The effect of the West Philadelphia Promise Zone on violent crime

Alexander Marsella*

Berry College

July 23, 2024

Abstract

This paper examines the effect of the West Philadelphia Promise Zone initiative on violent crime rates in a high-crime area of West Philadelphia, where a series of educational, public-safety, and quality-of-life improvement grants were disbursed from 2014 onward. Synthetic difference-in-differences estimates suggest that this resulted in approximately four fewer violent crimes per thousand residents per year, primarily attributable to a reduction in assaults. A block-level synthetic control analysis corroborates this result. This is estimated to increase quality of life (through reduced crime victimization cost) by millions of dollars within the Promise Zone.

JEL Codes: H70, K42, R50
Keywords: Crime, Crime Prevention, Place-Based Policies, Public Economics, Urban Economics

Abbreviations: ACS - American Community Survey, ATT - Average Treatment Effect on Treated, BCJI - Byrne Criminal Justice Initiative, LAPZ - Los Angeles Promise Zone, MVM - Mount Vernon Manor, NRF - Neighborhood Renewal Fund, PPD - Philadelphia Police Department

*Department of Economics, Berry College, Mt. Berry, GA, USA; Email: amarsella@berry.edu

1 Introduction

Crime causes a plethora of negative externalities in a city in addition to harming the victim; it leads to economic damage, population loss, and psychological damage for residents. Many programs and policies have been proposed to reduce crime in cities, including "place-based" policies. In this paper, I study a recent place-based policy in Philadelphia: the "West Philadelphia Promise Zone".

This area, home to approximately 2% of the city's population incurs over $100 million annually in direct and indirect costs[1] of violent crime. In 2014, it was designated as the West Philadelphia Promise Zone by the Obama administration. This designation, which was part of a broader Obama-administration urban revitalization program, fast-tracked grants to community organizations within specific geographic areas and created a network of local organizations to work with local law enforcement and community leaders to address local problems such as violent crime. Through this program, over $75 million in grant funding was secured to be disbursed from 2014 to 2022 (Stoker and Rich, 2020).

This paper fills a niche in the literature regarding place-based policy analysis in that, to my knowledge, no published research has studied the Philadelphia Promise Zone directly and few have studied Promise Zones in general. Previous analyses done by community groups in Philadelphia provide descriptive statistics but do not use inferential techniques such as difference-in-differences.

One would expect that releasing large sums of grant money into the communities within the Promise Zone, especially money for diversion programs for at-risk youth and criminal offenders returning to the community, would induce a gradual reduction in violent crime rates as the treatment takes hold. In addition to this, various quality-of-life improvement grants, with millions of dollars of funding being released every year, should result in gradual but varying reductions in violent crime. Further, large grants targeted at area schools should reduce truancy and therefore, crime, committed by youth attending those schools.

That being said, the literature is mixed on whether federally sponsored place-based policies as a general concept are effective in improving areas with high rates of poverty and violence. Some research finds that certain place-based policies were ineffective at helping area residents (Neumark and Young, 2019; Chen et al., 2020; Freedman et al., 2021; Sage et al., 2021) or were too expensive

[1] Based on crime data from OpenDataPhilly.org and estimated crime costs from the RAND Corporation.

given the mild improvements created (Glaeser and Gottlieb, 2008). On the other hand, there is research that finds more positive effects from place-based policies (Busso et al., 2013; Austin et al., 2018). To my knowledge, the only other empirical analysis of a Promise Zone is Kitchens and Wallace (2022)'s analysis of the Los Angeles Promise Zone (LAPZ). While they find that the LAPZ appreciated housing values, they find no effect on violent crime. Unlike their finding regarding the LAPZ, I find a significant reduction in violent crime attributable to the West Philadelphia Promise Zone's overarching impact.

2 Background on the Promise Zone

2.1 Byrne Criminal Justice Innovation

The Byrne Criminal Justice Innovation (BCJI) facilitated the process of getting a grant for the neighborhood of Mantua[2] from 2012 to 2016 focused on data-driven policing strategies in coordination with community residents and leaders to study criminogenic areas. For six months, Mount Vernon Manor Community Development Corporation (MVM)[3] planned and coordinated with the police department, the US Attorney's Office, and a research partner from Drexel University. Any planned community intervention had to be motivated by data and other evidence facilitated through the research partner. During this time, the team gathered community feedback, administrated focus groups, interviewed residents, and studied crime occurrences geographically using the OpenDataPhilly API, which tracks crime incidents.

This program aimed to develop hot spot policing strategies and data-driven crime-reduction policies and to engage with local community leaders.[4] It also used a competitive grant system in support of partnerships between the local government and nonprofit organizations in the area. At one particular crime hot spot—the corner of 34th and Haverford Avenue—Mount Vernon Manor reported a 65% reduction in 911 calls and an elimination of all arrests. This claim was not based on a causal inference analysis, but it furnishes us the hypothesis that the BCJI affected crime.

The BCJI facilitated several programs, implemented in the fall of 2013, in Mantua that may

[2]Mantua covers approximately one-quarter of the Promise Zone.

[3]This nonprofit organizes joint action by the police, other local nonprofits, and the community to address crime. See http://www.mvmcdc.org/programs/public-safety/ for more information.

[4]More information at https://bja.ojp.gov/program/byrne-criminal-justice-innovation-bcji-program/overview

have reduced violent crime, such as removal of blight, community collaboration with police, more youth programs, and a school-based youth court.

Stokes (2020) provides an in-depth descriptive analysis of this program along with the Promise Zone. He argues that the early stages of the BCJI were contentious for several reasons. First, the Philadelphia Police Department (PPD) misunderstood parts of the proposal; for example, a misunderstanding regarding whether the grant would support police equipment (it did not) delayed a memorandum of understanding that would establish a budget for police overtime pay in Mantua. Second, since Mount Vernon Manor was the beneficiary of the grant and allocated grant funds in the area, this made things more difficult for city planners, since Mount Vernon is a private entity. This resulted in a two-year-long reduction in trust between the police department and Mount Vernon Manor, which was reversed when a new police captain took office two years into the program. Coincidentally or not, this was around when the Promise Zone initiative began in West Philadelphia (which contains Mantua). Stokes (2020) goes on to assert that it was not until the Promise Zone designation for West Philadelphia that the effort in Mantua became more effective. The actual BCJI interventions did not begin until the fall of 2013.

