The Impact of Legal Firearm Access on Violence in a High-Violence Context: Evidence from Brazil

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Abstract

I estimate the effect of legal firearm access on violence in Brazil. Between 2004 and 2018, the opening of a shooting range, an institution tied by law to legal firearm acquisition, shifted local access to firearms across Brazilian municipalities. Ranges tend to open in larger and richer municipalities with greater institutional capacity. I focus on their neighbors: the access a range creates crosses municipal borders, and neighboring municipalities are smaller and less able to contain violence. Using a staggered difference-in-differences design that models spatial spillovers, I find that firearm homicides rise by 1.9 per 100,000 inhabitants (24%) in municipalities within 70 km of a new range, with no offsetting decline in homicides by other weapons. Municipalities where ranges open show no robust effect, and effects decline with baseline income in both groups. Firearm registrations rise in both groups, consistent with expanded legal ownership as a mechanism. The additional homicides concentrate among male victims aged 15 to 39 and occur outside the home, where the law prohibits legal firearms from being carried, patterns consistent with illegal carry and diversion to the illegal market. Over the period, the spillover effect implies about 6,100 additional firearm homicides. These results show that the harm from expanded firearm access can surface away from the point of access, in places with less capacity to contain it.

1.1 Introduction

For at least three decades, concern over gun violence has driven the debate on firearm regulation across countries (Small Arms Survey 2011). One side of this debate holds that citizens have a right to own firearms and that the state cannot enact laws that infringe on that right. The other side calls for stricter laws in fear of victimization (Pierre 2019). The empirical literature on how firearms affect violence is extensive, but the evidence is not strong. In 2005, the critical review conducted by the National Research Council committee found insufficient evidence to draw a conclusion about the causal relationship between gun prevalence and violent crime (NRC 2005). Studies suffered from methodological issues related to measurement and internal validity. These limitations have persisted in the literature.

In this paper, I examine how increases in firearm availability affect violence using an empirical strategy that addresses common limitations in the literature. In particular, I exploit a new dataset on shooting-range openings in Brazilian municipalities to estimate how increases in firearm availability affect homicide. In Brazil, shooting ranges are linked to the acquisition, use, and diversion of firearms. I use them as a source of variation in firearm availability, sidestepping reliance on proxies that have led to measurement errors in prior studies. I then use a staggered difference-in-differences design with spatial spillover effects to estimate the causal effect of shooting ranges on violence and discuss its internal validity.

Brazil is one of the most violent countries in the world. In 2018, it registered 27 homicides per 100,000 inhabitants, and 70% of them involved a firearm. For comparison, the United States, which concentrates most of the academic literature on gun violence, registered 5 homicides per 100,000 in the same year. In Brazil, the Disarmament Statute, a federal law enacted in 2003, governs legal access to firearms. The law allows citizens to register firearms for self-defense, shooting sports, hunting, or collection, but makes registration costly: applicants must demonstrate an effective need or affiliate with a shooting range, pass background checks, and complete technical and psychological exams. Shooting ranges occupy a central position in this process, and their opening can affect violence through at least three channels.

First, shooting ranges can increase firearm registration. The law requires affiliation with a licensed range to register a firearm for shooting sports or hunting, and requires every applicant

to pass a shooting test at an accredited facility. Ranges also lower the administrative burden of licensing: staff know the process and can guide members through it, much as application assistance shifts take-up in other bureaucratic settings (Bettinger et al. 2012). Second, ranges can increase firearm circulation, since owners begin transporting their weapons between home and the facility. Third, ranges can increase diversion to the illegal market. Members may resell their firearms or act as straw purchasers, and the facilities themselves may be robbed. My data allow me to test the first channel directly. The other two are plausible given the evidence on diversion in Brazil (Langeani and Passos 2022), but I cannot test them.

I estimate the effect of shooting range openings on homicide using a staggered difference-in-differences design. I focus on the municipalities that neighbor host municipalities, defined as those where a shooting range opens. Where shooting ranges open is not random: they open in larger, richer, and more educated municipalities, which plausibly have stronger institutions and policing. The access a range creates, however, does not stay within municipal borders. Residents of nearby municipalities can commute to the range to qualify for ownership, and firearms can cross borders through the illegal market. These nearby municipalities are smaller and tend to have less capacity to contain the violence that expanded access may bring.

To capture effects beyond the host municipality, the design does not assume that the treatment stays within municipal borders. If a shooting range affects violence in neighboring municipalities, assigning those municipalities to the control group would violate the no-interference assumption and bias the local estimate toward zero. Following Butts (2023), I therefore model spillover exposure explicitly: municipalities that open their first shooting range form the treated group, municipalities within 70 km of a treated municipality form a separate exposure group, and the control group contains only municipalities beyond that distance. The 70 km benchmark corresponds to the mean distance between municipalities within the same IBGE microregion, and I vary it in robustness checks.

The data cover 3,739 Brazilian municipalities at the municipality-year level from 2004 to 2018. The panel begins after the enactment of the Disarmament Statute and ends before the Bolsonaro administration's regulatory changes, so a single legal regime governs firearm access throughout the period. I estimate treatment effects with the two-stage difference-in-differences estimator of Gardner et al. (2024), which remains valid under staggered adoption when

treatment effects vary across groups and over time. Identification rests on parallel trends between exposed and distant municipalities, which I assess through pre-treatment coefficients in the event-study analysis. Anticipation is unlikely given the nature of the treatment: a shooting range requires an open facility, so an announced range provides no access.

The data allow me to go beyond the aggregate effect. I observe self-defense firearm registrations at the municipality level, so I can test directly whether shooting ranges increase legal ownership. The mortality records characterize each death by weapon, victim sex, victim age, and place of occurrence, which lets me decompose the estimated effect and assess which channels are consistent with the composition of the additional homicides. Finally, I examine how the effect varies with baseline municipality characteristics.

I find that municipalities within 70 km of a new shooting range experience an increase of 1.9 firearm homicides per 100,000 inhabitants, or 24% of the group's pre-treatment mean. Homicides committed with other weapons do not fall, so the overall homicide rate rises by 1.7 per 100,000. In the event-study analysis, the effect appears in the year of the opening and persists over the six-year window, while pre-treatment coefficients remain close to zero. The absence of pre-trends indicates that violence was not already rising near future shooting ranges. The spillover estimate is stable across robustness checks, including alternative exposure distances, excluding the North region, and excluding large municipalities.

As expected given their greater capacity, host municipalities show no robust effect on homicide: the local point estimate changes sign across specifications and is never precisely estimated. Heterogeneity analysis supports this interpretation. Estimates decline as baseline income per capita rises in both host and neighboring municipalities, and the local effect loses significance at the 75th percentile of income. Expanded firearm availability thus produces violence where state capacity is weakest.

Firearm registration is a relevant mechanism behind this effect. After a shooting range opens, self-defense firearm registrations rise by 5.1 per 100,000 inhabitants in the host municipality and by 5.5 in municipalities within 70 km. Legal ownership thus expands beyond the host municipality. The composition of registrations differs between groups: handguns drive the increase in host municipalities, while long guns account for most of the increase among neighbors, consistent with their less urban character. Host municipalities therefore

experience the increase in registrations without a corresponding increase in homicides.

The composition of the additional homicides narrows down the channels through which registered firearms translate into violence. The effect concentrates among male victims, among victims aged 15 to 39, and in deaths occurring outside the home. Each margin points away from the legal owner as the victim. Registered owners in Brazil average 45 years of age, must store firearms at a registered address, and may not carry them in public. The additional deaths therefore involve younger people than the average buyer and occur where the law forbids the firearm to be. I find no effect on female victims, which suggests that domestic violence is not a relevant margin. These patterns are consistent with two channels: legal owners carrying their firearms illegally, and firearms diverted to third parties who use them in crimes. However, my data do not allow me to distinguish between them.

My paper contributes most directly to a large empirical literature on how firearm availability affects violence (Lott and Mustard 1997; Duggan 2001; Ayres and Donohue III 2003; Cook and Ludwig 2006; Manski and Pepper 2018; Edwards et al. 2018; Donohue et al. 2019; Depew and Swensen 2022; Billings 2023; Koenig and Schindler 2023). Due to a lack of reliable data on gun ownership, many studies use indirect measures for firearm prevalence. The most common is the share of suicides committed using a firearm (FS/S) to capture the prevalence of household firearm ownership (Cook and Ludwig 2006; Kovandzic et al. 2013; Siegel et al. 2014). However, the correlation between FS/S and actual gun ownership weakens at finer geographic or demographic levels of aggregation, and the relationship shifts over time and across demographic groups, pointing to a range of social and cultural factors that remain poorly understood (NRC 2005). Others use even more contested proxies, such as gun magazine subscriptions (Duggan 2001). The concern is that the proxy moves in response to the same shocks that move the outcome. I sidestep this problem by using the shooting range openings as the source of variation for firearm availability. I also show directly that these openings increase firearm registrations, a link between the source of variation and legal ownership that this literature has rarely documented.

Later work focused on changes in firearm legislation at the state level in the U.S., using them as a reduced-form for the effect of firearms on violence. For example, Depew and Swensen (2022) use the may-issue licensing regime created by the 1911 Sullivan Act in New

York; Edwards et al. (2018) and Koenig and Schindler (2023) use variation in firearm purchase delays; and Manski and Pepper (2018) and Donohue et al. (2019) use variation in right-to-carry laws. Aggregation to the state level, however, leaves few treated units, which limits statistical power and masks heterogeneity within states. The imprecise results of such studies have long been pointed out (NRC 2005). My design shares the reduced-form logic of this literature but uses municipal rather than state variation. The finer unit sharpens precision and reveals heterogeneity that state aggregates mask. There is also anticipation of legislative changes. Depetris-Chauvin (2015) finds that the surge in gun demand before the 2008 election was driven partly by fear of future gun-control policy. Bollman et al. (2025) find similar evidence. Violations of the no-anticipation assumption can bias the results of these studies. In my setting, this violation is unlikely because the shooting range must be open before citizens can become members, register firearms, or use it to divert such weapons.

