Slow train coming: The impact of brownfield cleanup on the median income

The case of France

Author: Béka LOMIDZE

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Conducted under the supervision of:

Pr. Fabien CANDAU - University of Pau and Adour Countries

Abstract

Deindustrialization marked the decline of industry dependent regions. Delocalized or bankrupt companies left behind factories and other industrial facilities. A rich literature grew on the economic, social and health hazard they can pose to surrounding communities and how their remediation can improve the well-being of surrounding inhabitants. However the literature mostly focused on housing prices and health benefits their redevelopment can bring and little has been done to estimate the impact of cleaning up a brownfield on the median income due to the potential incentivizing effect for reinvestment it can have. Using data of the ADEME on 420 brownfields, and retrieving data from the Land Information System, the BRGM and from local city halls and prefectures to retrieve the date when the cleanups ended and merging it with neighborhood-level census data. I use the latest Difference-in-Differences approaches to estimate the impact of brownfields cleanup on median income. We find no significant impact of the cleanup on the median income in surrounding neighborhoods. Our findings align with past literature that cleanup in itself has no visible effect on the surrounding community and that cleanup should be included in vast remediation and redevelopment efforts of the overall urban landscape in which they are included.

Contents

1. Introduction

Manufacturing and extractive industries were the economic staple of most European countries during the 20^{th} century. However following the shift toward tertiary activity, national and local companies either changed their activities or relocated to more advantageous regions of the world or if unable to compete went bankrupt. This inevitably diminished the resources of local authorities that were then unable to maintain local amenities as they were during times of economic growth, loss of job prospects went hand in hand with the exodus of the qualified population and led to urban decay. Today, abandoned industrial sites and polluted land are the symbol of a bygone era and represent major economic, health and social hazards. A large amount of research undertaken during the 2000s focused on the impact of brownfield cleanup on local housing prices, and on the social and economic characteristics of the surrounding communities. Especially in the United States where the US Environmental Protection Agency estimates that more than 450 000 brownfield sites are present across the country (United States Environmental Agency, 2011). However in France, mapping brownfield sites is a recent endeavor for a long time their status and how many were spread across the country was a difficult information to find due to the absence of a national database. In the 1990s, after two decades of deindustrialization, the newly created BASIAS/BASOL national database aimed to register every polluted and abandoned sites throughout France, however, some brownfield sites are badly localized or simply not registered. This is not an issue only in France, the number of brownfield sites is underestimated in most western countries despite the fact that they present a non-negligible potential for reuse and can help to fight unnecessary urban sprawl in unaffected natural areas and can be a major factor in future sustainable land management (Hou and Al-Tabbaa, 2014). However the main issues that lead developers to favor unused land instead of abandoned brownfields is the cost and liability of a cleanup, taking charge of a cleanup operation opens up a risk for developers if future unreported contamination is found on the premises of the reconverted brownfield (Green, 2018). The ALUR law aimed to clarify the legislation surrounding the liability and responsibility of the cleanup by giving future owners of a brownfield the ability to shift the responsibility to a third party for the cleanup (Lafeuille and Steichen, 2015). Local legislation has an important role to incentivize cleanups either through a more flexible and accommodating local legislation or by taking charge of the cleanup in areas where the fixed cost of remediation is so high that the private owners of the land fail to or cannot clean up by themselves. There is little quantitative literature on the potential impact of brownfield cleanup on the median income due to hightened economic prospect for future developers. We aim to fill that gap, by trying to estimate what is the impact of brownfields cleanup on the median income. We use geolocated data given by the Environment and Energy Management Agency(ADEME) on 420 facilities filed under the ICPE[1], to find the year in which the cleanup was completed we cross-referenced available information with reports by the Bureau of Geological and Mining Research (BRGM), and CEREMA, detailed information in the Land Information System, the SCE, the Public Land Institution of the Western Rhône-Alpes and through prefectural decrees and information given by urban planning agencies and city halls. We then matched this dataset with census data at the neighborhood level published by the National Institute of Statistics and Economic Studies(INSEE) from 2006 to 2018. In Section 2 we present a literature review on brownfields redevelopment to build a sound knowledge from which we are going to build on, in Section 3 I present our dataset and empirical strategy, we first estimated a static two-way fixed effects difference-in-differences with staggered treatment adoption, we then clearly visualize that the TWFE estimator is negatively biased when treatment is heterogeneous with a bacon decomposition, we then turn to a Difference-in-differences with multiple time periods strategy developed by Callaway and Sant'Anna (2021), we present our results in Section 4, we then do multiple alternative estimations as robustness checks in Section 5 to confirm our results using the latest developments in the difference-in-differences literature and a TWFE estimation using grided census data for the years 2015 and 2017. We find that brownfield cleanup in itself is not enough to have a significant effect on the median income or the poverty rate, in line with the literature, we assess that cleanup should be included in larger neighborhood-level economic development efforts. We discuss our limits in the conclusion of our paper.

[1]Facilities Filed for Environmental Protection

2. Literature Review

2.1. Industrial Revolution and Industrial collapse

European countries developed through strong and sustained industrial growth from the 19th century up to the second half of the 20th century. With this sustained development, a substantial amount of physical capital grew to shape and reshape our cities. Communities living in and around these cities relied heavily on jobs provided by local factories and in general by any private institutions of the industrial sector. This reliance specialized and shaped the local population's long-term professional perspective. However Western European countries after benefiting from a strong growth stimulated by their industrial base, shifted from an industrial economy to a service-based economy. With globalization, companies preferred to look for a more advantageous region to settle for their future growth (Rodrik, 2016).

This deindustrialization led to an important shortage of decent-paying low-skilled jobs especially in communities heavily reliant on their industrial capacity. Disappearing job prospects in the secondary sector was accompanied by a rural exodus and highly skilled individuals migrated toward regions with more dynamism leaving behind mostly lower-skilled individuals in regions with less professional prospects (Howland, 2007). Shrinking industrial activity together with a smaller tax-base and diminishing resources for local institutions led to urban decay and a dwindling of the local highly skilled population generated Urban decay and a vicious cycle of economic collapse. The deterioration of the urban landscape, led to a broken-window phenomenon where the current urban deterioration stimulated future deterioration (Green, 2018). Non-existent economic prospects and the overall dire social and economic state of these regions led to a substantial increase in joblessness and the poverty rate together with a non-negligible increase in criminal activity (Lee and Mohai, 2012) due to endemic poverty.

Private industrial companies that used to manage those facilities either went bankrupt were bought by competing firms or moved to a region with a more advantageous economic environment for the development of their activities. In doing so, they left behind facilities varying in size. To this day these brownfields remain either abandoned and for a few went through remediation effort. However most of the time local authorities in the face of limited resources and pressing incentives to produce quick results focused on brownfields who didn't need major remediation efforts in the first place, rather put their efforts on the "low hanging fruit" (Howland, 2007). However, focusing on the easiest brownfield to clean up in the neighborhood yielding the highest economic returns led to local authorities mostly focusing on brownfields located in more affluent neighborhoods with less social and economic challenges where private developers would be most interested to take a risk with contaminated land and with better economic prospects (Lee and Mohai, 2012).

2.2. Environmental justice and brownfields remediation

Local authorities mostly focus on brownfields located in urban areas where economic returns are potentially the highest for quick results. However most of the brownfields are located in areas with a history of poverty, unemployment and economic distress, in the same way private companies prefer to focus on areas with a dynamic economic and social background to settle their activity. The presence of abandoned and polluted urban facilities drive down housing prices in the surrounding areas (Greenstone and Gallagher, 2008). And more affluent households price the cost of living near polluted areas at a higher level than poorer households, the latter tends to tolerate higher levels of pollution in exchange for more affordable housing (Banzhaf et al., 2019). Therefore the more toxic the pollution the less well off the population will be, therefore the brownfields that are the most in need of remediation are the ones that are located in neighborhoods who in themselves have few economic potential and with a smaller probability of yielding substantial economic benefits for the local institutions and private developers. Due to limited funding and resources, authorities of less well-off regions will tend to take less risk and focus on the "low-hanging fruit".

Not only there is an objective to yield result, but brownfields cleanup in itself doesn't yield significant result it has to be followed with important investment in the surrounding neighborhoods to yield important positive social and economic results (Howland, 2007). Polluted facilities can also have a negative effect on the health of the surrounding socially vulnerable population. Cumulative barriers to remediation for private developers and public authorities generate environmental injustices, where the most vulnerable population who are the most in need for urban remediation are the ones that benefit the less from it, whereas the communities the less in need for urban renewal receive the most benefits from those programs.In short communities living in areas with an advanced state of urban decay tend to gobble a considerable amount of funds with no guarantee that local or national entrepreneurs will invest in these localities due to uncertain economic perspective and unclear liability when it comes to land contamination (Green, 2018) together with the political and economic considerations of municipal administrations (Walzer and Hamm, 2004) crippling any attempt to clean up those areas. McCarthy (2009) in a case study of Milwaukee and Wisconsin found that census tracts with above average percentage of brownfields per square mile, have an above-average percentage of African-Americans and Hispanics, yet receive below average municipal funding for brownfields redevelopments.

2.3. Investments, liability and the legislative challenges of brownfields cleanup

Given the unwillingness of private investors and local authorities in brownfields located in disenfranchised neighborhood. The legislator faces multiple challenges to incentivize current and future investment in brownfields. Property developers are disincentivized due to lack of clarity about their liability when it comes to land pollution. They generally prefer to build on unused land that does not pose such a risk and remain cheaper because there is no need for cleanup and remediation, contributing to the urban sprawl of cities. Land pollution can persist for a long time and can contaminate newly built structures in the area. Private stakeholders are wary of brownfield given the difficulties to detect hazardous pollution before the project is implemented. Moreover pollution can have health consequences on the individuals working in the area and if a source of pollution is discovered late the private company owning the premises might be liable for the cleanup and for the health issues that appeared as a consequence of brownfield contamination. On the other hand, public authorities are hindered by a lack of resources and by deteriorating conditions, the longer contaminated land is left abandoned the worst it becomes, contaminated lands tends to seep into local water systems and built structures creating major sanitary hazards (Greenberg et al., 1998; Klemick et al., 2020)[1].

Therefore national and local legislatures need to find new tools and channels to incentivize investment by private structure in local brownfields. Brownfield remediation is a long-winded endeavor that necessitates massive resources and coordination between different branches, and costs sustained on the long-term, this leads to price discrimination where only important structures can afford to cleanup or convert brownfields into a useful asset (Merle and Perrin, 2018).

The law ALUR aims to facilitate investment and redevelopment of brownfields and abandoned industrial facilities by creating information channels about the type of pollution in each brownfields and clearing up liability issues when it comes to contaminated land. One of the major measures implemented by this law is the ability to shift the responsibility to a third party for the cleanup, leading to the creation of a sector specialized in brownfields requalification (Lafeuille and Steichen, 2015). The owner will act as a caution for the third party. This law also gives the capacity to local prefectures to give responsibility to public structures for cleanups if the owners or the third party fail to clean up the land themselves, this measure put the expenses and liability for the cleanup on public organizations decreasing the costs for private companies. The main objective of the legislator is to lift the main hurdles that prevent the reuse of land occupied by brownfields. French authorities try in this way to join their neighboring European countries which already have advanced legislation for the management of polluted land. In doing so, local authorities open up new possibilities by creating "loopholes" within the legislature for private stakeholders to benefit from, this aims to create a competitive environment between

[1] Lead poisoning is the cause of major social challenges and health scares in the US

private stakeholders and to generate the backdrop for cooperation between public structures and property developers. This new law extends the scope of already existing laws to potentially polluted land, the State as an obligation to disclose information about land pollution in a Land Information System integrated in the Local Urban Plan(PLU)[2]. This law is reinforced by multiple European directives that aim to curb land pollution and prevent unnecessary land occupation by increasing the land supply through brownfields remediation and "land recycling" (Garnier, 2018; Valdiguié and Schmit, 2018). The ALUR law as been completed with the adoption of the law ELAN by the senate in 2018, which also aims to further facilitate brownfield reuse.

2.4. The potential of brownfields

In current circumstances where an important emphasis is put on sustainability, brownfields have become an important part of public policies focusing on the restructuring of the urban landscape in a way that is more respectful of the climate and of the environment. Therefore pushing against unnecessary urban sprawl is now an important part of a wider sustainable strategy. Brownfields remediation can lead to an increase in land supply, by cleaning up the area it gives the abandoned site a new social or economic use. Given the right incentives private developers might turn toward using remediated land instead of polluting unaffected soil (Banzhaf et al., 2018, 2019; Green, 2018).

[2]Plan Local d'Urbanisme

3. Empirical Strategy

3.1. Data

For our estimation we want to measure the impact of brownfields cleanup on the median income, given that brownfields remediation might lead to economic stimulus in the future because it can be given an economically viable use, instead of further falling in disrepair. The Environment and Energy Management Agency(ADEME) under the authority of multiple ministries manages environmental policies implemented by the state and also help enforce those policies from a wide range of issues, from energy management to land pollution. The ADEME deals with brownfields included in the ICPE[1], if owners or managers of such facilities fail to implement or undertake depolluting activities a prefectural decree can transfer responsibility of the depollution to the ADEME that then manages and implements this strategy and bears the costs. The ADEME updates a database with the geolocation of those brownfields, with 419 brownfields of which 238 have been secured and cleaned. However this database doesn't provide the year in which cleanup has been finished, we therefore cross available information about the brownfields included in our dataset with informations in the Land Information System, reports by the French Geological Survey, the West Rhône-Alpes Public Land Establishment, the SCE and from prefectural decrees and directly from local city halls to find the year in which the remediation ended. We managed to find the year cleanup efforts ended for most facilities.

[1]Facilities Filed for Environmental Protection

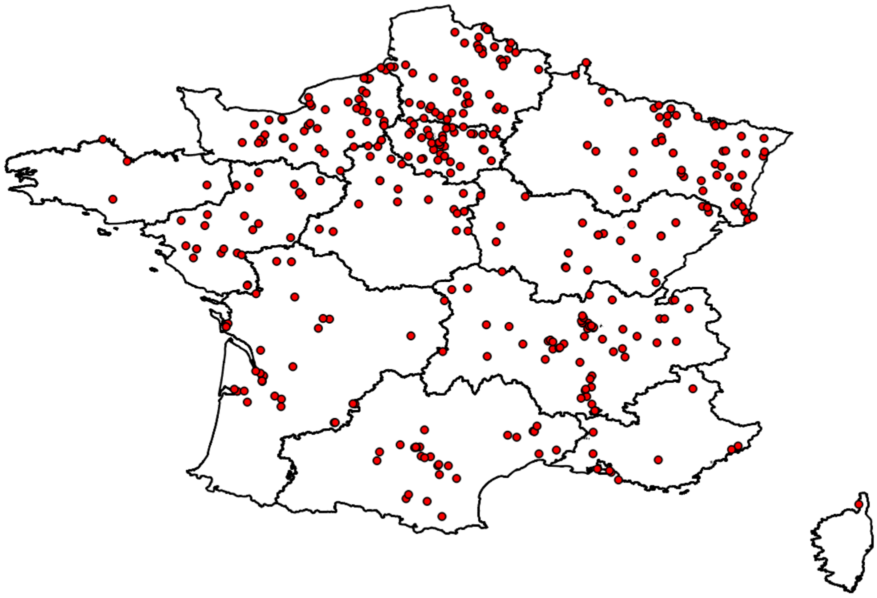

Figure 3.1: Localisation of every brownfields contained in the dataset

Using this geolocated data we then merge it with census data at the neighborhood level(IRIS[2]) given by the French National Institute of Economics & Statistics from 2006 to 2018. The data gives information about the name of the neighborhood, the name of the city in which this neighborhood is included and census data for a given year, there are different types of IRIS[3] that cover the entirety of metropolitan France, each category represents the function of the census tract, there are three broad categories of census tracts:

-Habitat IRIS, their population is between 1800 and 5000 inhabitants. They are homogeneous in their type of habitat and their limits are determined by major roads and geographical limits.

