Compact Review of Difference-in-Differences

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1 Introduction

The difference-in-differences (DID) design is one of the most widely used methods for causal inferences from observational data Imbens and Wooldridge (2009) . In particular, when randomization is not possible in many cases in reality, DID is a popular approach among researchers to evaluate the impact of a new policy or a large-scale program implementation. The purpose of this paper is to provide the comprehensive understanding of the DID method by giving the compact review, from the model description to empirical application with Monte Carlo simulation. For the empirical application of the DID method, we replicate the paper, "Do police reduce crime?" by Di Tella and Schargrodsky (2004), where the authors employ a DID approach to answer the question of whether the street presence of police reduces car thefts. While our replication confirms the main results of the police deterrence effect, we extend our analysis with Monte Carlo simulation. We examine our parameter estimators under four different specifications with inclusion or exclusion of month and block fixed effects. Furthermore, we look into the case where the key assumption of DID, the parallel trend, does not hold.

The paper proceeds as follows. Section 2 gives the general description of the DID design. Section 3 introduces our examination of the paper by Di Tella and Schargrodsky (2004). In Section 4, we conduct the Monte Carlo simulation. Lastly, Section 5 gives the concluding remarks.

2 Model Description

2.1 Standard DID model

We start with the simple set up with an example to show the main ideas of DID. As the basic form, we compare two groups over two time periods. That is, we have a treatment group who receives the treatment and a control group without the treatment, over one pre-treatment and one post-treatment period. For our example, suppose that we investigate how the vaccination for a contagious disease affects the infection rate. The aim of the vaccination program is to curb the spread of the disease, thus we choose the infection rate as the outcome indicator. We look into 500 districts in two groups; State A that received the vaccination and State B without the vaccination. As the vaccination starts in baseline year t, the outcome is measured in year $t-1$

and year $t + 1$; one pre-treatment and one post-treatment period. Table 1 and Figure 1 display the outcomes of our example case study.

Figure 1: Average Infection Rate

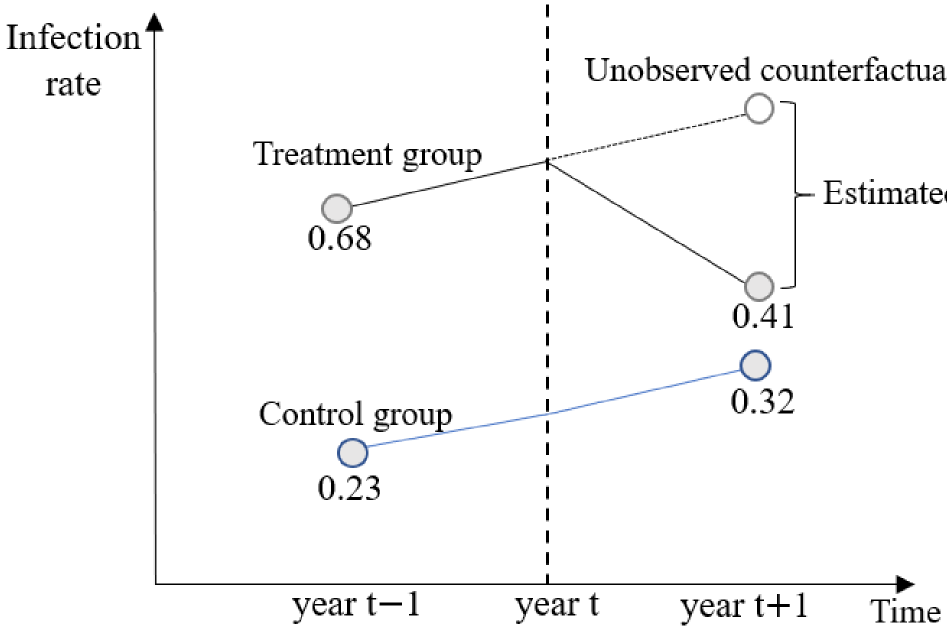


Table 1: Average Infection Rate

	Pre-vaccination	Post-vaccination	Difference
Treatment	0.68	0.41	−0.27
Control	0.23	0.32	0.09
Difference	0.45	0.09	−0.36

With Table 1, we can intuitively understand the method and compute the DID estimate. The first row contains the infection rates for State A, which is the treatment group, before the vaccination 0.68 and after the vaccination 0.41. Thus, *"the pre-post comparison"* for State A, which is *"the first difference"*, is (0.41 – 0.68). It is important to note that this pre-post comparison for the treatment group alone is not the impact of the treatment. Since this difference does not capture time-varying factors, the change in outcomes cannot be exclusively attributed to the vaccination. This is why we need *"the second difference"* to remove the time trend which confound the treatment effect. Likewise, the second row contains the outcomes for State B, which is the control group, and *"the second difference"* is $(0.32 - 0.23)$. Finally, by subtracting these two differences, we can obtain the DID estimate $(0.41 - 0.68) - (0.32 - 0.23) = -0.36$ as the difference between the pre-post difference in outcomes for the treatment group, $(0.41 - 0.68)$, and the pre-post difference for the control group, (0.32 – 0.23). This DID estimate could be named as a DID estimator. By removing time-varying factors with the subtraction of two differences, the DID estimator eliminates a potential source of bias and shows the treatment effect.

