ERASMUS UNIVERSITY ROTTERDAM

Erasmus School of Economics

Bachelor Thesis Policy Economics

Should Rotterdam Act?

The effect of selective social housing assignment on neighbourhood crime rates

Armin Hoendervangers

428365

Supervisor: Vladimir Karamychev

Second assessor: Kyra Hanemaaijer

Date: July 21, 2022

Abstract

This thesis investigates the effects of the so-called Rotterdam Act on neighbourhood crime rates. Using data from the municipality of Rotterdam, I construct a measure for treatment exposure and use an event study difference-in-difference design to estimate potential treatment effects. I find that application of Rotterdam Act measures on average has no effect on crime rates. In contrast with previous literature, I also find that treatment effects depend on treatment exposure, where an increase in exposure leads to a decrease in crime rates and vice-versa.

Keywords: Rotterdam Act, Wet Bijzondere Maatregelen Grootstedelijke Problematiek, Rotterdam, Neighbourhood Policy, Social Housing, Crime, Difference-in-Difference, Event Study, Impact Evaluation

Contents

1 Introduction

In 2006 the Dutch government passed the "Act on Extraordinary Measures for Urban Problems" (*Wet Bijzondere Maatregelen Grootstedelijke Problematiek*). The goal of this act is to provide municipalities additional measures to improve the livability and safety of neighbourhoods. This act is commonly referred to as the Rotterdam Act,[1] as the municipality of Rotterdam is both the driving force behind its implementation and the first municipality to implement the measures (Hochstenbach et al., 2015).

In the succeeding years, the Rotterdam Act has not been without controversy. For example, the Netherlands Institute for Human Rights has stated that the Act infringes on the basic human right of freedom of settlement and private life (van Dooijeweert, 2016). Since its implementation, there have been several reports regarding the effects of the Rotterdam Act (e.g. Hochstenbach et al., 2015; Kromhout et al., 2021), with the general consensus that there is a negative association between application of the Act and neighbourhood safety, but that identification of a causal effect is difficult. Hochstenbach et al. (2015), often cited in discussions regarding the Act, use a multivariate ordinary least squares regression model to identify the treatment effect. Treatment assignment is not random, however. Identification of a causal effect using this method is thus not just difficult, but impossible.

In this thesis I aim to expand the literature by examining the Act through methodology new to this strand of literature. I specifically aim to identify the potential effects of the Rotterdam Act on neighbourhood crime rates with an Event Study Difference-in-Difference model. One of the main advantages of this model is that it allows for estimation of treatment effect dynamics. By estimating these treatment effect dynamics I aim to deepen the understanding of the effects of the Rotterdam Act. Accordingly, this thesis' research question is as follows:

Has the application of selective housing assignment measures in Rotterdam neighbourhoods affected the crime rates in these neighbourhoods?

To estimate this potential effect I use publicly available administrative data obtained from the municipality of Rotterdam. From these data I construct a measure for neighbourhood exposure to allow for varying degrees of treatment intensity. By exploiting this variation I find that although on average the Act has no effect on crime rates, treatment effects differ by neighbourhood exposure. To be precise, an increase in exposure leads to a decrease in crime rates and vice-versa. After checking for robustness of my results I conclude that although the effects remain significant, they appear to be transient in nature.

The rest of this thesis is structured as follows. The next section presents related literature. Section 3 describes the relevant institutional background of the Rotterdam Act. Sections 4 and 5 describe the data and methodology. Sections 6 and 7 present and discuss the results. Section 8 concludes.

[1] For the remainder of this thesis, I follow this practice and also speak of "the Rotterdam Act" or "the Act".

2 Related Literature

2.1 The Rotterdam Act

Hochstenbach et al. (2015) thoroughly evaluates the Rotterdam Act on behalf of the Ministry of Internal Affairs. They are concerned not only with the effects on neighbourhood safety, but also on moving behaviour and population composition. In chapter six of their report, the authors aim to identify potential effects of the Rotterdam Act on neighbourhood livability. For this, Hochstenbach et al. use a recalculated iteration of the municipality of Rotterdam's safety index. They perform a multivariate OLS regression to identify differences in development of the recalculated safety index between treated and untreated neighbourhoods. The authors find a negative association between application of the Rotterdam Act and the development of the recalculated safety index. They conclude that the Rotterdam Act has not contributed to improvement of neighbourhood safety, although it is difficult to determine a causal effect.

From a methodological point of view, it is not surprising that the identification of a causal effect is difficult. As the application of the Rotterdam Act is not random, the treated and untreated groups of neighbourhoods are unlikely to be very similar. The with-and-without comparison of a regular multivariate regression is therefore not suitable for causal inference.

Kromhout et al. (2021) study the effects of the Rotterdam Act on a national level. Their analysis is based on a quantitative with-and-without comparison and qualitative research. For the effects on the municipality of Rotterdam, the authors mostly refer to an earlier study by an external research institute commissioned by the municipality of Rotterdam. The authors conclude that for Rotterdam specifically there has been a relatively negative development in the treated neighbourhoods, but that they cannot unambiguosly identify a causal effect.

2.2 Social Housing Policy

Atkinson (2005) looks into the impact of a neighbourhood's social mix. By reviewing existing literature, Atkinson shows that several housing projects improved neighbourhoods. For instance, by redistributing poor households across neighbourhoods so that the socio-economic composition of neighbourhoods diversifies. As a result, crime rates in these neighbourhoods dropped. It should be noted that, in contrast with the Rotterdam Act, these mobility projects (e.g. the Moving to Opportunity Programme) accomplished neighbourhood diversification by presenting poor households with the opportunity to move to a higher-status neighbourhood. On the other hand, the Rotterdam Act aims to achieve socio-economic diversification by refusing or preventing households from moving into a neighbourhood. This constrasting approach to neighbourhood improvement might explain why this study find opposing results with respect to Hochstenbach et al. (2015) and Kromhout et al. (2021).

Pawson and Kintrea (2002) investigate social housing allocation policies in Britain and how these contribute to or counter social exclusion. The authors conclude that allocation policies contribute to social exclusion in three distinct ways. First, a large proportion of social housing landlords restrict the eligibility for social housing, directly excluding a fraction of tenants. Second, mechanisms within allocation systems segregate the most excluded to the worst residential areas. Finally, British allocation policies became increasingly coercive in the 1990, in contrast with the available choice in the private market. This study highlights potential issues of allocation policies, such as increasing segregation and increasing social exclusion of socio-economically vulnerable households.

The Rotterdam Act is heavily prone to these issues, as the measures it provides are—by construction—based on exclusion. Implementation of the Rotterdam Act could thus be counter-productive regarding the improvement of neighbourhoods.

3 Institutional Background of the Rotterdam Act

3.1 Goals and Context

The Rotterdam Act was introduced in 2006, following a time when an increasing amount of data was made available on the postcode level. The municipality's administration of the early 2000s strongly pushed the quantification and ranking of neighbourhoods, following the electoral victory of *Leefbaar Rotterdam* (Uitermark et al., 2017). Out of this this numbers-based governing one statistic became central to Rotterdam's policies: the so-called Safety Index. This index, allowing for easy—although opaque—comparison of neighbourhoods, laid the groundwork for argumentation advocating the necessity of more extensive and intrusive neighbourhood policies.[2]

Simply put, the goal of the Rotterdam Act is the improvement of vulnerable neighbourhoods (Directie Democratie en Burgerschap, 2016). Its aim is to reduce the influx of prospective tenants with, e.g. nuisance or criminal behaviour. Consequently, the municipality can give current tenants more perspective as there is then more room to focus on non-problematic tenants with low socio-economic status.

3.2 Measures

If a muncipality wants to use the measures in one of the articles in the Rotterdam Act, they must request approval from the responsible Minister. In this request the municipality must provide plausible argumentation to illustrate that the application of the article is (1) necessary and appropriate in combatting urban problems, and (2) meets the requirements of subsidiarity and proportionality (Artikel 6, "Wet Bijzondere Maatregelen Grootstedelijke Problematiek", 2005). If approval is given, measures may be applied for a period of up to four years, and an extension of four years can be requested a maximum of four times (Artikel 5, "Wet Bijzondere Maatregelen Grootstedelijke Problematiek", 2005).

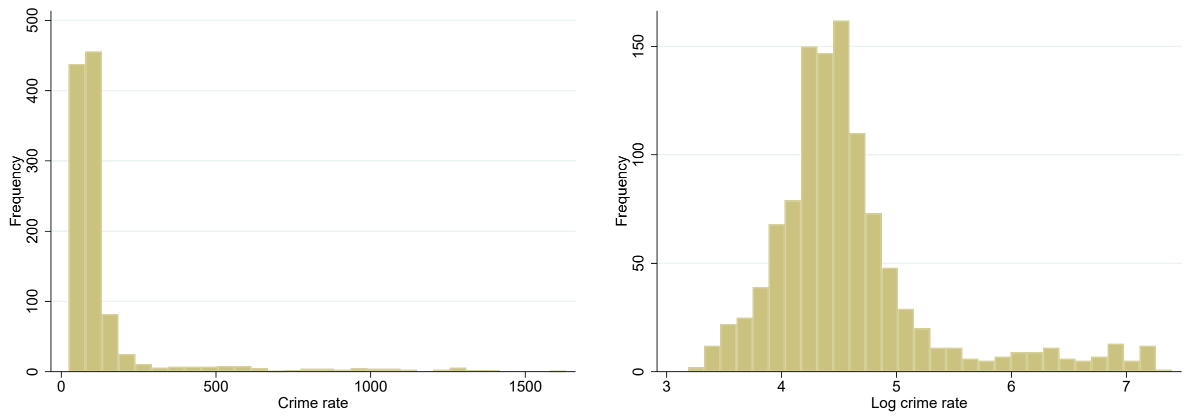
There are three articles in the Act which provide measures regarding social housing allocation; article 8, article 9, and article 10.

Article 8 enables the municipality to set an additional requirement for prospective tenants. This is targeted at those who have not continuously lived in the municipality six years prior to application for the housing permit. Specifically, prospective tenants only qualify for a housing permit on the condition that they have income from labour, an independent business, a pension, or a student grant (Artikel 8 "Wet Bijzondere Maatregelen Grootstedelijke Problematiek", 2005).

[2] see Uitermark et al. (2017) for an extensive metaphysical analysis of the establishment of the Rotterdam Act.

Figure 1: Distribution of Observed Crime Rates

(a) Crime Rate (b) Log Crime Rate



Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: Panel (a) depicts the frequency of observed crime rates in all neighbourhoods, Panel (b) depicts the frequency of observed log crime rates in all neighbourhoods. Frequencies in both panels are reported for the 2005–2020 period.

Article 9 enables the municipality to grant priority to prospective tenants for whom urgent housing is necessary, or those who are economically or socially connected to the municipality (Artikel 9, "Wet Bijzondere Maatregelen Grootstedelijke Problematiek", 2005).

Finally, Article 10 enables municipalites to reject applications for a housing permit if they have a well-grounded presumption that housing the applicant will lead to an increase of disturbances or crime rate in that neighbourhood (Artikel 10, "Wet Bijzondere Maatregelen Grootstedelijke Problematiek", 2005).

In this thesis I do not distinguish between the measures of the different articles. Rather, I consider the first year in which any of the measures is applied as the time of treatment.

