Do Bans on Carrying Firearms Work for Violence Reduction? Evidence from a Department-level Ban in Colombia [1]

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This paper develops a theoretical setting that models gun interactions between workers and delinquents under a rational expectations equilibrium. We show that a gun ban increases the gun carrying costs of both workers and delinquents, decreasing unambiguously the fraction of armed delinquents while weakly lowering (not necessarily) the fraction of armed workers. We then evaluate the impact of a temporary ban on gun-carrying licenses in Colombia during December of 2009 up to February of 2010 at the department level to verify our theoretical prediction. The gun control intervention operated by extending law enforcement activities that target gun carriers across territories and periods, thus increasing gun costs for all gun carriers especially for illegal guns. Under a common trend assumption between treated and untreated departments, which is then empirically verified, we exploit regional and temporal variations of the gun ban finding a large and significant violence reduction impact, both in terms of fatal (gun homicides drop by approximately a 23% on average) as well as non-fatal gun related intentional injuries (gun injuries drop a 53% on average). Moreover, we do not find evidence of an increase in homicides and injuries with non-firearms, suggesting that the gun ban did not generate a substitution of weapons by potential attackers. Nonetheless, effects for only gun homicides seem to diminish as time passes by since the effect starts to deteriorate after 40 days after the implementation of the ban even though the enforcement of the ban did not diminish over time.

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1. INTRODUCTION

There has been an increasing recognition of the large negative impacts that violence has in the developing process of a society. Policies that intend to reduce violence have made of violence reduction strategies an emerging issue in the agenda of public security, especially in developing countries. However, very few violence reduction initiatives have been assessed using sound evaluation techniques and as a result most security-related policy making and implementation proceeds in the dark and usually guided by preconceptions or ideological bias.

Disarmament programs like bans on firearms are viewed as certain shock gun control policies with the intent of reducing severely the levels of lethality among violent confrontations in high violent societies. Around the world gun control policies are subject to strong debate since there are two sides of it. The proponents of gun control seem to believe that restricting access to firearms can reduce the level of gun violence in a society through the reduction in lethal interactions. The gun control critics seem to believe that restricting access to firearms disarms mostly law abiding citizens making them more vulnerable to attacks from delinquents. Even though there is some evidence in favor of both views no consensus seems to exist of the way gun violence could be reduced. Becker (1995) suggests two ways of reducing gun use in crimes without attacking the ownership of guns: i) "to increase the sentence significantly for people who use guns to commit crimes" and ii) to give "a little more freedom to police to frisk people whom they have a reasonable suspicion might be carrying weapons". We develop a model in which these two policies are considered. Nonetheless, the empirical exercise we present is focused on the second of these recommendations.

Importantly, within gun control type of policies, a particular and common violence reduction strategy implemented in high leveled violent environments is a temporal or even permanent disarmament policy, based on the expectation that restricted access to the most lethal tools used to inflict damage, namely guns, might have a positive impact on homicides and injuries. Again, there are very few sound assessments of the effect of small arms control programmes on violence in general and on armed violence in particular, and most of them are designed and implemented without recourse to empirical support of its effectiveness and efficacy. Gun control advocates usually point to the usefulness of small arms control and campaign for restrictions on weapons supply but rarely provide supporting arguments based on evidence. Gun related regulation in most countries does restrict supply and access but seldom considers the complications of demand-driven misuse of guns and other small arms, including, for example, potential cross-substitution effects on less lethal weapons or black-market undesired consequences. Enforcement strategies are not usually planned considering the different typologies of small arms-related violent crimes while alternative control regimes are not necessarily implemented seeking to maximize violence reduction.

This paper assesses the effects on homicides and injuries of one such disarmament program that took place over the Christmas and end-of-year festive season in 2009-2010 in Colombia. We beleive that Colombia is an ideal country to assess the effect of these type of programs since it has suffered one of the highest homicide rates in the world (between 80 to 60 homicides per 100,000 inhabitants). In November 2009, a department-level temporary ban on carrying firearms in Colombia was allowed by the national government for certain departments and days of the season which was perceived as the period in which armed violence was most prevalent. The ban temporally suspended the carrying permits and, during enforcement operations, effectively allowed the police to confiscate all type of guns, both legal (covered by permits) as well as illegal, when carried in violation of the ban. Even though the exact number of days that the ban was enforced differed among departments that ended up enforcing the ban the modal period was December 7 of 2009 to January 7 of 2010. We exploit the geographical and temporal variations of the restriction at the department level that separated those departments that ended up enforcing the ban (treated departments) from those that did not (untreated departments) to estimate the causal effect of the ban through a difference in difference estimation procedure under a common trend assumption, which is empirically verified, between treated and untreated departments. Using panel data on reported homicides and injuries for the relevant period and previous pre treatment periods, we find a practical and significant decrease in gun homicides and gun injuries in departments in which the ban on carrying guns was implemented relative to control departments, of approximately 23% for the former and 53% for the latter. These effects are driven by an increase in the confiscation of mainly illegal guns since we find a practical and statistically significant increase in these type of confiscations in treated departments relative to untreated of about 130%; moreover we fail to find an increase in the confiscation of legal firearms during the ban for treated relative to untreated departments. Furthermore, we do not find evidence of an increase in homicides and unintentional injuries with non-firearms in departments that implemented the ban relative to those that did not, suggesting that the gun ban did not generate a substitution of weapons by potential attackers, like knives for firearms. Nonetheless, the evidence suggests that the effect occurs only in the very short run (a month) while decreasing rapidly in time suggesting that delinquents seem to learn how to commit violent crimes with guns even under a general firearm ban.

2. LITERATURE REVIEW

Gun control is a highly debated policy in the world, especially in the United States, with two opposing sides. On the one hand, gun control advocates point to high levels of gun ownership as the cause of armed violence arguing that gun control inevitably reduces it by lowering the number of firearms in circulation for any given amount of violent interactions in the population. See for example Cook [1983], Cook and Ludwig [2004], Cook, Moore and Braga [2000]. Duggan [2001] provides somewhat weak evidence for the United States that gun availability increases gun violence. In this view general bans on carrying firearms would be expected to reduce overall gun violence. Villaveces et al. [2000] report the effect of an intermittent police enforced ban on carrying firearms on homicide rates for the cities of Cali and Bogota from Colombia during 1993 and 1997 finding a significant drop of 14% in these rates for both cities. Sherman et al. [1995] report that in 1994 a police department special unit targeting illegal gun carrying in a high-rate firearm-violence neighborhood in Kansas (United States) was able to reduce it in 49% relative to non intervened neighborhoods. On the other hand, anti control advocates take a demand-driven approach, arguing that high levels of gun ownership in a population are a response to, not a cause of, violent crimes, and that gun control operating as supply restrictions to buy and carry a gun in normal times tend to be applied mostly to law-abiding citizens, not to criminals (See Kleck and Gertz [1995], Polsby and Brennen[1995], Lott [1998, 2001] and Bartley [1999]). For empirical evidence supporting this position Lott and Mustard [1997] find that for states in the American Union that passed laws allowing concealed hand guns to circulate (shall issue laws) there was a reduction in certain type of crimes relative to those states that did not. Nonetheless, with this same data a more mixed evidence scenario is shown in Dezhbakhsh and Rubin [1998] and Rubin and Dezhbakhsh [2003]. See also Ayres and Donohue (2003) for an interesting critique to the anti gun control empirical evidence.

There are some theoretical models in the literature that model interactions between law-abiding citizens and criminals that use guns but have done so mainly to rationalize the self defense argument. See for example Taylor (1995), Ghatak (2001) and Mialon and Wise (2005). For models that rationalize gun control policies see Chaudhri and Geanakoplos (1998), Aguiar de Oliveira (2007) and Villa (2007). Our conceptual framework does contribute to the literature in that it is able to model gun interactions between workers and delinquents and assess the theoretical effects of a general gun ban.

Becker (1995) himself has argued that gun control is a divisive issue for the United States more than for other countries but has recognized that "guns add to the likelihood of more serious crimes". Nonetheless he argues that for the United States, with over more than 70 million guns in hands of the public, a gun control policy of restricting ownership seems unfeasible since "[m]ost of the guns in the hands of the public are obtained illegally, not by going to a registered gun dealer and through the waiting period". Becker then goes on to point out two ways of reducing gun use in crimes without attacking the ownership of guns:

i) to increase the sentence significantly for people who use guns to commit crimes and ii) to give "a little more freedom to police to frisk people whom they have a reasonable suspicion might be carrying weapons". In other words, the first point emphasizes a deterrence mechanism of increasing sentences if a gun is used in a felony, which relies explicitly on rational expectations from offenders, while the second point emphasizes an incapacitation mechanism that seeks to prevent attacks from gun offenders and which requires law enforcement authorities to confiscate illegal guns, which are usually guns in the wrong hands.

The situation in Colombia is not that different from that of the United States. Actually there is a more stringent and restrictive regulation of firearms in Colombia than in the United States with the state maintaining the formal ownership of all firearms by constitutional disposition and only granting holding and carrying permits to those formally demonstrating security requirements and self-protection needs. But as Becker points out for the United States illegal ownership of firearms in Colombia is proportionally higher since it constitutes the prevalent way that citizens (specially delinquents) obtain guns. Since violent indicators for Colombia are so high we believe that this country is ideal to study and assess whether a temporal gun ban could reduce these high levels of violence. In this sense our paper contributes to the literature in studying at the national level the effects that a temporary gun ban could have on the reduction of violence measured as homicides and intentional injuries with and without firearms. Our results go in line with the mechanism of mainly increasing the confiscation of illegal guns as Becker (1995) suggests and are consistent with those of Stolzenberg and D'Alessio (2000) who find for pool cross section *county-level* data, drawn for South Carolina (United States), a strong positive relationship between illegal gun availability and violent crime, gun crime and juvenile gun crime while little or no effect for the legitimate gun availability measure they use in any of their estimated models. Moreover, we also find as they do that offenders seem not to be substituting knives or other cutting instruments when illegal firearms become less available. Nonetheless, this study which is only at the county level relies on observational data that does not come from a policy evaluation exercise which limits the causal interpretation of their results.

Despite the value of these works, most of the findings in the literature studying impacts of gun control and gun supply restrictions face several shortcomings, mainly due to data availability and the same nature of these policy restrictions. Indeed, most impact evaluation exercises consider only locally-based interventions and thus cannot assess the mobility of violent crime and the transportability of guns. In other cases, no account of the actual degree of enforcement is included into the empirical models and the identification of causal effects is severely limited. Other studies face issues related with the interventions itself, which make difficult to isolate the effect of the intervention from other simultaneous efforts, or to pinpoint the direction of causality. We aim to tackle these issues thanks to the availability of high-frequency data on violent crimes and by exploiting the design and implementation of a gun ban intervention in Colombia, which we refer in detail below.

3. CONCEPTUAL FRAMEWORK

The following framework shows a simple model that is used to interpret our empirical findings. In this sense the model is quite abstract and illustrates some of the main incentives at work when implementing a general ban on firearms. Let there be a continuum of risk neutral individuals with mass size L where each has one unit of time and individuals utility is generated only by consumption, denoted by C, of the only good in the economy. Individuals do not inherit any endowment for the period and can be either *workers* or *non-workers*. On the one hand, *workers* can choose either to carry a gun or not for self defense from delinquents, where their gun carrying costs are represented by $g^W > 0$ and include all type of legal regulations that generate carrying costs for them but can also include permits, ammunition costs, sales taxes etc. On the other hand non-workers engage in illegal activities since there is no other occupation that would yield a source of income for them. Hence, in this model non-workers are necessarily entrepreneurs of illegal activities or delinquents.[2] Delinquents can either choose to carry a gun or another weapon (say a knife, fake guns etc) to prey on workers since we assume that without a credible threat delinquents would not be able to subdue their victims and achieve any income at all.

Assume random matching in a given period in a pair-wise fashion between all individuals. Nonetheless only matches between workers and delinquents redistribute income through violent means and therefore these are the matches that are relevant for expected payoffs.[3] In these matches assume that unsuccessful workers (unarmed and those that even though are armed do not get the upper hand in the match with probability of a half) in any given encounter with a delinquent lose their income and risk a gun injury of 3γ when the delinquent uses a gun, otherwise they risk injury γ if the delinquent uses another weapon, say a knife.[4] In this sense we assume that guns are three times more lethal than other weapons

[2] Naturally in the real world non-workers can engage in legal activities since they can become legal entrepreneurs and run their business'. Nontheless the simplification is to make the model as simple as possible.

[3] Naturally one can think that matches between delinquents or even between workers might have, with a certain probability, a violent interaction. Disputes between workers could arise but they would not involve income redistribution and therefore we abstract from these matches. Moreover, only matches between delinquents could be thought to have eventually a redistribution of income since it could be argued that a delinquent is also robbed by another deliquent where the former had already stolen some income. Even though this is a real possibility we assume implicitly that delinquents do not prey on each other. A simple reason could be that they might be able to recognize each other easily in a given match and therefore they do not interact violently.

