen and Panjamapiro (2012) find that stricter background checks are associated with a decline in homicide and suicide rates among those aged 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 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. Anderson et al. (2018) find that CAP laws reduce the likelihood of gun carrying among teenagers.

[9] 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, Ayres and Donohue (2002) 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 et al. 2005; Durlauf et al. 2016). Finally, Donohue et al. (2019) uses both event-study analyses and synthetic control approaches to find that right-to-carry laws are associated with increases in violent crime rates.

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 implemented one of the largest gun buybacks in history, and the largest ever in terms of percentage of privately-owned firearms relinquished.

In total, the GBP collected over 640,000 firearms, representing 20 percent of privately-owned firearms in Australia and cost taxpayers $230 million (Braga and Wintemute 2013).[10] In terms of firearms per capita, a comparable GBP in the United States would have collected 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, 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). Using a two-way fixed effects model, Leigh and Neill (2010) exploit state-level variation in the number of firearms bought back and 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 their research design failed to isolate the causal effect of the buyback.[11] Taylor and Li (2015) find that Australia's NFA led to decreases in armed robberies and attempted murders relative to sexual assaults, which they argue should be unaffected by changes in Australia's gun supply.

[10] 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).

[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.

In the United States, GBPs have largely occurred at the city level. Typically, these GBPs have destroyed 1,000 or fewer firearms, with city governments paying owners between $25 to $200 per gun (Braga and Wintemute 2013).[12] Only two studies of which we are aware have studied the relationship between U.S. 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 statistically significant decline in gun crime or assault-related firearm injuries. Braga and Wintemute (2013) study 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 it required participants to document Boston residency and specifically targeted high-crime areas for 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 crime reports, including information on offenders, victims, and circumstances of the crime. Approximately 29 percent of the U.S. population is covered by the NIBRS, a population that is 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 United States.

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. Vital for our analyses, these data include information on whether a firearm was used in the commission of a crime.[14]

[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 were in working condition. The mean participant age was 51.

[14] There are limitations of the NIBRS to note. In contrast to the Uniform Crime Reports (UCR), geographic coverage of the NIBRS is limited. While the NIBRS collects information from 37 states, only 15 states report crime data from all

Our main dependent variable is *Gun Crime*, which is an agency-by-month count of crimes involving a handgun, shotgun, automatic weapon, or long gun. As shown in Table 1, between 3 and 4 percent of all crimes involve the use of a firearm. Seventy-two percent of firearm-related offenses were violent in nature (Appendix Table 1).[15] In Appendix Table 1, we show means of the dependent variable by gender, age, and race/ethnicity of the offender. Males, Blacks, and those under the age of 35 are disproportionately likely to be arrested in connection with a gun crime.

3.2. National Vital Statistics System 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 homicides and suicides for the period 1991-2015. We restrict our sample to counties that have at least one city with a population of 50,000 or greater to ensure that we have identified all GBP enactment dates. Our main dependent variables from these data are *Firearm Death, Firearm Homicide,* and *Firearm Suicide*, which represent county-level total firearm deaths, firearm-related homicides, and firearm-related suicides, respectively.

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

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of their policing agencies. The Midwest and North Central regions have a high NIBRS participation rate among their policing agencies while coverage in the West is sparser.

[15] We use definition the FBI's definition of a violent crime, which includes robbery, aggravated assault, murder/non-negligent manslaughter, and forcible sex offenses. Non-violent firearm-related crimes predominantly consist of weapon law violations.

of gift cards for gas and/or 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 contribute identifying variation in our NIBRS analysis.[16]

3.4. NIBRS Analysis

Using data from the 1991-2015 NIBRS and a difference-in-differences (DD) framework, we estimate the following Poisson regression:[17]

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

where *Gun Crime* measures criminal offenses involving a firearm in law enforcement agency α in county *c* and state *s* during month-by-year *t*, and κ proxies exposure using agency-level population served.[18] Our key right hand-side variables are a set of binary indicators for the following time periods after the enactment of a GBP: 0 to 2 months after (*GBP0to2*), 3 to 5 months after

[16] The sources used to identify each GBP are available from the authors upon request.

[17] See Osgood (2000) for a discussion of the benefits to using a Poisson regression when analyzing crime data. Modelling crime data via ordinary least squares (OLS) introduces the following two problems: (i) the 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 using only positive values. The results presented below, however, are qualitatively similar if we define the dependent variable as a rate (or the natural log of a rate) and estimate equation (1) with OLS.

[18] 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 that 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 types of agencies are in our sample. The NIBRS compiles agency-level population at the year level.

(*GBP3to5*), 6 to 11 months after (*GBP6to11*), and 12 or more months after (*GBP12More*). Agency and month-by-year fixed effects are denoted by σ_α and Ψ_t, respectively.

The vector $\boldsymbol{X_{cst}}$ includes controls for demographics, socioeconomic and political conditions, and gun-related policies and policing resources at the county and state levels. Demographic controls are measured at the county level and include the percent of the population with a bachelor's degree or higher, the percent of the young adult population (i.e., 15-to-19 and 20-to-29 years of age), the percent male, and the percent by race/ethnicity (i.e., White, Black, and Hispanic). 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 Democratic party. Finally, gun-related policies and policing resources 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 locks be sold alongside firearms, and a law requiring a minimum gun purchase age of 18.[19] We also control for police officers 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 94 GBPs held across 43 cities with populations greater than 50,000. Table 2 lists each city's gun buyback program, the date of the initiative, and the number of guns sold.[20] 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.

The credibility of our identification strategy relies on whether the parallel trends assumption holds. 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 experiment with adding agency-specific linear time trends ($\sigma_\alpha * t$) and census

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[19] Population distribution and per capita income come from the Surveillance, Epidemiology, and End Results (SEER). Educational attainment rates come from the American Community Survey (ACS), and unemployment data come from the Bureau of Labor Statistics (BLS). Background check counts come from the National Instant Criminal Background Check System (NICS), state CAP law policies are taken from Anderson and Sabia (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 come from the Gifford Law Center (https://giffords.org/lawcenter/gun-laws/).

[20] Information on the number of guns sold was unavailable for 16 of the 94 NIBRS GBPs.

region-by-year fixed effects (Θ_{ry}) to the right hand-side of the estimating equation, where *r* indexes one of the four census regions and *y* indexes the years 1991 through 2015. These controls allow us to account for unmeasured time shocks across law enforcement agencies and census regions. Second, we conduct event-study analyses where we allow β_1 through β_4 to vary over time before and after the gun buyback was held:

$$GunCrime_{\alpha csrty} = \kappa_{\alpha csrty} Exp(\varphi_0 + \left[\textstyle\sum_{i=-12, i\neq-1}^{12} \varphi_i D_{\alpha csrty}^{i}\right] + X'_{csrty}\beta_5 + Z'_{srty}\beta_6 + \sigma_\alpha + \sigma_\alpha * t + \Psi_t + \Theta_{ry} + \varepsilon_{\alpha csrty}), \quad (2)$$

where $D_{\alpha csrty}^{i}$ is a set of indicators set equal to 1 if a GBP occurred *i* months from period *t*. In this monthly event-study framework, we focus on the year immediately prior to and the year immediately following a GBP. We also report estimates based on longer-run event-study models, where the time period of interest is four years before through four years after GBP enactment.[21]

Third, we replace *Gun Crime* with *Non-Gun Crime*. 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 would be consistent with the idea that GBPs are simply markers of other unobserved crime trends, perhaps driven by unmeasured social preferences or attitudes. There may, however, 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 (DDD) 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 those in "treatment" cities, this could bias estimated effects of GBPs toward zero. We explicitly model spillover effects by including controls for whether a GBP was held in another city within the same county or in a bordering county.

[21] In the monthly event-study models, the reference category is the month before a GBP goes into effect. In the longer-run analysis, the reference category is the year before a GBP goes into effect. Because a single city may conduct multiple GBPs, our event-study framework takes into account multiple events (Sandler and Sandler 2014).

3.5. Synthetic Control Analysis

As noted above, there is substantial heterogeneity across city GBPs, including the size of the gun buyback program and the characteristics of the affected population. While we experiment with interacting our gun buyback variables 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 2021).[22] To conduct statistical inference, we assign a placebo GBP to each city in the donor pool (on the date the treated city enacted a GBP) and generate a *p*-value for the estimated treatment effect by ranking the treated city's pre-post mean squared prediction error (MSPE) ratio to each donor city's pre-post MSPE (Abadie et al. 2010).

3.6. NVSS Analysis

Finally, using county-level NVSS data and an event-study framework, we estimate the relationship between GBPs and firearm-related deaths. Our outcome of interest is *FirearmDeaths*, which is a county-by-month count of firearm-related deaths.[23] A county is coded as having a gun buyback program if it contained a city with a population greater than 50,000 that held a buyback. In addition to exploring total firearm-related deaths, we also focus separately on firearm-related homicides and suicides.

4. Results

Tables 3 through 10 present the main findings for this study. Standard errors are corrected for clustering at the city level (Bertrand et al. 2004).

[22] In unreported results that are available upon request, we also explored "matching" on every other pre-treatment year and on the observable economic and gun-related policy controls described above. These estimates produced substantially worse pre-treatment matches, but a similar pattern of findings in the post-treatment period.

[23] The NVSS analysis is conducted at the county level because only 35 percent of all deaths in the NVSS data set include city identifiers. To the extent that GBP effects are localized, a county-level analysis may bias estimates toward zero. Estimates from supplemental city-level analyses were qualitatively similar to those reported below and are available from the authors upon request.

4.1. NIBRS Results

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 fixed effects and month-by-year fixed effects, provide no evidence that GBPs are associated with reductions in gun crime, either during the first 12 months following the buyback or in subsequent years. Controlling for demographic characteristics (column 2), socioeconomic and political conditions (column 3), and other gun-related policies and policing resources (column 4) does not change this principal finding. Moreover, the stability of the estimated coefficients across specifications supports the notion 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 declines in gun crime in the 12 months following a GBP of greater than 1.1 percent. One year or more after a GBP is held, we can rule out declines greater than 4.0 percent. We also find evidence that GBPs are associated with an *increase* in firearm-related offenses during the first two months after a program is held. However, the roughly 7 percent increase in gun crime that we detect is modest, suggesting, at most, two additional gun crimes.[24]

In the last two columns, we test the robustness of our findings to controls for spatial heterogeneity. Specifically, we find that the estimated effects of GBPs are not sensitive to controlling for agency-specific linear trends (column 5) or region-by-year fixed effects (column 6).[25]

Figure 2 presents the coefficient estimates from leads and lags of the event-study analysis described in equation (2). Importantly, we find no evidence of systematic pre-treatment trends in gun crime in the months leading up to a GBP. During the first two months after a GBP, we find a 4.8 to 7.0 percent increase in gun crime, followed by no change in gun crime in the subsequent months. Figure 3 presents estimates where we examine longer-run annual leads and lags. Again, the findings in the pre-treatment period suggest that the common trends assumption holds. In the post-

[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 (column 4). Estimated p-values for the remaining post-treatment windows range from 0.490 to 0.869.

[25] The region-by-year fixed effects are constructed by interacting indicators for the four major census regions (i.e., West, Midwest, Northeast, and South) with the year fixed effects In Appendix Table 2, we explore the sensitivity of our findings to (1) the inclusion of agency-specific quadratic time trends, (2) the inclusion of census division-by-year fixed effects, and (3) the use of a negative binomial model. Census divisions include the following areas: Pacific, Mountain, West North Central, East North Central, West South Central, East South Central, Middle Atlantic, South Atlantic, and New England. The negative binomial model has the advantage of not requiring the dependent variable mean to equal the variance but is more likely to suffer from an incidental parameters problem. With the exception of one specification (column 5), the pattern of estimates is similar across models (i.e., Poisson versus negative binomial). Specifically, the negative binomial model appears to be sensitive to the inclusion of linear time trends.

treatment period, we find a small increase in gun crime over the first year following a GBP, followed by 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 suggest that the short-run positive effect we detect in Panel I of Table 3 is not driven by time-varying agency-specific unmeasured heterogeneity.

DDD 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 local law enforcement. Across the three specifications presented in Table 4, we find that GBPs are associated with a nearly 7 percent increase in gun as compared to non-gun crime in the two months following a buyback. We find no change in gun versus non-gun crime thereafter.[26] The corresponding event-study analysis is shown in Figure 4.

4.2. Robustness Checks

In Table 5, we explore the sensitivity of our main DD and DDD estimates to alternative sample selection criteria. For the ease of comparison, column (1) shows the estimates reported in panel I and column (6) of Table 3 and column (3) of Table 4.

In column (2), we restrict our buyback jurisdictions to those that implemented a GBP, thus identifying our effects entirely from differences in the timing of enactment. In both the DD and DDD models, our estimates change little when making this restriction.

In column (3), we restrict our sample to a strictly balanced panel of law enforcement agency-months for the period 2005-2015, when electronic news sources in larger cities were more likely to have information on city GBPs. The pattern of results is generally similar to those shown in our main specifications, in that we find little evidence to suggest that GBPs are associated with decreases in gun-related crime.

To assess the sensitivity of our estimates to adjustments for data quality, we drop agency-months where crime counts are two or more standard deviations away from the city-specific mean (column (4)). The estimates from this exercise are consistent with those in column (1).

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

[26] Including controls for agency-specific quadratic trends and division-by-year fixed effects to further account for spatial heterogeneity do not materially change the estimates shown in Table 4.

treatment effects and the use of early-adopting GBP cities as controls for late-adopting GBP cities (Goodman-Bacon 2021).[27] In Figure 5, we present the results of an event-study analysis using the approach developed by Callaway and Sant'Anna (2021). Using cities that never held a GBP as the sole counterfactuals for the treated cities, we continue to find evidence that the common trends assumption holds. In the post-treatment period, and consistent with the estimates presented above, we find no evidence that GBPs led to reductions in firearm-related crime.[28]

4.3. 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" is set equal to 1 if a city within the county held a gun buyback and equal to 0 otherwise; "Border County GBP" is set equal to 1 if a city in a border county held a gun buyback and equal to 0 otherwise. Note that in the creation of these variables, we also include GBPs held in neighboring cities even if those cities were not included in the NIBRS data set. In general, the estimates reported in Table 6 provide no evidence that GBPs generated spillovers to neighboring jurisdictions.

In Table 7, we report estimates disaggregated by the type of violent (panel I) and non-violent (panel II) gun crime. Across all types of crime, we find little evidence to suggest that GBPs were effective in either the short- or longer-run. The only statistically significant effects that we estimate are, in fact, positive in sign. Specifically, GBPs are associated with increases in robberies, assaults (aggravated and simple), weapon law violations, and kidnappings.

Finally, we explore whether heterogeneous effects exist by age, gender, or race of the offender. The estimates shown in Table 8 provide no support for the hypothesis that GBPs reduced gun crime among a particular demographic group. During the period 0 to 2 months after treatment, we find that buybacks are positively and statistically significantly associated with gun crime for 18- to 23-year-olds, individuals over the age of 35, both males and females, and Blacks.

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[27] Of the 251 large cities in our sample, 206 never held a GBP.

[28] The event study in Figure 5 is generated using linear regression, which, at the time of this writing are requirements imposed by the available *R* package. The dependent variable is equal to the inverse hyperbolic sign of gun-related crime, adjusting for agency population, which imposes a further restriction that we use a balanced panel of agency-months for the Callaway and Sant'Anna (2021) estimator.

