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# Evaluating a National Anti-Firearm Law and Estimating the Causal Effect of Guns on Crime

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**ABSTRACT**

We report two results. First, we evaluate the impact of a nationwide anti-firearm legislation enacted in December 2003 (*Estatuto do Desarmamento*, henceforth *ED*). Our identification strategy hinges on the hypothesis that the law had a stronger impact in places where gun prevalence was higher in the baseline. We find evidence that homicides (reduced form) and firearms prevalence (mechanism or first-stage) dropped faster in places with higher gun prevalence after the 2003. Using our preferred estimates, the *ED* saved between 2,000 and 2,750 lives from 2004 through 2007 in cities with more than 50,000 inhabitants in the state of São Paulo. Second, assuming the *ED* causes homicide only through its impact on firearms prevalence, we recover a causal estimate of the impact of firearms on homicides. One standard deviation in the prevalence of firearms reduces homicides by quarter of a standard deviation. We find no impact of both ED and firearms on property crime in general or on robberies.

Keywords: homicide; Brazil; firearms, gun violence; property crimes

JEL Classification: K42

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## 1. Introduction

Following the surge in violent crime in the second half of the 1980s, the role of guns as a criminogenic factor has been hotly debated in the US. One side argues that firearms cause violence because they increase the lethality of disputes, a statement mainly about violent crime, especially homicides. Several papers have found empirical support for this *lethality hypothesis*. A non-exhaustive list includes Duggan (2001), Sherman et al (1995), Stolzenberg and D´Alessio (2000), McDowall (1991), McDowall et al. (1995), Cook and Ludwig (1998, 2002), Sloan et al. (1990), Ludwig (1998), and Newton and Zimring (1969).

Other scholars argue the opposite. The *defensive gun use hypothesis* states that the ability of law-abiding citizens to carry firearms deters criminals. Thus, it is a statement mainly about property crime, with only second-order implications for violent crime. Evidence on the deterrence role of firearms is provided by Lott (1998), Lott and Mustard (1997), Kleck (1997), and Bartley and Cohen (1998).

The dispute over the causal impact of fire weapons availability on crime is still unsettled. In this context, we make two contributions to the literature. First, we evaluate the impact of the *Estatuto do Desarmamento* (henceforth ED).[2] ED is a nationwide firearms legislation enacted in December 2003 by the Brazilian Congress. It severely restricted the possibility of legal firearms possession, and increased the penalties for the illegal possession. Our reduced-form estimates show that the ED caused a reduction on homicides. According to our preferred estimate, the ED saved between 2,000 and 2,750 lives from 2004 through 2007. We find no systematic impact on property crime. We also document the mechanism. Using suicides by firearms as a measure of prevalence of guns – Cook and Ludwig´s (2002) well validated measure – we find that the ED reduced the availability of firearms.

These results are important per se. First, the literature on firearms is narrowly focused on the US, while crime is arguably a more important in other countries such as Brazil. Most US inspired, the literature has focused on local or state-level interventions. In contrast, we evaluate large scale intervention to control fire weapons' availability, which may inspire other similar policies. Nationwide interventions do pose identification challenges for the lack of cross-section variation. On the other hand, nationwide interventions are exogenous to states or municipalities, and differences in baseline variables may be used to identify the causal impact of the intervention, under some assumptions.

The second contribution is recovering a causal estimate of firearms on crime, which has been an ongoing challenge in the literature for two reasons. First, violence begets arms. Second, measurement is hard because the prevalence of firearms is non-observable. Measures based on police reports, i.e. illegal possession, capture not only prevalence but also enforcement, which, in turn, is typically difficult to control for. Furthermore, one cannot sign the bias caused by police measures of fire guns prevalence.

We address the problem of reverse causality by assuming that the impact of the law is stronger where firearms were more prevalent. Under this assumption, we can use cross-city differences in the

[2] Law 10.826, from 12/22/2003.

baseline prevalence of firearms as an instrument for within-city changes in prevalence of firearms. We address the measurement problem using Cook and Ludwig's (2002) well validated suicide by firearms proxy. We find that firearms' prevalence have a small but non-negligible impact on violence. Using our most conservative estimate, a reduction in one standard deviation in firearms causes homicides to drop by ¼ of a standard deviation. We find no impact on property crime.

The paper is organized as follows. Section 2 is a brief review of the large literature on the firearms-crime nexus. We focus on the theoretical mechanisms behind the *nexus* and on the empirical challenges to recover causal parameters and the methods employed to establish causality. Section 3 provides background on crime and enforcement in the state of São Paulo. Section 4 describes the data. Section 5 describes the *Estatuto do Desarmamento*. Section 6 outlines the empirical strategy. Results are in Section 7. Section 8 discusses and concludes.

## 2. Literature Review: The Search for the Causal Impact of Firearms on Crime

The causal effect of the prevalence of firearms on crime has been on the holy grails of social science. The interest in the subject is proportional to the controversy about the results obtained in several studies, which, to some extent, reflect the limited data availability and the complexity of the phenomenon which imposes methodological challenges to researchers.

The results that make clear the positive correlation between firearms, suicides, and homicides at an international level have been less controversial. Based on information from 16 European nations, Lester (1991) found a high correlation between firearm-related homicides and two proxy measures of the spread of firearms in those countries. Using data from 14 countries, Killias (1993) also documented a positive correlation between the availability of firearms and the rates of firearm-related homicides and suicides.

From a theoretical perspective, it is not clear whether firearms are criminogenic (Dezhbakshsh and Rubin 2003, in Moody and Marvell, 2002). Two clear opposing forces are at work. On the one hand, the spread of firearms among the population increases the lethality of the means used by individuals to solve violent conflicts, conditional on the conflict happening; and facilitates the access and reduces the cost of firearms for the criminal, either by increasing the supply in the black market or by increasing the quantity of stolen guns. All these factors suggest that the availability of firearms will increase crime, especially (but not only) violent crime. This is the *lethality hypothesis*. On the other hand, firearms have a deterrence effect. Better armed law-abiding citizens, ceteris paribus, increases the expected cost of the committing a crime. This is the *defensive gun use hypothesis*, mainly a statement about property crimes when the perpetrator has contact with the victim, and only incidentally about violent crime. Finally, the effect of the increased lethality of firearms on the number of conflicts is ambiguous. Increased lethality dissuades violent conflict-resolution because of the increased cost of the violence. However, firearms increase the power to coerce exerted by the bearer of the weapon, encouraging non-peaceful responses to solve conflicts. Consider a situation in which criminals have more weapons but all the sudden their average lethality is lower, i.e., because firearms are successfully banned. In this case, it is conceivable that the ability to coerce is diminished, reducing the payoff of committing crimes. Theoretical ambiguity implies that the causal relationship between firearms and crimes is an empirical question.

Most empirical studies find evidence that firearms are criminogenic, especially for violent crimes such as homicides. Nevertheless, several authors who argue the opposite or even argue that there is no causal relationship between firearms and crimes. Empirical ambiguity is hardly surprising when theory does not resolve the issue.

The empirical literature struggles with on two identification issues: measurement and endogeneity broadly understood confounding factor and reverse causation. Many a paper focus on providing good proxies of firearms and the quantity of guns for defensive use in the hands of American households (defensive gun use). Second, cross-sectional and time-series differences in the prevalence of firearms are not exogenous, even if firearms' prevalence was well measured.

Several strategies have been used to deal with endogeneity.[3] McDowall (1991) – a pioneering attempt to establish causality – uses instrumental variables strategy with aggregate data to compute firearms-elasticities of crime. Duggan (2000) finds that past gun ownership and current homicides are strongly positively related. Directly related to the deterrence hypothesis, Cook and Ludwig (2002) used data from the Uniform Crime Reports (UCR) and the National Crime Victimization Survey (NCVS) and find that the elasticity of the firearms in relation to burglaries vary in the range between 0.3 and 0.7.

The debate of the Shall Issue Concealed Handgun Laws stands on its own in the literature. Following Florida in 1987, 31 states passed laws to allow citizens to obtain licenses to carry firearms. McDowall et al. (1995) evaluate the effects on homicides in large cities from three US states of the amendment to the Concealed Firearms Laws, in which the law changed from "may issue" to "shall issue". They find that "shall issue law" did not led to a reduction in overall homicides, at least in large urban areas; but did increase the number of firearm-related homicides. One of the most discussed and controversial articles in this literature, Lott and Mustard (1997) investigate the effect of the implementation of the "shall issue law" on both violent and property crimes. They find that "shall issue laws" caused violent crimes to drop without an increase in accidental deaths caused by firearms. They also found evidence that criminals substitute from "high contact" property crimes, say armed robbery, "low-contact" offences. See also Lott (2000). Several authors have shown that Lott and Mustard (1997) suffer from methodological deficiencies, and reach a different conclusion using similar data or using other datasets. Ludwig (1998) finds that, if anything, "shall issue laws" cause an increase in adult homicide rate. Using data similar to Lott and Mustard (1997), Dezhbakhsh and Rubin (1998, 2003) find mixed results: a slight drop in homicides, an increase in robberies, but and mostly ambiguous results on other crime categories.[4]

[3] We postpone the discussion on how the literature has tried to solve the measurement problem to Section 6.1.
[4] It is beyond the scope of this paper to review the whole controversy, which runs long. Among supports, see Barteley and Cohen (1998), Plassmann and Tideman (2001), Moody (2001), and Olson and Maltz (2001). Among critics, see Ayres and Donohue (2003),

Another point of contention is the legal and illegal use of firearms by young people to commit lethal and nonlethal crimes. Stolzenberg and D'Alessio (2000) use of an unprecedented database from the National Incident-Based Reporting System (NIBRS), for South Carolina, which identified, from 1991 to 1994, not only the number of violent crimes by county, but also those (even the non-lethal) crimes committed with the use of a firearm and by young people. The y find that violent crimes, crimes committed with firearms and crimes committed by young people were positively affected by the availability of illegal firearms, but not by the availability of legal firearms. Furthermore, the authors found no evidence of substitution of firearms for bladed weapons.

The literature finds mixed results on the firearm- crime *nexus*. While most papers find that guns are criminogenic, especially for violent crime, there is non-negligible body of dissenting articles. Furthermore identification is difficult because of omitted variables, reverse causality and measurement (section 6 below), which adds to the empirical ambiguity. In this context, our paper contributes by proposing a different identification strategy, by using data from a country other than the US, and by focusing on a large-scale restrictive intervention which is quite different from the Right-to-Carry US state-level interventions typically evaluated in the literature.

## 3. The Empirical Setting: Crime and Law Enforcement in São Paulo

The state of São Paulo had 41.3 million inhabitants in 2009. It is the largest and most important economically state in Brazil. It is comparable to a middle income country, with GDP per capita of around U$15,000 in 2009.[5] After the troublesome 1990s, the years 2000s brought better economic performance and improving social indicators. However, social indicators still lag behind countries with comparable income. Income is poorly distributed (Gini coefficient of 0.49 in 2009) and almost 5% of the adult population was illiterate in 2009.

Violence was at moderate level in the end of the decade 2000s, after reaching high levels in the late 1990s. Homicides were a little more than 10 per 100,000 inhabitants in 2010, down from 33 per 100,000 inhabitants in 1999. Murders have dropped steadily for over a decade (see De Mello and Schneider, 2010).

In Brazil, law enforcement is primarily the attribution of state governments. Executive and administrative authority rests with the state-level secretaries of security (the *Secretarias Estaduais de Segurança Pública*), which respond directly to the governor, who allocates the budget to the secretary. The administrative and strategic decisions are made by the state security secretary, who is appointed by the governor. The institutional and administrative structure of law enforcement is determined by constitutional law. Enforcement is shared between two police forces that respond directly to the secretary:

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Webster et al (1997), Zimring and Hawkins (1997), Black and Nagin (1998). For a more complete account of the debate, containing Lott's responses to his critics, we refer to David Friedman's website (http://www.daviddfriedman.com/Lott_v_Teret/Lott_Mustard_Controversy.html).

[5] Aggregate data cited here are from IPEADATA (http://www.ipeadata.gov.br/) and Fundação SEADE (http://www.seade.gov.br/).

the military police, responsible for patrolling and crime prevention, and the civil police, an investigative agency. The commanders of the two police forces are also appointed by the governor. Differently from the US, sheriffs are not elected but appointed after passing competitive public examinations.

The federal and municipal levels have a secondary participation in law enforcement. Repression of drug trafficking is shared between the federal police force – *Polícia Federal* (PF), equivalent to the American FBI – and the state-level *Secretarias*. The *Polícia Federal* is responsible for dealing with cross-state and international traffic. The state-level police forces have jurisdiction within state borders. Unlike the state-level *Secretarias*, municipal police forces (*Guardas Municipais*) are not mandated by the constitution but rather a choice of the municipality. As of 2006, 28% of municipalities in São Paulo state had a municipal police force. Of those police forces, 52% carried firearms and were involved in street-level policing.

## 4. Data

We use information from two data sources. From the *Secretaria de Segurança do Estado de São Paulo* we have annual data at the municipal level on the total number of property crime, car robbery, theft, attempted homicides, apprehension of firearms and assault. The data runs on an annual frequency from 2001 through 2007. The state of São Paulo is one of the few to have high quality police report data. Under-reporting on car robbery is residual because of insurance and legal liability reasons (Biderman et al, 2010). For categories such as assault and attempted murder, victimization survey data suggest that under-reporting – which is high - has been declining over time, which lead us to take results using this data with a grain of salt (De Mello and Schneider, 2010). For minor felonies such as theft under-reporting is still rampant. Data on illegal firearms possession are available but we choose not to use it in the regression analysis because it is contaminated with enforcement (De Mello and Schneider, 2010).