[4] Cook and Ludwig (2000) argue that "knife assaults are about one third as likely to result in the victim's death as those assaults that involve guns and on average have less serious consequences for survivors as

and that this is common knowledge among delinquents and workers. Successful armed workers that get the upper hand in any given match with a delinquent (with probability of a half) save their income, do not get injured and potentially inflict gun injuries to a delinquent. We assume that armed workers fend off an attack of delinquents that use other weapons different than guns. The carrying costs of a gun for a delinquent are represented by g^D while those for carrying another weapon are represented by a^D where we assume $g^D > a^D$. Delinquents either armed with a gun or another weapon are assumed to be always successful in acquiring income in a match with an unarmed worker, apprehending fraction ρ of the worker's income. Now when encountering a gun armed worker a delinquent has probability only of a half of being successful and not facing an injury while with the other half of probability the delinquent is unsuccessful and risks a gun injury of 3γ. We assume furthermore that law enforcement authorities apprehend and convict delinquents with probability π but the penalties vary according to the type of weapon that the delinquent used in the assault: penalty Γ_g when a gun was used or penalty Γ_a when another weapon was used where $\Gamma_g \geq \Gamma_a$ as Becker (1995) suggests. Finally, the only heterogeneity assumed is that individuals (both workers and delinquents) differ in their *perceptions* of *lethality* that comes with interactions with other individuals with weapons and is distributed according to a strictly increasing cdf Ψ in the population on $(0, \infty)$ such that $\int_0^\infty d\Psi = L$. Let α^e stand for the perceived probability (or belief) that a delinquent has of encountering a worker with a gun in a given match while β^e stands for the perceived probability that a worker has of encountering a delinquent with a gun in any given match. We assume that individuals have rational expectations about these probabilities in equilibrium which means that their perceived probabilities coincide with the objective probabilities determined in equilibrium.

We assume that firms do not know the delinquent history of a potential worker, hire potential workers under a minimum wage floor $\underline{w} > 0$ perfectly enforced by the government. This minium wage comes as an institutional restriction for the firm which makes unfeasible wages in equilibrium less than $\underline{w}$.Workers are hired by a competitive representative price taker firm (assume there exist n of these) at wage w such that it maximizes profits given by $\pi = f(l)\,\overline{K} - wl - \overline{r}\overline{K}$ where $f(l)\,\overline{K}$ is a production function in terms of the mass of laborers needed[5] l and w is wage. We assume that f has diminishing marginal returns in l so that $f' > 0$ and $f'' < 0$. A representative firm maximizing profits with respect to l yields the following first order condition $f' - w = 0$ where a labor demand function comes out as $L^d = f'^{-1}\left(w; \overline{K}\right)$ such that $\frac{\partial L^d}{\partial w} < 0$ because of $f'' < 0$ and $\frac{\partial L^d}{\partial K} > 0$. For n given firms the aggregate amount of laborers demanded is simply nL^d. This motivates the following

well" pg 42.

[5] We interpret l as the number of individuals employed since each individual is endowed with a unit of time which implies that when aggregating time across individuals yields the amount of potential workers in the economy.

definition.

Definition 1. *An equilibrium with involuntary unemployment (EIU)* is a price vector $w^* > 0$ with allocation $(L^*, C^*) \gg (0,0)$ such that all individuals (workers and non-workers) consume there disposable expected income, firms maximize profits and there is a rationing scheme that does not allow aggregate excess demands to clear.

The following proposition is straightforward.

Proposition 1. *Assume there is a perfectly enforced minimum wage binding for firms and that $L > nf'^{-1}\left(w; \overline{K}\right)$ then there exists a unique EIU such that $w^* = \underline{w}$.*

Proof. Since it has been assumed that no endowment is inherited by any individual in the economy and firms cannot observe if an individual was apprehended or even convicted of a criminal felony then all individuals must offer themselves to work in the legal sector so labor supply is L. Moreover, we have shown above that aggregate demand is nL^d. Hence the aggregate excess demand of labor is defined as $Z_l = nl\left(w\right) - L$ which is a strictly decreasing function in w. The expected aggregate excess demand for the good is $Z_C = E\left[C\right] - nf\left(nL^d\right)$ where $E\left[C\right]$ is the expected aggregate demand of the good by workers and non-workers where uncertainty arises from the uncertain redistribution that is generated by violent interactions between workers and delinquents. Under $L > nf'^{-1}\left(w; \overline{K}\right)$ and since $Z_l = nl\left(w\right) - L$ is a strictly decreasing function in w.since we have that the wage that makes $Z_l = 0$ would be a wage such that $w^* < \underline{w}$ which is unfeasible by institutional constraints. Since there is a perfectly enforced minimum wage binding for firms the equilibrium wage is $w^* = \underline{w}$ and there is an excess of labor so that L^* is determined by demand $L^* = nf'^{-1}\left(\underline{w}; \overline{K}\right) > 0$. Uniqueness follows from the strict decreasing behavior of Z_l in w. Walras law here is $Z_C + wZ_l = 0$ since utility of individuals is monotone in the consumption good. Hence at the EIU we then must have a positive expected aggregate excess demand for the good $Z_C > 0$.[6] ∎

The intuition is that under $L > nf'^{-1}\left(w; \overline{K}\right)$ and a minimum wage policy perfectly enforced the equilibrium has involuntary unemployment and the mass of unemployed individuals become necessarily predators given the lack of any other source of occupation for this population. In this model all individuals have the same skills, there is no criminal history that firms can observe of an individual, and since we assume away nepotism the probability of becoming a *worker* in the EIU is $p \equiv \frac{nl(w)}{L} \in (0,1)$.

[6] Note that EIU is similar in spirit to a fixprice-fixwage equilibrium with classical unemployment as shown by Bénassy (2002) in chapter 2, since classical unemployment as in EIU has both excess supply of labor and excess demand of the good.

3.1. Optimal Decisions in Gun Matches

In an EIU with $p \in (0,1)$ workers earn wage $\underline{w}$ and choose to carry a gun if and only $E\left[C_g^W\right] \geq E\left[C_{ng}^W\right]$ which simply says that a gun is chosen to be carried if the expected consumption under this choice (C_g^W) is no less than the expected consumption under not carrying one (C_{ng}^W). Recall that individuals do not obtain utility of holding a weapon (for both workers and delinquents) so in this sense the demand for carrying one is simply an instrumental motive and if carried its costo is substracted from the corresponding expected disposable income. Let us compute these expected consumptions under the assumptions stated above where a worker has a rational belief β of encountering a delinquent with a gun while with belief $(1-\beta)$ she encounters a delinquent with another weapon (say a knife)

$$\beta\left(0.5\underline{w} + 0.5\left[(1-\rho)\underline{w} - 3\gamma\right]\right) + (1-\beta)\underline{w} - g^W \geq$$

$$\beta\left((1-\rho)\underline{w} - 3\gamma\right) + (1-\beta)\left((1-\rho)\underline{w} - \gamma\right)$$

Rearranging we get

$$\gamma \geq \max\left\{0, \frac{g^W - \rho\underline{w}\left(1-\frac{\beta}{2}\right)}{1+\frac{\beta}{2}}\right\} \equiv \widehat{\gamma}\left(\beta\right). \tag{1}$$

This says that all workers with a perception of lethality higher or equal than threshold value $\widehat{\gamma}(\beta)$ would choose to carry a gun. Note that threshold $\widehat{\gamma}(\beta)$ is weakly increasing in β if the fraction ρ that can be stolen is high enough, i.e. $\rho \in \left(\frac{g^W}{2\underline{w}}, 1\right)$ which is a non empty set under the assumption $\frac{g^W}{2\underline{w}} < 1$. Rational expectations then implies that the belief a delinquent has of encountering an armed worker with a gun should satisfy[7]

$$\alpha^e = p\frac{\int_{\hat{\gamma}(\beta)}^{\infty} d\Psi}{L}. \tag{2}$$

A delinquent on the other hand would choose to carry a gun relative to carrying another weapon like a knife if and only if $C_g^D \geq C_a^D$ which comes down to comparing the expected consumption levels in each case. Let us compute these expected consumptions where with belief α a delinquent faces an armed worker and again with probability a half he gets the upper hand in the match

$$(1-\pi)\left[\alpha\left(0.5\rho\underline{w} + 0.5\left(-3\gamma\right)\right) + (1-\alpha)\rho\underline{w}\right] - \pi\Gamma_g - g^D \geq$$

[7] The outcome of being a worker (W) is a binary random variable, since all workers are exante the same, which then takes the value one with probability $p \equiv \frac{nl(\underline{w})}{L}$ and zero with probability $1-p$. This random variable is assumed independent of the random variable of being armed (G) with probability $\frac{L-\Psi(\hat{\gamma}(\beta))}{L}$. Hence the rational expectations belief α is the joint probability of facing a worker (W) that carries a gun (G)

$$\alpha^e \equiv \Pr(W)\Pr(G) = p * \frac{L - \Psi\left(\hat{\gamma}\left(\beta\right)\right)}{L}.$$

$$(1-\pi)\left[\alpha(-3\gamma)+(1-\alpha)\rho\underline{w}\right]-\pi\Gamma_k-a^D$$

Again rearranging yields

$$\gamma \geq \max\left\{0, \frac{2}{3}\left(\frac{g^D-a^D+\pi\left(\Gamma_g-\Gamma_a\right)}{\alpha(1-\pi)}-\frac{\rho\underline{w}}{2}\right)\right\} \equiv \tilde{\gamma}(\alpha). \tag{3}$$

This says that all unemployed individuals with a perception of lethality higher or equal than threshold value $\tilde{\gamma}(\alpha)$ would choose to prey on workers with a gun instead of another weapon. Note here that threshold $\tilde{\gamma}(\alpha)$ is a weakly decreasing function of α under $g^D+\pi\left(\Gamma_g-\Gamma_a\right)>a^D$. Again the rational expectations belief for a worker of encountering a delinquent with a gun should satisfy

$$\beta^e=(1-p)\frac{\int_{\tilde{\gamma}(\alpha)}^{\infty} d\Psi}{L}. \tag{4}$$

3.2. Armed Rational Expectations Equilibrium

We need to define an armed rational expectations equilibrium (AREE) in order to prove its existence and uniqueness.

Definition 2. An AREE is defined as a situation in which: i) there is an equilibrium with involuntarily unemployment (EIU) such that $w^*=\underline{w}$, fraction $\frac{nl(\underline{w})}{L}>0$ are employed workers while fraction $\frac{L-nl(\underline{w})}{L}>0$ are involuntarily unemployed individuals which become delinquents; ii) workers choose optimally to carry a gun or not while delinquents choose optimally to prey on workers using a gun or another less lethal weapon; finally iii) rational beliefs must satisfy conditions (2) and (4) simultaneously.

The following proposition shows that a rational expectations equilibrium exists and is unique.

Proposition 2. *Under the assumptions above there exists a unique AREE such that* $(\beta,\alpha)\in[0,1]^2$.

Proof. The argument is simple since equilibrium in rational expectations must occur in the space $[0,1]^2$ where both equations (2) and (4) intersect given that the other conditions of the definition of a rational expectations equilibrium are satisfied by proposition 1. Consider equation (2) when $\widehat{\gamma}(\beta)>0$. Differentiate with respect to β which yields

$$\frac{\partial\alpha^e}{\partial\beta}=-\left(\frac{p\Psi'\left(\hat{\gamma}(\beta)\right)}{L}\right)\left(\frac{\partial\hat{\gamma}(\beta)}{\partial\beta}\right)\leq 0 \tag{5}$$

since $\Psi' > 0$ is the density of Ψ which is always positive and $\frac{\partial \hat{\gamma}(\beta)}{\partial \beta} \geq 0$ under assumption $\rho \in \left(\frac{g^W}{2\underline{w}}, 1\right) \neq \varnothing$. Now in case $\widehat{\gamma}(\beta) = 0$ then $\alpha^e = p$ which means that $\frac{\partial \alpha^e}{\partial \beta} = 0$. We conclude then equation (2) for $\widehat{\gamma}(\beta) \geq 0$ generates a weakly negative relation between α^e and β. Consider now equation (4) if $\tilde{\gamma}(\alpha) > 0$ i.e. when $\frac{g^D - a^D + \pi(\Gamma_g - \Gamma_a)}{\alpha(1-\pi)} - \frac{\rho w}{2} > 0$ which requires necessarily $g^D + \pi(\Gamma_g - \Gamma_a) > a^D$ to be satisfied and which is by assumption given that $g^D > a^D$ and $\Gamma_g \geq \Gamma_a$. Differentiating now with respect to α yields

$$\frac{\partial \beta^e}{\partial \alpha} = -\left(\frac{(1-p)\Psi'(\tilde{\gamma}(\alpha))}{L}\right)\left(\frac{\partial \tilde{\gamma}(\alpha)}{\partial \alpha}\right) \geq 0 \tag{6}$$

since again $\Psi' > 0$ and $\frac{\partial \tilde{\gamma}(\alpha)}{\partial \alpha} \leq 0$. In case $\tilde{\gamma}(\alpha) = 0$ then $\beta^e = 1 - p$. We conclude that in space $[0,1]^2$ equation (4) for $\tilde{\gamma}(\alpha) \geq 0$ generates a weakly positive relation between α and β^e. Hence if the equilibrium exists at all then it must be unique given that equations (5) and (6) have opposite slopes respectively in the space $[0,1]^2$. To prove existence under rational expectations we must have that (2) and (4) must be satisfied simultaneously such that $\beta^e = \beta$ and $\alpha^e = \alpha$. Take equation (4) when $\tilde{\gamma}(\alpha) > 0$ which rearranged such that α is left as an explicit function of β yields

$$\alpha = \frac{g^D - a^D + \pi(\Gamma_g - \Gamma_a)}{1.5(1-\pi)\left(\frac{\rho w}{3} + \Psi^{-1}\left(L - \frac{\beta L}{1-p}\right)\right)}. \tag{7}$$