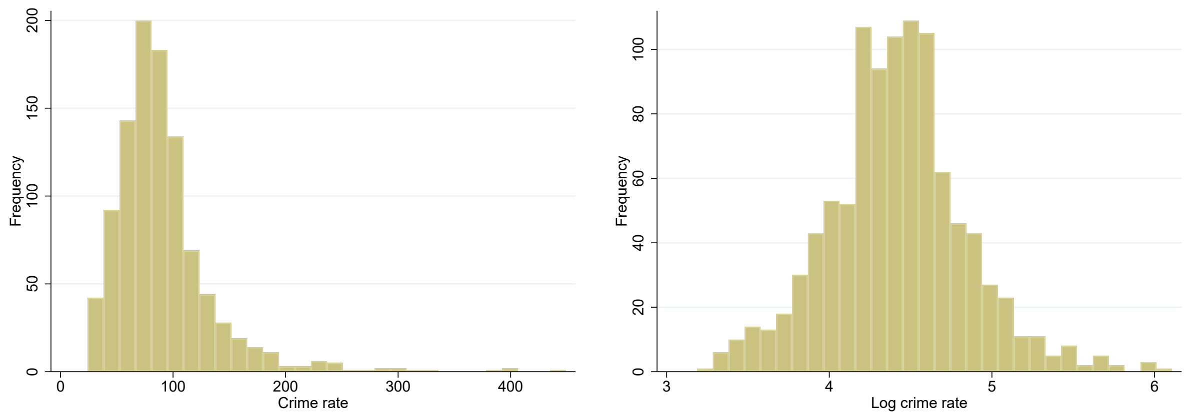
4 Data

For this thesis I construct a neighbourhood-level panel dataset consisting of several neighbourhood characteristics. Core data are obtained from the department Research and Business Intelligence of the municipality of Rotterdam. For each neighbourhood I obtain data on population and settlements from outside the municipality for the period 2001–2020, and crime rates for the period 2005–2020 from the municipality's database *Onderzoek010*. Data on other neighbourhood characteristics, e.g. land surface area and neighbourhood type, are obtained from the municipality's *Wijkprofiel Rotterdam*. Time variant characteristics from the Wijkprofiel are obtained for the period 2014–2022, with one observation per neighbourhood every two years. Finally, time of treatment for each neighbourhood is obtained from the archive of the Dutch Senate, by inspecting the responsible minister's responses to the municipality's requests regarding the application of the Rotterdam Act.

In constructing my sample I perform several transformations to combine the abovementioned data sources. As observed neighbourhoods in the Wijkprofiel and the municipality's database

Figure 2: Distribution of Observed Crime Rates without Outliers

(a) Crime Rate (b) Log Crime Rate



Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: Panel (a) depicts the frequency of observed crime rates in all neighbourhoods, Panel (b) depicts the frequency of observed log crime rates in all neighbourhoods. Frequencies in both panels are reported for the 2005–2020 period.

are not exactly the same,[3] I merge all applicable neighbourhoods in each dataset so that both datasets' units of observations are identical. Treated neighbourhoods' time of treatment is added to obtain the full sample. Each neighbourhood's cohort is then coded as the year in which the Rotterdam Act is first applied in the neighbourhood. Never treated neighbourhoods' cohort is coded as 0 (untreated).

To account for differences in neighbourhood population size I calculate neighbourhood crime rates as the amount of reported crimes for every thousand residents. Time varying neighbourhood characteristics from the Wijkprofiel are averaged for each neighbourhood as a proxy for baseline characteristics. Finally, I calculate population density as the amount of residents divided by the land surface area in hectare.

Figure 1a plots the distribution of the dependent variable, neighbourhood crime rates. The distribution appears right-skewed, so I log transform crime rates to obtain an approximately normal distribution. The distribution of log crime rates is plotted in Figure 1b. Several outliers are visible on the right side of the distribution in Figure 1b. Observations with a log crime rate above 6.5 consist of six neighbourhoods: C.S.-Kwartier, Cool, Kop van Zuid, Nieuwe Werk/Dijkzigt, Zuiderpark en Zuidrand, and Zuiplein.

A common characteristic of these neighbourhoods is their type—they are all central urban neighbourhoods.[4] Furthermore, the fraction of buildings designated as housing ranges from 0.104 to 0.248. This suggests that the high crime rates observed are not caused by residents, but rather visitors from outside the neighbourhoods. As the Act affects crime rates by allowing manipulation of the composition of neighbourhood residents, neighbourhoods where crimes are (potentially) mostly comitted by non-residents might bias estimation results. I thus drop all neighbourhoods where the fraction of buildings designated as housing is less than 0.25 from the

[3] Some smaller neighbourhoods are combined in the Wijkprofiel, while a single neighbourhood (IJsselmonde) is combined in the municipality's database.
[4] Corresponding to the Wijkprofiel neighbourhood Type-1, as I elaborate on later in this section.

Table 1: Summary Statistics by Cohort

	Cohort					
	Untreated	2006	2010	2014	2018	2021
Crime Rate	94.85	94.44	83.84	95.24	84.92	84.42
Population	6060.11	11857.63	13878.55	10314.51	13893.85	8888.74
Population Density	64.94	138.65	177.93	153.66	93.01	97.97
Settlements	411.62	1004.99	906.65	889.82	626.46	401.27
Low Income HH	0.39	0.63	0.72	0.61	0.59	0.58
Social Housing	0.31	0.33	0.59	0.46	0.57	0.57
Resident-Job Balance	0.27	0.15	0.14	0.18	0.24	0.23
Housing Fraction	0.74	0.78	0.86	0.80	0.76	0.74
Neighbourhood Type						
Type 1	0.03	0.00	0.00	0.00	0.00	0.00
Type 2	0.03	1.00	1.00	0.67	0.17	0.00
Type 3	0.38	0.00	0.00	0.33	0.11	0.60
Type 4	0.17	0.00	0.00	0.00	0.72	0.40
Type 5	0.38	0.00	0.00	0.00	0.00	0.00
Observations	580	80	20	120	360	100
Neighbourhoods	29	4	1	6	18	5

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: All values reported as cohort means. *Crime Rate* is reporterd for the 2005–2020 period. *Population*, *Population Density* and *Settlements* are reported for the 2001–2020 period. *Low Income HH*, *Social Housing*, *Resident-Job Balance*, and *Housing Fraction* are reported for the 2014–2022 period. *Population Density* is calculated as neighbourhood population divided by neighbourhood land size in hectare. *Housing Fraction* denotes the fraction of buildings in a neighbourhood designated as housing.

sample, corresponding to the six aforementioned neighbourhoods.

For this new sample Figure 2a and Figure 2b depict the distribution of neighbourhood crime rate and log neighbourhood crime rate, respectively. Removing the neighbourhoods with a small housing fraction appears to take care of log crime rate outliers.

Table 1 provides descriptive statistics by neighbourhood cohort. Treated neighbourhoods primarily consist of densely populated neighbourhoods, with a relatively large amount of social housing. The fraction of households with a low income is accordingly overrepresented in treated neighbourhoods compared to untreated neighbourhoods. Resident-job balance provides an indication if a neighbourhood is mostly residential. This is calculated by dividing the amount of jobs by the sum of jobs and residents in a neighbourhood. In other words, it indicates the amount of jobs per resident in a neighbourhood, with a balance of 0.5 indicating the amount of jobs is equal to the amount of residents in a neighbourhood. Treated neighbourhoods appear to have a lower balance, indicatings a smaller amount of jobs per resident compared to untreated neighbourhoods. The housing fraction provides a different indication for residential neighbourhoods, showing an inversed pattern compared to the resident-job balance. It appears slightly larger for the treated neighbourhoods, although of similar size. As neighbourhoods with a small fraction (< 0.25) have been dropped from the sample, this lack of variation is to be expected. Finally, the treatment appears to be mostly focused on the urban neighbourhoods (types 2 and 3).

The neighbourhood types reported originate from the Wijkprofiel, where neighbourhoods are given a neighbourhood type classification (*Type-1* to *Type-5*), based on neighbourhood characteristics.[5]

Type-1 neighbourhoods are functionally mixed, central neighbourhoods, with a large amount of workers relative to neighbourhood residents. Additionally, the shops and cultural institutions in these neighbourhoods attract many visitors from outside the neighbourhood.

Type-2 neighbourhoods are compact, urban neighbourhoods, located within the ringroads surrounding Rotterdam. These neighbourhoods are densely built with a large amount of small and low-valued rental housing. Most households in these neighbourhoods belong to the lower end of the income distribution.

Type-3 neighbourhoods are "greener", urban neighbourhoods. These neighbourhoods are densely built, although less so than Type-2 neighbourhoods. The value of houses is generally somewhat higher as well, and relatively more higher income households and students live in these neighbourhoods.

Type-4 neighbourhoods are the "green" suburban neighbourhoods. These neighbourhoods are located outside of the ringroads or are after-war expansionary neighbourhoods. They consist of a relatively large amount of family homes, and are mostly populated by families and the elderly.

Type-5 neighbourhoods are old villages and "golden outskirts". These neighbourhoods consist of a mixture of the most newly built expansionary neighbourhoods and old villages. The building density here is lowest of all neighbourhood types, while the density of owned, expensive, and family homes is highest.

Figure 3 depicts the development of crime rates by neighbourhood cohort over time, with a vertical line indicating the year of treatment. There appears to be a small downward trend for the treated neighbourhoods, which is not as pronounced for all untreated neighbourhoods. On first inspection, treatment does not seem to cause any trend break in crime rates, i.e. the observed trend does not appear to change after treatment.

5 Identification Strategy

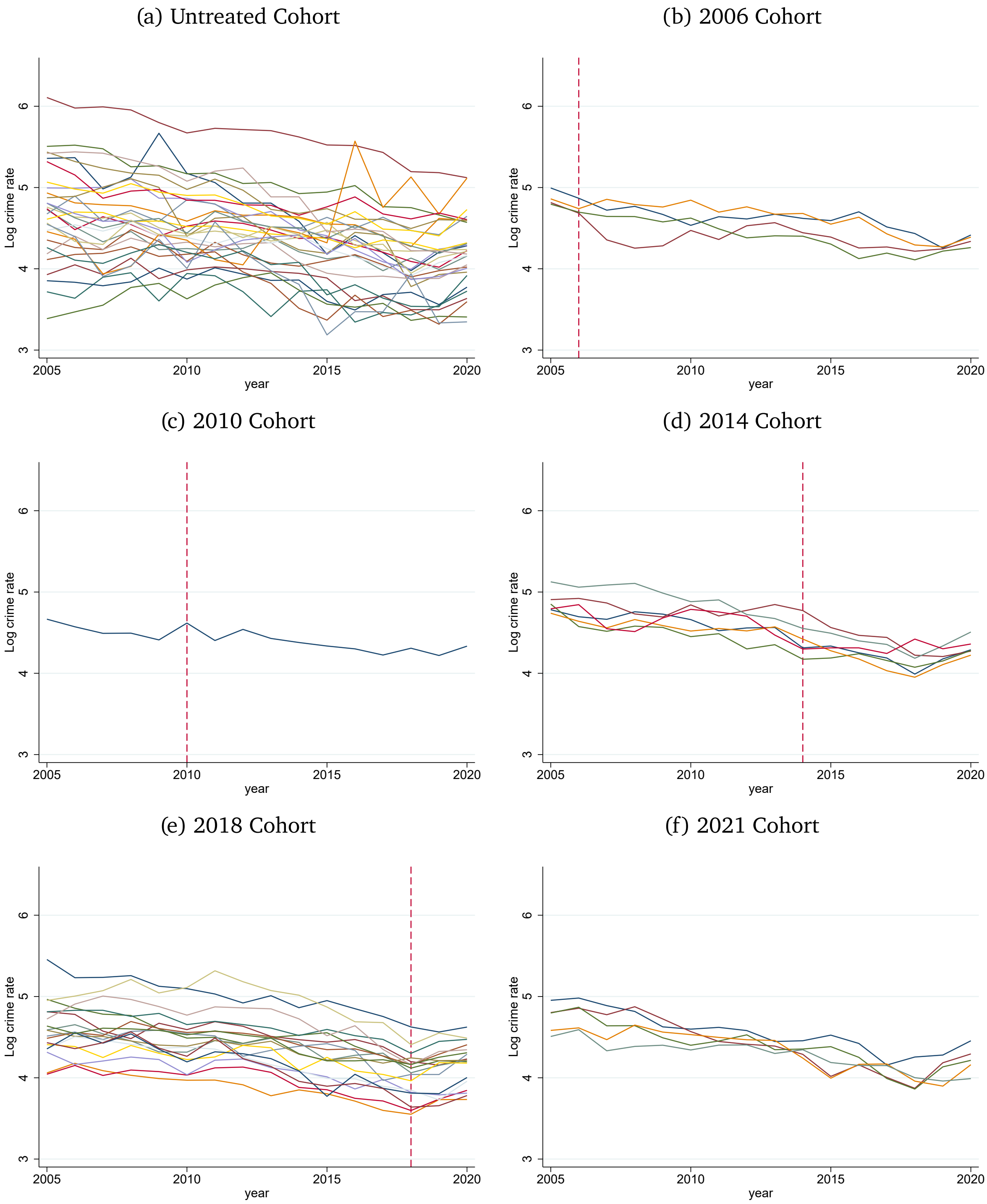
This section describes the methodology used to identify the effect of the Rotterdam Act on neighbourhood crime rates. I begin with a description of the general Event Study Difference-in-Difference (ESDiD) model, where I explain the advantages and disadvantages of this method, before extending the model to fit my research question.

