Operation Ceasefire: Violent Crime, Mechanism Design, and the Option Value of Change

by

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Abstract

We provide a case study of Operation Ceasefire, a homicide reduction program initiated in Boston in the mid 1990s, adopted in several dozen US cities. We argue that it provides a practical illustration of Divide-and-Conquer schemes that have received significant interest in the mechanism design literature. We find no unconditional effect of Operation Ceasefire on homicide rates, but consistently with theory, we argue that it should be valued as a real option. We test for the real option value of the program using a placebo approach, and argue that Operation Ceasefire indeed has significant option value.

1 Introduction

This paper provides a case study of Operation Ceasefire, a homicide reduction program initiated in Boston in the mid 1990s, adopted in several dozen US cities, and vividly described in Kennedy (2011). We argue that Operation Ceasefire provides a practical illustration of Divide-and-Conquer mechanisms (Abreu and Matsushima, 1992, Segal, 2003, Winter, 2004). By focusing a disproportionate amount of police resources to the first crime taking place after a specific date, Operation Ceasefire makes it iteratively dominant for gangs to refrain from committing crimes: no gang wants to be the first mover.

We provide a simple model of Operation Ceasefire. A police department (PD) seeks to reduce gang related homicides, and decides on how to allocate a marginal unit of resources. It can assign the unit to general policing, thereby reducing non-gang violence while maintaining the ability to investigate a *randomly selected* gang-related homicide. Alternatively, it can assign the unit of resources to a prioritized enforcement scheme which allows it to investigate the *first* gang-related homicide. Absent other frictions, in equilibrium, prioritized enforcement induces gangs to entirely refrain from violence.

Following the account of Kennedy (2011), we highlight that actually following through with prioritized enforcement is a non-trivial exercise. In practice, it often requires the commitment of several entities, including local and federal law enforcement, as well as District and US Attorneys. In the presence of frictions, whether prioritized enforcement improves over general policing becomes uncertain. We emphasize the option value of change: if a PD's ability to execute on prioritized enforcement is persistent, it may be worthwhile for the PD to experiment with the scheme even if the short-term expected return is negative.

We then seek to evaluate the impact of Operation Ceasefire. Exploiting documentation maintained by the National Network for Safe Communities, as well as systematic internet searches, we manually reconstruct a timeline of counties experimenting with Operation Ceasefire and related schemes over the last 25 years. We match this data with monthly level homicide data from Supplementary Homicides Reports and demographics from Bureau of Labor Statistics and Bureau of Economic Analysis. A preliminary evaluation of the impact of Operation Ceasefire using a difference-in-differences approach suggests that it had a large effect on homicide rates, consistent with previous evaluations performed in selected contexts (Braga et al., 2001, 2008, Braga and Bond, 2008, Braga et al., 2014). However, the data suggests that counties are more likely to experiment with Operation Ceasefire when homicide rates are high and rising. The decision to experiment is not exogenous, and the positive measured impact of Operation Ceasefire may be due to return to the mean following a positive shock in the homicide rate. To address this issue, we construct individual control groups for each treatment county, matching the path of the homicide rate over time. A difference-in-differences estimate of treatment effects including (county, year) fixed effects finds no significant impact of Operation Ceasefire on the homicide rate.

In accordance to the theory, we then investigate the possible option value of Operation Ceasefire. We show that difference-in-differences estimates are positively correlated over time at the matching group level, suggesting that there may indeed be a positive option value to Operation Ceasefire. To address the potential bias due to auto-correlation in the homicide rate, we perform the following placebo test: we compare the naïve option value of treatment obtained using treatment counties to the one obtained excluding treatment counties, and using a random county in each matching group as a placebo treatment group. We find that the option-value of treatment is much greater using actual treatment counties than the one obtained using placebo treatment counties.

This paper provides what we believe is the first evaluation of a divide-and-conquer mechanism in the field. This class of incentive schemes has received extensive attention from the mechanism design literature (Abreu and Matsushima, 1992, Segal, 2003, Winter, 2004, Halac et al., 2019, 2020) in the context of full implementation, i.e. implementation of socially desirable outcomes in *all* equilibria. Recently Chassang et al. (2020) provides laboratory evidence on the effectiveness of such mechanisms in the context of tax collection. It emphasizes bounded rationality frictions on play and the role of information in simplifying players' optimal choice of play.