-Activity IRIS, they regroup more than 1000 employees and they usually have twice as much salaried jobs than residents.

-Various IRIS, include area without inhabitants that represent, ports, parks and natural spaces within a city.

[2]"Units Grouped for Statistical Information" - Ilots Regroupés pour l'Information Statistique

[3]For simplicity, IRIS and census tracts will be used interchangeably

We merged this census data with census data on revenues and poverty on selected neighborhoods[4], this dataset given by the INSEE provides information on the median income, and the income composition of certain neighborhoods. After spatially identifying the centroids of each neighborhood, we create a 3 kilometers buffer around each brownfields. We assume that the selected neighborhoods might benefit from a cleanup of their associated brownfield. To balance out the data and prevent bias from repeated treatment we remove the neighborhoods treated multiple time by different brownfields.

Most of the available brownfields in the ADEME dataset benefited from cleanups of different intensities, a small part of those brownfields only benefited from environmental monitoring or investigations. If we look at the brownfields that were treated, we can see that out of the 238 treated, 206 benefited from waste management, disposal and depollution and sometimes with extra treatments such as environmental monitoring. 3 brownfields benefited from an aid agreement by the ADEME, 27 from only environmental investigations and/or environmental monitoring, and 2 from other types of help from the ADEME.

Table 3.1: Typology of interventions

Types of intervention	Frequence	Percent
Aid agreement	3	1,26
Waste management, disposal and depollution and others	206	86,55
Environmental investigations and environmental monitoring	27	11,34
Other	2	0,84
Total	238	100

From this dataset we then build a control group made of the brownfields that between 2006 and 2018, haven't been treated and as the treated group the brownfields whose cleanup ended within this time frame. We then create buffer zones around each brownfield with a radius of 3

[4] the FILOSOFI datasets

kilometers[5] to capture the census tracts within those buffers to observe their evolution and take into account any effects of the cleanups on the median income, after cleaning our dataset we end up with the following treated and control group, in table 3.2. However brownfields might be located in area with very low-density meaning that the census tracts might cover more extensive spaces whereas census tracts located in urban areas with a higher density of population will be smaller. Therefore brownfields in rural area will capture sometime only a handful of census tracts where a urban brownfield will capture a dozen more census tracts (figure A.1, A.3). If we look at the descriptive statistics of selected variables of the census tracts that fall in the control group with the census tracts that fall in the treatment group, we can see that before the treatment those variables are comparable, they diverge slightly from one another but we can consider that both groups are comparable before treatment (See table A.1). However, one of the limits of the data is that the average surface in square meter of a treated brownfield compared to a untreated brownfield is smaller.

Table 3.2: Descriptive statistics of our final dataset

Groups	Sites	IRIS	Average number of IRIS per sites
Treatment	141	14,352	101,79
Control	148	12,363	83,53
Total	289	26,715	

3.2. The Empirical strategy

In the last 5 years the difference-in-differences literature went through some important upheaval starting with Borusyak and Jaravel (2017). This new literature found that the traditional two way fixed effect estimator can be biased in certain circumstances leading to some skewed estimates and coefficients (Goodman-Bacon, 2021). This led to a paradigm shift in the difference-in-differences literature that gave way to a variety of methods that control for the usual bias coming from the TWFE estimator and that are more adapted for heterogeneous treat-

[5]The radius was chosen based on practicality given the level of aggregation of the data

ments effects and multiple time periods (Roth et al., 2022; De Chaisemartin and D'Haultfoeuille, 2022). To test our model we will first start from a simple difference-in-differences framework with TWFE. However our estimator was expectedly biased due to our staggered treatment timing design. The usual implementation of the difference-in-differences strategy is not adapted to a dataset with multiple time periods and with variations in the depth of pre and post trends. The TWFE estimator is a weighted average of every possible comparison. However it makes forbidden comparison with always treated groups and because it's a weighted average it's sensitive to the variance of the observations and the size of the samples and might negatively weight our coefficient of interest (Goodman-Bacon, 2021). Taking this into account, we then implement a Difference-in-differences strategy as developed by Callaway and Sant'Anna (2021) with a doubly robust estimator taken from (Sant'Anna and Zhao, 2020). As a robustness check we will use grided data from the INSEE from 2015 and 2017 to deal with spatial aggregation issues, however, by doing so we trade temporal depth for more spatial details. We further cement our results by estimating alternative difference-in-differences estimations with the latest methods developed in the literature.

4. Results

4.1. First two-way fixed effect estimation and Bacon Decomposition

As a first benchmark analysis we estimate a simple static two way fixed effect difference-in-differences with staggered treatment adoption to withdraw the impact of the treatment on the median income. The equation can be written down as such:

$$y_{it} = \psi_i + \tau_t + T_{it}\beta + X_{it}\delta + \epsilon \tag{4.1}$$

Where y_{it} is the dependent variable and equal to the logarithmic value of the median income for i through time t, where ψ_i are census tracts fixed effects, τ_t time fixed effects, $T_{it}\beta$ is the binary treatment variable for each census tract that falls within the buffer of a treated[1] brownfield, X a vector of controls for the gender composition of the neighborhood, the socio-professional status, the share of immigrants, the share of individuals with only a high school degree, the unemployment and inactivity rate and the type of residential housing in the area to account for the type of housing surrounding the brownfield, and ϵ represents the error term.

Our first estimation finds that brownfield cleanup is significant at the 10% threshold and leads to a decrease in median income of 0.46% (figure A.5) we also observe that some of our controls are significant and explain some of the variance, the higher the share of immigrants the share of only high-school and CAP-BEP graduates the smaller the median income, the higher the share of artisans, business executives and CEOs the higher the median income, our model explains 66% of the variance. However we know that a simple TWFE estimator is not adapted to our case where treatment is staggered and with heterogeneous treatment effect. This estimator is

[1]The treatment for all of our estimations is the clean-up

more adapted to a situation with a homogeneous treatment with two time periods, the simple 2x2 design. Even by estimating this difference using stacked cross-sections it yields significant and negatively biased results. We also estimated this equation using unbalanced panel data from 2001 to 2019, to allow for more temporal depth but relaxing the need for balanced data we still find negatively biased results. For these last two estimations, we found a significant effect of treatment of -0.4% at the 1% threshold.

To visualize the bias with our balanced panel dataset, we use a bacon decomposition that plots and yield the weights and coefficients of every Difference-in-differences comparison. The bacon decomposition is useful here because compared to the homogeneous treatment case, the heterogeneous treatment case yields a situation where every observation spends a different amount of time in treatment and without. In our case if a census tract is treated in 2008, it will spend more than 80% of the time in treatment. However a census tract treated in 2016 will spend 20% of its time in treatment and given that the two-way fixed effect estimator in a differential timing framework is a weighted average of every possible comparison between early, late treated, never treated and always treated. The sample size, and treatment variance is important because it determines the weight it will have in the differential timing design and can lead to important biases, the TWFE might also bias the result by making some forbidden comparison between treated and always treated.

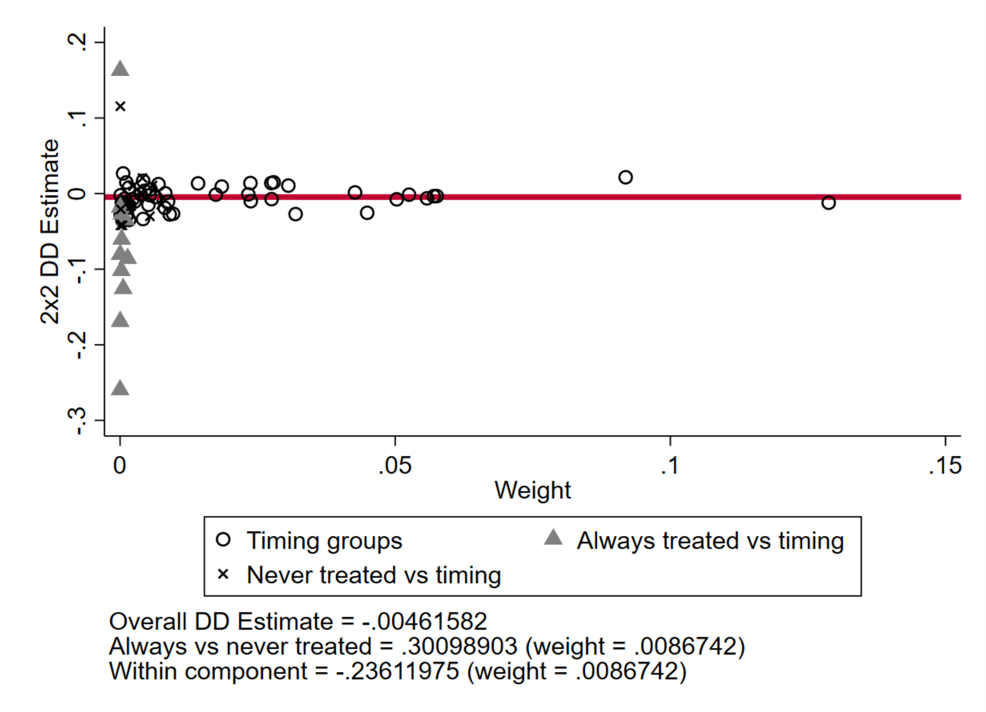

Figure 4.1: Bacon decomposition of the TWFE estimator

Our Bacon decomposition lets us see that our TWFE is negatively biased due to forbidden comparison with the always treated even though their weights are individually small they are numerous and together pull the coefficient down. To control for this bias we decide to adopt a difference-in-differences strategy with multiple time periods as described by Callaway and Sant'Anna (2021) with a doubly robust estimator by Sant'Anna and Zhao (2020).

Table 4.1: Treatment Coefficient

log of the median income	Coef.	Std.Err.	z	P>z	[95% Conf.	Interval]
Treatment	-.0046158	.0025202	-1.83	0.067	-.0095553	.0003237

Table 4.2: Bacon decomposition

	Beta	TotalWeight
Treatment group	-.0022546033	.9561424902
Always treated group v treatment group	-.0719831732	.003840209
Never treated group v treatment group	-.0044475072	.0313400197
Always treated group v never treated group	.3009890318	3.07896e-06
Within	-.2361197472	.0086742022

Our results are in line with the recent development in the literature for difference-in-differences with staggered treatment. The TWFE estimand in a multiple time periods and staggered treatment doesn't have an intuitive causal interpretation as in the case with homogeneous treatment (Roth et al., 2022). Mostly due to forbidden comparison with always treated observations and the bigger weight given to observations spending more time in treatment.

4.2. Difference-in-differences with multiple time periods

Given the inherent bias of the TWFE estimator, the Callaway and Sant'Anna (2021) method with Sant'Anna and Zhao (2020) estimator helps us generate more reliable results due to the way the results are retrieved. This method estimates groups time average treatment effect(ATT) for each treatment group, it aggregates the results per year of treatment and then aggregate every treatment period into one average $ATT(g,t)$. Given that there are multiple time periods, this estimates multiple ATT at once. However one of the challenges is that the further we go in time, the less untreated group there is to be compared to. This method allows us to maintain an never treated group as comparison. Using the potential outcomes language the group-time average treatment effect, we are looking for can be written as such:

$$ATT(g,t) = E[Y_{it}(g) - Y_{it}(\infty)|G_{it} = g] \tag{4.2}$$

Which gives the average treatment effect on the treated at time t for the first group treated in time g, where $Y_{it}(g)$ is the treatment group and $Y_{it}(\infty)$ the control group made up of yet to be treated brownfields. The Callaway and Sant'Anna (2021) approach, as described in Roth et al. (2022) identifies the $ATT(g,t)$ by comparing the expected outcome for cohort g between periods $g-1$ and t to the never treated group at period t. This approach uses either never treated units as a control group or yet-to-be treated group. In a situation where there is a limited number of cohorts, withdrawing the $\hat{ATT}(g,t)$ for all (g,t) is reasonable. This approach is more adapted than the classic TWFE approach as it provides more reliable estimands when there is an heterogeneity of treatment effects, and is transparent when it comes to which group is used as control. The doubly robust estimator integrates a generalized propensity score as a weight to better compare both groups. After controlling for confounders, we find that the treatment increases the median income by 0.38%. However the ATT is not significant(Table A.4, A.5, A.6)[2].

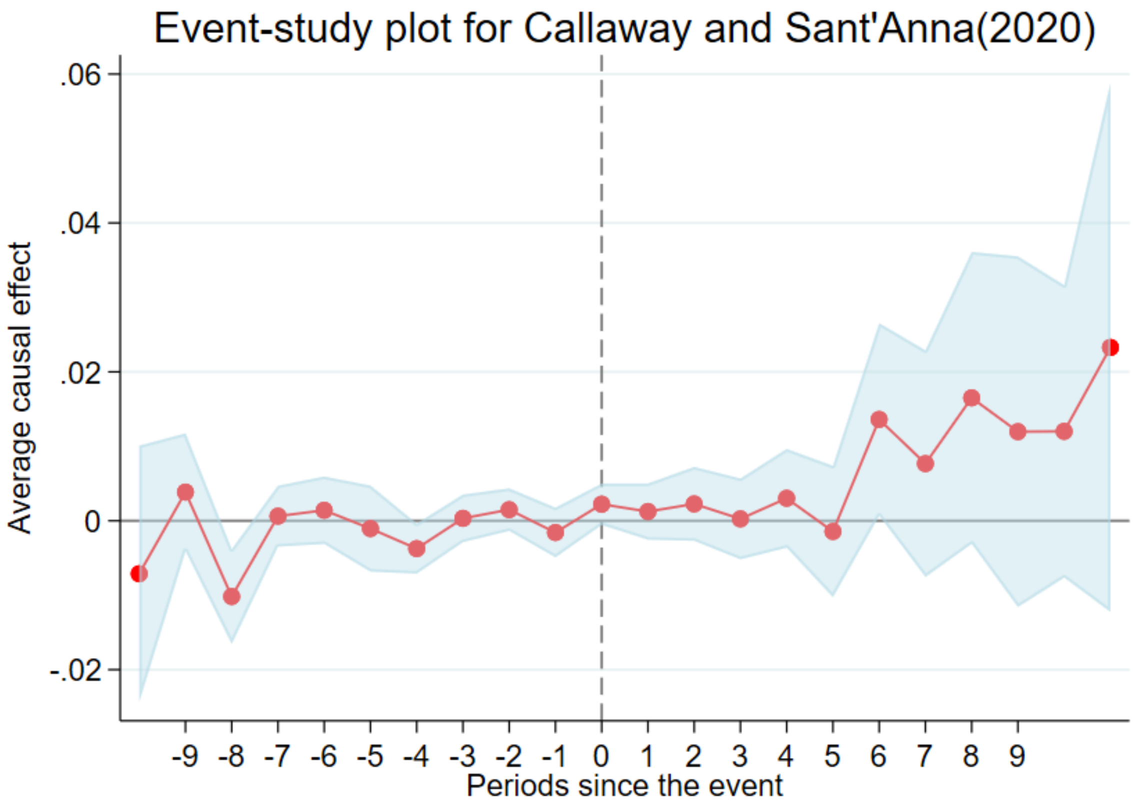

Figure 4.2: Event-Study Plot of Callaway and Sant'Anna (2021) Estimation

[2]For the ATT per treated group see figures A.20, A.21, A.22, A.23, A.24, A.25, A.26, A.27, A.28, A.29

We can see that the average post-trend drifts positively, however, because the ATT is nonsignificant this cannot be interpreted as a causal relationship of a brownfield cleanup. Nonetheless this gives early estimates of what can possibly be with further control. We make the hypothesis that our data respects the parallel trends assumption due to the fact that the pre-trend coefficients remains close to zero and drifts only after the treatment. We nonetheless realized multiple robustness checks using the recent advance in the difference-in-differences literature and visualized the parallel trends for our dependant variable.