Municipal-level data also allows me to estimate spillovers onto neighboring municipalities, something that has been overlooked in the literature. Most studies do not account for spatial spillovers, despite evidence of their relevance to the effect of firearms on violence. Knight (2013) uses crime-gun tracing data to show that firearms flow from U.S. states with weak regulations to nearby states with strict regulations, and that criminal gun possession is higher in states exposed to weak regulations next door. Across the border, Dube et al. (2013) find that the 2004 expiration of the U.S. assault weapons ban raised gun homicides in Mexican municipalities close to states where the expiration bound, relative to municipalities close to California, which retained a state-level ban. In both settings, local differences in legal access displace violence and criminal gun possession toward neighboring jurisdictions, which suggests that studies confined to the treated unit understate the total effect of firearm availability.

Lastly, I contribute to this literature by providing evidence for a context outside the United States. The U.S. is a setting with a uniquely strong firearm culture and permissive firearm legislation (Small Arms Survey 2011), and where the illegal firearm market is considered "thin" and with higher prices than the legal market (Cook, Ludwig, et al. 2007; UNODC 2020). In Brazil, acquiring a legal firearm involves stringent bureaucratic requirements, and right-to-carry permissions are rare. Legal acquisition is often more expensive than purchasing from the illegal market when it comes to handguns. My findings showcase that, even in a

setting where the full price in the legal market is higher due to higher firearm prices and the administrative burden, the opening of a node in the legal market can move the needle on violence. Lower police capacity to solve homicides and more prevalent organized crime compared to the U.S. might overcome these other factors. This setting is common in Latin American countries, making my results relevant to public policy in the region.

The remainder of this paper is organized as follows. Section 1.2 provides background on the Brazilian legal framework governing firearms and shooting ranges. Section 1.3.1 details the data used as well as the sample selection and summary statistics. Section 1.3.2 presents the empirical strategy for the staggered differences-in-differences approach. Section 1.4 reports the results. Section 1.5 presents the study's conclusion.

1.2 Background

1.2.1 Legal firearm access in Brazil

Until 2003, anyone over the age of 21 could legally carry a firearm in public spaces in Brazil. Handguns could be purchased with few bureaucratic requirements in sporting goods stores and were advertised in newspapers and magazines, often using public insecurity as a marketing appeal (Alessi 2017). In 2003, the federal law known as the Disarmament Statute was enacted to regulate access to firearms. According to its proponents, wide firearm availability, whether legal or illegal, contributed to an increase in homicides driven by trivial motives. The law therefore introduced stricter regulations on the possession, carrying, and commercial sale of firearms and ammunition (Ribeiro 2023). As of today, it is the law that regulates access to legal firearms in Brazil. Under the Disarmament Statute, citizens can legally own a firearm if they obtain authorization from either the Federal Police or the Army, with shooting ranges playing an important role in both processes.

Federal Police registration. The Federal Police regulates firearms intended for self-defense. An applicant must declare an effective need for the firearm, which the Federal Police evaluates and may reject. Beyond need, the applicant must be at least 25 years old, demonstrate good standing through a clean criminal record and the absence of any ongoing investigations, and

have a lawful occupation and fixed residence. The applicant must also demonstrate technical capacity through a shooting test conducted by an accredited instructor and psychological aptitude through an exam administered by an accredited psychologist. These requirements filter who may buy; a second set of rules restricts what may be bought.

What may be bought is limited to firearms of permitted use (*uso permitido*), a category defined by Army regulation. In practice, permitted use meant handguns in lower-energy calibers plus some long guns such as smoothbore shotguns (*espingardas*). The self-defense purpose shapes how the firearm may be used. Possession confines the firearm to the interior of the owner's residence, or to the workplace when the owner is the business owner or legal representative. The firearm may leave these premises only with a specific transport authorization from the Federal Police, granted for purposes such as repair or sale.

Data from SINARM between 2011 and 2018 show that 97% of citizens who registered a self-defense-intended firearm were male and averaged 45 years old. Handguns represented 72% of registrations (55% pistols and 17% revolvers) and long guns represented 27%. The approval rate for firearm acquisition requests is influenced by political context. The decisions are made by the Federal Police superintendents of each state. These superintendents are appointed by the director-general of the Federal Police, who in turn is appointed by the president. Under pro-gun president Jair Bolsonaro (2019-2022), the approval rate reached 88%. Under the Lula administration (2023-2026), which opposes arming the population, this figure fell to 58%.

Army registration. The Army regulates firearms intended for collection, sports shooting, and hunting – categories known as CACs (*colecionadores, atiradores, caçadores*). The personal requirements mirror the Federal Police route: good standing, lawful occupation, fixed residence, and the same minimum age of 25 for acquisition. The routes diverge at the justification. Instead of convincing the authority of a need, the applicant proves participation in a regulated activity, which for sport shooters means affiliation with a shooting club registered with the Army. Because the activity itself justifies the firearm, there is no effective-need test.

The activity also widens the menu of firearms. CACs may acquire permitted-use firearms and, with specific Army authorization, firearms of restricted use, a category closed in the Federal Police route. Use follows the activity as strictly as acquisition does. A sport shooter's

firearm is meant to be fired at registered shooting ranges and competitions, a hunter's for authorized hunting, and a collector's, essentially, not to be fired at all. Movement between residence and shooting ranges is permitted only under a transport permit (*guia de tráfego*) issued by the Army, with the firearm unloaded and separated from ammunition. Outside the activity, the CAC holds the firearm under the same rules as a self-defense owner: the firearm stays at home, and carrying remains prohibited.

Data from SIGMA show that 79% of firearms were registered for sport shooting, 16% for hunting, and 5% for collection. As for self-defense firearms, handguns are the most common, representing 62% (48% pistols and 13% revolvers) while long guns represent 38%. However, firearms registered with the Army are higher-powered. Among pistols, for example, 73% are in the restricted-use category. There is no information on the approval rate of CACs registries.

Shooting ranges Based on the routes discussed, shooting ranges can influence local access to and the circulation of firearms. First, shooting ranges are needed for practice and technical exams. In both routes, the law requires applicants to demonstrate technical capacity and that this demonstration take place in a range accredited by the Army. This cost is recurring because technical capacity is verified at each registration renewal, which was supposed to occur every 3 years between 2004 and 2018. Second, registration as a sport shooter, who make up the majority of CACs, requires affiliation with a shooting range. After 2015, the Army began requiring documented participation in training or competition as well. Furthermore, lawful practice of sport shooting occurs only at Army-authorized shooting ranges. Third, shooting ranges can reduce the administrative burden on members interested in acquiring their own firearm. Therefore, a shooting range opening in a previously unserved city generally lowers the cost of qualifying under Federal Police rules, makes the sport shooter registry attainable in practice, creates the only local site where a registered firearm could lawfully be fired, and helps interested members navigate the document-heavy process of purchasing a legal firearm.

1.2.2 Diversion from the legal market

A large share of the firearms circulating in Brazil's illegal market originates in the legal market. Langeani and Passos (2022) report that 46% of all recorded diversion incidents between 2011 and 2020 in the state of São Paulo came from private homes. Most are handguns (78%), split between revolvers (49%) and pistols (29%), and most were made by national manufacturers (82%). Private homes lose an average of 1.28 firearms to diversion. However, homes of registered collectors, sport shooters, and hunters (CACs) lose 20 firearms to diversion. The study shows that, in one 2017 case, thieves took 43 firearms from a collector's home, including 12 rifles and 2 machine guns. Diversion of firearms in Brazil, however, is no surprise.

The link between the legal and the illegal firearm market is sustained by institutional failures. The Federal Police system, SINARM, and the Army system, SIGMA, do not communicate, and state police forces cannot access SIGMA even to trace firearms seized from criminals, which limits both prevention and recovery. Background checks fail to screen out ineligible applicants: an audit by the Federal Court of Accounts (TCU) found active CAC registrations held by deceased persons and by individuals with criminal convictions and outstanding arrest warrants, evidence that the Army did not verify the legal requirements at concession or renewal (TCU 2024). Each failure widens the channel through which firearms leave the legal market and enter criminal use.

Diverted firearms enter criminal use quickly and stay in it for years. Firearms traced from the legal market into police seizure records turn up in robberies, homicides, and domestic violence cases, in some instances within days of leaving their registered owner. There is also the important spatial aspect of this phenomenon. In 55% of the matched cases, Langeani and Passos (2022) found that police seized the firearm in a city other than the one where it had been diverted. Therefore, diversion in a given city is likely to harm neighboring municipalities.

1.3 Empirical Strategy

1.3.1 Data

I use municipality-year data from 2004 to 2018 and combine multiple data sources to construct the outcomes, treatment, and control variables. I exclude years after 2018 because legislative changes implemented by former President Jair Bolsonaro during his time in office (2019-2022) significantly altered the process for purchasing legal firearms in the country. The period analyzed also excludes the COVID-19 pandemic, which disrupted citizens' and shooting ranges' routines. I exclude years before 2004 because the Disarmament Statute, enacted in 2003, also significantly altered the process for purchasing legal firearms in the country.

I gathered homicide data from the Mortality Information System (*Sistema de Informação Sobre Mortalidade* (SIM)), a database made available by the Unified Health System (*Sistema Único de Saúde* (SUS)) that records all deaths in the country.[1] SIM is a detailed database that characterizes each death across various dimensions, making it possible to know not only its cause but also the individual's characteristics, such as gender, age, educational level, and the municipality where the death occurred, among other information.