5.1 General Model

The ESDiD model estimates the causal effect of a treatment on the dependent variable by comparing outcomes over both treatment and time. By including leads and lags for event time, i.e. time relative to the time of treatment, it allows for inspection of pre- and post-treatment outcome dynamics.

[5] The neighbourhood type descriptions are summarised and translated from Dutch to English by the author. Original descriptions can be found on the Wijkprofiel website, under each neighbourhood's context-indicators.

Figure 3: Log Crime Rate Development by Cohort



(a) Untreated Cohort (b) 2006 Cohort
(c) 2010 Cohort (d) 2014 Cohort
(e) 2018 Cohort (f) 2021 Cohort

Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: This figure depicts the development of log crime rate per neighbourhood. Each panel depicts one of the treatment cohorts. The vertical red dashed line represents the time of treatment for each cohort.

The general model is specified as follows:[6]

$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=T_0, k\neq -1}^{T_1} \left(\gamma_k D^k_{n,t}\right) + \varepsilon_{n,t} \ , \tag{1}$$

where n indicates a neighbourhood and t time. Coefficients α and ζ represent neighbourhood and year fixed effects, respectively. T_0 is the first observed treatment lead, T_1 the last observed treatment lag, and D^k is a dummy variable equal to 1 if a neighbourhood's event time is equal to k. Event time $k = -1$ is omitted to serve as a pre-treatment baseline and to avoid multicollinearity with neighbourhood and time fixed effects.[7] When estimated, the coefficients γ_k show the estimated treatment effect on the outcome of interest Y over time.

The coefficients for $k < -1$ give an indication if the parallel trends assumption holds and of any anticipation effects. Pre-treatment effects should be statistically insignificant for the parallel trends assumption to hold, indicating no difference over time between treated and untreated units. If one or more coefficients directly preceding the time of treatment are statistically significant and non-zero, this suggests anticipation effects may be present. Coefficients further removed from treatment should be insignificant regardless. This indicates that the observed effects are indeed due to anticipation and because of the lack of parallel trends.

An additional benefit of the (ES)DiD design is that it also allows for estimation under staggered treatment adoption,[8] adding more variation between units to exploit for estimation.

5.2 Extended Model

Before estimation, I extend the general model of Equation (1) in four ways.

5.2.1 Event Time Bins

Due to the staggered non-random adoption of treatment, the distribution of event time observations is not uniform. Values at both ends of the distribution quickly become sparse as there are much more neighbourhoods with observations close to treatment than neighbourhoods with observations long before or after treatment. In other words, the panel is unbalanced in event time. Consequently, estimation of treatment effects further away from the time of treatment may be biased due to compositional changes: what units are included in the set of observations for each event time may differ across event times. For example, early adapters of treatment will be overrepresented in long-term post treatment estimates.

To make the panel more balanced I aggregate observations for event times far away from the time of treatment. To be precise, I first test what event times have less than ten observations. Of these low frequency event times, I aggregate all the pre-treatment observations into a single group (or "bin"), and all post-treatment observations in a second bin. Although this no longer allows

[6] The subscripts (and their meaning) are chosen to fit the structure of my dataset, so that the notation of the general model is consistent with the models in the rest of this thesis.

[7] Effectively, the $T = T_1 - T_0$ terms $\gamma_k D^k_{n,t}$ denote event time fixed effects. Including all of these in addition to regular time fixed effects and unit fixed effects is not possible, as for every unit the event time and regular time fixed effects would be perfectly collinear.

[8] Although estimation is possible, recent econometric literature has found that a significant bias may arise under staggered adoption. This is briefly discussed in Section 7.

for full identification of treatment dynamics, the tail ends of the estimated treatment dynamics should be more precise as they are based on larger subsample. Furthermore, by reducing the amount of coefficients estimated the degrees of freedom increase, potentially increasing precision of estimates. The model is then specified as follows:

$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=A-1,k\neq -1}^{B+1} \left(\gamma_k D_{n,t}^k\right) + \varepsilon_{n,t} \ , \tag{2}$$

where A and B denote the first and last observed event time dummy with a frequency of ten or higher, respectively. $k = A - 1$ denotes the event time bin for all observations before event time $k = A$, while $k = B + 1$ denotes the event time bin for all observations after event time $k = B$. Aggregating event times implicitly assumes that treatment effects are constant before $k = A$ and after $k = B$; the coefficients γ_{A-1} and γ_{B+1} estimate the treatment effect for all observations before and after the considered period $A \leqslant k \leqslant B$, respectively.

5.2.2 Controls

To account for potential confounding factors I add a vector of control variables $\mathbf{x}_{n,t}$ to the model:

$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=A-1,k\neq -1}^{B+1} \left(\gamma_k D_{n,t}^k\right) + \boldsymbol{\beta}'\mathbf{x}_{n,t} + \varepsilon_{n,t} \ , \tag{3}$$

where $\boldsymbol{\beta}$ is a vector of coefficients corresponding to the vector of controls $\mathbf{x}_{n,t}$. Specifically, I control for the population density of each neighbourhood and the amount of schools in each neighbourhood.

I add baseline characteristics for each neighbourhood interacted with either a linear time trend or year fixed effects as addition control variables:

$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=A-1,k\neq -1}^{B+1} \left(\gamma_k D_{n,t}^k\right) + \boldsymbol{\beta}'\mathbf{x}_{n,t} + \boldsymbol{\phi}'\mathbf{C}_n + \varepsilon_{n,t} \ , \tag{4}$$

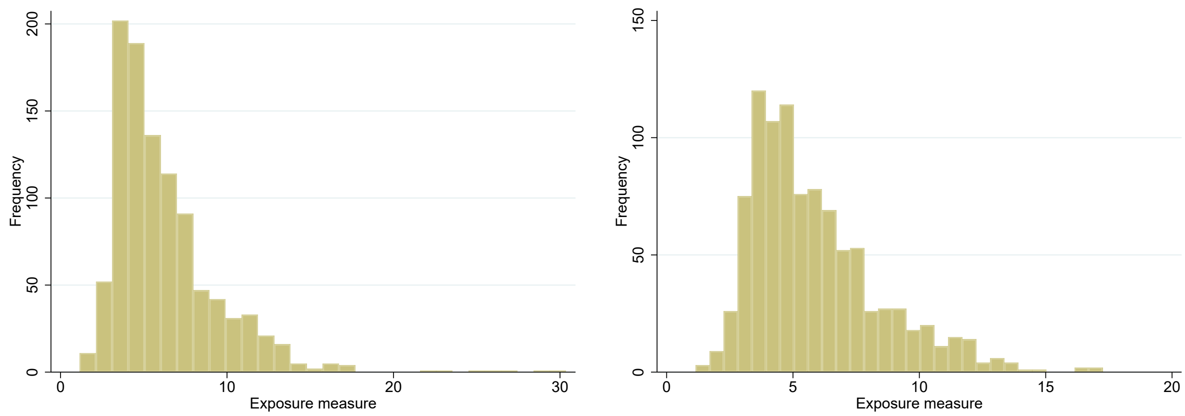
where $\mathbf{C}_n$ is a vector of baseline characteristics and $\boldsymbol{\phi}$ is the corresponding vector of coefficients. For the full year fixed effects interaction the model instead becomes:

$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=A-1,k\neq -1}^{B+1} \left(\gamma_k D_{n,t}^k\right) + \boldsymbol{\beta}'\mathbf{x}_{n,t} + \boldsymbol{\phi}_t'\mathbf{C}_n + \varepsilon_{n,t} \ , \tag{5}$$

where vector $\boldsymbol{\phi}_t$ now varies over time. I include the fraction of low income households, the fraction of social housing, the resident-job balance, and neighourhood type for each neighbourhood as baseline characteristics.[9]

Figure 4: Exposure Distribution

(a) With Outliers (b) Without Outliers



Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: This figure depicts the frequency distribution of neighbourhood treatment exposure values. Panel (a) depicts the original distribution, while panel (b) depicts the distribution after removing outliers.

5.2.3 Measure of Treatment Exposure

The measures provided to municipalities by the Rotterdam Act can only be applied to social housing. Furthermore, the grounds on which a housing permit can be denied apply only to those who have not continuously lived in the municipality in the six years prior to requesting a housing permit. Intuitively, this means that neighbourhoods with a larger amount of settlements from outside the municipality are affected more by the Act than neighbourhoods with a smaller amount of settlements from outside the municipality. Following this reasoning, I use settlements from outside the municipality to construct a proxy for treatment exposure:

$$E_{n,t} = \frac{\text{Settlements}_{n,t}}{\text{Population}_{n,t}} \cdot 100\% \,,$$

so that the exposure for neighbourhood n at time t is equal to the amount of settlements from outside the municipality as a percentage of that neighbourhood's population.

Figure 4a depicts the distribution of exposure values. There are several outliers (with a value over twenty) visible on the right side of the distribution. Singling out observations with an exposure more than three standard deviations away from the median exposure shows these observations belong to three different neighbourhoods: Kralingen Oost/Kralingse Bos, Stadsdriehoek, and Struisenburg. All three of these neighbourhoods have a large student population ($> 10\%$), which possibly accounts for the large amount of settlements relative to neighbourhood population.

As the Rotterdam Act does not apply to student housing, a large student population may inflate the exposure measure, potentially biasing estimates. I thus drop all neighbourhoods with an average student population of over fifteen percent from the sample, corresponding to Delfshaven,

[9] For the full year fixed effects and baseline characteristics interaction, neighbourhood type is excluded from the model. The large amount of variables this interaction generates reduces a large amount of variation and thus leads to a risk of overfitting.