5. Robustness Checks

To test for the robustness of our results, we first realize a simple TWFE difference in differences with grided data, we trade temporal depth for spatial details, grided data published by the INSEE is vastly more deagregated than neighborhood-level data[1]. However, to estimate a difference-in-differences strategy with TWFE our data needs to fulfill some assumptions.

Assumption 1. ***Parallel trends assumption:*** *the pre-treatment outcomes of the treated group needs to be the same as the untreated group.*

$$y_{it}(\infty) = \psi_i + \tau_t + \epsilon_{it} \tag{5.1}$$

In potential outcomes term, strong unconditional parallel trends in staggered treatment adoption can be written as such:

$$E[y_t(\infty) - y_{t-1}(\infty)|G = g] = E[y_t(\infty) - y_{t-1}(\infty)|G = g'] \tag{5.2}$$

Assumption 2. ***No anticipation effect:*** *the announcement of the treatment doesn't change the outcome prior to the treatment itself.*

$$y_{it} = y_{it}(\infty) \tag{5.3}$$

Assumption 3. ***Restricted causal effects:*** *The treatment effects are homogeneous.*

We will first estimate a TWFE with grided data for the brownfields treated in 2016. Then a simple Event-Study with leads and lags with a two-way fixed effects estimator, which will also help us test our parallel trends assumption we will then estimate a Difference-in-differences with imputation approach developed by Borusyak et al. (2021) which will also help us test our pre-trends. Afterwards we will estimate a difference-in-differences method with heterogeneous

[1] For comparison see tables A.1 and A.2 for a rural area and tables A.3 and A.4 for an urban area

treatment effect as developed by De Chaisemartin and d'Haultfoeuille (2020) to further cement our findings. We assume that there is no anticipation effect because private developers cannot invest in brownfield while the depollution efforts are ongoing, because the ADEME shuts down all activity in the area within the brownfield's perimeter. Therefore there can't be any anticipation effect. Potential investors also wait for the results of the cleanup before taking any risks.

5.1. Two-way fixed effect with grided data

Our new dataset taken from the grided data distributed by the INSEE gives us more accurate spatial details. However, the data is available only in 2015 and 2017, which means that we do not have as much temporal depth as we had with census tract data. Nonetheless this dataset can still give us some insight[2]. We focus on brownfields cleaned up in 2016 for which we can have at least 2015 as a pre-trend and 2017 as a post-trend, even though this is less robust than what we could expect. However we do not have the median income in this dataset we will therefore rely on the poverty rate to determine the impact of the treatment on the 2016 treated group. In this case the treatment is homogeneous with only two groups, treated and untreated and two time periods before and after treatment.

Table 5.1: Descriptives statistics of our grided data

Groups	Sites	Squares	Average number of 200m squares per sites
Control	160	64313	401,96
Treatment	15	2961	197,4
Total	175	67274	

Our estimation is not robust, the r-squared is very low, our model doesn't explain the variation of the poverty rate. The lack of robustness of this dataset is evident, more temporal depth would be needed to reach a causal conclusion, the treatment is artificially considered significant

[2] To visualize and compare the spatial aggregation see figure A.2 compared to figure A.1 and figure A.4 compared to figure A.3

due to the fact that only two years[3] are considered in this dataset (figure A.6).

5.2. Comparison with other estimators

To check the robustness of our method, we generate leads and lags to compute an event-study regression with a two-way fixed effect estimator with differential timing. We then estimate a difference-in-differences with an imputation approach developed by Borusyak et al. (2021). We also compute a Difference-in-differences estimation à la De Chaisemartin and d'Haultfoeuille (2020) which is an alternative method of estimation. We then compare our estimations to see if they reach the same conclusions.

5.2.1. Canonical event-study design with TWFE

We first estimate a canonical event study with TWFE developed by Guimaraes and Portugal (2010); Gaure (2010), whose equation can be written as such:

$$y_{it} = \psi_i + \tau_t + \sum_{\lambda=-2}^{-q} \gamma_\lambda D_{i\lambda} + \sum_{\lambda=0}^{m} \delta_\lambda D_{i\lambda} + \epsilon_{it} \tag{5.4}$$

Where y_{it} is the logarithmic value of the median income, ψ_i the census tracts fixed effects, τ_t a vector of time-year dummies, the γ_λ are pretreatment "leads" and δ_λ post-treatment "lags" coefficients that we will estimate using a TWFE estimator. We can see that our linear equation still highly resembles the classic difference-in-differences setup. Pretreatment lags are often seen as a test for pre-trends. After estimating our event study, we find that the treatment is nonsignificant and that our pre-trends are respected, nonetheless this represents a "low" power test for pre-trends. A few of our leads are significant and none of our lags, nonetheless it seems that our treatment as no significant effect on median income(Tables A.7, A.8) and the full sets of leads and lags seems to create a multicollinearity problem.

[3] One year pretreatment and one-year post-treatment

5.2.2. Difference-in-Differences with the imputation approach and parallel trend assumption

Goodman-Bacon (2021),De Chaisemartin and d'Haultfoeuille (2020), Borusyak and Jaravel (2017) reached the same conclusions that the TWFE estimator can be biased. In Borusyak et al. (2021), shows that the usual TWFE estimation assumes parallel trends(Assumption 1), no anticipation(Assumption 2) and homogeneous treatment effects(Assumption 3). However with staggered treatment timing using a TWFE estimator can lead to an under-identification of the leads in event-study, negative weighting of the coefficients as we explained with our bacon decomposition, and spurious identification of long-run causal models. In the case of an Event-study estimation with TWFE. Staggered treatment might lead to a multicollinearity problem. A complete set of treatment leads and lags, which is equivalent to the fixed effects of relative time, will be collinear with the unit and period fixed effects. The imputation method by Borusyak et al. (2021) is therefore intuitive, it drops the assumption 3 that there is restricted causal effects, tweaks assumption 1, and adds a fourth assumption:

Assumption 4. ***Homoskedastic residuals:*** *The magnitude and direction of the treatment effect is the same for all observations, regardless of any other observations characteristics.*

However this assumption, only provides a benchmark as they show in their article that even when treatment effects residuals are heterogeneous it still provides interpretable and coherent estimates. Their approach is intuitive, first we take the static TWFE equation 4.1 and remove the treatment (and also the covariates for simplicity):

$$y_{it} = \psi_i + \tau_t + \epsilon_{it} \tag{5.5}$$

It means that the outcomes for the treated group no longer depends on the treatment and therefore follows the same trend as the untreated group. They estimate this equation to then generate a counterfactual for every observation using individual fixed effects ψ_i and time-year dummies τ_t from the untreated groups and before treatment for the treatment group. They will then aggregate the fitted values to:

$$\hat{y}^0_{it} = \hat{\psi}_i - \hat{\tau}_t \tag{5.6}$$

Where $\hat{\psi}_i$ is the fitted value of ψ_i and $\hat{\tau}_t$ the fitted value of τ_t. Therefore the initial equation can then be estimated as such:

$$y_{it} - \hat{\psi}_i - \hat{\tau}_t = \hat{\beta}_{it} \tag{5.7}$$

Where β is the treatment coefficient. They then estimate the treatment effect of interest as a weighted sum.

$$\hat{\beta}_w = \sum_{it} w_{it}\hat{\beta}_{it} \tag{5.8}$$

With this estimation we find no significant effect of the treatment on the median income. Both our pre-trends and post-trends are non-significant. This remains in line with our initial estimations that brownfield cleanup is not enough. And we can also see that our coefficients have a small value and are non-significant in pre and post-treatment periods. This imputation method is a different approach than the usual difference-in-differences approach, and relies on a synthetic control group like Gardner (2021).

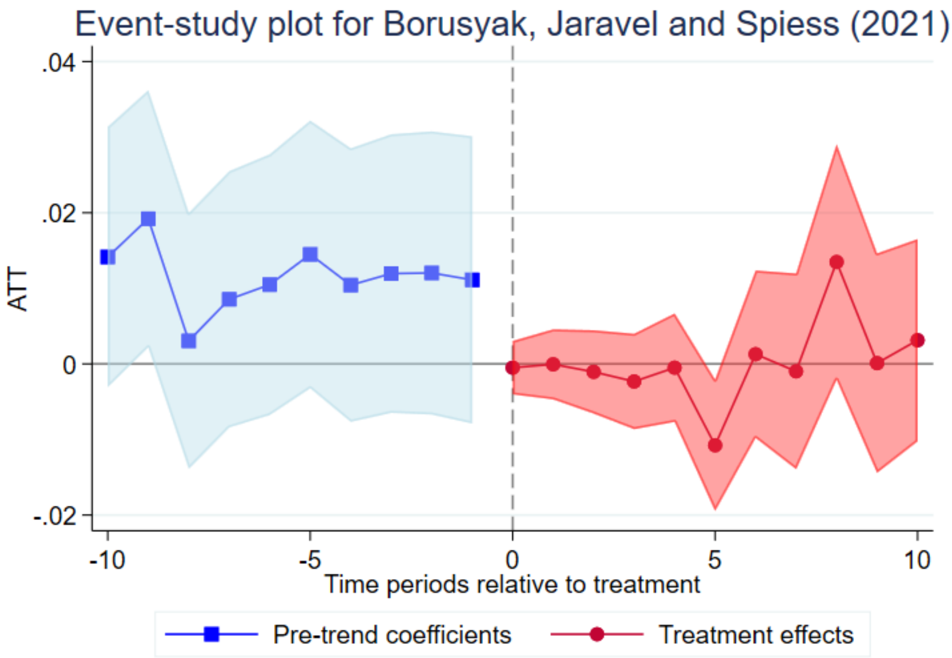

Figure 5.1: Event-Study Plot for the Imputation Method by Borusyak et al. (2021)

Our pretrend coefficients are not significant in our imputation results, it shows that parallel trends are respected. However, this is not a substitute for pre-trend testing and is a low-power determination of the parallel trends. We cannot accept the null hypothesis that all pre-treatment is equal to zero (tables A.2 and A.9) nonetheless our covariates control for most of the pre-treatment differences between control and treatment groups. We can visualize the parallel trend and see that the difference between the control group and the treated group before treatment is minimal and their characteristics are similar(table A.1) and when we visualize the trends we can see that every treated group seems to follow and respect assumption 1 (figures A.9, A.10, A.11, A.13, A.14, A.15, A.16, A.17, A.18, A.19).A.1.

5.2.3. Difference-in-differences with heterogeneous treatment effects

To confirm our results we estimate a difference-in-differences with heterogeneous treatment effect following the methodology developed by De Chaisemartin and d'Haultfoeuille (2020) to solve the usual caveats TWFE has when it comes to staggered treatment timing. De Chaisemartin and d'Haultfoeuille (2020) like Borusyak et al. (2021) impose additional assumptions. They use unit specific causal effects as building blocks as we can see in equation 5.9:

$$UTE_{it}^{g} = y_{it}(g) - y_{i}t(\infty) \tag{5.9}$$

They consider a very broad setup in which treatment can switch on and off. However their strategy is still compatible with irreversible staggered treatment adoption, for their decomposition they suppose SUTVA[4] with no anticipation and a strong unconditional parallel trend assumption(see assumption 1), they find like Goodman-Bacon (2021) and Borusyak and Jaravel (2017) that the weights for TWFE are non-convex and can be negative. Therefore their procedure aims to bypass most of the problems posed by TWFE and they mainly focus on instantaneous treatment effect measures. We find a non-significant result of our treatment on median income with very small variation in treatment coefficients(Table A.10).

[4]Stable Units Treatment Variable Assumptions implies that the potential outcome for a unit i is unrelated to the treatment status of another unit i, it imposes a no spillover condition (Angrist et al., 1996)

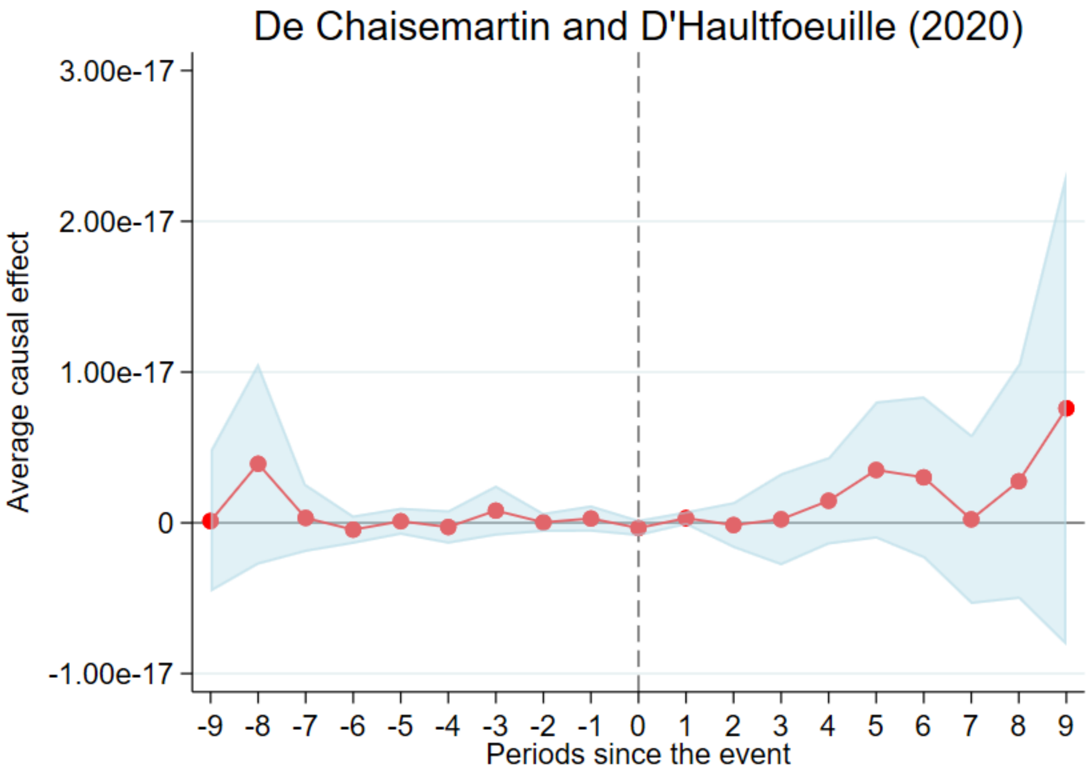

Figure 5.2: Event Plot of De Chaisemartin and d'Haultfoeuille (2020) Estimation

Figure 5.2 allows us to see that the pre-trend is close to zero. However the post-trend fails to show a change due to the treatment. However we can see that there's heterogeneity in terms of results even though the difference is minimal. This result further cements the fact that depolluting a brownfield in itself is not enough to improve the economic dynamism of the surrounding area.

