# The 12.7 Million Dollar Question: What is the Effectiveness of Crime Prevention in New York City?

Final Project: Program Evaluation and Causal Inference

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# Abstract

Security is a recurrent theme in all countries around the world and especially topical in the US, where around 300 million weapons are in circulation (Neue Zürcher Zeitung (2019)). In this paper, we analyze the implementation of a program called Crisis Management System (CMS) throughout some of the roughest neighborhoods of NYC to prevent violence, especially gun violence. Using aggregated monthly crime data on a precinct level before and after the program's implementation, we find that the Crisis Management System successfully reduced weapon crimes in the treated areas, especially among individuals under the age of 25 and the Afro-American community.

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## List of Abbreviations

| | |
|---|---|
| CMS | Crisis Management System |
| CV | Cure Violence |
| DiD | Difference-in-Difference |
| NYC | New York City |
| NYPD | New York Police Department |

## 1. Introduction

Serious struggles in getting a grip on gun violence has been a long-standing challenge for New York City. In 2011, following a series of shootings, the city formed the "Task Force to Combat Gun Violence", which recommended a method called "Crisis Management System" (CMS) to prevent gun violence (Delgado et al. 2013). The CMS is an enhancement of the prominent Cure Violence (CV) method, that initially was designed to reduce shooting incidents in the roughest areas in Chicago. Put simple, the idea of both programs is that mediators, often former criminals, work closely with individuals at risk to prevent, mediate and calm down escalating situations. Their main focus lies on young individuals within their catchment area. Furthermore, both programs intend to go beyond targeting gun violence itself, but also support individuals and communities to profoundly turn their lives and norms around. However, the CMS extends the CV by providing additional community services and offering wrap around services for participants and families. (Cure Violence, n.d.; Delgado et al.,2013; NYC Office to Prevent Gun Violence, n.d.).

In 2012 and 2013 respectively, the CMS initiative was piloted within hot-spot areas in one precinct of each borough (2012: 75th, 32nd, 113th; 2013: 40th, 120th). The CMS's implementation was conducted through so called "host organizations", existing organizations that often already have had the CV program as their strategy of choice (Delgado et al.,2013). After the pilot phase, the City Council granted 12.7 million dollars to fund the initiative's expansion into another 9 precincts on the 13th of August 2014. By the time of the grant, these 14 precincts were said to be responsible for 51% of all shootings in NYC, which raised hopes for this target-orientated way of intervention to successfully reduce gun violence. (NYC Office of the Mayor, 2014).

The time has passed and still, quantitatively speaking, very little is known about the CMS's effectiveness among communities. While the pilot phase had been assessed in a mainly qualitative way (Delgado et al.,2013), to our current knowledge, the initiative has not been quantitatively studied yet. Then again, the CV program has been subject to several studies, however, resulting conflicting conclusions: While some authors find the CV method (formerly known as CeaseFire-Chicago) led to a successful decrease in violence (Skogan, Hartnett, Bump & Dubois, 2008), others find mixed or even adverse results when assessing the CV or very similar programs (Wilson & Chermak, 2011; Fox, Katz, Choate, & Hedberg, 2015).

The aim of this study is to quantitatively assess, whether the introduction of the CMS had a significant impact on violence in the treated areas. We study the largescale expansion of the initiative in 2014, using arrest data publicly available from the New York Police Department (NYPD). Our study omits the assessment of the pilot phase due to the lack of information on the exact initiation dates of the CMS in the respective precincts.

Assessing whether the implementation of a crime prevention initiative has been effective is important for the following reasons. Firstly, a successful implementation can save lives, turn around norms and increase personal as well as economic success of communities within the roughest neighborhoods. Furthermore, it is crucial to know whether the allocation of funding for crime prevention is done in an economically effective way.

## 2. Data and Methodology

The NYPD arrest dataset provides detailed information on each arrest that was executed as well as some information on the perpetrator's characteristics. For each arrest it is stated in which precinct the arrest took place, what the reason for the arrest was, as well as the date of the arrest. The information on the perpetrator contains for example sex, age-category and ethnicity.

For our analysis we focus on a time window of four years, two years before and after the CMS's expansion in August 2014. To avoid bias in our sample, we exclude all precincts that started receiving treatment already throughout the pilot phase. This leaves us with the following 9 precincts in the treatment category: 46th, 47th, 60th, 67th, 73rd, 101st and 114th precinct. To find a proxy for gun violence, we exploit the detailed raw dataset, displaying the reason for a given arrest. We classify any arrest that occurred due to "dangerous weapons" or due to "unlawful possession of weapons on school grounds" as a crime associated with gun violence. Subsequently, we aggregate the dataset such that we are left with the total number different crime types (overall crimes, weapon crimes, overall young crimes and weapon crimes among the young) committed each month in each precinct. However, the dataset does not provide any information on the overall population's characteristics in a given precinct by month. As a result, our population being studied is the aggregate of all individuals that committed a crime in the observed period of four years.

Even though the CMS mainly targets young individuals being at risk of involving in gun violence, we do not only focus on these two dimensions, but also want to investigate whether the program had any positive spillover effect on other criminal activities. We assume that individuals who are prone to be entangled in gun violence, are also very likely to commit other, possibly less severe, types of crimes. Hence, we want to test whether the CMS could potentially have far greater effects on overall crimes than its initial intention. Therefore, the focus of our analysis lies on the following four main outcome variables:

$$All\ crimes\ _{i,t} = \quad total\ number\ of\ crimes\ commited\ in\ precinct\ i\ at\ month\ t$$

$$Weapon\ crimes\ _{i,t} = \quad total\ number\ of\ \ weapon\ crimes\ commited\ in\ precinct\ i\ at\ month\ t$$

$$Youth\ crimes\ _{i,t} = \quad total\ number\ of\ crimes\ commited\ in\ precinct\ i\ at\ month\ t$$
$$by\ individuals\ below\ the\ age\ of\ 25$$

$$Youth\ weapon\ crimes\ _{i,t} = \quad total\ number\ of\ \ weapon\ crimes\ commited\ in\ precinct\ i\ at\ month\ t$$
$$by\ individuals\ below\ the\ age\ of\ 25$$

*Table 1: Variable Description*

To study the effect of the program on different ethnical groups, we define the variables like *All_crimes* or *W_crimes*, and count these crimes by precinct and month, depending on which ethnical group the offenders were associated with.