Based on Table 1, we can write the equation regression (1) with the following notations. Let Y_{it} denote the outcome for the unit i measured at the time t. As an indicator of the treatment group, X_i is a dummy variable with $X_i = 1$ for the state with the treatment and $X_i = 0$ for the state without the treatment. As an indicator of the post-treatment time period, T_t is a dummy variable with $T_t = 0$ for the pre-treatment period and $T_t = 1$ for the post-treatment period. Lastly, D_{it} denotes a treatment dummy so that $D_{it} = 1$ if the unit i is treated in time t, and $D_{it} = 0$ otherwise, which makes D_{it} identical to an interaction dummy $X_i * T_t$.

$$Y_{it} = \beta_0 + \beta_1 X_i + \beta_2 T_t + \theta D_{it} + \epsilon_{it} \tag{1}$$

Table 2: DID coefficients

	Pre-treatment	Post-treatment	Difference
Treatment	$\boldsymbol{\beta}_0$+$\boldsymbol{\beta}_1$	$\boldsymbol{\beta}_0$+$\boldsymbol{\beta}_1$+$\boldsymbol{\beta}_2$+$\boldsymbol{\theta}$	$\boldsymbol{\beta}_2$+$\boldsymbol{\theta}$
Control	$\boldsymbol{\beta}_0$	$\boldsymbol{\beta}_0$+$\boldsymbol{\beta}_2$	$\boldsymbol{\beta}_2$
Difference	$\boldsymbol{\beta}_1$	$\boldsymbol{\beta}_1$+$\boldsymbol{\theta}$	$\boldsymbol{\theta}$

Table 2 disentangles the coefficients of the equation (1) which correspond to DID estimates. β_1, as the coefficient of X_i, is the pre-existing difference between the treatment and the control group. β_2, which is the coefficient of T_t, is the time effect. Our main interest θ, which is the coefficient of D_{it}, is the treatment effect which can be computed in two directions as below. Where $\hat{\theta}$ is our DID estimator,

$$\hat{\boldsymbol{\theta}} = (E[\bar{Y}_1^T] - E[\bar{Y}_0^T]) - (E[\bar{Y}_1^C] - E[\bar{Y}_0^C]) = (\boldsymbol{\beta}_2 + \boldsymbol{\theta}) - (\boldsymbol{\beta}_2) = \boldsymbol{\theta} \tag{2}$$

$$\hat{\boldsymbol{\theta}} = (E[\bar{Y}_1^T] - E[\bar{Y}_1^C]) - (E[\bar{Y}_0^T] - E[\bar{Y}_0^C]) = (\boldsymbol{\beta}_1 + \boldsymbol{\theta}) - (\boldsymbol{\beta}_1) = \boldsymbol{\theta} \tag{3}$$

$\bar{Y}_0^T$ and $\bar{Y}_1^T$ denote the sample average of the outcome for the treatment group before and after treatment respectively. Likewise, $\bar{Y}_0^C$ and $\bar{Y}_1^C$ are the corresponding sample averages of the outcome for the control group. The equation (2) shows that the DID estimator is defined as the difference in *"the pre-versus-post difference"* in average outcomes of the treatment group and the control group. By taking the expectation with the assumptions, we can see that the DID estimator is unbiased, resulting from removing the time effect β_2 with double differences. (We will revisit the assumptions in the following chapter.) Plus, the equation (3) shows the other way to present the DID estimator, which is the difference in *"the treatment-versus-control difference"* in average outcomes before and after the treatment. The double subtractions lead to unbiased estimate by eliminating the pre-existing differences between two groups β_2. In brief, DID shows the impact of treatment and avoids potential bias threats by differencing away any permanent differences *between the groups* and any common trend *in both groups*.

Moreover, when we analyze panel data in which the same units are observed repeatedly over time, we obtain the same estimate from the equation (1) via a linear regression with unit and fixed effects. This is because, first, the observations are divided into the groups $X_i = 0$ and $X_i = 1$, and secondly, T_t is equivalent to a time index. This numerical equivalence is the justification of the two-way fixed effects regression as the DID design (Angrist and Pischke, 2008). Thus, the equation (1) is identical to the equation (4), which is the two-way fixed effects regression where u_i is a unit fixed effect and v_t is a time fixed effect. For i = 1, ..., N and t = 1, ..., T, the equation (4) is the standard DID model. Our main interest is the coefficient θ, which is the treatment effect of D_{it} on Y_{it} as the double difference in sample means. The equation (5) presents the extended regression with control variables, x_{it}.