A sufficient condition then for existence of an equilibrium is that equation (7) takes a value of alpha lower than the alpha value that equation (2) takes when $\beta = 0$ since this guarantees that the former intersects the latter from below. Evaluate equation (2) at $\beta = 0$ which yields $p\frac{L - \Psi(g^W - \rho w)}{L}$. Hence, the corresponding alpha takes the value $\alpha_1^0 = p\frac{L - \Psi(g^W - \rho w)}{L} \geq 0$. Now evaluate equation (7) at $\beta = 0$ which yields $\frac{g^D - a^D + \pi(\Gamma_g - \Gamma_a)}{1.5(1-\pi)\left(\frac{\rho w}{3} + \Psi^{-1}(L)\right)} = \frac{g^D - a^D + \pi(\Gamma_g - \Gamma_a)}{1.5(1-\pi)\left[\frac{\rho w}{3} + \Psi^{-1}(\Psi(\infty) - \Psi(0))\right]} = 0$ since $L = \Psi(\infty) - \Psi(0) = \Psi(\infty)$ given that $\Psi(0) = 0$. The corresponding alpha value is $\alpha_2^0 = 0$. Since $\alpha_1^0 \geq \alpha_2^0$ the intersection must occur necessarily under the conclusion about the slopes above of equations (2) and (4). Hence there exists a rational expectations equilibrium $(\beta, \alpha) \in [0,1]^2$. ∎

Figure 1 illustrates the rational expectations equilibrium under a parametric specification such that $L = 1$, $\Psi(x) = 1 - \exp(-10x)$, $p = 0.5$, $w = 1$, $\rho = 0.22$, $\pi = 0.3$, $g^W = 0.2$ and $g^D - a^D = \Gamma_g - \Gamma_a = 0.05$. Note that the vertical and horizontal parts of the corresponding curves in Figure 1 occur when $\beta = 1 - p$ and $\alpha = p$ respectively.

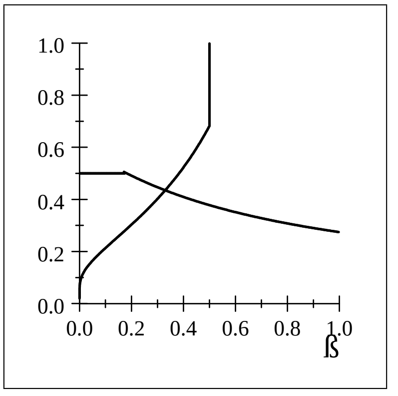

Figure 1

3.3. Comparative Statics

The model developed has several comparative statics than one might want to study. Consider the case of increasing the sentence a delinquent faces if he is apprehended using a gun i.e. increasing Γ_g. This increases (weakly) the threshold value $\tilde{\gamma}(\alpha)$ and therefore the upward sloping curve that comes out of equation (4) is shifted upwards as the dashed curve in Figure 2a illustrates for $\Gamma_g - \Gamma_a = 0.1$ with the rest of parameters as in Figure 1 unchanged. This upward shift occurs since in equation (4) for the same value of α the corresponding β^e is reduced. Hence, the equilibrium fraction β is reduced which means that delinquents substitute guns for less lethal weapons in response to the increase in Γ_g. Moreover, the equilibrium fraction α increases meaning that armed workers increase. The model predicts the point emphasized by Becker (1995), namely that increasing sentences for delinquents that use guns could deter some in the margin of committing these type of attacks.

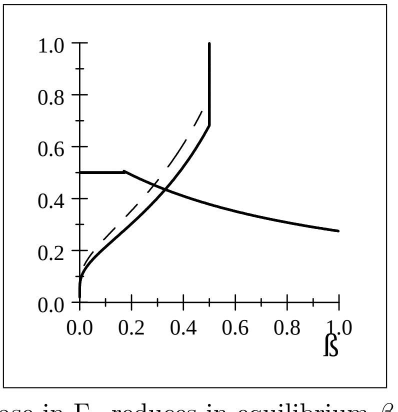

Figure 2a: An increase in Γ_g reduces in equilibrium β while increasing α.

For our empirical exercise the comparative static in which we are mainly interested is that of a change in the gun carrying costs for workers g^W and for delinquents g^D. Consider increasing g^W in equation (1). This change makes threshold value $\widehat{\gamma}(\beta)$ increase (weakly) and therefore curve (2) shifts downward as shown in Figure 2b with a dashed line (g^W increases from 0.2 as in Figure 1 to 0.215). As seen in Figure 2b this change reduces both equilibrium fractions (β, α).

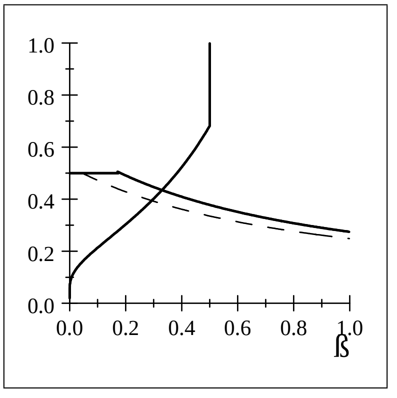

Figure 2b: An increase in g^W reduces both equilibrium fractions β and α

The intuition behind this result is that higher gun carrying costs for workers deters some workers at the margin of carrying a gun and therefore it is associated with a lower amount of armed workers in the population. Hence it is more profitable for delinquents to substitute guns by using other weapons since they are less costly to carry and because the lower fraction of armed workers entails a higher success of delinquents with these other weapons.

Consider now increasing the gun carrying costs for delinquents g^D in equation (3). This change increases threshold value $\tilde{\gamma}(\alpha)$ and shifts upward the positively sloped curve that corresponds to equation (4) as shown in Figure 2c (g^D increases from 0.05 as in Figure 1 to 0.075 in the parametric benchmark). Hence this change reduces β but increases α. The effect is similar than an increase in Γ_g as was shown above.

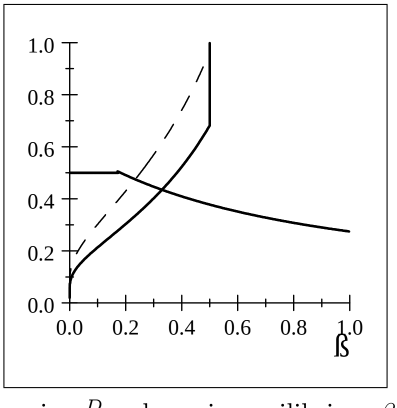

Figure 2c: An increase in g^D reduces in equilibrium β while increasing α.

The intuition is that higher gun carrying costs for delinquents makes them substitute guns for other less lethal weapons necessarily at the margin which makes workers more successful in the interactions with them increasing the fraction of armed workers.

Another policy which requires less information about identification of delinquents and workers is that of a general ban on firearms which could be thought as a policy that increases both g^W and g^D simultaneously. This type of policy could be enacted due to the difficulty in policy terms of increasing only the gun carrying costs for delinquents without affecting those for workers. In this case the fraction of delinquents with guns β decreases unambiguously as shown above in the two previous cases while the fraction of armed workers α could be reduced but not necessarily as illustrated in Figure 2d with the parametric specification of Figure 1 (g^W increasing from 0.2 to 0.215 and g^D from 0.05 to 0.075). The reason that workers might not necessarily be disarmed by this policy is the following: as shown above higher gun carrying costs for delinquents makes them substitute guns for other weapons necessarily at the margin which makes workers more successful in the interactions with them increasing the fraction of these but since the gun carrying costs for workers also increases with the ban then some workers willingly disarm themselves at the margin. The net effect is ambiguous for this population since the first effect might dominate the second one or vice versa. This is summarized in the following proposition.

Proposition 3. *A ban on firearms that increases the gun carrying costs of both workers and delinquents decreases unambiguously the fraction of armed delinquents while weakly lowering (not necessarily) the fraction of armed workers.*

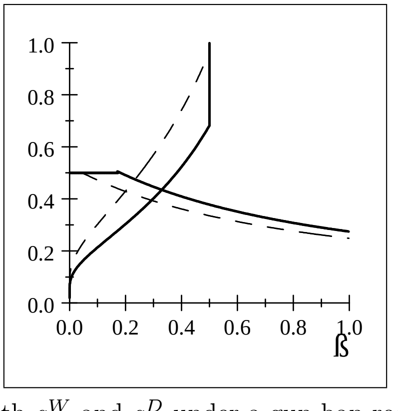

Figure 2d: An increase in both g^W and g^D under a gun ban reduces in equilibrium β while not necessarily α.

Moreover, the lethality of gun interactions is reduced under a ban on firearms. To see this let us define the conditional expectation of a lethal outcome between an armed worker and an armed delinquent as

$$E\left[lethal\right| \text{ armed worker, gun armed delinquent}] = \frac{3\gamma\alpha\beta}{2} \tag{8}$$

since with a probability of a half in any given random match between a gun armed worker and a delinquent there is a lethal outcome for either of them. Hence, a ban on firearms would decrease unambiguously β while only weakly α which in turn would decrease the expected value in equation (8). This is summarized as another proposition.

PROPOSITION 4. *A ban on firearms lowers the conditional expectation of a lethal outcome between an armed worker and an armed delinquent.*

Importantly since we only end up observing empirically if a confiscated firearm is legal or not we need to relate this issue with our conceptual framework without necessarily modelling the corresponding gun markets. If delinquents get their guns in second hand illegal markets while workers buy them legally a ban on firearms presumably would increase relatively less the gun carrying costs for legal carriers (workers in the model) than for illegal gun carriers (delinquents in the model). This is because the policy of confiscating guns for a certain amount of time (short period of a month say) has to give back necessarily and eventually the confiscated legal guns to their carriers while retaining the illegal ones for sure. This increases more the costs for delinquents (given the initial value of the parameters) than for workers. Hence the transmission mechanism through which the policy of banning firearms for a short period of time could reduce gun injuries is the following: some delinquents choose at the margin not to carry a gun since carrying costs are increased for them under the ban, moreover they expect more workers to be unarmed increasing the success of other weapons which reduces delinquent attacks with guns lowering gun injuries (fatal and non-fatal) and eventually raising the number of injuries (fatal and non-fatal) with other weapons. This mechanism is a way of rationalizing the following conjecture.

CONJECTURE 1. *A ban on firearms increases the carrying costs for illegal and legal carriers generating a drop in the amount of gun injuries (fatal and non-fatal) in matches between workers and delinquents that are armed with firearms.*

3.4. More Guns Less Crime?

The simple model developed above can address the case advocated by anti gun control proponents that have followed Lott (1998). According to Lott (1998) if more workers (law abiding citizens in his case) are armed then less delinquency with firearms arises. He actually coined a motto for this policy view in the title of his book "More Guns Less Crime". To see this in our model note that since delinquents are involuntary unemployed individuals an increase in the fraction of workers is simply an increase in p either due to more firms n that would hire more workers or a higher level of capital $\bar{K}$ used by firms all of which induce more employment in a given moment. All these end up increasing p making α increase while decreasing one-for-one β. Importantly it is the increase in the absorption of workers that generates this effect and not the decrease in gun carrying costs for workers. Actually in our model a decrease in the gun carrying costs for workers generates an upward displacement in equation (2), generating the opposite effect of figure 2b, while not shifting equation (4), all of which generates an increase in both equilibrium fractions (β, α) and hence an increase on expected lethality in gun matches. Hence, in our model a policy that makes less costly to carry a gun for workers actually would end up increasing the lethality of the delinquent-worker matches since more of both are armed in the AREE. This reveals an arms chase scenario in our model.|

In what follows our main *conjecture* is meant to be tested empirically, which is what we turn to show now for the case of Colombia under the temporal policy of a ban on firearms.