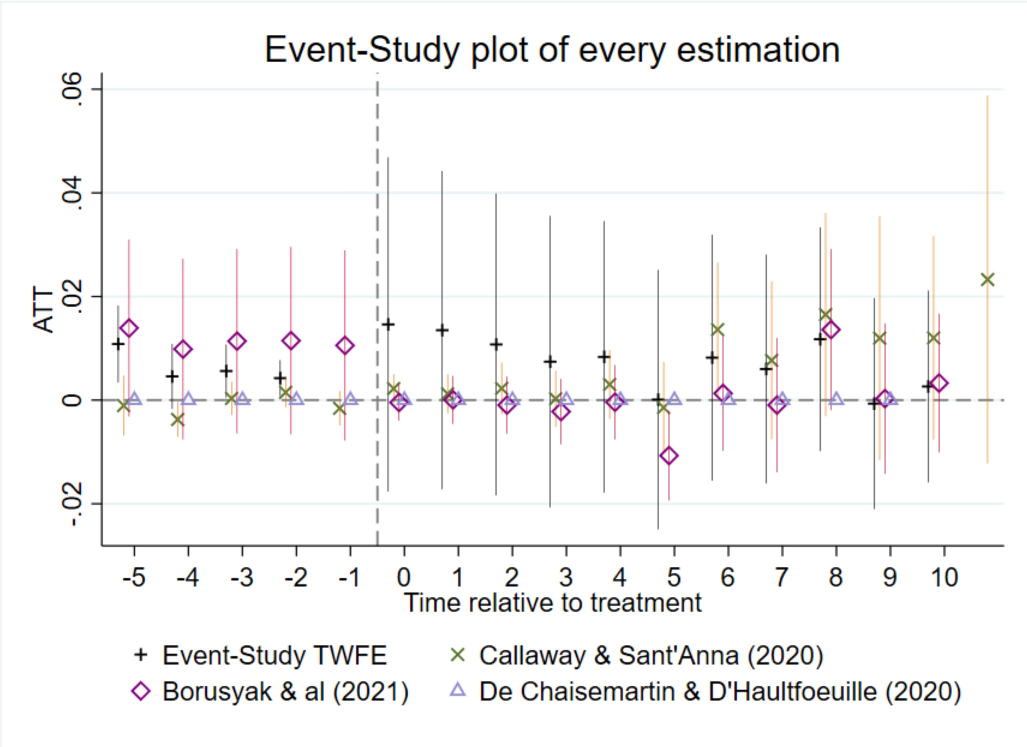

Figure 5.3: Event-Study Plot of Our Four Estimations

Plotting our four estimations together (figure 5.3) we can see that our Event-Study estimation with TWFE is biased and has important heterogeneity. However our estimation using the Callaway and Sant'Anna (2021) yields very accurate estimation result. However the further we go in time the more spurious the effect becomes, we lose the quality of our specification due to the different dynamics each neighborhood follows and a diminishing sample. De Chaisemartin and d'Haultfoeuille (2020) approach yield very accurate estimations with little treatment effect heterogeneity for which they control, and the imputation method by Borusyak et al. (2021) yields coefficients that vary around zero more than the rest of our estimations but still shows that there's no substantial effect of treatment. According to our results, we can assume that our parallel trends are respected[5]. However our treatment coefficient is either non-significant or close to zero. We can safely assume that according to our results, cleanup is not enough to economically reinvigorate the surrounding area.

[5]For a visualization of the parallel trends for our dependant variable for each treatment year see figure A.9 to A.19

6. Conclusion

Our results align with the findings in the literature. Brownfields cleanup in itself is not enough to improve the socio-economic background of the neighboring communities. Usually for a redeveloped brownfield to have a positive impact on the neighborhood, it needs to be remediated in conjunction with other infrastructure investments in the area. Our dataset also does not control for other maybe more important brownfields located in close proximity to the facilities we are analyzing, maybe those brownfields have a more important impact on the area than any benefits the reconversion our smaller brownfields may have. Also due to our highly aggregated dataset we are opening our flanks to the ecological fallacy, that might bias our results. In the future focusing on the BASIAS/BASOL dataset or the new, vastly improved CASIAS dataset might be more useful for our analysis, given that they aim to register every brownfields on French territory, from this more expanded dataset we can take into account the distance to another brownfield and see if it might impact our estimation. We are also susceptible to the omitted variable bias, given that due to lack of data we cannot control for the number of individuals in a census tract that holds higher education degrees. Limits can be posed to the homogeneous radius we chose for our buffer, some facilities are less important in size and economic interests than others. However we kept a 3-kilometer radius around each brownfield independently from their importance. Given that the number of census tracts captured by each brownfield is not homogeneous across units, more weight might be given to brownfields that capture the biggest amount of census tracts. Further improvement of the specifications of our models is needed to withdraw a clear causal relationship. One of the limits is also our pretrends test, the procedures developed by Rambachan and Roth (2022) and Roth (2019) are the most robust and should be a future improvement for this master thesis to further justify our parallel trend assumption. In this paper we mostly relied on two "low" power pretrend test and a visualization of the parallel trends of our variable of interest. A more careful approach to the covariates we chose is also of importance.

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A. Appendix

Table A.1: Means and differences for selected variables (pre-treatment)

Variable	Control(C)	Treatment(T)	Difference(T-C)
Declared median income(in euros)	19106,15	18992,86	-113,29
Unemployment and inactivity rate	18,46697	18,554	0,08703
Population	2256,293	2165,364	-90,929
Share under 19	25,13386	25,14182	0,00796
Share between 20 and 64	58,72607	59,30577	0,5797
Share above 65	16,14002	15,55241	-0,58761
Men(in %)	48,4336	48,31934	-0,11426
Women(in %)	51,56605	51,68066	0,11461
Immigrants(in %)	14,22558	10,37131	-3,85427
CAP-BEP graduates(in %)	22,1967	23,37742	1,18072
Baccalaureate graduates(in %)	15,83629	15,29428	-0,54201
Artisans, tradespeople and CEOs(in %)	2,911722	2,73037	-0,181352
Business exectutives, intellectual professions(in %)	9,578366	9,426863	-0,151503
Intermediate professions(in %)	14,24491	14,00083	-0,24408
Workers(in %)	13,36024	14,04319	0,68295
Pensioners(in %)	23,68608	23,61643	-0,06965
Houses(in %)	42,36352	48,59653	6,23301
Appartments(in %)	56,40506	50,23886	-6,1662

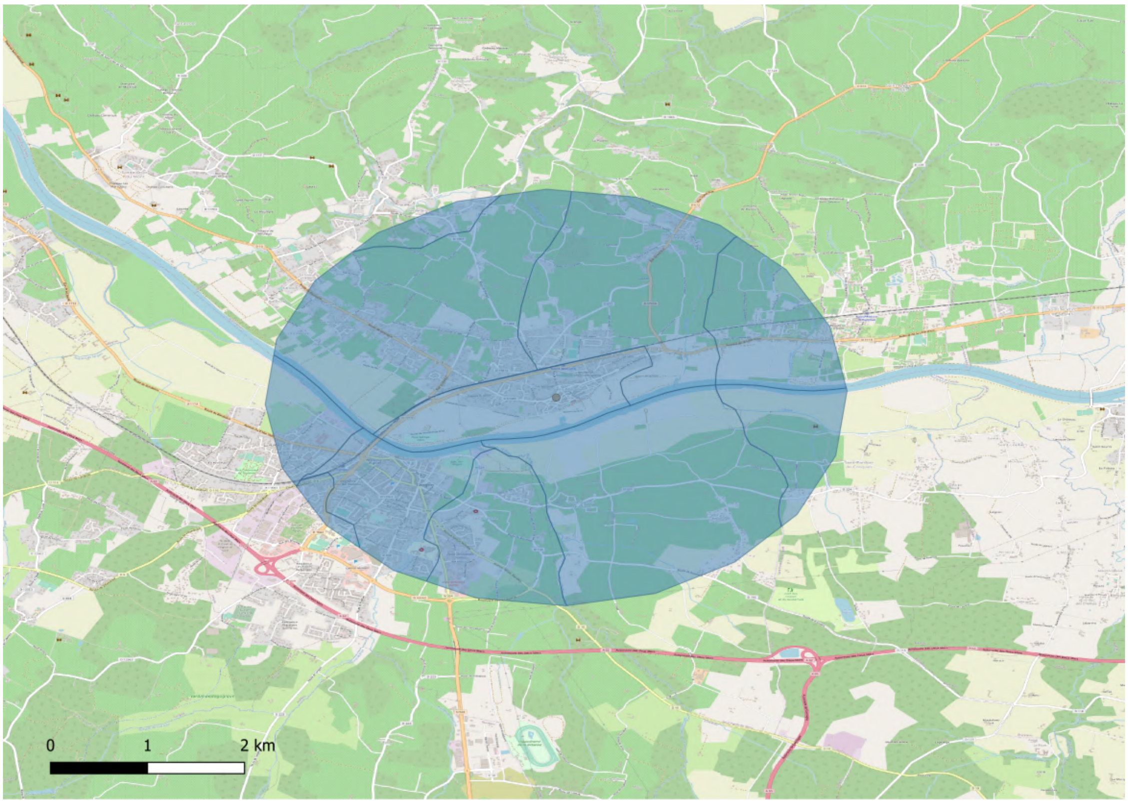

Figure A.1: Buffer zone around the brownfield PATRICK SAGE in Saint-Macaire with IRIS level data

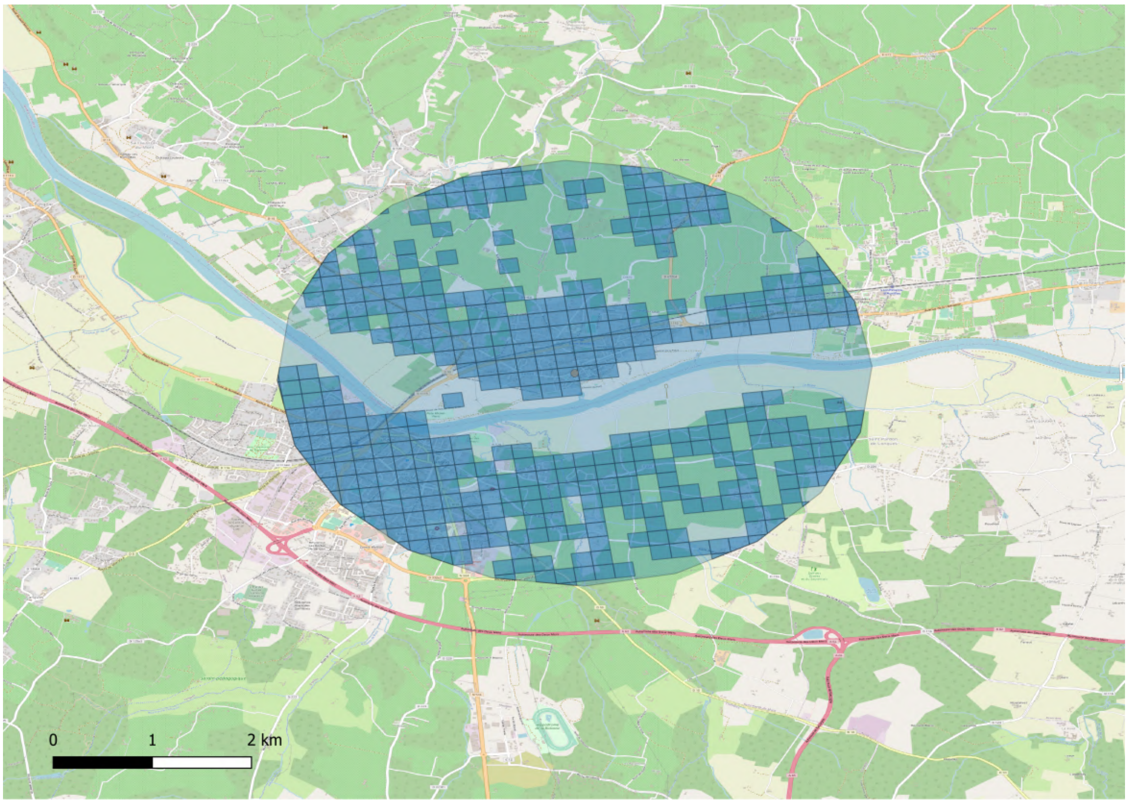

Figure A.2: Buffer zone around the brownfield PATRICK SAGE in Saint-Macaire with grided data

Figure A.3: Buffer zone around the brownfield WIPELEC in Romainville (Parisian suburb) with IRIS level data

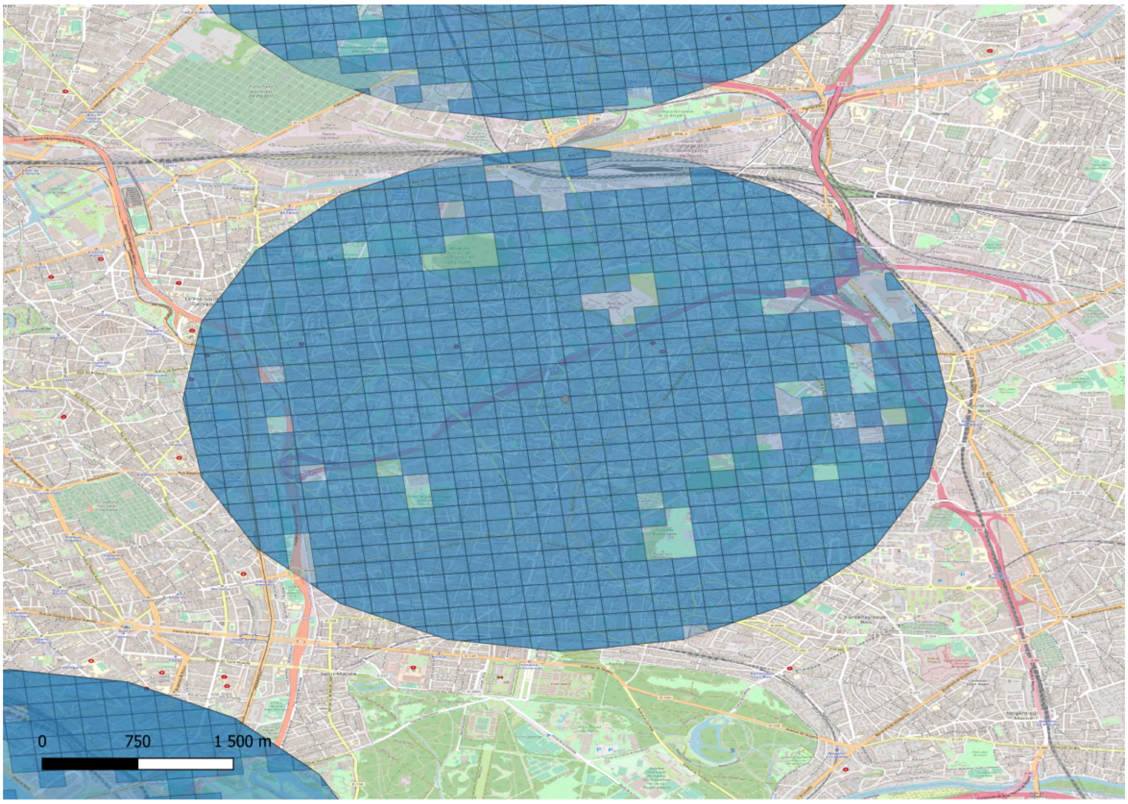

Figure A.4: Buffer zone around the brownfield WIPELEC in Romainville (Parisian suburb) with grided data