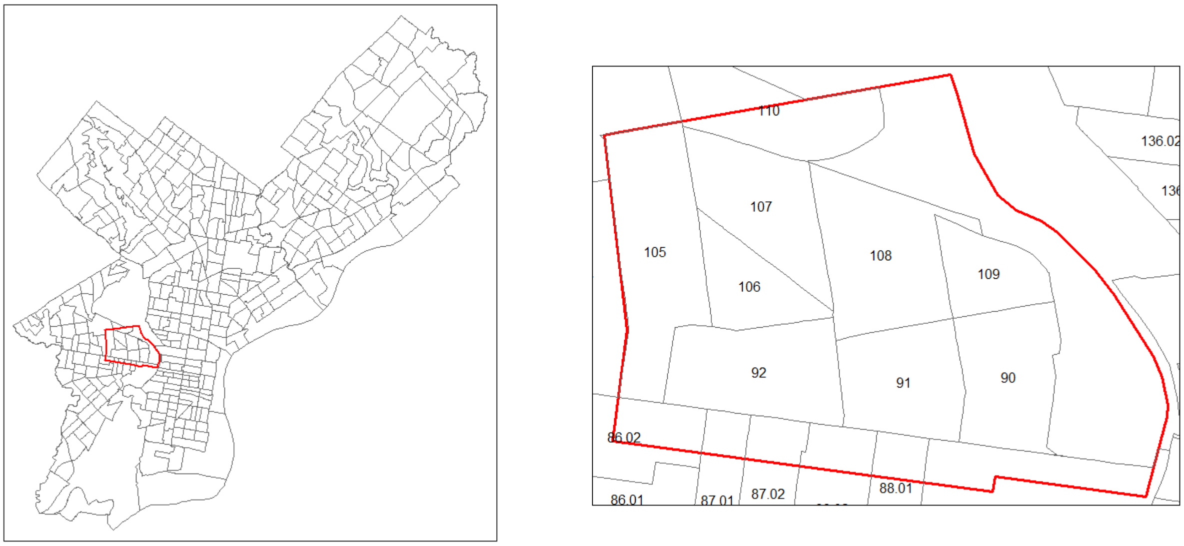
2.2 The West Philadelphia Promise Zone

As of January 2014, much of West Philadelphia, including Mantua and surrounding neighborhoods, was contained in the Promise Zone. The Promise Zone is depicted in Figure 1a as the area outlined in Red. In Figure 1b, which zooms in on this area, Mantua is comprised of tracts 108 and 109. One of the requirements for the designation of the West Philadelphia Promise Zone was the presence of some form of preexisting place-based program. In this case, an active grant from the BCJI qualified the area. In addition, Promise Zones must be[5] contiguous, contain between 10,000 and 200,000 residents, and have a poverty rate exceeding 32.5%. The area chosen experienced a poverty rate around 50%, high crime rates, and low rates of educational attainment, and it contained numerous abandoned homes. Each city was responsible for outlining the goals of its promise zone.[6] Philadelphia's zone was implemented to improve education, create jobs, stimulate the local economy, and reduce violent crime. Zones do not receive extra funding outright, but the governmental and

[5] https://www.hud.gov/sites/documents/PZ_R3_APP_GUIDE_URBAN.PDF

[6] The City of Philadelphia states that the designation was created to "ensure that the ZIP code a person is born in does not determine their future." https://www.phila.gov/programs/west-philadelphia-promise-zone/

nongovernmental organizations within them receive fast-tracked approval for federal grants (Stoker and Rich, 2020). Ultimately, Promise Zones receive more federal grant funding than comparable areas that are not Promise Zone designees.



(a) Philadelphia's Census Tracts, with the Promise Zone border in red.

(b) The Promise Zone, zoomed in with tract numbers.

Figure 1: Philadelphia and the Promise Zone

Promise Zones differ from Empowerment Zones, Enterprise Zones, and Opportunity Zones. Instead of focusing on spurring outside business investment, the Promise Zone program coordinates federal grant money for a wide variety of urban-renewal programs, many of which focus on improving schools and opportunities for youths. Philadelphia's Promise Zone designation expanded the BCJI's strategies, programs, and initiatives in Mantua[7] to the rest of West Philadelphia while also establishing new ones. This expansion along with the preferential treatment federal grants gave to community organizations in West Philadelphia turned the BCJI efforts into a much more substantial force in Philadelphia. To facilitate the goals of the Promise Zone, the Office of Community Empowerment and Opportunity was established in 2014. Under the administration of that office, each initiative of the zone was spearheaded by a specialized organization (of which there are approximately 30). These include community-development corporations such as Mount Vernon Manor, the Local Initiative Support Corporation, and People's Emergency Center; public institutions such as the Philadelphia Housing Authority, the police department, and department of commerce; and

[7]For reference, Mantua houses approximately 6,000 residents, while the area spanned by the Promise Zone houses over 30,000 residents.

universities such as Drexel University and the University of Pennsylvania.[8]

The grants supported a wide range of programs: programs matching employees to employers, job-training programs for residents, antirecidivism programs targeted at adjudicated youth, better preschool programs, expanded educational support, and more. They intended to reduce violent crime by implementing community-oriented policing strategies, removing blight, and maintaining vacant lots. They also had some less specifically outlined goals, such as reducing poverty and encouraging healthy eating.[9] The primary goal of this paper is to study the designation's effect on various types of crime.

Unlike police-patrol interventions that begin at time T and can be clearly measured, the plethora of programs that were funded by grants within the Promise Zone and that sought to reduce violent crime began at various times. While the Promise Zone was designated and officially began in January 2014, it took time for the grants to be allocated and programs to be created. For example, the Face Forward 2 program, a public-safety treatment that provides diversion programs for hundreds of 14- to 24-year-old delinquents living in the Promise Zone, did not begin operating until January 2015.

Over \$3 million in grants were dedicated specifically to crime-reduction social interventions. For the Training to Work and Face Forward grants, hundreds of offenders and at-risk youth were treated directly with interventions. These grants were among the first to be disbursed. While this is a small number of people in absolute terms, it is large relative to the offending population and the population of the zone. Interventions such as these should reduce crime rates.

Much of the crime literature argues that making an outside option more desirable than criminal activity makes people less likely to be involved in crime. This is also supported by economic theory since the Promise Zone programs make youth and adult offenders who are released from detention more desirable job candidates and give them extracurricular activities that make them less likely to return to crime. In a meta-analysis of youth diversion programs (one of the treatments included in the Promise Zone grants), Wilson and Hoge (2013) find that these programs are effective in reducing recidivism. More broadly, I expect that the zone itself, by establishing an initiative to

[8] For example, Drexel University coordinates the "improved education" goal, while Mount Vernon Manor works with the Philadelphia Police Department to improve public safety and the Housing Authority to improve housing access.

[9] https://www.hudexchange.info/sites/onecpd/assets/File/Promise-Zones-Designee-West-Philadelphia.pdf

induce more community cooperation with police, would also reduce violent crime by helping to catch more criminals outside the groups treated directly with grant-based programs; there could be potential spillover effects among individuals within the zone. While this mechanism is difficult to isolate, the direction of the effect should be a reduction in crime captured in an analysis of the overall effect of the program.

3 Related Literature

Becker (1968) theorizes that crime is incentivized by expected reward to the criminal and disincentivized by a higher risk of the criminal being caught or a greater sanction if caught. Freeman (1999) expands on this theory by arguing that, while sanctions for a crime (such as imprisonment) may disincentivize crime, new criminals simply replace imprisoned criminals in the market for crime. He offers economic opportunity as an alternative to sanctions. If individuals have a better outside option, primarily in the form of gainful employment, they will have less need to commit crimes.

That crime occurs more often in cities than elsewhere is often taken for granted, and it is also a well-studied empirical fact. Glaeser and Sacerdote (1999) dig into the factors behind this phenomenon, focusing primarily on the Becker model but adding external factors such as female-headed households.[10] In line with Becker, they find that approximately one-quarter of crime variation can be explained by higher possible reward due to more concentrated wealth in cities. Moving beyond Becker's model, they claim that one-third to one-half of the additional crime in urban areas can be explained by the more concentrated presence of female-headed households.