The paper also contributes to a large body of theoretical and empirical work on police deterrence along the lines initiated by Becker (1968). It provides a theory of focused enforcement that complements the work of Eeckhout et al. (2010) on random focused crackdowns. It corroborates while qualifying past evaluations of Operation Ceasefire focusing on more selected cases (Braga et al., 2001, 2008, Braga and Bond, 2008, Braga et al., 2014).

Finally, we hope that the paper serves to popularize the use of divide-and-conquer schemes as a policy tool, beyond its application to homicide reduction. In principle, as Chassang et al. (2020) shows, this class of mechanisms can be used to enhance the effectiveness of limited government capacity in varied settings. Recent empirical work has shown that sensible implementations of sophisticated governance mechanisms can be quite impactful, even in developing countries (Duflo et al., 2013, Pomeranz, 2015, Banerjee et al., 2018, Fuchs et al., 2018). We believe that divide-and-conquer mechanisms fall in that category.

2 Operation Ceasefire

Operation Ceasefire was initiated in Boston in 1995, largely because homicide rates that had risen rapidly during the crack epidemic of the late 1980s were not declining fast enough. The program was led by David Kennedy, Anne Piehl and Anthony Braga, in partnership between the Boston Police Department, the Harvard Kennedy School, and numerous community stakeholders, including parole officers, community outreach organizations, and members of the District Attorney's office. Policies developed during this initial effort have since spread to several dozen cities and counties across the US. Kennedy (2011) provides a vivid description of the program, and the high and lows of its adoption.

The program was predicated on the following surprising realizations. First, the Boston police had fairly confident guesses of who had committed each homicide. Out of 155 victims under the age of 21, 125 (i.e. 80%) had a known or associated killer. Second, a relatively small number of organized groups were responsible for the majority of murders: 60% of homicides were assigned to one of 61 gangs operating in Boston at the time. Finally, gang members made up the majority of both offenders and victims.

In spite of this information, police investigation and judicial pursuits were not an effective deterrent. Cases infrequently led to significant jail time. One difficulty is that generating convictions that stick requires extensive work. Without sufficient evidence a District, or US Attorney is unlikely to take on the case, let alone generate a conviction leading to real prison time. Successful convictions often require coordination between local police departments, federal law enforcement, as well as local and federal district attorneys. Building up such cases requires resources that become stretched thin in periods of high crime. As a result, gang members operate under perceived impunity, or worse, under the impression that law enforcement and the community simply do not care about gang on gang violence.

Operation Ceasefire consisted of the following steps:

1. Members of different gangs were brought together for a “call-in”, often at the behest of trusted parties, including family members, parole officers, and community leaders.[1]

2. Police publicly established that they were able to associate crimes with gangs.

3. Police and District Attorneys committed to a plausible promise: that they would allocate a disproportionately large amount of resources to getting convictions against gangs responsible for the next several homicides.

In his account, Kennedy (2011) relates a comment by then lieutenant Gary French identifying the strategic importance of assigning disproportionate resources to the first few homicides following the call-in:

[1]The working team of the original Boston Operation Ceasefire also met some particular group members individually and delivered individualized warnings after call-ins (Kennedy and Friedrich, 2014). Such individual meetings are commonly referred to as custom notifications. A few replications of Operation Ceasefire, for example, the Gun Involved Violence Elimination initiative in Buffalo, NY, relied solely on custom notifications, while the others only used them as a supplementary communication method for call-ins.

> "I remember saying to you six or seven months ago, we're responding all the time, we may not be able to stay in one area for long. [...] But now that we're not just putting out fires all the time, the guys really want something to do, we can do it right."

3 Theoretical Framework

We now reinterpret this account of Operation Ceasefire through the lens of a formal model highlighting the mechanics of divide-and-conquer. The goal is to clarify policy elements that matter, as well as delineate sources of heterogeneous treatment-effects across locations and across time.