4.4. Synthetic Control Estimates

To flexibly test for heterogeneous effects, we turn to a synthetic control analysis (Abadie et al. 2010). To be included in this analysis, the treatment city must have reported gun crime data to the NIBRS in each month for at least two years prior to the city's first reported GBP during the 1991-2015 period. If a treatment city held multiple GBPs, our synthetic control figures indicate when these future treatments occurred in the post-treatment period.[29]

Figure 6 shows synthetic control plots for each treated city that met the above sample analysis criteria. Our synthetic control "matching strategy" chooses a weighted linear combination of donor cities — those cities of greater than 50,000 persons that never held a GBP over the period under study — to generate a synthetic control city with the most similar gun crime rates in each of the pre-treatment years.

We highlight three findings from the synthetic control analysis. First, pre-treatment trends in gun crime are similar across treatment and synthetic control jurisdictions. Second, we find little evidence that GBPs are associated reductions in gun crime. For only 4 out of the 37 cities in our analysis do we estimate *any* post-treatment coefficients (for the year following the gun buyback and up to four years following the GBP) that are negative and statistically significant at the 5 percent level. In contrast, for 12 cities, we find evidence of post-treatment effects that are positive and statistically significant. Third, we find no systematic evidence of heterogeneous effects when we organize our synthetic control estimates by the size of the buyback.[30] Specifically, the pattern of estimates shown in Appendix Table 4 suggest that larger gun buyback programs, like smaller ones, have been generally ineffective at deterring gun-related crimes.[31]

4.5. NVSS Results

Finally, we use county-level mortality data from the National Vital Statistics System to estimate the effect of GBPs on total gun-related deaths, gun-related suicides, and gun-related

[29] We do not estimate synthetic control estimates of later gun buybacks (beyond the first) because the pre-treatment period could include dynamically evolving effects of earlier gun buybacks.

[30] We measure the size of GBPs as the number of guns bought back per 10,000 population. Appendix Table 4 simply reports the same estimates shown in Table 9 but organizes them by the size of the GBP.

[31] Column (1) of Appendix Table 5 shows results from a Poisson model where a continuous measure for the size of a GBP is interacted with indicators for the post-treatment period. Consistent with the synthetic control analysis, these estimates provide no evidence that larger GBPs are effective at reducing gun-related crimes.

homicides.[32] The event-study coefficients reported in Table 10, along with those shown in Figures 7-9, provide no evidence that any of these outcomes fell in the wake of GBPs being implemented. This general null result holds when focusing on a longer-run time horizon (Appendix Figures 2 and 3) or when exploring heterogeneity by GBP size (Appendix Table 6, columns (2)-(4)).[33]

5. Discussion

Our estimates provide compelling evidence that GBPs have done little to reduce gun-related crime or mortality in the United States. This general finding is of substantial policy relevance, as GBPs have become one of the most popular levers among city-level officials hoping to deter firearm-related violence (Godley 2021; Goshay 2021; Chinchilla 2021; Birmingham PD 2021; Ly 2021; Associated Press 2021; Daily Freeman Staff 2021; Milian 2021; Rubinstein et al. 2021; Vasilogambros 2022). There are a number of potential reasons why GBPs have been ineffective. 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 no observable effect on gun-related crime. For instance, the 2014 GBP in Somerville, Massachusetts netted 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 into context, Indiana has a gun ownership rate of 44.8 percent, suggesting that approximately 467,037 individuals own at least one firearm (World Population Review 2020). Most buybacks have, at most, a modest effect on the local supply of firearms, which could be a reason for their ineffectiveness.

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[32] City-level identifiers are only available for 35 percent of all gun-related deaths in the NVSS. A supplementary analysis based on the sample with city identifiers produced qualitatively similar results.

[33] DDD estimates, which are available from the authors upon request, provided little evidence that GBPs caused a reduction in gun-related relative to non-gun-related deaths.

Second, most GBP participants 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), over the age of 55 (59 percent), lives outside of the city limits, and typically has a household income well above the U.S. average (Violano et al. 2014). GBPs are also unlikely to appreciably reduce the number of gun-owning households. More than half of GBP participants who sell a firearm at a buyback have another one at home (Kasper et al. 2017; Green et al. 2017; Violano et al. 2014).

In addition, firearms sold at buybacks do not appear to be those that would typically be used in the commission of a 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 GBPs in Milwaukee, Wisconsin found that the firearms relinquished were more likely to be older models with longer barrels and smaller magazine sizes (Kuhn et al. 2002).[34] Guns of this nature are less likely to be used to commit violence (Planty and Truman 2013). These findings are consistent with a story of adverse selection in firearm quality, which is perhaps expected when buyback prices are set well below market value and there is no price discrimination. Moreover, income gains to GBP participants selling lower quality firearms — which are often destroyed — could result in an increase in the sale of newly manufactured, well-functioning guns. Anecdotal evidence indicates that some GPB participants turn in their guns to upgrade to better weapons (Casiano 2018).

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 purchased with one's own income, this could suggest that GBPs are one way to transfer unwanted guns.

6. Conclusion

A recent surge in city gun buyback programs has raised hopes that such efforts can curb gun violence in the United States. Congressman Donald Payne (D-New Jersey), a lead sponsor of H.R. 1279, *Safer Neighborhoods Gun Buyback Act*, predicted that if federal funding for buybacks is expanded, "there will be fewer guns in circulation, which will help reduce crime" ("Payne, Jr. introduces gun buyback legislation" 2019). On the other hand, former Representative Luke Simons (R-North Dakota), who sponsored a bill to ban GBP funding in his state, argued that "firearm buybacks do

[34] Some GBPs will even accept "non-powder" or "imitation" firearms, such as Airsoft and BB guns (Grubb 2022).

nothing to increase public safety and shouldn't be subsidized by taxpayer money" (MacPherson 2019), sentiments echoed by the NRA (Ellis and Hicken 2015; National Rifle Association-Institute for Legislative Action 2021).

Over the last decade, more than 100 U.S. cities have implemented GBPs in the hopes of reducing gun crime. However, while local policymakers continue to advocate for and hold GBPs, little is known about their effectiveness. 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 effect of city GBPs on gun crime and firearm-related violence.

Our findings provide compelling evidence that U.S. GBPs have not deterred gun crime, firearm-related homicides, or firearm-related suicides in either the short- or longer-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 immediately following a buyback. One or more years after a GBP is held, we can rule out declines of greater than 2.3 percent. Our general null findings are consistent with descriptive evidence that (i) firearm buyback prices are set too low to appreciably reduce the local supply (Reuter and Mouzos 2004), (ii) most GBP participants are drawn from populations with relatively low crime risk (Planty and Truman 2013; Violano et al. 2014; Romero et al. 1998), and (iii) guns bought back tend to be older and less functional 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 it (Kuhn et al. 2002) and with the hypothesis that some criminals may be emboldened by the perception that victims will be less likely to defend themselves with deadly physical force (Lott 1998).

Our results suggest that U.S. GBPs have been an inefficient use of taxpayers' dollars. Perhaps alternative firearm-related policies, such as safe storage laws (Anderson et al. 2018, 2021), stricter background checks (Gius 2015), or mandatory handgun purchase delays (Edwards et al. 2018) would be better at deterring gun violence. Our findings also suggest that city GBPs have been poorly designed to achieve their policy objectives. In contrast, buyback programs that target high-crime neighborhoods or price discriminate across weapons of heterogeneous quality may affect gun violence differently. For instance, in 2021, New York City mayoral candidate Kathryn Garcia proposed a tenfold increase in the price paid (by the local government) for each firearm, which would amount to $2000 per firearm (Rubinstein et al. 2021). However, the lack of public appetite for large-scale government spending on gun confiscation, coupled with the inherent difficulties of targeting weapons ultimately used by criminals but not law-abiding citizens, limit the promise of buybacks as an effective anti-gun violence tool.

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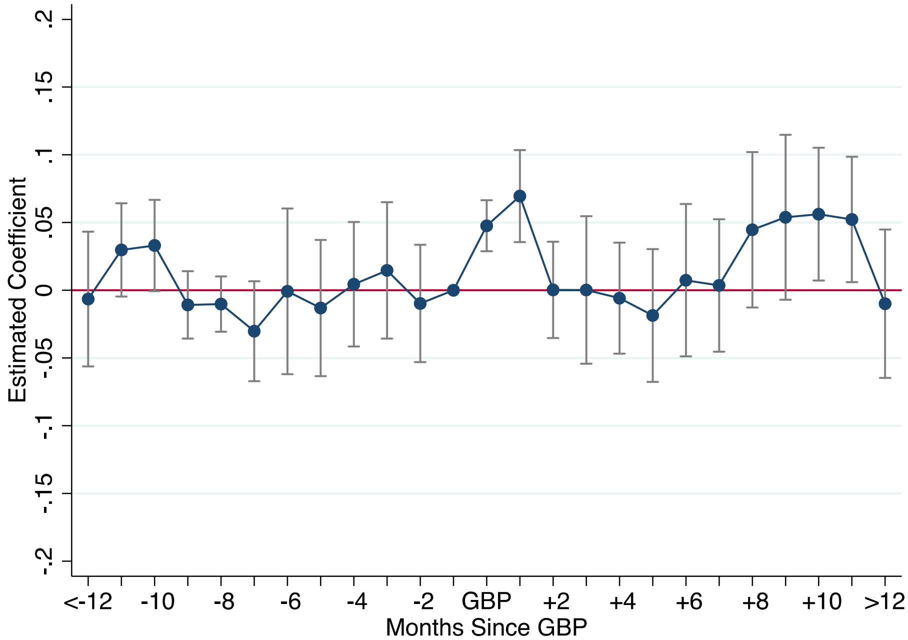
Figure 1: Gun Buyback Programs in Cities with Greater than 50,000 Population

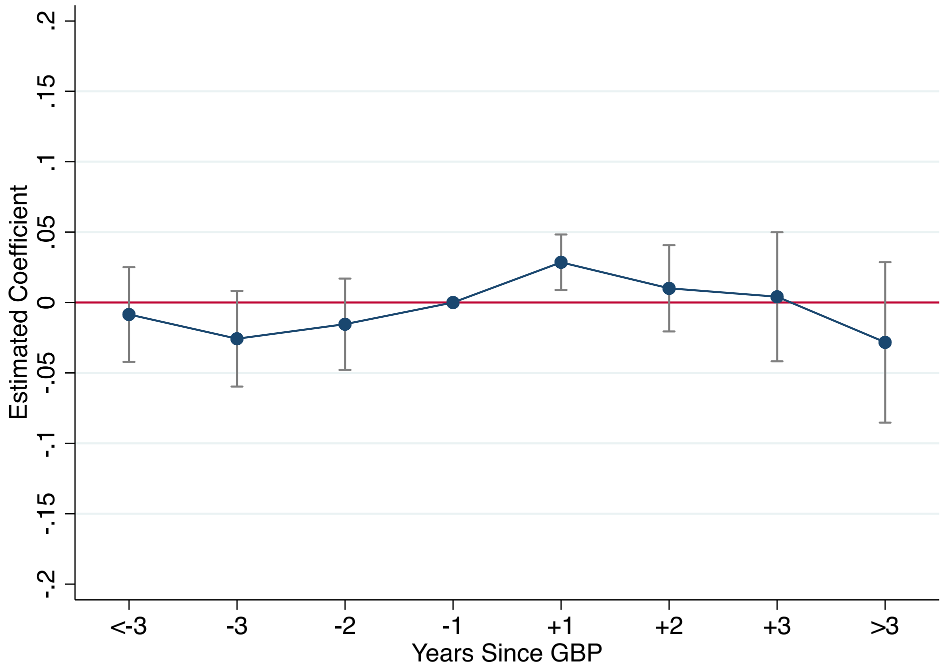
Notes: Black dots indicate cities with a GBP in our sample. Larger dots represent cities with more guns bought back per capita.

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



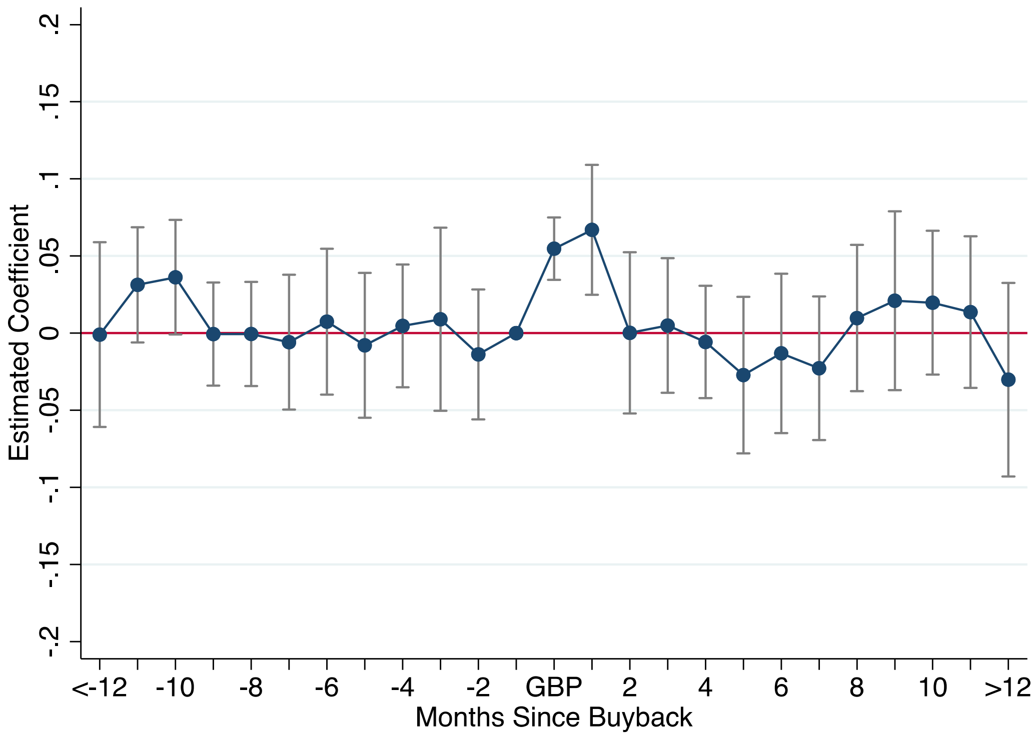
Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one month before treatment. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the gun-related crime count in agency *a* and month *t*. Controls include the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, region-by-year fixed effects, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

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



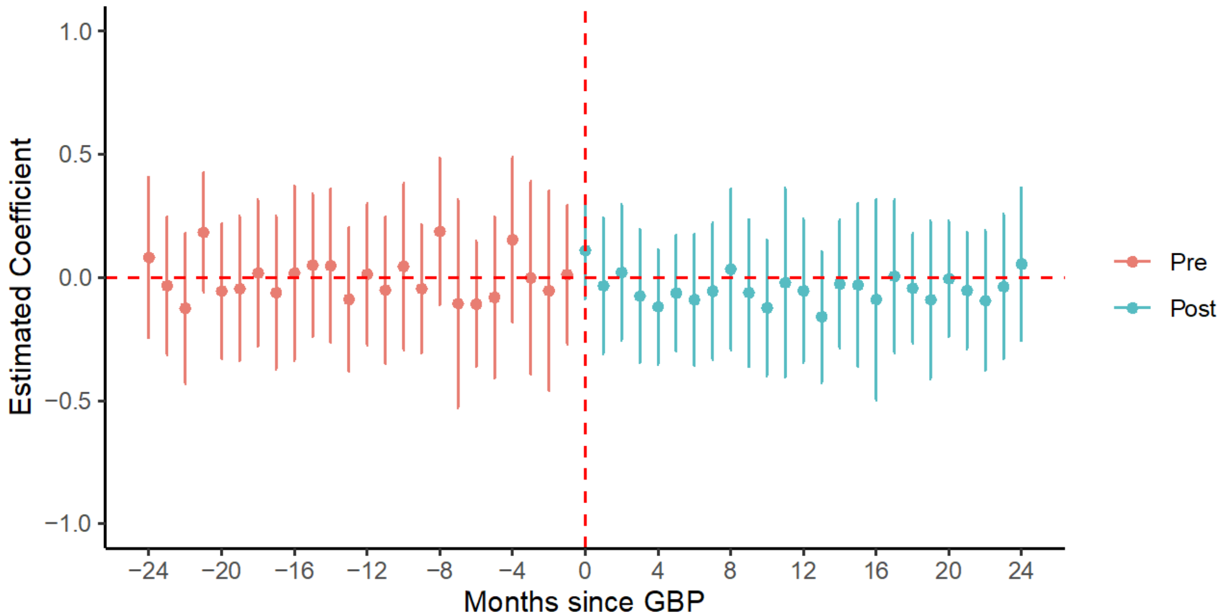
Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one year before treatment. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the gun-related crime count in agency *a* and month *t*. Controls include the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, region-by-year fixed effects, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