Regarding place-based policies within urban settings, there is some debate in the literature around their effectiveness. Well-known federally guided place-based policies include Empowerment Zones, Enterprise Communities, and Renewal Communities—all of which were designated from 1993 to 2000—and, more recently, Opportunity Zones, designated in 2017. For the first three, their overall effectiveness is generally considered minimal and their cost-effectiveness is often called into question. For example, Glaeser and Gottlieb (2008) study the federal government's use of localized policy to assist specific regions and neighborhoods through the Empowerment Zone program. This program created eight urban zones across the country that provided tax and regulatory waivers

[10] There is a near 1-to-1 relationship between female-headed and "single-parent, no father" household. They essentially proxy using data that describes the sex of the head of household, not the number of parents.

to firms along with block grants for infrastructure spending; though it helped obtain place-based grants, this program was quite different from the Promise Zone program. The authors find that Empowerment Zone neighborhoods experienced a small reduction in poverty and unemployment. These areas also experienced a mild increase in housing prices and rents. But the authors calculate that the program, which cost $3 billion, only increased economic output by $1 billion. Thus, Glaeser and Gottlieb (2008) argue that Empowerment Zones are cost-inefficient.

In contrast, Busso et al. (2013) find, in their welfare analysis of the first round of Empowerment Zone grants, that they created approximately $750 million in value while only costing $400 million over the period studied. They also document reductions in poverty and unemployment rates, similarly to Glaeser and Gottlieb (2008), and ultimately argue that Empowerment Zones were modestly cost-efficient. Reynolds and Rohlin (2015) examine heterogeneous effects across the household income distribution, finding that Empowerment Zones do little to help low-income residents while potentially benefiting high-income residents.

Enterprise Zones, which focus specifically on spurring business, are praised as cost-efficient in Glaeser and Gottlieb (2008), but criticized in more recent literature. Neumark and Young (2019), in a review of the literature on Enterprise Zones, conclude that they do very little to improve employment or income for individuals living in poor neighborhoods. A common thread throughout the literature regarding the aforementioned place-based policies (which focus on incentivizing outside capital investment into areas) is that, even if they create some economic surplus, they do little to benefit the low-income residents of these areas (Reynolds and Rohlin, 2015; Neumark and Young, 2019).

Austin et al. (2018)'s study represents a major departure from some of the earlier literature regarding place-based policies. The authors find, in regard to place-based policies aiming at increasing economic opportunities, programs tailored to specific locations are more effective than large-scale transfers that do not account for local circumstances. This is particularly relevant regarding Promise Zones, since they involve not a general transfer of funds (for example, more funding for police or more money for schools across the board) but funding for specific programs facilitated and crafted by and in collaboration from locals, taking into account the culture and circumstances of the neighborhoods in the zones.

Since the Philadelphia Promise Zone seeks to coordinate public, private, and nonprofit organizations with the ultimate goal of improving a disadvantaged urban area, it best fits the definition of *neighborhood-renewal program* provided by Alonso et al. (2019).[11] Alonso et al. (2019) examine the effect of England's Neighborhood Renewal Fund (NRF) on violent crime. Examining 345 localities from 2000 to 2007, they study the effect of fund resources that were distributed to 81 of those areas. Their main empirical strategy involves a two-way fixed-effects difference-in-differences approach examining how yearly crime rates are affected after funding is made available to these areas. Similar to the Promise Zone, the NRF involved partnerships among local governments and community organizations in applying renewal interventions using UK government funds. They find that the binary effect of receiving funding reduced burglary by 13%, robbery by 24%[12], and violence by 13%. Renewal of vacant lots and removal of blight reduce violent crime as well, according to Branas et al. (2018), and Paredes and Skidmore (2017) find that removing dilapidated housing raises nearby property values. That said, it is not clear from the master list of grants that any grants specifically focused on removing blight or dilapidated buildings, even though this goal was laid out explicitly by the facilitators of the Promise Zone.

Kitchens and Wallace (2022) examine the Los Angeles Promise Zone and its effect on local housing prices. The authors find that the Los Angeles Promise Zone caused property value to increase by 6-11 percent[13], or about $50,000 on average. They explore potential mechanisms but do not isolate a specific mechanism through which property values increase. They find no significant change in building-permitting, crime, or educational outcomes.

To my knowledge, the only study that addresses the Philadelphia Promise Zone directly is Stokes (2020), which is part of a special report studying the Byrne Criminal Justice Innovation as a whole. This paper provides in-depth background to the BCJI, Mantua, the Promise Zone, and the interactions among the three. It also provides a descriptive analysis of changes in rates of crime in various hot spots across Mantua. In general, I find that through the BCJI, mostly after the Promise Zone took effect, Mantua moved from a high-crime neighborhood toward a more average Philadelphia neighborhood. When we consider that Promise Zone programs are

[11] "Neighborhood renewal programs are place-based interventions for the regeneration of distressed urban areas" Alonso et al. (2019).

[12] In my paper, I categorize robbery as a violent crime, since unlike burglary, it involves a direct confrontation with the victim. Additionally, the FBI categories robbery as violent. See `https://ucr.fbi.gov/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/violent-crime/violent-crime`.

[13] Using matching, they find a more modest effect of 3-5 percent. Interestingly enough, matching increases the absolute value of my points estimates.

implemented differently from city to city, it is clear that this research supplements Stokes (2020) by using inferential techniques and serves as a complement to Kitchens and Wallace (2022) in a (hopefully) growing literature around the effects of the federal Promise Zone program.

4 Data and Methodology

4.1 Data

The entire area studied, including the boundaries of the Promise Zone, were obtained as a shapefile for all Philadelphia census tracts. Census tracts with fewer than 100 residents at any point during the period studied are excluded. The Promise Zone area is bounded by the Schuylkill River to the east, Girard Avenue to the north, 48th Street to the west, and Sansom Street to the south. Using its establishment in 2014, I study the effect of these changes on violent crimes in the West Philadelphia Promise Zone. An "OpenStreetMap" view of the zone is displayed in Figure 2.