4. THE SELECTIVE DEPARTMENT LEVEL BAN ON CARRYING FIREARMS IN COLOMBIA

Colombia remains one of the most violent countries in the world, showing a decreasing but very high homicide rate of close to 31 rate per 100,000 inhabitants for 2009, much higher than the average word homicide rate of 7 rate per 100,000 [Geneva Declaration, 2008, Aguirre and Restrepo, 2004: 4]. The country also shows a variety of violence indicators with high incidence in the population including intentional injuries, internally forced displacement and kidnapping [Granada et a, 2009]. Yet, as in many other cases, violence is not distributed homogeneously throughout the country: the Gini coefficient of distribution of homicides (with respect to population at municipality level) reached 0.74 in 2009, with armed violence showing also large variations across cities [Aguirre et. al., 2010].

In contrast, the country exhibits a stringent and restrictive regulation of firearms, with the state maintaining the formal ownership of all firearms by constitutional disposition, and only granting holding and carrying permits to those formally demonstrating security requirements and self-protection needs. This has lead to a separation of the legal and illegal demand for firearms and to a relatively permanent enforcement of the regime. Furthermore, the control authority and enforcement of such a regime rests in national (central government controlled) authorities (Small Arms Survey, 2006).

The rapid increase in homicidal violence the country has experienced since the early eighties coincided with the first election of local mayors by popular vote in 1986 and its peak with the first election of departmental governors. Since then, local authorities have lobbied the central government for the devolution of powers in order to restrict gun-carrying permits, despite the almost total absence of information on the involvement of legally licensed firearms on crimes. The central state and the military forces in particular, have opposed permanent bans and have only granted temporary bans, which have had national coverage, but during particular dates (like election days). Still, the current regulation allows for a local authority to request to the military commander of the region a temporary suspension of gun licenses.

Only in a few large cities the national authorities have granted extended restrictions, and have done so only on a temporary and intermittent basis, mostly during weekends. These have been the cases of the restrictions imposed during the terms of Rodrigo Guerrero in Cali and of Antanas Mockus in Bogotá. More recently, the cities of Medellín and Bogotá have requested and managed to obtain such intermittent bans and, arguing the need to stop mobility of guns and criminals, and have slowly started to incorporate other neighboring municipalities (the case of Medellín was the first conurbation area of the Aburrá Valley) and, without recourse to evidence, have requested and obtained an extension of the ban to longer in-week periods. Yet, devolution of powers in terms of gun licensing or in terms of permanent or temporary restrictions has proved elusive, despite voiced requests by groups of mayors and governors [Aguirre and Restrepo, 2010].

By November of 2009, and after voiced requests of the Association of Governors, the national government yielded to pressure and allowed for a general gun ban to go ahead. Specifically, the ban was designed to be implemented in all departments from December 7 of 2009 up to January 7 of 2010, the main period of holiday festivities and celebrations in the country and effectively suspended the concessionaire carrying permits for all civilians. The stated overall objective was to reduce violence in general and specifically gun violence related to homicides. And although the ban was backed up directly by the executive power -in particular by the Vice President, the National Police Director and the General Commander of the Armed Forces- there was no consensus inside the government as it was seen by some high officials (including the Vice President) as a test of the goodness of the intervention.

Figure 3a

Interestingly, after the authorization by the government was announced several local authorities voiced their opposition to the ban. The formula adopted by the national government was to accommodate such disparity in policy criteria by allowing the restriction to go ahead only in those departments and large cities in which the local authority was supporting the intervention, effectively allowing governors to decide whether to implement it or not and even to modify the number of days the gun restriction would apply. The actual decision of implementing the ban in a department therefore was made by the respective incumbent governor. *Figure 3a* illustrates the map of Colombia showing the departments that implemented the ban on firearms (called treated departments and which were 18 in total without including Bogota as a separate entity which forms part of the department of Cundinamarca) and those that did not (called untreated or control departments that were 14 in total). *Figure 3b* shows the homicide rates for the departments which illustrates the high levels that some of the departments had at the time of the decision of implementing the ban on firearms.

The unilateral decision by governors of whether or not to implement the ban generates identifying problems of the causal effect of the intervention. Even so we show evidence against the idea that governor's decisions were influenced by any partisan policy or ideological orientation and therefore the decision could be understood as an idiosyncratic decision correlated only with the level of homicides in the previous 12 months in the department. We then argue that a double difference estimator under common trends among treated and control departments allow us to recover a causal interpretation. Before this we present the data that we use for the empirical exercise.

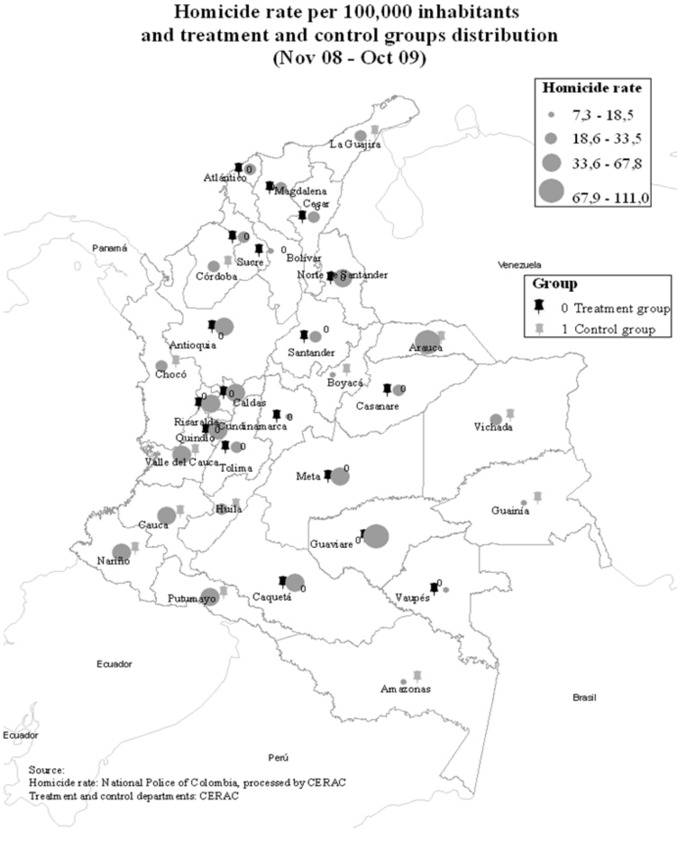

Figure 3b

5. DATA

The data used in this paper comes directly from the National Police Department which provided us with all the information on both homicides and injuries with and without guns at the department level. *Tables 1a*, *1b* and *1c* present summary statistics of the ban period for the treated departments, population of each department, population density per square kilometer, average homicides and injuries with firearms respectively reported for the ban period and for similar periods averaged out between 2003 and 2008; finally it reports the average confiscated firearms both legal and illegal used in the empirical analysis. *Table 1a* shows that treated departments did not implement the ban the same number of days: while departments like Casanare, Guaviare and Vaupés implemented the ban for 16 days Bolivar and Sucre did so for 93 days. The table also shows the exact dates of the ban for treated departments showing that the timing was not exactly the same for all since some treated departments started the ban during November of 2009 while others started it during December of 2009. This time variation in the implementation actually allows us to study if the difference in days had any influence on the marginal effect of the ban among treated departments. *Table 1a* also shows statistics for the control departments during the generic treatment period December 7 of 2009 up to January 7 of 2010. The table shows the population densities of the departments of Colombia, with an average of 247.2 per square kilometer, which varies widely across departments.

Table 1a. Statistics: Ban Period and Population

	Department	Days under ban	Period under Ban (day/month/year)	Population	Population Density (Population per square kilometer)
Control group	Boyacá	0	07/12/09 - 07/01/10	1.265.517	54,71
	Cauca	0	07/12/09 - 07/01/10	1.288.499	43,25
	Córdoba	0	07/12/09 - 07/01/10	1.558.267	62,17
	Chocó	0	07/12/09 - 07/01/10	485.515	10,28
	Huila	0	07/12/09 - 07/01/10	1.068.820	54,93
	La Guajira	0	07/12/09 - 07/01/10	791.027	38,55
	Nariño	0	07/12/09 - 07/01/10	1.619.464	52,98
	Valle del Cauca	0	07/12/09 - 07/01/10	4.337.909	205,53
	Arauca	0	07/12/09 - 07/01/10	244.507	10,28
	Putumayo	0	07/12/09 - 07/01/10	322.681	12,53
	San Andrés y Providencia	0	07/12/09 - 07/01/10	72.735	1.192,38
	Amazonas	0	07/12/09 - 07/01/10	71.190	0,65
	Guainía	0	07/12/09 - 07/01/10	37.705	0,53
	Vichada	0	07/12/09 - 07/01/10	62.013	0,89
	Sub Total	-----------	-----------	13.225.849	-----------
Treatment group	Antioquia	43	01/12/09 - 12/01/10	5.988.984	94,64
	Atlántico	36	07/12/09 - 11/01/10	2.284.840	689,91
	Bogotá D.C	35	04/12/09 - 07/01/10	7.259.597	4.417,78
	Bolívar	93	19/11/09 - 19/02/10	1.958.224	73,66
	Caldas	32	07/12/09 - 07/01/10	976.438	130,95
	Caquetá	32	07/12/09 - 07/01/10	442.033	4,89
	Cesar	32	07/12/09 - 07/01/10	953.827	42,32
	Cundinamarca	35	04/12/09 - 07/01/10	2.437.151	108,01
	Magdalena	32	07/12/09 - 07/01/10	1.190.585	51,69
	Meta	16	23/12/09 - 07/01/10	853.115	10,01
	Norte de Santander	32	07/12/09 - 07/01/10	1.286.728	58,22
	Quindio	32	07/12/09 - 07/01/10	546.566	275,67
	Risaralda	32	07/12/09 - 07/01/10	919.653	252,08
	Santander	31	08/12/09 - 07/01/10	2.000.045	65,67
	Sucre	93	19/11/09 - 19/02/10	802.733	75,14
	Tolima	26	16/12/09 - 10/01/10	1.383.323	57,60
	Casanare	16	23/12/09 - 07/01/10	319.502	7,21
	Guaviare	16	23/12/09 - 07/01/10	101.794	1,85
	Vaupés	16	23/12/09 - 07/01/10	41.094	0,49
	Sub Total	-----------	-----------	31.746.232	-----------
	Total	-----------	-----------	44.972.081	-----------

In passing it is important to assess that during the period from November of 2008 up to November of 2009 the homicide rate was actually higher for control departments than for treated departments: 46 and 30 respectively. This is because the population in the treated departments corresponds to 31.7 million out of the 44.9 million in the country (70% of the population) while for control departments it amounts to only 13.2 million (30% of the population). As a general fact reported homicides were committed with a firearm: 83% for control departments and 79% for treatment departments, which indicates the prevalence of firearms use in homicidal violence.

Table 1b reports the descriptive statistics of the daily average of reported homicides and injuries with firearms during the period of the ban for both groups of departments while also reporting the daily average for the exact same period in which the ban was implemented but averaged out for previous years, from 2003-2008. For the control group departments we use the period December 7 of 2009 to January 7 of 2010, the ban period which would have been implemented by these departments if they would have complied with the general ban. The table reveals that homicides and injuries with firearms tended to drop on the majority of the departments of the treatment group while not in the control group. As can be observed in the table, even though in daily terms there is a great dispersion across departments there is a strong positive correlation among levels of daily reported gun homicides and population levels (the Spearman correlation coefficient is of 0.71 for treatment departments during the corresponding ban period for 2003-2008 and 0.75 for the actual ban period 2009-2010; similarly for control departments 0.92 and 0.93 using the average period December 7 of 2009 to January 7 of 2010). This is also the case for gun injuries (i.e. correlation coefficient of 0.81 for treatment departments during the corresponding ban period for 2003-2008 and 0.83 for the actual ban period 2009-2010; similarly for control departments 0.92 and 0.85 using the average period December 7 of 2009 to January 7 of 2010).