Data on violent deaths are from the DATASUS, the hospital dataset from the Ministry of Health. The data follow the taxonomy from the World Health Organization. From DATSUS we use homicides, homicides by firearms, suicides and suicides by firearms. Brazilian hospital data is generally considered high quality, especially the one from São Paulo (Cerqueira, 2011). Data on population and age distribution are also from DATASUS. Hospital data is available starting in early 1990s. For the regression analysis we use only data running from 2001 because this is the period available for the other types of crimes. For the preliminary graphical analysis of homicides and suicides we go to back to 1998 to have a longer view (see Section 5 below). This is important because homicides and suicides –relatively rare events – are noisy.

We focus on a window of four years after and three years before the establishment of the ED. The choice is partially data driven, and partially conceptual. Municipal-level data police-report data are only available starting in 2001. The new, tougher restriction on firearms registration, possession, and carrying was effective with the sanctioning by the president in December 2003. Four years should be enough time to capture the impact of the law, and we cut the sample in 2007.

Because our main variables of interest are very noisy in smaller cities, we focus on cities with more than 50,000 inhabitants on average in the baseline (2001 through 2003). In particular, the main proxy for the prevalence of firearms – suicides by firearms – is particularly noisy because it is rare event (see Section 7.1 below). [6]

Finally, expenditures and purchases of firearms are from the *Pesquisa de Orçamento Familiar* (POF), the Household Expenditure Survey conducted by the *Instituto Brasileiro de Geografia e Estatística* (IBGE), the Brazilian Bureau of Statistics. The POF was conducted three times: 1995/1996, 2002/2003 and 2008/2009. We use data from the second and third rounds of POF.

## 5. The Intervention and the Theoretical Mechanisms

In Brazil, individual rights are legislated at the federal level. In December 2003, the Brazilian Congress passed Lei Nº 10.826, the *Estatuto do Desarmamento* (ED). The ED legislates on many aspects of firearms possession, such as right-to-carry, procedures to apply for possession, and penalties for violation. Among other things, it creates a National Registry (SINARM) where all fire weapons sold have to be registered, and it establishes that licenses are to be issued by the *Polícia Federal*. It also increases significantly the requirements and the red tape for applying to a fire gun permit. In order to possess a gun, the applicant cannot have a criminal record, must have a formal job, show proof of residency, pass a psychological exam, take a course on handling guns, and pay a fee close to U$1,000.00 (Article 4). Registration (i.e., possession) only allows for possession inside one's residency (or place of business). Article 6, which regulates the right-to-carry, forbids carrying fire arms except in special cases.[7] Before the ED, registration implied the right-to-carry.

The ED changed the penal status of illegal possession and illegal carry of fire arms from misdemeanor to felony. Before ED the penalty for illegal possession and carrying was 1 to 3 months of incarceration *or* a fine; typically, the offender was out on bail. The ED establishes a penalty from 2 to 4 years **and** fine; the offender does not have the right to bail if the gun is not properly registered (Article 14).

We have no direct evidence on enforcement. São Paulo's police force is known as one of the best in Brazil, and often credited with the having played a significant role in the reduction of violence after the late 1990s (Kahn and Zanetic, 2007; De Mello and Schneider, 2010). Kahn and Zanetic (2007) document a major enforcement effort starting in the mid 1990s to crack down on illegal possession and carrying of fire arms.

We expect the ED to impact the prevalence of firearms through several channels. First, a demand channel: people will demand less legal arms because of the increased red tape and requirements in applying, and the reduced benefit from possessing a legal gun (no longer being able to carry outside one's

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[6] Robustness analysis was performed using all cities and is available upon request.

[7] Exceptions: military personnel, state and federal police officers during work hours, municipal police officers in cities with population over 50,0000 inhabitants, private security employees (properly registered with the SINARM), sportive hunters (properly registered with both the official league and the SINARM), rural dwellers whose subsistence depends on hunting.

residence). Second, because once legal firearms are a source for the illegal market, the reduction in the demand for legal firearms will reduce the supply of illegal firearms available for criminals *and* non-criminals alike. Thus, the impact of the ED on prices will depend on the relative sizes of the supply and demand shifts. Third, non-criminal owners of illegal registered and unregistered firearms will carry less because of the increased penalty for illegal possession. Finally, it is not clear theoretically how the number of guns carried by criminals changes in equilibrium. It depends on whether guns carried by criminals and non-criminals are strategic substitutes or complements, and on the effect on prices of illegal firearms.

Two pieces of indirect evidence are available. First, anecdotal information on prices is available. According to the *Polícia Federal*, the price of illegal firearms has increased dramatically after 2003. Federal Marshall Marcos Dantas, in a 2008 interview, reports that the price of the AK-15 jumped from US$ 2,500 in 2005 to more than U$10,000 in 2008.[8] The black market price 9mm pistol in 2008 was U$1,250, up from U$400 in 2005.[9] In addition, a *Parliamentary Commission on Firearms* from the Rio de Janeiro State Assembly gathered information on the type of fire arms apprehended from 2000 onwards. The proportion of homemade firearms, which remained constant at a negligible 0.2% from 2000 through 2003, starts to rise in 2004, reaching almost 11% in 2007. On the other hand, the proportion of handguns (pistols and revolvers) among apprehended guns dropped from roughly 79% in the 2000-2003 period to 68% in 2007. Assuming homemade firearms are a substitute for non-homemade, this evidence suggests an increase in the price of non-homemade weapons after 2003.[10] Increases in prices are only compatible with a large inward shift in supply, because the demand effect would tend to *reduce* prices.[11]

Second, we use the *Pesquisa de Orçamento Familiar* (POF) to compute an indirect measure of what happened with prices and quantities purchased. We reproduce results from Neri (2012) in Table 1. POF is a measure of flows, not stocks. Around 2003, 0.0397% of households bought firearms at any given year. Not surprisingly, it is a small fraction because firearms is a relatively expensive durable good and even in 2003 it was difficult to acquire one legally. *Circa*, 2009, the figure dropped to 0.00236%, a 40% reduction. This amounts to an aggregate reduction from 57,000 to 37,000 of 21,000 firearms purchases per year. Conditional on purchasing a gun, the household spent, in constant R$, R$79 on firearms in 2003 and R$88 in 2009, which suggests that prices went up. A reduction in quantities with prices

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[8] See the newspaper article (in Portuguese) at http://agenciabrasil.ebc.com.br/noticia/2008-10-24/aumento-da-repressao-faz-preco-de-armas-quadruplicar-no-mercado-ilegal-avalia-pf

[99] A demand shift for unregistered firearms due to the increased penalties for illegal possession and carrying would have caused *a reduction* in prices.

[10] Source: compilation by Julio Pucerna based on the information on the *Relatório da Comissão Parlamentar de Inquérito das Armasi, Assembléia Legislativa do Estado do Rio de Janeiro* (2010). Of course, this pattern could also be rationalized by an advancement in the technology of household production of firearms, or by an improvement in apprehension of homemade firearms. There is no evidence on neither of these alternative explanations.

[11] It is conceivable that the demand for illegal arms by criminals increases after the ED if arms on the hands of non-criminals and in the hands of criminals are strategic substitutes. In this case, two empirical facts should follow: a reduction in the prevalence of firearms and, through the deterrence effect, an increase in "contact" property crimes. Both hypothesis are testable, and are tested for below.

increasing corroborates the anecdotal evidence: the ED shifted demand and supply inwards, but the latter retracted more strongly.

[TABLE 1 HERE]

## 6. The Empirical Strategy

### 6.1 The Causal Impact of the ED (Reduced-Form)

We use a strategy inspired in Cook and Durrance (2011). Identification comes from assuming that the ED had a stronger impact in places where firearms were more prevalent when the policy was established. We assume that the impact of the ED is moderated by the proportion of crime that is attributable (or prevented by) to fire guns.

Let $C_{it}$ be the crime rate per 100,000 inhabitants at city *i* in year *t*. We decompose $C_{it}$ into crimes attributable to firearms ($C_{it}^{F}$) and crimes non-attributable to firearms ($C_{it}^{NF}$):

$$C_{it} = C_{it}^{F} + C_{it}^{NF} \tag{1}$$

The formulation allows firearms to save crimes, i.e., $C_{it}^{F} < 0$ although this is clearly a counterfactual object. Let $C_{ib}$ the homicide rate at city *i* at the baseline year *b*. Let $\beta_{ED}^{t}$ be the fraction of crimes attributable to firearms prevented by (or induced by) the ED between the baseline year *b* and year *t*. Assuming that the fraction of homicides prevented by the ED is constant across cities, we postulate that:

$$C_{it}^{NF} = C_{ib}^{NF} \tag{2}$$

$$C_{it}^{F} = (1 - \beta_{ED}^{t})C_{ib}^{F} \tag{3}$$

$\beta_{ED}^{t} \in (0,1)$ means that the ED reduces the number of crimes between years b and *t* (the *lethality hypothesis* is dominant). If $\beta_{ED}^{t} < 0$, then ED causes increase in crime. First-differencing (3) and (2) we have:

$$\Delta C_{it}^{F} \equiv C_{it}^{NF} - C_{ib}^{NF} = 0 \tag{4}$$

$$\Delta C_{it}^{F} \equiv C_{it}^{F} - C_{ib}^{F} = -\beta_{ED}^{t} C_{ib}^{F} \tag{5}$$

Adding (4) and (5):

$$\Delta C_{it} = -\beta_{ED}^{t} C_{ib}^{F} \quad (6)$$

Note that all time-invariant heterogeneity in the levels of crime will be accounted for by the differencing procedure. We do not assume that the amount of crimes attributable and non-attributable to firearms would stay constant in the absence of the ED. In fact, we allow changes in crimes attributable and not attributable to firearms have a common component $\mu_t$ (year fixed effect), depend on changes in controls, on baseline crime, and on a random unobservable city-year specific shock that is common for crimes attributable and non-attributable to firearms ($\varepsilon_{it}$):

$$\Delta C_{it} = -\beta_{ED}^{t} C_{ib}^{F} + \mu_t + \Delta Controls_{it} + Crime\_Baseline_{it} + \varepsilon_{it} \quad (7)$$

where $\Delta Controls_{it}$ includes changes in population and crime-prone population (ages 15 through 24). More importantly, $Controls\_Baseline_{it}$ is the baseline level of crime. Its inclusion is crucial for causal interpretation. Aggregate crime is dropping in the state of São Paulo. It is conceivable that it will drop more where violence was higher to start with because of mean reversion, or because of unobserved policy interventions, which are more likely to be implemented in more violent places. Including $Crime\_Baseline_{it}$ mitigates the possibility that $\beta_{ED}^{t}$ captures these spurious effects.

We have four cross-sections: four equations (7), one for each year from 2004 through 2007. We stack them and estimate an average $\beta_{ED}$. We do estimate year effects separately.

$$\Delta C_{it} = -\beta_{ED} C_{ib}^{F} + \mu_t + \Delta Controls_{it} + Crime\_Baseline_{it} + \varepsilon_{it} \quad (8)$$

If the ED reduced crime, then $-\beta_{ED} < 0$. Otherwise $-\beta_{ED} > 0$.

Implementing the estimation of (8) requires observing $C_{ib}^{F}$. For homicides, we could use the number of murders perpetrated with firearms, which we observe. We do not follow this path for two reasons. First, we do not want to attribute all homicides perpetrated by firearms to firearms.[12] Second, we have no equivalent information for other crimes. We assume that the baseline number of crimes attributable to firearms depends linearly on the availability of firearms:

$$C_{ib}^{F} = a + b \times guns_{ib} \quad (9)$$

Combining (8) and (9), we have:

$$\Delta C_{it} = \theta + \rho guns_{ib} + \mu_t + \Delta Controls_{it} + Crime\_Baseline_{it} + \varepsilon_{it} \quad (10)$$

[12] We will use the information on homicides by firearms to corroborate the idea that the relative importance of firearms in homicides depends on our measure of the prevalence of firearms, and as a robustness check.

where $\theta \equiv -a\beta_{ED}$ and $\rho \equiv -b\beta_{ED}$. We cannot identify $\beta_{ED}$ because $b$ is not identifiable. However, knowledge of $\rho$ (which is identifiable) helps us quantify the impact of the ED assuming $b$ is positive. If the deterrence hypothesis is dominant, then $\rho > 0$; on the other hand, availability of firearms increases crime postulates that $\rho < 0$.

The measure $guns_{ib}$ is also unobservable. We adapt Cook and Ludwig's (2002) and use the number of suicides perpetrated by firearms per 100,000 inhabitants. A small digression on the choice of proxy is warranted. No direct measure of the number of legal firearms circulating is available in Brazil. Let alone illegal firearms. Not even good quality survey data are available at the aggregate level. For the US, the suicide by firearms measure is well validated. Suicides by firearms outperform all other measured used in the literature. Using aggregate survey data as a benchmark, Azrael et al (2001) show that the correlation between survey-based data and the proportion of suicides by firearms is higher than two other clean measures: membership in the National Rifle Association, and subscription to Guns & Ammo (Duggan, 2001).

Evidence suggests that suicides by firearms are related to crime, which poses challenges to identification. Potash *et al*. (2000) show that suicides are related to psychological and social characteristics, such as bipolar disorder or substance abuse.[13] Suicides in general show little resemblance with homicides or suicides by firearms (see discussion in Section 7.2).

There is a concern with the use of suicides by firearms in the context of the ED. The ED has sharply increased the penalties for illegal possession. It is conceivable that following the ED people that would surrender the guns kept at home, reducing suicides by firearms. However, one may be skeptical of this mechanism because the chances of getting caught at home with an illegal firearm are slim. The "ease-of-acquiring-guns" is a more convincing reason why less availability of firearms translates into fewer suicides by firearms (Ludwig and Cook, 2002). The ED has made it more difficult to acquire a legal weapon, i.e., an inward shift in the supply of legal firearms. The legal and illegal markets have communicating vases; thus, the general equilibrium effect is an increase in prices in *both* markets, and an overall reduction in equilibrium quantity of firearms. Higher prices mean fewer suicides with firearms. In fact anecdotal evidence presented in section 5 suggests that prices in the illegal market increased sharply after 2003.