$$Y_{it} = \theta D_{it} + u_i + v_t + \epsilon_{it} \tag{4}$$

$$Y_{it} = \theta D_{it} + x'_{it}\beta + u_i + v_t + \epsilon_{it} \tag{5}$$

2.2 Key Assumption: The parallel trend

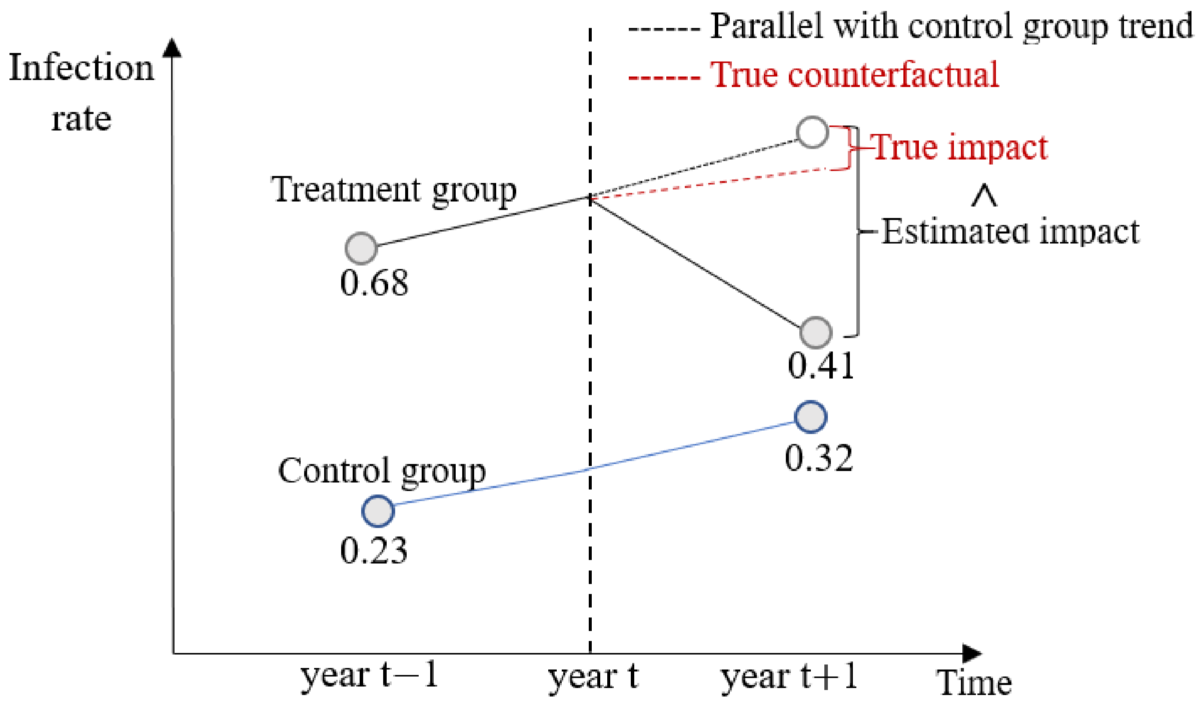
In the previous section, we identified the unbiased DID estimator with the hidden assumptions. Now we clarify these assumptions under which θ refers to the causal effect of treatment D_{it} on Y_{it}.

- The outcome equation equals the specified linear regression model with additive structure.
- The error term on average equals zero; $E(\epsilon_{it}) = 0$
- The error term is uncorrelated with the binary treatment indicator; $\text{Cov}(\epsilon_{it}, D_{it}) = 0$

The last assumption is known as the *the parallel trend assumption*. As the most critical assumption for DID, we will look into the parallel trend assumption in detail. The parallel trend assumption tells that the average change in the outcome would have been the same for both treatment and control groups in the absence of treatment (Angrist and Pischke (2008)). With the equal trend, we can credibly rule out any time-variant changes that may confound the treatment, thus successfully provide a valid estimate of the counterfactual.

We can further deepen our understanding of the parallel trend assumption by answering the following question: ***what happens if the parallel trend assumption does not hold?*** Going back to our vaccination example, Figure 2 shows that the non-parallel trend leads to a biased estimate of the treatment effect. That is, if the treatment group and the control group have the different outcome trend, the estimated treatment effect via DID method would be invalid. This is because the non-parallel trend implies that the control group would not be the appropriate counterfactual of the trend that the treatment group would have followed in the absence of the treatment. Figure 2 illustrates that, when the outcome trend for the control group in reality has slower growth than that for the treatment group if there were no treatment, we would overestimate the treatment effect.

Figure 2: Violation of the parallel trend assumption



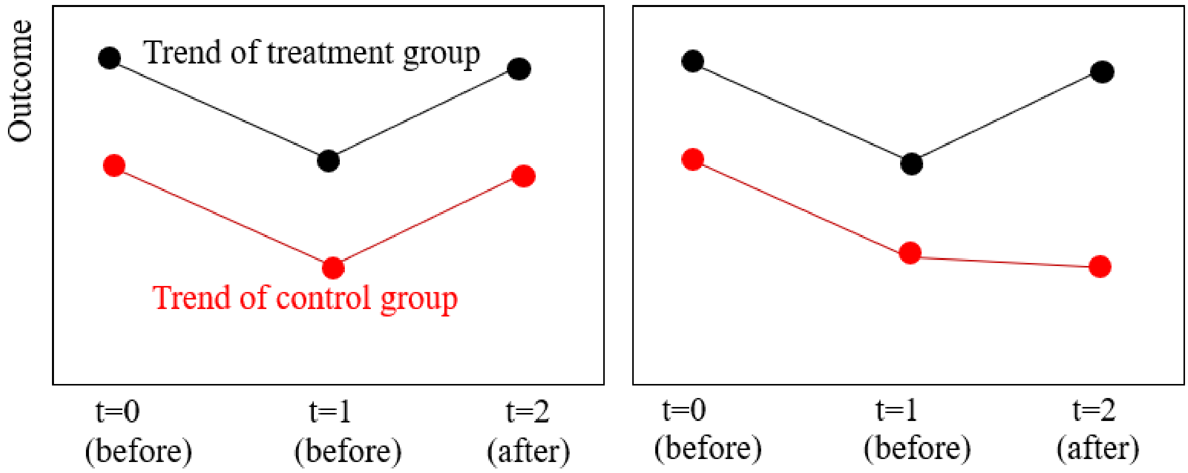
2.3 The validity check for the parallel trend

As the validity of the DID method counts on the parallel trends assumption, it is important to test the plausibility of the assumption in any application. However, since the assumption itself involves counterfactual outcomes, it is difficult to test it empirically. Despite the difficulty of direct proof, we can assess the validity of the parallel trend assumption with the following tests.

- Falsification test with over two pre-treatment periods observations
- Placebo test with a fake treatment group
- Placebo test with alternative outcome

First, we can examine whether trends for treatment and control groups are parallel *in pre-treatment periods* (Angrist and Pischke (2008)). The idea behind this test is that if the outcomes moved in parallel before the treatment, we can assume that outcomes would have shown parallel moves after the treatment. The check for pre-treatment parallel trends requires at least three observations; two pre-treatment observations for the pre-treatment trends, and one post-treatment observation for the impact with the DID method. For our vaccination example, we can compare the change in infection rate between treatment and control groups in pre-vaccination periods; between year t-2 and year t-1 and between year t-1 and year t. Figure 3 displays two cases where the right box keeps the parallel trend before pre-treatment periods and the left box shows the non-parallel pre-treatment trend.