VARIABLES	log of the median income
Treatement	-0.005*
	(0.003)
Population	0.000**
	(0.000)
Share under 19 years old	0.030
	(0.066)
Share aged between 20-64 years old	0.035
	(0.066)
Share aged over 65 years old	0.036
	(0.066)
Artisans, tradespeople and CEOs(in %)	-0.001
	(0.002)
Business executives, intellectual professions(in %)	0.003***
	(0.001)
Intermediate professions(in %)	0.001
	(0.001)
Workers(in %)	-0.001
	(0.001)
Pensioners(in %)	-0.002
	(0.001)
Unemployment and inactivity rate	-0.003***
	(0.001)
Men(in %)	0.199***
	(0.073)
Women(in %)	0.198***
	(0.073)
Immigrants(in %)	-0.004***
	(0.001)
CAP-BEP graduates	-0.001**
	(0.001)
High-School graduates	-0.002***
	(0.001)
Houses(in %)	0.001
	(0.001)
Appartements(in %)	0.000
	(0.001)
Constant	-13.382*
	(7.589)
Time-year dummies	YES
Observations	7,315
Number of idIRIS	594
R-squared	0.660

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A.5: Static TWFE results

Table A.2: Pretrend test

Pretrend Test. H0: All Pre-treatment are equal to 0
chi2(54) = 229.4521
p-value = 0.0000
Average Treatment Effect on Treated

Table A.3: ATT coefficient

	Coef.	Std.Err.	z	P>z	[95% Conf.	Interval]
ATT	.0038614	.002906	1.33	0.184	-.0018342	.0095571

Table A.4: ATT by groups

	Coef.	Std.Err.	z	P>z	[95% Conf.	Interval]
GAverage	.0025555	.0024208	1.06	0.291	-.0021892	.0073003
G2007	.0148363	.0068782	2.16	0.031	.0013553	.0283172
G2008	-.016538	.0175849	-0.94	0.347	-.0510038	.0179279
G2009	-.0080271	.0370374	-0.22	0.828	-.0806191	.064565
G2010	.0166185	.0128865	1.29	0.197	-.0086385	.0418756
G2011	.0015713	.0076745	0.20	0.838	-.0134705	.0166131
G2012	.0095172	.0072974	1.30	0.192	-.0047855	.0238199
G2013	-.0076624	.0043502	-1.76	0.078	-.0161886	.0008638
G2014	.004484	.0033549	1.34	0.181	-.0020914	.0110595
G2015	-.033639	.006241	-5.39	0.000	-.045871	-.021407
G2016	.0066972	.0055734	1.20	0.230	-.0042265	.0176208
G2017	-.0068921	.0095819	-0.72	0.472	-.0256723	.0118881

Table A.5: ATT by calendar periods

	Coef.	Std.Err.	z	P>z	[95% Conf.	Interval]
CAverage	.0040426	.0026707	1.51	0.130	-.0011918	.0092771
T2007	-.0003788	.0028347	-0.13	0.894	-.0059346	.0051771
T2008	.002939	.003989	0.74	0.461	-.0048794	.0107574
T2009	.0077191	.0050444	1.53	0.126	-.0021678	.0176059
T2010	.0032508	.0049081	0.66	0.508	-.0063689	.0128705
T2011	.005266	.0040732	1.29	0.196	-.0027174	.0132494
T2012	.0087695	.0046102	1.90	0.057	-.0002664	.0178053
T2013	.0044619	.0033256	1.34	0.180	-.0020561	.0109799
T2014	.0034266	.0031806	1.08	0.281	-.0028073	.0096605
T2015	.0041976	.0036681	1.14	0.252	-.0029918	.011387
T2016	.004958	.0035751	1.39	0.165	-.002049	.011965
T2017	.0032371	.0037044	0.87	0.382	-.0040234	.0104977
T2018	.0006648	.0043518	0.15	0.879	-.0078647	.0091942

Table A.6: ATT by Periods Before and After treatment

	Coef.	Std.Err.	z	P>z	[95% Conf.	Interval]
Pre_avg	-.0015937	.0011405	-1.40	0.162	-.003829	.0006415
Post_avg	.0077189	.004889	1.58	0.114	-.0018633	.0173011
Tm10	-.0071126	.0087722	-0.81	0.417	-.0243057	.0100805
Tm9	.0038505	.0040277	0.96	0.339	-.0040437	.0117446
Tm8	-.0101747	.0032487	-3.13	0.002	-.016542	-.0038074
Tm7	.0006227	.0020873	0.30	0.765	-.0034683	.0047137
Tm6	.0014126	.0023112	0.61	0.541	-.0031173	.0059425
Tm5	-.0010473	.0029484	-0.36	0.722	-.006826	.0047314
Tm4	-.0037372	.0017145	-2.18	0.029	-.0070975	-.000377
Tm3	.0003263	.0016306	0.20	0.841	-.0028697	.0035223
Tm2	.001498	.0014568	1.03	0.304	-.0013573	.0043532
Tm1	-.0015754	.0017002	-0.93	0.354	-.0049079	.001757
Tp0	.0022281	.0014163	1.57	0.116	-.0005478	.0050041
Tp1	.0012352	.001925	0.64	0.521	-.0025378	.0050081
Tp2	.0022684	.0025324	0.90	0.370	-.0026951	.0072319
Tp3	.0002514	.0027659	0.09	0.928	-.0051696	.0056725
Tp4	.00302	.0033848	0.89	0.372	-.003614	.009654
Tp5	-.0014394	.004496	-0.32	0.749	-.0102514	.0073725
Tp6	.013615	.0065945	2.06	0.039	.0006899	.02654
Tp7	.0076728	.0077619	0.99	0.323	-.0075403	.0228859
Tp8	.016513	.0100029	1.65	0.099	-.0030923	.0361183
Tp9	.0119687	.012011	1.00	0.319	-.0115724	.0355099
Tp10	.0120114	.0100138	1.20	0.230	-.0076153	.0316382
Tp11	.0232827	.0181189	1.28	0.199	-.0122297	.0587951

VARIABLES	(1) Poverty rate
Treatement	0.496***
	(0.136)
Population	0.003*
	(0.002)
Share aged 80+	-0.128***
	(0.024)
Share aged between 65-79	-0.135***
	(0.021)
Share aged between 55-64	-0.076***
	(0.021)
Share aged between 40-54	-0.139***
	(0.023)
Share aged between 25-39	-0.151***
	(0.020)
Share aged between 18-24	0.007
	(0.016)
Share aged between 11-17	0.050**
	(0.022)
Share aged between 6-10	0.014
	(0.023)
Share aged between 4-5	0.006
	(0.026)
Share aged between 0-3	0.029
	(0.024)
Share of owning households	-0.083***
	(0.008)
Share of households with 5 individuals	0.020*
	(0.011)
Share of households with 1 individual	0.083***
	(0.007)
Constant	22.820***
	(1.871)
Time FE	YES
Census Tracts FE	YES
Observations	67,274
Number of idCARR	33,637
R-squared	0.032

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A.6: Results of the TWFE estimation with grided data

VARIABLES	log of the median income
Treatment	-0.016
	(0.017)
Population	0.000**
	(0.000)
Share aged under 19 years old	0.029
	(0.065)
Share aged between 20-64 years old	0.034
	(0.065)
Share aged above 65 years old	0.035
	(0.065)
Artisans, tradespeople,CEOs(%)	-0.001
	(0.002)
Executives and intellectuals(%)	0.002***
	(0.001)
Intermediate professions(in %)	0.001
	(0.001)
Workers(in %)	-0.001
	(0.001)
Pensioners(in %)	-0.002
	(0.001)
Unemployment and inactivity rate	-0.003***
	(0.001)
Men(in %)	0.206***
	(0.072)
Women(in %)	0.205***
	(0.072)
Immigrants(in %)	-0.004***
	(0.001)
CAP BEP graduates(in %)	-0.001**
	(0.001)
High school graduates(in %)	-0.002***
	(0.001)
Houses(in %)	0.001
	(0.001)
Appartements(in %)	0.000
	(0.001)
Constant	-13.941*
	(7.470)
Observations	7,315
R-squared	0.988
Within R-squared	0.179

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A.7: Event Study results (covariates)

VARIABLES	lmed
L_0	0.015
	(0.016)
L_1	0.013
	(0.016)
L_2	0.011
	(0.015)
L_3	0.007
	(0.014)
L_4	0.008
	(0.013)
L_5	0.000
	(0.013)
L_6	0.008
	(0.012)
L_7	0.006
	(0.011)
L_8	0.012
	(0.011)
L_9	-0.001
	(0.010)
L_10	0.003
	(0.009)
F_2	0.004**
	(0.002)
F_3	0.006**
	(0.003)
F_4	0.005
	(0.003)
F_5	0.011***
	(0.004)
F_6	0.006
	(0.004)
F_7	0.006
	(0.005)
F_8	-0.004
	(0.006)
F_9	0.018***
	(0.007)
F_10	0.013*
	(0.008)
Constant	-13.941*
	(7.470)
Observations	7,315
R-squared	0.988
Within R-squared	0.179

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A.8: Event Study results for Leads(F) and Lags(L)

Table A.7: Estimates from the Difference-in-differences à la Borusyak et al. (2021)

				Number of obs =		17,344
DVAR: log of median income	**Coef.**	**Std. Err.**	**z**	**P>z**	**[95% Conf.**	**Interval]**
Population	.0000129	6.06e-06	2.13	0.034	1.01e-06	.0000248
Share under 19	.097594	.0770291	1.27	0.205	.0533803	.2485683
Share between 20-64	.1005641	.0770155	1.31	0.192	.0503836	.2515117
Share above 65	.1023462	.0769535	1.33	0.184	.0484799	.2531722
Artisans, tradespeople,CEOs(%)	-.0029131	.0014946	-1.95	0.051	.0058424	.0000162
Executives and intellectuals(%)	.0027127	.0008022	3.38	0.001	.0011404	.004285
Intermediate professions(%)	.0009462	.0007244	1.31	0.192	.0004737	.002366
Workers(in %)	.000141	.0008397	0.17	0.867	.0015047	.0017867
Pensioners(in %)	-.0025375	.0008175	-3.10	0.002	.0041397	-.0009352
Unemployment, inactivity rate	-.0022247	.0006157	-3.61	0.000	.0034314	-.0010181
Men(in %)	-.0469103	.0788047	-0.60	0.552	.2013647	.1075442
Women(in %)	-.0487418	.078666	-0.62	0.536	.2029243	.1054408
Immigrants(in %)	-.0032724	.0006268	-5.22	0.000	.0045009	-.0020439
CAP-BEP graduates(in %)	-.0007277	.0004175	-1.74	0.081	-.001546	.0000906
High school graduates(in %)	-.0017603	.0005294	-3.33	0.001	-.002798	-.0007227
Houses(in %)	.0003276	.0007423	0.44	0.659	.0011272	.0017824
Appartements(in %)	.000151	.0005541	0.27	0.785	.0009351	.001237

Table A.8: Leads and lags estimates from the Difference-in-differences à la Borusyak et al. (2021)

				Number of obs =		17,344
DVAR: log of median income	Coef.	Std. Err.	z	P>z	[95% Conf.	Interval]
tau0	-.0004982	.0018012	-0.28	0.782	.0040285	.0030321
tau1	-.0000513	.002366	-0.02	0.983	.0046886	.004586
tau2	-.0010525	.0028091	-0.37	0.708	.0065582	.0044533
tau3	-.0023242	.0032204	-0.72	0.470	.0086359	.0039876
tau4	-.0004978	.0036574	-0.14	0.892	.0076662	.0066706
tau5	-.01076	.0044296	-2.43	0.015	.0194419	-.0020782
tau6	.0012874	.0056492	0.23	0.820	.0097848	.0123597
tau7	-.0009817	.0066059	-0.15	0.882	-.013929	.0119656
tau8	.0134826	.007947	1.70	0.090	.0020931	.0290584
tau9	.0001041	.0074065	0.01	0.989	.0144125	.0146206
tau10	.0031363	.0068238	0.46	0.646	.0102381	.0165106
pre1	.0111166	.0096958	1.15	0.252	.0078869	.03012
pre2	.0120323	.0095536	1.26	0.208	.0066924	.0307571
pre3	.0119515	.0094009	1.27	0.204	-.006474	.030377
pre4	.0104288	.0092327	1.13	0.259	.0076668	.0285245
pre5	.0144902	.0090325	1.60	0.109	.0032132	.0321937
pre6	.0104834	.0087985	1.19	0.233	.0067614	.0277282
pre7	.0085481	.0086512	0.99	0.323	-.008408	.0255042
pre8	.0030459	.0086349	0.35	0.724	.0138782	.0199701
pre9	.0192143	.0086741	2.22	0.027	.0022133	.0362152
pre10	.0141444	.0087817	1.61	0.107	.0030674	.0313563

Table A.9: Pretrend test from the imputation

scalars:		
e(N)	=	17344
e(Nc)	=	13559
e(Niter)	=	5
Pretrend F-statistic	=	3.465828636414672
Pretrend p.value	=	.0001633881204116
Pretrend D-F	=	1318