The literature normally uses the *proportion* of suicides by firearms. In our case, it is natural to use the number suicides by firearms (per 100,000 inhabitants) because the specification calls for a variable that resembles the absolute number, not a relative measure. In all regressions we include the total number of suicides per 100,000 inhabitants as a control to avoid capturing spurious effects due to substance abuse for example. We also use the ratio of suicides by firearms to probe the robustness of results.

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[13] Insofar as substance abuse causes both suicides and homicides, one could be concerned that suicides may captures changes in substance abuse at the local level. The literature normally deals with this issue by using the proportion of suicides by PAF, assuming implicitly that substance abuse increase the odds of suicide in general, not suicides by firearms in particular.

We also measure firearms prevalence by the number of firearms apprehended per 100,000 inhabitants. Because apprehensions are contaminated with enforcement, the apprehension data has limited usefulness. We use it for robustness purposes only.[14]

In summary, our estimation strategy assumes that some cities were "treated" more intensely with the ED. The intensity of treatment depends on the baseline prevalence of firearms, as measures by suicides per

### 6.2 The Causal Impact of the ED on Firearms Availability (Mechanism or First-Stage)

We first estimate the impact of ED gun availability. The credibility of any estimate of the causal impact of ED on crime rests on documenting the mechanism. The ED must have had an impact on the availability of firearms. Incidentally, the impact of the ED on availability will serve as the first stage when estimating the causal impact of firearms on crime.

The estimation strategy is similar to the one we use to recover the causal impact of the ED on crime. We postulate that any intervention to reduce the availability of firearms has a larger impact in reducing the *number* of guns per capita depends on the prevalence of guns at the baseline. Let $\gamma_{ED}^{t}$ be the fraction of guns out of circulation at year $t$ because of the ED. We postulate that

$$guns_{it} = (1 - \gamma_{ED}^{t})guns_{ib} \tag{11}$$

Taking differences between $t$ and the baseline year $b$ in (11), we have:

$$\Delta guns_{it} \equiv guns_{it} - guns_{ib} = -\gamma_{ED}^{t} guns_{ib} \tag{12}$$

We do not estimate $\gamma_{ED}^{t}$, but stack the four cross-sections and estimate an average $\gamma_{ED}$. The estimated model is

$$\Delta guns_{it} = -\gamma_{ED} guns_{ib} + \mu_t + \Delta Controls_{it} + Crime\_Baseline_{it} + \epsilon_{it} \tag{13}$$

where $\epsilon_{it}$ is a random shock. Again, *guns* are proxied by the number of suicides by firearms per 100,000 inhabitants. We cannot control for the baseline level of dependent variable as we do in the crime equations. We control for the baseline level of homicides, the best measured crime category, to account for mechanical

[14] We could control for the number of policemen at the city level, an imperfect measure of enforcement intensity. Because enforcement is endogenous to crime, its inclusion would solve one problem by creating another, possibly harder to solve.

drops in guns in places that were more violent to begin with. We hypothesize that the ED reduced the prevalence of firearms, i.e., $-\gamma_{ED} < 0$.

### 6.3 Estimating the Causal Effect of Firearms on Crimes

Under the identification assumption that the ED caused a change in crime only through its impact on the availability of arms, changes in firearms explained by the baseline number of firearms are exogenous variation to estimate the causal impact of firearms on crime. Equation (13) is the first stage.

The crucial identification assumption is that baseline firearm does not belong to the structural crime equation (exclusion restriction). The *inclusion* assumption (i.e., what justifies the first stage) is that the impact of the ED is stronger where there were more arms in the baseline. Sure enough, firearms in 2003 cannot cause crime in 2007 above and beyond firearms in 2007. Still, baseline firearms can be systematically related to unobserved factors that belong to the structural equation of crime. Violence was particularly high where baseline arms were high, and bound to drop anyway because of mean reversion or because of stronger enforcement reaction at the local level. Both issues can be dealt with by controlling for the baseline level of crime. Finally, we also need that, prior to the ED, crime was not following systematically different trends in cities with more and less guns in the baseline, which is verifiable empirically.

## 7. Results

### 7.1 Summary Statistics

Table 2 contains the summary statistics on the crime categories investigated both for the 118 cities included in the main sample (more than 50,000 inhabitants on average during years 2001-2003), and statewide. Some relevant facts emerge. The typical city in the main sample was violent in the years 2001-2003: 21.09 homicides per 100,000 inhabitants on average, with a standard deviation of homicides across cities of 12.01.[15] Statewide violence was considerably higher: 37.66 homicides per 100,000 inhabitants.[16] From 2001 through 2003, 94% of homicides were perpetrated by firearms in a typical city in the main sample. Statewide, "only" 67% of homicides were perpetrated by firearms. From 2004 through 2007, homicides perpetrated by firearms dropped in the main sample more than homicides by other means. In 2007, homicides by firearms represented no more than 60% of all homicides in a typical city in the main

[15] The World Health Organization considers endemic violence rates above 10 homicides.

[16] Averages for the main sample treat cities with different populations equally. Larger cities, which are more violent, have more impact on aggregate statewide violence.

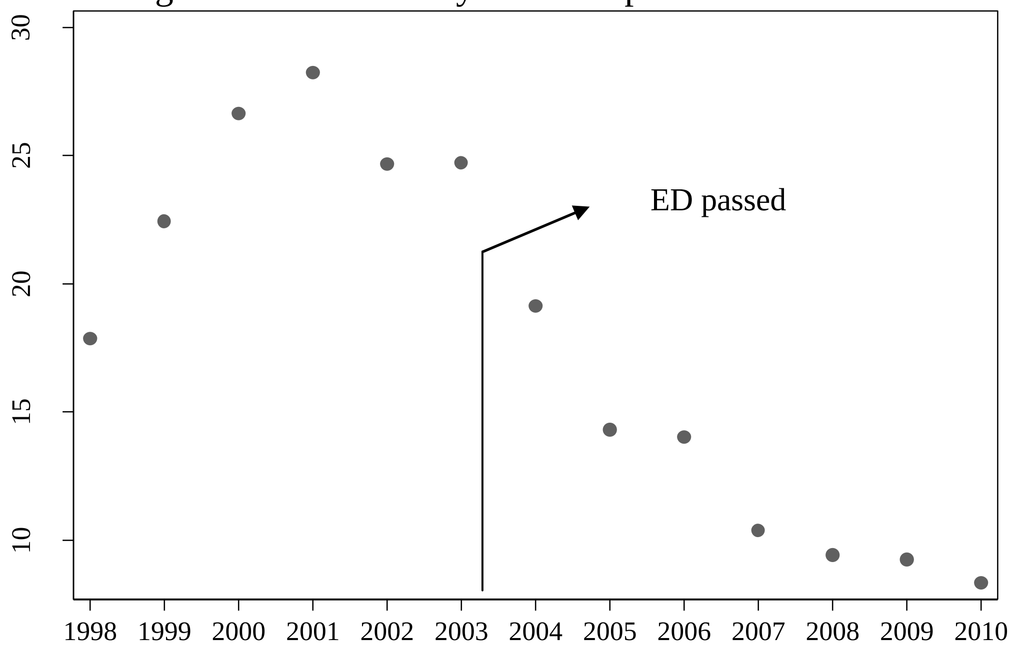
sample.[17] In contrast, most suicides are committed through means other than firearms. Suicides by firearms are not only rare – only 0.70 per 100,000 inhabitants –, but also very volatile across cities, with most noise coming from smaller cities. In about 8% of the cities no suicides occurred during the 2001-2003 period. Interestingly, averages suggest that suicides by firearms dropped by more than 20% over period, while suicides in general remained flat. Property crime and vehicle robbery also dropped after 2003, suggesting a general reduction in crime during the period. However, they dropped much less than homicides.

[TABLE 2 HERE]

### 7.2 Aggregate Time-Series Evidence

Before proceeding with the estimation results, we investigate the aggregate pattern of the data. Figure 1 depicts homicides by firearms per 100,000 inhabitants over the period 1998 – 2010. It shows two facts: 1) gun violence rose sharply in the 1990s, reaching a peak in 2001, and then dropped steadily; 2) the reduction accelerated after 2003.

Figure 1: Homicides by Firearms per 100thd inhabitants




Inspection of Figure 2, which depicts the evolution of homicides by means other than firearms, shows that non-gun peaked years earlier. The late 1990 epidemic of violence is mainly an epidemic of gun violence. In addition, we do not see any marked difference after 2003.

[17] Figure not reported in Table 1. In 2007, there were 8.21 and 13.67 homicides by firearms and overall homicides in the main sample.

Figure 2: Homicides Not by Firearms per 100thd inhabitants

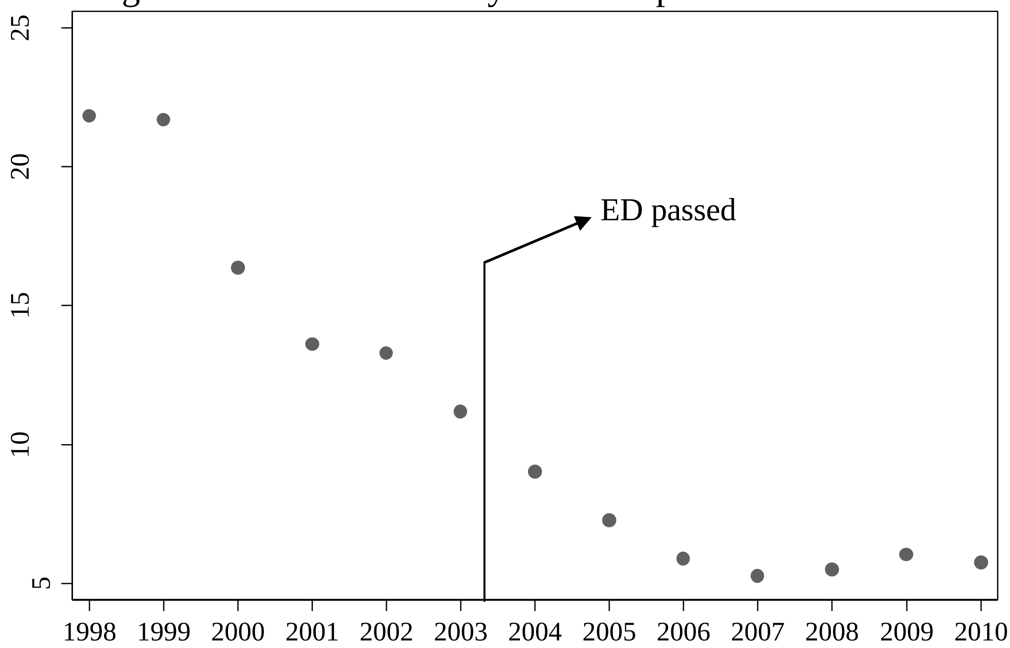



Figure 3 and 4 depict the ratio of homicides perpetrated by firearms to homicides not by firearms. Figure 3 uses data from all cities with more than 50,000 inhabitants in 2003. The ratio increases steadily until 2003, when it seems to reach a peak. It then drops steadily, except for the year 2006, when it again raises sharply. The year 2006 is an outlier. Gun violence skyrocketed because of the attacks of the *Primeiro Comando da Capital* on the police and its retaliation.[18] The attacks happened almost exclusively in the São Paulo Metropolitan Area. Figure 4 is the same as Figure 3, except that we exclude the São Paulo Metropolitan Area for the whole period. We clearly see now that the ratio of homicides by firearms to homicides not by firearms peaks in 2003, the year the *ED* passed into law.