Information on the shooting ranges comes from the Brazilian Army. The data reports the shooting range name, the municipality where it is located, and the year its license was issued. I construct the treatment variable from the license year. I also use firearms registration data from the Federal Police. It reports the number of firearms registered in each municipality by year. As explained previously, the firearms registered by the Federal Police do not account for the total number of legal firearms registered in the country. The Army also registers legal firearms, but it does not provide municipality-level data.

I also use data from the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística* (IBGE)). It reports demographic and socioeconomic characteristics of municipalities. I use these variables from the 2000 Census to assess balance on baseline covariates and as controls in the regressions. I also use IBGE's shapefiles to calculate

[1] Homicides are constructed by filtering deaths by CID-10 codes from X85 to Y09. Firearm-related homicides are filtered by CID-10 codes X93, X94, X95. I also use the third digit of CID-10 codes to separate deaths by the type of place of the event (e.g., residence, public spaces).

the Euclidean distance between municipalities' seats. During the period analyzed, a few new municipalities split off from existing ones. Following IBGE's records, I recode these municipalities to their municipality of origin.

I model the effect of shooting range openings on treated municipalities and the spillover effect on nearby municipalities. For this, I use a benchmark distance of 70 kilometers to define a municipality as being in spillover range, which I later vary for robustness checks (see Section 1.3.2). The 70 km threshold corresponds to the mean distance between municipalities within the same IBGE Microregion – legally defined groupings of municipalities sharing socioeconomic, productive, and environmental characteristics. By 2004, the first year in the sample, 29% of municipalities already had a shooting range or were located within 70 km of a municipality with at least one. I exclude these from the analysis because they fall in the always-treated category or were already within the defined spillover distance.

My estimation sample has 3,739 municipalities, which can be in one of three groups: (1) treated – the ones that experienced the opening of its first shooting range during the period of analysis; (2) spillover – the ones within 70 km from a treated municipality; (3) never treated – the ones further than 70 km from treated municipalities. Table 1.1 reports means and standard deviations (in parentheses) for baseline socioeconomic and violence outcomes across the full sample and by group: treated, spillover, and never-treated municipalities.

On average, treated municipalities exhibit higher baseline levels of violence than never-treated municipalities. Spillover municipalities, in turn, lie between these two groups but remain closer to never-treated municipalities in most violence measures. Treated municipalities are also more populous, more educated, and have lower poverty rates. In contrast, the share of males, share of individuals aged 15 to 24, and Gini Index are very similar across groups. These differences reinforce the importance of estimating specifications with and without baseline controls, as they speak to the plausibility of unconditional parallel trends.

Figure 1.1 shows the spatial distribution of the sample groups across Brazil's territory. Shooting range openings reach every region of the country, so the treatment does not identify a single regional trend. Treated municipalities are few, 163 in total, but each one places its neighbors within 70 km of a new range, which expands the exposed set to 2,371 municipalities. A design that assigned neighboring municipalities to the control group would compare treated

municipalities against places that also gained access to firearms. Municipalities in grey drop from the sample because a range had already opened nearby before 2005. The map also shows that never-treated municipalities concentrate in the North, where population density, urbanization, and distance between municipalities differ from conditions in treated areas, and that treated municipalities cluster in space rather than scattering evenly.

Table 1.1: Summary statistics

Variable	All municipalities	Treated	Spillover	Never treated
Homicide rate	15.04 (15.58)	19.9 (14.5)	15.08 (14.99)	14.32 (16.71)
Firearm homicide rate	8.08 (11.46)	12.14 (11.01)	8.27 (11.64)	7.17 (11.01)
Population (in 1000)	17.98 (28.7)	84.03 (81.9)	14.62 (20.25)	15.64 (15.14)
Share of 15-24 years old	0.19 (0.02)	0.19 (0.02)	0.19 (0.02)	0.2 (0.01)
Share of males	0.51 (0.01)	0.5 (0.01)	0.5 (0.01)	0.51 (0.01)
Aging rate	6.41 (1.93)	5.74 (1.93)	6.97 (1.77)	5.39 (1.77)
Years of schooling	7.89 (1.78)	8.94 (1.4)	8.32 (1.69)	6.92 (1.57)
Income pc	287.1 (160.69)	485.7 (183.73)	305.68 (158.7)	223.68 (126.53)
GDP pc	8.96 (11.91)	14.61 (10.98)	9.53 (12.91)	7.05 (9.36)
Gini index	0.56 (0.07)	0.57 (0.05)	0.54 (0.07)	0.58 (0.07)
Health expenditure pc (in Reais)	81.8 (112.36)	80.48 (51.49)	85.47 (51.92)	74.31 (186.43)
Municipalities	3739	163	2371	1205

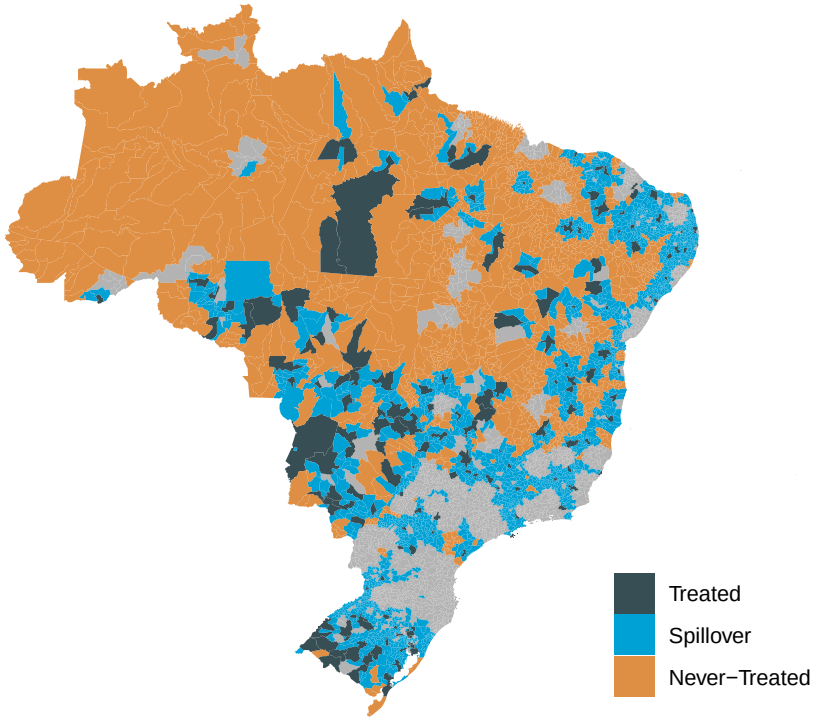
Note: The table reports the mean and standard deviations (in parentheses) for the sample of municipalities. Homicide rates and health expenditure are the mean values in the 2000-2003 period. All other variables are from the 2000 Brazilian Demographic Census. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analyis.

Table 1.2: Mean of dependent variables before treatment

Dependent variable	Treated	Spillover	Spillover1	Spillover2
Homicide rate	25.05	13.83	14.40	13.34
Firearm homicide rate	15.08	8.06	8.60	7.61
Sharp homicide rate	6.60	3.63	3.63	3.64
Force homicide rate	0.60	0.37	0.36	0.38
Blunt homicide rate	1.56	1.00	1.07	0.94
Other homicide rate	1.22	0.77	0.75	0.78
Male homicide rate	22.63	12.44	13.00	11.97
Female homicide rate	2.39	1.37	1.39	1.36
Home homicide rate	3.79	2.40	2.38	2.42
Street homicide rate	12.75	6.64	6.92	6.40
Age 0-14 homicide rate	0.34	0.19	0.22	0.17
Age 15-24 homicide rate	7.23	3.32	3.49	3.17
Age 25-39 homicide rate	9.87	5.49	5.71	5.30
Age 40-59 homicide rate	5.33	3.43	3.52	3.35
Age 60-above homicide rate	1.31	1.06	1.11	1.02

Note: The table reports the mean of all variables used as dependent variables in regressions along this study, considering periods before treatment. Treatment is the opening of a shooting range. Municipalities in the spillover group are within 70 km from at least one municipality that becomes treated in the period of analyis.

Figure 1.1: Geographic distribution of treated, spillover, and never-treated municipalities



Notes: Treated municipalities are those where a shooting range opened during the study period. Spillover municipalities are those within 70 km of a treated municipality. Never-treated municipalities had no shooting range opening throughout the period. I exclude grey municipalities because they were already treated or within spillover range before the analysis period.

1.3.2 Difference-in-Differences model

I use a Difference-in-Differences (DiD) model to assess the causal effect of shooting range openings on violence outcomes. Consider the model in Equation 1.1:

$$Y_{it} = \lambda_i + \alpha_t + \beta D_{it} + X'_{it}\,\gamma + \varepsilon_{it} \tag{1.1}$$

where Y_{it} is the violence outcome for municipality i in year t. λ_i represents municipality fixed effects, which control for time-invariant differences between municipalities. α_t represents year fixed effects, which control for factors that affect municipalities equally over time. D_{it} equals 1 if municipality i has a shooting range in year t and zero otherwise. X_{ist} contains municipality-level control variables. The error term ε_{ist} completes the model.

Equation 1.1 is a Two-Way Fixed Effects model. Recent DiD literature has raised concerns about its potential limitations in identifying the average treatment effect on the treated in staggered adoption when the average treatment effect varies across groups and over time (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021). I then follow the Two-Stage Difference-in-Differences (2SDD) approach proposed by Gardner et al. (2024), which is well suited to staggered adoption.

In the 2SDD, the first stage regresses outcomes on group and period fixed effects using only the subsample of untreated observations, that is, all observations from never-treated municipalities and the pre-treatment observations of treated municipalities. In the second stage, the estimated group and period effects are subtracted from the observed outcomes, and the adjusted outcomes are regressed on treatment status. Assuming that the parallel trends condition holds, this method identifies the overall average treatment effect on the treated, even when treatment effects vary across groups and periods.