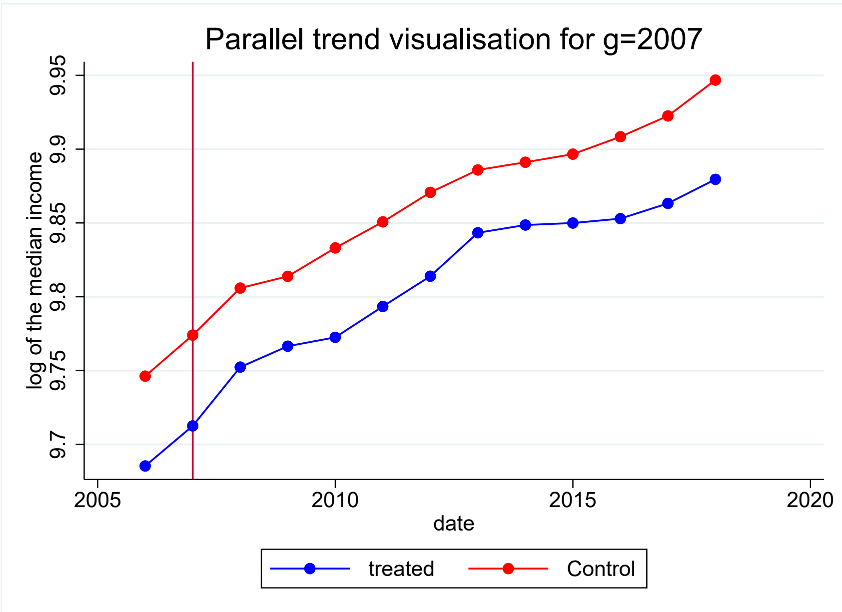

Figure A.9: Parallel trend visualisation for the group treated in 2007

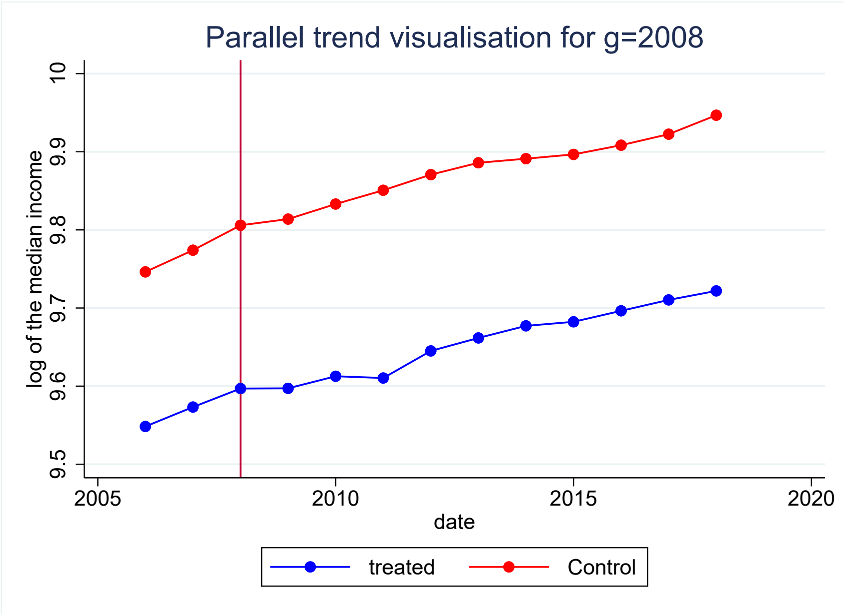

Figure A.10: Parallel trend visualisation for the group treated in 2008

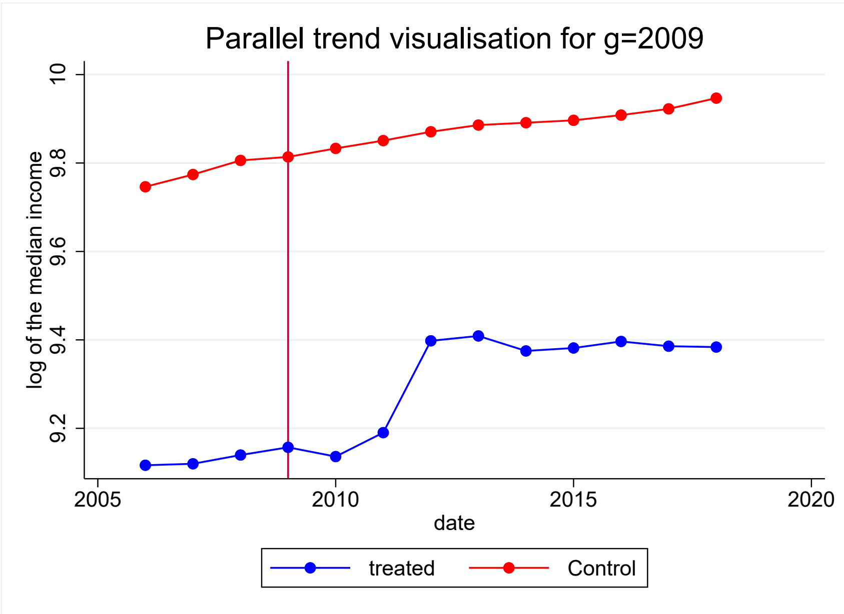

Figure A.11: Parallel trend visualisation for the group treated in 2009

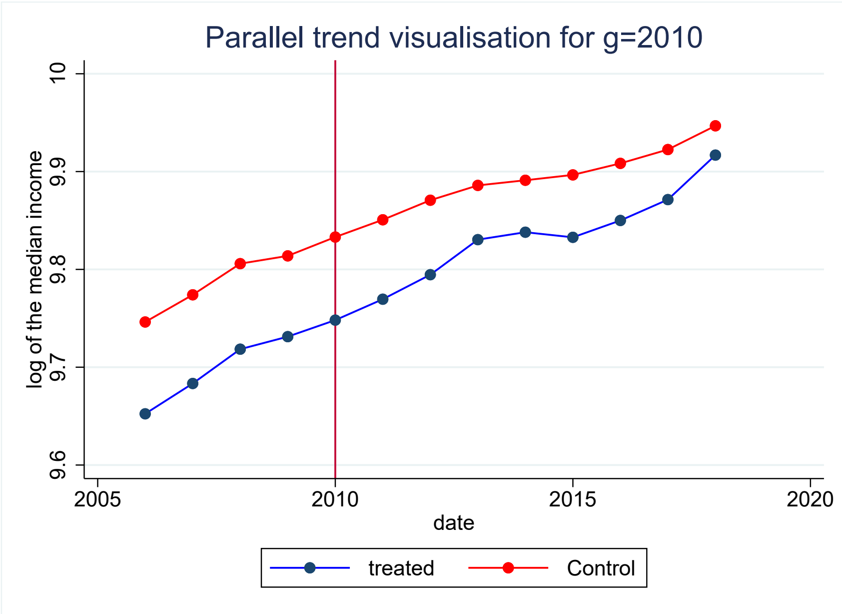

Figure A.12: Parallel trend visualisation for the group treated in 2010

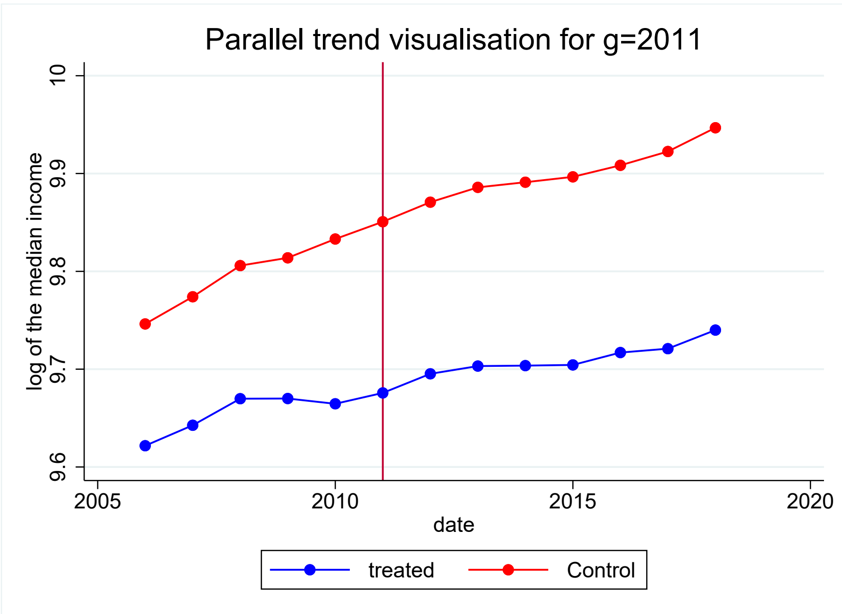

Figure A.13: Parallel trend visualisation for the group treated in 2011

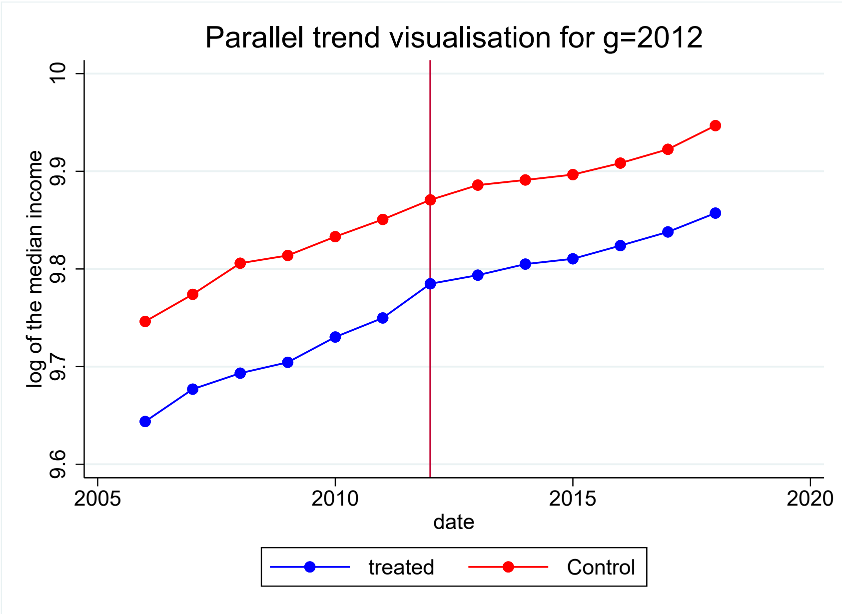

Figure A.14: Parallel trend visualisation for the group treated in 2012

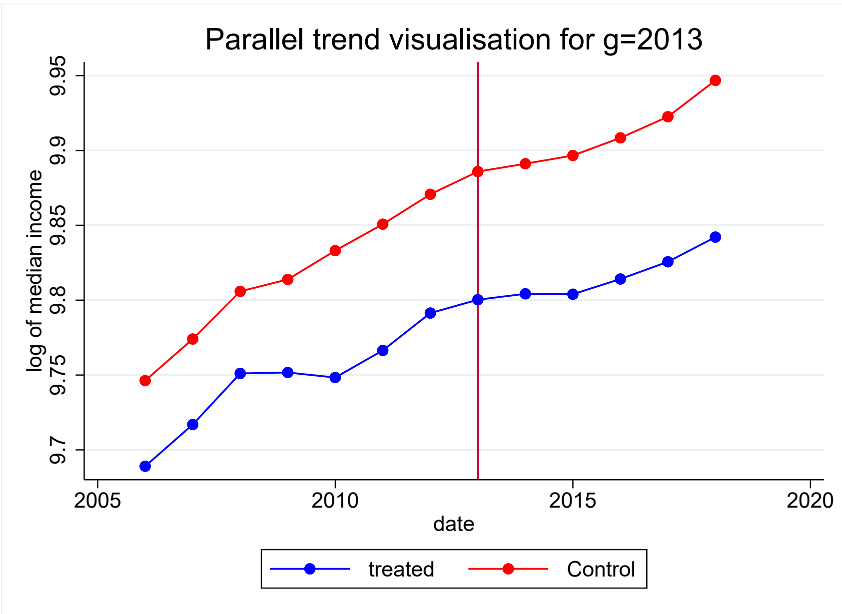

Figure A.15: Parallel trend visualisation for the group treated in 2013

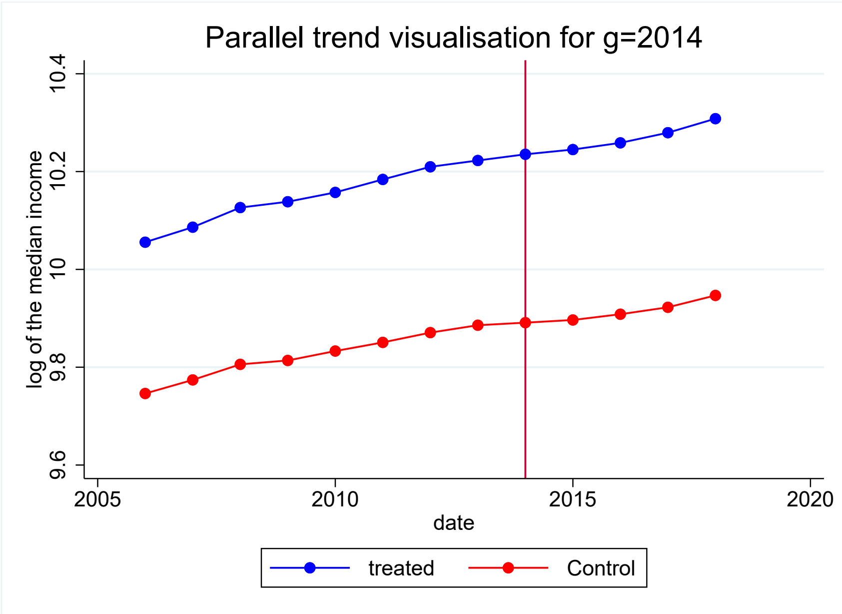

Figure A.16: Parallel trend visualisation for the group treated in 2014

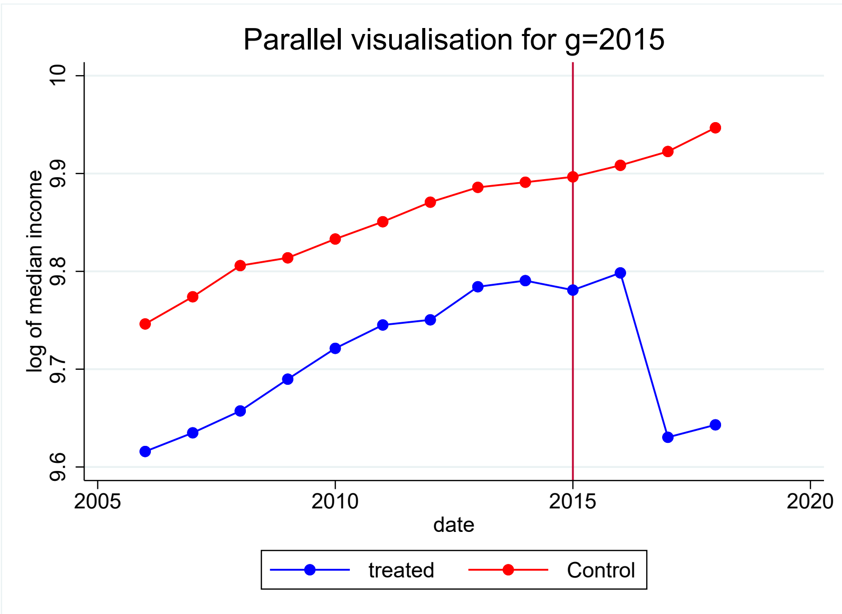

Figure A.17: Parallel trend visualisation for the group treated in 2015

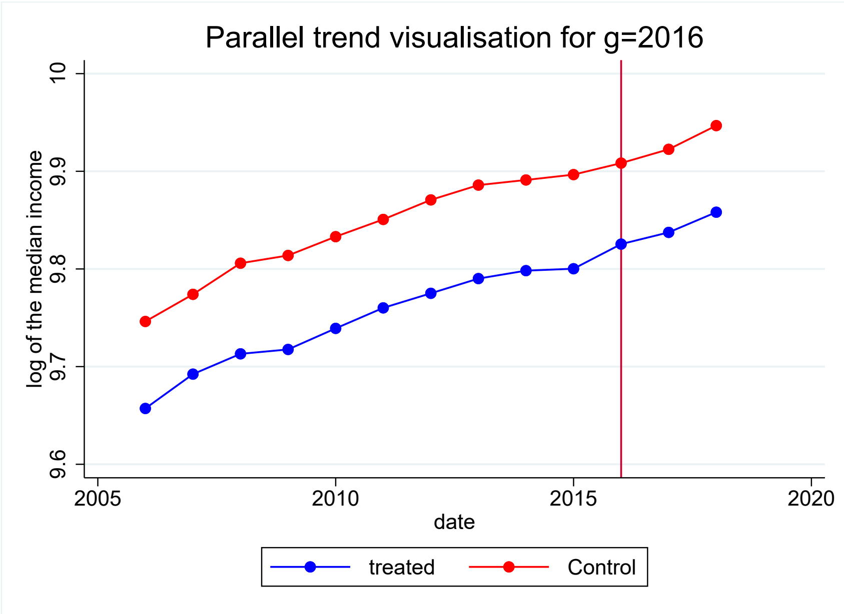

Figure A.18: Parallel trend visualisation for the group treated in 2016

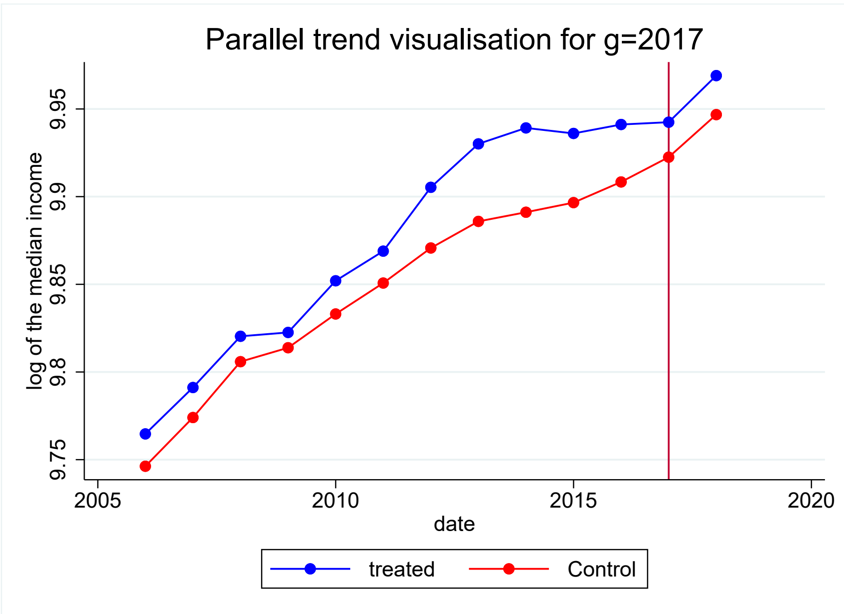

Figure A.19: Parallel trend visualisation for the group treated in 2017

Table A.10: De Chaisemartin and d'Haultfoeuille(2020) estimation results with 10 iterations

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	-3.40e-19	2.77e-19	-8.84e-19	2.03e-19	3392	544
Effect_1	3.09e-19	2.33e-19	-1.48e-19	7.66e-19	2841	502
Effect_2	-1.46e-19	7.80e-19	-1.68e-18	1.38e-18	2354	501
Effect_3	2.34e-19	1.56e-18	-2.82e-18	3.29e-18	1758	387
Effect_4	1.46e-18	1.48e-18	-1.44e-18	4.37e-18	1181	234
Effect_5	3.50e-18	2.32e-18	-1.05e-18	8.06e-18	780	196
Effect_6	3.02e-18	2.74e-18	-2.35e-18	8.39e-18	403	132
Effect_7	2.32e-19	2.87e-18	-5.39e-18	5.85e-18	205	94
Effect_8	2.76e-18	3.98e-18	-5.05e-18	1.06e-17	149	91
Effect_9	7.60e-18	8.03e-18	-8.14e-18	2.33e-17	78	72
Effect_10	.	.	.	.	.	.
Placebo_1	2.87e-19	4.49e-19	-5.92e-19	1.17e-18	2803	464
Placebo_2	3.24e-20	3.25e-19	-6.05e-19	6.70e-19	2297	444
Placebo_3	8.13e-19	8.51e-19	-8.56e-19	2.48e-18	1811	440
Placebo_4	-2.79e-19	5.68e-19	-1.39e-18	8.34e-19	1347	400
Placebo_5	1.08e-19	4.60e-19	-7.95e-19	1.01e-18	923	339
Placebo_6	-4.53e-19	4.81e-19	-1.40e-18	4.90e-19	572	301
Placebo_7	3.22e-19	1.15e-18	-1.93e-18	2.58e-18	260	149
Placebo_8	3.92e-18	3.42e-18	-2.78e-18	1.06e-17	100	42
Placebo_9	1.21e-19	2.40e-18	-4.59e-18	4.83e-18	47	41
Placebo_10	.	.	.	.	.	.