Table 1 summarizes the most important variables of our aggregated dataset by treatment and control, each before and after treatment. For more detailed summary statistics, especially concerning the crime rates by ethnical groups, we refer to our Tables 6 and 7 in the Appendix A.

**Treatment group before**

| Statistic | N | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
|---|---|---|---|---|---|---|---|
| All_crimes | 225 | 595.262 | 237.976 | 121 | 424 | 729 | 1,235 |
| W_crimes | 225 | 30.524 | 17.453 | 4 | 17 | 42 | 81 |
| Young_crimes | 225 | 216.631 | 87.749 | 52 | 151 | 265 | 461 |
| Young_W_crimes | 225 | 13.973 | 8.658 | 2 | 7 | 19 | 51 |
| **Treatment group after** | | | | | | | |
| All_crimes | 216 | 514.301 | 181.725 | 201 | 405.5 | 607.2 | 1,049 |
| W_crimes | 216 | 25.995 | 14.001 | 3 | 15 | 36 | 73 |
| Young_crimes | 216 | 173.935 | 67.989 | 66 | 127 | 208 | 405 |
| Young_W_crimes | 216 | 12.019 | 7.357 | 0 | 7 | 16 | 42 |

**Control group before**

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| All_crimes | 1,564 | 375.151 | 209.556 | 15 | 221 | 477 | 1,427 |
| W_crimes | 1,564 | 11.706 | 10.286 | 0 | 4 | 16 | 90 |
| Young_crimes | 1,564 | 126.517 | 70.056 | 2 | 74 | 161 | 424 |
| Young_W_crimes | 1,564 | 5.231 | 4.996 | 0 | 2 | 7 | 39 |

**Control group after**

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| All_crimes | 1,512 | 316.431 | 170.060 | 2 | 199 | 389 | 1,310 |
| W_crimes | 1,512 | 10.219 | 8.679 | 0 | 4 | 14 | 58 |
| Young_crimes | 1,512 | 102.122 | 57.798 | 0 | 63 | 126 | 388 |
| Young_W_crimes | 1,512 | 4.409 | 4.219 | 0 | 1 | 6 | 26 |

*Table 2: Summary Statistics Treatment and Control, Before and After*

Not only considering means but also looking at the percentiles quickly reveals that treated areas have been treated for a reason. Taking a closer look at weapon crimes and youth (weapon) crimes shows that the 75 percentiles of the control group mostly is very close to matching the 25 percentiles of the treated group in the corresponding period, before or after.

As our baseline model, we perform a Difference-in-Difference (DiD) regression as follows:

$$y_{it} = \beta_1 treatment \cdot after + \eta_t + \delta_i \qquad (1)$$

Where $\eta_t$ denotes the time fixed effects included in our regression, $\delta_i$ the precinct fixed effects and $y_{it}$ one of our four main outcome variables. Apart from analyzing overall effects, we specifically examine whether the city's program had heterogeneous impacts on different ethnical groups. The extended model we use for that is:

$$y_{itr} = \beta_1 treatment \cdot after + \eta_t + \delta_i \qquad (2)$$

Where $\eta_t$ denotes the time fixed effects included in our regression, $\delta_i$ the precinct fixed effects and $y_{itr}$ our outcome variables for each specific precinct, month and race[1]. Note that we divided the race into four sub-groups: white, black, hispanic and others.

The DiD approach compares the treatment and control groups before and after treatment in order to make a causal statement. This approach assumes that the treatment and control groups would have behaved alike, if treatment never occurred (common trend assumption). Obviously, we cannot test the common trend assumption for the hypothetical case of no treatment. However, Figure 1 compares the average number of crimes between treatment and control group and allows us to trust that the common trend assumption holds before the treatment period. Since we look at multiple outcome variables throughout all our regressions, we need to check every single outcome variable for this assumption. For illustration purposes, we only implemented one graph right below this paragraph. All the other graphs regarding the common trend assumption can be found in Appendix A. In most of the cases, these various graphs show that the common trend assumption holds, especially for our four main outcome variables.


*Figure 1: Common Trends All Crimes*

[1] Note that - on a case by case basis - race fixed effects could also have been used for the analysis. However, since the dataset only accounts for the specific crime category and since we are specifically interested in the amount of crimes committed by precinct and month, it is technically not possible to include this kind of clustering. Not aggregating our data by precinct and month, but analysing our data on a case by case basis, would leave us with a dependent variable that always takes the value 1 for crime. Hence, it does not make sense analysing the data on a case by case basis.

However, the common trend assumption does not seem to be valid at all in the cases of weapon crimes committed by white or "*other"*[2] ethnicities. As the common trend assumption clearly does not hold for these two groups we exclude them from our further analysis. The common trend assumption for weapon crimes among the Hispanics and members of the Afro-American community are also raising our doubts, however, are not as clearly violated as the stated cases just before.

In order to allow for a smoother introduction of the effect in treated areas, we additionally estimate two versions of a generalized DiD model, using interaction terms between our treatment variable and dummy variables for yearly and half-yearly periods after treatment. This is especially important in our context for the following reason: even though it is known that the government granted funding for the CMS's expansion in August 2014, it does not mean that the organizations that received the funding also implemented the strategy straight away. By estimating a generalized DiD model, we better account for potential implementation time needed by social workers and organizations.

$$y_{it} = \sum_{j=1}^{2} \beta_2 treatment \cdot year\_dummy_j + \eta_t + \delta_i \qquad (3)$$

$$y_{it} = \sum_{j=1}^{4} \beta_2 treatment \cdot half\_year\_dummy_j + \eta_t + \delta_i \qquad (4)$$

Although we know the precise precincts that received additional funding and timing of the grant, we assume that the expansion of the program happened just like the funding in the pilot phase. Namely, additional funds were granted to existing organizations involving in social work within the targeted precincts. This, however, leaves the question open whether these organizations are indeed able to capture the whole precinct they operate in with applying the CMS. We rather expect the social workers to target hot-spot areas *within* their respective precinct, which would cause us underestimating the true effect. Furthermore, it is unclear in which precincts the CV (or a similar) program was in place. Hence, we find it worthwhile mentioning that if organizations already actively engaged in social work before our treatment, our estimation does not measure the effect of a new implementation of the CMS, but rather an expansion of the social work beyond the CV or similar programs.