Table 2: Exposure by Cohort

	Cohort						Total
	Untreated	2006	2010	2014	2018	2021	
Exposure							
Mean	5.85	8.67	6.54	8.39	4.82	4.90	5.87
SD	2.74	2.17	0.63	2.71	1.35	1.52	2.55
Standardised Exposure							
Mean	−0.01	1.10	0.26	0.99	−0.41	−0.38	0.00
SD	1.08	0.85	0.25	1.06	0.53	0.60	1.00
Observations	432	64	16	80	288	80	960

Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: This table reports the mean and standard deviation of neighbourhood treatment exposure and standardised neighbourhood treatment exposure for all cohorts. Values are calculated over the 2005–2020 period.

Kralingen Oost/Kralingse Bos, and Struisenburg.

Figure 4b depicts the distribution of the exposure values after dropping these neighbourhoods. By excluding neighbourhoods with a large student population the strongest outliers appear to have been removed from the sample.

After removing outliers I standardise the exposure measure:

$$E^s_{n,t} = \frac{E_{n,t} - \bar{E}}{s_E}, \tag{6}$$

so that a standardised exposure measure of zero indicates that a neighbourhood's exposure is equal to the sample mean exposure of all neighbourhoods—both treated and untreated—over all observed years. A standardised exposure measure of one then indicates an exposure one standard deviation larger than the sample mean exposure.

Table 2 reports the mean and standard deviation of both standardised and unstandardised exposure per cohort, as well as for the entire sample. While the untreated cohort's exposure is very close to that of the sample mean, the 2006 and 2014 cohorts are much more exposed. If my reasoning holds, this suggests that these cohorts are likely more affected by the Rotterdam Act than the other cohorts.

To test if the exposure measure is independent of treatment I estimate both a general DiD and a general ESDiD model with (standardised) exposure as dependent variable. For the DiD model I estimate the following equation:

$$E_{n,t} = \alpha_n + \zeta_t + \gamma \text{Post}_{n,t} + \beta \frac{\text{Emigrations}_{n,t}}{\text{Population}_{n,t}} + \varepsilon_{n,t} \tag{7}$$

where $\text{Post}_{n,t}$ is a dummy variable equal to one if the observation is post-treatment and zero otherwise, and Emigrations/Population controls for the amount of people from the neighbourhood moving out of the municipality ("emigrations") relative to that neighbourhood's population.

For the ESDiD model I estimate Equation (3) where vector x consists of the ratio of emigrations to neighbourhood population.

Table 3 reports the estimates of Equation (7). Column (1) reports the estimates for the unstan-

Table 3: Difference-in-Difference Estimates of Treatment Effect on Exposure

Coefficients	Exposure (1)		Standardised Exposure (2)	
Post	−0.1128	(0.2298)	−0.0442	(0.0901)
Relative Emigration	0.4857***	(0.1142)	0.1905***	(0.0448)
Constant	3.0421***	(0.6667)	−1.1093***	(0.2615)
Year FE	Yes		Yes	
Neighbourhood FE	Yes		Yes	
Observations	960		960	
R^2	0.862		0.862	
Adjusted R^2	0.850		0.850	
Within R^2	0.162		0.162	

Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: This table reports difference-in-difference estimates of the treatment effect on exposure. Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

dardised exposure measure, while Column (2) reports the estimates for the standardised exposure measure. While rescaled by the standard deviation, the estimated coefficients in Column (2) have exactly the same sign and statistical significance as the coefficients in Column (1). The R^2 statistics being equal for both models further underlines this.

Figure 5 presents the estimates of the ESDiD model. Figure 5a and Figure 5b depict the estimates for unstandardised and standardised exposure, respectively. Exact coefficients for the ESDiD estimates are reported in Appendix Table A1. Again, while rescaled, the estimates for standardised and unstandardised exposure appear equivalent.

The estimates indicate that the relative amount of people moving into a neighbourhood is not significantly affected by the application of the Rotterdam Act. The relative amount of people moving out of a neighbourhood does appear to be a good predictor, however. This can be easily accounted for: for each person that moves out of a neighbourhood, a vacancy appears for people from outside that neighbourhood. Note that this is still the case when a vacancy is filled by people from the same neighbourhood—the residency they leave will, after all, become vacant instead.

Following these results, I conclude that there appears to be no confounding factor and that the exposure measure is thus fit to be used. I thus extend the model to include estimates for the effect of treatment exposure:

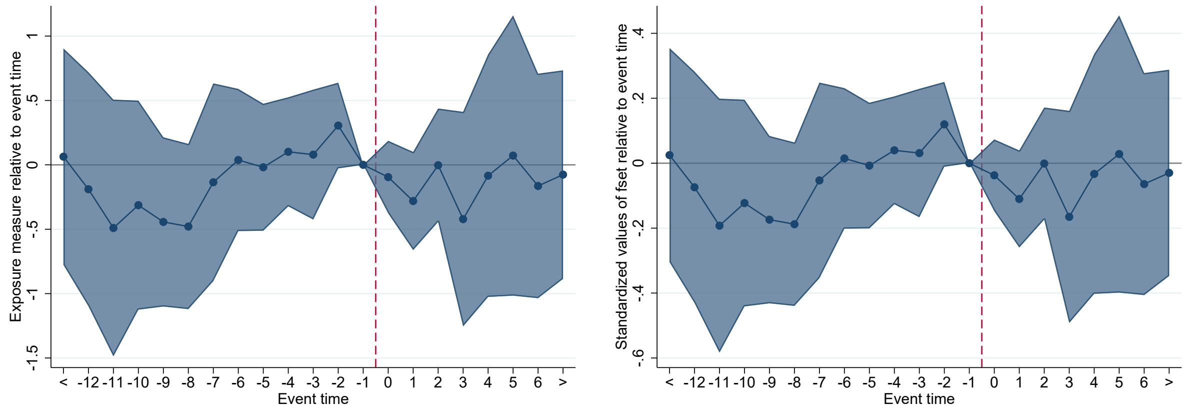
$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=A-1,k\neq-1}^{B+1} \left(\gamma_k D^k_{n,t} + \delta_k D^k_{n,t} \times E^s_{n,t}\right) + \rho E^s_{n,t} + \boldsymbol{\beta}'\mathbf{x}_{n,t} + \boldsymbol{\phi}'_t \mathbf{C}_n + \varepsilon_{n,t} ,$$

where standardised exposure measure E^s of Equation (6) is interacted with the event time dummies, so that coefficients δ_k trace out how the outcome of interest develops over time for more exposed neighbourhoods compared to less exposed neighbourhoods.

Note that by using the standardised exposure, the coefficients γ_k trace out how the outcome of interest develops over time for neighbourhoods with exposure equal to the sample mean: their standardised exposure would be zero. If unstandardised exposure is used, coefficients γ_k would instead trace out the development of the outcome of interest if exposure were nil—the effects of

Figure 5: Event Study Estimates of Treatment Effect on Exposure

(a) Unstandardised Exposure (b) Standardised Exposure



Source: Municipality of Rotterdam; OBI; Onderzoek010 database, author's calculations.
Notes: This figure depicts the estimates of a ESDiD model with the exposure measure as dependent variable. The plotted line represents the coefficients while the shaded area encompasses a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) and Panel (b) depict estimates for unstandardised and standardised exposure, respectively.

mean exposure would be estimated by the combination of the interaction term and the regular event time coefficients. An exposure of zero implies no one from outside the municipality would move into a neighbourhood at all, in which case the Rotterdam Act would have no effect. I find this unrealistic as no single observation in my sample has an exposure of zero (see Figure 4), suggesting this is indeed not very plausible. For the remainder of this thesis I thus use the standardised exposure and when mentioning exposure I refer to standardised exposure.

5.2.4 No Anticipation Effects

As the application or renewal of Rotterdam Act measures happen every four years, anticipation effects are possible if and only if potential tenants are aware (1) of the Rotterdam Act, (2) at what time the Rotterdam Act is applied, (3) if their prospective new home is subjected to Rotterdam Act measures, (4) if they meet the requirements of Rotterdam Act measures at the time of application. This would require potential tenants to be exceptionally well-informed or perhaps even have some degree of foresight, especially considering the very short timeframe of social housing vacancies.[10] I thus assume there are no anticipation effects and aggregate all pre-treatment event time dummies into a single bin. Consequently, the first considerend unbinned event time is now $A = 0$, so that:

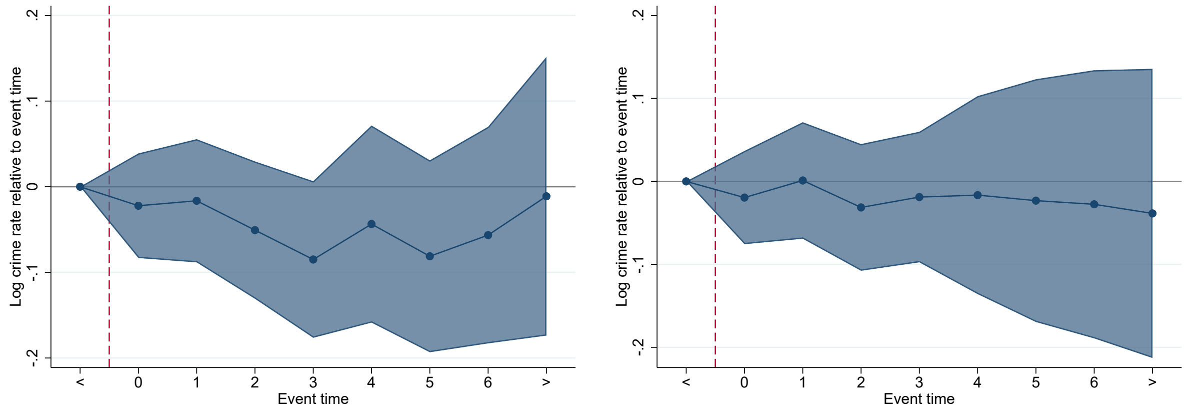
$$Y_{n,t} = \alpha_n + \zeta_t + \sum_{k=0}^{B+1} \left(\gamma_k D^k_{n,t} + \delta_k D^k_{n,t} \times E_{n,t}\right) + \rho E_{n,t} + \boldsymbol{\beta}'\mathbf{x}_{n,t} + \boldsymbol{\phi}'_t \mathbf{C}_n + \varepsilon_{n,t}\,, \tag{8}$$

where all pre-treatment observations are now grouped together under $k = -1$ and omitted as the reference category.

[10] At the time of writing, not a single home listed on *Woonnet Rijnmond* had a timeframe larger than 8 days for application. NB: *Woonnet Rijnmond* is the sole provider of social housing vacancies in the municipality of Rotterdam.

Figure 6: Event Study Estimates of General Treatment Effect

(a) General Model (b) Extended Model



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of ESDiD models with neighbourhood log crime rate as dependent variable. The plotted lines represents the coefficients while the shaded area encompasses a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) depicts the coefficients of the general model with linear baseline interactions. Panel (b) depicts the coefficients of the extended model with baseline interactions.

6 Results

This section presents the estimation results of the models presented in Section 5. It begins by presenting the results of the models without the exposure measure (Section 6.1) and continues with the estimates of the models including the exposure measure (Section 6.2). Only the plots for the model with the highest Within R^2 are reported in the main text. Plots for the other models are reported in the appendix. Section 7 presents several robustness checks and their implications.