[18] For a full account of the *PCC* attacks see

Fig. 3: Ratio Homicides by Firearms - Non Firearms

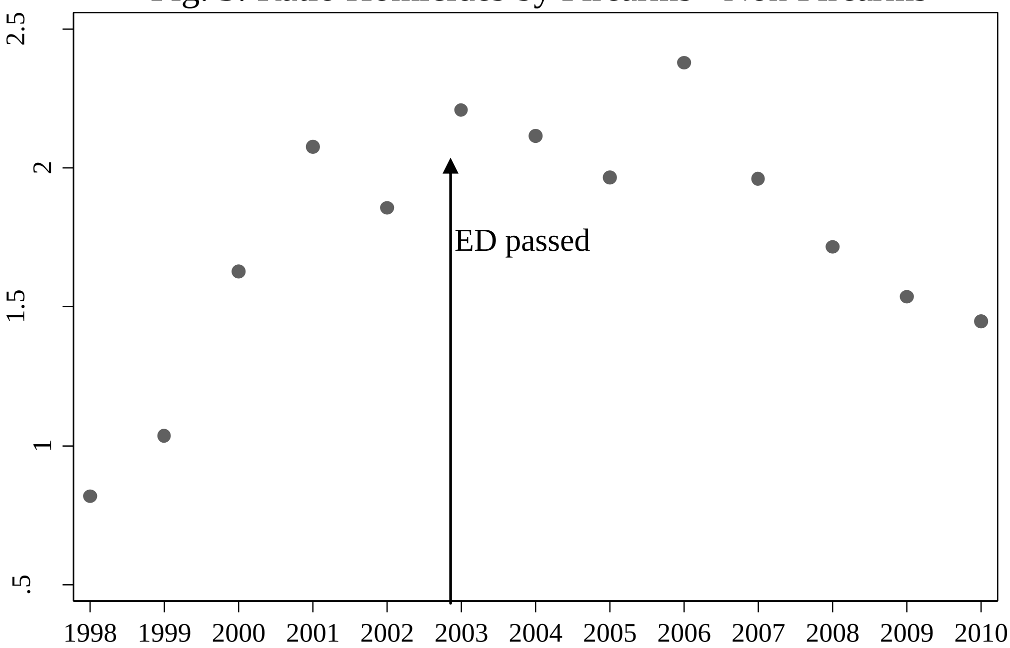
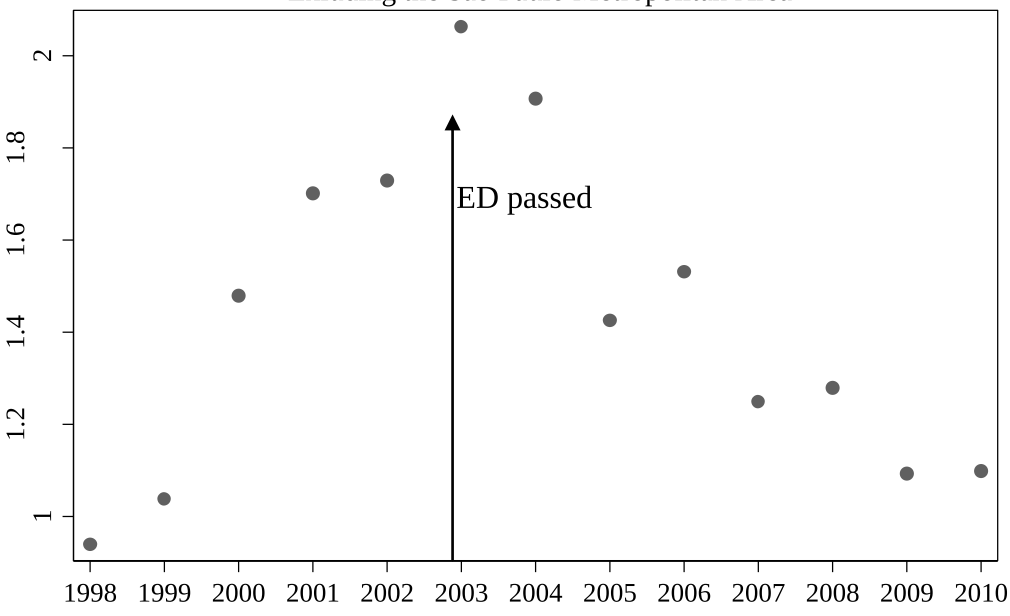



Fig. 4: Ratio Homicides by Firearms - Non Firearms

Exluding the Sao Paulo Metropolitan Area



Figures 5 and 6 depict our two main measures of firearms prevalence: suicides by firearms per 100,000 inhabitants and the ratio suicides by firearms-to-suicides by other means (see Section 6.1), for which we have data from 1998 through 2010. Suicide data is noisy. Figure 5 shows that suicides by firearms per 100,000 inhabitants were all over the place before 2004. There is a big drop between 2003 and 2004. Although suicides by firearms seem to be dropping since 2001, only after 2003 one sees what seems to be a definite drop in the number of suicides by firearms. In fact, 2001 and 2002 could well be outliers. Figure 3 depicts the ratio of suicides by firearms to suicides by other means. There is a 2003 - 2004 drop is now even more pronounced. The same pattern arises for the ratio of suicides by firearms to suicides by means other than firearms (Figure 6).

Figure 5: Suicides by Firearms per 100 thousand inhabitants

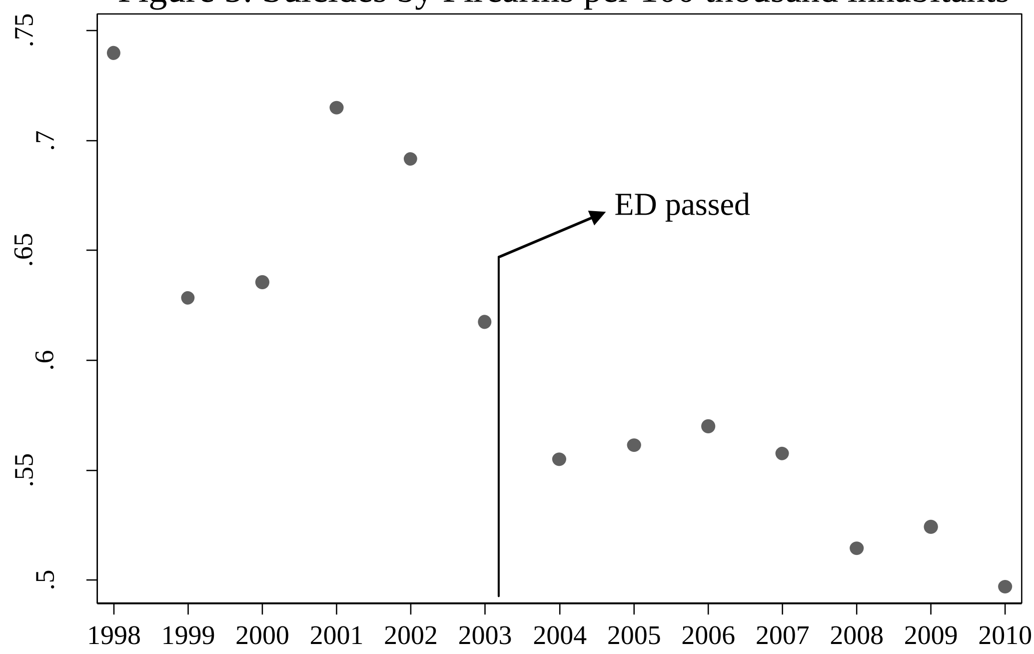
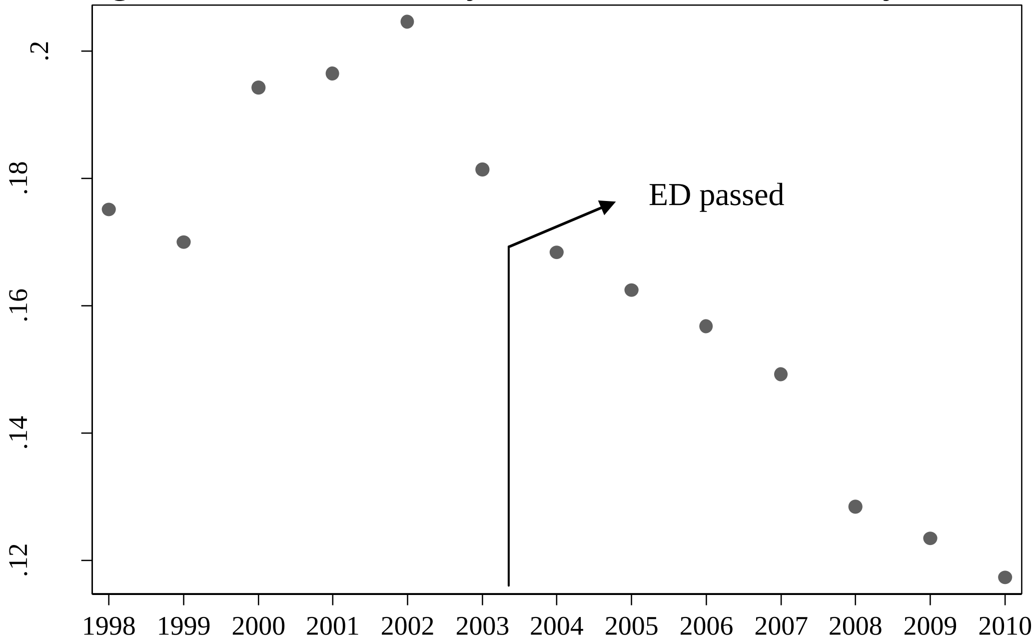


Figure 6: Ratio Suicides by Firearms to Suicides not by Firearms




Finally, Figure 7 depicts an alternative measure of firearms presence: apprehension of illegal carrying of firearms per 100,000 inhabitants at the state level from 2001 through 2007 (period of data availability). The figure shows two facts: 1) illegal carrying had been declining before the law; 2) the decline accelerated after 2004. Illegal carrying confounds enforcement and the prevalence of arms, and it is not theoretically clear whether illegal possession should go down with the ED, especially in the short run. Apprehension could have gone up because ED turned carrying fire weapons (even registered) into a felony. Bearing that in mind, the pattern is similar to the patter of suicides by firearms.

Fig 7: Apprehension of Firearms per 100 thousand inhabitants
2001 through 2007

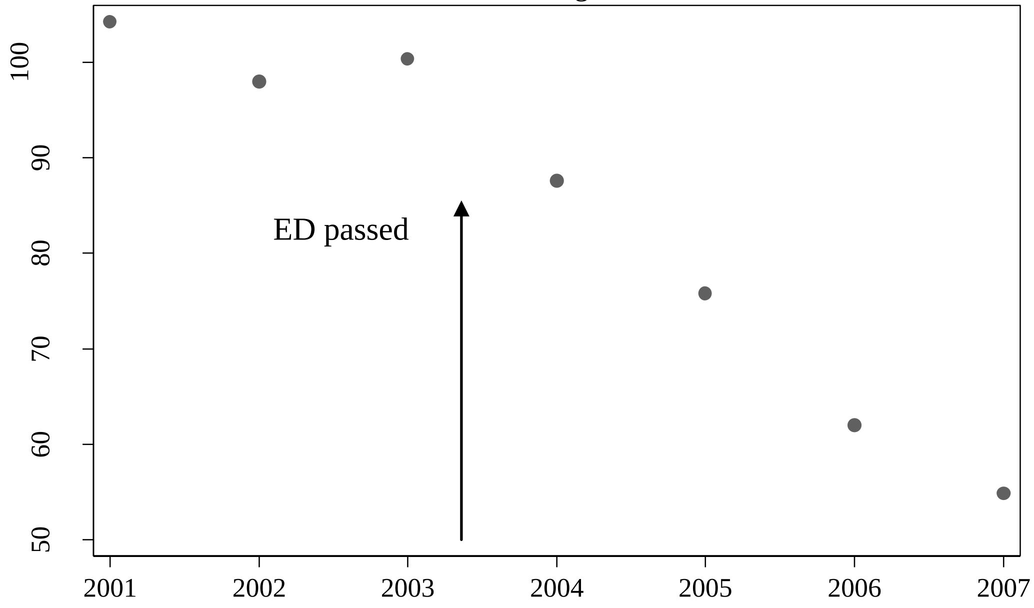



Figure 8 depicts the dynamics of the index of property crime. The first noticeable feature is the jump in 2003, which illustrates the perils in interpreting pure time-series variation in property crimes. Reporting of thefts of cellular phones, which increased strongly in the beginning of the 2000s, is the main culprit for explaining the jump. Second, no distinguishable pattern arises between 2003 and 2004/2005. Figure 9 depicts the trends in car robbery. Besides the secular decline throughout the 2000s, no other feature is noticeable. In particular, no visible change occurs after 2003.

Fig 8: Property Crime per 100 thousand inhabitants

2001 through 2007

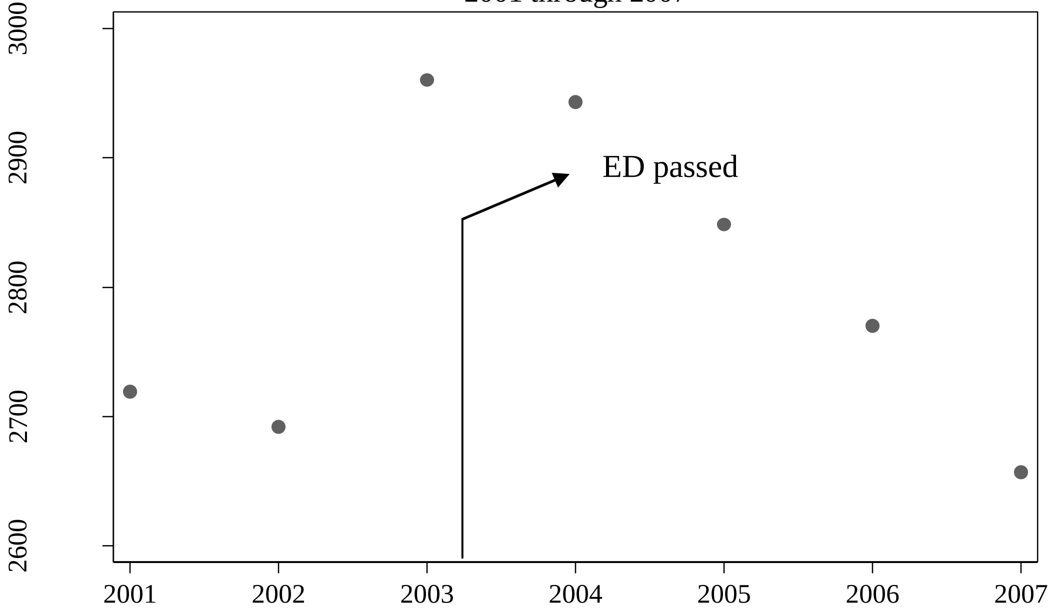
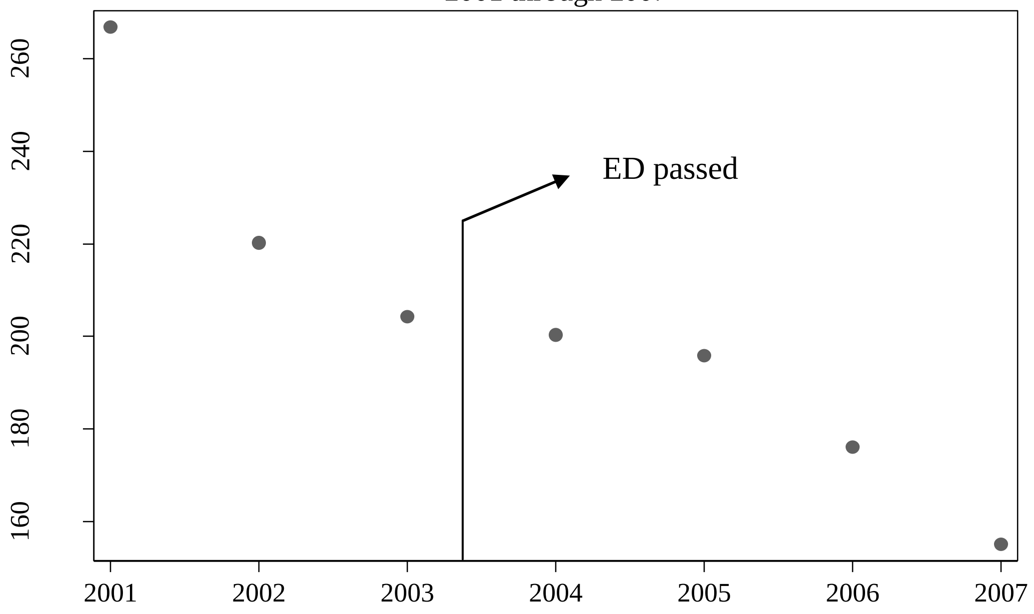