Figure 3: Pre-treatment Parallel Trends and Pre-treatment Non-Parallel Trends



Another validity check for the parallel trend is *a placebo test* in which an additional DID estimation is performed "with a fake treatment" group, a group that is not supposed to be affected by the treatment. Since we know that the treatment does not affect this additional group, when we perform the DID estimation with the original control group and the fake treatment group, the impact should be zero. Otherwise, the non-zero impact implies the existence of underlying difference in trends between the original control group and the fake treatment group, suggesting that our original DID might be biased.

Lastly, we can also have the placebo test "with a fake outcome", outcome that is not supposed to be affected by the treatment. Let's say we want to check the validity of our control group. By estimating the treatment effect on the fake outcome, if the placebo DID estimation is non-zero, it indicates that there must be flaws in our control group.

3 Empirical application

3.1 Do police reduce crime?

Assessing policing effect is one of the challenging issues for scholars. In classical criminology, criminals are considered to be rational. Gary Becker Becker (1968) was the first to outline the theory of crime based on the criminals' rational behavior leading to the increased interest in the economics of crime. The main prediction of Becker's theory is that police presence leads to the reduction of the crime rate. However, this prediction was not supported by empirical surveys conducted by fellow researchers. The main problem with these studies is the presence of endogeneity due to the simultaneity issue as crime and police are determined simultaneously. It is likely that the areas with a high crime rate will get more police surveillance than the areas with a lower crime rate, introducing a positive bias. So in order to investigate the causal effect of police on crime researchers should dissolve the occurring problem of endogeneity. One of the solutions to this problem is to use a natural experiment. A natural experiment occurs when an exogenous event, such as terrorism, leads to the reallocation of police forces breaking the simultaneous determination of crime and police.

One of the studies using the natural experiment was conducted by Di Tella and Schargrodsky (2004). To see DID's implementation and performance we replicate their paper "Do police reduce crime?". In July 1994, after a terrorist attack against the main Jewish center in Buenos Aires, all city blocks with Jewish institutions received police surveillance. Di Tella and Schargrodsky (2004) used difference-in-differences method to investigate whether police surveillance decreased the car theft rate by studying the natural experiment.

3.2 Data

Data used in the paper is panel data of car thefts in three noncontagious neighborhoods in Buenos Aires for each of the nine months before and after the terrorist attack. The neighborhoods are selected based on three criteria: first is that these neighborhoods had a large number of Jewish institutions in the city; second - a significant number of blocks of the neighborhood do not have a protected institution, allowing to use them as a control group; and third - three was the maximum number of police stations willing to provide data on car thefts. Each neighborhood is divided into blocks, that is the unit of observation, in total there are 876 blocks. Authors consider a block as the segment of a street between two corners. Blocks were divided into two groups - control and treatment. There are 37 blocks, which got 24-hour police protection in the treatment group and the rest 839 blocks are in the control group. A key dimension in this paper is the distance of each block to the nearest Jewish institution. Depending on the distance, authors divide the control group further into three subgroups: blocks which are one block, two blocks, and more than two blocks away from the nearest Jewish institution.

3.3 Model

For the replication of the results we use two-way fixed effects regression:

$$Y_{it} = \theta D_{it} + v_t + u_i + \epsilon_{it} \tag{6}$$

where:

- Y_{it} is the number of car thefts in block i for month t;
- D_{it} is a dummy variable that equals 1 for the months after the terrorist attack if there is a protected institution in the block, 0 otherwise;
- θ is a DID estimator;
- v_t is a month fixed effect;
- u_i is a block fixed effect;
- ϵ_{it} is the error term

Month fixed effects are included to capture any aggregate shocks in the evolution of crime and block fixed effects - to control for time-invariant influences.

As in general, police forces are allocated on the areas with expected high crime rates, obtaining the causal effect of police on crime can be constrained by the problem of simultaneity of crime and police. Therefore, researchers mostly use natural experiments to avoid the problem of simultaneity. In this paper, the geographical re-allocation of police was a response to the terrorist attack, thus, the introduction of police protection on some blocks is exogenous to the distribution of a common crime rate.

Table 3: Number of car thefts by city block

	Same Block	Not on Same Block	Difference
Before Attack	0.0895	0.0815	-0.0079
After Attack	0.0351	0.1047	0.0695
Difference	-0.0543	0.0231	**-0.0775**

Notes: Before attack period: from April till the 17th of July. 18-31 July is excluded due to the non-availability of data. After attack period: August-December.

Table 3 shows the average number of car thefts per block for the months before and after the attack, and for the blocks with and without protected institutions. From this table, we see that the mean number of car thefts were similar for the blocks in treatment and control groups before the attack period, namely, between months April and July with a difference in means almost equal to 0. But from the second row, we see that the average number of auto thefts significantly decreased for the blocks with a protected institution, while on contrary, for the blocks without a protected institution this number increased by 0.0231. The difference-in-differences of means is equal to -0.0775. An extended table with summary statistics of car thefts for each group for each of the nine months can be found in Table A.1 in the Appendix.

In addition, to claim that the deterrence effect of police is causal and to use the regression model (6) the assumption of homogeneity of the treatment effect should hold. In order to check it, we look at Table A.1 in the Appendix. From that table, we see that for the control group the average number of car thefts is near-constant across the months. For the treatment group, results show that the mean number of car thefts before the attack is similar to the mean of the control group's, but in the post-attack period, this number is significantly lower for each of the 5 months.