6.1 General Model

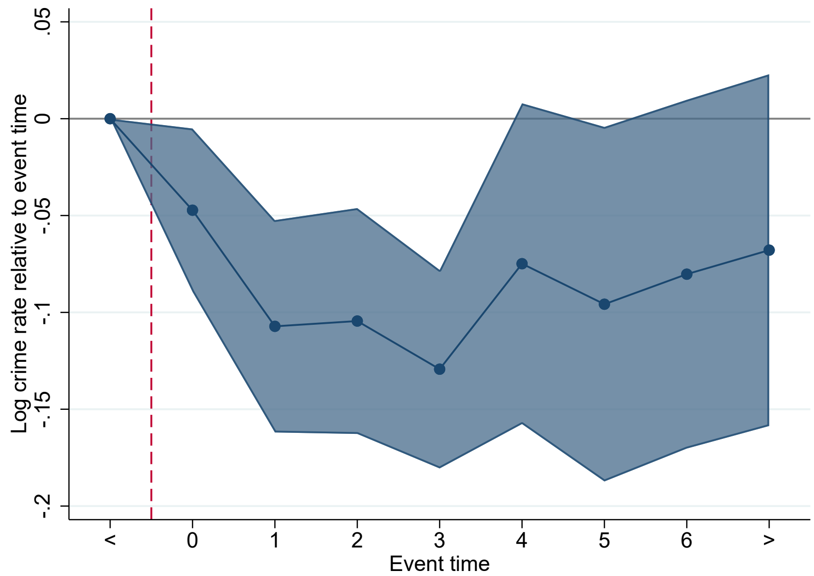
Figure 6a plots the event time coefficients for the model estimated using Equation (4), with $A = 0$. Plots of the coefficients estimated using Equations (2), (3) and (5) are presented in Appendix Figure B1a. Exact coefficients and standard errors for all estimated models are reported in Columns (1)–(4) of Appendix Table A2.

Although the estimated event time coefficients appear to diverge from zero, they do not significantly differ from zero, suggesting the Rotterdam Act had no effect on neighbourhood crime rates. This is somewhat in line with the findings of Hochstenbach et al. (2015) and Kromhout et al. (2021), who find that the Rotterdam Act has not contributed to an increase in neighbourhood safety.

6.2 Extended Model

Figure 6b plots the coefficients of the event time dummies for the model estimated using a specification of Equation (8), corresponding with the general model of Equation (4). Plots for the coefficients estimated using the extended models corresponding to Equations (2), (3) and (5)

Figure 7: Event Study Estimates of Exposure Treatment Effect



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of a ESDiD model with neighbourhood log crime rate as dependent variable. The plotted line represents the coefficients while the shaded area encompasses 95% confidence interval. The red dashed line indicates the time of treatment.

are presented in Appendix Figure B1b. Exact coefficients and standard errors for all estimated models are reported in Columns (5)–(8) of Appendix Table A2.

Again, all coefficients do not significantly differ from zero. The coefficients are closer to zero compared to the general model, supporting the idea that the application of the Rotterdam Act on average has no effect on crime rates.

Figure 7 plots the coefficients of the event time dummies interacted with the exposure measure for the same model as in Figure 6b. Plots for the other models are presented in Appendix Figure B2. Exact coefficients and standard errors for all estimated models are reported in Appendix Table A3.

In contrast with the general event time coefficients, the exposure coefficients do diverge from zero. The coefficients are significantly lower than zero for the first post-treatment years, suggesting that neighbourhood crime rates decrease if a neighbourhood's exposure is higher than the sample mean exposure. This decrease does appear transient, however, as coefficients get closer to zero with a wider confidence interval in the following years.

To estimate an average treatment effect I aggregate all post-treatment event time dummies:[11]

$$Y_{n,t} = \alpha_n + \zeta_t + \gamma \text{Post}_{n,t} + \delta \text{Post} \times E^s_{n,t} + \rho E_{n,t} + \boldsymbol{\beta}' \mathbf{x}_{n,t} + \boldsymbol{\Phi}'_t \mathbf{C}_n + \varepsilon_{n,t} \,, \tag{9}$$

where all treatment effects are now captured by coefficients γ and δ.

Table 4 reports coefficients estimated with these models. Application of the Act in and of

[11] Note that by aggregating all event time dummies the model reverts to a regular DiD model. Furthermore, due to the smaller amount of independent variables, including the type#year interaction brings much less risk of overfitting and is thus included.

Table 4: Difference-in-Difference Estimates

Coefficients	Log Crime Rate			
	(1)	(2)	(3)	(4)
Post	0.0019	0.0103	−0.0130	0.0035
	(0.0363)	(0.0358)	(0.0299)	(0.0419)
Exposure	0.0249	0.0222	0.0060	0.0165
	(0.0392)	(0.0400)	(0.0286)	(0.0310)
Post # Exposure	-0.0581^{*}	−0.0417	-0.0928^{***}	-0.0972^{***}
	(0.0241)	(0.0257)	(0.0228)	(0.0230)
Controls	No	Yes	Yes	Yes
Linear baseline #	No	No	Yes	No
Full baseline #	No	No	No	Yes
Observations	960	960	944	928
R^2	0.881	0.885	0.917	0.916
Adjusted R^2	0.870	0.874	0.909	0.896
Within R^2	0.013	0.046	0.093	0.040

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This table reports the estimates of DiD models with neighbourhood log crime rate as dependent variable. Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

itself does not appear to have a significant effect on crime rates in any of the models. The exposure interaction, however, is significantly lower than zero for models (1), (3), and (4), confirming the results of the Event Study estimates. My preferred model, (3), estimates that an increase of one standard deviation in a neighbourhood's exposure to the Rotterdam Act, i.e. a one standard deviation increase in the amount of settlements from outside the municipality relative to a neighbourhood's population, decreases neighbourhood crime rates by 8.86%[12].

In conclusion, my results suggest that the Rotterdam Act on average has no effect on crime rates. The Rotterdam Act does however reduce neighbourhood crime rates if and only if the amount of people moving into that neighbourhood after application of the Act is large enough.

7 Robustness

This section begins with an analysis of the estimated model's residuals. It then continues by relaxing several of the assumptions made in Section 5 and discussing their implications. In this section the preferred model (including exposure and linear baseline interactions) is used for comparison.

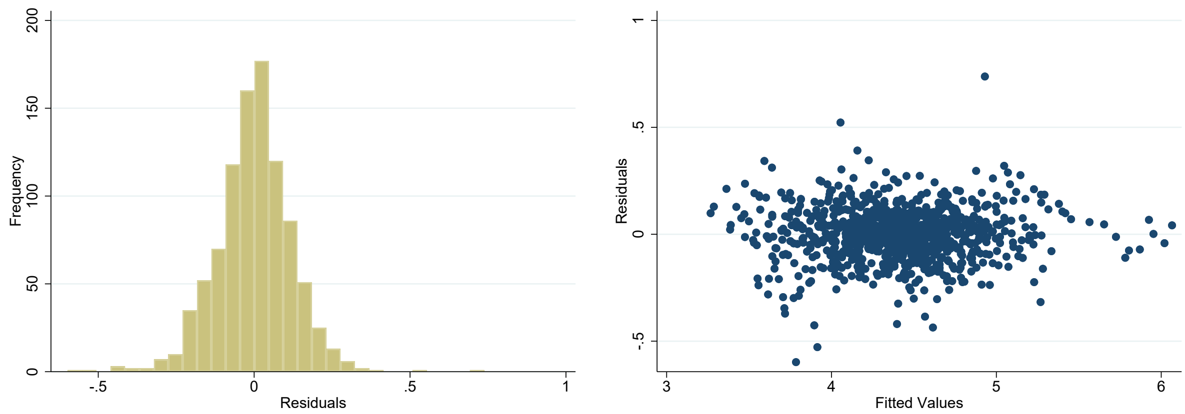
7.1 Residual Analysis

In panel data models serial correlation is likely to occur due to the included time dimension. In general, a quick and easy fix to account for this is clustering standard errors at a higher level than panel units (Chapter 8, Angrist & Pischke, 2009). Unfortunately, this easy fix is not feasible as it

[12] $(\exp(-0.0928) - 1) \cdot 100\% = -8.86\%$

Figure 8: Residual Plots

(a) Residual Distribution (b) Residuals vs Fitted Values



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the distribution of the ESDiD model's residuals. Panel (a) depicts the frequency distribution of residuals. Panel (b) plots the residuals against the fitted values of the model.

would leave the model with a small amount of clusters, increasing the risk of spurious identifcation of treatment effects. For any meaningful inference it is therefore necessary to test for residual serial correlation in addition to testing for normality of residuals.

Figure 8a plots the distribution of residuals for the event study model. Figure 8b plots the residuals against the fitted values for the event study model. The residuals appear homoskedastic and approximately normal, suggesting that the model is a good fit.

I perform the bias-corrected Born and Breitung (2015) LM(1)-test and the Inoue and Solon (2006) LM-test to test for residual serial correlation. The null hypothesis for these tests is no serial correlation of order 1 and no serial correlation of any order, respectively. The alternative hypotheses claim some serial correlation of order 1 and serial correlation up to order 1, respectively. Both tests thus test for AR(1) correlation of the residuals. Table 5 Column (1) reports the results of both tests. Both tests return a very small p-value and reject the null hypotheses. As this indicates residual serial correlation in the model, the earlier estimates cannot be assumed to be correct.

I add an AR(1) term for log crime rate ($Y_{n,t-1}$) to the model to account for serial correlation and perform both tests again. Results are reported in Column (2) of Table 5. Both tests no longer reject the null hypothesis, suggesting residuals are no longer serially corrrelated. Figure 9 depicts the residual plots for the new model. Residuals again appear homoskedastic and approximately normal, suggesting the new model is a good fit.

Figure 10 depicts the estimated coefficients of the AR(1) model. Exact coefficients and standard errors are reported in Appendix Table A4 Column(2), with Column (1) containing the estimates without the AR(1) term for comparison. Regular event time coefficients appear largely unchanged, moving slightly closer to zero. In contrast, the exposure coefficients have decreased for $k \geqslant 2$, while remaining statistically significant. Table 6 reports estimates for the DiD model estimated with the AR(1) term in Column(1), with the estimates without the AR(1) term in Column(1)for comparison.

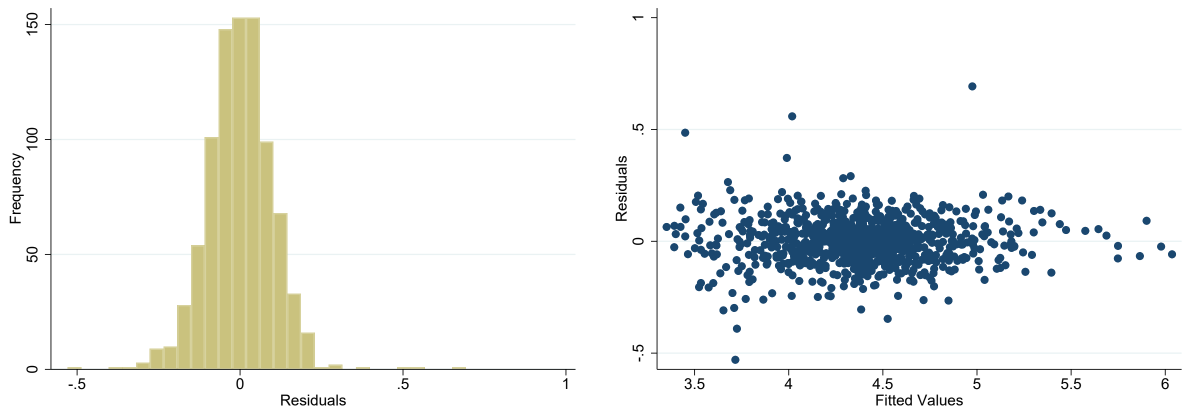
Table 5: Serial Correlation Tests for Residuals

Test	Event Study (1)	with AR(1) (2)
Born and Breitung		
LM(1)-stat	5.38	0.23
p-value	0.000	0.818
Inoue and Solo		
IS-stat	31.99	18.18
p-value	0.006	0.199
N	59	59
Max T	16	15
Balanced Panel	Yes	Yes

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This table reports the results of the serial correlation tests on the residuals of the ESDiD model.