3.1 Model

Players, actions and timing. A police department (PD) seeks to reduce intentional and unintentional homicides. Several gangs indexed by $i \in \{1, \cdots, N\}$, potentially benefit from committing intentional homicides. Along the lines of Becker (1968), gangs rationally weigh the pros and cons of crime before taking action.

The interaction between the police and gangs takes place over three main stages:

- In stage 1, the PD can control the allocation of a marginal unit of resources. The PD can either assign resources to fund a prioritized enforcement task-force (corresponding to Operation Ceasefire), or to general policing effort. The PD does not have perfect commitment.

- Stage 2 takes place over time $t \in [0, 1]$. Each gang i is able to commit a homicide at a uniformly drawn time $t_i \in [0, 1]$, independently drawn across gangs. The number of homicides committed H_t is publicly observable in real time. For simplicity, we assume that the timing of moves is common knowledge among gangs.

- In stage 3, investigation takes place. If the PD assigned resources to the prioritized enforcement task-force, the PD has the capacity to investigate one *specific* homicide. With probability $\rho \in (0, 1)$ the PD maintains commitment and investigates the first homicide that is committed. With probability $1-\rho$ the PD loses commitment and has the option to reassign resources to general policing, at some efficiency loss.

Payoffs. The PD seeks to minimize violence. It is risk neutral and its payoff takes the form

$$U_{PD} = -H - K \tag{1}$$

where H is the number of intentional homicides committed in stage 2, and $K \in \{K_L, K_M, K_H\}$ is the number of unintentional homicides.

If the PD assigns its marginal resources to general policing in stage 1, then the number of unintentional homicides is $K = K_L < K_M < K_H$. If instead the PD assigns resources to prioritized enforcement in stages 1 and 3, then $K = K_H$. If the PD assigns resources to prioritized enforcement in stage 1 but reassigns them to general policing in stage 3, then $K = K_M$.

In turn, gang i gets a payoff

$$U_i = \pi\delta_i - Dp_i \tag{2}$$

where $\pi > 0$ denotes the benefit of crime, $\delta_i \in \{0, 1\}$ is equal to 1 if the gang commits a homicide, $D > 0$ denotes the cost of being successfully prosecuted, and $p_i \in \{0, 1\}$ is equal to 1 if the gang is successfully prosecuted.[2]

Investigation and prosecution. If the PD remains committed to prioritized enforcement in Stage 3, it is able to investigate a specific homicide: the first one committed, regardless of the gang. The investigation allows the PD to identify the gang responsible with probability

[2]The analysis would be unchanged if gang payoffs reflected the fact that they are frequently the victims of other gang homicides.

$\lambda \in [0, 1]$. Once the responsible gang is identified, it is successfully prosecuted by the district attorney with probability μ.

If the PD assigns resources to general policing (whether in stage 1 or stage 3), it is able to investigate a *randomly* selected homicide. As in the case of prioritized enforcement, the responsible gang is identified with probability $\lambda \in [0, 1]$ and successfully prosecuted with probability μ.

Uncertain commitment. The principal's ex ante belief that stage 1 commitments will hold in stage 3 is set to ρ. In stage 2, gangs observe a joint signal of the principal's commitment. For simplicity, we assume that the signal is perfect: with probability ρ, gangs learn that the PD's commitment will hold, with probability $1 - \rho$, gangs learn that the PD's commitment won't hold.

3.2 Analysis

Gang behavior. We first study the behavior of gangs conditional on their expectation over the behavior of the PD in stage 3.

Assumption 1. *We assume that*

$$\lambda\mu D > \pi, \quad \textit{and} \quad \frac{\lambda\mu D}{N} < \pi.$$

In words, deterrence is effective against gangs who expect their crimes to be investigated for sure, but not against gangs expecting to be investigated at random.