I begin with the standard 2SDD specification that relates a municipality's outcome to its own treatment status. This specification is a useful benchmark, but it is restrictive because it assumes that opening a shooting range does not affect nearby municipalities, thus imposing the no-interference component of SUTVA. In this case, the first stage of the 2SDD runs the

following regression on the sample of observations for which $D_{it} = 0$:

$$Y_{it} = \lambda_i + \alpha_t + X'_{it}\,\gamma + \varepsilon_{it} \tag{1.2}$$

The second stage regresses the residualized outcomes, $\tilde{Y}_{it}$, on D_{it} to estimate the treatment effect on the treated, ϕ, which is unbiased only in the absence of spatial spillovers.

$$\tilde{Y}_{it} = \phi\, D_{it} + \nu_{it} \tag{1.3}$$

In the context analyzed, however, the no-interference assumption is unlikely to hold. Citizens can commute to access services provided by shooting ranges and ultimately acquire a legal firearm if a shooting range is located within a reasonable distance. Firearms can also flow to nearby municipalities through the illegal market. If such spatial spillovers exist, the control group is contaminated, and the estimated treatment effect may be biased. In particular, if spillovers increase violence in nearby municipalities, the standard specification will tend to underestimate the direct treatment effect.

To address this issue, I extend the baseline specification to explicitly model the potential spatial spillover effects of shooting ranges following Butts (2023). Define variable S_{it} as:

$$S_{it} = \mathbf{1}\left[D_{it} = 0 \text{ and } \exists\, j \neq i : D_{jt} = 1 \text{ and } d_{ij} \leq \delta\right] \tag{1.4}$$

where d_{ij} is the distance in kilometers between the municipal seats of municipality i and municipality j. Intuitively, S_{it} identifies untreated municipalities that may be exposed to spillover effects from nearby treated municipalities within a radius of δ km. Here, the first stage equation is estimated on the sample of observations for which $D_{it} = 0$ and $S_{it} = 0$ – that is, untreated municipalities located more than δ km from the nearest treated municipality.

The second stage regresses the residualized outcomes, $\tilde{Y}_{it}$ on D_{it} and S_{it}:

$$\tilde{Y}_{it} = \rho\, D_{it} + \theta\, S_{it} + \upsilon_{it} \tag{1.5}$$

where ρ is the treatment effect on the treated group and θ captures the spillover effect on

nearby municipalities. I also estimate a specification in which S_{it} is decomposed into two indicator variables, S_{it}^1 and S_{it}^2 defined in equations 1.6 and 1.7, allowing spillover effects to vary with distance. The first stage is again estimated on observations for which $D_{it} = 0$ and $S_{it} = 0$. The second stage regresses the residualized outcomes, $\tilde{Y}_{it}$ on D_{it} and both S_{it}^1 and S_{it}^2:

$$S_{it}^1 = \mathbf{1}\left[D_{it} = 0 \text{ and } \exists\, j \neq i : D_{jt} = 1 \text{ and } d_{ij} \leq \delta/2\right] \tag{1.6}$$

$$S_{it}^2 = \mathbf{1}\left[D_{it} = 0 \text{ and } \exists\, j \neq i : D_{jt} = 1 \text{ and } \delta/2 < d_{ij} \leq \delta\right] \tag{1.7}$$

In Section 1.4, I show estimates for the three 2SDD specifications explained here. The first specification does not consider spillover effects. The second and third specifications model spillover explicitly, either with a single spillover group or broken down into two groups. Importantly, all specifications also allow me to analyze dynamic treatment effects.

The DiD design identifies the causal effect of treatment under the assumption that, absent the treatment, treated municipalities would have experienced the same evolution in violence as untreated municipalities. Table 1.1 shows that treated and exposed municipalities differ from the never-treated group at baseline: they are more violent, populous, and have higher income levels. The baseline difference in violence does not by itself invalidate the design, since the identifying assumption concerns trends rather than levels – a point I assess through pre-treatment coefficients in event-study analysis. In addition, although DiD differences out time-invariant factors, this does not eliminate bias if baseline levels predict trend differences in the untreated potential outcome (Baker et al. 2026). The concern is whether municipalities with different income or population levels would have followed different violence trajectories even without any shooting range opening. I assess this by comparing estimates with and without time-interacted covariates, which allow municipalities to evolve differently over time.

The design also assumes no anticipation, meaning the expected opening of a shooting range does not affect violence before the opening actually occurs. This is unlikely to be a concern here. The proposed channel through which shooting ranges affect violence is access: a shooting range must be physically open for residents to use its facilities as part of the legal firearm acquisition process. An announced but not yet operating range offers no such access, so there is little reason to expect violence to respond before the opening date.

In addition, I express violence outcomes as rates per 100,000 inhabitants. I do not use log or hyperbolic sine transformations because several concerns arise, particularly when the dependent variable includes zeros – which is the case for homicides. Recent literature shows that log transformation (with adjustments like adding a constant) shifts the entire distribution, which can lead to misleading interpretations, especially for observations near zero. The inverse hyperbolic sine transformation accommodates zeros and negative values but introduces an arbitrary scaling factor that can substantially affect the results. Therefore, I use rates and run robustness checks using Poisson regressions, since they often yield more robust and interpretable results (Mullahy and Norton 2024; Chen and Roth 2024; De Brauw and Herskowitz 2021; Aihounton and Henningsen 2021).

1.4 Results

1.4.1 Main results

I find no robust effect of shooting range opening on firearm homicide rates in treated municipalities. However, neighboring municipalities experience an increase in firearm-related homicide. I report these results in Table 1.3. Columns (1) to (3) report the estimated effect of shooting range opening on firearm-related homicide rate, comparing treated municipalities with all other municipalities (see Equation 1.2). This specification assumes no spillover between units. Column (2) includes baseline controls interacted with year dummies, and Column (3) includes region-year fixed effects. Columns (4) and (5) report the estimated effect of shooting range opening on firearm-related homicide rate, comparing treated municipalities and municipalities within 70 km with municipalities located further away (see Equation 1.5).

For treated municipalities, point estimates change direction across specifications and are imprecise. However, municipalities within 70 km of a newly opened shooting range experience a statistically significant increase of 1.9 firearm-related homicides per 100,000 inhabitants, which represents approximately 24% of the group's mean. Column (5) splits the spillover band into two rings: municipalities within 35 km of a range, and those between 35 and 70 km. Both spillover rings show positive effects on firearm homicides. The point estimate is

Table 1.3: Treatment effect on firearm related homicide rate

	Firearm homicides rate				
	(1)	(2)	(3)	(4)	(5)
Shooting range opening	0.904	−0.332	−0.515	0.047	−0.243
	(1.165)	(1.057)	(1.012)	(1.268)	(1.160)
Spillover (0, 70 km]				1.898***	
				(0.545)	
Spillover (0, 35 km]					2.082***
					(0.688)
Spillover (35, 70 km]					1.692***
					(0.456)
Controls	No	Yes	Yes	Yes	Yes
Region x Year FE	No	No	Yes	Yes	Yes
Spatial Spillover	No	No	No	Yes	Yes
Num. obs.	56085	56085	56085	56085	60075

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km from at least one municipality that becomes treated in the period of analyis. All regressions include municipality fixed effects and year fixed effects. Controls include log of the population and log of the mean income per capita, both measured in 2000 and interacted with year dummies. Standard erros are clustered at the microregion level. Level of significance $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

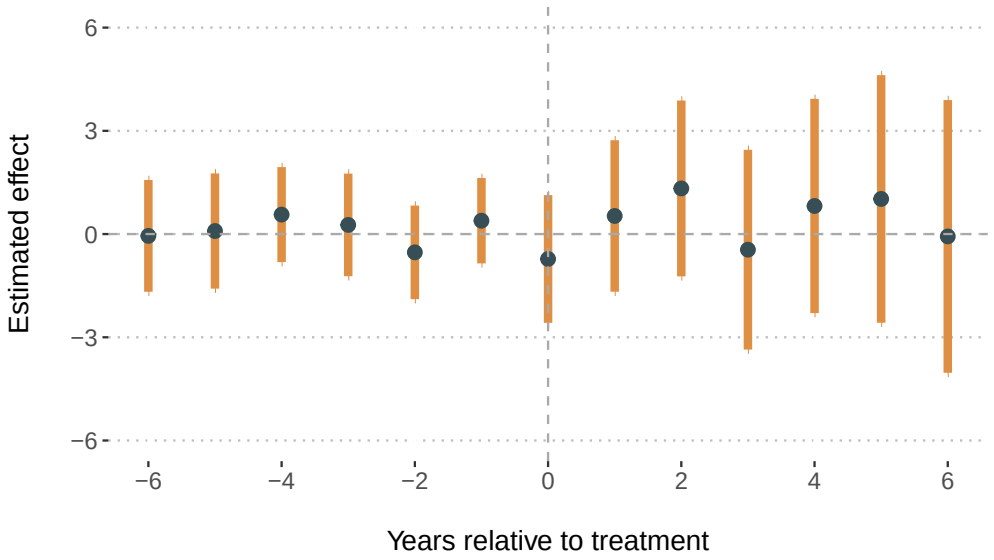
larger in the inner ring, but the two are not statistically different.

Figure 1.2 shows the dynamic estimates for the specification in Column (4), the main specification for the remainder of the paper. Pre-treatment coefficients are close to zero for both treated municipalities (Figure 1.2a) and neighboring municipalities (Figure 1.2b), which supports the parallel-trends assumption and suggests that shooting ranges are not opened in response to a recent rise in violence. For neighboring municipalities, estimates show a positive and significant effect from the treatment period onward, which persists throughout the six-year post-treatment window analyzed.