Figure 9: Residual Plots with AR(1) Term

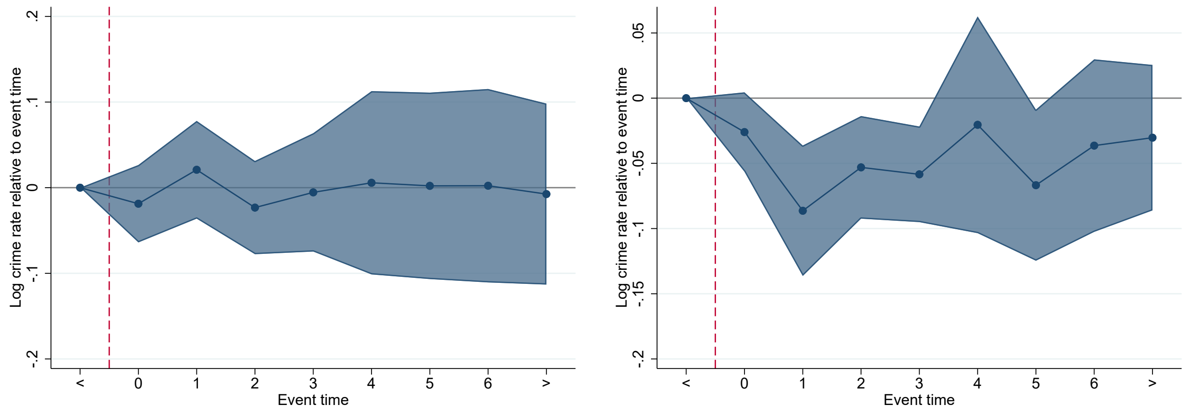
(a) Residual Distribution (b) Residuals vs Fitted Values



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes:This figure depicts the distribution of the AR(1) ESDiD model's residuals. Panel (a) depicts the frequency distribution of residuals. Panel (b) plots the residuals against the fitted values of the model.

Figure 10: Event Study Estimates of AR(1) Model

(a) General Treatment Effect (b) Exposure Treatment Effect



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of the AR(1) ESDiD model with neighbourhood log crime rate as dependent variable. The plotted lines represents the coefficients while the shaded area encompasses a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) depicts the regular event time coefficients, while panel (b) depicts the exposure interacted event time coefficients.

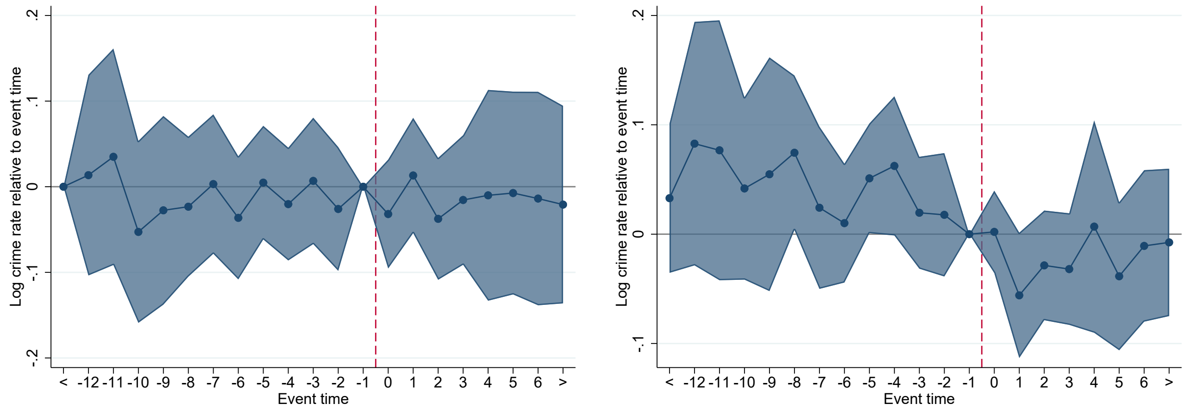
Table 6: Difference-in-Difference Estimates of AR(1) Model

Coefficients	DiD (1)	With AR(1) (2)
Post	−0.0130	0.0046
	(0.0299)	(0.0228)
Exposure	0.0060	0.0020
	(0.0286)	(0.0183)
Post # Exposure	−0.0928***	−0.0545***
	(0.0228)	(0.0142)
Lag 1 Log Crime Rate		0.4647***
		(0.0446)
Observations	944	885
R^2	0.917	0.937
Adjusted R^2	0.909	0.930
Within R^2	0.093	0.308

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This table reports the estimates of the AR(1) DiD model with neighbourhood log crime rate as the dependent variable. Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 11: Event Study Estimates of Treatment Anticipation Effects

(a) General Treatment Effect (b) Exposure Treatment Effect



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of the AR(1) ESDiD model with neighbourhood log crime rate as dependent variable, including pre-treatment event times. The plotted lines represents the coefficients while the shaded area encompasses a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) depicts the regular event time coefficients, while panel (b) depicts the exposure interacted event time coefficients.

As with the ESDiD estimates, the post-treatment coefficient remains insignificantly different from zero, while the post-exposure interaction coefficient decreases in size. The AR(1) term is, as expected, significant and positive. As the coefficient for the AR(1) term is almost one half, it is not surprising the post-exposure coefficient almost halved. After all, almost half of the previous year's decrease in crime rates due to the post-exposure interaction will be carried over by the AR(1) term. In practice, the AR(1) DiD model estimates that a one standard deviation increase in exposure leads to a 5.30%[13] decrease in neighbourhood crime rates. Including the AR(1) term takes care of serial correlation in the residuals, while simultaneously increasing the within R^2. I therefore include it in the remainder of the robustness checks.

7.2 No Anticipation

To check if the assumption of no anticipation effects in Section 5.2.4 is valid, I re-estimate the model including pre-treatment dummies. Figure 11 plots the coefficients of the re-estimated model. Exact coefficients and standard errors are reported in Appendix Table A5.

For both regular (Figure 11a) and exposure interacted (Figure 11b) event time dummies, all pre-treatment coefficients do not significantly differ from zero. I therefore conclude that the assumption of no anticipation effects is valid.

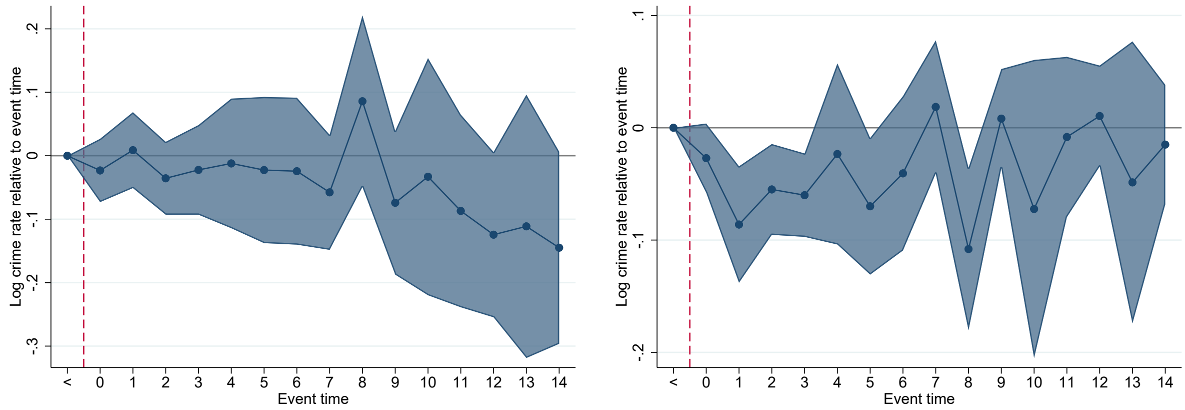
7.3 Event Time Binning

By binning low frequency event time observations, I implicitly assume treatment effects beyond the considered post-treatment event time remains constant. To see if this is indeed the case I

[13] $(\exp(-0.0545) - 1) \cdot 100\% = -5.30\%$

Figure 12: Event Study Estimates without Binning Event Times

(a) General Treatment Effect (b) Exposure Treatment Effect



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of the AR(1) ESDiD model with neighbourhood log crime rate as dependent variable, without binning post-treatment event times. The plotted lines represents the coefficients while the shaded area encompasses a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) depicts the regular event time coefficients, while panel (b) depicts the exposure interacted event time coefficients.

re-estimate the model without binning post-treatment dummies. Figure 12 plots the resulting coefficients of this re-estimated model. Exact coefficients are reported in Appendix Table A6.

The regular coefficients (Figure 12a) appear to follow a downward trend, but remain insignificant. The average treatment effect thus appears to remain zero. The exposure interacted coefficients (Figure 12b) remain mostly unchanged for the previously estimated event times. Afterward, the pattern becomes erratic and inaccurate. Taking into account the size of the cohorts this is not unsurprising: only the 2006 and 2010 cohorts are included for $k > 6$, as the observed period goes up to 2020. Estimates for $k > 6$ are thus based on a maximum of only five observations and therefore prone to inaccurate estimation.

As the coefficients for $k \leqslant 6$ appear to follow the same pattern as the model with binned event time dummies and coefficients for $k > 6$ do not provide a trustworthy estimate, I conclude that the binning of event time dummies is justified.

7.4 Staggered Adoption Bias

Recent econometric literature has found that a significant bias may arise when using the Two-Way Fixed Effects DiD estimator with staggered treatment adoption and dynamic treatment effects, the canonical ESDiD specification (e.g. Baker et al., 2022; Goodman-Bacon, 2021; Sun & Abraham, 2021). Specifically, event time coefficient estimates are contaminated by other event times, i.e. a given event time is compared not only to the reference event time ($k = -1$), but also to other event times included in the specification *and* event times *excluded* from the specification. The bias then arises when treatment not only differs over time, but also by cohort, i.e. when there are heterogenous (dynamic) treatment effects (Borusyak et al., 2022; Sun & Abraham, 2021).[14]

[14] See Cunningham (2022) for a concise illustriation of this bias.

Table 7: Difference-in-Difference Estimates by Cohort

Coefficients	Log Crime Rate (1)	
Post	0.0029	(0.0288)
Exposure	0.0051	(0.0259)
Lag 1 Log Crime Rate	0.4634***	(0.0469)
Post # Exposure	−0.0612*	(0.0249)
Post # Cohort		
2006	0.0444	(0.0595)
2010	0.0518	(0.0521)
2014	−0.0195	(0.0741)
2018	Omitted	
Exposure # Cohort		
2006	−0.0118	(0.0544)
2010	0.0301	(0.0522)
2014	−0.0110	(0.0386)
2018	−0.0131	(0.0299)
Post # Exposure # Cohort		
2006	0.0083	(0.0470)
2010	−0.0734	(0.0783)
2014	0.0189	(0.0425)
2018	Omitted	
Controls	Yes	
Linear baseline #	Yes	
Observations	885	
R^2	0.937	
Adjusted R^2	0.930	
Within R^2	0.309	

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This tables report the estimates of the AR(1) DiD model with cohort interaction terms. Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

While the implementation of new estimators suggested to tackle this potential bias is beyond the scope of this thesis, I nonetheless perform a simple test to see if treatment effects are heterogenous across cohorts. Specifically, I estimate Equation (9) with the post-treatment dummy and exposure measure now also fully interacted with cohort dummies.[15]

Table 7 reports the results of this estimation. The 2018 cohort is omitted from the Post#Cohort interactions to serve as a reference category as the untreated cannot be a reference for variation in treatment effects. All estimated cohort interacted coefficients are not significantly different from zero, suggesting that treatment effects are not significantly different between cohorts. Furthermore, the estimated treatment effect for all cohorts remains significant and similar in size to the previous estimate. I thus conclude that bias due to heterogenuous treatment effects and staggered treatment adoption is unlikely.