Besides, the authors use these extended versions of the main regression in order to check if police presence in blocks with protected institutions has an effect on neighboring, namely one and two blocks removed blocks:

$$Y_{it} = \theta_1 D_{it} + \theta_2 Z_{it} + v_t + u_i + \epsilon_{it} \tag{7}$$

$$Y_{it} = \theta_1 D_{it} + \theta_2 Z_{it} + \theta_3 W_{it} + v_t + u_i + \epsilon_{it} \tag{8}$$

where:

- $\theta_{1,2,3}$ regression coefficients;
- Z_{it} is a dummy variable that equals 1 after the terrorist attack (August, September, October, November, and December) if the block is one block away from the nearest protected institution, 0 otherwise;
- W_{it} is a dummy variable that equals 1 after the terrorist attack (August, September, October, November, and December) if the block is two blocks away from the nearest protected institution, 0 otherwise.

3.4 Results

Table 4 columns A, B, C depicts the results for regression equations (6), (7), and (8). The first regression in column A uses a dummy variable which is equal to 1 for blocks with protected institution for the period of August-December as an independent variable, considering all other blocks as a control group. The DID estimator is negative and significant, meaning the police presence decreases the number of car thefts by -0.077 on average.

Column B depicts the results of the regression (7) which takes into consideration the blocks which are adjacent to the blocks with the protected institution. The coefficient estimator of D is still significant and negative, whereas the coefficient estimator for the dummy Z, being 1 for the blocks one block away from protected blocks in the post-attack period and 0 otherwise, is insignificant.

Column C shows us the results of regression equation (8), which includes the third measure of proximity to the protected institution-two-blocks away from the Jewish institution. The DID estimator remains negative and highly significant at the 1-percent level, whereas other coefficients of regression showed insignificant results. The result suggests that police surveillance in the blocks with a protected institutions does not affect the neighboring blocks. We can conclude,

Table 4: The Effect of Police Presence on Car Theft

	Difference-in-difference			Cross-section	Time-series
	(A)	(B)	(C)	(D)	(E)
D (same block police)	-0.07752***	-0.08007***	-0.08007***	-0.07271***	-0.05843***
	(0.022)	(0.022)	(0.022)	(0.011)	(0.022)
Z (one-block police)		-0.01325	-0.01398	-0.01158	-0.00004
		(0.013)	(0.014)	(0.010)	(0.013)
W (two-blocks police)			-0.00218	-0.00342	-0.01701
			(0.012)	(0.009)	(0.010)
Block fixed effect	Yes	Yes	Yes	No	Yes
Month fixed effect	Yes	Yes	Yes	Yes	No
Number of observations	7884	7884	7884	4380	3816
R^2	0.1983	0.1983	0.1983	0.0036	0.1891

Notes: Dependent variable: number of car thefts per month per block. Least-squares dummy variables (LSDV) regressions. Car thefts that occurred between July 18 and July 31 are excluded. Column (D) excludes observations for the preattack period (April through July). Column (E) excludes observations for the blocks that are more than two blocks away from the nearest protected institution. Huber-White standard errors are in parentheses.

that the blocks, which are one and two blocks away from the block with a protected institution did not experience the declined number of car thefts due to the police surveillance in the neighboring blocks. The coefficient estimates of that blocks are the same as those of the control group.

Overall, the results suggest that the introduction of observable police presence reduced the number of auto thefts in the blocks with police protection, but had no effect on car crime in the neighboring blocks. Therefore, the effect of police is local and has no effect outside a narrow area. The results do not change with the inclusion of one and two blocks removed from the protected site in the control group.

To check for the validity of results two alternative estimators also were tested. First is a simple cross-section estimator, results of which are shown in column D of Table 4. In this regression, authors only consider the post-attack period and exclude block fixed effects, assuming similar demographic characteristics and preintervention car thefts. The coefficient of D is equal to -0.073 which is quite similar to the DID coefficient in the first three equations. The second alternative estimator is the time-series estimator represented in column E. With this regression authors compare the average number of car thefts before and after the attack excluding the month fixed effects and the observations in the control group. The coefficient equals -0.058, smaller than in other columns, because of the small upward trend in the control group.

Several further tests were conducted by the authors in order to assess the validity of the results. Issues such as parking restrictions on the protected blocks, change of parking preferences of car owners due to the fear of another terrorist attack, crime dynamics in the neighborhood before the attack, serial correlation, spatial correlation were considered in the paper. All these tests proved the validity of the main results in Table 4. The results of the robustness check can be found in the paper itself, as this project is centered in DID method we replicated only the main results and checked the assumptions.

4 Monte Carlo Simulation

The current section describes the basic Monte Carlo Simulation settings and shows the effects of different misspecifications. We are considering the following misspecifications:

1. omitted variable bias due to omitted fixed effects;
2. assuming parallel trend in the case of a non-parallelism.

4.1 Set up DID method for the Monte Carlo Simulation

We have an assumed true model which is equivalent to the two-way fixed effects regression.

$$Y_{it} = \theta D_{it} + v_t + u_i + \epsilon_{it}, \quad i = 1, ..., n \quad and \quad t = 1, ..., k \tag{9}$$

where:

- Y_{it} is the total number of car thefts per block during each month from April to December;
- i= 1, ..., n with n=876 (total number of blocks);
- t= 1, ..., k with k=9. (the 1^{st} month is for April, 2^{nd} month is for May ... 9^{th} month is for December) [1];
- θ=-0.07752 is the assumed true treatment effect;
- $\epsilon_{it} \overset{i.i.d}{\sim} \mathcal{N}(0,\ 0.2166)$ is Huber-White standard error term.

We have three main explanatory variables D_{it}, v_t and u_i in our regression. Now we explain each independent variable one by one. D_{it} is a dummy variable that equals 1 after terror attack (August, September, October, November, December) if there is a protected institution in the

[1] We defined the sequence of months from 1^{st} till 9^{th} because of convenience in R coding (T=9).

block, otherwise D_{it} is 0. Table 5 shows how in overall blocks are divided into treated and control blocks.