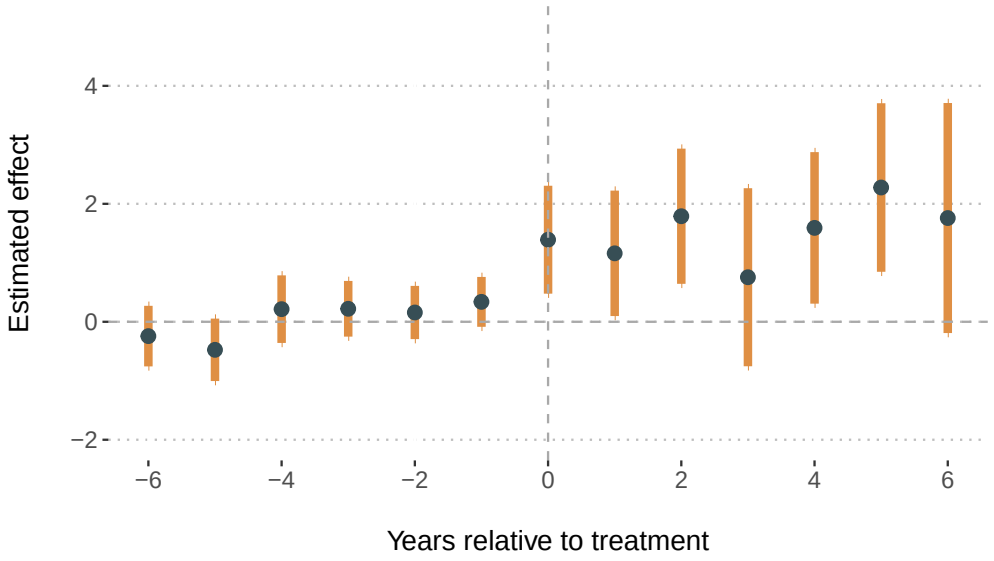
1.4.2 Heterogeneity analysis

The absence of a robust direct effect on treated municipalities may reflect specific characteristics of these areas. As shown in Table 1.1, treated municipalities are more populous, more educated, and have lower poverty rates than spillover municipalities. These differences plausibly translate into greater institutional capacity – better policing, stronger oversight, and

Figure 1.2: Dynamic effect of shooting range opening on firearm homicide rate



(a) Treated municipalities



(b) Spillover municipalities

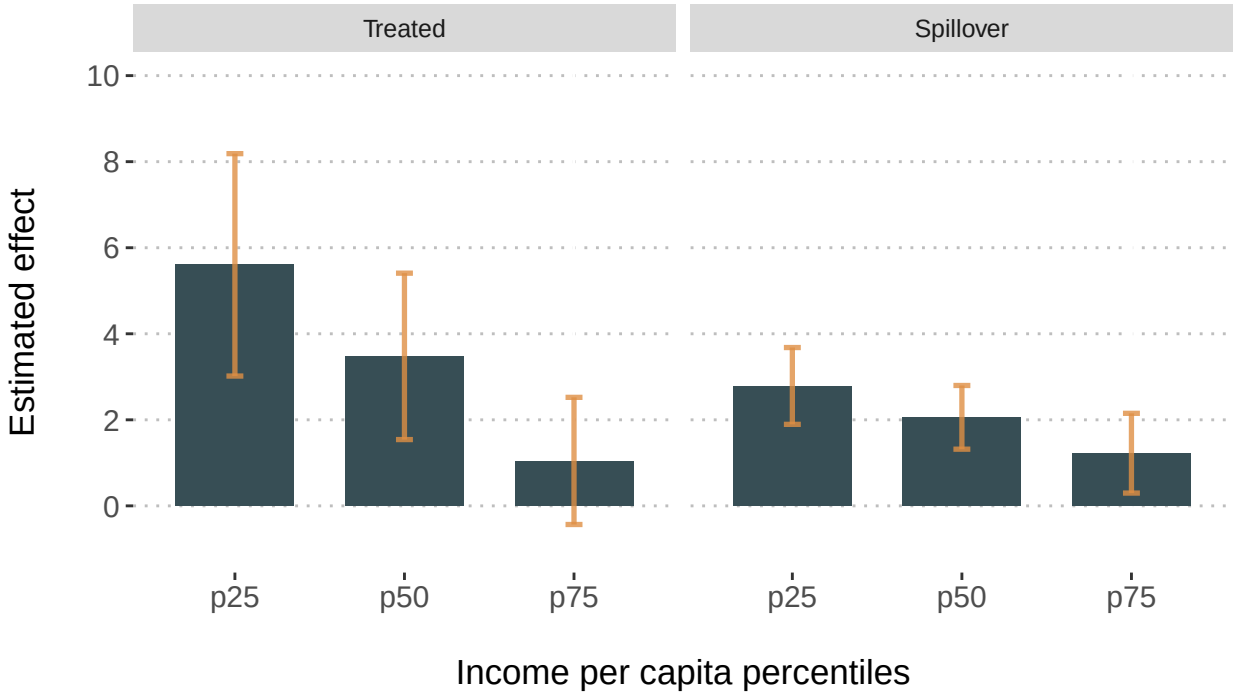
Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

higher compliance with legal requirements – which may limit the extent to which expanded firearm access translates into additional violence locally. Spillover municipalities, by contrast, share the socioeconomic profile of never-treated areas: higher poverty and lower schooling

levels. In these settings, the same increase in firearm availability may find weaker institutional barriers, making violence a more likely outcome.

Figure 1.3 provides suggestive evidence for this interpretation. The figure reports the estimated treatment effect on firearm homicide rates at different baseline income-per-capita percentiles: the 25th (R$ 173), 50th (R$ 308), and 75th (R$ 463). For both treated and spillover municipalities, the estimated effect declines as baseline income rises. Among treated municipalities, the effect is positive and significant at the 25th and 50th percentiles, but not distinguishable from zero at the 75th percentile. Among spillover municipalities, the effect remains positive and significant across all three percentiles, though smaller in magnitude at higher income levels. This pattern indicates that the effect of expanded firearm access on violence is concentrated in lower-income municipalities in both groups, and that it may not persist among treated municipalities with higher income.

Figure 1.3: Treatment effect on firearm-related homicide rate by income percentile



Notes: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level.

Table 1.4: Robustness checks for the treatment effect on firearm related homicide rate

	Firearm homicide rate				
	(1)	(2)	(3)	(4)	(5)
Shooting range opening	−0.475	−0.191	0.540	−0.243	−0.295
	(1.361)	(1.349)	(1.153)	(1.160)	(1.420)
Spillover (0, 70 km]	1.930***	1.833***	1.538***	1.838***	1.741***
	(0.543)	(0.561)	(0.589)	(0.475)	(0.651)
Drop pop. outliers	Yes	No	No	No	No
Drop North Region	No	Yes	No	No	No
Restricted window	No	No	Yes	No	No
Exposure distance	70	70	70	60	80
Num. obs.	55530	50490	42325	60075	51930

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km from at least one municipality that becomes treated in the period of analyis. All regressions include municipality fixed effects and year fixed effects. Controls include log of the population and log of the mean income per capita, both measured in 2000 and interacted with year dummies. Standard erros are clustered at the microregion level. Level of significance $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

1.4.3 Robustness

Table 1.4 shows robustness checks for the main specification. The effect of shooting range opening on neighboring municipalities is stable across all columns, ranging from 1.5 to 1.9 additional firearm-related homicides per 100,000 inhabitants and remaining statistically significant throughout. Column (1) drops municipalities outside the 99th percentile of the population distribution, addressing the concern that a small number of large municipalities may be driving the result. Column (2) excludes the North region, where municipalities cover vast areas and internal mobility is more limited. Column (3) restricts the sample to relative periods within a six-year window around treatment, ensuring the estimate is not influenced by distant relative periods. Columns (5) and (6) vary the distance that defines neighboring municipalities to 60 and 80 km, respectively, showing that the results are not specific to the 70 km benchmark. The local effect on treated municipalities remains imprecise in all specifications, consistent with the main results in Table 1.3.

Table 1.5: Treatment effect on firearm registration by type of firearm

	Firearm registration rate by type...				
	Overall	Pistol	Revolver	Long	Other
Shooting range opening	5.099**	3.588**	1.442**	0.170	0.017
	(2.588)	(1.458)	(0.622)	(1.243)	(0.017)
Spillover (0, 70 km]	5.501**	1.124	1.097**	3.217**	0.003
	(2.288)	(0.721)	(0.500)	(1.414)	(0.014)
Num. obs.	19336	19336	19336	19336	19336

Note: The outcome is the registration of firearms per 100,000 inhabitants. Treat indicates if a municipality has at least one shooting range. All regressions include municipality fixed effects and year fixed effects. Standard erros are clustered at the microregion level. Level of significance $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

1.4.4 Mechanisms

I argue that shooting range openings increase homicide rates through greater firearm availability, driven in part by citizens becoming more likely to register a legal firearm once a shooting range opens nearby. Consistent with this channel, I find a statistically significant increase in firearm registrations per 100,000 population in both treated and neighboring municipalities, of 5.1 and 5.5, respectively. Table 1.5 shows that this increase is driven primarily by handguns. In treated municipalities, registrations rise significantly for both pistols and revolvers, with no detectable effect on long guns or other firearms. The spillover pattern differs: municipalities within 70 km experience a significant increase in both revolver and long-gun registrations, with the latter accounting for most of the effect. The larger response in long-gun registrations in nearby municipalities may reflect their less urban character, where long guns may be more commonly used or demanded.

Figure 1.4 shows the dynamic effect of shooting range openings on firearm registration for each group. The effect emerges more clearly among neighboring municipalities: registrations rise steadily and significantly starting in the second year after treatment, while the pattern for treated municipalities is noisier and less precisely estimated, despite a similar average magnitude. One possible explanation for this asymmetry lies in differences in baseline access to registration channels between host and neighboring municipalities.