Fig 9: Vehcicle Robbery per 100 thousand inhabitants

2001 through 2007



### 7.3 The Causal Effect of the ED on Crime (the Reduced Form)

#### 7.3.1 Homicides

Table 3 contains estimates of the parameters in equation (10) and a few variants. The main outcome is homicides by firearms. But we also gauge the impact of the ED on homicides non-firearms, which could serve both as a placebo and to capture any substitution effect away from firearms. All reported standard errors are computed by clustering observations at the city level.[19] First, we regress the difference between homicides by firearms at *t* and the baseline on the measure of firearms prevalence at the baseline, i.e., suicides by firearms per 100,000 inhabitants (Column 1). Because suicides by firearms are quite noisy, the baseline is the average from 2001 through 2003. The estimated coefficient on firearms proxy ($\hat{\rho}$) is -2.842 and statistically significant at the 10 percent level. The ED reduced the number of homicides.

[TABLE 3 HERE]

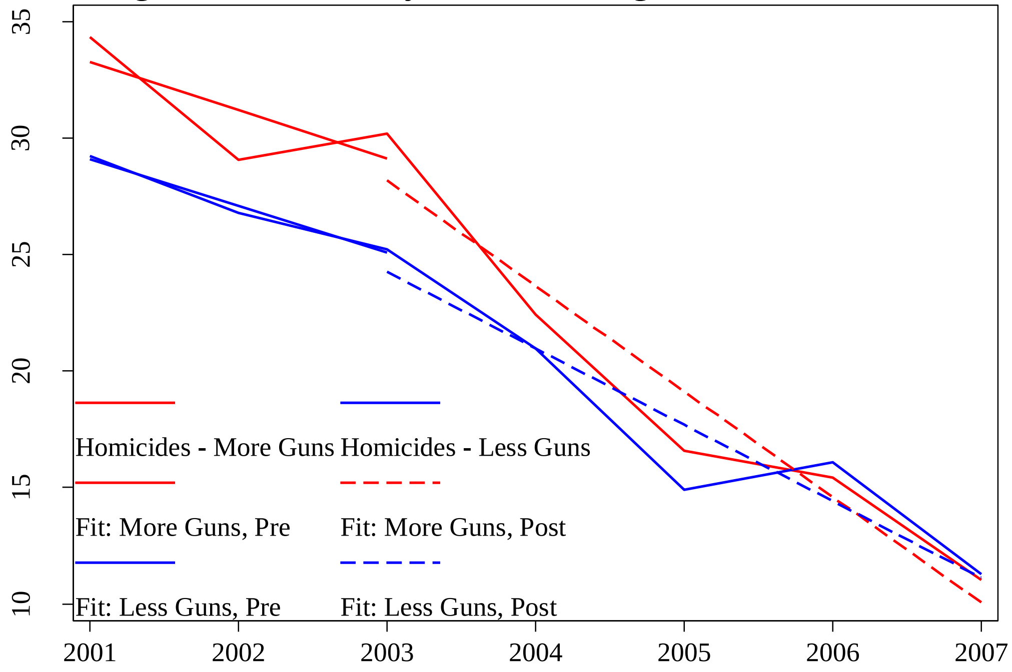
In Column (2) we include the baseline homicides. Firearms are more prevalent in more violent places (see Figure 10). It is plausible that homicides dropped more sharply where violence was higher because of mean reversion or unobserved crime reducing policies. Indeed, the coefficient on baseline homicides is highly significant statistically. As expected, the impact of the ED is now smaller, but only slightly (-2.637), but is *more precisely* estimated (statistically significant at the 5% level). In Column (3) we define treatment not continuously but as a dummy that assumes the 1 if the city had suicides by firearms above the median in the baseline, 0 otherwise. Defining treatment as a dichotomous variable simplifies the computation of the number of lives saved by the ED. The coefficient associated with the treatment dummy is -2.500 (*p*-value < 3%), similar to the coefficient associated with the continuous treatment variable in column 3. The baseline homicide rate in the "treatment" group was 19.81. Thus the impact of the ED is to reduce the homicide rate by 12.6%. This represents 2,000 lives from 2004 through 2007.

The dummy treatment approach also makes it explicit one identification assumption: prior to the ED, homicides had to be dropping at the same rate in cities with more guns and in cities with fewer guns. We can verify the plausibility of this assumption. Figure 10 depicts homicides by firearms for the two groups of cities, as well as fitted lines before and after the ED. Before the ED, cities with more guns had higher violence, which is not surprising. Nut the trend is almost perfectly parallel. After the ED, homicides dropped faster in cities with more guns. The difference between 2003 and 2007 between the two lines is 10 homicides per 100,000 inhabitants, which implies an annual effect of 2.5 homicides per 100,000. This is exactly the estimated coefficient on the dummy in Column (3).

---

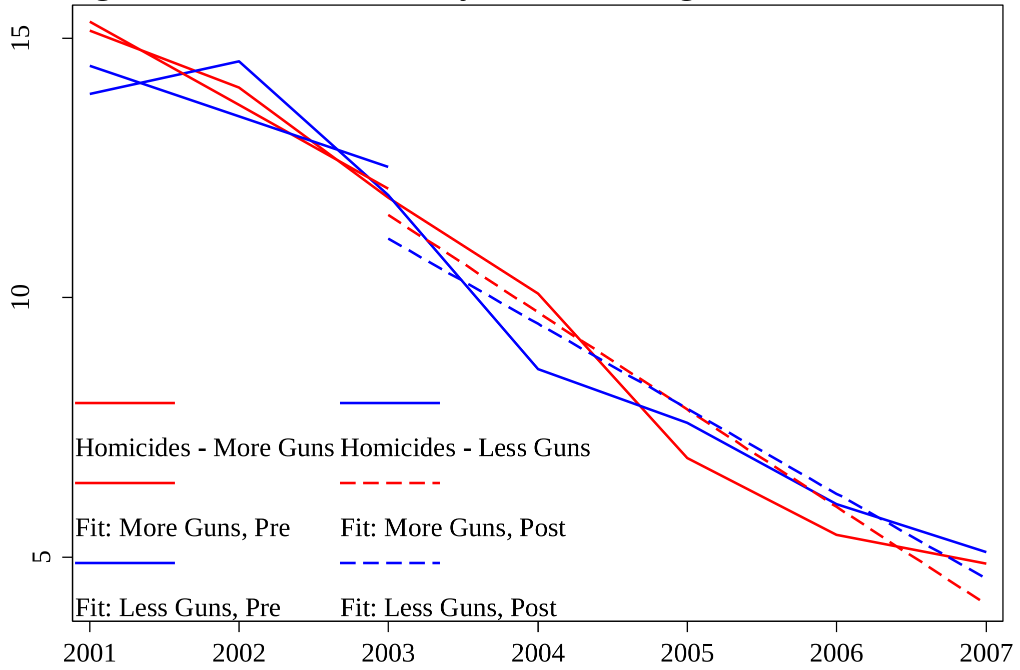
[19] Clustering is important in our case. The model is a stacked cross-section, and the main regressor only varies between, not within, city.

Fig. 10 Homicides by Firearms - High and Low Gun Cities




In Column (4), the dependent variable is homicides not perpetrated by firearms. The impact of the baseline prevalence of firearms is now indistinguishable from zero (point estimate is positive but negligible, and statistically insignificant). Again, we can see this result graphically. Figure 11 is the same as Figure 100 but for homicides perpetrated by means other than firearms. Homicides not by firearms were dropping faster in cities with more guns before the ED; and they continue to drop faster after the ED, at about the same rate.

Figure 11 Homicides not by Firearms- High and Low Gun Cities




In Column (5) we weight the observations by population, a common procedure in the literature because homicides are noisier is smaller cities (and suicides by firearms even noisier). In addition, weighing observation by population yields an estimate that is representative of the aggregate impact of the ED. The estimated coefficient is now -4.075. Performing the same exercise that we did in Column (3) (omitted for conciseness), the ED caused an 11% drop in homicides. Because larger cities are more violent, weighing by population represents a higher number of lives saved: 2,750 lives. In Columns (6) through (9) we re-estimate the same models using another commonly used in the literature: the ratio suicides by firearms/suicides by other means. If anything, estimates are more precise.[20]

Table 4 contains robustness exercises. We first report estimates when we include the lag of the dependent variable, which we hope will pick up any additional mean reversion, or unobserved policy reactions, not captured by baseline homicides.[21] Columns (1) and (2) have the results for the un-weighted and weighted samples. The coefficient associated with the lagged dependent variable is positive and highly statistically significant, even after the inclusion of baseline homicides. The model is becoming close to saturated, with an $R^2$ of almost 80%. The inclusion of the lagged dependent variable does absorb some of the effect due to the baseline firearms measure (*and* the baseline homicides). But homicides still dropped more where there were more firearms in 2003 (and more precisely estimated than in Table 3). We then run two robustness tests on specification: Deltas-in-Logs and Logs-in-Logs, for the weighted and un-

[20] In terms of practical significance, magnitudes are similar. We omit the discussion for conciseness.

[21] We do not dwell into the difficulties in identifying lagged dependent variable models, especially because we have a time-series of four years.

weighted procedures. Coefficients are more precisely estimated and, in terms of practical significance, results are in line with Table 3.

[TABLE 4 HERE]

### 7.3.3 Property Crime and Car Robbery

In order to test the deterrence hypothesis, we change the dependent variable to be an index of property crime per 100,000 thousand inhabitants.[22] The right-hand side specifications are the same as in Table 3. Results are in Table 5. Columns (1), (2) and (3) replicate Columns (1), (2), (4) in Table 3 (Column 3 – homicides by non-firearms – is not replicable for property crimes). In all three cases the estimated coefficient is positive, meaning property crime *increased* where arms were more prevalent in the baseline, in line with the deterrence hypothesis. However, none of the results are statistically significant. The property crime index is not particularly imprecise in small cities due to the rarity of the event. Thus, we take a leap and estimate the model for the sample of *all* 648 cities in the state of São Paulo, both weighting and un-weighting observations by population (Columns 4 and 5). The estimated coefficients are small and oscillate around zero. In both cases they are insignificant statistically. The remainder of the table replicates the results using the ratio of suicides-by-firearms-to-suicides-by-other-means measure of firearms prevalence. Results are similar.

Absence of statistical significant could be due to lack of precision. However, the coefficients are not large in magnitude. We cannot gauge practical significance in terms of contribution to the drop in property crimes because most coefficients suggest that, if anything, the ED has caused an *increase* in property crimes. Thus we measure it in terms of standard deviations. The largest positive estimated coefficient is in Column (3), 53.186, and represents roughly 13% of the standard deviation of the change in property crimes (396 in Table 2). In contrast, the equivalent coefficient associated with homicides – -4.075 (Table 3, Column 5) – amounts to 38% of standard deviation of the change in homicides by firearms (10.69 in Table 2).

The use of the property crime index is problematic for two reasons. Survey data suggest that property crime under-reporting is rampant in São Paulo.[23] In addition, under-reporting may be changing over time, and we cannot guarantee that the dynamics across cities are similar. The second reason lies in the composition of the index. The deterrence hypothesis applies more naturally for robberies, a type of crime in which there is contact between the perpetrator and the victim. The Brazilian Penal Code defines thefts as occurring without the use of physical threat. Thus, guns should play less of a role in deterring thefts.

[22] The property crime index is the sum of all property crimes. It includes robberies, thefts, larceny and burglaries.
[23] Total property Crime rates in São Paulo are a little lower than the US. Car theft and car robbery are less susceptible to under-reporting because of insurance and legal liability reasons. Not surprisingly, they are significantly higher than in the US. See Biderman et al (2010).