Figure 4: Event-Study Analysis of Gun-Related Versus Non-Gun-Related Crimes



Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one month before treatment. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. Controls include the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, region-by-year fixed effects, interactions between a gun-crime indicator (i.e., Gun Crime) and all right-hand-side variables, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

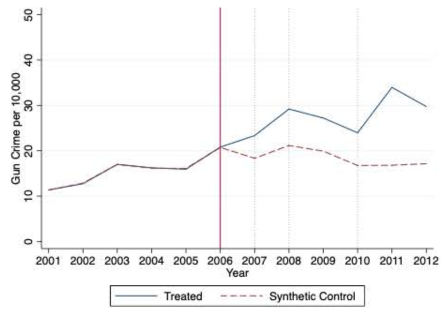
Figure 5: Callaway and Sant'Anna (2021) Event-Study Analysis of Gun-Related Crimes



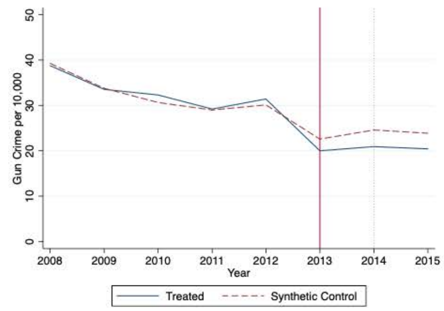
Notes: Estimates of group-time average treatment effects on the treated (ATTs) and their 95% confidence intervals are reported. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. ATT estimates are from equation (3.4) in Callaway and Sant'Anna (2021). The dependent variable is equal to the inverse hyperbolic sign of the gun-related crime count in agency *a* and month *t*, weighting by agency population. Controls include mean agency population, agency fixed effects, and month-by-year fixed effects. Standard errors are corrected for clustering at the city level.

Figure 6: Synthetic Control Estimates

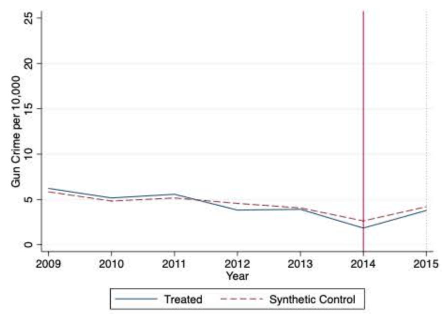
(a) Akron, OH



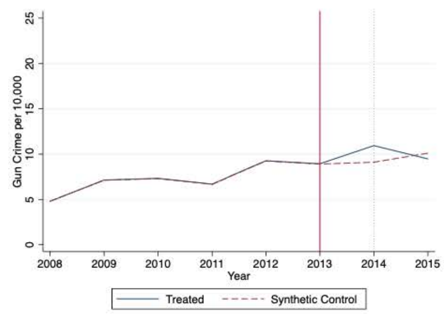
(b) Battle Creek, MI



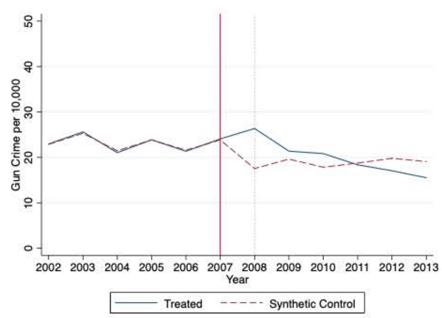
(c) Cambridge, MA



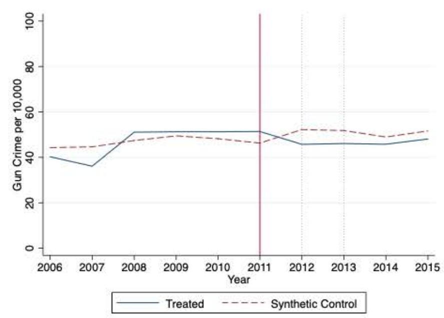
(d) Cedar Rapids, IA



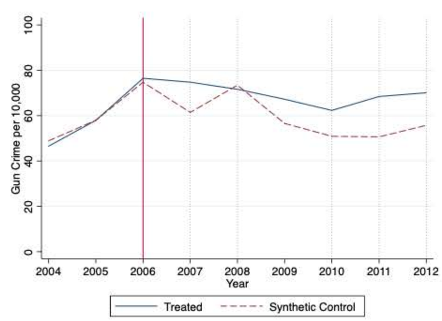
(e) Chesapeake, VA



(f) Cincinnati, OH



(g) Cleveland, OH



(h) Columbia, SC

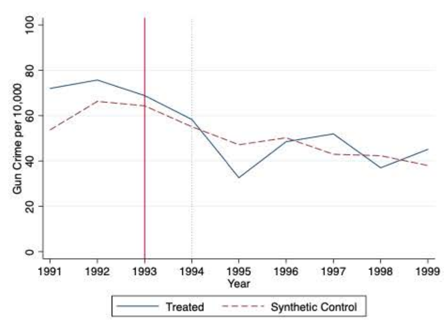
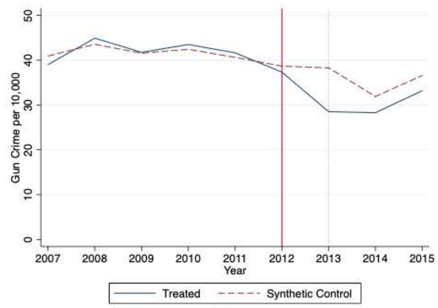
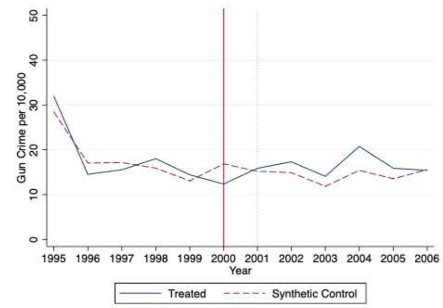


Figure 6: Synthetic Control Estimates (continued)

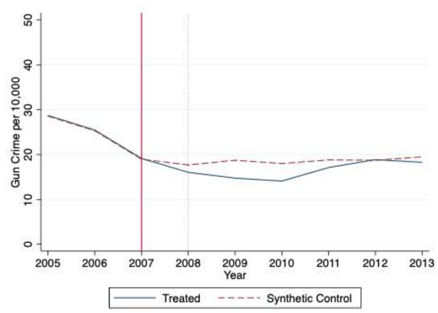
(i) Columbus, OH



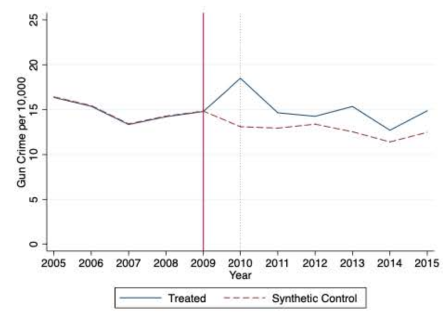
(j) Davenport, IA



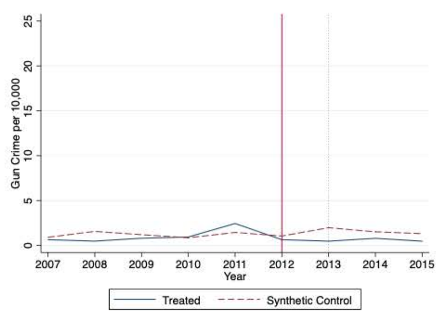
(k) Denver, CO



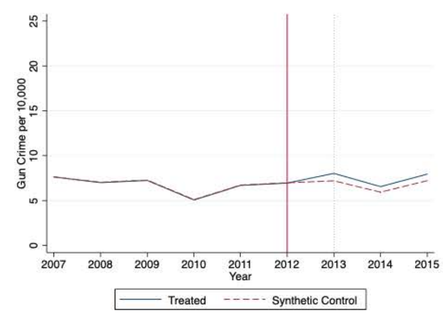
(l) Fall River, MA



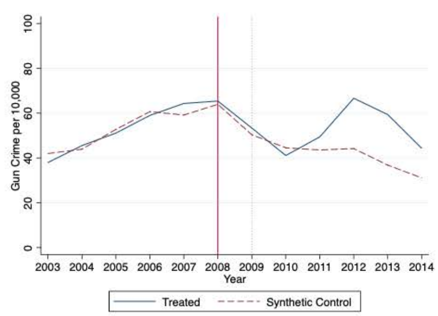
(m) Greenwich, CT



(n) Haverhill, MA



(o) Jackson, TN



(p) Kalamazoo, MI

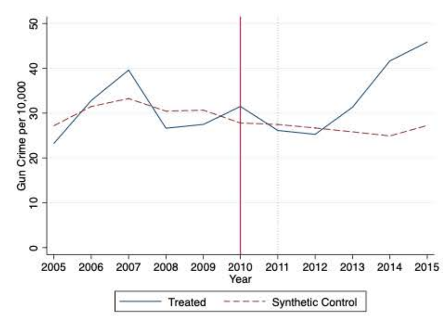
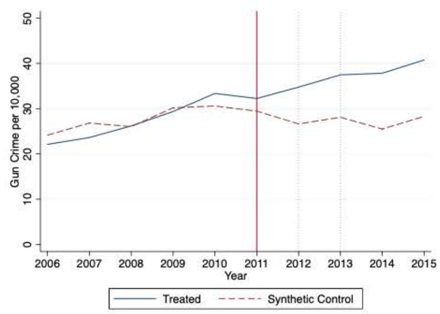
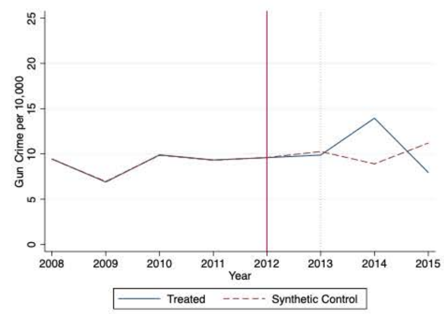


Figure 6: Synthetic Control Estimates (continued)

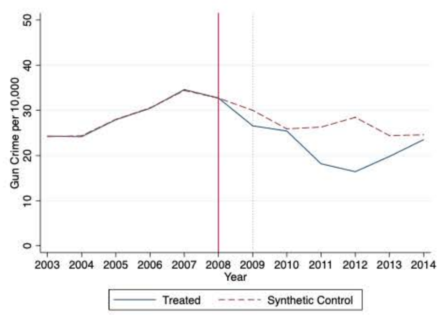
(q) Lansing, MI



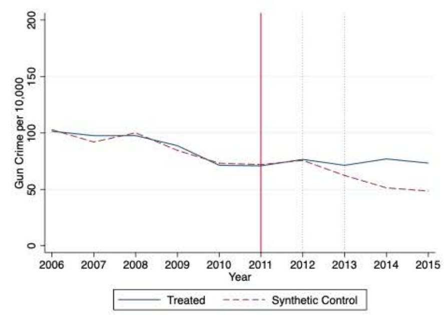
(r) Lowell, MA



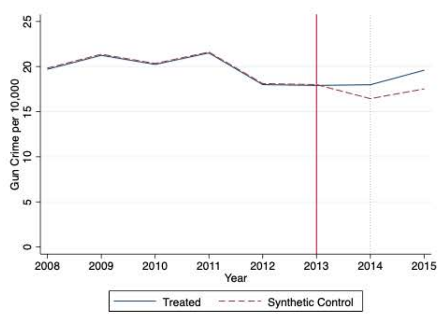
(s) Lynchburg, VA



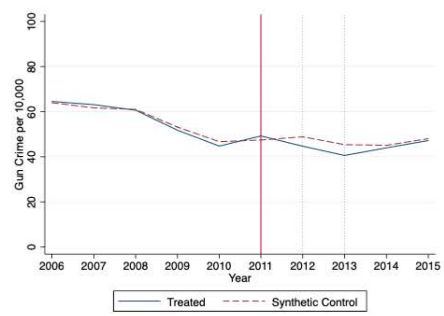
(t) Memphis, TN



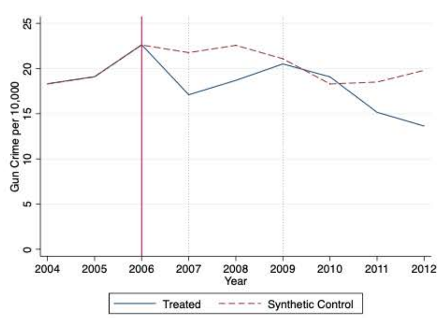
(u) Murfreesboro, TN



(v) Nashville, TN



(w) New Bedford, MA



(x) North Charleston, SC

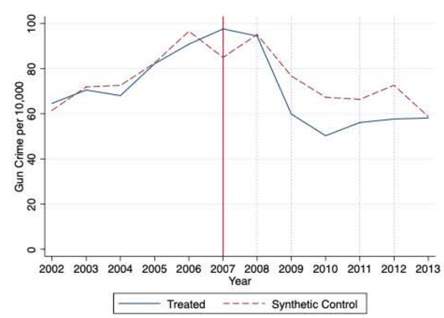
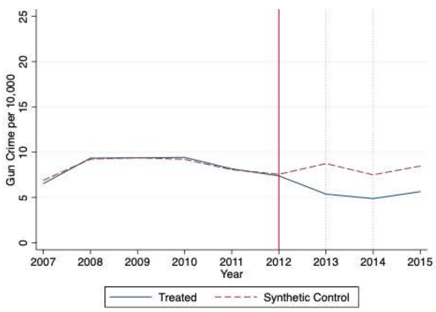
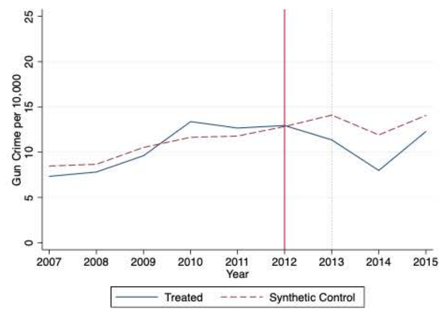


Figure 6: Synthetic Control Estimates (continued)