Host municipalities tend to be larger and more developed. Their residents might already

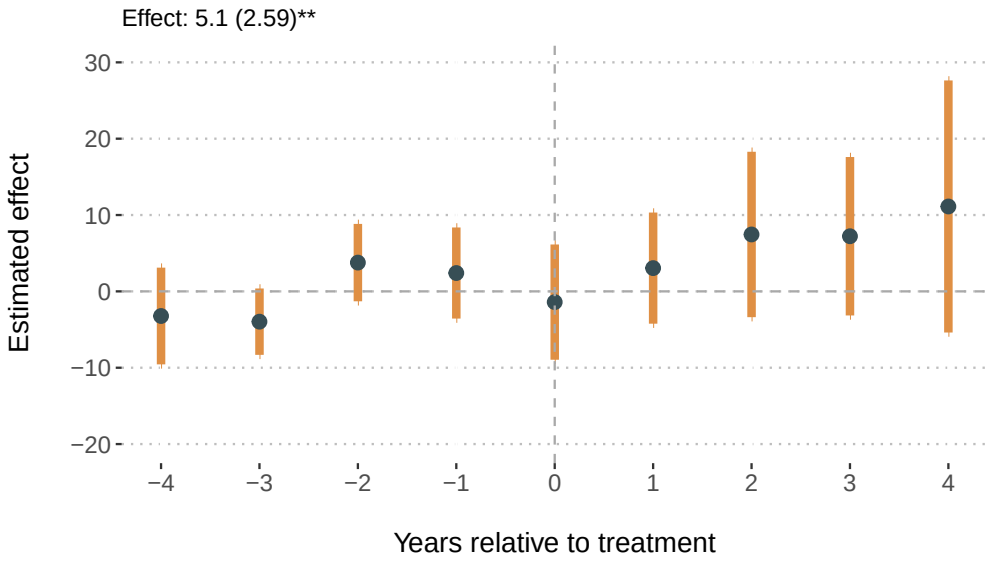
access alternative ways to complete the registration process, such as specialized intermediaries (*despachantes*) or travel to more distant ranges. For these residents, a local shooting range mainly offers a place to train rather than a new opportunity to register a firearm, since they were unlikely to be on the margin of registration to begin with. Residents of neighboring municipalities, by contrast, likely faced higher costs to access these channels before treatment, so opening a nearby shooting range represents a more meaningful reduction in registration costs and shifts a larger share of the population from non-ownership to legal ownership.

It is possible that the increase in firearm homicides does not lead to an increase in the overall homicide rate if shooting ranges only cause a substitution of the weapon used to commit homicides. Table 1.6 breaks down the treatment effect by weapon type to investigate this possibility. I find that the opening of a shooting range does not impact the rate of homicide committed using other weapons and that neighboring municipalities do become more violent after the indirect exposure to the treatment. Column (1) reports the effect of treatment on firearm-related homicide rate; Column (2) reports the effect of treatment on the rate of homicides committed using a sharp object; Column (3) reports the effect of treatment on the rate of homicides committed using bodily force, such as punches; Column (4) reports the effect of treatment on the rate of homicides committed using blunt objects; Column (5) reports the effect of treatment on the rate of homicides committed using other means, such as poisoning; and Column (6) reports the effect of treatment on the overall homicide rate. The 1.9 increase in firearm homicide rate is not offset by reductions in other weapon types. As a result, I find a 1.7 increase in the overall homicide rate in neighboring municipalities.

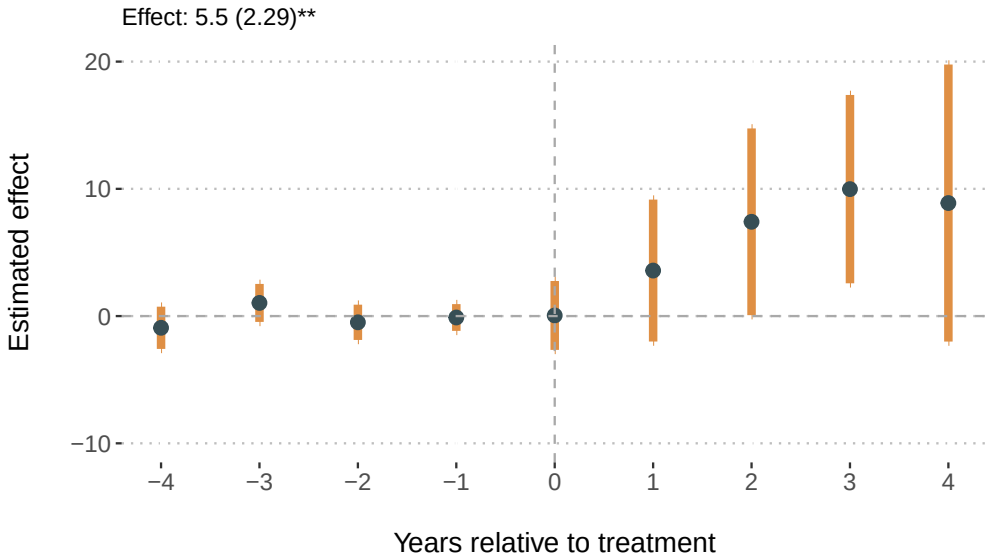
1.4.5 Composition of the effect

Having established the effect of shooting range openings on firearm homicides, I now look into the type of violence underlying this increase and the channels through which it may operate. First, I consider the victims' gender. It is well documented in the literature that femicides are mostly committed by partners and inside the home (UNODC 2025). Given that around 95% of citizens who register firearms in Brazil are men, an increase in firearms within households could plausibly raise gender-related killings. However, I find no evidence supporting this channel. Columns (1) and (2) of Table 1.7 show that the effect of shooting

Figure 1.4: Dynamic effect of shooting range opening on firearm registration



(a) Treated municipalities



(b) Spatial spillover municipalities

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates whether a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

ranges on firearm homicides is driven by male victims. Figure 1.5 shows the dynamic effect of shooting ranges on firearm homicide rate by victims' gender for neighboring municipalities.

Second, I consider homicides' place of occurrence. I examine whether the estimated effect

Table 1.6: Treatment effect on homicide rate by weapon type

	Homicide rate by weapon type...					
	Firearm	Sharp	Force	Blunt	Other	Overall
Shooting range opening	0.047	−0.541	−0.028	0.073	0.005	−0.444
	(1.268)	(0.558)	(0.279)	(0.116)	(0.274)	(1.469)
Spillover (0, 70 km]	1.898***	−0.248	−0.014	−0.012	0.070	1.695**
	(0.545)	(0.361)	(0.174)	(0.062)	(0.184)	(0.731)
Num. obs.	56085	56085	56085	56085	56085	56085

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km from at least one municipality that becomes treated in the period of analyis. All regressions include municipality fixed effects and year fixed effects. Controls include log of the population and log of the mean income per capita, both measured in 2000 and interacted with year dummies. Standard erros are clustered at the microregion level. Level of significance $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

is driven by firearm homicides occurring inside or outside the victim's home. In Columns (3) and (4), I show that the effect of shooting ranges on firearm homicide is driven by homicides outside the home. This further supports the argument that gender-related killings are not a relevant channel. Rather, this result is consistent with illegal carry and firearm diversion. As discussed in Section 1.2, legal ownership does not confer the right to carry firearms in Brazil. Registered owners may keep their weapons only on their property or transport them, unloaded and out of reach, to their affiliated shooting range. Thus, an increase in homicides occurring outside the home may indicate that legal owners are illegally carrying their firearms and using them in situations that escalate to lethal violence. Alternatively, these weapons may be diverted and subsequently used by third parties to commit the homicides. These two channels are not mutually exclusive. My data do not allow me to test them directly. Figure 1.6 shows the dynamic effect of shooting ranges on firearm homicide rate by place of occurrence for neighboring municipalities.

Third, I consider victims' age. In Brazil, citizens must be 25 or older to own a legal firearm. However, the average age of citizens who own a legal firearm is 45. If only owners were the ones being killed, victim age should track this figure or be even higher. Instead, I show in Columns (5) to (9) that the estimated increase in firearm homicides is driven by victimization among individuals aged 15 to 39 years old. This pattern suggests that individuals other than the firearm owners themselves are being victimized. This is also consistent with firearms

being diverted to third parties who use them against a younger population than the average legal buyer. Figure 1.7 and Figure 1.8 show the dynamic effect of shooting ranges on firearm homicide rate by victims' age range for neighboring municipalities.

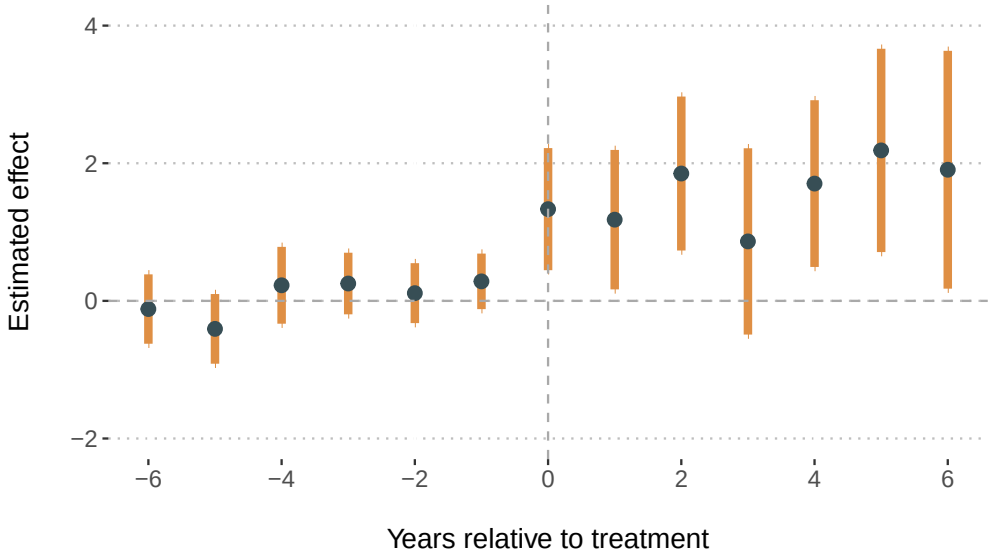
Taken together, these results help characterize the type of violence driving the estimated effect and narrow down its likely channel. The absence of an effect on female victims suggests that the increase in firearm homicides is not driven by femicide, a pattern reinforced by the concentration of the effect on homicides occurring outside the home. The age pattern further indicates that the effect is unlikely to reflect only legal owners being killed. The results are consistent with illegal carrying and the diversion of firearms into the hands of others who subsequently use them in homicides. Both are consistent with an expansion in the availability of firearms outside the licensed and regulated environment of the shooting range.