Proposition 1 (homicides absent commitment)**.** *Assume that gangs anticipate the PD not to follow prioritized enforcement in stage 3. Then there exists a unique perfect Bayesian equilibrium: all gangs commit an intentional homicide.*

Proposition 2 (homicides given commitment)**.** *Assume that gangs anticipate the PD to* *follow prioritized enforcement in stage 3. Then the unique rationalizable behavior of gangs is to refrain from committing intentional homicides.*

Treatment effects and optimal policy. Altogether, the behavior of gangs leads to treatment effects described by Table 1. This yields the following optimal choice for the PD.

PD policy	unintentional hom.	intentional hom.	total hom.
commits in stages 1 and 3	K_H	0	K_H
commits in stage 1 but not 3	K_M	N	$K_M + N$
never commits	K_L	N	$K_L + N$

Table 1: Outcomes depending on PD's ability to meet its promises

Proposition 3. *It is optimal for the PD to assign its marginal unit to prioritized enforcement if and only if*

$$\rho K_H + (1-\rho)K_M - K_L < \rho N. \tag{3}$$

Note that even if (3) holds, a sufficiently risk-averse PD would choose to assign resources to general policing.

Condition (3) implies that

$$K_H < K_L + N < K_M + N. \tag{4}$$

Furthermore the expected number of homicides conditional on stage 1 commitments satisfies

$$\begin{aligned} \mathbb{E}[H + K|\text{general policing}] &= N + K_L \\ &> (1-\rho)N + \rho K_H + (1-\rho)K_M = \mathbb{E}[H + K|\text{prioritized enforcement}] \end{aligned} \tag{5}$$

Together equations (4) and (5) imply that when it is chosen, prioritized enforcement increases the uncertainty over the number of homicides but reduces the average number of homicides in expectation.

3.3 Repeated interaction and the option value of change

Assume that the PD and gangs interact over T periods (each consisting of the three stages discussed above), and that the PD's commitment ability is fixed over this time. I.e., if the police department is revealed to have commitment power, then it maintains commitment power in all subsequent periods.

In this setting committing to a prioritized enforcement program creates an option value. If the program works in its first period of implementation, then it is optimal for the PD to maintain a prioritized enforcement scheme. If instead the program does not work, the PD can assign resources to general policing in future period.

Proposition 4. *Under Assumption 1, it is optimal for the PD to commit to prioritized enforcement if and only if*

$$\rho K_H + (1-\rho)K_M - K_L < \rho N + (T-1)\rho(N + K_L - K_H). \tag{6}$$

Term $(T-1)\rho(N + K_L - K_H)$ corresponds to the option value of change.

We now investigate empirically the impact of Operation Ceasefire.

4 Data

This section discusses our data sources. We first describe our sources of homicide and demographic data. We then describe our process for identifying treatment counties.

4.1 Homicide data

We use intentional homicide data from the Supplementary Homicide Reports (SHR) database covering the period 1990-2018. The unit of analysis in the database is the homicide incident defined by the Uniform Crime Reporting (UCR) Program. Because county codes are frequently missing in the SHR database, we associate each incident with the unique seven-digit Originating Agency Identifier (ORI) of its reporting law enforcement agency and locate the county associated to the agency based on its county Federal Information Processing Standards (FIPS) code and state FIPS code contained in the *Law Enforcement Agency Identifiers Crosswalk, 2012* file.[3] We sum up the numbers of homicides in a county in a given month to obtain county-level monthly data. The county-level annual data are the yearly sums of the county-level monthly data.

We obtain 1990-2018 county-level annual unemployment rate data from Bureau of Labor Statistics (BLS) and population and per capita personal income data from Bureau of Economic Analysis (BEA). We merge datasets according to county and state FIPS codes.

4.2 Treatment assignment

The main difficulty in creating a timeline of counties experimenting with policies similar to Operation Ceasefire is that there is no pre-existing centralized database. We base our data collection effort on three sources:

- A list of jurisdiction members of the National Network for Safe Communities, an organization supporting the continued expansion of the original Operation Ceasefire experiment.

- Books and articles, including Kennedy (2011) and the articles it cites.