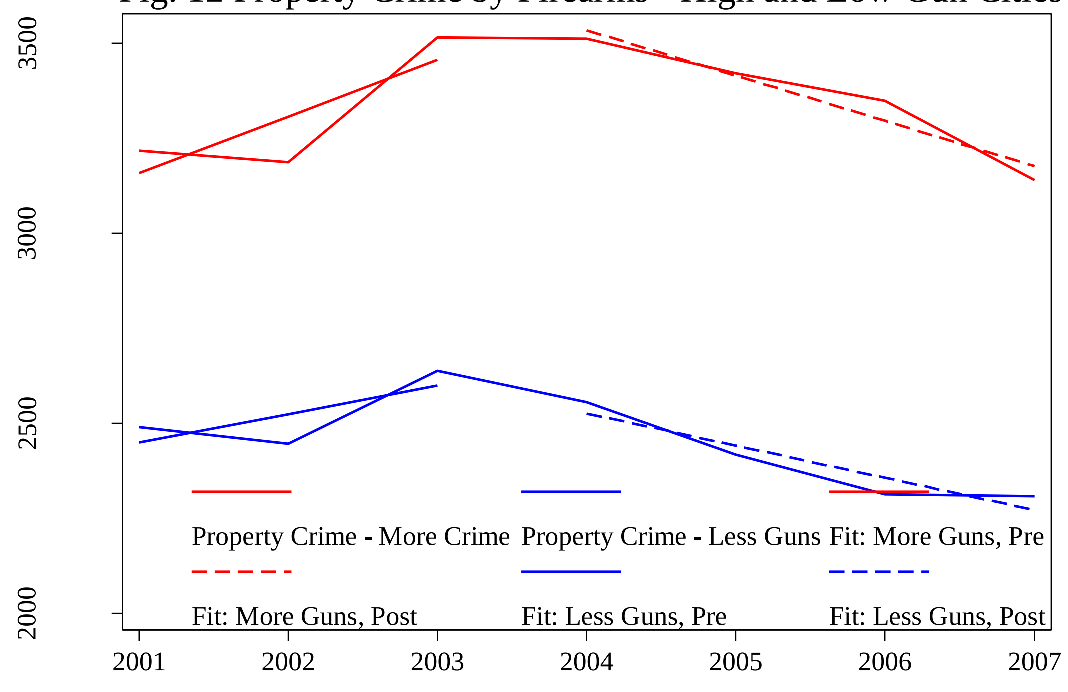
We mitigate both problems by focusing on car robberies, which are perpetrated by use of physical threat and are fairly well-reported to the police.[24] Table 6 has the results of the same models in Table 5. The sign of the coefficient associated with baseline car robberies is positive across the board, in line with the deterrence hypothesis. However, we can never reject the zero-null at standard significance levels. In terms of magnitudes, the largest estimated coefficient represents roughly 39% of a standard deviation in the change in car robberies, about the same as the impact on homicides. All other coefficients are smaller in magnitude (in terms of standard deviations) than their homicide counterparts. Results in Columns (5) through (10) show essentially the same facts.

[TABLE 5 HERE]

[TABLE 6 HERE]

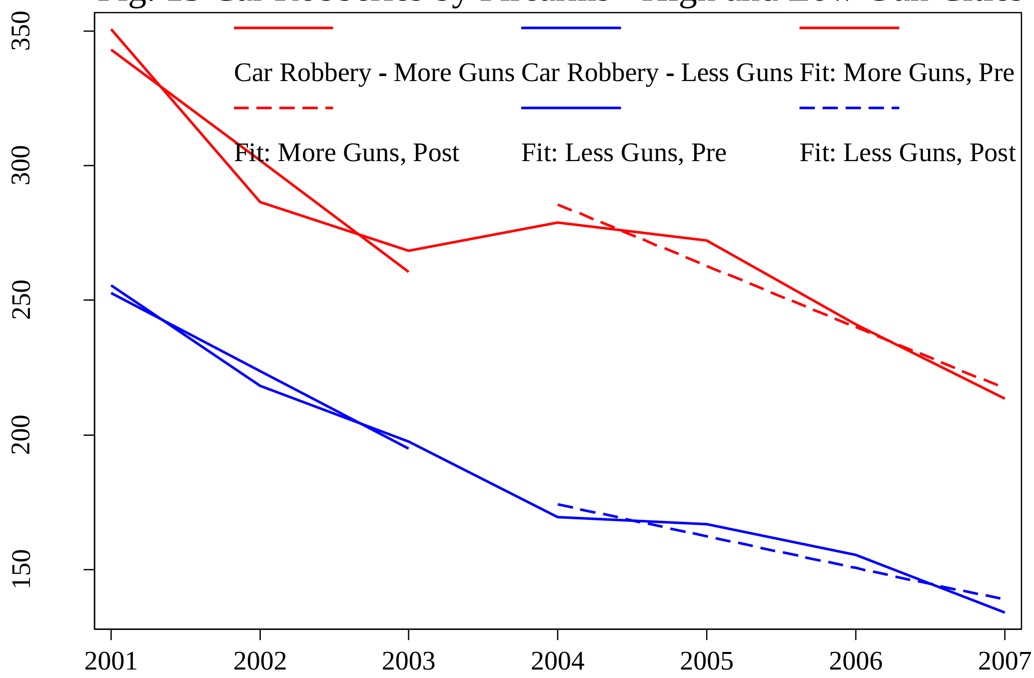
Figure 12 and 13 depicts the trends and the linear fits for the groups of cities for property crime and vehicle robbery, respectively. In both cases, trends seem quite similar before the ED, and continue to be similar afterwards.

Fig. 12 Property Crime by Firearms - High and Low Gun Cities




[24] Car robbery *and* theft are well reported for insurance and to avoid having one's name associated with crimes perpetrated with stolen cars (Biderman et al, 2010).

Fig. 13 Car Robberies by Firearms - High and Low Gun Cities




In summary, we have no evidence that the ED caused an increase in property crimes in general. If one is willing to interpret statistically insignificant estimated coefficients, in the best case there is weak evidence that the ED caused an increase in car robberies.

### 7.4 The Causal Effect of the ED on Gun Availability (the Mechanism or First-Stage)

Table 7 shows estimates of several specifications of the model in equation (13). Column (1), show that firearms' prevalence dropped more pronouncedly where they were more prevalent in the baseline. Cities with higher baseline arms were also more violent to begin with. In Column (2) we include baseline homicides to account for baseline differences in violence. Results are unchanged.

Reversion of the mean could rationalize results. In Column (3) we include the lag of the dependent variable as a regressor, which will capture reversion of the mean, at least partially. The estimated impact of the ED on firearms availability is now smaller, but still negative and statistically significant.[25] In Column (4) we perform a placebo exercise by changing the dependent variable to the suicides perpetrated with means other than firearms. We find no impact of ED. In columns (5) and (6) we weight estimates by population and run the same model as in Column (2) for the whole sample and for our preferred sample of cities with

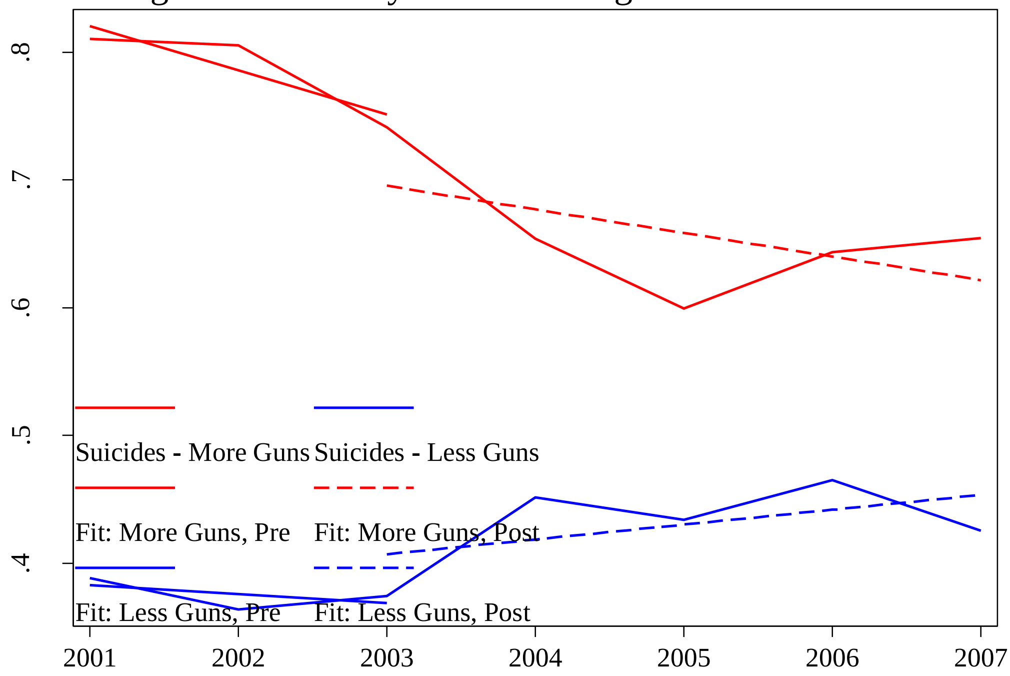
[25] Given the difficulties in identifying dynamic models, we only report OLS regressions from Table 4, column 7.

more than 50,000 thousand inhabitants. Results are similar. In Columns (7) through (9) we repeat some of the exercises for the alternative measure of baseline firearms prevalence. Results are unchanged.[26]

[TABLE 7 HERE]

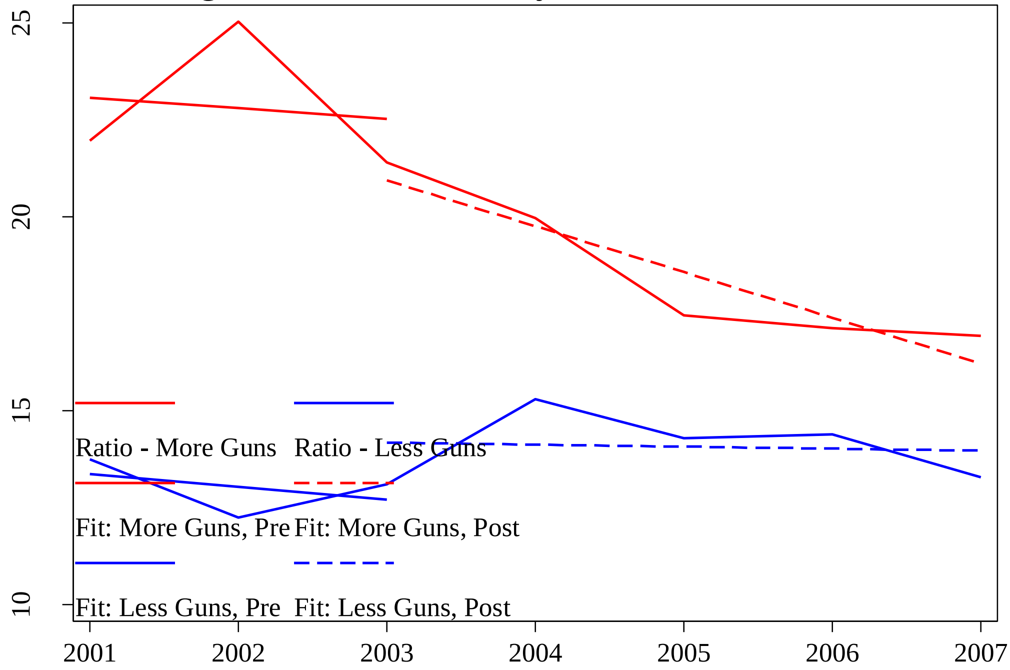
Again, we check whether the trends gun prevalence were different before the ED in high and low gun cities. Figures 14 and 15 depict the trends for suicides by firearms and the ratio of suicides by firearms to suicides by other means. For both measures, the trends are very similar before the ED and before dissimilar afterwards.

Fig. 14 Suicides by Firearms - High and Low Gun Cities




[26] For conciseness we do not report all exercises. Results always remain similar.

Fig. 15 Ratio: Suicides by Firearms/Non Firearms




### 7.4 The Causal Effect of Firearms on Homicide (the structural equation)

Table 7 shows that the enactment of the ED reduced the availability of arms as measured by the prevalence of suicides by firearms. Assuming the impact of the ED on homicides is intermediated by its impact on the availability of firearms, we can use the baseline prevalence of firearms instrument for changes in firearms prevalence and estimate the causal impact of firearms on crime.

The exclusion restriction is plausible in our data. It is hard to imagine any reason why criminals would take into account the stricter gun law when deciding to commit a crime. The fact the ED is a national legislation helps the credibility exclusion restriction because the ED was not implemented as a local response to crime, thus is it exogenous to variation across cities.

The identifying exogenous variation is the differential drop in in the prevalence of firearms depending on the initial baseline prevalence of firearms. Because prevalence was higher in more violent cities, reversion to the mean in both variables, or unobserved local policy responses to high crime, could both cause the instrument to capture unobserved heterogeneity in the structural equation relating arms to crime. This possibility poses little threat to our identification strategy. Baseline prevalence of firearms is associated with subsequent reductions in homicides *even after controlling for baseline homicides* (see Table 2). Thus, the reduced-form association between these two variables is not driven by reversion to the mean. In addition, if more violent cities adopted other (unobserved) crime fighting policies circa 2003, the baseline homicides will capture this variation in policy reactions.

[TABLE 8 HERE]

Estimated coefficients are in Table 8. Column (1) has our preferred specification, with all controls included in the first-stage and the reduced form. A reduction of one suicide by firearms per 100,000 inhabitants causes a reduction of 2.155 homicides by firearms per 100,000 inhabitants. To assess practical relevance, 1.15 is roughly one standard deviation in ∆Suicides by firearms per 100,000 inhabitants; the standard deviation of ∆Homicides by firearms per 100,000 inhabitants is 10.69. Thus a one standard deviation reduction in our measure of firearms prevalence causes a drop of roughly on fourth of a standard deviation in homicides by firearms ($(1.15 \times 2.155)/10.69$). In Columns (2) and (3) we weight observations according to population and estimate the model for the main sample and for all cities. Results, if anything, are stronger. In Column (4) we change the dependent variable to all homicides (not only those perpetrated by firearms). As expected the estimated coefficient is much lower. In Columns (5) through (10) we change the instrument to the ratio suicides by firearms – to – suicides by other means. Results are similar.