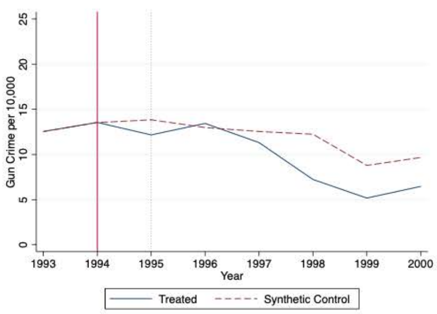
(y) Norwalk, CT



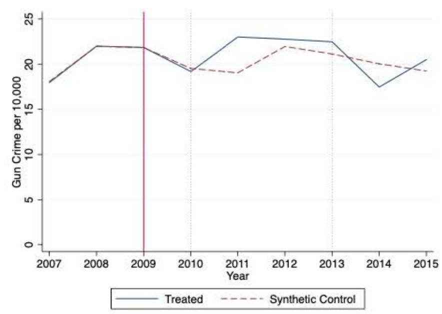
(z) Pawtucket, RI



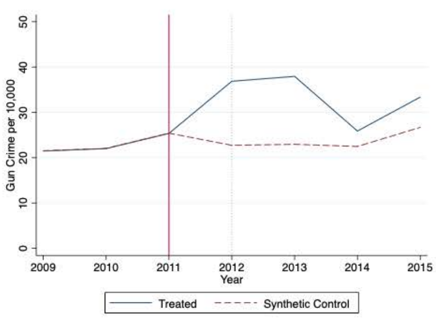
(aa) Pocatello, ID



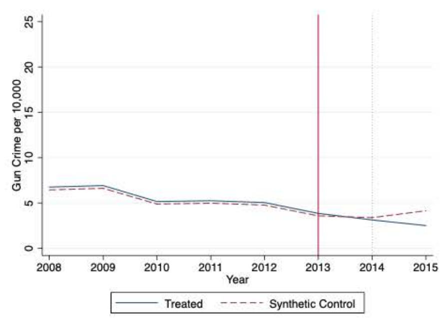
(bb) Providence, RI



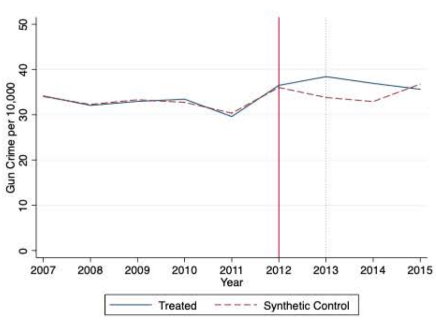
(cc) Pueblo, CO



(dd) Somerville, MA



(ee) Springfield, MA



(ff) Stamford, CT

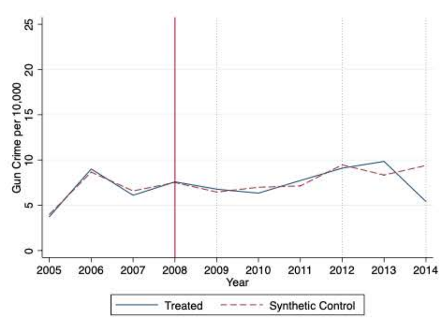
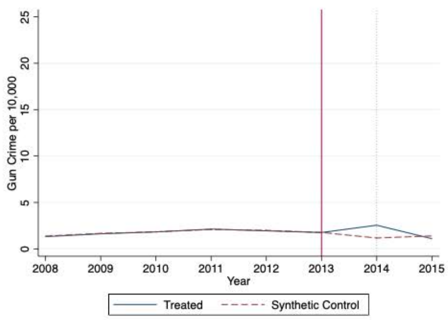
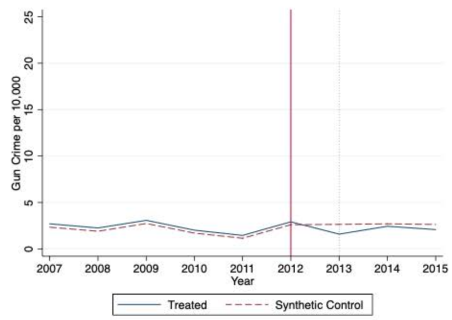


Figure 6: Synthetic Control Estimates (continued)

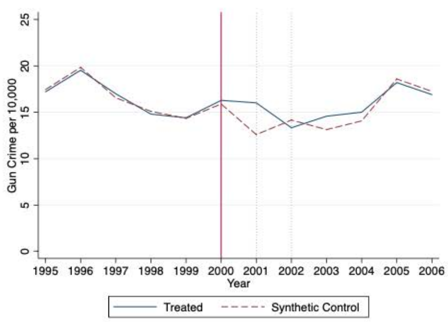
(gg) Waltham, MA



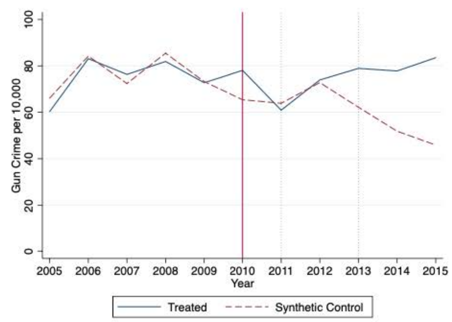
(hh) Warwick, RI



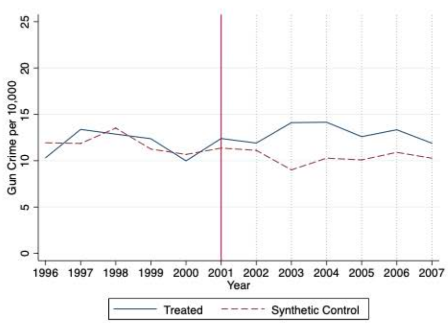
(ii) Waterloo, IA



(jj) Wilmington, DE

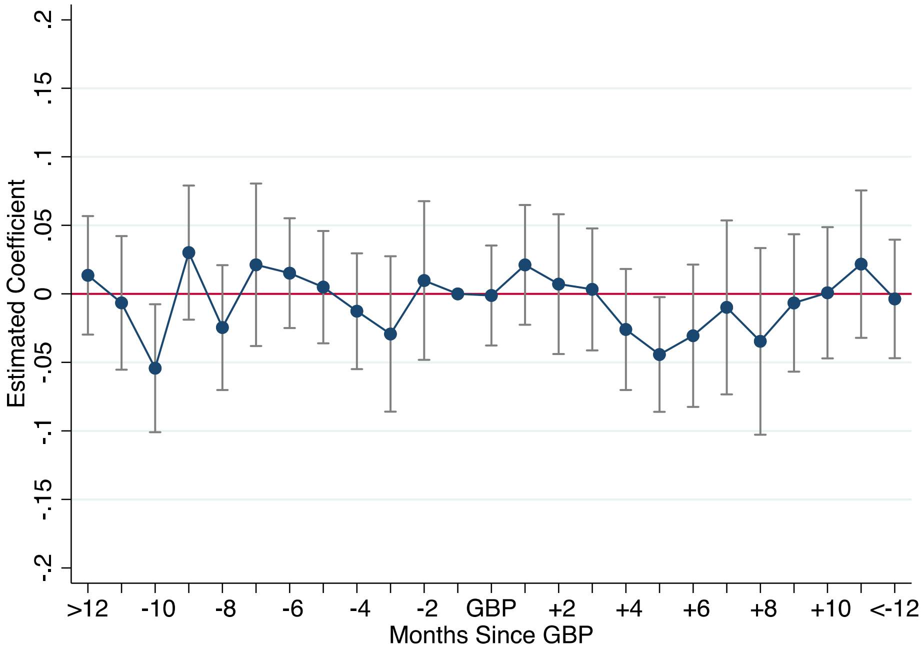


(kk) Worcester, MA



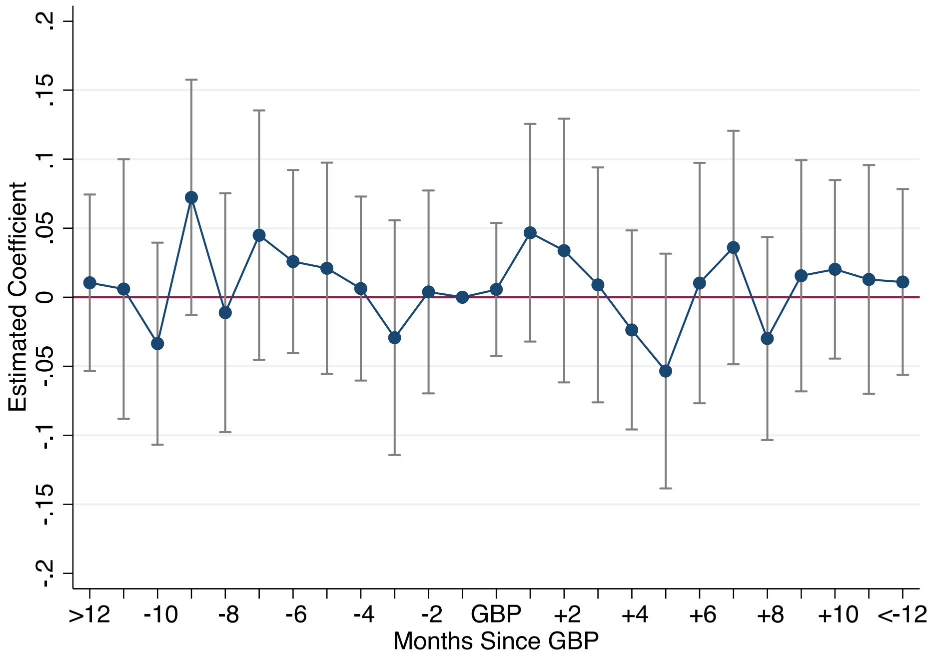
Notes: Vertical red lines indicate the year prior to a GBP being held. Vertical dashed lines indicate the year a GBP was held. Multiple vertical dashed lines indicate that a city had more than one GBP.

Figure 7: Event-Study Analysis of Gun-Related Deaths



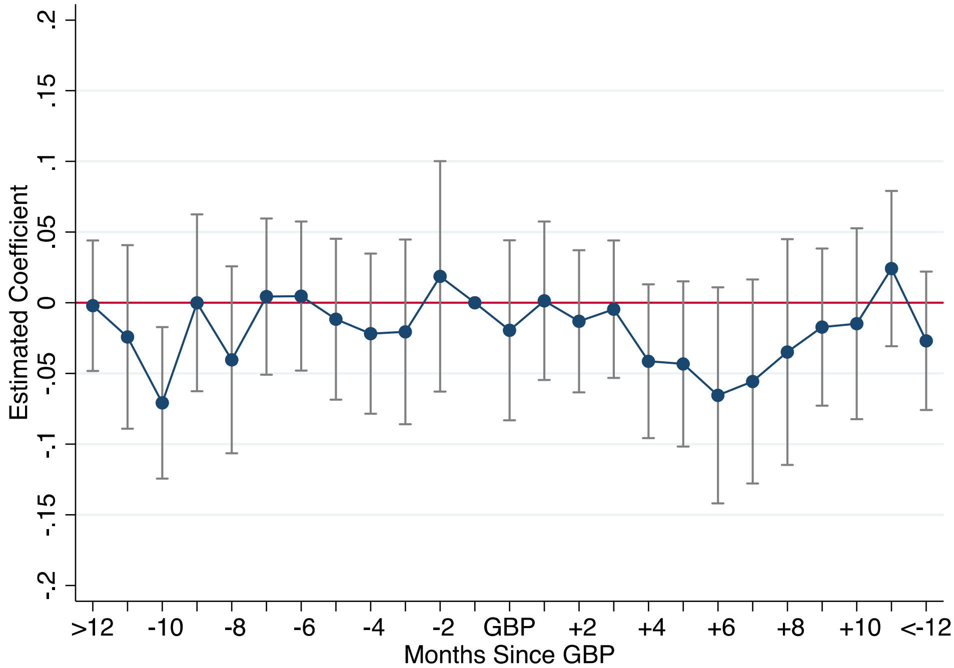
Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one month before treatment. Data on gun-related mortality at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the gun-related mortality count in county *c* and month *t*. Controls include the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering at the county level.

Figure 8: Event-Study Analysis of Gun-Related Suicides



Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one month before treatment. Data on gun-related suicides at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the gun-related suicide count in county *c* and month *t*. Controls include the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering at the county level.

Figure 9: Event-Study Analysis of Gun-Related Homicides



Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one month before treatment. Data on gun-related homicides at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the gun-related homicide count in county *c* and month *t*. Controls include the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering at the county level.

Table 1: Descriptive Statistics, 1991-2015

	Mean	SD	Source
Crime outcomes			
Gun Crime Count	79.511	146.618	National Incident-Based Reporting System (NIBRS)
Gun Crime Count, Non-Violent	20.178	33.483	
Gun Crime Count, Violent	59.333	116.006	
Non-Gun Crime Count	1,984.522	2,304.982	
Non-Gun Crime Count, Non-Violent	1,877.066	2,167.005	
Non-Gun Crime Count, Violent	107.457	155.722	
Vital statistics outcomes			
Firearm-Related Deaths	12.081	23.614	National Vital Statistics System (NVSS)
Firearm-Related Homicides	7.268	17.327	
Firearm-Related Suicides	4.648	7.068	
Demographic controls			
Population % Age 15-19[a]	0.071	0.009	Surveillance, Epidemiology, and End Results (SEER)
Population % Age 20-29[a]	0.151	0.031	
Population % White[a]	0.693	0.149	
Population % Black[a]	0.163	0.144	
Population % Hispanic[a]	0.101	0.101	
Population % Male[a]	0.489	0.009	
Percentage College Graduates[b]	0.295	0.063	American Community Survey (ACS)
Socioeconomic and political controls			
Per Capita Income ($2015)[a]	45,674	11,647	American Community Survey (ACS)
Unemployment Rate[a]	6.001	2.478	Bureau of Labor Statistics (BLS)
Minimum Wage ($2015)[a]	7.470	0.972	Vaghul and Zipperer (2016)
Democrat Governor[b]	0.446	0.497	Ballotpedia
Gun-related policies and policing resources			
Background Checks per 100,000 Population[b]	5,365	6,362	National Instant Criminal Background Check System (NICS)
Stand Your Ground Law[b]	0.357	0.478	Giffords Law Center
Shall Issue Law[b]	0.803	0.398	
Gun Lock Required[b]	0.296	0.457	
State/Federal Minimum Age Law[b]	0.993	0.084	
Negligent Child Access Prevention Law[b]	0.251	0.433	Anderson and Sabia (2021)
Reckless Child Access Prevention Law[b]	0.344	0.475	
Police Expenditure per 100,000 ($2015)[b]	268.550	42.726	Bureau of Justice Statistics (BJS)
Police Officers per 100,000 Population[b]	2.151	0.376	

[a] Varies at the county level.
[b] Varies at the state level.

Notes: Means are weighted by agency population. Unweighted means are shown in Appendix Table 6.