Table 1b. Statistics: Homicides and Injuries with Firearms

	Department	Daily average Homicides with firearms on similar restriction days (2003-2008)	Daily Average Homicides with firearms during actual restriction (2009-2010)	Daily average injuries with firearms on similar restriction days (2003-2008)	Daily average injuries with firearms during actual restriction (2009-2010)
Control Group	Boyacá	0,4358	0,1875	0,2991	0,1250
	Cauca	1,2570	0,8438	0,6567	1,1250
	Córdoba	0,7436	0,9000	0,2060	0,3333
	Chocó	0,3913	0,4286	0,2232	0,1429
	Huila	0,9674	0,8148	0,5508	0,8889
	La Guajira	0,7164	0,5714	0,3285	0,3929
	Nariño	1,5683	1,3438	0,8024	0,6563
	Valle del Cauca	4,9010	4,8750	1,6510	1,9688
	Arauca	1,0962	0,7391	0,2588	0,0870
	Putumayo	0,8886	0,4783	0,2592	0,1739
	San Andrés y Providencia	0,0348	0,2727	0,2503	0,5455
	Amazonas	0,0290	0	0,0290	0,1176
	Guainía	0	0	0,0208	0
	Vichada	0,1485	0	0	0
		------------------	------------------	------------------	------------------
Treatment Group	Antioquia	4,5582	5,8605	1,0862	1,4884
	Atlántico	0,5036	0,3333	0,4433	0,4167
	Bogotá D.C	2,2514	2,4286	2,9740	2,1429
	Bolívar	1,0034	0,9121	0,5838	0,4505
	Caldas	1,4167	0,5000	1,0104	1,1563
	Caquetá	0,8237	0,6333	0,3977	0,3000
	Cesar	1,0129	0,3333	0,1216	0,2667
	Cundinamarca	1,0905	0,5714	0,2429	0,0857
	Magdalena	1,1743	0,5556	0,3949	0,2222
	Meta	1,7326	0,7333	0,6326	0,4000
	Norte de Santander	1,9184	1,1613	0,3242	0,3226
	Quindio	0,5817	0,5714	0,5232	0,1786
	Risaralda	1,9688	1,2813	1,0417	0,5000
	Santander	1,0489	0,3226	0,6238	0,3871
	Sucre	0,4190	0,3837	0,2271	0,3140
	Tolima	0,8772	0,7692	0,4841	0,9231
	Casanare	0,5374	0,3077	0,1263	0,1538
	Guaviare	0,4167	1,0000	0,1190	0,2500
	Vaupés	0,2500	0	0	0

In passing it is important to note that treated departments had almost twice as many injuries as control departments during November of 2008 and November of 2009: 4,151 and 2,493, respectively. Again injury rates are higher for control departments than for treatment departments: 135 and 117 injuries per one hundred thousand inhabitants, respectively, and 19 and 13 injuries per one hundred thousand for firearm injury rates. Moreover, injuries with firearms are 14% and 11% of total injuries for treated and control departments respectively. This contrasts with homicides starkly since as noted above homicides with firearms are around 80% of total homicides. This of course has to do with the huge lethality that firearms bring to violent interactions in the population and by the diversity of weapons used to produce personal injuries.

Table 1c. Descriptive Statistics: Firearms Confiscated

	Department	Daily Average of Total Firearms conficated on similar restriction days (2003-2008)	Daily Average of Total Firearms conficated on similar restriction days (2009-2010)	Daily Average confiscated legal firearms on similar restriction days (2003-2008)	Daily Average confiscated legal firearms during actual restriction (2009-2010)	Daily average confiscated illegal firearms on similar restriction days (2003-2008)	Daily Average confiscated illegal firearms during actual restriction (2009-2010)
Control group	Boyacá	2,3565	2,7500	0,7040	0,1875	1,6526	2,5625
	Cauca	2,0172	2,4688	0,2634	0,1563	1,7537	2,3125
	Córdoba	2,7047	1,2000	0,8871	0,2667	1,8176	0,9333
	Chocó	1,2000	0,5238	0,0598	0,0952	1,1402	0,4286
	Huila	1,7174	1,0000	0,2693	0,1111	1,4481	0,8889
	La Guajira	1,9529	1,3571	0,7437	0,2500	1,2092	1,1071
	Nariño	3,2798	4,9063	0,3155	0,3750	2,9643	4,5313
	Valle del Cauca	10,4948	53,7501	3,0156	1,0938	7,4792	52,6563
	Arauca	0,2128	0,1304	0,0952	0,0435	0,1176	0,0870
	Putumayo	1,2200	0,7391	0,1244	0,1304	1,0955	0,6087
	San Andrés y Providencia	0,4803	0,4545	0,0111	0,0909	0,4692	0,3636
	Amazonas	0,2719	0,7647	0,0761	0,0588	0,1958	0,7059
	Guainía	0,1491	0,5000	0,0238	0,3333	0,1253	0,1667
	Vichada	0,7174	0	0,0280	0	0,6895	0
Treatment group	Antioquia	16,0254	71,1627	1,9033	0,9767	14,1221	70,1860
	Atlántico	1,6746	1,1667	0,4195	0,4722	1,2551	0,6944
	Bogotá D.C	23,7055	85,4857	3,7641	0,8857	19,9414	84,6000
	Bolívar	2,6181	68,0000	0,6452	0,2527	1,9729	67,7473
	Caldas	3,1563	74,5626	0,5990	0,7813	2,5573	73,7813
	Caquetá	1,1254	1,1000	0,3753	0,0667	0,7500	1,0333
	Cesar	2,3512	1,1333	0,2805	0,3333	2,0706	0,8000
	Cundinamarca	4,9619	23,6286	1,4524	0,6857	3,5095	22,9429
	Magdalena	3,4693	3,1111	1,2231	0,3704	2,2462	2,7407
	Meta	2,0313	13,1334	0,8444	0,2667	1,1868	12,8667
	Norte de Santander	4,7534	3,6129	2,1178	0,9032	2,6356	2,7097
	Quindio	2,2473	3,6429	0,3475	0,5714	1,8998	3,0714
	Risaralda	3,5938	7,0938	1,0104	0,7500	2,5833	6,3438
	Santander	4,9276	13,4194	0,9403	0,8710	3,9873	12,5484
	Sucre	1,5051	2,0233	0,6031	0,1163	0,9020	1,9070
	Tolima	3,3540	96,1923	0,3694	0,1538	2,9847	96,0385
	Casanare	1,8340	3,9231	0,2344	0,0000	1,5996	3,9231
	Guaviare	0,4643	1,2500	0,1230	0,1250	0,3413	1,1250

Finally, in *Table 1c* reports the daily average within departments of firearms confiscated for both periods and for treated as well as control departments. As observed the amount of firearms confiscated daily in treated departments is much higher than in control departments during the period of the restriction. The increase is substantial since it is more than an eight fold increase for some of the treated departments. This huge increase is evidence that there was an enforcement of the ban in the treated departments. Nonetheless it is important to assess whether that increase corresponds to legal or illegal guns. The table also reports the daily average of *legal firearms* (firearms confiscated that have an ownership certificate but not a carrying permit during the ban) as well as *illegal firearms* (firearms confiscated that did not have either an ownership certificate or a carrying permit) confiscated in both types of departments during the ban for treated departments and the generic period for control departments. As seen there is a huge increase in the confiscation of mainly *illegal* firearms during the period of the ban for the treated departments while not apparent for the control departments except for Valle del Cauca.

5.1. The case of Antioquia

There are several reasons why we needed to exclude the department of *Antioquia* from the treated group, the largest department in terms of population (excluding Bogotá from Cundinamarca) and one of the most violent, in order to assess the impact evaluation of the ban. The primary reason has to do with the fact that starting in 2008 this department took the lead in implementing a city-wide restriction (in Medellín, the capital) that was later made permanent (non only during weekends, for example) and further extended in mid-2009 to the whole department. Such previous experience precludes us from identifying the impact of the intervention in this department. Another reason is the peculiar dynamic of violence of this department during the period of the intervention which was strikingly different from the rest of the departments, mainly because of the conflicts between groups of drug lords and the negative impacts of a local process of disarmament, demobilization and reintegration of the paramilitary groups which were notoriously prevalent in the area. We do present results with and without Antioquia to get a sense of the change in the estimates.

6. IDENTIFICATION OF THE CAUSAL EFFECT OF THE BAN

As noted above, the actual decision of implementing the ban in a department was made by the respective incumbent governor. One presumably could argue that partisan policies regarding gun control could be behind these decisions where ideological beliefs within parties about how gun control could lower or not violence would actually end up influencing a governor's decision. In the United States for example there is a division in political parties about the issue of gun control: while gun control is associated strongly with the Democratic Party the anti gun control movement (especially influenced by the National Rifle Association) is closer to the Republican Party. Even so we argue that for Colombian political parties there does not seem to be a clear ideological preference for bans on firearms. *Table 1d* shows the political affiliation of the governors that won the elections in 2007 and that were the incumbents by November of 2009 when the decision on implementing the ban was made. The table distinguishes between the control and treatment departments, and is evident that there does not seem to be any ideological bias towards implementing the firearm ban. For example, the governors affiliated with the two traditional parties in Colombia, the *Partido Liberal Colombiano* and *Partido Conservador Colombiano* have a similar distribution among control and treatment departments. Moreover, the other incumbent governors do not seem to have a clear ideological preference for gun control. For example, *Partido de la U*, the party of the incumbent president of that time, Alvaro Uribe, did not seem to have any preference for adhering or not to the ban since almost half of the governor's of this party went with the ban while the other half did not. The only governor of the leftist *Polo Democrático* decided not to implement the ban. Of the total, 18 governor's from all ideologically preferences and party affiliation decided to implement the ban while 14, with also similar distribution of parties and preferences, did not. This evidence points to the idea that political ideology of incumbent governor's did not seem to influence the adoption or not of the ban.

Table 1d: Gubernatorial elections 2007

Political Party	*Control*	*Treatment*
El Pueblo Decide	1	0
Integración Regional	1	0
Movimiento Alas-Equipo Colombia	0	1
Movimiento Alianza Social Indígena	0	1
Movimiento Nacional Afrocolombiano Afro	1	0
Partido Cambio Radical	1	2
Partido Colombia Democrática	1	0
Partido Conservador Colombiano	1	4
Partido Convergencia Ciudadana	1	0
Partido Liberal Colombiano	1	4
Partido Social de Unidad Nacional Partido de la U	3	4
Partido Verde Opción Centro	1	1
Polo Democrático Alternativo	1	0
Por Un Quindío Para Todos	0	1
Por Un Valle Seguro	1	0
Total	14	18

Source: Registraduría General de la Nación

It can also be argued that the ban was implemented by governors that believed that there were high levels of violence in the department (measured as homicides and/or injuries) or even if the victimization risk was rising or stable but at a relatively high level. *Figures 4a* through *4f* show the monthly frequency of daily average reported total homicides and injuries as well as homicides and injuries with and without firearms distinguishing the control (excluding Valle del Cauca) and treated (excluding Antioquia) departments from January of 2007 up to June of 2010. In each figure we show with a bar the generic ban period from November of 2009 to January of 2010.

Figure 4a reports the monthly frequency of the daily average of total homicides per department reported in treated as well as control departments. It can be observed that there is a similar weak negative trend in both groups and furthermore during the ban the treated group had a decrease in total homicides reported relative to the control group. Hence, during the ban lethality in violent interactions decreased which is consistent with the theoretical prediction of our model. *Figure 4b* reports the monthly frequency of the daily average of reported homicides *with firearms* per department. The figure reveals a similar trend of these averages for both treated and untreated departments. Even though not reported the figure including Antioquia shows actually an upward trend for the treated group relative to the control group which is our main concern that would violate the common trend assumption. Hence, the exclusion of Antioquia is key for identification in order to get similar trends for both treated and control departments prior to the ban and which allows us to credibly interpret our results in a causal way.

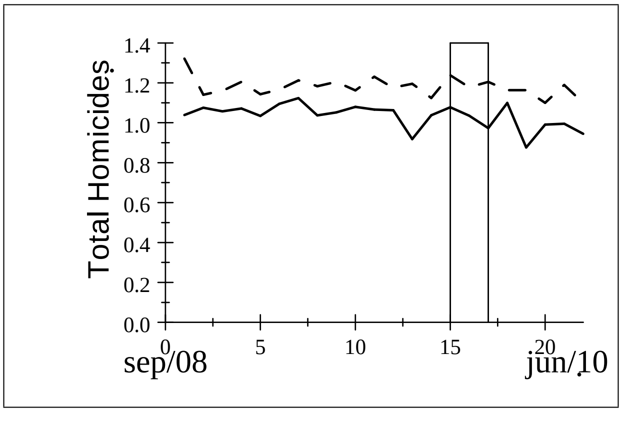

Figure 4a: Monthly frequency of daily average total homicides where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to January of 2010.

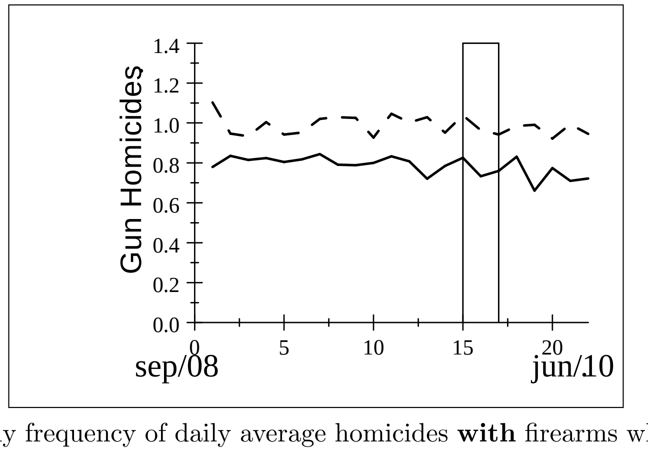

Figure 4b: Monthly frequency of daily average homicides **with** firearms where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to January of 2010.

Figure 4c reports the monthly frequency of the daily average per department of reported homicides *without firearms*. The figure reveals again a similar trend in both type of departments. Note that total homicides are conformed by adding up gun and non gun homicides and therefore one can see that gun homicides accounts for more than 80% of total homicides.