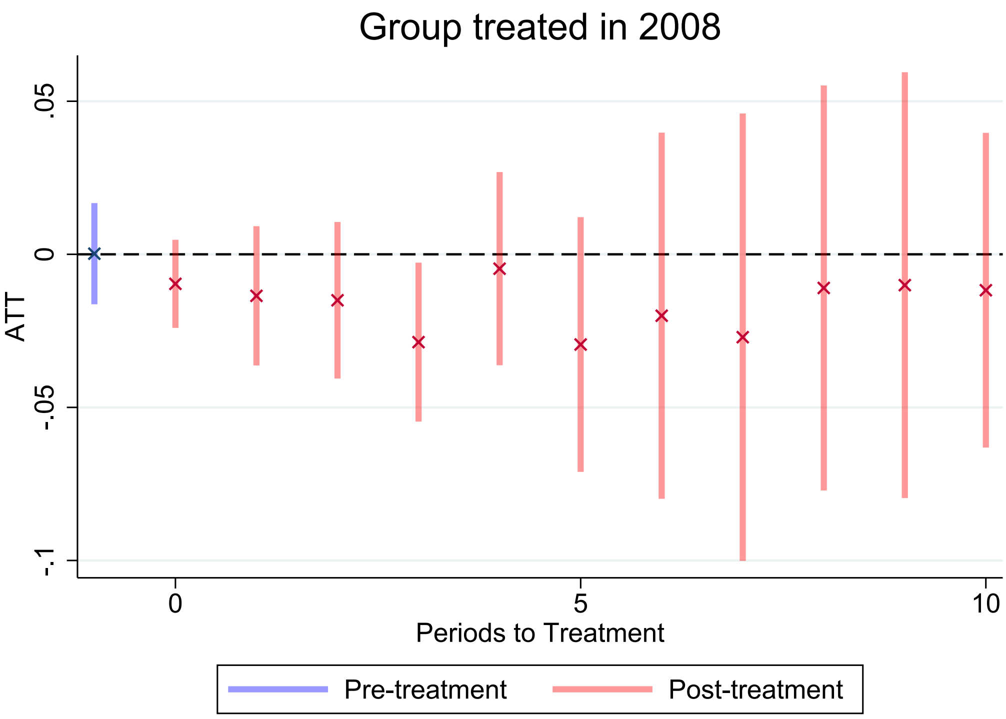

Figure A.20: ATT for the group treated in 2008

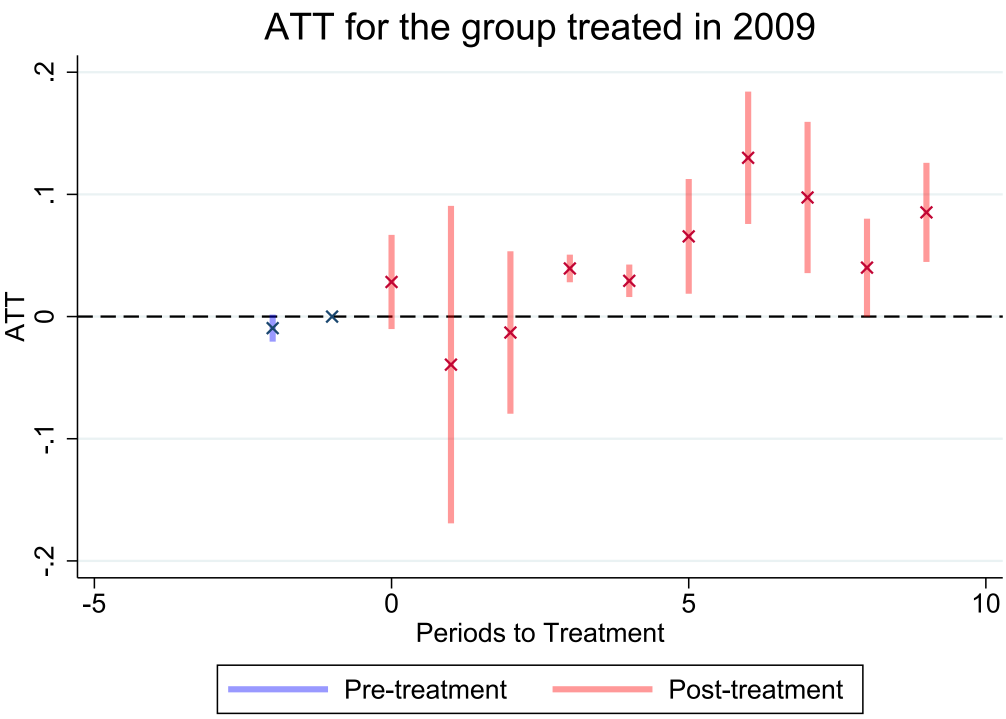

Figure A.21: ATT for the group treated in 2009

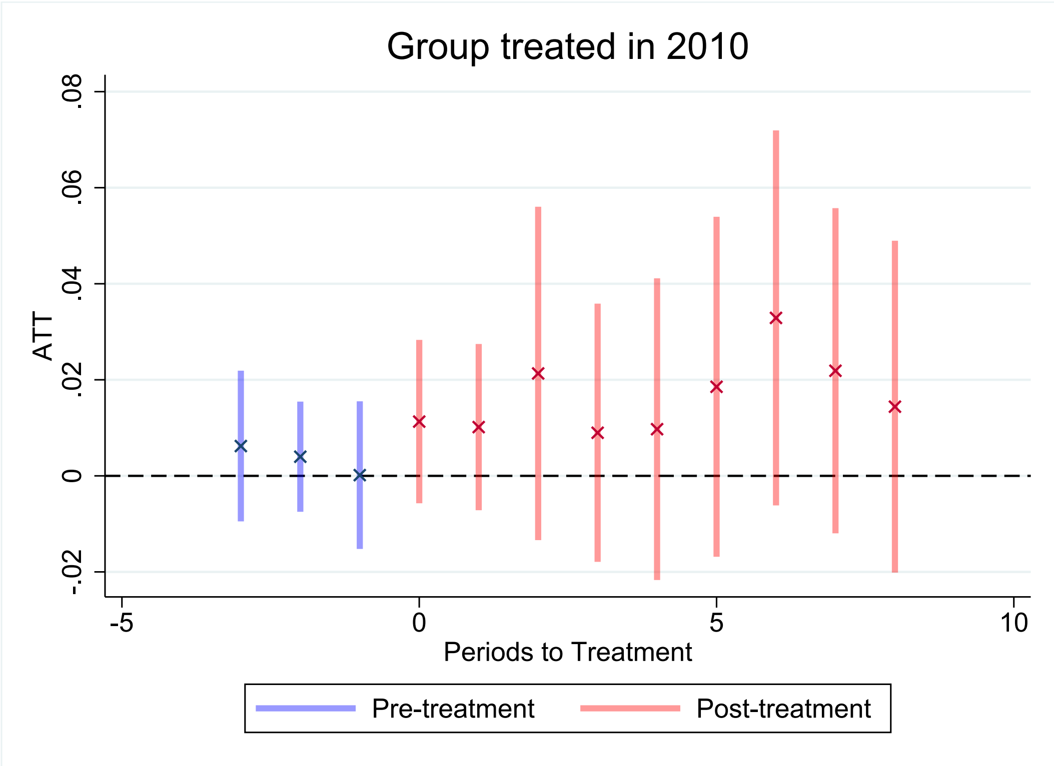

Figure A.22: ATT for the group treated in 2010

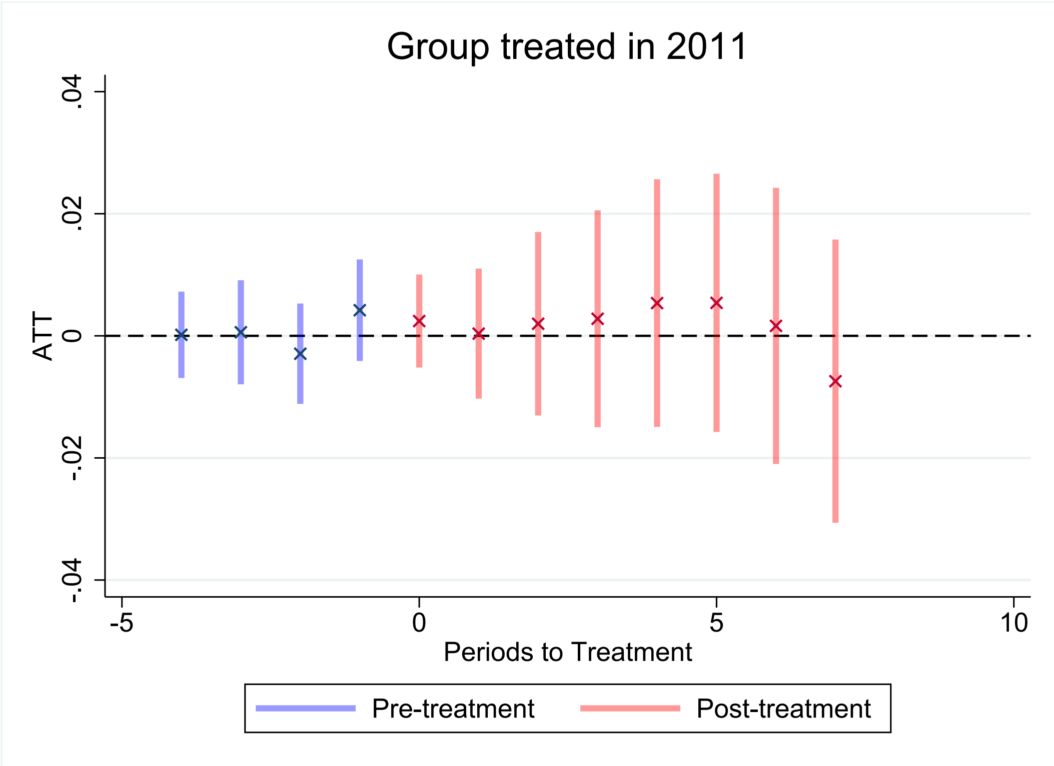

Figure A.23: ATT for the group treated in 2011

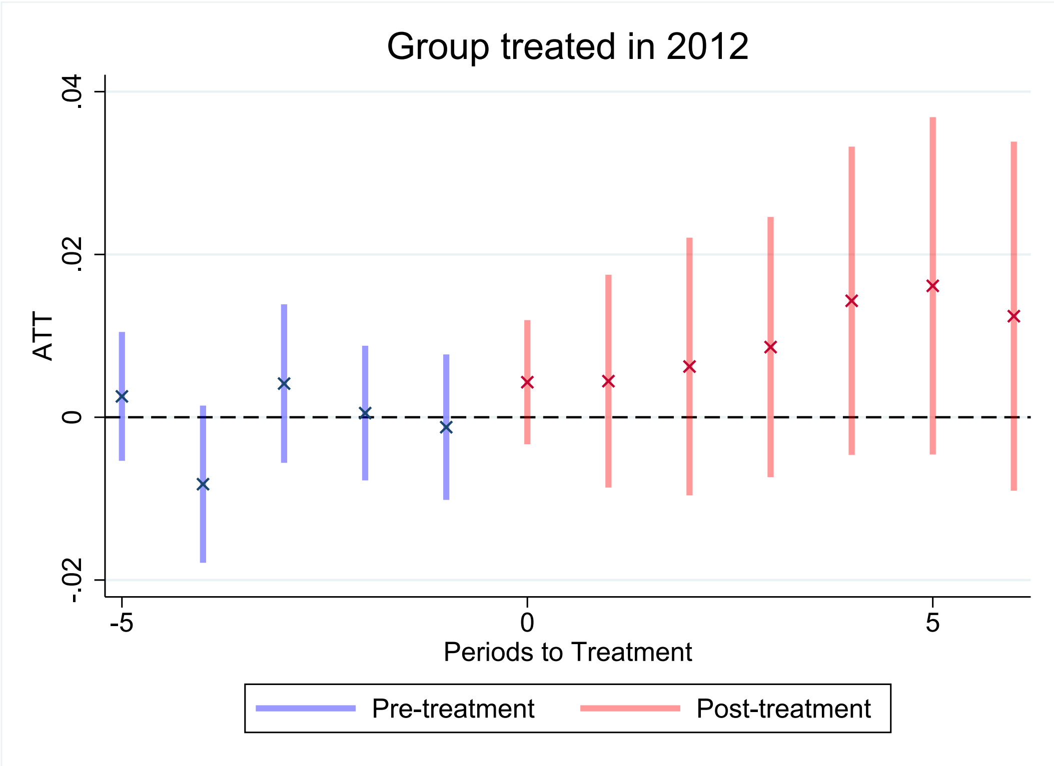

Figure A.24: ATT for the group treated in 2012