[15] Note that the 2021 cohort is coded as untreated, as it does not receive treatment within the observed time period and thus—by construction—cannot introduce variation or bias in treatment effect estimates.

8 Conclusion

This thesis estimates the effect of the application of the Rotterdam Act on neighbourhood crime rates. Using both regular and Event Study Difference-in-Difference models, I show how neighbourhood crime rates are affected by the application of Rotterdam Act measures in a neighbourhood. I find that the Act on average does not cause a change in neighbourhood crime rates. By including a measure for treatment exposure I allow for heterogenuous treatment effects and find that although the average treatment effect remains insignificant, treatment effect does depend on treatment exposure. Furthermore, I check for robustness of my results by challenging several assumptions made and checking for potential bias arising from staggered treatment adoption, as described in recent econometric literature (e.g. Baker et al., 2022; Borusyak et al., 2022; Goodman-Bacon, 2021; Sun & Abraham, 2021).

On the one hand, my findings are in line with previous studies (e.g. Hochstenbach et al., 2015; Kromhout et al., 2021), suggesting the Rotterdam Act, on average, has no effect on neighbourhood crime rates. On the other hand, in contrast with previous literature, I find that the Rotterdam Act does affect crime rates, although this depends on how exposed a neighbourhood is to the Act: if a neighbourhood's exposure increases, crime rates decrease, and vice-versa.

This thesis thus underlines the importance of modeling a treatment's mechanism when estimating the effects of policy interventions. While the exposure measure used in this thesis already leads to different results than previous literature, it would most certainly be worthwhile to develop a less crude measure for estimating the Rotterdam Act's effects. Such a measure could then be used to, for example, reassess results of previous literature and this thesis, or applied to a larger sample of neighbourhoods than just those inside the municipality of Rotterdam.

Outside of academic research, I believe policy makers should pay extra attention to neighbourhood susceptability when considering applying these—or similar—measures to improve neighbourhood safety. Without considering real-world circumstances there could be no effect at all, or even the opposite of what was intended. Furthermore, it is important to remember that although the Rotterdam Act measures can reduce crime rates in a neighbourhood, they do not tackle the cause of these crimes. The people who are barred from moving into the neighbourhood—whom the Act (and thus the models in this thesis) implicitly appoint as the cause of crime—will still have to live somewhere, after all. The Act should thus only be applied in conjunction with other measures aimed at the root cause of crime rates, as the Act itself only redistributes this problem. Finally, one must always consider if this redistribution is worth the cost of barring people from living in certain neighbourhoods, which can be seen as nothing more than socio-economic discrimination.

Data Availability

Code replicating the combining of data, estimation, and tables and figures in this thesis can be found in a repository on my personal Github page (https://github.com/Ahvns/BScThesisCode).

All neighbourhood data is obtained from the municipality of Rotterdam through the *Onderzoek010* database (available at https://onderzoek010.nl/) and *Wijkprofiel Rotterdam* (available at https://wijkprofiel.rotterdam.nl/).

Acknowledgements

Several user-written Stata packages were used when writing this thesis, namely: `xtistest` and `xtqptest` (Wursten, 2018), `reghdfe` (Correia, 2016), `coefplot` (Jann, 2014), and `estout` (Jann, 2005).

References

Angrist, J. D., & Pischke, J.-S. (2009). *Mostly harmless econometrics - an empiricist's companion*. Princeton University Press.

Atkinson, R. (2005). *Neighbourhoods and the impacts of social mix: Crime, tenure diversification and assisted mobility*. Housing and Community Research Unit / ESRC Centre for Neighbourhood Research. http://neighbourhoodchange.ca/wp-content/uploads/2011/06/Atkinson-2005-Nhoods-and-Impacts-of-Social-Mix.pdf

Baker, A. C., Larcker, D. F., & Wang, C. C. (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, *144*, 370–395. https://doi.org/10.1016/j.jfineco.2022.01.004

Born, B., & Breitung, J. (2015). Testing for serial correlation in fixed-effects panel data models. *Econometric Reviews*, *35*, 1290–1316. https://doi.org/10.1080/07474938.2014.976524

Borusyak, K., Jaravel, X., & Spiess, J. (2022). Revisiting event study designs: Robust and efficient estimation. *SSRN Electronic Journal*. https://doi.org/10.2139/SSRN.2826228

Correia, S. (2016). *Linear models with high-dimensional fixed effects: An efficient and feasible estimator* (tech. rep.) [Working Paper].

Cunningham, S. (2022). *Illustrating the bias of twfe in event studies*. https://causalinf.substack.com/p/illustrating-the-bias-of-twfe-in?s=r

Directie Democratie en Burgerschap. *Factsheet wet bijzondere maatregelen grootstedelijke problematiek*. 2016, October. https://www.rijksoverheid.nl/documenten/brochures/2015/04/01/factsheet-wet-bijzondere-maatregelen-grootstedelijke-problematiek

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*, 254–277. https://doi.org/10.1016/j.jeconom.2021.03.014

Hochstenbach, C., Uitermark, J., & van Gent, W. (2015). *Evaluatie effecten wet bijzondere maatregelen grootstedelijke problematiek ("rotterdamwet") in rotterdam*. AISSR, Universiteit van Amsterdam. https://hdl.handle.net/11245/1.495535

Inoue, A., & Solon, G. (2006). A portmanteau test for serially correlated errors in fixed effects models. *Econometric Theory*, *22*(5), 835–851. https://doi.org/10.1017/S0266466606060385

Jann, B. (2005). Making regression tables from stored estimates. *The Stata Journal*, *5*, 288–308. https://doi.org/https://doi.org/10.1177%2F1536867X0500500302

Jann, B. (2014). Plotting regression coefficients and other estimates. *The Stata Journal*, *14*, 708–737(30). https://doi.org/https://doi.org/10.1177%2F1536867X1401400402

Kromhout, S., Kranenborg, P., Wever, T., & Vrieler, A. (2021). *Selectieve woningtoewijzing*. RIGO. https://www.eerstekamer.nl/eu/overig/20210812/selectieve_woningtoewijzing/document3/f=/vllskwe1mnyq_opgemaakt.pdf

Pawson, H., & Kintrea, K. (2002). Part of the problem or part of the solution? social housing allocation policies and social exclusion in britain. *Journal of Social Policy*, *31*, 643–667. https://doi.org/10.1017/S0047279402006797

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*, 175–199. https://doi.org/10.1016/J.JECONOM.2020.09.006

Uitermark, J., Hochstenbach, C., & van Gent, W. (2017). The statistical politics of exceptional territories. *Political Geography*, *57*, 60–70. https://doi.org/10.1016/j.polgeo.2016.11.011

van Dooijeweert, A. (2016). *Brief aan de voorzitter van de tweede kamer over het wetsvoorstel selectieve woningtoewijzing*. College voor de Rechten van de Mens. https://mensenrechten.nl/en/publicatie/36439

Wet bijzondere maatregelen grootstedelijke problematiek [Accessed on 30.06.2022]. (2005). *Staatsblad*. https://wetten.overheid.nl/jci1.3:c:BWBR0019388&z=2018-06-13&g=2018-06-13

Wursten, J. (2018). Testing for serial correlation in fixed-effects panel models. *Stata Journal*, *18*(1), 76–100(25). https://doi.org/https://doi.org/10.1177%2F1536867X1801800106

A Supplementary Tables

Table A1: Event Study Estimates of Treatment Effect on Exposure

Coefficients	Exposure (1)		Standardised Exposure (2)	
Relative Emigration	0.4870***	(0.1145)	0.1910***	(0.0449)
Event time				
k = -13	0.0635	(0.4200)	0.0249	(0.1647)
k = -12	−0.1891	(0.4540)	−0.0742	(0.1781)
k = -11	−0.4905	(0.4980)	−0.1924	(0.1953)
k = -10	−0.3130	(0.4056)	−0.1228	(0.1591)
k = -9	−0.4433	(0.3283)	−0.1739	(0.1288)
k = -8	−0.4790	(0.3207)	−0.1879	(0.1258)
k = -7	−0.1352	(0.3836)	−0.0530	(0.1505)
k = -6	0.0377	(0.2758)	0.0148	(0.1082)
k = -5	−0.0188	(0.2464)	−0.0074	(0.0966)
k = -4	0.1012	(0.2115)	0.0397	(0.0830)
k = -3	0.0795	(0.2520)	0.0312	(0.0988)
k = -2	0.3051	(0.1663)	0.1197	(0.0652)
k = 0	−0.0951	(0.1408)	−0.0373	(0.0552)
k = 1	−0.2810	(0.1905)	−0.1102	(0.0747)
k = 2	−0.0027	(0.2199)	−0.0011	(0.0863)
k = 3	−0.4216	(0.4162)	−0.1654	(0.1633)
k = 4	−0.0844	(0.4705)	−0.0331	(0.1845)
k = 5	0.0721	(0.5438)	0.0283	(0.2133)
k = 6	−0.1641	(0.4356)	−0.0644	(0.1709)
k = 7	−0.0758	(0.4046)	−0.0297	(0.1587)
Year FE	Yes		Yes	
Neighbourhood FE	Yes		Yes	
Observations	960		960	
R^2	0.864		0.864	
Adjusted R^2	0.849		0.849	
Within R^2	0.176		0.176	

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: Event Study General Coefficient Estimates

Coefficients	General Model				Extended Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
k = 0	−0.0249	−0.0138	−0.0222	−0.0210	−0.0257	−0.0145	−0.0195	−0.0129
	(0.0411)	(0.0380)	(0.0305)	(0.0361)	(0.0407)	(0.0380)	(0.0281)	(0.0347)
k = 1	−0.0050	−0.0014	−0.0164	−0.0218	0.0111	0.0134	0.0011	−0.0031
	(0.0378)	(0.0376)	(0.0359)	(0.0380)	(0.0371)	(0.0381)	(0.0351)	(0.0363)
k = 2	−0.0447	−0.0414	−0.0507	−0.0249	−0.0294	−0.0295	−0.0314	−0.0092
	(0.0453)	(0.0450)	(0.0401)	(0.0405)	(0.0436)	(0.0455)	(0.0381)	(0.0392)
k = 3	−0.0707	−0.0349	−0.0850	−0.0414	0.0023	0.0321	−0.0189	0.0068
	(0.0453)	(0.0403)	(0.0457)	(0.0519)	(0.0420)	(0.0459)	(0.0393)	(0.0433)
k = 4	−0.0238	0.0195	−0.0436	−0.0860	0.0041	0.0397	−0.0166	−0.0375
	(0.0658)	(0.0520)	(0.0575)	(0.0636)	(0.0644)	(0.0582)	(0.0596)	(0.0629)
k = 5	−0.0453	0.0034	−0.0812	−0.0595	0.0035	0.0446	−0.0232	−0.0360
	(0.0528)	(0.0497)	(0.0560)	(0.0639)	(0.0661)	(0.0680)	(0.0731)	(0.0712)
k = 6	−0.0188	0.0268	−0.0565	−0.0550	0.0003	0.0400	−0.0276	−0.0324
	(0.0578)	(0.0524)	(0.0632)	(0.0667)	(0.0742)	(0.0728)	(0.0807)	(0.0770)
k = 7	0.0640	0.1529	−0.0112	−0.0499	0.0314	0.0862	−0.0386	−0.0602
	(0.0748)	(0.0811)	(0.0813)	(0.1038)	(0.0874)	(0.1007)	(0.0871)	(0.1018)
Exposure	No	No	No	No	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear baseline #	No	No	Yes	No	No	No	Yes	No
Full baseline #	No	No	No	Yes	No	No	No	Yes
Observations	960	960	944	944	960	960	944	944
R^2	0.880	0.887	0.915	0.920	0.883	0.889	0.918	0.922
Adjusted R^2	0.869	0.876	0.906	0.907	0.870	0.877	0.908	0.908
Within R^2	0.012	0.062	0.072	0.013	0.031	0.080	0.100	0.039