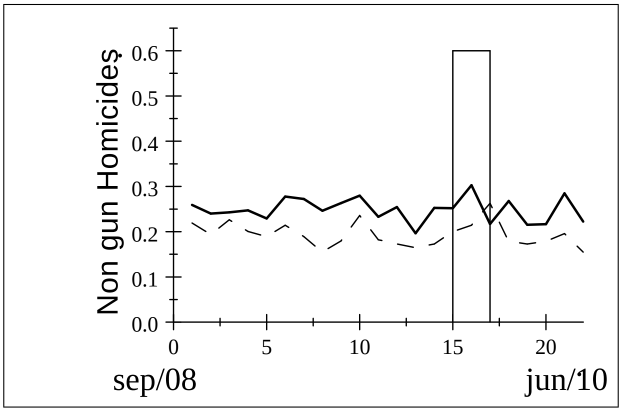

Figure 4c: Monthly frequency of daily average homicides **without** firearms where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to February of 2010.

As can be seen in all these figures, both the treatment and control groups seem to have very similar behavior from prior to the adoption of the ban in terms of reported homicides. Moreover, figures *4d* to *4f* report the same for total injuries, injuries with firearms and injuries without firearms. Again there is a similar trend in each violent outcome. Note that for injuries the scale is different for gun injuries.

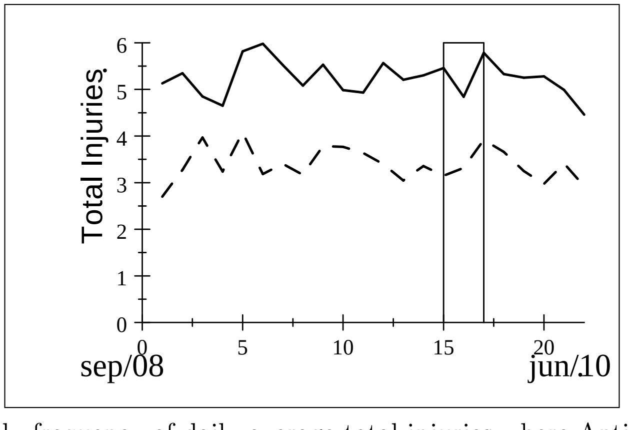

Figure 4d: Monthly frequency of daily average total injuries where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to January of 2010.

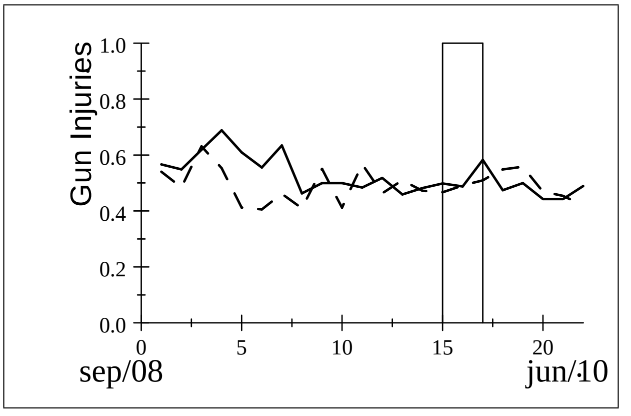

Figure 4e: Monthly frequency of daily average injuries **with** firearms where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to January of 2010.

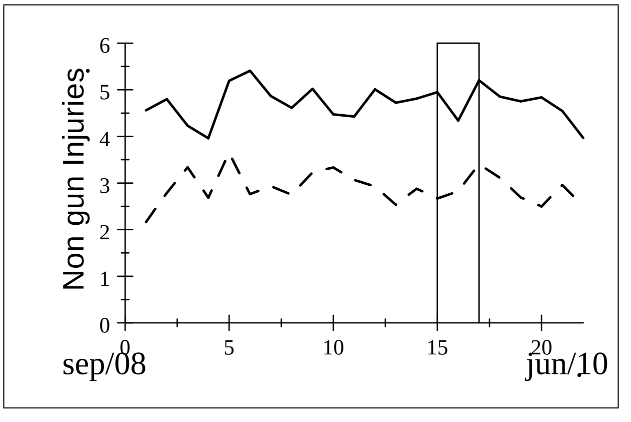

Figure 4f: Monthly frequency of daily average injuries **without** firearms where Antioquia is excluded from treatment group (solid line) Rectangle represents ban period from November of 2009 to January of 2010.

The similar behavior of the series for both treated and untreated departments is remarkable and gives compelling evidence that the assumption of a *common trend* is *credible* for these violent outcomes between the control and treated departments and which allows us to interpret the treatment "as if it would have been randomly assigned" given that we control for department fixed effects in our empirical estimations. Importantly, the common trend empirically holds *without* Antioquia which is why we end up excluding this department. Under this scenario we believe that our empirical estimates can be interpreted in a causal way.

7. EMPIRICAL FRAMEWORK

Taken together, the evidence shown above suggests that the ban on firearms in Colombia during the end of 2009 and the beginning of 2010 could be interpreted as a type of quasi-experiment under the common trends assumption which would deliver a causal interpretation of a difference in difference estimator of the marginal effect of the ban. To be more precise consider the following empirical model given a panel of departments

$$V_{it} = h_i + \delta D_t + \beta_1 B_{it} + \lambda \mathbf{C}_{it} + u_{it} \tag{9}$$

where $i = 1, .., 33$ denotes Colombia's departments (including Bogotá as a separate entity) and $t = 1, 2$; V_{it} denotes a violent outcome (i.e. number of reported gun homicides per day in department i during period t or the number of reported gun injuries per day in department i during period t), h_i reflects all department time invariant unobservable factors (department fixed effects) that determine the violent outcome like idiosyncratic gun demand at the department level or the operation of illegal armed groups in the department, among other determinants; the binary variable D_t takes the value one if $t = 2$ and zero otherwise capturing a time effect while the variable B_{it} is a binary variable that takes the value one in department i during period t if a gun carrying ban was implemented in the department and zero otherwise; $\mathbf{C}_{it}$ is the amount of intervention-related law enforcement which is proxied by the confiscation of legal and illegal firearms in department i during period t ($\mathbf{C} = (C_l, C_{il})$ where l denotes legal and il denotes illegal), and finally u_{it} is the idiosyncratic error term. Period $t = 2$ is the period in which the gun carrying ban was implemented at the department level which for most departments was typically from December 7 of 2009 to January 7 of 2010 while period $t = 1$ is a comparable pre treatment period that contains the daily average of violent outcomes during exactly the same period for the department in previous periods that span from 2003 to 2008.

Taking the difference between period two from one and denoting $\Delta V_i \equiv V_{i2} - V_{i1}$, $\Delta B_i \equiv B_{i2} - B_{i1} = B_{i2}$ since $B_{i1} = 0$ and $\Delta \mathbf{C}_i \equiv \mathbf{C}_{i2} - \mathbf{C}_{i1}$ yields the following empirical specification

$$\Delta V_i = \delta + \beta_1 B_{i2} + \lambda \Delta \mathbf{C}_i + \Delta u_i \tag{10}$$

for $i = 1, .., 33$ where the department fixed effects h_i is eliminated and the constant δ of the linear model is the time dummy coefficient associated with D_t from (9). Given that treated departments are heterogenous in terms of population density ($Popdy$) measured as population of the department per square kilometer in 2009, number of days during which the ban was implemented ($Days$) and the amount of firearms confiscated ($\mathbf{C}$) we end up interacting B_{i2} with this other variables which yields the following specification

$$\begin{aligned} \Delta V_i &= \delta + \beta_1 B_{i2} + \beta_2 (Days_i * B_{i2}) + \beta_3 (Days_i^2 * B_{i2}) \\ &\quad + \beta_4 (Popdy_i * B_{i2}) + \beta_5 (\Delta \mathbf{C}_i * B_{i2}) + \lambda \Delta \mathbf{C}_i + \Delta u_i \end{aligned} \tag{11}$$

for $i = 1, .., 33$.

The parameters of interest are the β's since the average marginal effect M turns out to be

$$\begin{aligned} AM &\equiv E\left[\Delta V | B_2 = 1, \mathbf{X}\right] - E\left[\Delta V | B_2 = 0, \mathbf{X}\right] \\ &= \beta_1 + \beta_2 Days + \beta_3 Days^2 + \beta_4 Popdy + \beta_5 \Delta \mathbf{C} \end{aligned}$$

where $\mathbf{X} = \left[Days, Days^2, Popdy, \Delta \mathbf{C}\right]$. Under the common trend assumption we would have $E\left(\Delta u | B_2\right) = 0$ which is the key identifying assumption. We then estimate (11) evaluated at the respective means of $Days$, $Days^2$, $Popdy$ and $\Delta \mathbf{C}$ which yields the coefficient associated with B_{i2} as the difference-in-difference estimator that we interpret as the causal effect of the Ban.

Interestingly, we can study how the marginal effect M varies with the number of $Days$ that the ban was implemented given by

$$\frac{\Delta AM}{\Delta Days} \simeq \beta_2 + 2\beta_3 Days \tag{12}$$

which evaluated at different levels of $Days$ that the ban lasted gives us a sense of change in the effect.

8. RESULTS

This section reports the results of the empirical framework proposed above under our identifying strategy of common trends. Our interest lies primarily in verifying the correctness of our conjecture of the transmission mechanism, derived from the theoretical framework, that the ban generates on homicides and injuries with and without firearms. We also consider two robustness exercises that allow us to assess the validity of our results.

8.1. Ban Enforcement

Table 2 reports first regressions that allows us to verify if the ban was enforced in the treated departments. We report in a stepwise manner different specifications with an eye on normality and heteroskedasticity tests due to the small sample size that we have (all specifications report the p value of such tests). Given this, the significance levels reported (the asterisks used also in subsequent tables) are done under valid standard errors: namely, if homoskedasticity is rejected at 10% then we conclude significance levels for the estimated parameters of interest with the heteroskedastic standard errors. Note that the first three specifications of Table 2 report the regression of the difference in firearms confiscated daily at the department level on the binary variable *Ban* for the ban period and for similar periods averaged out between 2003 and 2008. Since the nature of the ban is to restrict the guns in the street whether they are legal or not, we would expect for the treatment departments should have had a higher amount of guns confiscated if enforcement was in place; if not, then the ban simply would not have been enforced and it would have been tantamount to a voluntary compliance restriction only applicable to legally licensed handguns.

The first specification of Table 2 shows that treated departments confiscated 0.83 more firearms on average daily than control departments and is statistically significant at the 10% level under robust heteroskedastic standard errors. This effect is practically significant since the control group had on average 0.36 firearms confiscated per department on a daily basis which amounts to a 130% increase in firearm confiscation during the ban period, a quite substantial increase. Controlling for Antioquia does not overturn the conclusion that treated departments increased substantially firearm confiscation relative to control departments. Note that the marginal effect when *Ban* is interacted with other variables is always reported in a separate row and is statistically significant at least at the 15% level and even more significant practically. The inclusion of $Days$ and $Days^2$ was done to study whether law enforcement authorities confiscated differently as the ban lasted longer. We find that this was not the case: statistically speaking, under robust standard errors, treated departments did not differ with respect to control departments in terms of the number of days that the ban lasted since the table shows that the regressors of $Days$ and $Days^2$ interacted with the dummy *Ban* were not statistically significant at any usual significance level. When including a dummy for Antioquia in the second specification the marginal effect decreases slightly to 0.78, which was obtained evaluating at the means of $Days$ and $Days^2$ which are 35.57 and 1683 respectively, but it is still statistically and practically significant.

Moreover, the third specification in table 2 includes additionally a dummy variable for Valle del Cauca to study if the marginal effect varied since the descriptive statistics of Table 1c showed an unusual increase in firearms confiscated in this department during the period of the ban even though it formed part of the control group. The marginal effect turns out to be 1.01 which is somewhat greater than the benchmark of 0.83 but still allows us to conclude that treated departments increased substantially the confiscation of firearms during the ban period relative to control departments. For this specification the 90% confidence interval shows that the marginal effect is between 0.3 and 1.7 firearms confiscated on average during the ban period per treated department. This is evidence in favor of the idea that treated departments enforced the ban during the period in which it was announced.