Table 2: Gun Buyback Program Dates for Treated Cities in the NIBRS, 1991-2015

City	Date of GBP	Guns bought
Akron, OH	December 7, 2007	950
	November 21, 2008	650
	November 15, 2010	*NR*
Battle Creek, MI	May 31, 2014	NR
Cambridge, MA	June 13, 2015	55
Cedar Rapids, IA	September 27, 2014	34
Chesapeake, VA	December 6, 2008	309
Cincinnati, OH	January 17, 2012	50
	January 15, 2013	135
Cleveland, OH	November 10, 2007	421
	November 22, 2008	*NR*
	November 5, 2010	*NR*
	September 17, 2011	706
	October 20, 2012	298
	June 15, 2013	*NR*
	September 6, 2014	270
	August 22, 2015	150
Columbia, SC	October 10, 1994	*NR*
	December 18, 2004	300
	February 14, 2009	*NR*
Columbus, OH	June 15, 2013	352
Davenport, IA	September 15, 2001	450
Denver, CO	December 27, 2008	15
Detroit, MI	July 29, 2006	*NR*
	September 7, 2010	*NR*
	December 16, 2010	*NR*
	August 30, 2012	365
	May 18, 2013	*NR*
	November 9, 2013	24
Fall River, MA	December 11, 2010	115
Flint, MI	April 29, 2000	1000
	March 16, 2007	*NR*
	June 6, 2009	178
Greenwich, CT	February 9, 2013	11
Haverhill, MA	May 11, 2013	27
Jackson, TN	December 19, 2009	*NR*

City	Date of GBP	Guns bought
New Bedford, MA	June 23, 2007	18
	October 4, 2009	41
New Haven, CT	December 12, 2006	235
	December 11, 2007	200
	December 3, 2011	60
	December 22, 2012	65
	June 28, 2014	106
North Charleston, SC	December 13, 2008	245
	December 13, 2009	127
	January 22, 2011	*NR*
	March 24, 2012	135
Norwalk, CT	February 2, 2013	18
	June 14, 2014	20
Pawtucket, RI	June 1, 2013	50
Pocatello, ID	April 22, 1995	22
Providence, RI	June 26, 2010	*NR*
	April 6, 2013	186
Pueblo, CO	December 30, 2012	7
Seattle, WA	January 26, 2013	712
Somerville, MA	August 16, 2014	15
Springfield, MA	March 2, 2013	333
Stamford, CT	January 26, 2009	56
	December 1, 2012	54
	March 29, 2014	32
Toledo, OH	June 8, 2013	185
Waltham, MA	September 20, 2014	46
Warwick, RI	April 6, 2013	186
Waterloo, IA	December 8, 2001	100
	April 27, 2002	*NR*
	June 11, 2011	34
Wilmington, DE	December 17, 2011	2040
	August 24, 2013	67
Worcester, MA	December 7, 2002	250
	December 6, 2003	244
	December 4, 2004	305
	December 3, 2005	206

Table 2: Gun Buyback Program Dates for Treated Cities in the NIBRS, 1991-2015 (continued)

City	Date of GBP	Guns bought	City	Date of GBP	Guns bought
Kalamazoo, MI	July 9, 2011	99	Worcester, MA	December 9, 2006	271
Lansing, MI	August 18, 2012	99		December 8, 2007	217
	February 9, 2013	100		December 6, 2008	127
Lowell, MA	November 2, 2013	36		December 5, 2009	241
Lynchburg, VA	July 11, 2009	12		December 4, 2010	195
Memphis, TN	September 15, 2012	497		December 3, 2011	113
	September 21, 2013	588		December 8, 2012	142
Milwaukee, WI	July 19, 2005	200		December 7, 2013	85
	May 17, 2014	353		December 6, 2014	149
Murfreesboro, TN	April 26, 2014	6		December 5, 2015	271
Nashville, TN	November 10, 2012	91			
	June 8, 2013	62			

Notes: *NR* identifies GBPs where information on the number of guns bought back was not available.

Table 3: Poisson Estimates of the Effect of GBPs on Gun-Related and Non-Gun-Related Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Months following GBP	**Panel I: Gun-related crime**					
0 to 2 Months	0.079**	0.071*	0.072**	0.079**	0.075**	0.069**
	(0.029)	(0.028)	(0.027)	(0.025)	(0.023)	(0.024)
3 to 5 Months	-0.002	-0.009	-0.006	0.009	-0.004	-0.003
	(0.021)	(0.025)	(0.025)	(0.023)	(0.034)	(0.030)
6 to 11 Months	0.040	0.033	0.035	0.047*	0.036	0.039
	(0.022)	(0.025)	(0.024)	(0.019)	(0.029)	(0.025)
≥ 12 Months	0.001	0.003	0.004	-0.001	-0.003	0.010
	(0.012)	(0.009)	(0.009)	(0.010)	(0.030)	(0.026)
	Panel II: Non-gun-related crime					
0 to 2 Months	0.012	0.010	0.012	0.010	-0.003	-0.008
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.013)
3 to 5 Months	0.004	0.002	0.004	0.004	-0.010	-0.010
	(0.019)	(0.021)	(0.020)	(0.021)	(0.023)	(0.021)
6 to 11 Months	0.032	0.031	0.033	0.034	0.020	0.021
	(0.019)	(0.020)	(0.019)	(0.020)	(0.022)	(0.020)
≥ 12 Months	0.007	0.010	0.010	0.011	0.018	0.024
	(0.008)	(0.008)	(0.008)	(0.008)	(0.016)	(0.015)
Observations	36,516	36,516	36,516	36,516	36,516	36,516
Demographic controls?	-	Yes	Yes	Yes	Yes	Yes
Socioeconomic and political controls?	-	-	Yes	Yes	Yes	Yes
Gun-related policies and policing resources?	-	-	-	Yes	Yes	Yes
Agency-specific linear trends?	-	-	-	-	Yes	Yes
Region-by-year fixed effects?	-	-	-	-	-	Yes

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column within each panel represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. All models control for agency fixed effects and month-by-year fixed effects, and agency population is set as the exposure variable. Controls for demographics, socioeconomic and political characteristics, and gun-related policies and policing resources are listed in Table 1. Standard errors are corrected for clustering at the city level.

Table 4: Difference-in-Difference-in-Differences Poisson Estimates of the Effect of GBPs on Gun-Related versus Non-Gun-Related Crimes

Months following GBP	(1)	(2)	(3)
0 to 2 Months	0.012	0.011	-0.009
	(0.016)	(0.015)	(0.013)
0 to 2 Months * Gun Crime	0.067*	0.068**	0.078**
	(0.026)	(0.024)	(0.026)
3 to 5 Months	0.005	0.006	-0.009
	(0.019)	(0.020)	(0.021)
3 to 5 Months * Gun Crime	-0.007	0.003	0.007
	(0.017)	(0.018)	(0.026)
6 to 11 Months	0.032	0.034	0.021
	(0.018)	(0.020)	(0.019)
6 to 11 Months * Gun Crime	0.008	0.012	0.017
	(0.015)	(0.014)	(0.022)
12 Months	0.007	0.011	0.024
	(0.008)	(0.008)	(0.015)
12 Months * Gun Crime	-0.006	-0.013	-0.013
	(0.015)	(0.012)	(0.028)
Observations	73,032	73,032	73,032
Controls listed in Table 1?	-	Yes	Yes
Agency-specific linear trends?	-	-	Yes
Region-by-year fixed effects?	-	-	Yes

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. All models control for agency fixed effects, month-by-year fixed effects, interactions between a gun-crime indicator (i.e., Gun Crime) and all right-hand-side variables, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

Table 5: Sensitivity of Estimated Effects to Alternative Sample Selection Criteria

	(1)	(2)	(3)	(4)
	Baseline specification	Only treated cities	Strictly balanced panel 2005-2015	Dropping observations that are 2+ SDs away from mean
Months following GBP:	**Panel I: Difference-in-differences estimates**			
0 to 2 Months	0.069**	0.068***	0.077***	0.049*
	(0.024)	(0.019)	(0.023)	(0.020)
3 to 5 Months	-0.002	-0.009	0.030	-0.003
	(0.030)	(0.028)	(0.025)	(0.026)
6 to 11 Months	0.039	0.026	0.072**	0.036
	(0.025)	(0.027)	(0.022)	(0.025)
≥ 12 Months	0.010	0.007	0.057*	0.008
	(0.026)	(0.034)	(0.027)	(0.022)
Observations	36,516	7,558	18,744	34,922
	Panel II: Difference-in-difference-in-differences estimates			
0 to 2 Months * Gun Crime	0.077**	0.073***	0.076***	0.083**
	(0.027)	(0.021)	(0.023)	(0.028)
3 to 5 Months * Gun Crime	0.007	0.002	0.016	0.005
	(0.027)	(0.025)	(0.028)	(0.028)
6 to 11 Months * Gun Crime	0.017	0.005	0.040	0.021
	(0.022)	(0.024)	(0.025)	(0.020)
≥ 12 Months * Gun Crime	-0.013	-0.030	0.033	-0.011
	(0.028)	(0.034)	(0.032)	(0.028)
Observations	73,032	15,116	37,488	71438

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column within each panel represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. All models control for the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, and region-by-year fixed effects, and agency population is set as the exposure variable. The DDD models also control for interactions between a gun-crime indicator (i.e., Gun Crime) and all right-hand-side variables. Standard errors are corrected for clustering at the city level.

Table 6: GBPs and Gun-Crime Spillovers

Months following GBP	(1)	(2)	(3)
0 to 2 Months			
City GBP	0.069**	0.071**	0.072**
	(0.024)	(0.024)	(0.023)
GBP in County		0.049	0.049
		(0.041)	(0.049)
Border County GBP			-0.008
			(0.028)
3 to 5 Months			
City GBP	-0.003	-0.001	(0.031)
	(0.030)	(0.030)	0.060
GBP in County		-0.019	-0.015
		(0.043)	0.077
Border County GBP			0.006
			0.041
6 to 11 Months			
City GBP	0.039	0.041	0.016
	(0.025)	(0.025)	(0.052)
GBP in County		0.026	0.026
		(0.044)	(0.023)
Border County GBP			0.001
			(0.030)
≥12 Months			0.038
City GBP	0.010	0.014	(0.047)
	(0.026)	(0.027)	-0.031
GBP in County		0.041	0.043
		(0.047)	0.072**
Border County GBP			-0.024
			0.038
Observations	36,516	36,516	36,516

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the gun-crime count in agency *a* and month *t*. All models control for the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, and region-by-year fixed effects, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

Table 7. Poisson Estimates of the Effect of GBPs on Gun-Related Crimes by Crime Type

	Panel I: Violent Gun Crime				
	(1)	(2)	(3)	(4)	(5)
Months following GBP	Total	Robbery	Aggravated Assault	Murder/ Manslaughter	Rape/Sexual Assault
0 to 2 Months	0.065**	0.065*	0.057**	-0.011	0.017
	(0.025)	(0.030)	(0.022)	(0.049)	(0.083)
3 to 5 Months	-0.007	-0.037	0.036	-0.005	0.039
	(0.028)	(0.035)	(0.028)	(0.044)	(0.081)
6 to 11 Months	0.046	0.029	0.081**	0.028	0.054
	(0.025)	(0.028)	(0.025)	(0.036)	(0.104)
≥ 12 Months	0.032	0.019	0.066*	0.040	0.032
	(0.031)	(0.034)	(0.029)	(0.039)	(0.066)
Mean	0.065**	0.065*	0.057**	-0.011	0.017
Observations	36,516	36,504	36,516	35,586	33,981

	Panel II: Non-Violent Gun Crime					
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Weapon Law Violations	Drug/ Narcotic Violations	Destruction of Property	Kidnapping/ Abduction	Simple Assault
0 to 2 Months	0.082**	0.140**	0.027	0.020	0.124*	0.071
	(0.031)	(0.048)	(0.036)	(0.062)	(0.057)	(0.077)
3 to 5 Months	0.013	0.069	-0.035	0.060	0.050	0.125
	(0.047)	(0.060)	(0.054)	(0.076)	(0.048)	(0.075)
6 to 11 Months	0.028	0.101	-0.019	0.043	0.039	0.057
	(0.038)	(0.065)	(0.048)	(0.069)	(0.069)	(0.076)
≥ 12 Months	-0.049	0.047	-0.011	0.044	0.066	0.151*
	(0.028)	(0.072)	(0.037)	(0.056)	(0.049)	(0.074)
Mean	7.399	8.421	1.835	0.967	0.405	0.318
Observations	36,516	36,516	36,516	35,255	35,385	34,916

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column within each panel represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. Means of the dependent variables are reported. All models control for the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, and region-by-year fixed effects, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

Table 8: Heterogeneous Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline specification	17-year-olds and younger	18- to 23-year-olds	24- to 35-year-olds	36-year-olds and older	Males	Females	White	Black
0 to 2 Months	0.069**	-0.011	0.060*	0.038	0.092**	0.057*	0.067*	0.044	0.063**
	(0.024)	(0.031)	(0.030)	(0.026)	(0.033)	(0.023)	(0.031)	(0.045)	(0.022)
3 to 5 Months	-0.003	-0.007	0.011	-0.011	-0.008	-0.017	-0.003	-0.072	-0.009
	(0.030)	(0.034)	(0.035)	(0.031)	(0.022)	(0.029)	(0.027)	(0.037)	(0.030)
6 to 11 Months	0.039	0.034	0.037	0.050	0.026	0.031	0.064	0.048	0.021
	(0.025)	(0.045)	(0.030)	(0.027)	(0.038)	(0.026)	(0.036)	(0.040)	(0.028)
>12 Months	0.010	0.034	0.015	0.035	-0.031	-0.004	0.017	0.004	-0.007
	(0.026)	(0.035)	(0.031)	(0.023)	(0.016)	(0.025)	(0.028)	(0.018)	(0.029)
Mean	25.88	2.64	9.05	6.08	3.81	20.92	2.02	5.13	16.25
Observations	36,516	36,516	36,516	36,516	36,516	36,516	36,516	36,516	36,516

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column represents a separate Poisson regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the specified crime count in agency *a* and month *t*. Means of the dependent variables are reported. All models control for the covariates listed in Table 1, agency fixed effects, month-by-year fixed effects, agency-specific linear trends, and agency-by-year fixed effects, and agency population is set as the exposure variable. Standard errors are corrected for clustering at the city level.

Table 9: Synthetic Control Estimates

City	Years Since Initial GBP					
	0	1	2	3	4	5+
Akron, OH[1,3]	5.00***	8.05*	7.30*	7.21*	17.17***	12.60***
	[0.000]	[0.024]	[0.024]	[0.024]	[0.000]	[0.000]
Battle Creek, MI	-3.67	-3.45				
	[0.117]	[0.250]				
Cambridge, MA	-0.41					
	[0.785]					
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***	-1.80	10.66*	11.42*	17.79***	14.36*
	[0.000]	[0.459]	[0.031]	[0.010]	[0.000]	[0.020]
Columbia, SC[5]	3.21	-14.55	-1.72	8.99	-5.32	7.12
	[0.600]	[0.200]	[0.800]	[0.200]	[0.600]	[0.400]
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
	[0.667]	[0.111]	[0.278]	[0.056]	[0.389]	[0.889]
Denver, CO	-1.65	-3.98	-3.90	-1.70	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, MI[1]	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]	-4.16	-4.79	-1.12	-0.85		
	[0.095]	[0.121]	[0.655]	[0.733]		

Table 9: Synthetic Control Estimates (continued)

	Years Since Initial GBP					
City	0	1	2	3	4	5+
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]
North Charleston, SC[1,3,4]	-0.63	-16.86*	-17.04*	-10.25*	-14.96*	-0.62
	[0.750]	[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	-1.67	0.45	-1.23***	-5.00***	-3.61***	-3.20***
	[1.000]	[1.000]	[0.000]	[0.000]	[0.000]	[0.000]
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***	3.40	6.67*		
	[0.000]	[0.000]	[0.105]	[0.026]		
Somerville, MA	-0.25	-1.64				
	[0.875]	[0.354]				
Springfield, MA	4.60	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	-0.30				
	[0.329]	[0.871]				
Warwick, RI	-1.05	-0.26	-0.55			
	[0.564]	[0.897]	[0.821]			
Waterloo, IA[1]	3.42***	-0.83	1.44	0.94	-0.39	-0.35
	[0.000]	[0.615]	[0.385]	[0.615]	[0.923]	[0.769]
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
	[0.810]	[0.048]	[0.143]	[0.238]	[0.238]	[0.381]

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

[n] A subsequent GBP occurred n year(s) after initial GBP.

Notes: Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is the gun crime rate per 10,000 population. Up to 6 years of pre-treatment gun crime data were used to match treated cities to their synthetic control. Placebo-based p-values are reported in brackets using the proportion of placebo agencies with a pre-post GBP mean square prediction error (MSPE) ratio as large as the treatment GBP (Abadie et al. 2010).