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[2] Note that we are referring to the category *other* from our dataset.

## 3. Results

Estimating our baseline model in equation (1) using clustered standard errors yields to no statistically significant effect in any of the four main outcome variables but youth crimes, which can be seen in Table 3. Even though, in the context of the CMS, it intuitively makes sense that crimes especially dropped among the younger population, as they were the main target of the program. However, its significance at the 10% level makes us question the robustness of the outcome. Analyzing different ethnical communities, we get a similar picture: little statistically significant effects, apart from the reduction of weapon crimes within the Afro-American community, which again is significant at the 10% level. Interestingly, we find a positive and highly significant effect among the white community, which is very counter-intuitive. However, as one can see from the summary statistics in the Appendix A, Tables 6 and 7, the white community is approximately 5 to 9 times less likely to commit crimes in the treated areas compared to the Hispanic or the Afro-American communities. Therefore, this community might be neglected by the mediators, which possibly focus more on other ethnicities and thus might be less targeted by the CMS program in the first place. Note that this intuition does not necessarily explain why we see an *increase* in white crimes but rather why there might not be a decrease.

| | *Dependent variable:* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All crimes | Weapon Crimes | Youth crimes | Youth weapon crimes | White crimes | Black crimes | Hispanic crimes | Other ethn. crimes | Black weapon crimes | Hispanic weapon crimes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment:after | -22.177 | -2.998 | $-18.165^{*}$ | -1.110 | $8.273^{***}$ | -25.877 | -7.292 | 2.719 | $-2.374^{*}$ | -0.820 |
| | (25.473) | (1.862) | (9.418) | (0.970) | (1.973) | (17.358) | (10.691) | (1.790) | (1.314) | (0.836) |
| Time Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Precinct Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Note:* | | | | | | | | | | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 |

*Table 3: DiD Estimation*

Estimating the generalized DiD models in equation (3) and (4) respectively, again using clustered standard errors, suggest statistically significant effects in some dimensions of our four main outcome variables as one can see in columns (1) to (4) of Tables 4 and 5. Even though the total number of crimes appear to be mostly consistent in terms of sign, there is no significant *steady* decrease in crimes however. We found some evidence that the CMS program decreased *weapon* crimes, as well as *overall and weapon crimes* among the *younger* population, which is the main purpose of CMS. However, the interaction terms using year or half-year dummies in column (2) and (3) respectively suggest that the CMS initiative seems to have rather long-term effects or could show its effect only months after the additional funding was granted, potentially because of implementation time needed by organizations. *In the short run*, e.g. a period length of about a year after its implementation, there is no evidence that the CMS program was successful in reducing crimes. Furthermore, we find that not all ethnical communities benefit in the same way. Analyzing the different ethnicities in the columns (5) to (10) from the Tables 4 and 5, we see similar patterns as those perceived in columns (1) to (4): some statistical significance at the 10% level for *overall* crimes but also *weapon* crimes committed by the *Afro-American community*. Yet again, there is no significant steady decrease either. Only the *white community* seems to be steadily affected by the program. However, like we already saw in our estimates of Table 2, the program has a rather opposite effect on this community, for which the reason remains unclear to us.

| | Dependent variable: | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All crimes | Weapon Crimes | Youth crimes | Youth weapon crimes | White crimes | Black crimes | Hispanic crimes | Other ethn. crimes | Black weapon crimes | Hispanic weapon crimes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| period_y1 | -5.831 | -0.929 | -11.486 | 0.326 | 8.157*** | -12.283 | -4.607 | 2.902* | -1.380 | -0.017 |
| | (20.074) | (1.569) | (7.990) | (0.839) | (2.370) | (13.914) | (7.993) | (1.600) | (1.280) | (0.599) |
| period_y2 | -38.523 | -5.067* | -24.844** | -2.547* | 8.390*** | -39.471* | -9.978 | 2.536 | -3.368* | -1.623 |
| | (32.327) | (2.601) | (11.471) | (1.408) | (1.819) | (22.202) | (13.567) | (2.213) | (1.751) | (1.192) |
| Time Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Precinct Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Note: | *p<0.1; **p<0.05; ***p<0.01 | | | | | | | | | |

*Table 4: Generalized DiD Yearly Interactions*

| | Dependent variable: | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All crimes | Weapon Crimes | Youth crimes | Youth weapon crimes | White crimes | Black crimes | Hispanic crimes | Other ethn. crimes | Black weapon crimes | Hispanic weapon crimes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| period_hy1 | -21.110 | -2.483 | -18.547** | -0.633 | 6.460*** | -24.341 | -6.661 | 3.433** | -3.261* | 0.048 |
| | (20.585) | (1.878) | (8.406) | (1.307) | (2.020) | (16.752) | (7.314) | (1.467) | (1.728) | (0.598) |
| period_hy2 | 9.448 | 0.625 | -4.426 | 1.285* | 9.854*** | -0.225 | -2.553 | 2.372 | 0.501 | -0.082 |
| | (22.739) | (1.778) | (8.996) | (0.729) | (2.900) | (13.437) | (9.518) | (1.957) | (1.095) | (0.827) |
| period_hy3 | -49.568 | -5.404** | -27.037** | -2.860** | 7.251*** | -47.664** | -11.603 | 2.449 | -3.914** | -1.238 |
| | (32.065) | (2.724) | (10.799) | (1.446) | (2.699) | (23.257) | (12.812) | (2.155) | (1.849) | (1.223) |
| period_hy4 | -27.478 | -4.729* | -22.650* | -2.233 | 9.529*** | -31.278 | -8.352 | 2.623 | -2.822 | -2.008* |
| | (34.140) | (2.582) | (12.623) | (1.389) | (2.187) | (22.165) | (14.681) | (2.396) | (1.729) | (1.189) |
| Time Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Precinct Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Note: | *p<0.1; **p<0.05; ***p<0.01 | | | | | | | | | |

*Table 5: Generalized DiD Half-Year Interactions*

The question remains on how big the effects are, when estimated by our models. For example, given the estimated coefficients in all three models, we find some evidence that the CMS program helped decrease *overall youth crimes* in the targeted precincts of NYC, as discussed before. Considering our baseline model (estimation (3) in Table (3)), we find that the CMS program reduced the total number of crimes committed by individuals under the age of 25 by around 18 crimes per month on average. Given that average number of crimes was 217 for individuals under the age of 25 *before* the CMS was implemented (see summary statistics in Table (2)), we have an idea of the effect's magnitude attributed to the program, which is about 8% ($\frac{18}{217} \approx 8.31\%$). Cumulated over a period of 2 years, the CMS initiative would reduce total crime by around 432 for individuals under 25 if the effect was persistently present. If we take an average of each of the nine treated precincts, the economic impact of CMS would be around 11% ($\frac{432}{3989} \approx 10.83\%$).