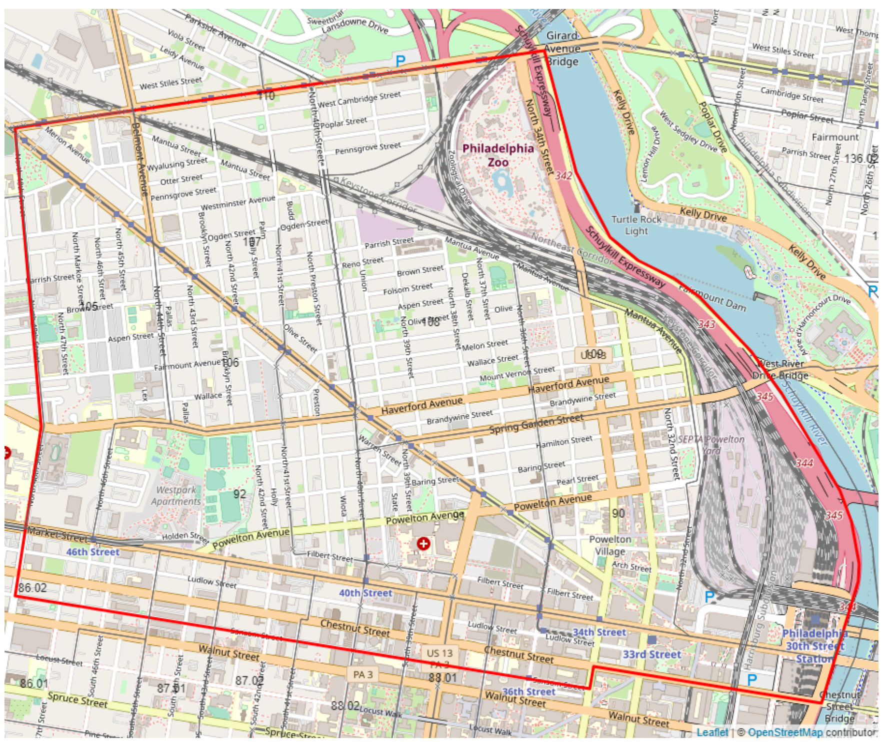


Figure 2: OpenStreetMap view of the Promise Zone.

Crime data for Philadelphia from January 2010 to December 2022 were obtained from the

OpenDataPhilly[14] tool provided by the City of Philadelphia. This API provides access to a data set (updated in real time) that tracks all crime incidents reported by the police. The data set includes the type of crime committed, exact location by coordinates, and exact time down to the minute. This allows for a robust understanding of exactly when and where any given crime occurred. Shapefiles of the City of Philadelphia's census tracts were also acquired, allowing me to map crime occurrences to census tracts.

4.1.1 Choice of Control Variables

For yearly demographic controls from the census for units with fewer than 65,000 residents, block-level data are unavailable but both block-group and tract-level five-year American Community Survey (ACS) estimates are available. Tract-level estimates have substantially less sampling error than block-group-level estimates, so I choose to use tract-level controls and aggregation. To understand the incidence of crime, population estimates must be as accurate as possible. Annual population estimates and demographic information from the US Census Bureau were obtained through the ACS at the census-tract level for 2010 to 2022. This paper uses the ACS five-year estimates from the Census Bureau[15] of racial composition (percent Black, White, and Hispanic); proportion of tract population 25 or older who have not completed high school, completed high school or a GED, completed some college, or completed a bachelor's degree; proportion of children under 18 living in a single-mother household; proportion of the tract population that are boys or men 15 to 29; proportion of tract population 16 to 64 who have not worked in the past 12 months; and real per capita income. Overall, the population of the Promise Zone grew faster than the rest of Philadelphia during the period studied, as shown in Figure 3.

Higher levels of educational attainment, particularly high school completion for boys and men,[16] are found to be causally linked to lower levels of violent crime (Lochner and Moretti, 2004; Lochner, 2020). Machin et al. (2011) find that more education substantially reduces property crime. Improving educational quality and attainment is a key goal of Promise Zones, and it determines where much of the funding goes. One discrepancy between the Promise Zones' educational targets and

[14] https://data.phila.gov/visualizations/crime-incidents

[15] According to https://www.census.gov/programs-surveys/acs/guidance/estimates.html, the 5-year estimates are more reliable than the 1-year or 3-year estimates. These estimates are recommended by the Census Bureau for performing research at the tract level.

[16] This is a key finding of Lochner and Moretti (2004).

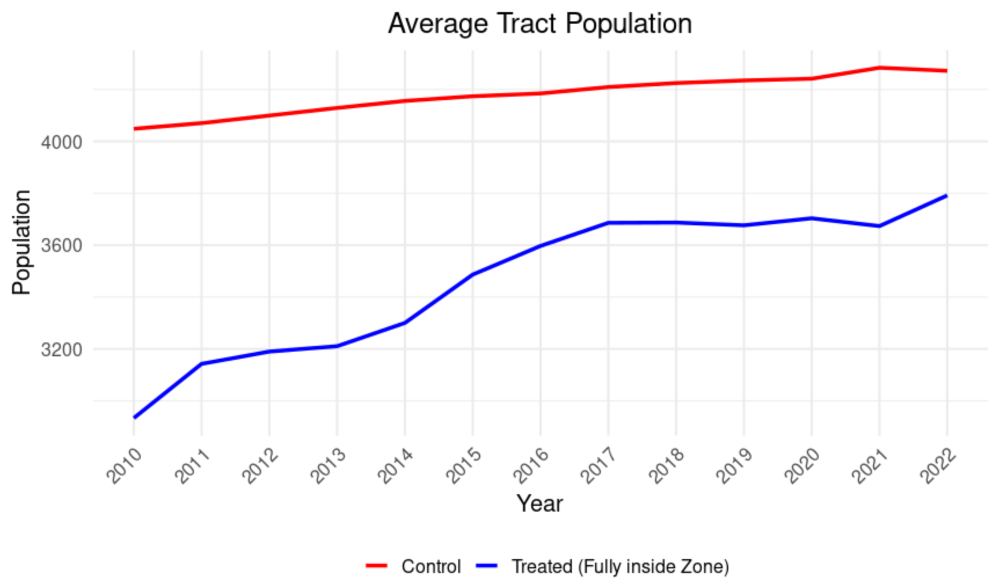


Figure 3: Population Growth

the way the ACS measures educational attainment is that grade schoolers and high schoolers are targeted by the Promise Zones' educational grants, while the ACS measures educational attainment for individuals 25 and older. Therefore, the effect of educational grants disbursed throughout the zone on educational attainment as measured by the ACS will not be observed. However, the Face Forward 2 grant and the Training to Work 1 grant, which are considered public-safety grants because they target youths and adults currently being adjudicated for crimes, fund GED and college-preparation services for adults and soon-to-be adults. For this reason, an improvement in educational attainment may partially mediate the zone's effect on violent crime.

Growing up in a single-mother household[17] is associated with higher youth involvement in crime (Glaeser and Sacerdote, 1999). For that reason, I include the percentage of children under 18 living in a female-headed household with no father[18] present. According to the FBI, young men commit the majority of violent crime (Ulmer and Steffensmeier, 2014). A change in the share of population that are 15- to 29-year-old boys and men might not be picked up by fixed effects if some exogenous change occurs at the tract level. Neither single-mother households nor presence of young men are confounders, since neither one should have affected the choice of whether to establish a Promise

[17]More specifically, Glaeser and Sacerdote (1999) claim that higher concentrations of female-headed households in cities can help explain much of the crime differential between urban and non-urban areas.
[18]Literally "no husband present" in the ACS.

Zone. Failing to control for them does not open a backdoor path (as described in Cunningham (2021)) but it still reduces the precision of my estimate. If the share of children living in single-mother households or the share of the population that are young men changes in a tract-specific way (uncaptured by year fixed effects), then it could bias the coefficient of the treatment on violent crime. Race is similar in this regard, as it did not have any direct causal link to the establishment of the zone but could affect criminality through a mediator variable such as income or education. I include it as well to increase the precision of the estimate. Lack of employment can be another causal factor behind committing violent crimes. I calculate the share of the population 16 to 64 that has not worked in the last 12 months as a measure of long-term joblessness.