Table 10: The Effect of GBPs on Gun-Related Deaths

Months relative to GBP	(1) Gun-Related Deaths	(2) Gun-Related Suicides	(3) Gun-Related Homicides
≥ 12 Months Before	0.005	0.001	-0.010
	(0.015)	(0.017)	(0.016)
6 to 11 Months Before	-0.012	0.009	-0.030*
	(0.012)	(0.020)	(0.015)
3 to 5 Months Before	-0.021	-0.008	-0.027
	(0.016)	(0.023)	(0.017)
1 to 2 Months Before	-	-	-
0 to 2 Months After	0.012	0.031	-0.006
	(0.012)	(0.021)	(0.016)
3 to 5 Months After	-0.022	-0.020	-0.031
	(0.016)	(0.023)	(0.016)
6 to 11 Months After	-0.025	-0.006	-0.040*
	(0.015)	(0.019)	(0.020)
≥ 12 Months After	-0.011	0.003	-0.035
	(0.017)	(0.019)	(0.019)
Observations	272,386	272,386	272,386

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

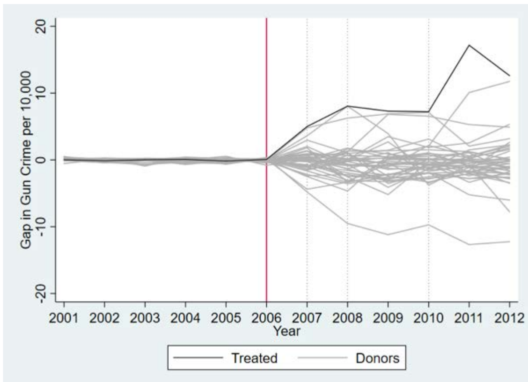
Notes: Each column represents a separate Poisson regression, where the omitted category is 1 to 2 months before treatment. Data on gun-related mortality at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the specified mortality count in county *c* and month *t*. All models control for the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering at the county level.

Appendix

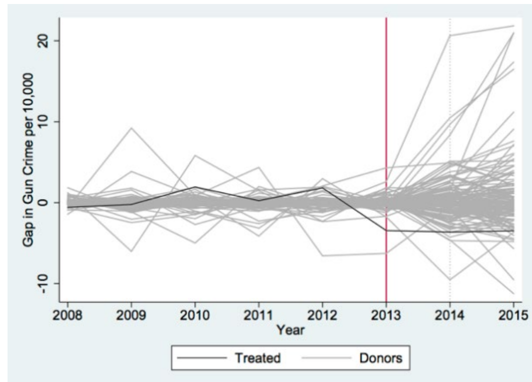
For Online Publication

Appendix Figure 1: Synthetic Difference in Gun Crime Rates, Treated Versus Placebo Cities

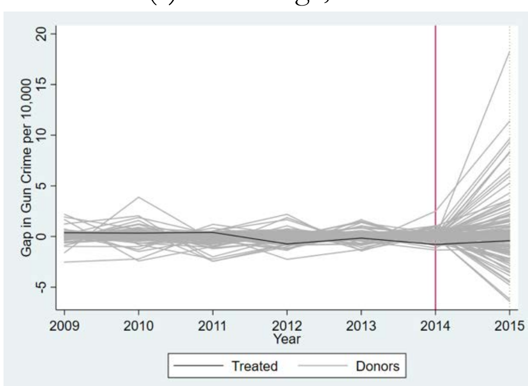
(a) Akron, OH



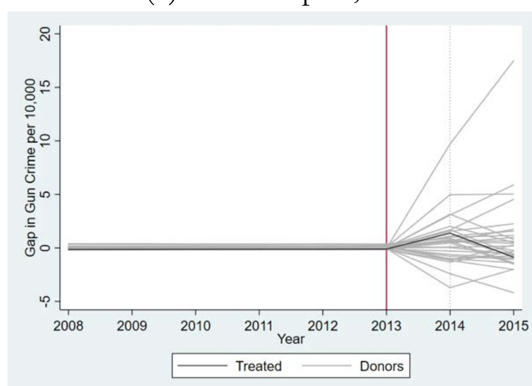
(b) Battle Creek, MI



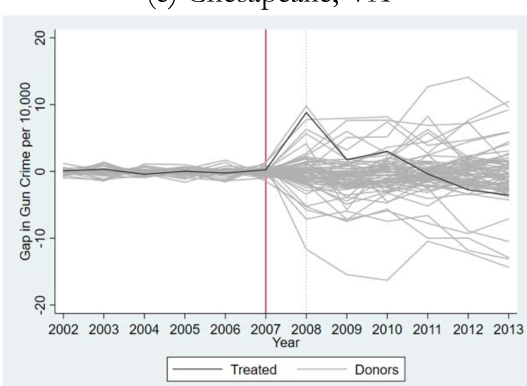
(c) Cambridge, MA



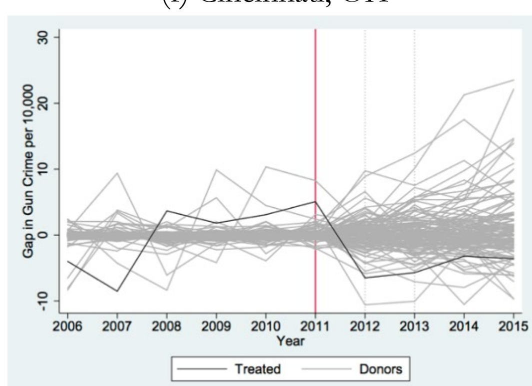
(d) Cedar Rapids, IA



(e) Chesapeake, VA

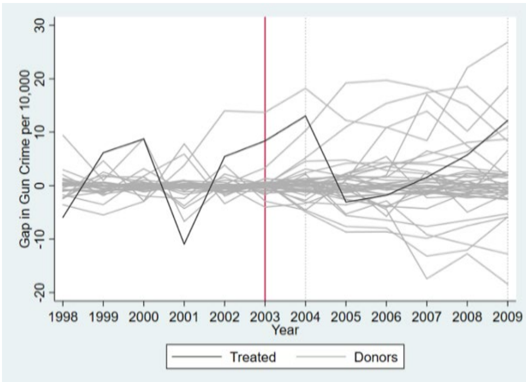


(f) Cincinnati, OH

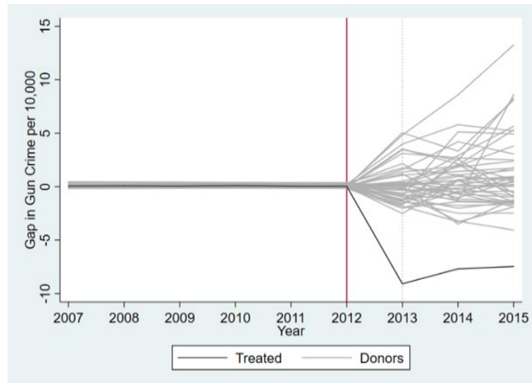


Appendix Figure 1: Synthetic Difference in Gun Crime Rates, Treated Versus Placebo Cities (continued)

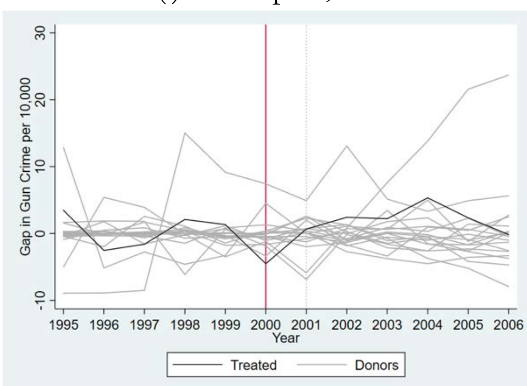
(g) Columbia, SC



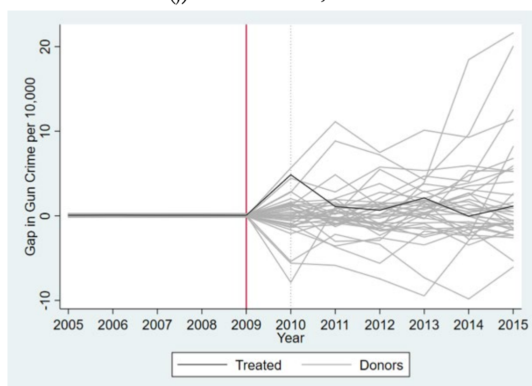
(h) Columbus, OH



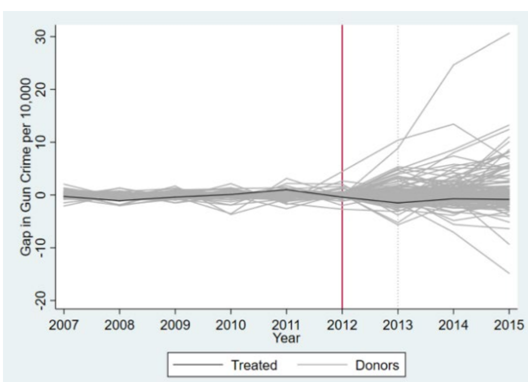
(i) Davenport, IA



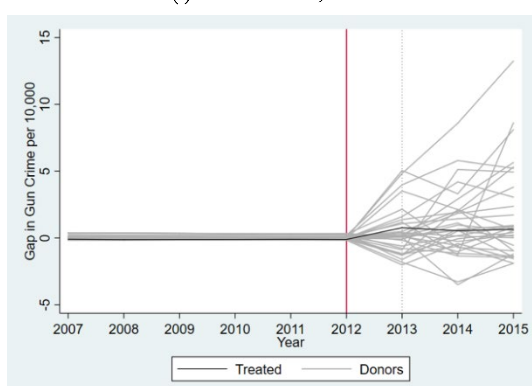
(j) Fall River, MA



(k) Greenwich, CT

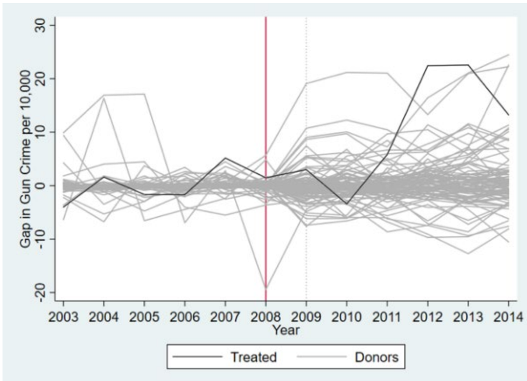


(l) Haverhill, MA

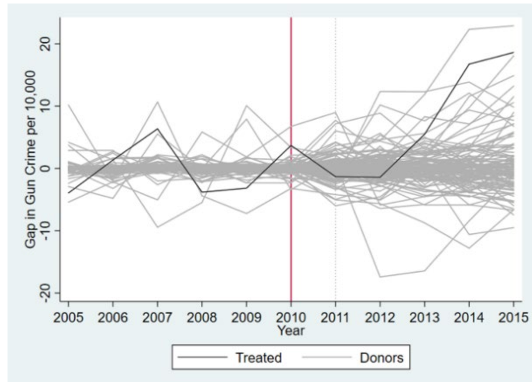


Appendix Figure 1: Synthetic Difference in Gun Crime Rates,
Treated Versus Placebo Cities (continued)

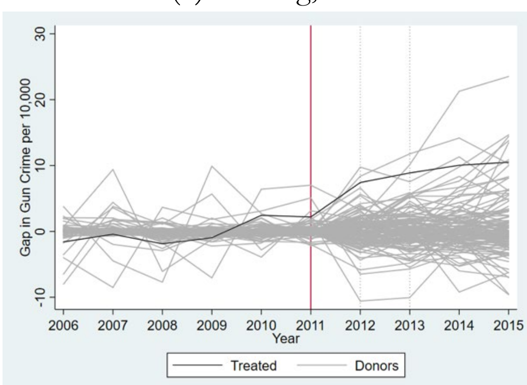
(m) Jackson, TN



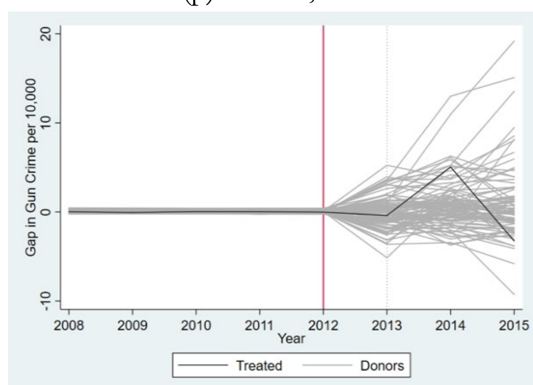
(n) Kalamazoo, MI



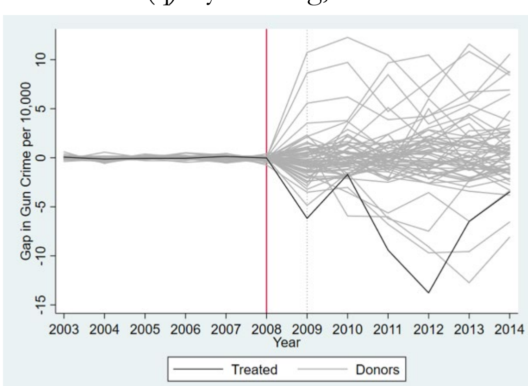
(o) Lansing, MI



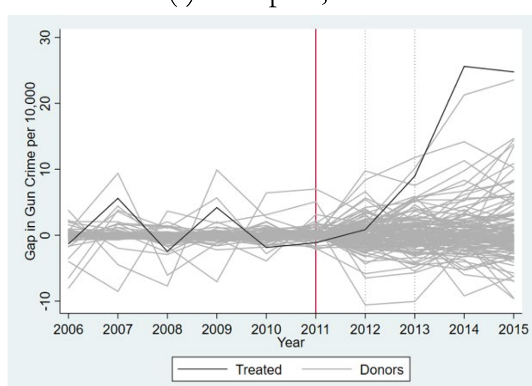
(p) Lowell, MA



(q) Lynchburg, VA

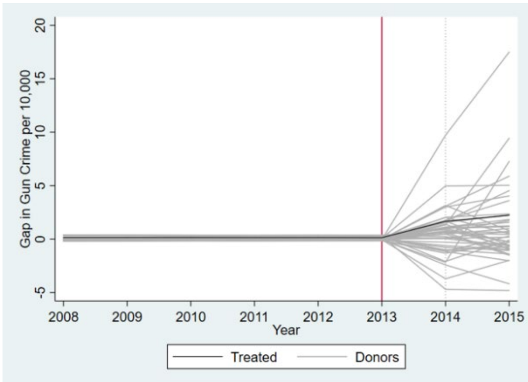


(r) Memphis, TN

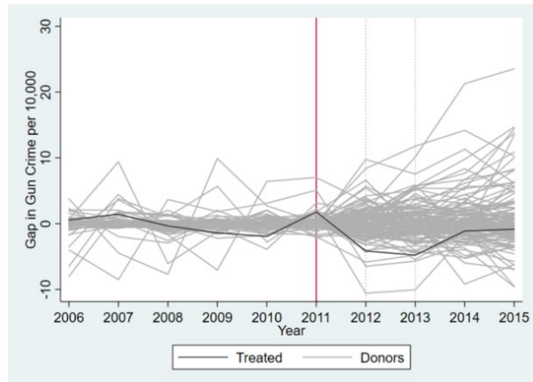


Appendix Figure 1: Synthetic Difference in Gun Crime Rates, Treated Versus Placebo Cities (continued)