On the one hand, a program that reduces overall crimes by around 8% can objectively raise doubt on the efficiency and effectiveness of the CMS, considering that expansion of the CMS cost 12.7 million dollars. Additionally, relative to other studies, our estimated effect attributed to the program is much smaller, even though these studies look at different outcome variables than we are[3]. As we will argue in the discussion part, we will see that our estimation potentially suffers from a positive bias. Thus, we expect that the true effect of the CMS indeed could be

[3] Skogan *et al* (2008) found that the CeaseFire program had approximately an impact of a 20% decrease in shootings and killings.

more negative than we estimated with our model. On the other hand, since violence, and especially gun violence, concerns people's security, lives and prospects an effect of around 8% attributed to the program can also be viewed as a very successful step towards more peaceful communities.

Finally, a view on the crime *ratios* rather than the absolute values would certainly be another dimension worth analyzing. However, we question whether it is truly appropriate to compare gun violence with any other crime committed as severity and potential lives at risk greatly vary among offences. If the assumptions of the DiD approach are satisfied, it would indeed be possible to make a causal statement on the absolute reduction of gun violence. The relative importance and value of gun violence compared to the total number of crimes, however, rather touches the idea of a cost benefit analysis than of a causal statement.

Keeping in mind that we potentially underestimate, or estimate the lower bound of the true effect, we conclude that the CMS might have very well caused a significant decrease in crimes, especially among the younger population and regarding gun violence, affecting different communities in heterogeneous ways.

## 4. Robustness: Synthetic Control

As mentioned in the introduction section, a predecessor of the CMS program was the Cure Violence (CV) method which was implemented in Chicago around the year 2000. A study from Skogan et al (2009) used a matching approach to identify the effect of the CV method on diverse outcomes, such as "killings". In this paper, we use a similar but more modern approach to check the findings in our result section, called *synthetic control*. The key concept of using synthetic control is that we construct an artificial control group, which is a weighted average of the available non-treated precinct units, to get a reasonable estimate for our missing counterfactual. In this case, we let an algorithm give more or less weight to each precinct of our control group, depending on how similar the characteristics of the control units match with those of the treated precinct. However, note that the synthetic control approach requires only one single treatment unit. That's why we need to exclude 8 out of the 9 treated precincts from our sample when estimating our model. We decided to choose the 44th precinct as our remaining treatment unit, since weapon crimes committed by young individuals in this area are the highest among all other treated precincts during pre-treatment phase (1'325 weapon crimes committed from August 2012 to August 2014).

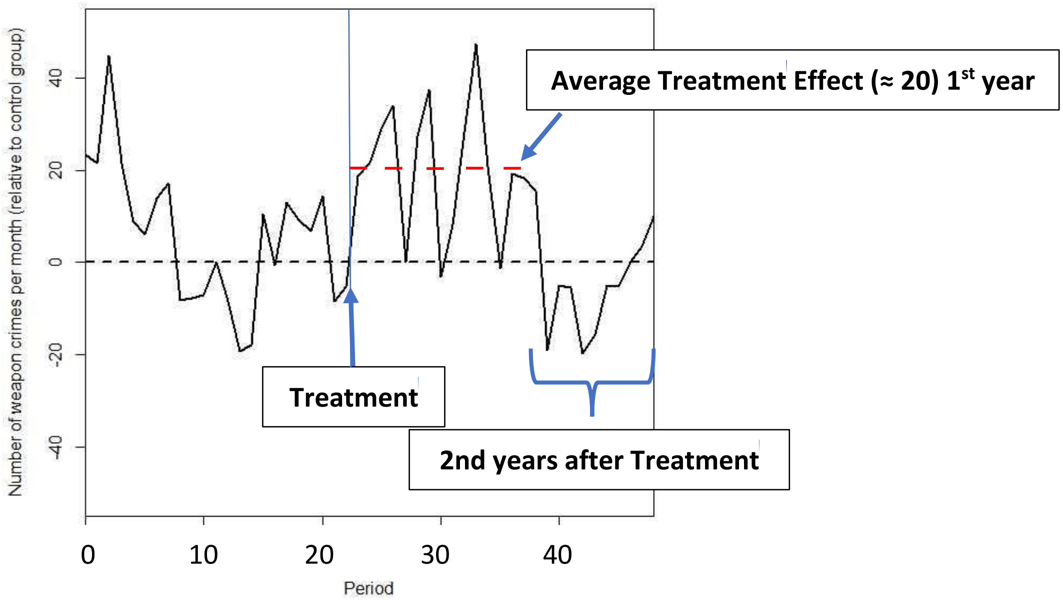


*Figure 2: Gap in number of W_crimes per month between 44th precinct and synthetic control*

Figure 2 shows how the treatment effect in the 44th precinct evolved over each month, starting in August 2012 (= period 0) and ending in August 2016 four years later. Note that treatment takes place in period 24 (= August 2014). We see that during the approximately first 12 months after treatment took place - that is until around period 36 – the number of weapon crimes committed in the 44th precinct increases relative to our control group. Eyeballing for the *average* treatment effect[4] for our treated precinct 44 in the synthetic control graph, we see that it matches the estimated coefficient $\hat{\beta}_{Treatment}$ in column (2) of Table 8 in Appendix C pretty well, where the treatment group has around 19 weapon crimes more than the control group[5]. This holds also true if we compare the average treatment effect of the 44th precinct in the 2nd year after treatment with the estimated year dummy $\hat{\beta}_{2nd\,Year}$ in the same estimation. Only the first 12 months after treatment don't match with the year dummy $\hat{\beta}_{1st\,Year}$ in column (2) of Table 9 in the Appendix, that is, we find a positive average treatment effect, when the coefficient in the regression finds a negative effect. However, since $\hat{\beta}_{1st\,Year}$ was estimated with all 9 treated precincts – whereas we only used the 44th precinct - and because the coefficient is not statistically significant in the regression, we conclude that the synthetic control still approximates our results. Finally, Figure 2 shows that the monthly treatment effects are getting

[4] Look at the dotted line in Figure 2 above, it should be around 20 weapon crimes per month (relative to the control group) for the first year.