For the purpose of this paper, the millions of observations in the longitudinal crime data were grouped as 4,849 tract-year observations. While there are 384 census tracts in Philadelphia, 11 census tracts that had fewer than 100 residents for at least one year were removed, leaving 373 tracts. Summary statistics for the data are displayed in Table 1. It is clear from the table that the Promise Zone area had a higher crime and lower income rate than the rest of Philadelphia in the pre-treatment period, and experienced a steeper violent crime decline than the rest of Philadelphia in the post-period. The difference in the differences for violent crime here (with no covariates or fixed effects) is -5.25.

4.2 Methodology

4.2.1 Difference-in-Differences of tract-level crime rates

I employ a difference-in-differences model of the following form:

$$Crime_{it} = \gamma_i + \sigma_t + \beta_1 * [Promise]_{it} + A_{it} + e_{it}, \tag{1}$$

where i indexes the tract and t indexes the year. $Crime_{it}$ represents the crime rate per thousand residents in tract i in year t. These crimes (measured separately) include all violent crimes, assaults, aggravated assaults, aggravated assaults with firearms, robberies, robberies with firearms, homicides, all non-violent crimes, and all property crimes. $[Promise]$ indicates if a tract is lying fully within the Promise Zone and the year is 2014 or later. γ_i represents a vector of tract-level

Table 1: Means for treated and control tracts before and after treatment.

Panel A: Outcomes

Variable	Control Pre	Control Post	Treated Pre	Treated Post
Violent Crimes (per 1000)	28.58	25.44	37.18	28.79
Assaults (per 1,000)	17.12	16.06	20.31	16.56
Agg. Assaults (per 1,000)	4.21	3.58	6.12	4.73
Agg. Assaults w/ Firearm (per 1,000)	1.56	1.78	2.53	2.35
Robberies (per 1,000)	3.24	2.19	4.54	2.68
Robberies w/ Firearm (per 1,000)	2.28	1.64	3.41	2.19
Homicides (per 1,000)	0.16	0.19	0.27	0.28
Non-Violent Crimes (per 1,000)	90.93	68.46	134.58	114.35
Property Crimes (per 1,000)	40.61	33.29	42.31	33.93

Panel B: Covariates

Variable	Control Pre	Control Post	Treated Pre	Treated Post
Proportion Black	0.43	0.42	0.73	0.68
Proportion White	0.43	0.41	0.18	0.19
Proportion Hispanic	0.11	0.13	0.03	0.03
Proportion No Father in Household	0.44	0.42	0.57	0.61
Proportion Males Aged 15-29	0.12	0.12	0.19	0.20
Proportion Less than High School	0.20	0.16	0.23	0.18
Proportion High School Graduate	0.34	0.32	0.31	0.30
Proportion Some College	0.22	0.22	0.25	0.26
Proportion Bachelor's Degree	0.14	0.17	0.10	0.15
Per Capita Income	$23,771$	$29,563$	$13,565$	$15,275$
Population	$4,087$	$4,220$	$3,119$	$3,622$

Note: All variables are measured at the tract-year level with 373 tracts (8 of which are treated) examined over the span of 13 years (9 of which occur in the post-period). The total sample size is N = 4849.

fixed effects and σ_t represents a vector of year fixed effects. e_{it} represents the error term. A_{it} is a matrix of covariates, namely the five-year ACS estimates discussed in Section 4.1, which are measured at the tract-year level. These variables include the percent of the population that are white, percent that are Black, percent that are Hispanic, percent of children under 18 who live in a household headed by a single mother, percent who are boys or men 15 to 29, percent with various levels of highest educational attainment (no high school diploma, high school or GED, some college, associate's degree, bachelor's degree, and the reference category of graduate degree), percent who are 16 to 64 and who have not worked in the past 12 months, and income per capita.

Figure 4 shows the average number of violent crimes per year per 1,000 residents within tracts in and outside the Promise Zone. This figure suggests a downward trend in violent crime in the pre-period; the trend flattens out in most of Philadelphia but continues to fall sharply in the Promise Zone until 2019, when it rises across the city. The zone maintains a similar violent crime rate to the rest of the city from 2018 to 2022.

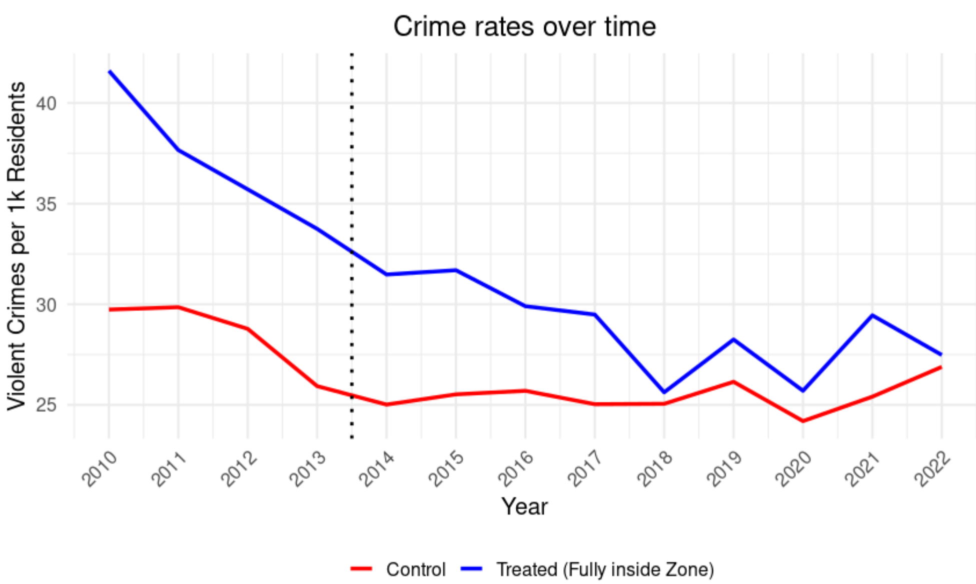


Figure 4: Violent Crime Trends

Synthetic control is particularly useful as an alternative to difference-in-differences in this case since graphically it looks like the zone area was already experiencing a steeper pre-treatment decline in crime. I argue that violent crime would have stopped trending downward and possibly trended upward had the Promise Zone not been established in 2014. One benefit of synthetic control is that it works well in situations in which the number of treated units is small in comparison to the number of untreated units; as mentioned, it does not require parallel trends to exist naturally in the data since it reweights units to match on pretreatment trends and levels (Abadie and Gardeazabal, 2003; Abadie et al., 2015; Arkhangelsky et al., 2021).

Arkhangelsky et al. (2021) develop an estimator that they demonstrate performs as well as or better than difference-in-differences and classic synthetic control (from Abadie and Gardeazabal (2003)) in settings in which one or the other would normally be appropriate. In a typical synthetic control, statistical software estimates unit weights (generally along with covariate and pretreatment outcome weights) using pretreatment data to simulate the treated unit's outcome variable (and covariates if applicable) during its pre-period and post-period using this weighted combination of control units. This imposes parallel trends in the pre-period econometrically; ideally, the synthetic control unit has the same trend and level[19] as the actual treated unit. Synthetic difference-in-differences introduces time weights in addition to the unit weights normally used, thus giving more weight to units that are historically similar to the treated unit. In contrast to synthetic control, synthetic difference-in-differences imposes parallel trends but not identical levels. Since the control unit's outcome begins at a different level from the treated unit's outcome (similar to a traditional difference-in-differences setting) the ATT (average treatment effect on the treated) point estimate is often more conservative than both synthetic control and difference-in-differences. I implement this estimator using the *synthdid* package in Rstudio. For the inclusion of covariates, I use the *xsynthdid* package from Kranz (2021).