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Event Study Exposure Coefficients Estimates

Coefficients	Extended Model			
	(1)	(2)	(3)	(4)
Exposure	0.0276	0.0261	0.0069	0.0162
	(0.0402)	(0.0426)	(0.0293)	(0.0312)
Event Time # Exposure				
k = 0	−0.0252	−0.0177	−0.0473*	−0.0637*
	(0.0220)	(0.0222)	(0.0211)	(0.0240)
k = 1	−0.0885***	−0.0758**	−0.1072***	−0.1069***
	(0.0238)	(0.0274)	(0.0274)	(0.0264)
k = 2	−0.0792**	−0.0603	−0.1045***	−0.0845**
	(0.0280)	(0.0314)	(0.0291)	(0.0283)
k = 3	−0.1013**	−0.0895***	−0.1293***	−0.0936**
	(0.0300)	(0.0254)	(0.0256)	(0.0312)
k = 4	−0.0459	−0.0333	−0.0749	−0.0863**
	(0.0509)	(0.0431)	(0.0414)	(0.0317)
k = 5	−0.0583	−0.0471	−0.0958*	−0.0498
	(0.0472)	(0.0364)	(0.0457)	(0.0346)
k = 6	−0.0326	−0.0224	−0.0803	−0.0525
	(0.0406)	(0.0335)	(0.0450)	(0.0398)
k = 7	0.0077	0.0517	−0.0679	−0.0574
	(0.0423)	(0.0571)	(0.0454)	(0.0441)
Controls	No	Yes	Yes	Yes
Linear baseline #	No	No	Yes	No
Full baseline #	No	No	No	Yes
Observations	960	960	944	944
R^2	0.883	0.889	0.918	0.922
Adjusted R^2	0.870	0.877	0.908	0.908
Within R^2	0.031	0.080	0.100	0.039

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Event Study with AR(1) Term Coefficient Estimates

Coefficients	Event Study		With AR(1)	
	General (1)	Exposure Interaction (2)	General (3)	Exposure Interaction (4)
Exposure	0.0069		0.0028	
	(0.0293)		(0.0189)	
Lag 1 Log Crime Rate			0.4678***	
			(0.0446)	
Event time				
k = 0	−0.0195	−0.0473*	−0.0187	−0.0260
	(0.0281)	(0.0211)	(0.0226)	(0.0152)
k = 1	0.0011	−0.1072***	0.0209	−0.0864**
	(0.0351)	(0.0274)	(0.0285)	(0.0250)
k = 2	−0.0314	−0.1045***	−0.0233	−0.0531**
	(0.0381)	(0.0291)	(0.0272)	(0.0197)
k = 3	−0.0189	−0.1283***	−0.0055	−0.0584**
	(0.0393)	(0.0256)	(0.0345)	(0.0183)
k = 4	−0.0166	−0.0749	0.0057	−0.0204
	(0.0596)	(0.0414)	(0.0535)	(0.0415)
k = 5	−0.0232	−0.0958*	0.0021	−0.0668*
	(0.0731)	(0.0457)	(0.0544)	(0.0290)
k = 6	−0.0276	−0.0803	0.0023	−0.0364
	(0.0807)	(0.0450)	(0.0564)	(0.0331)
k = 7	−0.0386	−0.0679	−0.0075	−0.0303
	(0.0871)	(0.0454)	(0.0528)	(0.0279)
Observations	944		885	
R^2	0.918		0.938	
Adjusted R^2	0.908		0.930	
Within R^2	0.100		0.316	

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: Event Study Including Pre-Treatment Event Time Coefficient Estimates

Coefficients	General Coefficients (1)		Exposure Interaction (2)	
Exposure	−0.0289	(0.0212)		
Lag 1 Log Crime Rate	0.4665***	(0.0489)		
Event time				
k = -13	Omitted		0.0329	(0.0341)
k = -12	0.0135	(0.0585)	0.0828	(0.0556)
k = -11	0.0349	(0.0631)	0.0767	(0.0594)
k = -10	−0.0528	(0.0531)	0.0418	(0.0417)
k = -9	−0.0276	(0.0550)	0.0548	(0.0534)
k = -8	−0.0233	(0.0409)	0.0745*	(0.0353)
k = -7	0.0031	(0.0406)	0.0242	(0.0371)
k = -6	−0.0364	(0.0359)	0.0101	(0.0271)
k = -5	0.0048	(0.0331)	0.0510*	(0.0251)
k = -4	−0.0203	(0.0329)	0.0624	(0.0318)
k = -3	0.0068	(0.0369)	0.0196	(0.0256)
k = -2	−0.0260	(0.0361)	0.0177	(0.0282)
k = 0	−0.0319	(0.0318)	0.0021	(0.0188)
k = 1	0.0131	(0.0336)	−0.0559	(0.0286)
k = 2	−0.0375	(0.0356)	−0.0285	(0.0251)
k = 3	−0.0155	(0.0378)	−0.0319	(0.0255)
k = 4	−0.0100	(0.0615)	0.0069	(0.0485)
k = 5	−0.0074	(0.0592)	−0.0385	(0.0338)
k = 6	−0.0138	(0.0623)	−0.0107	(0.0346)
k = 7	−0.0209	(0.0576)	−0.0075	(0.0337)
Observations	885			
R^2	0.939			
Adjusted R^2	0.929			
Within R^2	0.327			

Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6: Event Study Without Binning Event Time Coefficient Estimates

Coefficients	General Coefficients (1)		Exposure Interaction (2)	
Exposure	0.0012	(0.0192)		
Lag 1 Log Crime Rate	0.4664***	(0.0458)		
Event time				
k = 0	−0.0232	(0.0249)	−0.0271	(0.0154)
k = 1	0.0088	(0.0299)	−0.0862**	(0.0259)
k = 2	−0.0356	(0.0287)	−0.0549**	(0.0202)
k = 3	−0.0223	(0.0353)	−0.0600**	(0.0186)
k = 4	−0.0121	(0.0510)	−0.0234	(0.0403)
k = 5	−0.0226	(0.0575)	−0.0699*	(0.0304)
k = 6	−0.0243	(0.0579)	−0.0406	(0.0343)
k = 7	−0.0577	(0.0453)	0.0185	(0.0296)
k = 8	0.0859	(0.0675)	−0.1080**	(0.0361)
k = 9	−0.0741	(0.0566)	0.0082	(0.0220)
k = 10	−0.0330	(0.0933)	−0.0723	(0.0663)
k = 11	−0.0870	(0.0758)	−0.0082	(0.0357)
k = 12	−0.1244	(0.0650)	0.0105	(0.0224)
k = 13	−0.1113	(0.1036)	−0.0486	(0.0627)
k = 14	−0.1450	(0.0756)	−0.0150	(0.0266)
Observations	885			
R^2	0.938			
Adjusted R^2	0.929			
Within R^2	0.322			

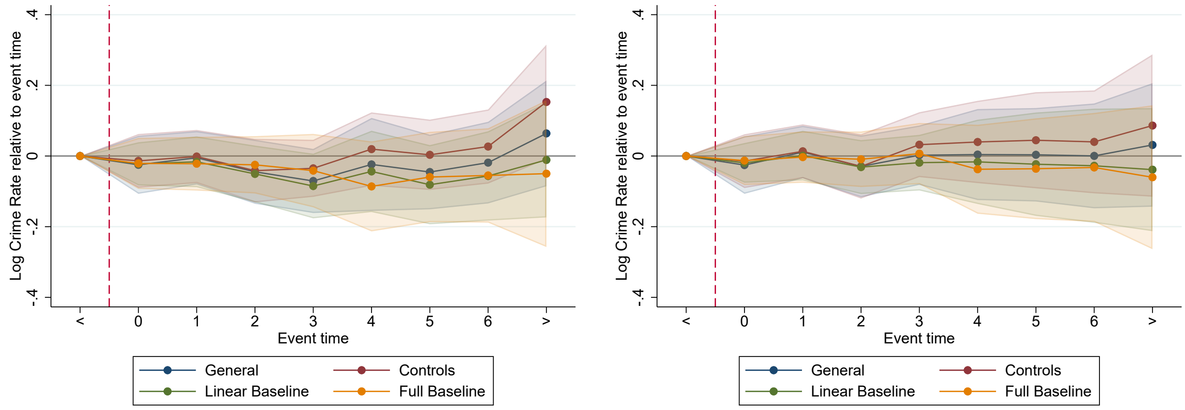
Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.

Notes: Standard errors are reported in parentheses and clustered by neighbourhood. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B Supplementary Figures

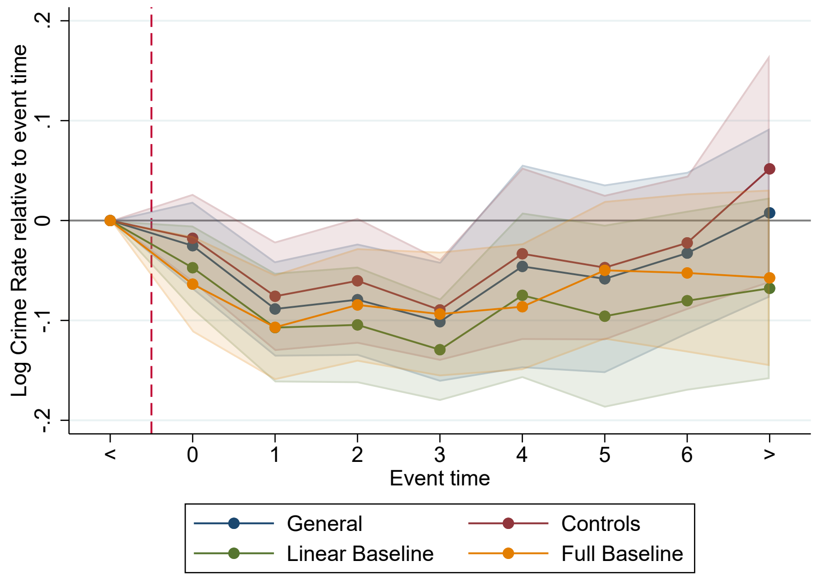
Figure B1: Event Study Estimates of General Treatment Effect

(a) General Model (b) Extended Model



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of ESDiD models with neighbourhood log crime rate as dependent variable. The plotted lines represents the coefficients while the shaded areas encompass a 95% confidence interval. The red dashed line indicates the time of treatment. Panel (a) depicts the coefficients of the general models. Panel (b) depicts the coefficients of the extended models.

Figure B2: Event Study Estimates of Exposure Treatment Effect



Source: Municipality of Rotterdam; OBI; Onderzoek010 database and Wijkprofiel Rotterdam 2014–2022, author's calculations.
Notes: This figure depicts the estimates of ESDiD models with neighbourhood log crime rate as dependent variable. The plotted line represents the coefficients while the shaded areas encompass a 95% confidence interval. The red dashed line indicates the time of treatment.