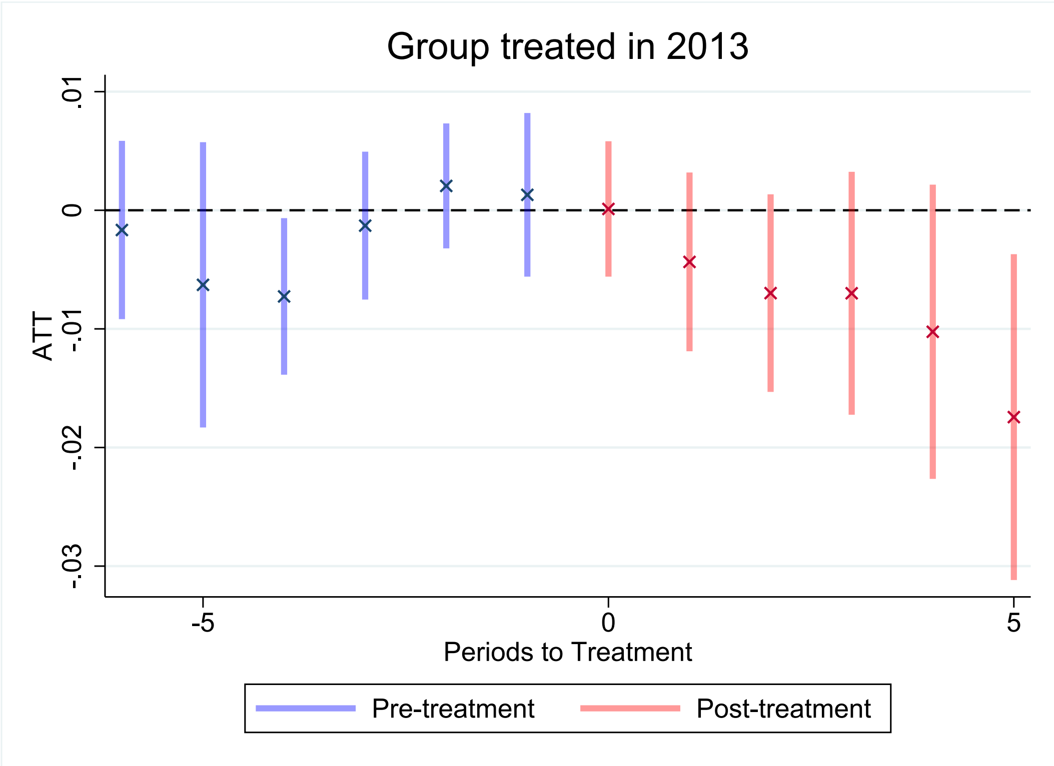

Figure A.25: ATT for the group treated in 2013

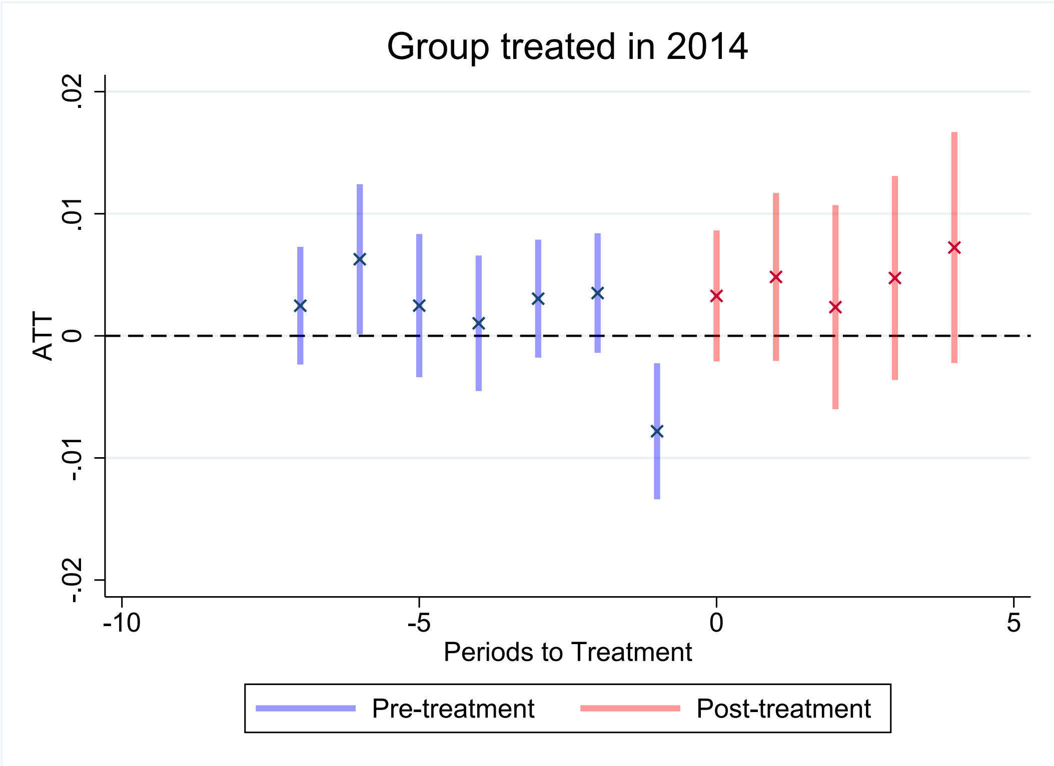

Figure A.26: ATT for the group treated in 2014

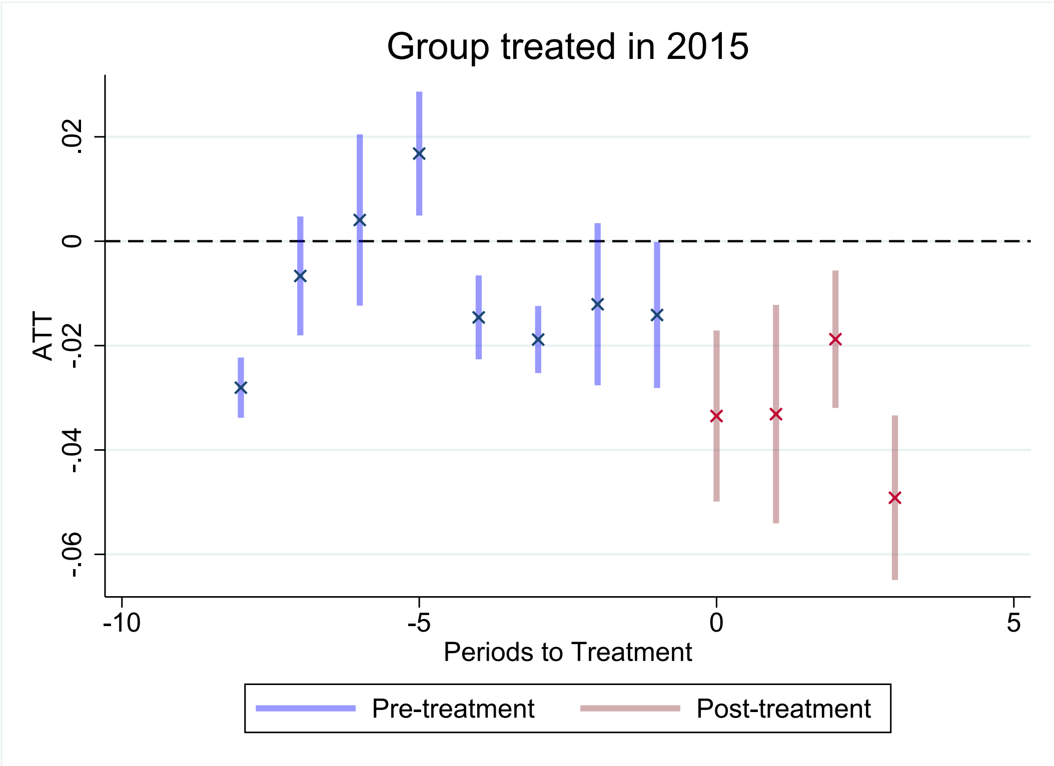

Figure A.27: ATT for the group treated in 2015

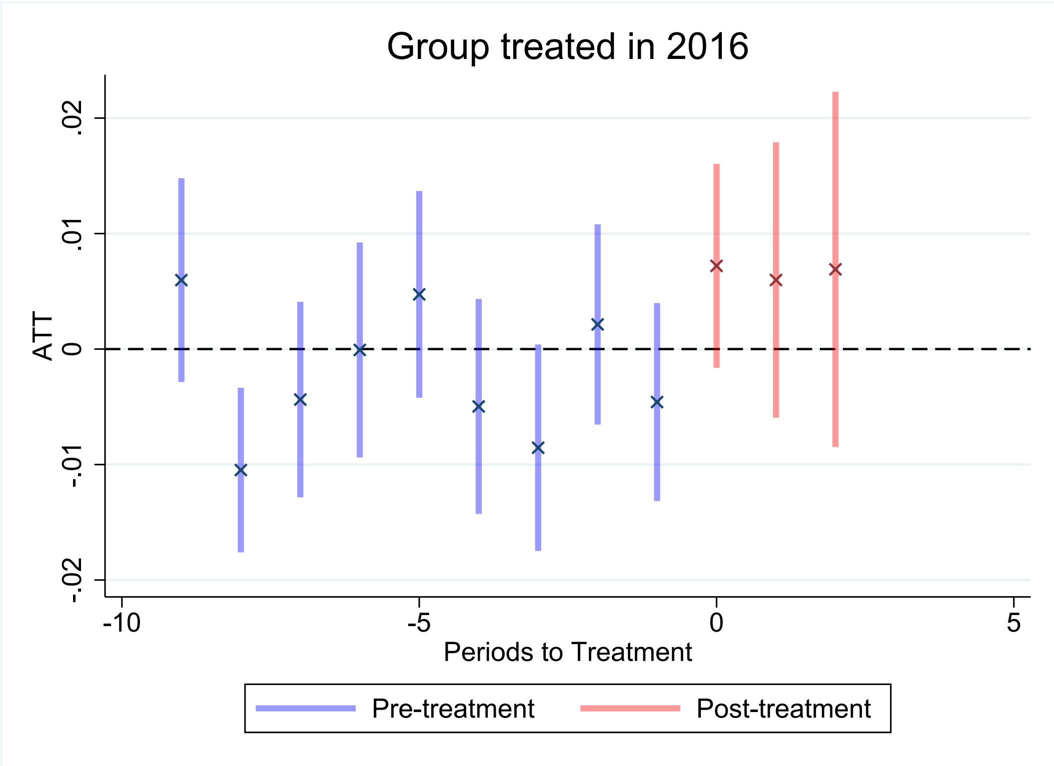

Figure A.28: ATT for the group treated in 2016

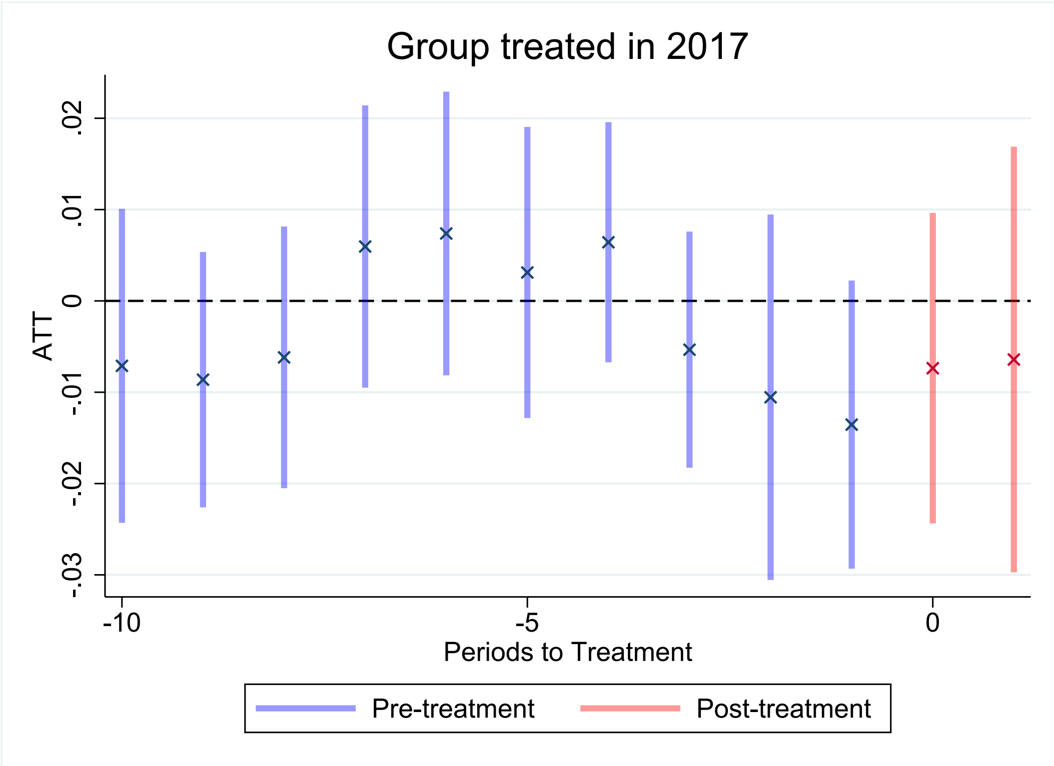

Figure A.29: ATT for the group treated in 2017