[5] Note that we had to exclude the precinct fixed effect in order to get $\hat{\beta}_{Treatment}$. If we would use precinct fixed effects, $\hat{\beta}_{Treatment}$ would get sucked up by those.

positive approaching period 45, which suggests that the CMS program may not have had long term effects on weapon crimes.

For the sake of completion and to have a more comprehensive measure for weapon crimes, we also performed a synthetic control estimation comparing the median weapon crimes of all treated areas with the control group, to see if the CMS had a significant impact applying this method. As we can see from Figure 4 in the Appendix D, the magnitude is halved compared to the precinct 44, as now we consider the median, but the picture looks similar to our Figure 2 above. However, performing the synthetic control using the median amount of weapon crimes not necessarily allows an unambiguous conclusion.

## 5. Discussion

The difference in difference estimator gives us an approximation of the impact of the CMS program on the *treated* precincts (= average treatment effect on the treated), if the parallel trend assumption is satisfied, which is the case in our setting. Note that the program selects specifically precincts which are responsible for most of the weapon crimes in NYC. Hence, we would expect that the effect of the program is *higher* on those areas compared to precincts which have a lower amount of weapon crimes per month in the first place.

### 5.1 Internal validity and bias

Even though we account for time-invariant changes within a precinct with precinct fixed effects and time trends which affect all precincts the same (treated & untreated precincts), there might be omitted variables that are precinct specific and vary over time, leading to a potential bias of our coefficients. For example, there may be precincts in which the unemployment rates are higher than in others. If we assume that precincts with higher unemployment rates are associated with higher amounts of weapon crimes and that our treated precincts have higher unemployment rates than the non-treated precincts, we would have a *positive bias* in all our estimated coefficients. Hence, we would find even more negative coefficients by controlling for characteristics like precinct-specific unemployment rates and thus, the effect of the CMS program would reduce the number of weapon crimes even more drastically. Another example of another omitted variable would be precinct-specific poverty rates, leading also to a positive bias on our estimated (same reasoning as for the unemployment rate) coefficients. Since we only could think of examples leading to potentially positive biases, it would be consistent with our expectation and suggest that we estimated a lower bound for the effect of the CMS program on weapon crimes. Thus, because we cannot control for precinct-specific trends, even though it would be very important to account for it in our context, our study has some limitations and may not approximate the true effects of the CMS program, especially concerning the magnitude of the effect[6].

### 5.2 Spillovers

Additionally, many other studies which look at crime prevention (often enforcement-based crime prevention) are often worried about spillovers, meaning that the effect of a program in a specific area would lead to opposite effects in the neighboring areas that are not treated. In our case, the CMS program could also increase the amount of weapon crimes committed in other areas. However, we argue that since the initiative is related to social work and those workers do not chase criminals like the police does, we would not expect those criminals to run off committing crimes in other areas.

### 5.3 External validity

Since the program was implemented in NYC, we also have to think carefully about how we can extrapolate these results on other parts of the world. Most importantly, the city-specific and precinct-specific characteristics greatly matter in this context. The success of the CMS most likely depends on the general level of crimes attributed to the cities looked at, unemployment rates, poverty rates or even the cultural aspects of the cities of interest, which is why external validity might be questionable. Furthermore, since *internal* validity may not be very high, we would first need to fix this point before further investigating external validity.

[6] Skogan et al (2008) found much bigger effects while looking at a similar program in the city of Chicago. However, they used matching as their identification strategy and seem to have access to precinct-specific variables.

## 6. Conclusion

Our study found some evidence that the CMS program reduced overall weapon crimes and (weapon) crimes committed by individuals under the age of 25. The effects found differ quite a bit in magnitude from previous studies, mostly done on a similar program, as for example the CV program in Chicago (Skogan et al (2008)). Accounting for precinct-specific characteristics would help improving internal validity of the study and most certainly lead to bigger and more significant findings in favor of the effectiveness of the CMS program. However, further research would have to be done in this field to gain more insight into the true effect of non-enforcement-based strategies of crime prevention such as the CMS.

## 7. References

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# Appendix

## A) Detailed Summary Statistics

| Treatment group before | | | | | | | |
|---|---|---|---|---|---|---|---|
| Statistic | N | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
| PERP_SEX_M | 225 | 497.822 | 192.863 | 102 | 355 | 597 | 999 |
| PERP_SEX_F | 225 | 97.440 | 47.148 | 17 | 66 | 119 | 236 |
| All_crimes | 225 | 595.262 | 237.976 | 121 | 424 | 729 | 1,235 |
| W_crimes | 225 | 30.524 | 17.453 | 4 | 17 | 42 | 81 |
| Young_crimes | 225 | 216.631 | 87.749 | 52 | 151 | 265 | 461 |
| Young_W_crimes | 225 | 13.973 | 8.658 | 2 | 7 | 19 | 51 |
| White_crimes | 225 | 36.480 | 37.067 | 4 | 13 | 35 | 143 |
| Black_crimes | 225 | 355.440 | 204.109 | 64 | 194 | 489 | 1,028 |
| Hispanic_crimes | 225 | 187.462 | 145.021 | 15 | 84 | 240 | 614 |
| Other_ethn_crimes | 225 | 15.880 | 11.839 | 1 | 8 | 19 | 69 |
| White_weapon_crimes | 225 | 1.551 | 1.887 | 0 | 0 | 2 | 9 |
| Black_weapon_crimes | 225 | 18.858 | 14.153 | 0 | 8 | 26 | 69 |
| Hispanic_weapon_crimes | 225 | 9.564 | 10.286 | 0 | 3 | 11 | 54 |
| Other_ethn_weapon_crimes | 225 | 0.551 | 0.935 | 0 | 0 | 1 | 5 |