[3]We build a county identifier for every jurisdiction in our manually compiled program list using a combination of its corresponding county FIPS code and state FIPS code. For jurisdictions lying in more than one county, we first identify the law enforcement agencies that serve them based on their place FIPS code and the crosswalk. Only for New York City do the related law enforcement agencies lie in multiple counties. Given that almost all homicide incidents in five counties in NYC are reported by agencies in New York County, homicide incidents in the SHR database are inseparable between counties. Therefore, we build the county identifier for NYC using the county FIPS code and state FIPS code of New York County, while the homicide and demographic data corresponding to the county identifier are aggregated to city-level. In the other cases, the unique county identifier is a combination of the county FIPS code and state FIPS code of the county where the agencies locate.

- Systematic internet searches.

We are interested in programs that satisfy one of the following two standards: (1) It is modeled on Boston Operation Ceasefire and emphasizes focused deterrence; (2) It used at least one of the two methods of communication between the program working teams and the targeted individuals explained in National Network for Safe Communities (2016).[4] The two communication methods are call-ins and custom notifications. Different references name call-ins and custom notifications differently. We regard "notifications," "forums," "meetings," "orientations," and "sessions" between a program working team and multiple targeted individuals as synonyms of call-ins and "customized notifications" as a synonym of custom notifications.

Our initial list of jurisdictions consists of listed member jurisdictions of National Network for Safe Communities (NNSC) excluding native American communities and cities that implemented a program of our interest based on Kennedy (2011), NNSC official websites, and the first page Google search results using "operation ceasefire," "group violence intervention," or "David Kennedy" as the searching term.[5] For each jurisdiction, we do three rounds of searches using the combination of the jurisdiction name and "operation ceasefire," "group violence intervention," or "David Kennedy." We check every link on the first page of the Google search results and conduct supplementary search for every program that appears to

[4]Boston Gun Project, Boston Miracle, and Group Violence Intervention (GVI) are regarded as synonyms in deciding the model that a program is based on. Note Drug Market Intervention (DMI) is another focused deterrence strategy designed by David Kennedy that focuses on eliminating overt drug markets partly by arresting violent drug dealers immediately and warning nonviolent drug dealers that future drug dealing will activate suspended criminal cases (National Network for Safe Communities, 2015). Since it also roots in Boston Operation Ceasefire, a program that follows the DMI model and primarily focuses on drug dealing is sometimes also reported as being modeled on Boston Operation Ceasefire. However, such a program is not of our interest here. Similarly, a DMI based program focusing on drug dealing that used call-ins, custom notifications, or both is not of our interest.

[5]See https://web.archive.org/web/20190331174953/https://nnscommunities.org/our-work/strategy/group-violence-intervention, National Network for Safe Communities (2016), and http://johnjay.jjay.cuny.edu/newsroom/2666.php for lists of member jurisdictions of NNSC. See https://nnscommunities.org/strategies/group-violence-intervention/ and https://web.archive.org/web/20190331174953/https://nnscommunities.org/our-work/strategy/group-violence-intervention for NNSC official websites. We compile the initial list in February 2020.

be of our interest but does not have detailed descriptions on all the first page search results to decide whether it implemented at least one of the two communication methods explained in National Network for Safe Communities (2016). While searching about the initial list of jurisdictions, we also add additional cities when they are mentioned as having implemented a program of our interest. Notice that if multiple cities implemented a program based on several crime reduction models, we add a city into the search list only if we encounter evidence showing that it implemented a version of the program that meets at least one of the above two standards.

The resulting list includes 113 jurisdictions in 35 states and 115 programs. We quantify the similarity between each recorded program and the original Boston Operation Ceasefire by hand-coding individual characteristics of each program, reflecting the role played by law enforcement agencies, community partners, and social service providers in the program.[6]

Our primary classification of treatment counties consists of counties with at least one program between 1996 and 2018 that used call-ins, custom notifications, or both as their method of communication. The annual number of homicides is zero in Alabama, Florida, Iowa, Kansas, Maine, Montana, New Hampshire, and Wisconsin for at least one year between 1990 and 2018, so we limit our analysis to counties in states other than them. We exclude the District of Columbia because of the same reason. In the end, there are 62 treatment counties. Excluding those that appear in our program list but implemented neither of the two communication methods and those that have incomplete demographic data, remaining non-treatment counties consist of 2336 counties that have county-level annual homicide and demographic data for all years between 1990 and 2018.