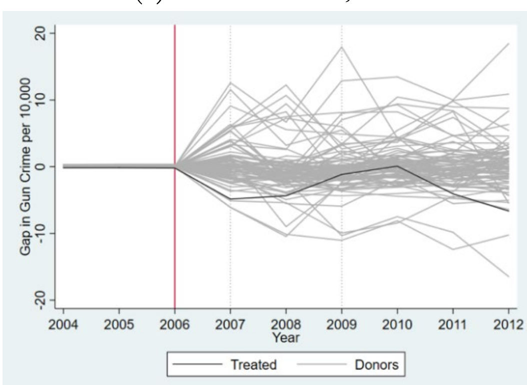
(s) Murfreesboro, TN



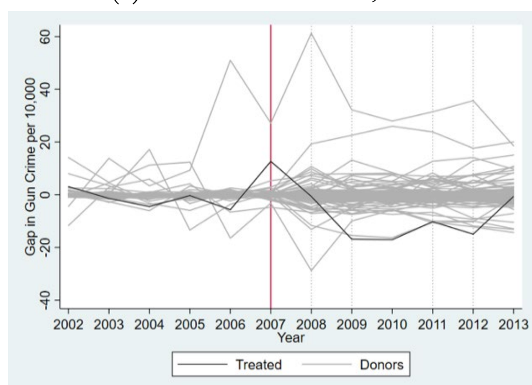
(t) Nashville, TN



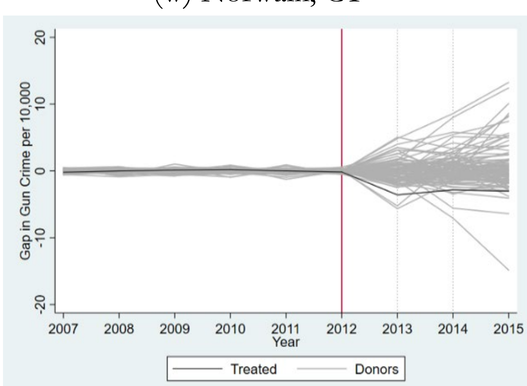
(u) New Bedford, MA



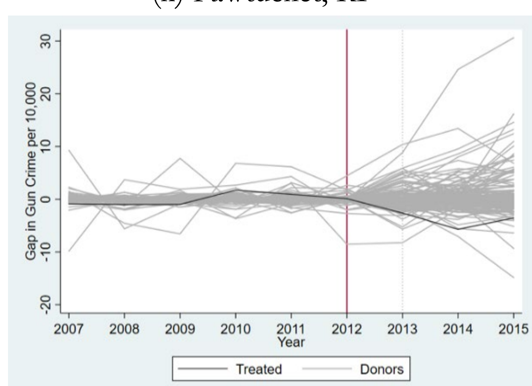
(v) North Charleston, SC



(w) Norwalk, CT

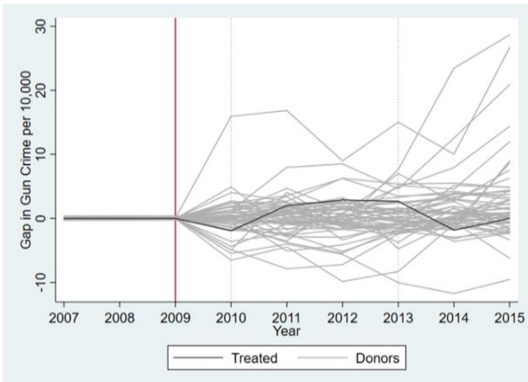


(x) Pawtucket, RI

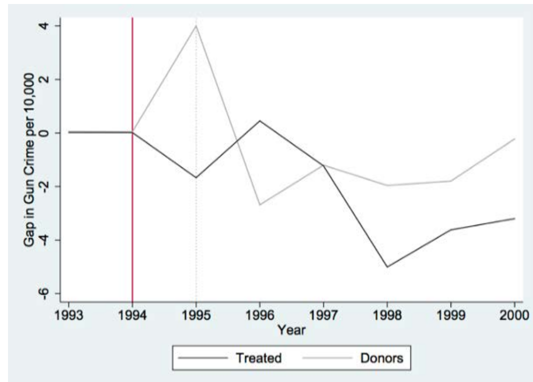


Appendix Figure 1: Synthetic Difference in Gun Crime Rates, Treated Versus Placebo Cities (continued)

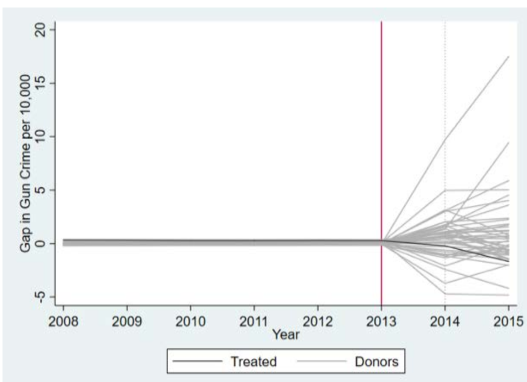
(y) Providence, RI



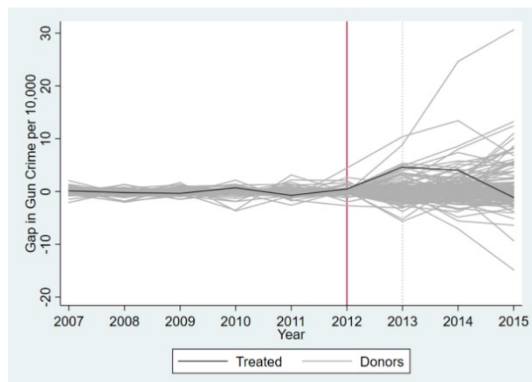
(z) Pocatello, ID



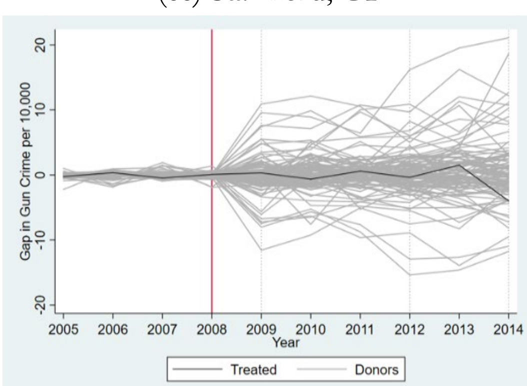
(aa) Somerville, MA



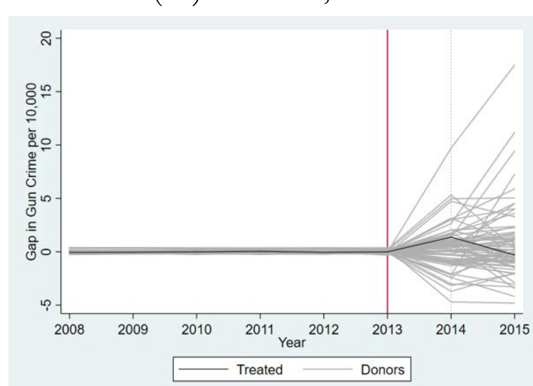
(bb) Springfield, MA



(cc) Stamford, CT

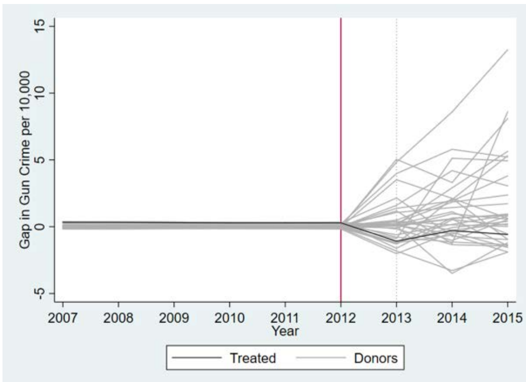


(dd) Waltham, MA

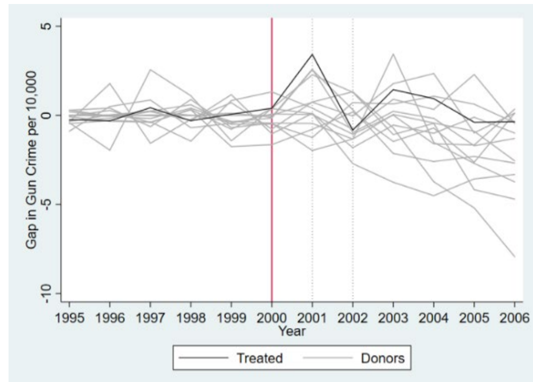


Appendix Figure 1: Synthetic Difference in Gun Crime Rates, Treated Versus Placebo Cities (continued)

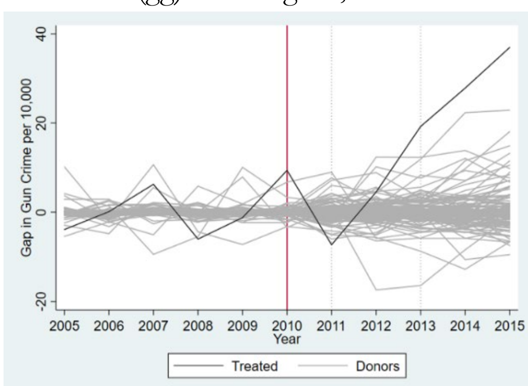
(ee) Warwick, RI



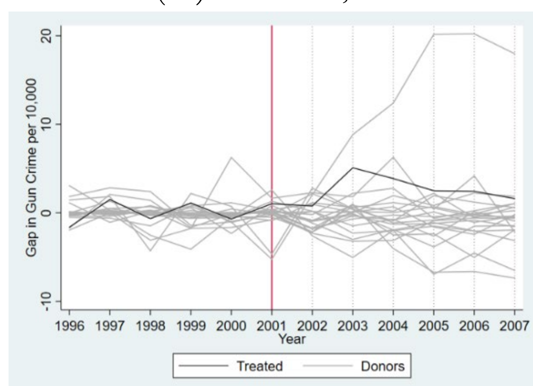
(ff) Waterloo, IA



(gg) Wilmington, DE

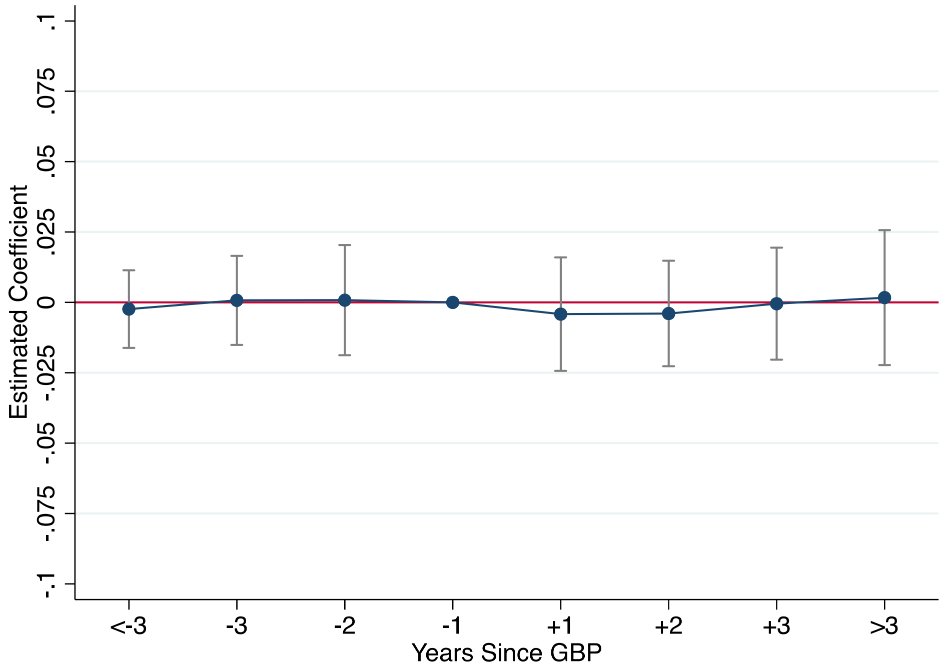


(hh) Worcester, MA



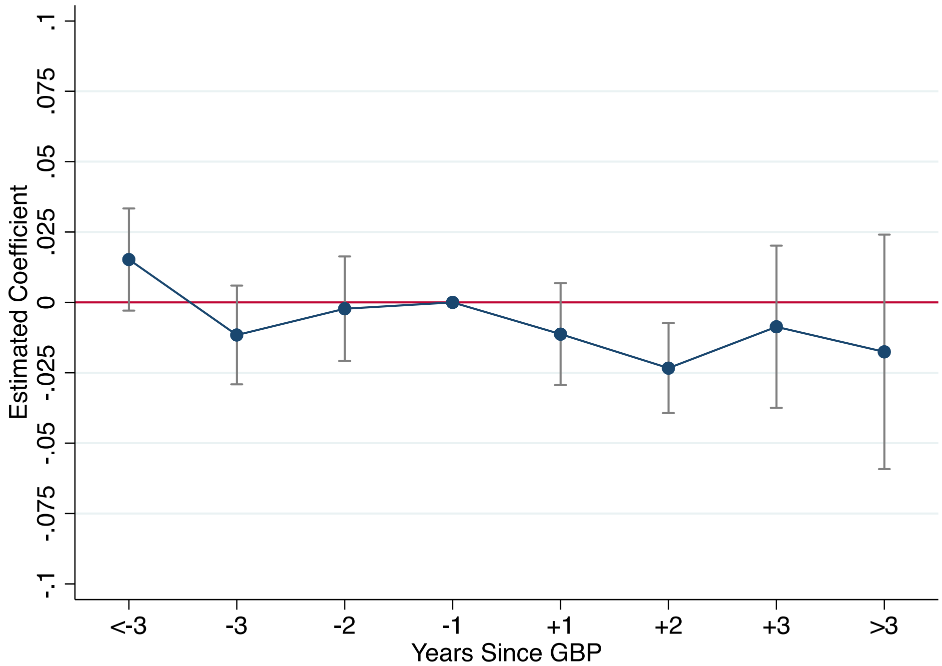
Notes: Each figure plots the results of a permutation test of the significance of the difference in the gun crime rate between the specified GBP city and its synthetic counterpart. The dark lines represent the difference for the treated city using the date of its intial GBP. The lighter lines represent the difference for the placebo cities using the date of the treated city's initial GBP.

Appendix Figure 2: Event-Study Analysis of Gun-Related Suicides, Long-Run



Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one year before treatment. Data on gun-related suicides at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the gun-related suicide count in county *c* and month *t*. Controls include the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering at the county level.

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



Notes: Poisson coefficient estimates (and their 95% confidence intervals) are reported, where the omitted category is one year before treatment. Data on gun-related homicides at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the gun-related homicide count in county *c* and month *t*. Controls include the covariates listed in Table 1, county fixed effects, month-by-year fixed effects, county-specific linear trends, and region-by-year fixed effects, and county population is set as the exposure variable. Standard errors are corrected for clustering 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

	Full	Hispanic	White	Black	Male	Female	Age ≤ 17	Age 18 to 23	Age 24 to 35	Age ≥ 36
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
Non-violent										
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: The category Hispanic is not mutually exclusive, including White Hispanic, Black Hispanic, and other Hispanic. Only the 20 most common non-violent gun crimes are show.