From the 1st of August 37 blocks out of 876 received police surveillance and the rest 839 blocks didn't receive any police protection. In our simulation part, we designed that from the 1st of August the first 37 blocks (1-37) are treated and 839 blocks (38-876) are controlled. Assigning the order of treated and control groups doesn't impact on simulation results since the database is randomly generated.The important thing is that the number of treated blocks should be 37 blocks and control blocks are 839.

Table 5: Treatment and control blocks

	Months	D_{it}= 0	D_{it}= 1
4	April	876	0
5	May	876	0
6	June	876	0
7	July	876	0
8	August	839	37
9	September	839	37
10	October	839	37
11	November	839	37
12	December	839	37

v_t is a month fixed effect and controls any aggregate shocks that might appear in the evolution of crime. By design month fixed effects are weakly correlated with the treated group. Table A.2 shows month fixed effect estimators and standard errors.

u_i is the block fixed effect. Block fixed effects control time-invariant influences and is strongly correlated with the treated blocks.

At the end our generated database for Monte Carlo Simulation looks as follows:

$$
\underbrace{\begin{pmatrix} Y_{1,1} \\ Y_{2,1} \\ \vdots \\ Y_{876,1} \\ \vdots \\ Y_{1,9} \\ Y_{2,9} \\ \vdots \\ Y_{876,9} \end{pmatrix}}_{=Y} = \begin{pmatrix} \theta \end{pmatrix} \times \underbrace{\begin{pmatrix} D_{1,1} \\ D_{2,1} \\ \vdots \\ D_{876,1} \\ \vdots \\ D_{1,9} \\ D_{2,9} \\ \vdots \\ D_{876,9} \end{pmatrix}}_{=D} + \underbrace{\begin{pmatrix} v_1 \\ v_1 \\ \vdots \\ v_1 \\ \vdots \\ v_9 \\ v_9 \\ \vdots \\ v_9 \end{pmatrix}}_{=v} + \underbrace{\begin{pmatrix} u_1 \\ u_2 \\ \vdots \\ u_{876} \\ \vdots \\ u_1 \\ u_2 \\ \vdots \\ u_{876} \end{pmatrix}}_{=u} + \underbrace{\begin{pmatrix} \epsilon_{1,1} \\ \epsilon_{2,1} \\ \vdots \\ \epsilon_{876,1} \\ \vdots \\ \epsilon_{1,9} \\ \epsilon_{2,9} \\ \vdots \\ \epsilon_{876,9} \end{pmatrix}}_{=\epsilon} \tag{10}
$$

where: $Y_{1,1}$ is total car theft in the first block during the first month. In our case the first month is April. θ is estimator from our assumed true empirical model. $D_{1,5}$ is a dummy variable and equals 1 because it is in treated block and in the fifth month (August in our case) which is after terror attack. v_1=1 is the first month, i.e., April and v_9=9 is the ninth month which is December in our case. u_1=1 is for the first block and u_{876}=876 is for the 876^{th} block. $\epsilon_{1,1}$ is Huber-White standard error for the first month and for the first block.

4.2 Checking Parameter Estimators via Monte Carlo Simulation

We now turn to examining estimator for treatment effect assuming true model in equation (9) under 4 different specifications to check estimated bias, mean squared error (MSE) and confidence intervals of $\hat{\theta}$.

In the first specification we run the equation (9) as given, i.e., there is no change in assumed true model equation. Table 6 shows that assumed true model estimator $\hat{\theta}$ performs better than the rest 3 specifications in estimated bias, mean squared error and confidence intervals.

In the second specification we run the assumed true model without time fixed effects (v_t) and still we have better results comparing with the last 2 specifications' results. (Look at the Table (A.2) for month fixed effects estimators).

In the third specification we run our assumed true model without block fixed effects (u_i). Block fixed effects are strongly correlated with the treated blocks. So, in case of omitting block fixed effects in assumed true model, $\hat{\theta}$ has high estimated bias and mean squared error with comparing the results of the first and second specifications. Because of omitted variable bias $\hat{\theta}$ has a high error in critical values.

In the last specification we run our assumed true model without month and block fixed effects. Results of fourth specification perform the worst due to omitted variable bias from two sources.

After conducting all 4 specifications we can say that if there are true fixed effects and we don't model them this leads to strong omitted variable bias depending on the correlation of fixed effect with treatment group and the size of fixed effect themselves. Figure A.1 shows the density of estimated $\hat{\theta}$ under 4 different specifications and proves the results of Table 6.

Table 6: Simulation output for $\hat{\theta}$

Specifi cations	Model equations	Number of Simulations	Estim. Bias of $\hat{\theta}$	MSE	CI lower	CI upper
1.	$\hat{Y_{it}} = \hat{\theta} D_{it} + \hat{v}_t + \hat{u}_i + \hat{\epsilon_{it}}$	1000	0.0016	0.0005	-0.1218	-0.0273
2.	$\hat{Y_{it}} = \hat{\theta} D_{it} + \hat{u}_i + \hat{\epsilon_{it}}$	1000	0.0017	0.0005	-0.1223	-0.0274
3.	$\hat{Y_{it}} = \hat{\theta} D_{it} + \hat{v}_t + \hat{\epsilon_{it}}$	1000	0.1057	0.0114	-0.0021	0.0593
4.	$\hat{Y_{it}} = \hat{\theta} D_{it} + \hat{\epsilon_{it}}$	1000	0.1434	0.0208	0.0350	0.0945