As Table 2 shows, treatment counties have higher average crime rates and larger population than non-treatment counties. This suggests that the decision to implement a program along the lines of Operation Ceasefire is not random: it is implemented in high crime coun-

[6]We start to compile the program list and build the evaluation matrix in March 2020 and finish it in September 2020.

ties, possibly after a spike in homicide rates. To control for the dynamics of pre-treatment homicide rates, we match each treatment county with 6 control counties, selected to match their homicide rates over the five years previous to treatment. Figure 1 illustrates the pre-treatment fit between treatment and control homicide rates previous to treatment. We refer to a tuple of treatment and associated control groups as matching groups.

	1^{st}-degree homicide-rate[7]	population
treatment counties	.81	970 000
all other counties	.27	80 000
control counties	.80	220 000

Table 2: Summary statistics.

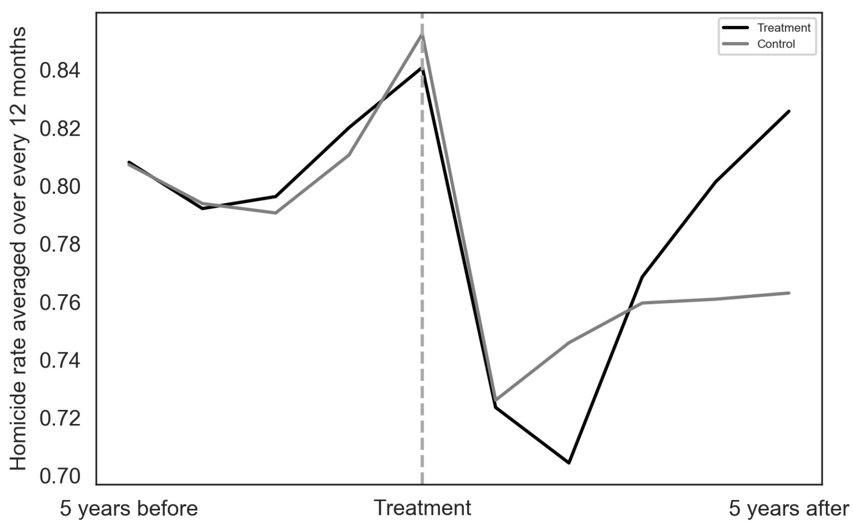

Figure 1: Treatment and control counties, pre-treatment fit.

[7]The homicide rate corresponds to monthly cases per 100 000. Treatment and controls means correspond to 5 year means before treatment.

5 Findings

5.1 Aggregate treatment effects

We first estimate short-term and long-term treatment effects on pooled data across different matching groups. Our different specifications for the analysis take the form summarized in (7). The variable treatment_12 is dummy equal to 1 for treatment counties in the first 12 months following treatment. The variable treatment_48 is dummy equal to 1 for treatment counties in the next 48 months (i.e. months 13 to 60 following treatment). We focus on the first implementation of prioritized enforcement in each treatment county and consider two specifications of fixed effects. A first specification includes county fixed-effects, year fixed-effects and county trends. A second specification includes (match group, year) fixed effects. Table 1 reports our findings. While our first specification estimates that treatment yielded a significant and meaningfully large reduction in treatment effects, this finding is not confirmed by our second specification.

$$\mathsf{homicide\_rate} \sim \mathsf{treatment\_12} + \mathsf{treatment\_48} + \mathsf{fe} \tag{7}$$

	1^{st}-degree homicide-rate[8] (std, p-value)	
Treatment first 12 months	-0.0476	-0.0223
	(0.0182, 0.0087)	(0.0324, 0.4914)
Treatment next 48 months	-0.0723	0.0368
	(0.0115, 0.0000)	(0.0211, 0.0817)
County fixed-effects	✓	✗
Year fixed-effects	✓	✗
County trends	✓	✗
Match group-year fixed effects	✗	✓
Observations	70104	70104
Adj. R^2	.14	.11

Table 3: Aggregate treatment effects.