Table 2: Regressions for Confiscated Firearms

Restriction period (Dic-2009 to Jan-2010) compared to average of similar restriction days (2003-2008)

Dependent variable	Δ Ilegal Firearms Confiscated	Δ Ilegal Firearms Confiscated	Δ Ilegal Firearms Confiscated	Δ Legal Firearms Confiscated	Δ Legal Firearms Confiscated	Δ Legal Firearms Confiscated
Independient variable	(1)	(2)	(3)	(4)	(5)	(6)
Ban	0.87**	-0.43	-0.20	-0.01	0.03	0.03
	(0.48)	(1.75)	(1.61)	(0.01)	(0.04)	(0.04)
	[0.44]	[1.05]	[1.03]	[0.011]	[0.03]	[0.03]
Ban*Days		0.05	0.05		-0.002	-0.002
		(0.08)	(0.07)		(0.002)	(0.002)
		[0.06]	[0.06]		[0.002]	[0.002]
Days^2*Ban		-0.0003	-0.0003		0.00002	0.00002
		(0.0007)	(0.0006)		(0.00002)	(0.00001)
		[0 .0005]	[0.0006]		[0.00002]	[0.00002]
Antioquia	No	Yes	Yes	No	Yes	Yes
Valle del Cauca	No	No	Yes	No	No	Yes
Constant	0.23	0.23	0.0001	-0.013	-0.013	-0.006
	(0.36)	(0.37)	(0.35)	(0.009)	(0.009)	(0.008)
	[0.2295]	[0.24]	[0.01]	[0.007]	[0.008]	[0.004]
Observations	33	33	33	33	33	33
F Statistic without Antioquia and Valle (p value)	0.05	0.32	0.13	0.34	0.61	0.31
R-squared	0.095	0.185	0.33	0.027	0.081	0.31
Marginal Effect of Ban at mean	0.87**	0.79*	1.01***	-0.01	-0.01	-0.02*
of variables	[0.49]	(0.48)	[0.41]	(0.01)	[0.01]	(0.01)
[90%Conf.Interval]	[0.05 1.70]	[-0.04 1.62]	[0.30 1.70]	[-0.03 0.008]	[-0.03 0.011]	[-0.03 0.001]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0.004	0.0006	0.0002	0.0001	0.0001	0.0001
Breusch-Pagan Heteroskedasticity test (p value)	0.03	0.21	0.09	0.31	0.0535	0.10

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

These results nonetheless include both legal as well as illegal firearms. Table 2 shows in specifications 4 to 6 the confiscation of illegal firearms while specifications 7 to 9 report those for legal ones. We find that the ban increased primarily the confiscation of illegal firearms since the marginal effect with all the controls is 1.01 illegal firearms confiscated more on average daily per treated department relative to control departments according to specification 6. In contrast we do not find evidence that the ban increased statistically the confiscation of legal firearms in treated departments. In contrast, for legal firearms the point estimates are sometimes negative and barely significant as in specification 9. This allows us to conclude that the ban increased confiscation of primarily illegal firearms and that this enforcement held steadily during the implementation period of the intervention. Moreover, we do not find evidence that the ban increased the confiscation of legal firearms.

8.2. The effect of the ban on firearm related homicides

Table 3a reports the results for *homicides with firearms*. The first specification reports a simple benchmark regression of the difference of daily homicides (during the period of the ban and a similar pre treatment period averaged out also daily for 2003 up to 2008) on the

Ban dummy. We find a negative but not statistically significant effect.

Table 3a: Regressions for Homicides with Firearms

Restriction period (Dic-2009 to Jan-2010) compared to average of similar restriction days(2003-2008)

Dependent variable	ΔHomicides with Firearms	ΔHomicides with Firearms	ΔHomicides with Firearms	ΔHomicides with Firearms	ΔHomicides with Firearms
Independent variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Ban	-0.14	-0.34**	-0.38***	-0.39***	0.48
	(0.16)	(0,18)	(0.18)	(0,18)	(0.40)
	[0.14]	[0.20]	[0.22]	[0.22]	[0.67]
Days*Ban		0,0033	0,0032	0,0036	-0.0366
		(0,0037)	(0,0036)	(0,0038)	(0.018)
		[0.0032]	[0.0034]	[0.0035]	[0.030]
Days^2*Ban					0.0004*
					(0.0001)
					[0.0002]
Population Density *Ban			0.00013*	0.0002*	0.0004***
			(0.00008)	(0,00011)	(0.0001)
			[0.00001]	[0.0001]	[0.0001]
Δ illegal firearms confiscated				-0.0001	
				(0.051)	
				[0.061]	
Δ legal firearms confiscated				1.82	
				(2.55)	
				[2.50]	
Δ illegal firearms confiscated*Ban					-0.030
					(0.050)
					[0.070]
Δ legal firearms confiscated*Ban					5.81**
					(3.41)
					[3.10]
Dummy for Antioquia ?	No	Yes	Yes	Yes	Yes
Constant	-0,12	-0,12	-0,12	-0,099	-0.13***
	(0,12)	(0,09)	(0,09)	(0,10)	
	[0.053]	[0.055]	[0.056]	[0.065]	[0.060]
Observations	33	33	33	33	33
F Statistic without Antioquia (p value)	0,4	0,16	0,09	0,23	0.0001
R-squared	0.024	0,4	0,5	0,51	0,62
Marginal Effect of Ban at mean	-0.14	-0.22**	-0.23***	-0.22**	-0.25***
of variables	[0.14]	[0.11]	(0.11)	[0.12]	(0.11)
[90%Conf.Interval]	[-0.37 0.01]	[-0.41 -0.03]	[-0.42 -0.04]	[-0.43 -0.007]	[-0.43 -0.05]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0,001	0,085	0.002	0.02	0.001
Breusch-Pagan Homoskedasticity test (p value)	0,005	0,052	0.91	0.04	0.88

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

The second specification of Table 3a controls for Antioquia because of the several features we noted above for these departments, and includes an interaction term of number of days the ban lasted with the Ban dummy. In this case the marginal effect of the ban is -0.22 at the mean of the control variable and is statistically significant at the 10% under heteroskedastic standard errors. The 90% confidence interval is [-0.41, -0.03] which seems somewhat wide. We do not find in this specification a statistically significant effect of the interaction variable of the dummy Ban and number of days under the ban which suggests that the marginal effect does not vary between departments depending on the number of days that the ban lasted. The marginal effect can be interpreted in the following manner: a given treated department had a drop (for bans on average lasting 35.5 days and for an average population density 247.2) of about -0.22 of daily homicides during the ban. For the average ban duration of

35.5 days this amounts to a reduction of approximately 8 ($= 0.22 * 35.5$) reported homicides with firearms in a typical department implementing the intervention. Multiplying this effect by 18 treated departments (excluding Antioquia) yields $8 * 18 = 144.0$ less homicides with firearms attributed to the ban. With respect to control departments this is a drop of almost 23% ($= \frac{0.22*100}{0.94}$) in homicides with firearms since 0.22 is less than a third of the average amount of daily homicides for control departments in similar days of the restriction for 2003-2008 which is 0.94. Viewed in this way the effect seems important and significant in practical terms.

The third and fourth specifications in Table 3a control now for the difference in legal and illegal firearms confiscated as well as the interaction of the dummy Ban with population density as the model in equation (11) implies. In summary, we find that only the population density is statistically significant individually but not jointly in specification four when the difference in legal and illegal firearms ($\mathbf{\Delta C}$) are controlled for. This last result means that more densely populated treated departments are more likely to have more homicides with firearms. Now the marginal effect at the mean of the regressors interacted with Ban is robust to these inclusions.

The fifth specification in Table 3a excludes the levels of $\mathbf{\Delta C}$ but adds $\mathbf{\Delta C}$ interacted with the ban dummy. We do this because the levels are not jointly significant in the fourth specification. Importantly, and in contrast with the previous specification, the variable ΔC_l is statistically significant at the 5% level. The dummy variable Ban interacted with $Days^2$ becomes statistically significant at the 15% level. Furthermore, the F statistic shows that all regressors (excluding Antioquia) are jointly significant at the 5% level. Note that the marginal effect does not change much in this last specification increasing to 0.25 which yields a drop of 26% ($= \frac{0.25*100}{0.94}$) rather than the 23% that we found for the second specification. Given these results we prefer this last specification since it seems the most credible specification and allows us to credibly assess under the identifying assumption that the ban lowered causally in a significant way the homicides in the treated departments relative to the control departments. More precisely the ban decreased almost 9 homicides daily per treated department which adds up to a reduction of 162 in total homicides during the restriction period. This seems a substantial effect in practical terms! Moreover, the 90% confidence interval in the fifth specification is $[-0.43, -0.05]$ which translates into an approximate confidence interval in terms of lives saved of about $[-275, -32]$. Hence, if our identifying strategy is correct then we can attribute to the ban *at least* 32 lives saved of being killed with a gun. Naturally, under our conceptual framework there can be a potential substitution effect since delinquents can turn to less lethal weapons and therefore we have to assess if there was an increase in homicides with these less lethal weapons. This is something we discuss in the next section.

Finally, in terms of equation (12) we find with specification five of Table 3a that the average marginal effect on homicides with firearms with respect to the heterogeneity in Days has a U curve behavior since $\frac{\Delta AM}{\Delta Days} = -0.0366 + 2\,(.0004)\,Days$ which suggests that the ban actually loses steam as the number of days pass under its implementation. This suggests an *optimal temporal* nature of a ban on firearms since delinquents seem to learn how to cope with the ban and still commit homicides. According to this the maximum amount of Days that the ban should last (which is found equalizing $\frac{\Delta AM}{\Delta Days}$ to zero) is 41.75 days, which is a little bit greater than the average length of the ban (35.5 days) that was actually implemented.

8.3. Non-gun related homicides

One concern according to the anti gun control literature, which is captured by our conceptual framework, is the possibility that a gun ban on firearms could make criminals substitute weapons towards other less lethal weapons such as knives, generating the possibility that non-gun homicides could actually increase during the ban. This prediction comes out of our theoretical model since the gun ban makes delinquents substitute guns for knives at the margin and therefore it is possible that the ban could generate higher non-gun homicides. Table 3b reports the same five specifications that were considered in Table 3a for gun homicides but now with the difference that the change in non gun homicides is the dependent variable. All of these specifications show that homoskedastic standard errors are valid but normality is rejected. The F statistic shows that the included regressors are jointly significant in the last three specifications at the 5% which is reassuring. As can be seen in the table the marginal effects are not statistically significant at any level and more importantly the point estimates are quite small in practical terms. We conclude that the ban did not seem to increase homicides with less lethal weapons or where guns were absent.

Table 3b: Regressions for Homicides without Firearms

Restriction period (Dic-2009 to Jan-2010) compared to average of similar restriction days(2003-2008)

Dependent variable	ΔHomicides without Firearms	ΔHomicides without Firearms	ΔHomicides without Firearms	ΔHomicides without Firearms	ΔHomicides without Firearms
Independent variable	(1)	(2)	(3)	(4)	(5)
Ban	-0.03	-0.03	-0.09	-0.09	-0.08
	(0.07)	(0,10)	(0.07)	(0.07)	(0,18)
	[0.067]	[0.075]	[0.050]	[0.060]	[0.128]
Days*Ban		0.00066	0,0006	0.0006	0.00042
		(0,0021)	(0,0014)	(0,001)	(0.0085)
		[0.0008]	[0.0004]	[0.0004]	[0.0058]
Days^2*Ban					0.000002
					(0.00007)
					[0.00002]
Population Density *Ban			0.0002***	0.0002***	0.00021***
			(0.00003)	(0,00004)	(0,00006)
			[0.00001]	[0.00001]	[0.00002]
Δ illegal firearms confiscated				0.008	
				(0.020)	
				[0.009]	
Δ legal firearms confiscated				1.03	
				(0.97)	
				[0.58]	
Δ illegal firearms confiscated*Ban					0.01
					(0.02)
					[0.01]
Δ legal firearms confiscated*Ban					1.12
					(1.48)
					[0.82]
Dummy for Antioquia ?	No	Yes	Yes	Yes	Yes
Constant	0.021	0.021	0.021	0.033	0.02
	(0.054)	(0.050)	(0,034)	(0.037)	(0.042)
	[0.042]	[0.043]	[0.044]	[0.050]	[0.046]
Observations	33	33	33	33	33
F Statistic without Antioquia (p value)	0,69	0,68	0.0001	0.0003	0.0009
R-squared	0.005	0.188	0.639	0.654	0,65
Marginal Effect of Ban at mean of variables	-0.002	-0.002	-0,021	-0.020	-0,022
	(0.07)	(0.066)	(0,05)	(0,05)	(0.065)
[90%Conf.Interval]	[-0.11 0.11]	[-0.11 0.11]	[-0.10 0.06]	[-0.10 0.06]	[-0.10 0.06]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0,0001	0,00001	0,0022	0,0027	0.0014
Breusch-Pagan Homoskedasticity test (p value)	0,17	0,51	0,92	0,76	0.88

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

8.4. The effect of the ban on injuries

Table 4a reports the results for the same specifications considered above for another violence outcome: the difference of daily average *injuries with firearms* during the period of the ban between treatment and control departments. Again all specifications report the corresponding normality and heteroskedasticity tests. Results show that the ban also had an impact on non-lethal violence related with firearms which are proxied by reported injuries with these weapons. Across specifications we find that there is a complementary reduction in non-lethal armed violence as measured by personal injuries by firearms. In fact, on our preferred specification (specification 5), we find a marginal effect drop of 0.21 with a 90% confidence interval of [-0.37, -0.07]. This effect relative to the average daily gun injury reported in similar days of the restriction from 2003-2008 control departments which is 0.39 represents a drop of about 53% for treated departments. The decrease is a significant effect practically speaking. In terms of equation (12) the regressors on Days and Days2 are not jointly significant in any specification. This implies that for gun injuries the ban did not lose steam as the number of Days increased.