Table 1.7: Treatment effect on firearm homicides rate by gender, location, and age

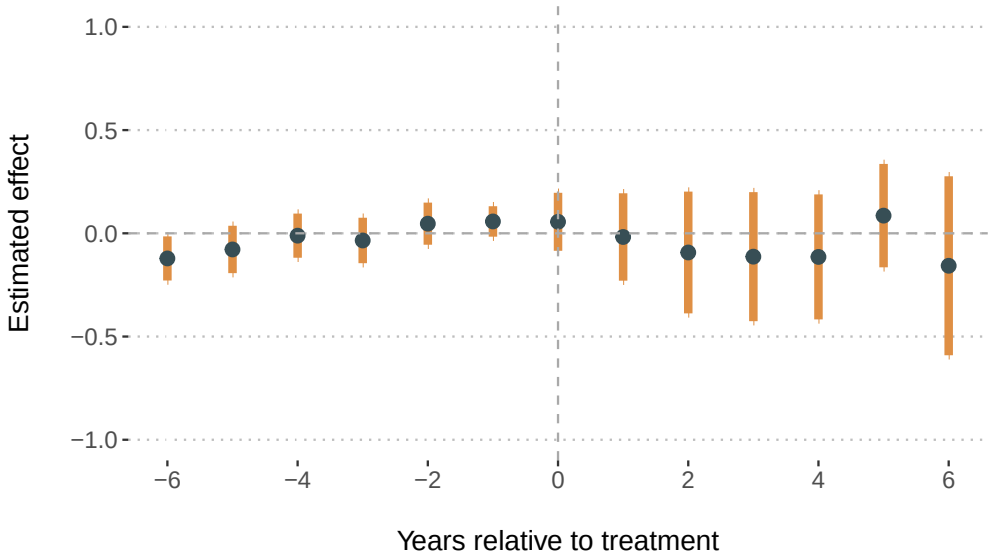
	Firearm homicides rate by gender, location, and age...								
	Males	Females	Residence	Outside Residence	Age 0-14	Age 15-24	Age 25-39	Age 40-59	Age $\geq$ 60
Shooting range opening	0.390	−0.347	−0.070	−0.173	−0.069	−0.111	0.753	−0.016	−0.380**
	(1.181)	(0.282)	(0.347)	(1.096)	(0.061)	(0.616)	(0.554)	(0.691)	(0.163)
Spillover (0, 70 km]	1.943***	−0.051	0.056	1.567***	−0.049	0.501*	1.052***	0.419	−0.052
	(0.521)	(0.089)	(0.225)	(0.459)	(0.039)	(0.258)	(0.291)	(0.278)	(0.099)
Num. obs.	56085	56085	56085	56085	56085	56085	56085	56085	56085

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km from at least one municipality that becomes treated in the period of analyis. All regressions include municipality fixed effects and year fixed effects. Controls include log of the population and log of the mean income per capita, both measured in 2000 and interacted with year dummies. Standard erros are clustered at the microregion level. Level of significance $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Figure 1.5: Dynamic effect of shooting range opening on firearm homicide rate by victim's gender in spillover municipalities



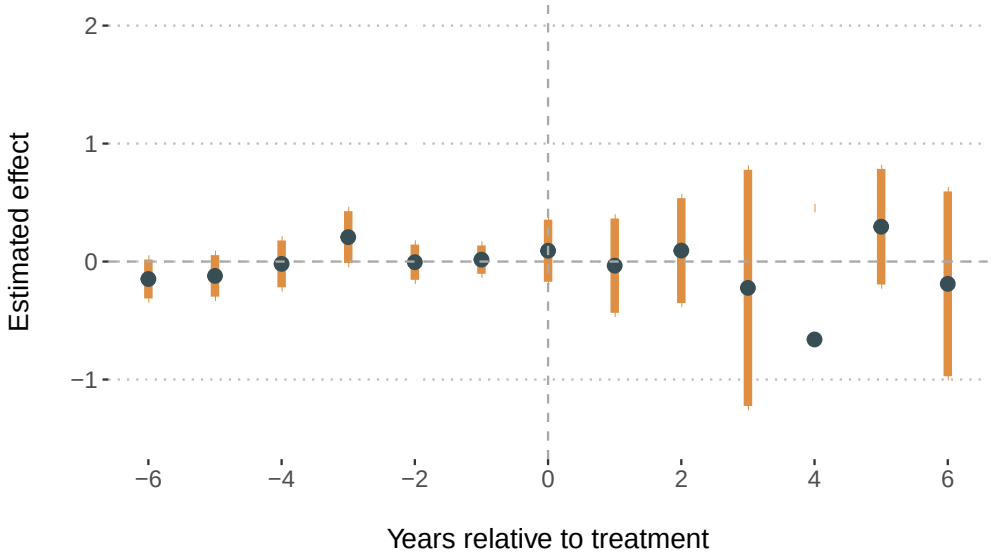
(a) Male victims



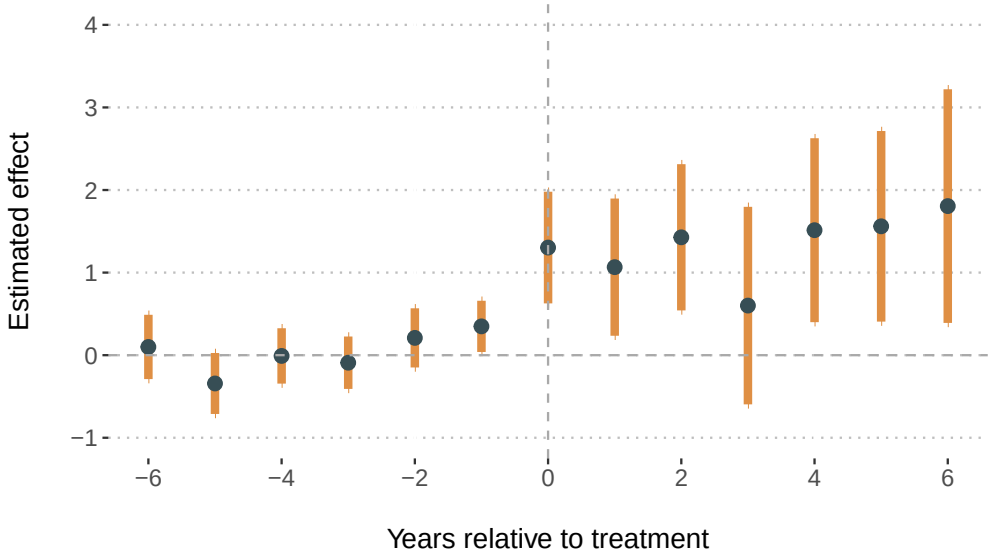
(b) Female victims

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

Figure 1.6: Dynamic effect of shooting range opening on firearm homicide rate by place of occurrence in spillover municipalities