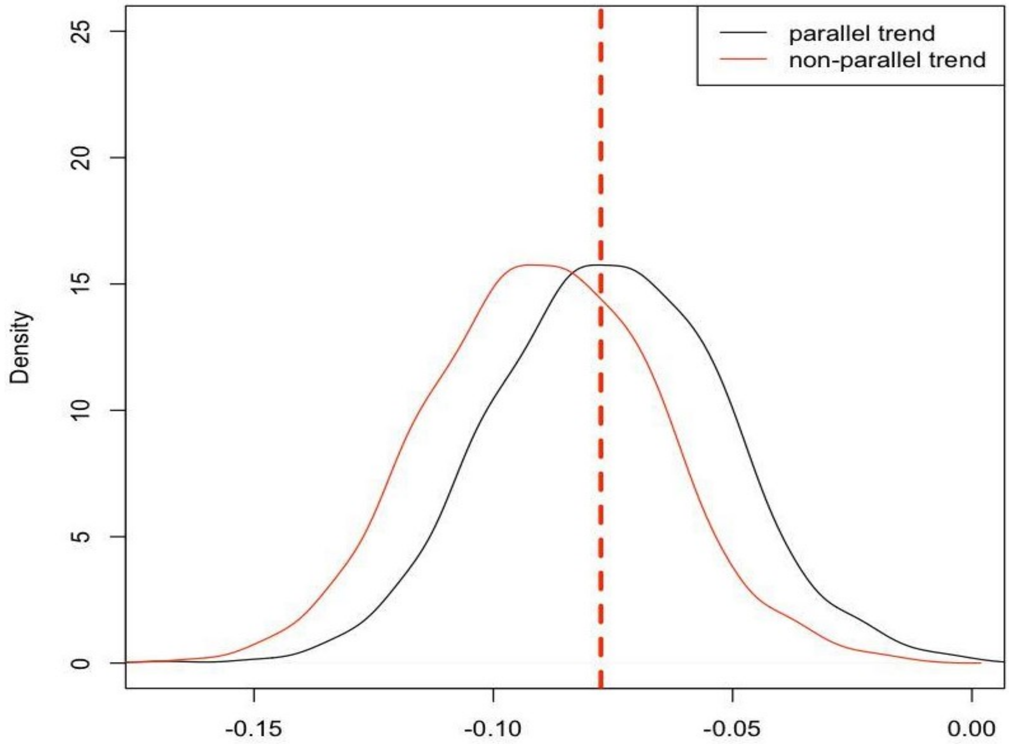
Notes: Number of Monte Carlo Simulations is 1000.There are given 4 model equations for different specifications. Confidence intervals are estimated at 5 percent significance level.

Econometricians and economists are interested in analyzing the DID estimator under the case when the parallel trends assumption does not hold. Therefore, we also check the estimation of θ under the case of non-parallelism of trends in our Monte Simulation. There are much literature and research on the analysis of parallelism trends in difference-in-differences estimation.

If parallel trends assumption doesn't hold for car theft then our estimator $\hat{\theta}$ will be biased[2]. Treatment was occurred due to month, i.e., after terror attack in July blocks are divided into control and treated blocks. So we change slope in our assumed true model by multiplying month fixed effects v_t with $(1 + D_{it})$. $(1 + D_{it}) \times v_t$ changes the slope which implies breaking of parallelism between control and treated blocks. Table 7 shows the estimation results for θ under the case of parallel and non-parallel trend. In case of non-parallel trend estimation of the parameter θ has the worst results in estimated bias, mean squared error and confidence intervals

[2]Look at the subsections 2.2 and 2.3 of this paper. Moreover, Bilinski and Hatfield (2019), Freyaldenhoven et al. (2019) and Rambachan and Roth (2019) suggest solutions to tackle the violation of parallel trends assumption between treated and control blocks

Figure 4: Density of $\hat{\theta}$ is with parallel and non-parallel trend



Notes: Red dashed line is our parameter θ which is -0.07752. Red line is for the density of $\hat{\theta}$ under the case of non-parallelism between treated and control blocks while black line is for the density of $\hat{\theta}$ under parallel trend.

comparing with the results of estimation of the parameter θ under the case of parallel trend. Figure (4) shows that if parallel trend doesn't hold in our car theft empirical model then θ will be overestimated.

Table 7: Parallel vs. non-parallel trend

Parallel trend	Month fixed effect & Block fixed effect	Estim. Bias of $\hat{\theta}$	MSE	CI lower	CI upper
Yes	Yes	0.0016	0.0005	-0.1218	-0.0273
No	Yes	-0.0122	0.0007	-0.1358	-0.0413

Notes: Confidence intervals are estimated at 5 percent significance level.

5 Conclusion

In this paper, we provide the comprehensive overview of difference-in-differences method with an empirical application and Monte-Carlo simulations. For a valid DID estimator, the satisfaction of the key assumptions such as the parallel trend assumption and exogeneity of the intervention is crucial. In case of non-parallel trend and non-exogeneity, DID estimator would be invalid and fail to estimate the causal effect of the treatment. This is shown in the Monte-Carlo simulations (4). Besides, in the simulations part by design, we set up a weak correlation between month fixed effects and treatment group, and a strong correlation between block fixed effects and treatment group. After running 1000 simulations we observe the main characteristics of the estimators: mean standard error, estimated bias, and confidence intervals. According to the simulated results, dropping block fixed effects leads to a huge bias, mean squared error, and overall insignificant results. The same conclusion can be drawn if we drop both fixed effects, block and month, which happens due to the omitted variable bias. The size of the bias depends on the level of correlation between fixed effects and treated group. Overall, the DID works well for studying the causal effect of a specific treatment on the treated group by comparing the changes in the outcomes over time between the treated and control groups.

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A Appendix

This appendix collects the tables, figures, proofs and coding part for main text of research.