| Treatment group after | | | | | | | |
|---|---|---|---|---|---|---|---|
| Statistic | N | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
| PERP_SEX_M | 216 | 431.458 | 154.689 | 165 | 332 | 505.5 | 907 |
| PERP_SEX_F | 216 | 82.843 | 29.653 | 30 | 62.8 | 100 | 171 |
| All_crimes | 216 | 514.301 | 181.725 | 201 | 405.5 | 607.2 | 1,049 |
| W_crimes | 216 | 25.995 | 14.001 | 3 | 15 | 36 | 73 |
| Young_crimes | 216 | 173.935 | 67.989 | 66 | 127 | 208 | 405 |
| Young_W_crimes | 216 | 12.019 | 7.357 | 0 | 7 | 16 | 42 |
| White_crimes | 216 | 35.500 | 39.590 | 1 | 11 | 31.8 | 158 |
| Black_crimes | 216 | 303.597 | 161.174 | 83 | 177.8 | 408 | 773 |
| Hispanic_crimes | 216 | 159.699 | 118.199 | 18 | 78.8 | 215 | 542 |
| Other_ethn_crimes | 216 | 15.505 | 14.690 | 0 | 7 | 16 | 81 |
| White_weapon_crimes | 216 | 1.255 | 1.745 | 0 | 0 | 2 | 10 |
| Black_weapon_crimes | 216 | 15.856 | 11.679 | 1 | 7.8 | 21 | 55 |
| Hispanic_weapon_crimes | 216 | 8.454 | 8.453 | 0 | 3 | 11 | 44 |
| Other_ethn_weapon_crimes | 216 | 0.431 | 0.744 | 0 | 0 | 1 | 4 |

*Table 6: Detailed Summary Statistics Treatment Group*

**Control group before**

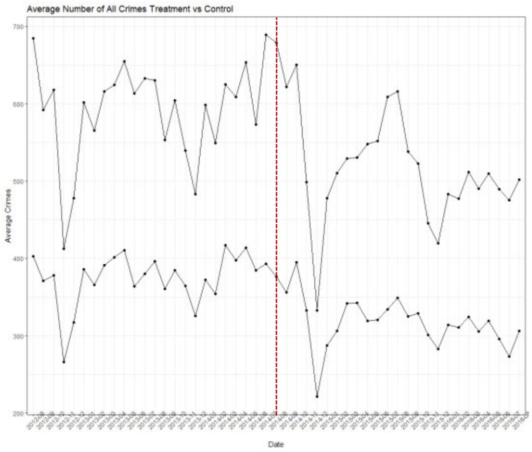
| Statistic | N | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
|---|---|---|---|---|---|---|---|
| PERP_SEX_M | 1,564 | 312.419 | 173.362 | 12 | 180 | 393.2 | 1,160 |
| PERP_SEX_F | 1,564 | 62.732 | 39.944 | 0 | 36 | 80 | 305 |
| All_crimes | 1,564 | 375.151 | 209.556 | 15 | 221 | 477 | 1,427 |
| W_crimes | 1,564 | 11.706 | 10.286 | 0 | 4 | 16 | 90 |
| Young_crimes | 1,564 | 126.517 | 70.056 | 2 | 74 | 161 | 424 |
| Young_W_crimes | 1,564 | 5.231 | 4.996 | 0 | 2 | 7 | 39 |
| White_crimes | 1,564 | 54.552 | 41.423 | 2 | 26 | 70 | 341 |
| Black_crimes | 1,564 | 162.378 | 139.154 | 1 | 55 | 232 | 737 |
| Hispanic_crimes | 1,564 | 132.527 | 110.850 | 4 | 54 | 168.2 | 555 |
| Other_ethn_crimes | 1,564 | 25.693 | 29.544 | 0 | 8 | 31 | 192 |
| White_weapon_crimes | 1,564 | 1.731 | 2.256 | 0 | 0 | 2 | 21 |
| Black_weapon_crimes | 1,564 | 5.130 | 6.577 | 0 | 1 | 7 | 64 |
| Hispanic_weapon_crimes | 1,564 | 4.276 | 5.148 | 0 | 1 | 6 | 39 |
| Other_ethn_weapon_crimes | 1,564 | 0.570 | 1.224 | 0 | 0 | 1 | 12 |