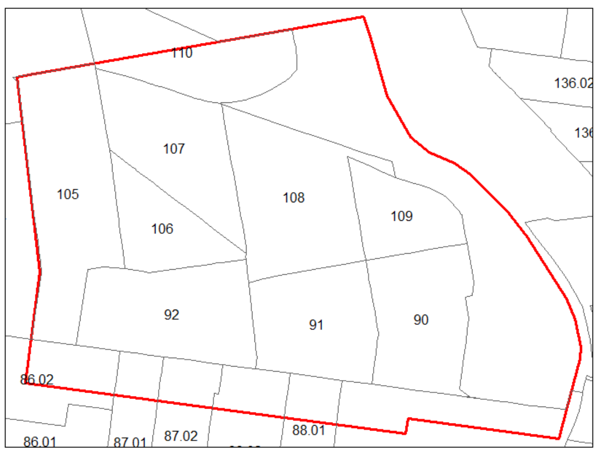
4.2.2 Block-Level Synthetic Control of crime incidents

As an alternative method of disaggregating the potential crime-reducing effect of the Zone, I employed the Synthetic Control Method for Microdata to offer an alternative counter-factual to the Zone had the treatment not occurred. This analysis operates at a finer level of aggregation and studies crime incident frequency as opposed to population-adjusted rates.

I apply the exact methodology of Robbins and Davenport (2021) to apply a block-level analysis of crime incidents. One benefit of this technique is that it can result in a perfect pre-treatment

[19] This is something Kahn-Lang and Lang (2020) note can strengthen classic DiD approaches if a suitable control group exists.

match between treated and synthetic, which is unlikely in most other synthetic control applications. Another major benefit in terms of proper effect identification is that street blocks, given their small size, provide a much more accurate delineation between what lies within the Promise Zone and what lies outside. This is clear from Figure 5, which contrasts the census tracts with the census blocks in relation to the Promise Zone border.



(a) The Promise Zone, zoomed in with tract numbers.

(b) The Promise Zone, zoomed in with blocks.

Figure 5: The Promise Zone area represented by tracts or by blocks

Block-level micro-data is only available in the decennial census, which I acquired using the *Tidycensus* package in R. Robbins and Davenport (2021) use cross-sectional decennial census data available in the “SeattleDMI” dataset.[20] There are 18,872 census blocks in Philadelphia, over 2000 of which had no crime occurrences (of any type) during the period studied. These blocks were removed[21] leaving 16,578, 324 of which are in the Promise Zone. I use the same time-invariant covariates as Robbins and Davenport (2021), which are block-level total population, Black population, Hispanic population, number of households, number of owner-occupied family households, number of female-headed households with no husband present, number of renter-occupied households, number of vacant houses, and number of males 15 to 21. I then merged this data with all

[20]Note that the dataset I build is essentially the same as the SeattleDMI dataset, except for Philadelphia. It is the combination of crime panel data and a cross-section of block demographic data.

[21]Even though I am examining violent crime, there are blocks where no offenses, including minor non-violent ones going back to 2006, never occurred a single time. These blocks are likely devoid of activity and their removal should not bias my synthetic control. On the other hand, I choose not to exclude blocks with zero population, as these blocks could still have corner stores, gas stations, and other points of interest that are subjected to crime.

crime occurrences from 2010 to 2022, which I aggregated at the quarterly level, once again following the methodology of Robbins and Davenport (2021).[22] Similar to the authors, I use every available outcome variable for my analysis. This includes a measure of all violent crimes, simple assaults, aggravated assault, aggravated assault with a firearm, robbery, robbery with a firearm, homicide, all non-violent crimes, and all property crimes.

5 Results

5.1 Difference-in-Differences

Results of both the difference-in-differences with two-way fixed effects and synthetic difference-in-differences (both with and without covariates) are displayed in Table 2. Standard errors are calculated using the bootstrap method from Arkhangelsky et al. (2021) with 500 replications. The preferred estimate in Column 4 suggests that 4.19 fewer violent crimes per thousand tract residents per year are attributable to the Promise Zone. Considering that there are eight tracts examined over nine years, with an average post-treatment population of 3600, this constitutes approximately 1000 fewer violent crimes over the post-treatment period. Of this, the majority of the reduction comes from a reduction in simple assaults: 3.07 fewer per thousand tract residents per year.

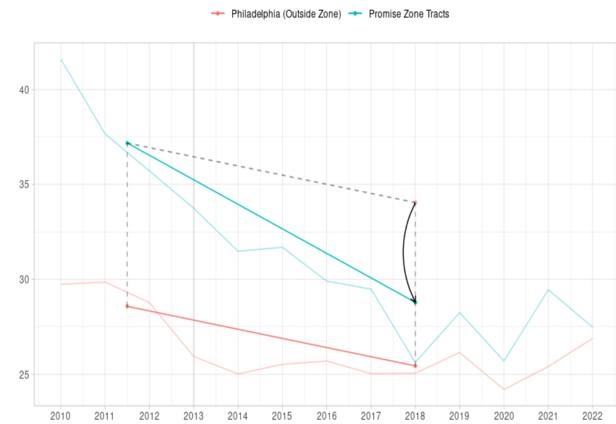
The synthetic difference-in-differences estimator with covariates should be a more accurate measurement of the effect. The method in Arkhangelsky et al. (2021) is focused primarily on cases without time-varying covariates. Kranz (2021) demonstrates that the method for covariate implementation suggested by Arkhangelsky et al. (2021) might not provide consistent estimates when covariates are correlated with both time period and group. Certain demographic characteristics are systematically different in the Promise Zone compared to the rest of Philadelphia and these characteristics change from year to year. The plots for violent crime are displayed in Figure 6. The plots display a parallel pre-trend (in the synthetic difference-in-differences plots) and the average treatment effect is represented by the black arrow. In addition, the red shaded area on the bottom left of the plot represents the time weights (the λ term as described in Arkhangelsky et al. (2021)).

[22] The authors suggest that if the treatment is expected to not have immediate effects, the final "pre-treatment" period should be the period the treatment begins. Therefore, the 17th quarter (Q1 2014) is the final pre-treatment period in my code.