Tables 9 and 10 contain the structural results for property crime and car robbery, which essentially show that changes in the prevalence of firearms has no impact property crime and car robberies.[27]

[TABLE 9 HERE]

[TABLE 10 HERE]

### 7.5 An Alternative Measure of Firearms: Arms Apprehended by the Police

For robustness and validation purposes, we present a summary of previous results using an alternative measure of firearms prevalence: the number of arms apprehended by the police per 100,000 inhabitants.[28] Table 11 shows both the first-stage and the impact of firearms on suicide by firearms and vice-versa. In Column (1) we see that the apprehension of firearms dropped faster where apprehension were more frequent at the baseline (first-stage). In column (2) we include baseline homicides to control for baseline violence. Results are unchanged. It is possible that enforcement increase more sharply where guns were more prevalent. In column (3) we include the change in enforcement (which is admittedly endogenous, but it is the best one can do to control for enforcement). Results are unchanged. In columns (4) and (5) we weight by population and use the sample of more than 50,000 inhabitants and the whole sample, respectively. Results are again similar.

Columns (6) and (7) present two validation exercises. First, we regress changes in firearms prevalence on the baseline *suicides by firearms* (6), and then we regress changes in *suicides by firearms* on baseline apprehension. In both cases, prevalence of firearms drops more intensely where prevalence was

[27] This should not come as a surprise given that the reduced form showed no impact of the ED on property crime and car robbery.
[28] For conciseness we do not present all results using the alternative measure. They are available upon request.

higher when using the *other proxy* of baseline firearms prevalence. This increases our confidence that first-stage estimates in Tables 6 and 8 are not driven mechanically by mean reversion, for example.

[TABLE 11 HERE]

Table 12 shows the reduced form and the structural estimates. Results should be viewed with a grain of salt, as explained above: apprehension is contaminated by enforcement, which we cannot account for. The dependent variable is changes in homicides. Column (1) shows a strong in impact of baseline apprehensions on homicides. However, most of the impact is capturing the fact more arms were apprehended where homicides were higher to begin with. Including baseline homicides reduces the impact of baseline apprehension on homicides by two thirds. But we still find that homicides dropped more where baseline apprehensions where higher. Results remain similar when we weight by population (Columns 3 and 4). Columns (5) through (7) show the structural estimates. The point estimate in column (5) is positive as before, but precision is low and we cannot reject the zero-null. In columns (6) and (7) we weight by population, and use both preferred sample (more than 50,000) and the whole sample. We also find that more arms cause more homicides. Now we are able to reject the zero-null at standard significance levels (5% and 10%).

[TABLE 12 HERE]

Finally, Tables 13 and 14 present reduced-form and structural estimates for the impact on property crime and car robbery. If results in Table 12 are interpretable at all, an increase in the prevalence of firearms *increases* property crime. But in none of the cases estimates are significant statistically or practically (except for the reduced-form when we do not control for baseline property crime, a result that is clearly nor interpretable as causal). We find similar results when the dependent variable is car robberies (Table 13).

In summary, when using firearms' apprehension, we find similar results across the board: firearms increase homicides, and estimates are mostly undistinguishable from zero for property crime and car robberies.

[TABLE 13 HERE]
[TABLE 14 HERE]

## 8. Discussion

We estimate that the ED caused saved some 2,800 lives in the state of São Paulo over the course of a four-year period (2004 through 2007). If we were to extrapolate this effect for the whole country, it would have meant that the ED saved almost 14,000 lives, roughly 7.4% of the homicides over the period.

This paper has two main findings. First, the ED – a national-level policy that restricted the legal possession of firearms and increased the penalties for illegal possession – reduced the prevalence of firearms. The enactment of the ED is also associated with a non-negligible reduction in homicides, with no discernible impact on an index of property crime and car robbery. Second, we recover a causal impact of firearms on homicide, confirming previous findings that firearms cause homicides. We find no systematic, interpretable impact on property crime and car robbery

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Table 1: Household Expenditure on Firearms: Comparing 2002/2003 to 2008/2009

| | Bought a Firearm? (%)[1] | | Δ(%) | ΔSpending per Person (%)[2] |
|---|---|---|---|---|
| | 2002/2003 | 2008/2009 | | |
| *Head of household* | | | | |
| Whole sample | 0.0397 | 0.0236 | -40.55 | -33.96 |
| Male | 0.0795 | 0.0437 | -45.03 | -28.05 |
| Southeast | 0.0212 | 0.0129 | -39.15 | 840.88 |
| Urban | 0.0341 | 0.0177 | -48.09 | -42.95 |
| 20 to 29 year-olds | 0.0750 | 0.0366 | -51.20 | -52.54 |

Source: *Pesquisa de Orçamento Familiar* (POF), 2002/2003 and 2008/2009, and Neri (2012). An observation is a household. Questions refer to purchases over the previous 12 months. 1: percentage of household in which someone in the household acquired a firearm. 2: spending on firearms per capita. Sampling weights used.

## Table 2: Summary Statistics

| | Mean | Std Deviation |
|---|---|---|
| Homicides (Baseline 2001-2003), main sample | 21.09 | 12.01 |
| Homicides by Firearms (Baseline 2001-2003), main sample | 19.76 | 16.48 |
| Homicides (Baseline 2001-2003), statewide | 37.66 | 103.08 |
| Homicides by Firearms (Baseline 2001-2003), statewide | 25.39 | 16.66 |
| ΔHomicides By Firearms (2004-2007), main sample | -7.33 | 10.69 |
| ΔHomicides By non-Firearms (2004-2007), main sample | -2.75 | 5.73 |
| Suicides by non-Firearms (Baseline 2001-2003), main sample | 3.48 | 1.76 |
| Suicides by Firearms (Baseline 2001-2003), main sample | 0.70 | 0.49 |
| ΔSuicides by non-Firearms (2004 - 2007), main sample | 0.05 | 2.72 |
| ΔSuicides by Firearms (2004 - 2007), main sample | -0.17 | 1.15 |
| Property Crime (Baseline 2001-2003), main sample | 2561.10 | 795.87 |
| Property Crime (Baseline 2001-2003), statewide | 2790.57 | 967.50 |
| ΔProperty Crime (2004 - 2007), main sample | -212.64 | 396.35 |
| Car Robbery (Baseline 2001-2003), main sample | 110.14 | 157.88 |
| Car Robbery (Baseline 2001-2003), statewide | 230.33 | 226.29 |
| ΔCar Robbery (Baseline 2004-2007), main sample | -10.83 | 49.98 |

Source: Secretaria de Segurança do Estado de São Paulo and Ministério da Saúde. Main sample: Unweighted averages across the 118 cities with population over 50,000 inhabitants on average during years 2001-2003. Years in parentheses: averages across these years. Statewide: all 635 cities in the state of São Paulo, weighted by population. All Δs with respect to baseline (2003). All figures per 100,000 inhabitants.

**Table 3: Dependent Variable = ΔHomicides per 100,000 inhabitants†**

| | (1) | (2) | (3) | (4) | (5)[a] | (6) | (7) | (8) | (9)[a] |
|---|---|---|---|---|---|---|---|---|---|
| | Homicides by Firearms | Homicides by Firearms | Homicides by Firearms | Homicides by non-Firearms | Homicides by Firearms | Homicides by Firearms | Homicides by Firearms | Homicides by non-Firearms | Homicides by Firearms |
| *Baseline Suicides by Firearms (per 100,000 inhabitants)* | -2.824 | -2.637 | -2.500 | 0.832 | -4.075 | | | | |
| | [1.640]* | [1.118]** | (1.090)** | [0.778] | [1.647]** | | | | |
| *Baseline Homicides (per 100,000 inhabitants)* | | -0.413 | -0.416 | -0.135 | -0.399 | | -0.409 | -0.127 | -0.393 |
| | | [0.044]*** | [0.045]*** | [0.035]*** | [0.046]*** | | [0.0420]*** | [0.034]*** | [0.042]*** |
| *100*Suicides,Firearms/Suicides, non Firearms* | | | | | | -9.510 | -5.690 | 1.020 | -11.290 |
| | | | | | | [4.190]** | [2.880]** | [1.940] | [4.500]*** |
| *Constant* | -7.330 | 8.360 | 7.930 | 1.590 | 8.560 | -11.880 | -3.660 | -1.440 | -3.120 |
| | [1.850]*** | [1.990]*** | [1.930]*** | [1.450] | [2.310]*** | [4.530]*** | [2.950] | [2.050] | [3.740] |
| $R^2$ | 0.197 | 0.554 | 0.552 | 0.197 | 0.656 | 0.147 | 0.550 | 0.182 | 0.660 |
| No of Cities | 118 | 118 | 118 | 118 | 118 | 117 | 117 | 117 | 117 |
| Observations | 472 | 472 | 472 | 472 | 472 | 468 | 468 | 468 | 468 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is homicides at time $t$ minus homicides in 2003, $t$ = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

**Table 4: Dependent Variable = ΔHomicides by Firearms per 100,000 inhabitants†**

| | (1) | (2)[a] | (3) | (4)[a] | (5) | (6)[a] |
|---|---|---|---|---|---|---|
| | Lagged Dependent Variable | | Delta-in-Logs | | Log-in-Logs | |
| *Baseline Suicides by Firearms by 100,000 inhabitants* | -1.790 | -2.843 | -2.955 | -4.012 | -0.195 | -0.196 |
| | [0.631]*** | [0.948]*** | [1.005]*** | [1.161]*** | [0.077]** | [0.063]*** |
| *Baseline Homicides per 100,000 inhabitants* | -0.211 | -0.190 | -9.100 | -10.010 | -0.325 | -0.256 |
| | [0.037]*** | [0.039]*** | [0.097]*** | [0.739]*** | [0.077]*** | [0.064]*** |
| *Lagged Dependent Variable* | 0.691 | 0.691 | | | | |
| | [0.062]*** | [0.062]*** | | | | |
| *Constant* | 1.301 | 0.285 | -113.108 | -118.709 | -5.098 | -3.747 |
| | [1.262] | [1.500] | [23.455]*** | [19.318]*** | [1.651]*** | [1.231]*** |
| $R^2$ | 0.744 | 0.796 | 0.513 | 0.645 | 0.255 | 0.390 |
| No of Cities | 118 | 118 | 105 | 105 | 102 | 102 |
| Observations | 354 | 354 | 420 | 420 | 401 | 401 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is Homicides at time t minus homicides in 2003, t = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include ΔLog(population), ΔLog(population ages 15 through 24). Baseline suicides (by any means) included in all specifications. Number of suicides not by firearms per 100,000 inhabitants at the baseline included in all specifications. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. In columns (3) through (6), all regressors are in logs. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

## Table 5: Dependent Variable = ΔProperty Crime per 100,000 inhabitants†

| | (1) | (2) | (3)[a] | (4)[b] | (5)[a,b] | (6) | (7) | (8)[a] | (9)[b] | (10)[a,b] |
|---|---|---|---|---|---|---|---|---|---|---|
| *Baseline Suicides by Firearms (per 100,000 inhabitants)* | 28.168 | 41.600 | 53.186 | -6.155 | 4.900 | | | | | |
| | [71.185] | [66.798] | [80.287] | [6.056] | [16.185] | | | | | |
| *Baseline Property (per 100,000 inhabitants)* | | -16.108 | 0.012 | -0.167 | -0.057 | | -0.076 | 0.002 | -0.164 | -0.047 |
| | | [19.148] | [0.044] | [0.020]*** | [0.032]* | | [0.054] | [0.042] | [0.022]*** | [0.034] |
| *100*Suicides,Firearms/Suicides, non Firearms* | | | | | | 60.839 | 96.057 | 176.610 | -36.126 | 57.387 |
| | | | | | | [170.099] | [168.098] | [192.082] | [60.737] | [96.063] |
| *Constant* | -396.945 | 131.292 | 5.398 | 227.060 | 136.385 | -480.135 | 94.346 | -69.298 | 203.410 | 107.966 |
| | [ 157.662]** | [ 131.761] | [ 119.217] | [48.284]*** | [83.896]* | [152.323] | [144.333] | [124.697] | [61.455] | [90.239] |
| $R^2$ | 0.070 | 0.090 | 0.162 | 0.120 | 0.112 | 0.066 | 0.088 | 0.133 | 0.133 | 0.114 |
| No of Cities | 118 | 118 | 118 | 645 | 645 | 117 | 117 | 117 | 432 | 432 |
| Observations | 472 | 472 | 472 | 2580 | 2580 | 468 | 468 | 468 | 1728 | 1728 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is property crime at time t minus property crime in 2003, t = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population. b = all cities included.

## Table 6: Dependent Variable = ΔCar Robbery Crime per 100,000 inhabitants†

| | (1) | (2) | (3)[a] | (4)[b] | (5)[a,b] | (6) | (7) | (8)[a] | (9)[b] | (10)[a,b] |
|---|---|---|---|---|---|---|---|---|---|---|
| *Baseline Suicides by Firearms* | 5.943 | 9.977 | 20.150 | 0.325 | 3.172 | | | | | |
| *(per 100,000 inhabitants)* | [7.865] | [8.122] | [13.498] | [0.353] | [2.414] | | | | | |
| *Baseline Car Robbery (per* | | -0.153 | -0.182 | -0.178 | -0.167 | | -0.155 | -0.183 | -0.149 | -0.168 |
| *100,000 inhabitants)* | | [0.076]** | [0.060]** | [0.060]*** | [0.060]*** | | [0.068]*** | [0.056]*** | [0.063]** | [0.058]*** |
| *100*Suicides,Firearms/Suicides, non Firearms* | | | | | | 24.966 | 44.822 | 70.530 | 5.064 | 25.969 |
| | | | | | | [31.179] | [35.363] | [49.541] | [4.977] | [22.550] |
| *Constant* | -37.935 | -26.342 | 47.012 | -3.341 | 36.266 | -35.178 | -0.753 | 29.428 | -9.132 | 29.013 |
| | [21.394]* | [22.415] | [21.910] | [2.845] | [17.562]** | [24.228] | [11.854] | [24.051] | [4.186] | [19.141] |
| $R^2$ | 0.038 | 0.233 | 0.443 | 0.170 | 0.397 | 0.040 | 0.251 | 0.454 | 0.170 | 0.408 |
| No of Cities | 118 | 118 | 118 | 645 | 645 | 117 | 117 | 117 | 432 | 432 |
| Observations | 472 | 472 | 472 | 2580 | 2580 | 468 | 468 | 468 | 1728 | 1728 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is car robberies at time t minus car robberies in 2003, t = 2004, 2005, 2006 and 2007.* significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population. b = all cities included.