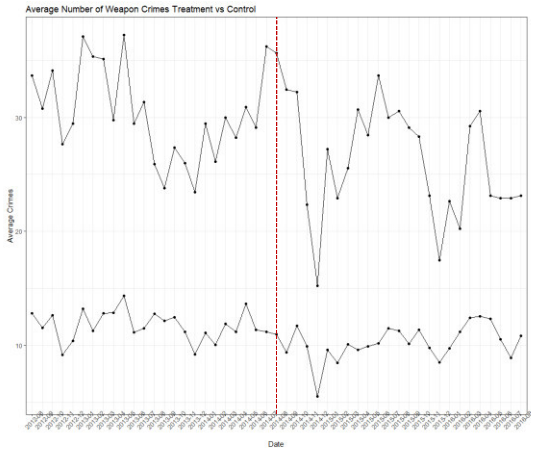
**Control group after**

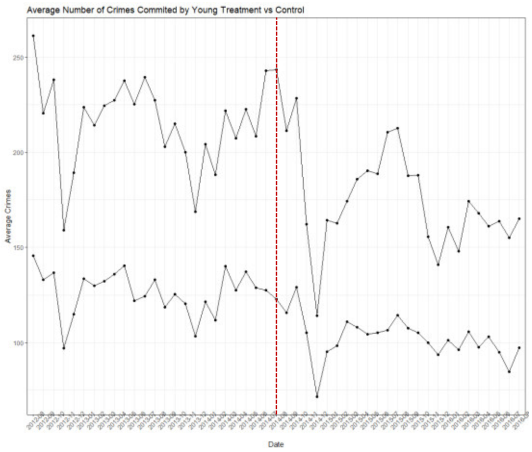
| Statistic | N | Mean | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
|---|---|---|---|---|---|---|---|
| PERP_SEX_M | 1,512 | 261.787 | 141.138 | 2 | 163 | 324.2 | 1,076 |
| PERP_SEX_F | 1,512 | 54.644 | 32.762 | 0 | 33 | 70 | 239 |
| All_crimes | 1,512 | 316.431 | 170.060 | 2 | 199 | 389 | 1,310 |
| W_crimes | 1,512 | 10.219 | 8.679 | 0 | 4 | 14 | 58 |
| Young_crimes | 1,512 | 102.122 | 57.798 | 0 | 63 | 126 | 388 |
| Young_W_crimes | 1,512 | 4.409 | 4.219 | 0 | 1 | 6 | 26 |
| White_crimes | 1,512 | 45.829 | 34.512 | 0 | 22 | 58 | 221 |
| Black_crimes | 1,512 | 136.292 | 111.353 | 0 | 49 | 202 | 630 |
| Hispanic_crimes | 1,512 | 111.796 | 89.595 | 0 | 47 | 144 | 529 |
| Other_ethn_crimes | 1,512 | 22.515 | 27.023 | 0 | 6 | 28 | 214 |
| White_weapon_crimes | 1,512 | 1.288 | 1.754 | 0 | 0 | 2 | 11 |
| Black_weapon_crimes | 1,512 | 4.528 | 5.322 | 0 | 1 | 6 | 39 |
| Hispanic_weapon_crimes | 1,512 | 3.985 | 4.861 | 0 | 1 | 5 | 30 |
| Other_ethn_weapon_crimes | 1,512 | 0.417 | 0.890 | 0 | 0 | 1 | 8 |

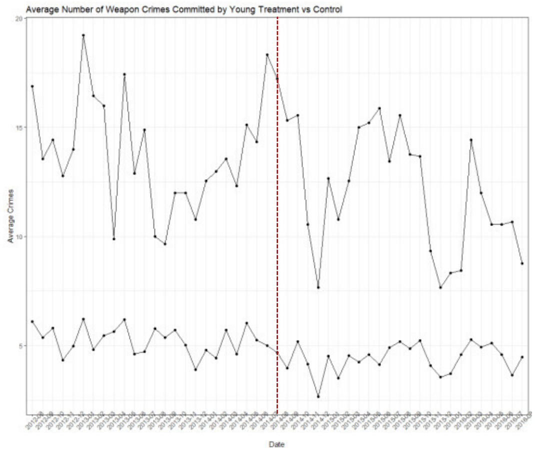
*Table 7: Detailed Summary Statistics Control Group*