Appendix Table 2: Robustness Checks to Additional Controls for Spatial Heterogeneity (Poisson Versus Negative Binomial Models)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Months following GBP				**Panel I: Poisson**				
0 to 2 Months	0.079**	0.071*	0.072**	0.079**	0.075**	0.088***	0.077***	0.079***
	(0.029)	(0.028)	(0.027)	(0.025)	(0.023)	(0.020)	(0.020)	(0.020)
3 to 5 Months	-0.002	-0.009	-0.006	0.009	-0.004	0.009	0.000	0.009
	(0.021)	(0.025)	(0.025)	(0.023)	(0.034)	(0.031)	(0.027)	(0.025)
6 to 11 Months	0.040	0.033	0.035	0.047*	0.036	0.054*	0.047	0.060*
	(0.022)	(0.025)	(0.024)	(0.019)	(0.029)	(0.026)	(0.025)	(0.024)
≥ 12 Months	0.001	0.003	0.004	-0.001	-0.003	0.049	0.042	0.067*
	(0.012)	(0.009)	(0.009)	(0.010)	(0.030)	(0.029)	(0.028)	(0.028)
				Panel II: Negative binomial				
0 to 2 Months	0.081*	0.072	0.072	0.072	-0.014	0.018	0.025	0.027
	(0.041)	(0.041)	(0.041)	(0.039)	(0.039)	(0.041)	(0.039)	(0.039)
3 to 5 Months	-0.007	-0.018	-0.018	-0.014	-0.101**	-0.071*	-0.060	-0.056
	(0.032)	(0.031)	(0.031)	(0.030)	(0.034)	(0.034)	(0.033)	(0.032)
6 to 11 Months	0.043	0.032	0.031	0.034	-0.061	-0.012	0.006	0.013
	(0.031)	(0.029)	(0.029)	(0.028)	(0.032)	(0.034)	(0.032)	(0.032)
≥ 12 Months	0.016	0.001	0.001	0.002	-0.073*	-0.005	0.022	0.042
	(0.012)	(0.012)	(0.012)	(0.012)	(0.031)	(0.033)	(0.030)	(0.032)
Observations	36,516	36,516	36,516	36,516	36,516	36,516	36,516	36,516
Demographic controls?	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic and political controls?	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Gun-related policies and policing resources?	-	-	-	Yes	Yes	Yes	Yes	Yes
Agency-specific linear trends?	-	-	-	-	Yes	Yes	Yes	Yes
Agency-specific quadratic trends?	-	-	-	-	-	Yes	Yes	Yes
Region-by-year fixed effects?	-	-	-	-	-	-	Yes	Yes
Division-by-year fixed effects?	-	-	-	-	-	-	-	Yes

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column within each panel represents a separate regression. Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the gun-related crime count in agency *a* and month *t*. All models control for agency fixed effects and month-by-year fixed effects, and agency population is set as the exposure variable. Controls for demographics, socioeconomic and political characteristics, and gun-related policies and policing resources are listed in Table 1. Standard errors are corrected for clustering at the city level.

Appendix Table 3: Donor Cities Used in the Synthetic Control Analysis and Their Corresponding Weights

City	Weight	City	Weight
Akron, OH		Battle Creek, MI	
Lawrence, KS	0.499	Tyler, TX	0.436
Southfield, MI	0.181	Clarksville, TN	0.350
Peabody, MA	0.115	North Little Rock, AR	0.129
Hamden, CT	0.101	Saginaw, MI	0.060
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	0.080
Hamden, CT	0.101	Brookline, MA	0.066
Saginaw, MI	0.082	Owensboro, KY	0.040
Chesapeake, VA		Cincinnati, OH	
Johnson City, TN	0.391	Chattanooga, TN	0.770
Amarillo, TX	0.314	Rockford, IL	0.230
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

Appendix Table 3: Donor Cities Used in the Synthetic Control Analysis and Their Corresponding Weights (continued)

Lansing, MI		Lowell, MA	
Brockton, MA	0.707	Bend, OR	0.344
Tyler, TX	0.152	Owensboro, KY	0.250
Rockford, IL	0.100	Novi, MI	0.197
Rapid City, SD	0.041	Hoover, AL	0.125
		Rockford, IL	0.084
Lynchburg, VA		**Memphis, TN**	
Dearborn Heights, MI	0.259	Saginaw, MI	0.479
Greenville, SC	0.220	Richmond, VA	0.331
Knoxville, TN	0.170	North Little Rock, AR	0.189I
Grand Rapids, MI	0.145		
Rock Hill, SC	0.104		
Murfreesboro, TN		**Nashville, TN**	
Ogden, UT	0.322	Richmond, VA	0.413
Hamden, CTI	0.100	Charleston, WV	0.269
Waterford Township, MI	0.085	Saginaw, MI	0.110
Little Rock, AR	0.068	Suffolk, VA	0.097
North Little Rock, AR	0.047	Norfolk, VA	0.090
New Bedford, MA		**North Charleston, SC**	
Ames, IA	0.370	Saginaw, MI	0.560
Flower Mound, TX	0.244	Greenville, SC	0.173
Missoula, MT	0.186	Norfolk, VA	0.134
Grand Forks, ND	0.084	Richmond, VA	0.126
St. George, UT	0.060		
Norwalk, CT		**Pawtucket, RI**	
Sterling Heights, MI	0.578	Livonia, MI	0.344
Lynn, MA	0.209	Redford Township, MI	0.317
Livonia, MI	0.141	Rapid City, SD	0.305
Brockton, MA	0.070	Rockford, IL	0.034
Taylor, MI	0.003		
Providence, RI		**Pocatello, ID**	
Tyler, TX	0.934	Provo, UT	0.409
Chattanooga, TN	0.056	Des Moines, IN	0.116
North Little Rock, AR	0.010	West Jordan, UT	0.069
		Boise, ID	0.065
		Sioux City, IA	0.061
Somerville, MA		**Springfield, MA**	
Corvallis, OR	0.391	Medford, OR	0.451
Brookline, MA	0.235	Chattanooga, TN	0.321
Victoria, TX	0.152	Rockford, IL	0.118
West Hartford, CT	0.074	Richmond, VA	0.110
Flower Mound, TX	0.050		

Appendix Table 3: Donor Cities Used in the Synthetic Control Analysis and Their Corresponding Weights (continued)

Stamford, CT		Waltham, MA	
Sioux Falls, SD	0.711	Fairfield, CT	0.316
Bismarck, ND	0.171	Plymouth, MA	0.259
Redford Township, MI	0.104	Brookline, MA	0.252
Saginaw, MI	0.015	Shelby Township, MI	0.133
		Novi, MI	0.038

Warwick, RI		Waterloo, IA	
Ames, IA	0.370	Fargo, ND	0.434
Flower Mound, TX	0.244	Greenville, SC	0.222
Missoula, MT	0.186	Charleston, SC	0.159
Grand Forks, ND	0.084	Saint George, UT	0.125
St. George, UT	0.060	Plymouth, MA	0.057
Wilmington, DE		**Worcester, MA**	
Chattanooga, TN	0.517	Des Moines, IA	0.447
Saginaw, MI	0.483	Sandy, UT	0.298
		Council Bluffs, IA	0.122
		Nampa, ID	0.077
		Sioux City, IA	0.056

Appendix Table 4. Synthetic Control Estimates by the Size of the Gun Buyback

	Years Since Initial GBP					
City	0	1	2	3	4	5+
Buyback Size > 75th Percentile						
Wilmington, DE[2]	-2.98	1.15	16.75***	25.97***	37.65***	
	[0.189]	[0.509]	[0.000]	[0.000]	[0.000]	
Davenport, IA	0.67	2.43	2.21	5.33	2.35	-0.16
	[0.667]	[0.111]	[0.278]	[0.056]	[0.389]	[0.889]
Akron, OH[1,3]	5.00***	8.05*	7.30*	7.21*	17.17***	12.60***
	[0.000]	[0.024]	[0.024]	[0.024]	[0.000]	[0.000]
North Charleston, SC[1,3,4]	-0.63	-16.86*	-17.04*	-10.25*	-14.96*	-0.62
	[0.750]	[0.013]	[0.013]	[0.038]	[0.013]	[0.838]
Columbia, SC[5]	3.21	-14.55	-1.72	8.99	-5.32	7.12
	[0.600]	[0.200]	[0.800]	[0.200]	[0.600]	[0.400]
Warwick, RI	-1.05	-0.26	-0.55			
	[0.564]	[0.897]	[0.821]			
Springfield, MA	4.60	4.03	-1.15			
	[0.061]	[0.122]	[0.687]			
Worcester, IA[1,2,3,4,5]	0.78	5.09*	3.88	2.51	2.44	1.62
	[0.810]	[0.048]	[0.143]	[0.238]	[0.238]	[0.381]
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]
Buyback Size: 25th to 75th Percentile						
Waterloo, IA[1]	3.42***	-0.83	1.44	0.94	-0.39	-0.35
	[0.000]	[0.615]	[0.385]	[0.615]	[0.923]	[0.769]
Kalamazoo, MI	-1.32	-1.42	5.48	16.74*	18.60**	
	[0.453]	[0.443]	[0.085]	[0.019]	[0.009]	
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]
Cleveland, OH[1,3,4,5]	13.32***	-1.80	10.66*	11.42*	17.79***	14.36*
	[0.000]	[0.459]	[0.031]	[0.010]	[0.000]	[0.020]
Lansing, MI[1]	8.12*	9.38*	12.33*	12.46*		
	[0.026]	[0.026]	[0.017]	[0.043]		
Memphis, TN[1]	0.84	8.95*	25.60***	24.76***		
	[0.638]	[0.026]	[0.000]	[0.000]		
Pawtucket, RI	-2.74	-3.93	-1.78			
	[0.179]	[0.128]	[0.479]			
Waltham, MA	1.37	-0.30				
	[0.329]	[0.871]				

Appendix Table 4. Synthetic Control Estimates by the Size of the Gun Buyback (continued)

	Years Since Initial GBP					
City	0	1	2	3	4	5+
Buyback Size: 25th to 75th Percentile						
Cambridge, MA	-0.41					
	[0.785]					
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]
Columbus, OH	-9.78**	-3.63	-3.39			
	[0.008]	[0.151]	0.303]			
Haverhill, MA	0.83	0.61	0.73			
	[0.571]	[0.750]	[0.750]			
Pocatello, ID	-1.67	0.45	-1.23***	-5.00***	-3.61***	-3.20***
	[1.000]	[1.000]	[0.000]	[0.000]	[0.000]	[0.000]
Lowell, MA	-0.39	5.05	-3.26			
	[0.828]	[0.057]	[0.230]			
Cedar Rapids, IA	1.83	-0.64				
	[0.308]	[0.692]				
Norwalk, CT[1]	-3.37	-2.63	-2.83			
	[0.087]	[0.184]	[0.282]			
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]
Buyback Size < 25th Percentile						
Somerville, MA	-0.25	-1.64				
	[0.875]	[0.354]				
Greenwich, CT	-1.50	-0.72	-0.84			
	[0.383]	[0.652]	[0.757]			
Cincinnati, OH[1]	-6.49*	-5.71	-3.19	-3.58		
	[0.034]	[0.069]	[0.241]	[0.284]		
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]
Nashville, TN[1]	-4.16	-4.79	-1.12	-0.85		
	[0.095]	[0.121]	[0.655]	[0.733]		
Pueblo, CO	14.13***	14.99***	3.40	6.67*		
	[0.000]	[0.000]	[0.105]	[0.026]		
Murfreesboro, TN	1.54	2.07				
	[0.412]	[0.265]				
Denver, CO	-1.65	-3.98	-3.90	-1.70	0.13	-1.21
	[0.378]	[0.134]	[0.122]	[0.451]	[0.976]	[0.707]

Appendix Table 4. Synthetic Control Estimates by the Size of the Gun Buyback (continued)

	Years Since Initial GBP					
City	0	1	2	3	4	5+
Buyback Size Not Reported						
Battle Creek, MI	-3.67	-3.45				
	[0.117]	[0.250]				
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]
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]

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

[n] A subsequent GBP occurred n year(s) after initial GBP.

Notes: Data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is the gun crime rate per 10,000 population. Up to 6 years of pre-treatment gun crime data were used to match treated cities to their synthetic control. p-values are reported in brackets using the proportion of placebo agencies with a post-treatment root mean squared error greater than the treated city for each given year.

Appendix Table 5: The Effect of GBPs on Gun-Related Crimes and Deaths by the Size of the Gun Buyback

	(1)	(2)	(3)	(4)
Months following GBP	Gun-Related Crime	Gun-Related Deaths	Gun-Related Suicides	Gun-Related Homicides
0 to 2 Months * GBP Size	0.009	0.007	0.023	0.007
	(0.006)	(0.005)	(0.012)	(0.006)
3 to 5 Months * GBP Size	-0.000	0.003	-0.006	0.005
	(0.005)	(0.005)	(0.012)	(0.008)
6 to 11 Months * GBP Size	-0.001	0.002	-0.002	0.005
	(0.004)	(0.006)	(0.010)	(0.006)
≥ 12 Months * GBP Size	-0.000	-0.001	0.010	-0.005
	(0.004)	(0.005)	(0.006)	(0.005)
Mean	25.88	2.42	1.30	1.07
Observations	35,509	269,004	269,004	269,004

*** Statistically significant at 0.1% level; ** at 1% level; * at 5% level.

Notes: Each column represents a separate Poisson regression, and GBP size is equal to the number of guns bought back per 1,000 population. In column (1), data on crime reports at the agency-month level are from the National Incident-Based Reporting System and cover the period 1991-2015. The dependent variable is equal to the gun-crime count in agency *a* and month *t*. In columns (2)-(4), data on gun-related mortality at the county-month level are from the National Vital Statistics System and cover the period 1991-2015. The dependent variable is equal to the specified mortality count in county *c* and month *t*. Means of the dependent variables are reported. All models control for the covariates listed in Table 1, unit-specific fixed effects (i.e., agency or county fixed effects), month-by-year fixed effects, unit-specific linear trends, and region-by-year fixed effects, and unit-specific population is set as the exposure variable. Standard errors are corrected for clustering at the unit level.

Appendix Table 6: Descriptive Statistics, 1991-2015, Unweighted

	Mean	SD	Source
Crime outcomes			
Gun Crime Count	25.879	68.140	National Incident-Based Reporting System (NIBRS)
Gun Crime Count, Non-Violent	7.399	16.674	
Gun Crime Count, Violent	18.480	53.282	
Non-Gun Crime Count	870.266	1,208.272	
Non-Gun Crime Count, Non-Violent	825.931	1,139.367	
Non-Gun Crime Count, Violent	44.336	77.038	
Vital statistics outcomes			
Firearm-Related Deaths	2.370	6.036	National Vital Statistics System (NVSS)
Firearm-Related Homicides	1.068	4.260	
Firearm-Related Suicides	1.302	2.363	
Demographic controls			
Population % Age 15-19[a]	0.072	0.010	Surveillance, Epidemiology, and End Results (SEER)
Population % Age 20-29[a]	0.149	0.035	
Population % White[a]	0.734	0.141	
Population % Black[a]	0.132	0.133	
Population % Hispanic[a]	0.092	0.088	
Population % Male[a]	0.490	0.009	
Percentage College Graduates[b]	0.293	0.066	American Community Survey (ACS)
Socioeconomic and political controls			
Per Capita Income ($2015)[a]	45,800	13,576	American Community Survey (ACS)
Unemployment Rate[a]	5.868	2.494	Bureau of Labor Statistics (BLS)
Minimum Wage ($2015)[a]	7.504	0.945	Vaghul and Zipperer (2016)
Democrat Governor[b]	0.432	0.495	Ballotpedia
Gun-related policies and policing resources			
Background Checks per 100,000 Population[b]	5,013	5,318	National Instant Criminal Background Check System (NICS)
Stand Your Ground Law[b]	0.335	0.471	Giffords Law Center
Shall Issue Law[b]	0.757	0.429	
Gun Lock Required[b]	0.330	0.470	
State/Federal Minimum Age Law[b]	0.993	0.084	
Negligent Child Access Prevention Law[b]	0.305	0.460	Anderson and Sabia (2021)
Reckless Child Access Prevention Law[b]	0.248	0.432	
Police Expenditure per 100,000 ($2015)[b]	263.838	45.656	Bureau of Justice Statistics (BJS)
Police Officers per 100,000 Population[b]	2.147	0.425	

[a] Varies at the county level.
[b] Varies at the state level.