**Table 7: Dependent Variable = ΔSuicides per 100,000 inhabitants†**

| | (1) | (2) | (3) | (4) | (5)[a] | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Suicides by Firearms | Suicides by Firearms | Suicides by Firearms | Suicides by non-firearms | Suicides by Firearms | Suicides by Firearms | Suicides by Firearms | Suicides by Firearms | Suicides by non-Firearms |
| *Baseline Suicides by Firearms (per 100,000 inhabitants)* | -1.274 | -1.278 | -0.804 | -0.066 | -1.133 | | | | |
| | [0.266]*** | [0.274]*** | [0.135]*** | [0.494] | [0.210]*** | | | | |
| *Baseline Homicides (per 100,000 inhabitants)* | | 0.001 | -0.002 | -0.011 | 0.002 | -0.002 | -0.044 | -0.005 | |
| | | [0.004] | [0.003] | [0.009] | [0.004] | [0.491] | [0.032] | [0.038] | |
| *100*Suicides,Firearms/Suicides, non Firearms* | | | | | | -2.594 | -1.462 | -2.304 | 0.495 |
| | | | | | | [0.639]*** | [0.303]*** | [0.471]*** | [0.767] |
| *Constant* | 0.372 | 0.340 | 0.259 | 1.480 | 0.216 | 1.086 | 0.557 | 0.670 | -0.013 |
| | [0.010]** | [0.264] | [0.176] | [0.617]** | [0.237] | [0.394]*** | [0.186]*** | [0.342]* | [0.009] |
| Lagged dependent variable? | No | No | Yes | No | No | No | Yes | Yes | No |
| $R^2$ | 0.248 | 0.249 | 0.403 | 0.042 | 0.221 | 0.195 | 0.378 | 0.181 | 0.391 |
| Number of Cities | 118 | 118 | 118 | 118 | 118 | 117 | 117 | 117 | 117 |
| Observations | 472 | 472 | 354 | 472 | 708 | 468 | 351 | 468 | 351 |

Source: Secretaria de Estado da Segurança Pública. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is suicides at time t minus suicides in 2003, t = 2004, 2005, 2006 and 2007.* significant at 10%; ** significant at 5%; *** significant at 1%. Δpopulation and Δpopulation ages 15 through 24 included in all speficiations. Only cities with more than 50,000 thousand included in the sample, unless otherwise noted. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

## Table 8: IV Regressions, Dependent Variable: ΔHomicides by Firearms per 100,000 inhabitants†

| | (1) | (2)[a] | (3)§ | (4) | (5)[a] | (6)§ |
|---|---|---|---|---|---|---|
| | Instrument = baseline suicides by firearms per 100,000 | | | Instrument = baseline ratio suicides by firearms/suicides not by firearms | | |
| *ΔSuicides by Firearms per 100,000 inhabitants* | 2.155 | 4.013 | -0.779 | 2.280 | 4.994 | -0.377 |
| | [1.135]* | [1.936]** | [0.655] | [1.400]* | [2.512]** | [0.765] |
| *Constant* | -4.477 | -4.805 | -0.242 | -4.801 | 7.902 | -0.779 |
| | [0.048] | [4.387] | [2.350] | [3.458] | [1.667]*** | [2.350] |
| Number of Cities | 118 | 118 | 118 | 117 | 117 | 117 |
| Observations | 472 | 472 | 472 | 468 | 468 | 468 |

Source: Secretaria de Estado da Segurança Pública. †: Dependent variable is homicides at time t minus homicides in 2003, t = 2004, 2005, 2006 and 2007. The Instrument is the baseline suicide by firearms in columns (1) through (3), and baseline suicides by firearms times a dummy for 2004 onwards in columns (4) through (6). Baseline means averages for the years 2001 through 2003. Standard errors in parentheses robust to clustering at the city level in all columns . * significant at 10%; ** significant at 5%; *** significant at 1%. Controls are the same as the 1st stage and the reduced form (including baseline homicides and baseline (all) suicides) except for the lagged dependent variable. Only cities with more than 50,000 thousand included in the sample, unless otherwise specified. All specidications include year fixed effects. a = weighted by population. §: homicides not perpetrated by firearms.

**Table 9: IV Regressions, Dependent Variable: ΔProperty Crime by Firearms per 100,000 inhabitants†**

| | (1) | (2)[a] | (4) | (5)[a] |
|---|---|---|---|---|
| | Instrument = baseline suicides by firearms per 100,000 | | Instrument = baseline ratio suicides by firearms/suicides not by firearms | |
| *ΔSuicides by Firearms per 100,000 inhabitants* | -28.803 | -42.030 | -27.299 | -77.100 |
| | [52.974] | [70.347] | [60.576] | [ 89.081] |
| *Constant* | -145.481 | -472.462 | -158.210 | -49.711 |
| | [182.630] | [159.881]** | [180.860] | [122.979] |
| Number of Cities | 118 | 118 | 117 | 117 |
| Observations | 472 | 472 | 468 | 468 |

Source: Secretaria de Estado da Segurança Pública. †: Dependent variable is property crime at time t minus property crime in 2003, t = 2004, 2005, 2006 and 2007. The instrument is the baseline suicide by firearms in columns (1) through (3), and baseline suicides by firearms times a dummy for 2004 onwards in columns (4) through (6). Baseline means averages for the years 2001 through 2003. Standard errors in parentheses robust to clustering at the city level in all columns . * significant at 10%; ** significant at 5%; *** significant at 1%. Controls are the same as the 1st stage and the reduced form (including baseline property crime and baseline (all) suicides) except for the lagged dependent variable. Only cities with more than 50,000 thousand included in the sample, unless otherwise specified. All specidications include year fixed effects. a = weighted by population.

## Table 10: IV Regressions, Dependent Variable: ΔAuto Robbery by Firearms per 100,000 inhabitants†

| | (1) | (2)[a] | (4) | (5)[a] |
|---|---|---|---|---|
| | Instrument = baseline suicides by firearms per 100,000 | | Instrument = baseline ratio suicides by firearms/suicides not by firearms | |
| *ΔSuicides by Firearms per 100,000 inhabitants* | -7.504 | -17.005 | -9.872 | -30.321 |
| | [ 6.133] | [12.176] | [10.687] | [ 22.572] |
| *Constant* | 15.383 | 50.685 | -36.926 | 41.354 |
| | [10.011] | [22.023]** | [25.627] | [20.761] |
| Number of Cities | 118 | 118 | 117 | 117 |
| Observations | 472 | 472 | 468 | 468 |

Source: Secretaria de Estado da Segurança Pública. †: Dependent variable is auto robbery at time t minus auto robbery in 2003, t = 2004, 2005, 2006 and 2007. The instrument is the baseline suicide by firearms in columns (1) through (3), and baseline suicides by firearms times a dummy for 2004 onwards in columns (4) through (6). Baseline means averages for the years 2001 through 2003. Standard errors in parentheses robust to clustering at the city level in all columns . * significant at 10%; ** significant at 5%; *** significant at 1%. Controls are the same as the 1st stage and the reduced form (including baseline auto robbery crime and baseline (all) suicides) except for the lagged dependent variable. Only cities with more than 50,000 thousand included in the sample, unless otherwise specified. All specidications include year fixed effects. a = weighted by population.

## Table 11: Dependent Variable = ΔFirearms Prevalence†

| | (1) ΔApprehension of Firearms | (2) ΔApprehension of Firearms | (3) ΔApprehension of Firearms | (4)[a] ΔApprehension of Firearms | (5) ΔApprehension of Firearms | (6) ΔSuicides by Firearms |
|---|---|---|---|---|---|---|
| *Baseline Apprehension of Firearms (per 100,000* | -0.632 | -0.610 | -0.608 | -0.497 | | -0.0048 |
| | [0.180]*** | [0.274]*** | [0.173]*** | [0.116]*** | | [0.0028]* |
| *Baseline Suicides by Firearms (per 100,000 inhabitants)* | | | | | -15.584 | |
| | | | | | [6.294]*** | |
| *Constant* | 48.120 | 48.930 | 10.410 | 37.490 | 10.210 | 0.236 |
| | [15.470]*** | [15.610]*** | [17.470] | [10.800]*** | [7.980]*** | [0.293] |
| Baseline Homicides Included? | No | Yes | Yes | Yes | Yes | Yes |
| ΔNumber of Policemen | No | No | Yes | No | No | No |
| $R^2$ | 0.433 | 0.435 | 0.436 | 0.507 | 0.204 | 0.026 |
| Number of Cities | 118 | 118 | 118 | 118 | 118 | 118 |
| Observations | 472 | 472 | 472 | 472 | 472 | 472 |

Source: Secretaria de Estado da Segurança Pública. Standard errors in parentheses robust to clustering at the city level in all columns . †: Unless otherwise noted, dependent variable is firearms apprehended at time t minus firearms apprehended in 2003, t = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. Δpopulation and Δpopulation ages 15 through 24 included in all speficiations. Only cities with more than 50,000 thousand included in the sample, unless otherwise noted. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population. b = all cities included.

## Table 12: Dependent Variable = ΔHomicides by Firearms per 100,000 inhabitants†

| | (1) | (2) | (3)[a] | (4) | (5)[a] |
|---|---|---|---|---|---|
| | Reduced-form | Reduced-form | Reduced-form | Structural form (IV) | Structural form (IV) |
| *Baseline Firearms Apprehended (per 100,000 inhabitants)* | -0.101 | -0.031 | -0.058 | | |
| | [0.029]*** | [0.019]* | [0.023]** | | |
| *ΔFirearms Apprehended per 100,000 inhabitants* | | | | 5.030 | 11.751 |
| | | | | [3.670] | [5.095]** |
| *Constant* | 6.660 | -3.000 | -1.900 | 6.737 | -1.748 |
| | [2.600]*** | [3.250] | [4.030] | [0.936]*** | [4.333] |
| Baseline Homicides Included? | No | Yes | Yes | Yes | Yes |
| $R^2$ | 0.233 | 0.549 | 0.656 | | |
| No of Cities | 118 | 118 | 118 | 118 | 118 |
| Observations | 472 | 472 | 472 | 472 | 472 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is homicides at time t minus homicides in 2003, t = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

## Table 13: Dependent Variable = ΔProperty Crime by Firearms per 100,000 inhabitants

| | (1) | (2) | (3)[a] | (4) | (5)[a] |
|---|---|---|---|---|---|
| | Reduced-form | Reduced-form | Reduced-form | Structural form (IV) | Structural form (IV) |
| *Baseline Firearms Apprehended (per 100,000 inhabitants)* | -1.951 | -1.579 | -1.138 | | |
| | [0.947]** | [0.965] | [1.001] | | |
| *ΔFirearms Apprehended per 100,000 inhabitants* | | | | 2.208 | 2.087 |
| | | | | [1.430] | [1.780] |
| *Constant* | 106.207 | -308.120 | -498.410 | -307.745 | -504.704 |
| | [94.115] | [167.909]* | [158.698]*** | [175.491]* | [160.137]*** |
| Baseline Property Crime Included? | No | Yes | Yes | Yes | Yes |
| $R^2$ | 0.096 | 0.101 | 0.158 | | |
| No of Cities | 118 | 118 | 118 | 118 | 118 |
| Observations | 472 | 472 | 472 | 472 | 472 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is property crime at time t minus property crime in 2003, t = 2004, 2005, 2006 and 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

## Table 14: Dependent Variable = ΔCar Robbery by Firearms per 100,000 inhabitants

| | (1) | (2) | (3)[a] | (4) | (5)[a] |
|---|---|---|---|---|---|
| | Reduced-form | Reduced-form | Reduced-form | Structural form (IV) | Structural form (IV) |
| *Baseline Firearms Apprehended (per 100,000 inhabitants)* | -0.237 | -0.174 | 0.022 | | |
| | [0.099]** | [0.104]* | [0.180] | | |
| *ΔFirearms Apprehended per 100,000 inhabitants* | | | | 0.274 | -0.002 |
| | | | | [0.188] | [0.337] |
| *Constant* | 15.986 | -22.566 | 40.307 | -26.924 | -60.561 |
| | [9.157]* | [19.884] | [21.008] | [20.400] | [26.901]** |
| Baseline Auto Robbery Included? | No | Yes | Yes | Yes | Yes |
| $R^2$ | 0.059 | 0.239 | 0.435 | | |
| No of Cities | 118 | 118 | 118 | 118 | 118 |
| Observations | 472 | 472 | 472 | 472 | 472 |

Source: Secretaria de Estado da Segurança Pública and Ministério da Saúde. Standard errors in parentheses robust to clustering at the city level in all columns . †: Dependent variable is car robbery at time t minus car robbery in 2003, t = 2004, 2005, 2006 and 2007.* significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include Δpopulation, Δpopulation ages 15 through 24. Baseline suicides (by any means) included in all specifications. Columns (1) through (4) include the number of suicides not by firearms per 100,000 inhabitants at the baseline. Only cities with more than 50,000 thousand included in the sample unless otherwise specified. All specidications include year fixed effects. Baseline means averages for the years 2001 through 2003. a = weighted by population.

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