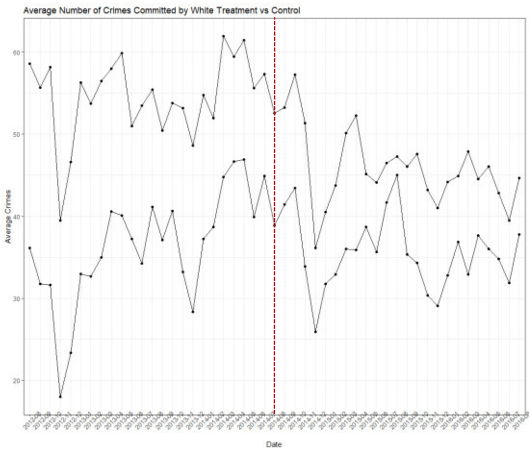
# B) Common Trends All Subgroups

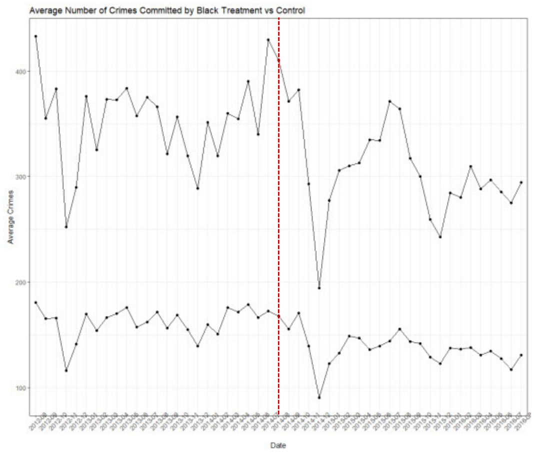


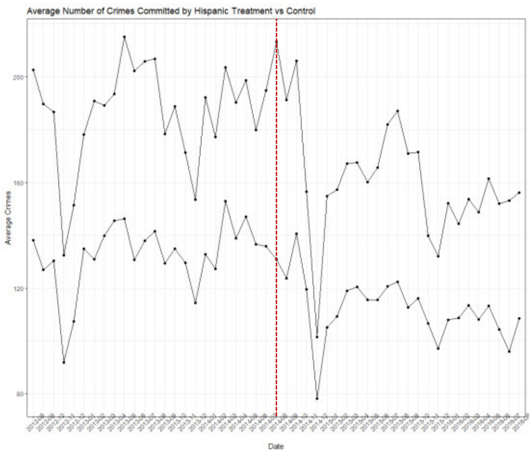



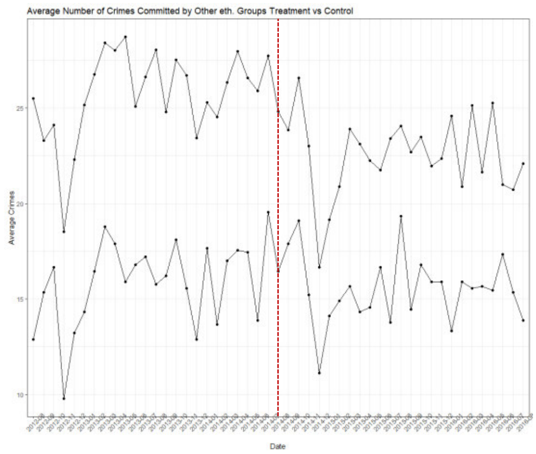



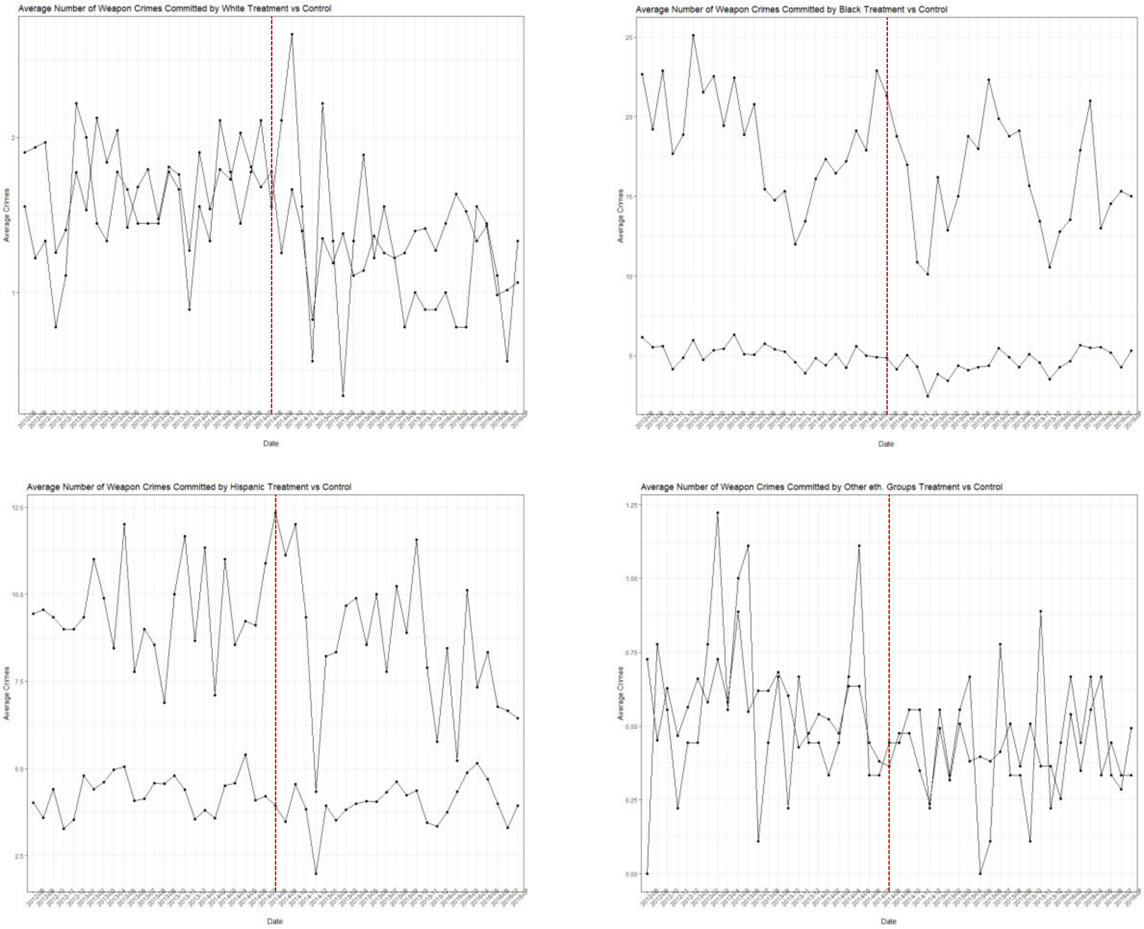



*Figure 3: Common Trends All Subgroups*

## C) Generalized DiD with Time-Fixed Effects only

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | All crimes | Weapon Crimes | Youth crimes | Youth weapon crimes |
| | (1) | (2) | (3) | (4) |
| Treatment | 220.158*** | 18.815*** | 90.339*** | 8.741*** |
| | (77.789) | (5.026) | (27.957) | (2.300) |
| period_y1 | -5.942 | -0.970 | -11.672 | 0.306 |
| | (20.130) | (1.567) | (8.014) | (0.839) |
| period_y2 | -38.634 | -5.107** | -24.802** | -2.567* |
| | (32.377) | (2.599) | (11.422) | (1.408) |
| Time Fixed Effects | Yes | Yes | Yes | Yes |
| Precinct Fixed Effects | No | No | No | No |
| *Note:* | | | | *p<0.1; **p<0.05; ***p<0.01 |

*Table 8: Generalized DiD with Half-Year Dummies & Time-Fixed Effects*

**D) Synthetic Control Median Crimes in the Treatment Group vs. Synthetic Control Group**

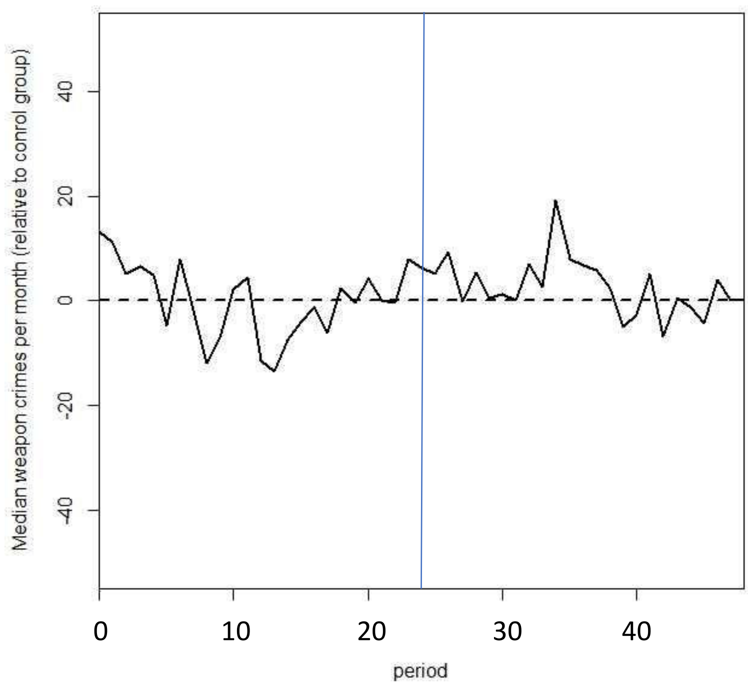



Figure 4: Gap between the median number of W_crimes in treated precincts per month and the synthetic control group