Table A.1: Monthly evolution of car theft

Months	Jewish institution on the block	One block from nearest Jewish institution	Two blocks from nearest Jewish institution	More than two blocks from nearest Jewish institution
	(A)	(B)	(C)	(D)
April	0.12162	0.1211	0.12278	0.09955
	(0.361)	(0.287)	(0.297)	(0.248)
May	0.08783	0.07763	0.09734	0.10840
	(0.205)	(0.181)	(0.259)	(0.235)
June	0.12837	0.07763	0.06969	0.07853
	(0.286)	(0.215)	(0.186)	(0.196)
July (1-17)	0.02027	0.05900	0.03097	0.03926
	(0.069)	(0.210)	(0.141)	(0.145)
July(18-31)	0.02702	0.07298	0.06858	0.03926
	(0.078)	(0.217)	(0.238)	(0.146)
August	0.04729	0.06677	0.12721	0.11836
	(0.175)	(0.219)	(0.304)	(0.287)
September	0.01351	0.09006	0.09845	0.10176
	(0.057)	(0.276)	(0.248)	(0.256)
October	0.06081	0.09782	0.08849	0.12112
	(0.215)	(0.260)	(0.236)	(0.267)
November	0.02702	0.11024	0.10176	0.09623
	(0.078)	(0.288)	(0.217)	(0.240)
December	0.02702	0.11645	0.10619	0.10176
	(0.078)	(0.278)	(0.225)	(0.268)
Number of blocks	37	161	226	452

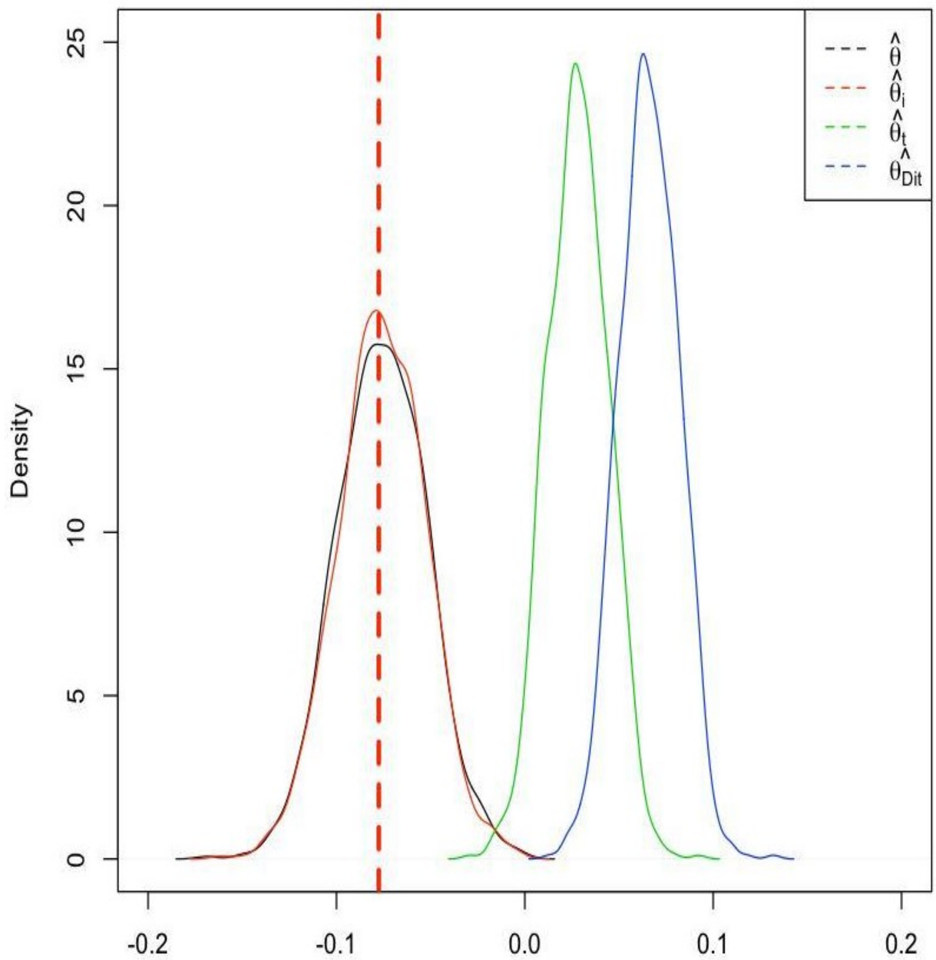
Notes: This table presents the mean and standard deviation (in parentheses) of the number of car thefts for each type of block per month.

Table A.2: Results with month fixed effect estimators

	Difference-in-difference
θ	-0.07752***
	(0.022)
Constant	0.016
	(0.077)
factor(mes)5	-0.011
	(0.011)
factor(mes)6	-0.032***
	(0.011)
factor(mes)7	-0.070***
	(0.011)
factor(mes)8	0.001
	(0.011)
factor(mes)9	-0.012
	(0.011)
factor(mes)10	-0.001
	(0.011)
factor(mes)11	-0.010
	(0.011)
factor(mes)12	-0.005
	(0.011)
Block fixed effect	Yes
Number of observations	7884
R^2	0.1983

Notes: This table shows the month fixed effects estimators in detailed form. Each month has an estimator and *Constant* is for the April (because one of the fixed effects appears a a constant in regression and in our case month April appears as a constant), *factor(mes)5* is for May, *factor(mes)6* is June ... *factor(mes)12* is for December. Huber-White standard errors are in parentheses. $***$ is significant at the 1-percent level.

Figure A.1: Density of $\hat{\theta}$ under 4 different specifications



Notes: This figure depicts the density of $\hat{\theta}$ under 4 different specifications. Red dashed line is parameter θ which is -0.07752.