Since our second specification is better able to control for the natural dynamics of homicide rates, and the endogenous implementation of treatment, we believe that the findings reported in Table 1 cast serious doubt on the aggregate, unconditional effectiveness of Operation Ceasefire. However, as we argue in Section 3, this does not preclude Operation Ceasefire from delivering positive option value. We now investigate this possibility.

5.2 Heterogeneous treatment effects and the option value of change

A naïve estimate. We focus on the programs before 2014 that were unique in their corresponding treatment counties 60 months before and after their months of implementation. We begin by computing a naïve estimate of the option value of treatment, which we believe is biased for reasons described below. We then propose a bias adjustment based on a measure of placebo option value.

Our primary input for the analysis are short and long-run treatment effect estimates from 48 difference-in-differences regressions performed at the matching group level. This provides us with a measure of the heterogeneity in treatment effects illustrated in the left-panel of Figure 2. Moreover, it allows us to investigate the relationship between short and long-term treatment effects. As the right panel of Figure 2 illustrates, there is a strong positive correlation between short and long-term treatment effects (the correlation coefficient is equal to 0.686).

Persistent treatment-effects suggest that there may be significant option value to Operation Ceasefire. We compute the following naïve estimate of the dynamic strategy that continues to implement Operation Ceasefire if and only if the difference-in-differences estimate over the first 12 months indicates that the policy reduces the homicide rate.

$$\widehat{OptV} \equiv \frac{1}{S}\sum_{i=1}^{S}(48 \times \beta_{48,i}\mathbf{1}_{\beta_{12,i}<0} + 12 \times \beta_{12,i}) \tag{8}$$

[8] Monthly number of 1^{st} degree homicides per 100 000.

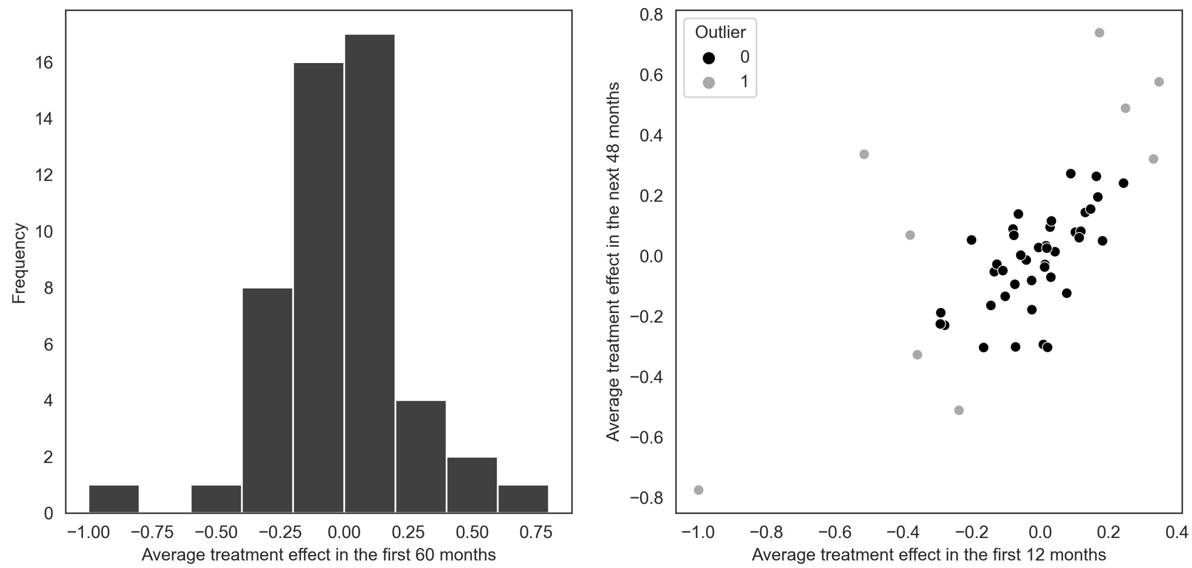

Figure 2: Distribution of treatment effects, and correlation across time

where S is the number of matching groups, and $\beta_{12,i}$, $\beta_{48,i}$ short and long-run difference-in-differences treatment effect estimates for matching group i.