Table 4a: Regressions for Injuries with Firearms

Restriction period (Dic-2009 to Jan-2010) compared to average of similar restriction days(2003-2008)

Dependent variable	ΔInjuries with Firearms	ΔInjuries with Firearms	ΔInjuries with Firearms	ΔInjuries with Firearms	ΔInjuries with Firearms
Independent variable	(1)	(2)	(3)	(4)	(5)
Ban	-0.15*	-0.17	-0.11	-0.11	0.0367
	(0.09)	(0.13)	(0.12)	(0,11)	(0,28)
	[0.09]	[0.12]	[0.12]	[0.12]	[0.21]
Days*Ban		-0.00012	-0.00005	-0.00110	-0.009
		(0,0030)	(0,0024)	(0,0023)	(0,0132)
		[0.0018]	[0.0015]	[0.0028]	[0.011]
Days^2*Ban					0.0001
					(0,0001)
					[0.0001]
Population Density *Ban			-0.00018***	-0.00023***	-0.0002***
			(0.00005)	(0,00007)	(0,0001)
			[0.00001]	[0.00001]	[0.00007]
Δ illegal firearms confiscated				0.0037***	
				(0,002)	
				[0.002]	
Δ legal firearms confiscated				-0.00796	
				(0,08)	
				[0.05]	
Δ illegal firearms confiscated*Ban					0.064**
					(0.035)
					[0.036]
Δ legal firearms confiscated*Ban					0.99
					(2.40)
					[2.13]
Dummy for Antioquia ?	No	Yes	Yes	Yes	Yes
Constant	0,073	0,073	0,073	0,059	0,073
	(0,07)	(0,07)	(0,06)	(0,06)	(0,06)
	[0.05]	[0.06]	[0.06]	[0.06]	[0.06]
Observations	33	33	33	33	33
F Statistic without Antioquia (p value)	0.13	0,19	0.003	0,002	0.007
R-squared	0.073	0,18	0,43	0,52	0,52
Marginal Effect of Ban at mean of variables [90%Conf.Interval]	-0.15*	-0.17**	-0.15**	-0.20***	-0.17***
	(0,09)	(0,09)	(0,08)	(0,07)	(0,08)
	[-0.31 0.01]	[-0.33 -0.01]	[-0.29 -0.02]	[-0.34 -0.07]	[-0.33 -0.04]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0,17	0,11	0,77	0,87	0,68
Breusch-Pagan Homoskedasticity test (p value)	0,18	0,17	0,88	0,86	0,86

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

With respect to non gun related injuries Table 4b shows the regressions and basically we do not find evidence of an increase in these type of violent interactions during the ban. Nonetheless, the F statistic shows that the last three specifications have regressors jointly significant at the 5%.

Table 4b: Regressions for non related gun Injuries

Restriction period (Dic-2009 to Jan-2010) compared to average of similar restriction days(2003-2008)

Dependent variable	ΔInjuries without Firearms	ΔInjuries without Firearms	ΔInjuries without Firearms	ΔInjuries without Firearms	ΔInjuries without Firearms
Independent variable	(1)	(2)	(3)	(4)	(5)
Ban	0.41	0.39	-0.43	-0.36	-0.09
	(0.79)	(1,22)	(0.71)	(0,71)	(1.86)
	[0.71]	[0.90]	[0.60]	[0.62]	[1.37]
Days*Ban		0,003	0,002	-0,001	-0,011
		(0,025)	(0,015)	(0,015)	(0,086)
		[0.012]	[0.009]	[0.011]	[0.080]
Days^2*Ban					0.0001
					[0.0008]
					[0.0007]
Population Density *Ban			0.0024***	0.0020***	0.0027***
			(0,0003)	(0,0004)	(0,0006)
			[0.0001]	[0.0003]	[0.0006]
Δ illegal firearms confiscated				0,106	
				(0,20)	
				[0.11]	
Δ legal firearms confiscated				-13.332	
				(9.940)	
				[9.180]	
Δ illegal firearms confiscated*Ban					-0.021
					(0.23)
					[0.121]
Δ legal firearms confiscated*Ban					8.001
					[15.67]
					[16.00]
Dummy for Antioquia ?	No	Yes	Yes	Yes	Yes
Constant	0,660	0,660	0.660**	0,466	0.660**
	(0,60)	(0,62)	(0,36)	(0,38)	[0.377]
	[0.32]	[0.33]	[0.33]	[0.28]	[0.353]
Observations	33	33	33	33	33
F Statistic without Antioquia (p value)	0,61	0.84	0,000001	0,000001	0,000001
R-squared	0.008	0.020	0.683	0.710	0.681
Marginal Effect of Ban at mean	0,41	0,48	0,23	0,07	0,24
of variables	[0.71]	[0.77]	(0,48)	(0,50)	(0,48)
[90%Conf.Interval]	[-0.80 1.62]	[-0.82 1.78]	[-0.59 1.05]	[-0.68 1.01]	[-0.61 1.08]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0,000001	0,000001	0,0127	0,0048	0,015
Breusch-Pagan Homoskedasticity test (p value)	0,012	0,014	0,549	0,522	0,583

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

8.5. Robustness

This section reports two robustness checks of our empirical exercise. The first critique that one can do against our results is that even though there was an increase in the amount of guns confiscated (specially illegal guns) shown above this could have actually happened not due to the ban itself but because the National Police Department increased the number of policemen in the street. So our results are not reflecting the effect of the ban but only the effect of an increase in the amount of police force. Statistically speaking this seems as a possibility but with the data we have at hand there is not a simple way of testing it. Therefore we went to interview directly one of the heads of the National Police Department, General Jose Leon Riaño, to find out the precise way the ban worked in terms of the internal organization of the Police Department. General Riaño confirmed to us that the National Police Department does not hire extra officers during these type of gun bans and neither does it reallocate police officers from departments that did not implement the ban towards departments that did. Mainly because the National Police Department needs always the police force they have deployed all over the country to counteract other security threats which are several in Colombia. Moreover, General Riaño also confirmed to us that under a gun ban the order for the policemen is to look and confiscate guns. This means that a priority during the ban is to stop civilians or suspicious fellows and look for guns. Hence, our results seem then not to be threatened by this possibility, at least if we take the word of the National Police Department in the way the gun ban is implemented. Furthermore, General Riaño also confirmed to us that under a gun ban a civilian that is caught carrying an illegal gun is detained for at least a couple of days and the confiscated gun is not given back. The individual goes on to pay a fine and meet a judge that usually lets him go which seems to be a hole in the judicial system of the country. On the other hand, a civilian that owns a legal gun but under the gun ban is apprehended carrying it has to surrender his gun and then request the Police Department to give it back. Hence, after a week or so if all the documentation is in order the legal gun is given back to its owner.

A second critique to our results is that the period that we analyzed was not special and that even before the actual ban there could have been an effect reflecting something else that is generating the result besides the ban. This would question our identifying strategy since if we find something in a previous period in which there was no gun ban then it could be that there is an unobservable factor not taken into account in our identifying strategy that would explain our findings making our empirical exercise spurious. With this in mind we considered a previous period that was not too far from November of 2009 but also not too close. We ended up choosing the period June-July of 2009 as the period to study. This choice was mainly because it was the middle of the year and also had festivities in the middle, somewhat similar to the December-January period of the year. We reproduced the same exercise with the same amount of *Days* for each treated department as well as the same econometric specification as in specifications 5 of Tables 3 to 4. Moreover, the common trend assumption would also hold for this period according to the *Figures 6a,* up to *6d.* This robustness Placebo Treatment exercise is reported in Table 5.

Table 5: Regressions for Placebo Treatment

Pseudo Restriction period (June-July of 2009) compared to average of similar restriction days(2003-2008)

Dependent variable	**Δ Ilegal Firearms Confiscated**	**Δ Legal Firearms Confiscated**	**ΔHomicides with Firearms**	**ΔHomicides without Firearms**	**ΔInjuries with Firearms**	**ΔInjuries without Firearms**
Independent variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Ban	0.209	-0.078*	0.953	0.010	0.339	0.087
	(0.166)	(0.047)	(0.657)	(0.161)	(0.363)	(1.833)
	[0.183]	[0.026]	[0.942]	[0.103]	[0.344]	[1.044]
Days*Ban	-0.011	0.005***	-0.0460	-0.004	-0.001	-0.017
	(0,14)	(0,04)	(0.033)	(0.008)	(0.018)	(0.092)
	[0.009]	[0.002]	[0.041]	[0.004]	[0.018]	[0.054]
Days^2*Ban	0.0001*	-0.00004***	0.0004	0.00001	0.0001	0.0003
	(0.00006)	(0.00002)	(0.0003)	(0.0001)	(0.0002)	(0.0008)
	[0.00008]	[0.00001]	[0.0003]	[0.00001]	[0.0002]	[0.0005]
Density of Population*Ban			-0.0001	0.0002***	-0.0003*	0.0025***
			(0.0002)	(0.00006)	(0.0001)	(0.0007)
			[0.0002]	[0.00002]	[0.0002]	[0.0004]
Δ ilegal firearms confiscated*Ban			-1.307	0.246	-0.694	3.093
			(1.685)	(0.412)	(0.931)	(4.698)
			[1.270]	[0.188]	[1.190]	[2.972]
Δ legal firearms confiscated*Ban			-1.612	0.587***	-2.293***	-7.125
			(2.751)	(0.673)	(1.519)	(7.668)
			[1.846]	[0.260]	[0.958]	[4.405]
Antioquia	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.018	0.011	-0.200*	-0.032	-0.069	0.329
	(0.035)	(0.001)	(0.127)	(0.031)	(0.070)	(0.354)
	[0.021]	[0.007]	[0.145]	[0.044]	[0.079]	[0.464]
Observations	33	33	33	33	33	33
F Statistic without Antioquia (p value)	0,50	0,04	0,61	0,00001	0,093	0,0001
R-squared	0.093	0,3	0.471	0.743	0.375	0.645
Marginal Effect of Ban at mean of variables	-0.013	0.025**	-0.03	0.0014	0.018	0.32
	[0.045]	[0.013]	(0,17)	(0,047)	(0,10)	0,51
[90%Conf.Interval]	[-0.09 0.06]	[0.003 0 .047]	[-0.33 0.26]	[-0.08 0.08]	[-0.14 0.18]	[-0.51 1.15]
Normality test by D'Agostino, Balanger, and D'Agostino Jr. (p value)	0,0001	0,0001	0,003	0,092	0,17	0,0001
Breusch-Pagan Homoskedasticity test (p value)	0,0009	0,0004	0,87	0,83	0,97	0,57

Standard errors in parentheses
Robust Standard errors in brackets
*** p<0.05, ** p<0.1, * p<0.15

We see in the table that there is no difference in illegal firearms confiscated between treated and control departments. Actually there is only marginal effect on legal guns but its magnitude is very small: treated departments confiscate a quarter of a gun daily on average more than untreated departments. In terms of violent outcomes we do not find a negative marginal effect in any of the outcomes. This gives us some confidence and more credibility that our previous results are not spurious.

9. CONCLUSIONS

We have developed a simple theoretical model of the way gun carriers interact consistent to some basic elements of the economics of crime approach due to Becker (1995) and show that a gun ban decreases unambiguously gun armed delinquents while only weakly reducing the amount of armed workers. With this model we conjecture that a ban on firearms increases the carrying costs for illegal and legal carriers generating a drop in the amount of gun injuries (fatal and non-fatal) in matches between workers and delinquents that are armed with firearms. We then perform a quantitative assessment of a nation-wide ban at the department level in Colombia, during November of 2009 to January of 2010, on gun carrying permits in terms of its potential for armed violence reduction. The results go in line with our theoretical predictions and show that such type of interventions have a promising effect on affecting violence and on improving the protection of individuals against armed violence risks. We find a significant drop of about 23% and 53% respectively on changes in gun homicides and gun injuries in the treated departments relative to the control departments during the gun ban. Such large positive effects rely on enforcement efforts that confiscates mainly illegal guns. These findings suggest that the key for violence reduction in these type of policies seems to rest on continuous authority enforcement of the ban and, of course, on its publicity.

We also found that these positive effects diminish with time, namely the reduction in gun homicides deteriorates after 41 days but gun injuries do not seem to be attenuated by time. Hence, another key policy recommendation that can be extracted from the exercise is the fact that these bans need to be temporal, since its effects seem to lose steam with the passing of time. Concentrating gun control programmes on high violence seasons and accompanying them with continuous enforcement seem to be the key factors for success in terms of armed violence reduction.

This initial exercise opens an interesting research agenda for the future. Most likely, the positive effect found relies on the fact that law enforcement and gun control is temporarily strengthened thanks to the ban, thus increasing the costs of illegal gun availability for violent criminals. Investigating and exploiting the disaggregation of the information further at the municipality level might yield more policy relevant conclusions for violence reduction.

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