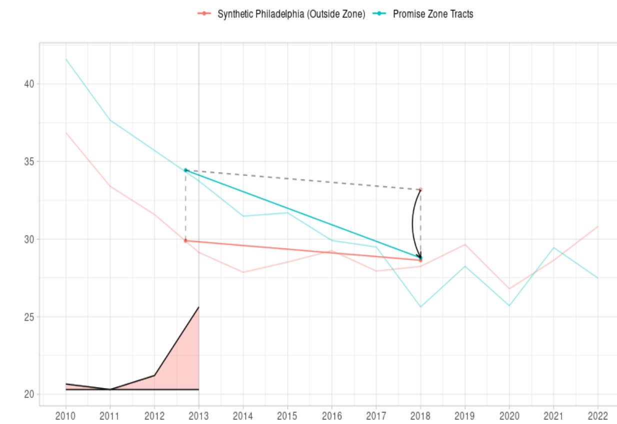
Table 2: Effects of Promise Zone Implementation on Tract Level Violent Crime

	DID	SDID	DID w/ cov	SDID w/ cov
	(1)	(2)	(3)	(4)
Violent Crime				
ATT	-5.26**	-4.40*	-5.23***	-4.19**
Standard Error	(2.24)	(2.37)	(1.77)	(1.89)
Simple/Other Assault				
ATT	-2.70**	-3.10**	-2.78**	-3.07**
Standard Error	(1.26)	(1.56)	(1.38)	(1.42)
Aggravated Assault				
ATT	-0.76**	0.20	-0.75***	0.20
Standard Error	(0.36)	(0.43)	(0.25)	(0.38)
Aggravated Assault w/ Firearm				
ATT	-0.40	-0.51	-0.57	-0.65*
Standard Error	(0.37)	(0.42)	(0.44)	(0.37)
Robbery				
ATT	-0.80	0.13	-0.64	0.24
Standard Error	(0.57)	(0.68)	(0.59)	(0.52)
Robbery w/ Firearm				
ATT	-0.58*	-0.48	-0.46	-0.39
Standard Error	(0.32)	(0.36)	(0.29)	(0.28)
Homicide				
ATT	-0.02	0.01	-0.04	-0.01
Standard Error	(0.11)	(0.12)	(0.12)	(0.10)
Non-violent				
ATT	2.24	9.57	5.72	12.93
Standard Error	(9.34)	(12.72)	(19.31)	(11.11)
Property				
ATT	-1.10	1.78	-1.70	2.33
Standard Error	(3.02)	(3.70)	(2.63)	(3.18)

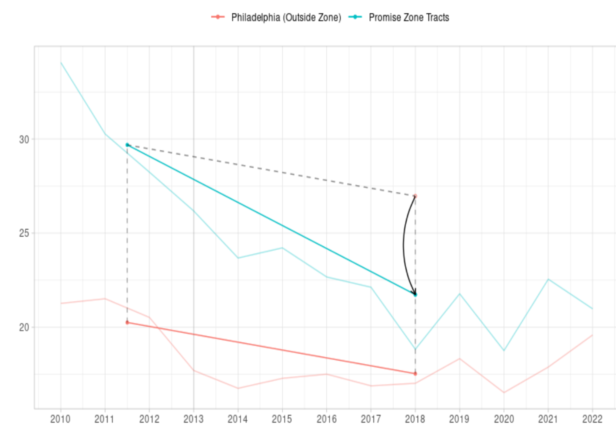
Notes: All standard errors are calculated using the cluster bootstrap method with 500 replications. N = 4849 for all specifications. Significance levels are reported as *** = p<0.01, ** = p<0.05, * = p<0.1.



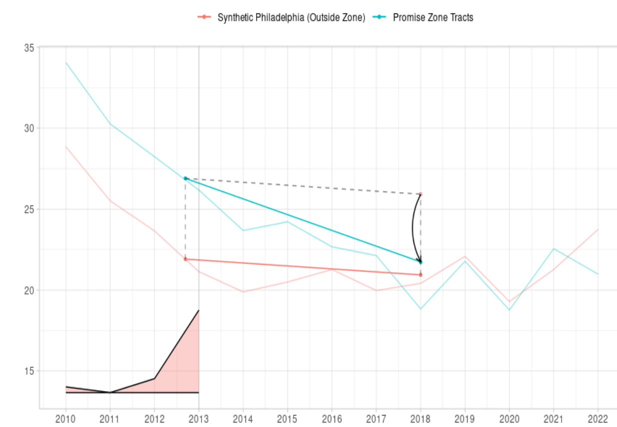
(a) Difference in Differences



(b) Synthetic Difference in Differences



(c) Difference in Differences w/ Covariates



(d) Synthetic Difference in Differences w/ Covariates

Figure 6: Result plots for Violent Crime

5.2 Block-Level Synthetic Control

The results for the block-level synthetic control, which are tabulated in Table 3 suggest a statistically significant reduction in violent crime occurrences (8%), mostly attributable to a 12.8% reduction in occurrences of simple/other assaults. In the OpenDataPhilly API, “other assaults” is any assault that is not aggravated. The simple/other assaults variable is significant at the 1% level for both the linear and jackknife p-values. However, jackknife inference methods are considered more robust and trustworthy in this context. It should be noted that this methodology does not account for changes in population, and takes the demographic covariates as time-invariant (which is required for the package according to the authors and is used solely for providing a strong pre-treatment match).

This may result in an understatement of effect if population is increasing, and an overstatement of the effect if population is decreasing.

While yearly population estimates are unavailable for blocks, the tract-level ACS 5-year estimates show an increase in Zone population throughout the period studied. This is especially important when noting the significant increase in non-violent crime occurrences attributable to the zone as demonstrated by the synthetic control. More people and more economic activity in an area leads to more incidents of non-violent crime, which includes not only property crime but “all other offenses”, the most common crime type reported in the database. “All other offenses” includes minor crimes that are neither Part 1 nor Part 2, but excludes traffic offenses. Non-violent offenses that are not property-related are minor and not particularly costly to society, such as loitering. In the population-adjusted synthetic difference-in-differences, there is no effect on non-violent crime. Plots for these results are in the Appendix in Figures A.1, A.2, and A.3.

Table 3: Effect Estimate of the Promise Zone using Block-Level Synthetic Control

	Trt	Con	Pct.Chng	Linear.PVal	Jack.PVal	LCL	UCL
Violent	8451	9185.12	-8.0%	0.0000	0.0052	-13.3%	-2.4%
Assault	4552	5222.05	-12.8%	0.0000	0.0001	-18.9%	-6.3%
AggAssault	1252	1265.51	-1.1%	0.7729	0.8087	-9.3%	7.9%
AggAssaultFirearm	642	708.52	-9.4%	0.0658	0.1182	-20.0%	2.6%
Robbery	773	840.89	-8.1%	0.0771	0.2266	-20.1%	5.7%
RobFirearm	581	584.50	-0.6%	0.9004	0.9205	-11.6%	11.8%
Homicide	76	59.59	27.5%	0.0834	0.1149	-4.2%	69.9%
NonViolent	29474	25726.10	14.6%	0.0000	0.0098	4.0%	26.2%
Property	11271	10850.10	3.9%	0.0857	0.4441	-5.8%	14.5%

To understand the Average Treatment Effect on Treated (ATT), *Microsynth* calculates the cumulative number of cases between the treated and synthetic control groups as shown in Table 3. Since the match is perfect in the pre-treatment period, the difference between the *Trt* and *Con* columns indicates the number of incidents of crime potentially prevented by the zone. From 2014 to the end of 2022, 734 violent crimes are estimated to have been prevented. Of those 734, 670 were simple assaults. It is important to understand these estimates in a slightly different context than the population-adjusted tract estimates for two reasons. First, this block-level analysis more accurately identifies areas within the zone, since the tract-level analysis treats partially treated tracts in the zone as untreated. Second, this analysis does not account for the increase in population that occurred in the zone during the post-treatment period.