Our estimate of $\widehat{OptV}$ is equal to -3.23. It is -0.05 in monthly terms, which corresponds to a 5.62% reduction in the average pre-treatment monthly homicide rate of treatment counties.

Bias. We refer to $\widehat{OptV}$ as a naïve option value because it suffers from several potential biases. First, expression 8 implicitly assumes that it is possible to interrupt Operation Ceasefire, and reallocate resources to the alternative policing strategy at no cost. Second, when low homicide rates are strongly auto-correlated over time, expression (8) may be large and negative even if treatment has no impact. Indeed, if the treatment county receives a negative idiosyncratic shock to the homicide rate relative to control groups, then this will be reflected in both β_{12} and β_{48}. As a result, persistence in estimation error will bias $\widehat{OptV}$ downward.

A placebo adjustment. We propose to adjust for bias using a placebo estimate of the naïve option value of treatment. For this purpose we exclude treatment counties from the data and select a control county to serve as a placebo treatment in each matching group. We then replicate the computation of matching-group-level short and long-run treatment effects in this data.

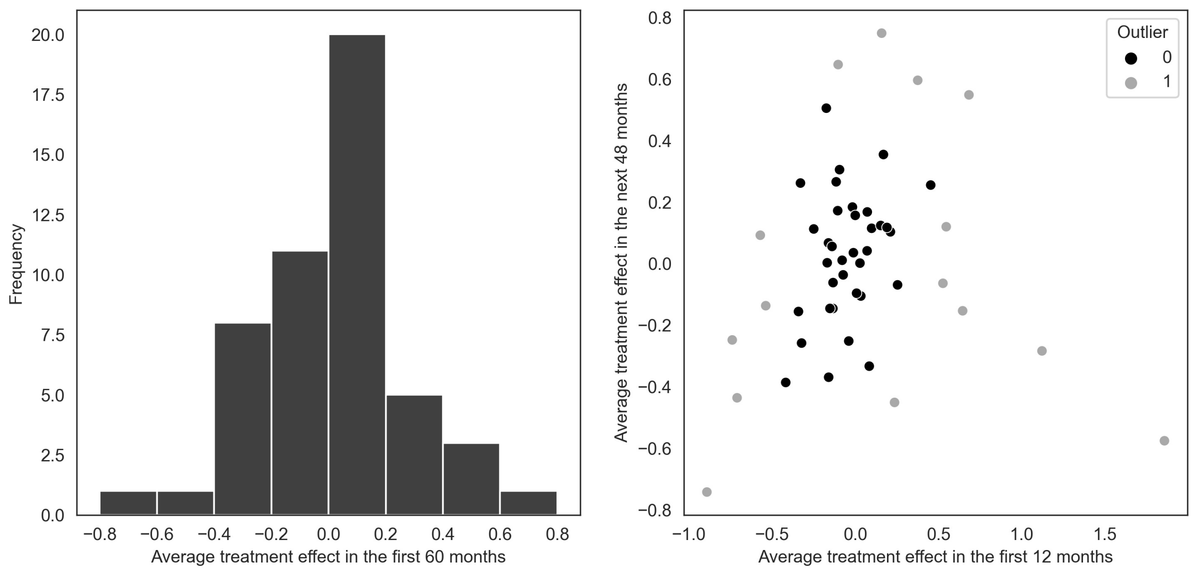

Figure 3: Distribution of placebo treatment effects, and correlation across time

Figure 3 shows that short and long-term treatment effects are much less correlated in the placebo sample, the actual correlation being equal to 0.145. In turn the placebo option value is equal to $\widehat{OptV}_{\mathsf{placebo}} = -0.493$.

This small estimated placebo option value suggests that much of the naïve option value of treatment is in fact associated to the impact of treatment.

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