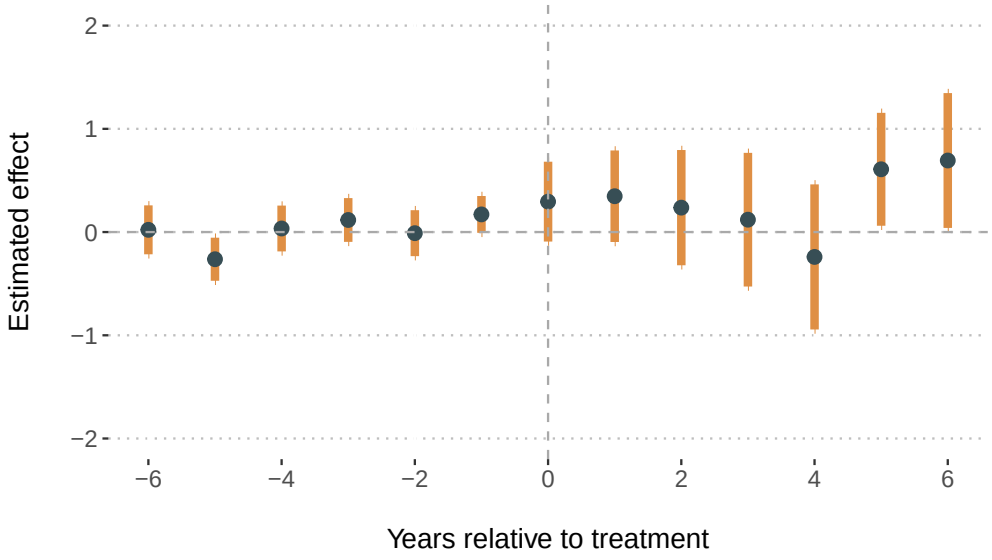
(a) Inside a residency



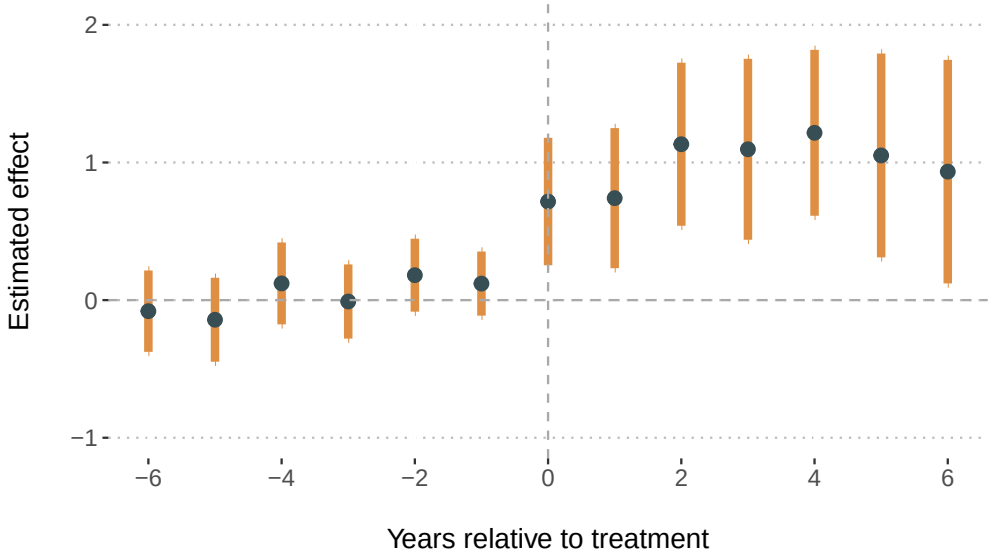
(b) On the streets

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

Figure 1.7: Dynamic effect of shooting range opening on firearm homicide rate by victim's age in spillover municipalities



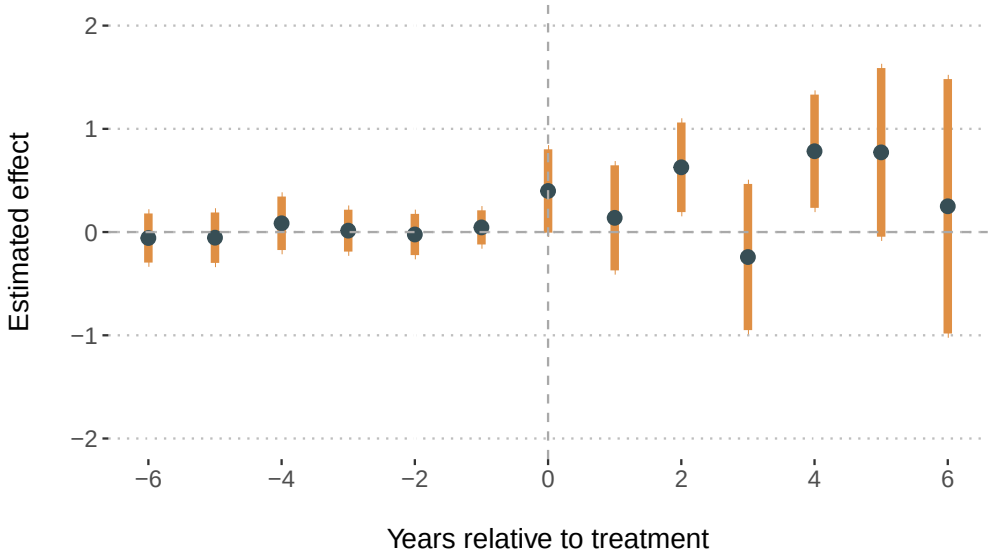
(a) Age 15-24 years old



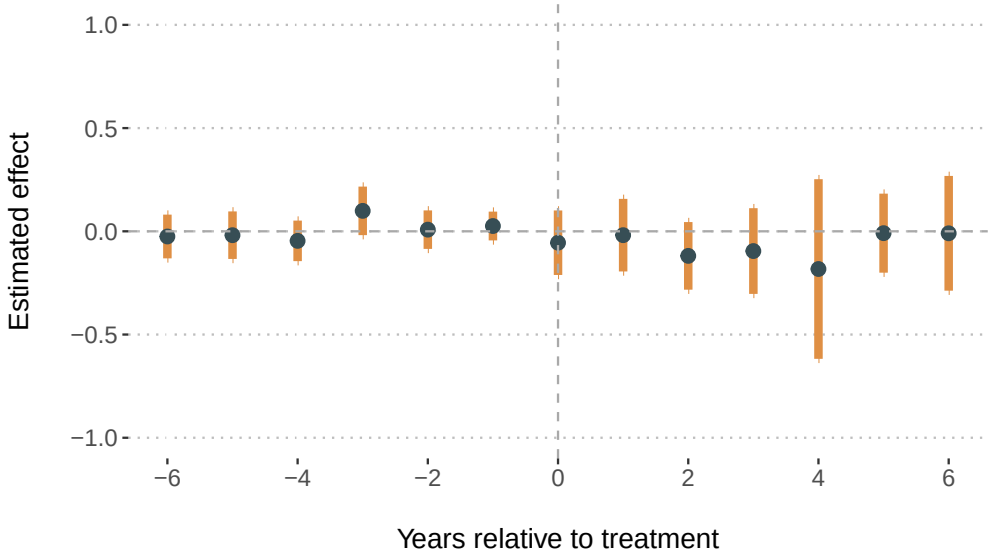
(b) Age 25-39 years old

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

Figure 1.8: Dynamic effect of shooting range opening on firearm homicide rate by victim's age in spillover municipalities



(a) Age 40-59 years old



(b) Age 60 years or older

Note: The dependent variable is firearm homicides per 100,000 inhabitants. Shooting range opening indicates if a municipality has at least one shooting range. Municipalities in the spillover group are within 70 km of at least one municipality that becomes treated in the period of analysis. All regressions include municipality fixed effects, year fixed effects, and controls for the log of the population and the log of mean income per capita, both measured in 2000 and interacted with year dummies. Standard errors are clustered at the microregion level. The level of significance is $p < 0.05$.

1.4.6 Quantitative implications

To gauge the magnitude of the spillover effect, I compute the number of additional firearm homicides it implies over the study period. For each municipality within 70 km of a host municipality, I multiply the estimated effect of 1.9 per 100,000 inhabitants by the municipality's population in each year after its first exposure, and sum across municipalities and years. Exposed municipalities accumulate about 322 million person-years of exposure between 2004 and 2018. The calculation implies about 6,100 additional firearm homicides over the period (95% CI: 2,700 to 9,600). These deaths represent 10% of the firearm homicides recorded in exposed municipalities after exposure. The calculation rests on three simplifications. Because average exposure lasts about nine years, the calculation extends the estimated effect beyond the six-year event-study window. This share is smaller than the 23% effect reported above because that effect is measured against the pre-treatment mean, and firearm homicide rates in these municipalities roughly doubled over the period.

1.5 Conclusion

High rates of lethal violence have fueled debates over civilian access to legal firearms for at least three decades, but the existing evidence is largely concentrated in the United States and faces recurring empirical challenges that limit its external validity and causal credibility. In this paper, I estimate the effect of expanding legal firearm access on violence in Brazil, using the opening of shooting ranges across municipalities in a staggered difference-in-differences framework that accounts for spatial spillovers. Three findings stand out.

Shooting range openings do not raise firearm homicide in the municipality that hosts the range. In neighboring municipalities, they do. The estimated effect is 1.9 firearm homicides per 100,000 inhabitants, or 24% of that group's pre-treatment mean. Homicides committed with other weapons do not fall, so the overall homicide rate rises by 1.7 per 100,000. The effect appears in the year of the opening and persists over the six-year window, and pre-treatment coefficients sit close to zero. Varying the spillover radius, dropping the North region, dropping the largest municipalities, and restricting the event window leave the spillover estimate

between 1.5 and 1.9.

Firearm registration accounts for part of this pattern. Self-defense registrations rise by 5.1 per 100,000 in treated municipalities and by 5.5 in neighboring ones, so a new range expands legal ownership well beyond the hosting municipality. Where those firearms end up is what the composition results speak to. The increase in homicides concentrates among male victims, among victims aged 15 to 39, and outside the home. Registered owners in Brazil average 45 years of age, may not carry in public, and must keep the firearm at a registered address. The victims of the additional homicides therefore differ from the people who register the firearms, and the deaths occur where the law forbids the firearm to be. Two channels fit this evidence: legal owners carry their firearms illegally, or those firearms reach third parties through diversion. My data do not separate them, and the two are not mutually exclusive.

Baseline income shapes the effect size in both groups. Estimates fall as income per capita rises and lose significance at the 75th percentile among treated municipalities. Treated municipalities are also larger, richer, and more educated than the places that surround them. One reading is that access expands where oversight is strongest and violence follows where it is weakest, which would explain why the host municipality absorbs the firearms, and the neighbors absorb the deaths. This reading remains suggestive: the design identifies where the effect concentrates, not the institutional feature that produces it.

These results carry a specific policy implication. The effect of a shooting range opening is not contained in the municipality that hosts it: firearms registered through a new range circulate across municipal borders, and the additional homicides appear among the neighbors. The cost falls unevenly. Spillover municipalities are poorer and less educated than the hosts, and the effect is largest where baseline income is lowest, which suggests that places with less state capacity bear the burden of firearm availability generated elsewhere. Any assessment of expanded legal access that looks only at outcomes where access points operate will therefore miss most of the harm. More broadly, Brazil shows that a legal market can move violence even where legal purchase is expensive and slow. Where police capacity is low and organized crime absorbs diverted weapons, conditions common across Latin America, the harm from expanded legal access is likely to surface away from the point of access and among the populations least equipped to contain it.

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