The estimate of 734 violent crimes prevented is of a similar magnitude to the estimates from the synthetic difference-in-differences method. Recall that the preferred estimate was -4.19. This would be approximately 1,093 fewer violent crimes when we adjust this number for the average tract population (in thousands), measured across eight tracts over nine years.

6 Discussion

6.1 Back-of-the-envelope crime cost analysis

Simple assault is defined by the FBI as an assault "where no weapon was used or no serious or aggravated injury resulted to the victim. Stalking, intimidation, coercion, and hazing are included." The Office of Justice Programs estimates that an assault with no injury as having a total cost of \$2000. Since some minor injury and/or domestic abuse (which are described as having a much higher cost) instances may be counted in the "other assaults" variable provided by the OpenDataPhilly API, this \$2000 estimate is a lower bound. Adjusted for inflation, given that the original estimate was in 1993 dollars, results in a cost of \$4218.55 in 2023 dollars. The block-level synthetic control estimates that there were 670 fewer simple assaults; this translates to \$2,826,428.50 in costs prevented. Nine years of the Promise Zone may have prevented nearly \$3 million in social costs related to violent crime victimization. This is similar to the amount of money spent on public safety grants in the Zone, but pales in comparison to the tens of millions spent on education in the area.

Using the synthetic difference-in-differences point estimate of -3.07, I find a similar number. Considering a reduction of 3.07 simple assaults per thousand residents per year, and assuming an average Promise Zone tract population of 3,622, this is 801 fewer simple assaults over nine years. Given the \$4218.55 potential cost of a simple assault, this works out to \$3,379,059 in reduced cost of crime victimization. It should be noted that simple assaults only account for 75% of the reduction in violent crime estimated by the synthetic difference-in-differences estimator.

6.2 Takeaway

The Promise Zone led to a reduction in violent crime overall, primarily through a reduction in simple assaults. This potentially increased social welfare by millions of dollars within the zone. Many advocates for the program and groups involved with its implementation reported decreased crime rates in the areas where they performed their outreach. This is in line with the results from this study. In that case, not only are these results statistically significant, they are economically significant and have implications for the city as a whole. Based on this analysis, the Mayor's Office of Community Empowerment and Opportunity and the many community groups and citizens involved seem to be meeting their goal of reducing violent crime in this pocket of West Philadelphia and making it a more livable place.

These results suggest that Promise Zones may be effective at reducing less serious types of violent crime. Since the Promise Zone program provides federal coordination and fast-tracking of grants but does not guarantee any specific set of grants, implementation of the program may vary greatly across cities. Therefore, Promise Zone programs should be studied on an individual basis to determine the strengths and weaknesses of different cities' approaches. The findings from Los Angeles in Kitchens and Wallace (2022) indicate that not all Promise Zone programs reduce violent crime. Given the violent-crime-reducing effects of the Philadelphia Promise Zone, policy makers interested in reducing violent crime could look to the Philadelphia Promise Zone's implementation for guidance.

References

Abadie, A., Diamond, A., and Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2):495–510.

Abadie, A. and Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque country. *American Economic Review*, 93(1):113–132.

Alonso, J. M., Andrews, R., and Jorda, V. (2019). Do neighbourhood renewal programs reduce crime rates? Evidence from England. *Journal of Urban Economics*, 110(C):51–69.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, 111(12):4088–4118.

Austin, B., Glaeser, E., and Summers, L. (2018). Jobs for the heartland: Place-based policies in 21st-century America. *Brookings Papers on Economic Activity*, 2018:151–255.

Becker, G. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, 76:169–217.

Branas, C. C., South, E., Kondo, M. C., Hohl, B. C., Bourgois, P., Wiebe, D. J., and MacDonald, J. M. (2018). Citywide cluster randomized trial to restore blighted vacant land and its effects on violence, crime, and fear. *Proceedings of the National Academy of Sciences*, 115(12):2946–2951.

Busso, M., Gregory, J., and Kline, P. (2013). Assessing the incidence and efficiency of a prominent place based policy. *American Economic Review*, 103(2):897–947.

Chen, J., Glaeser, E. L., and Wessel, D. (2020). The (non-)effect of opportunity zones on housing prices.

Cunningham, S. (2021). *Causal Inference: The Mixtape*. Yale University Press SN - 9780300255881.

Freedman, M., Khanna, S., and Neumark, D. (2021). Jue insight: The impacts of opportunity zones on zone residents. Working Paper 28573, National Bureau of Economic Research.

Freeman, R. B. (1999). Chapter 52: The economics of crime. *Handbook of Labor Economics*, 3:3529–3571.

Glaeser, E. and Sacerdote, B. I. (1999). Why is there more crime in cities? *Journal of Political Economy*, 107:S225 – S258.

Glaeser, E. L. and Gottlieb, J. D. (2008). The economics of place-making policies. *Brookings Papers on Economic Activity*, pages 155–239.

Kahn-Lang, A. and Lang, K. (2020). The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications. *Journal of Business & Economic Statistics*, 38(3):613–620.

Kitchens, C. and Wallace, C. T. (2022). The impact of place-based poverty relief: Evidence from the federal promise zone program. *Regional Science and Urban Economics*.

Kranz, S. (2021). Synthetic difference-in-differences with time-varying covariates.

Lochner, L. (2020). *Chapter 9 - Education and crime*, pages 109–117. Academic Press.

Lochner, L. and Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. *American Economic Review*, 94(1):155–189.

Machin, S., Marie, O., and Vujić, S. (2011). The crime reducing effect of education*. *The Economic Journal*, 121(552):463–484.

Neumark, D. and Young, T. (2019). Enterprise zones, poverty, and labor market outcomes: Resolving conflicting evidence. *Regional Science and Urban Economics*, 78(C).

Paredes, D. and Skidmore, M. (2017). The net benefit of demolishing dilapidated housing: The case of detroit. *Regional Science and Urban Economics*, 66:16–27.

Reynolds, C. L. and Rohlin, S. M. (2015). The effects of location-based tax policies on the distribution of household income: Evidence from the federal empowerment zone program. *Journal of Urban Economics*, 88:1–15.

Robbins, M. W. and Davenport, S. (2021). microsynth: Synthetic control methods for disaggregated and micro-level data in r. *Journal of Statistical Software*, 97(2):1–31.

Sage, A., Langen, M., and Van de Minne, A. (2021). Where is the opportunity in opportunity zones?

Stoker, R. P. and Rich, M. J. (2020). Obama's urban legacy: The limits of braiding and local policy coordination. *Urban Affairs Review*, 56(6):1607–1629.

Stokes, R. (2020). *Improving Community Governance to Reduce Crime: The Case of the Philadelphia's Mantua BCJI Program.* Innovations in Community-Based Crime Prevention: Case Studies and Lessons Learned. Springer International Publishing.

Ulmer, J. and Steffensmeier, D. (2014). *The age and crime relationship: Social variation, social explanations*, pages 377–396.

Wilson, H. A. and Hoge, R. D. (2013). The effect of youth diversion programs on recidivism: A meta-analytic review. *Criminal Justice and Behavior*, 40(5):497–518.

A Appendix

A.1 Additional Figures

Figure A.1: Block-level: Any violent crime and assaults.

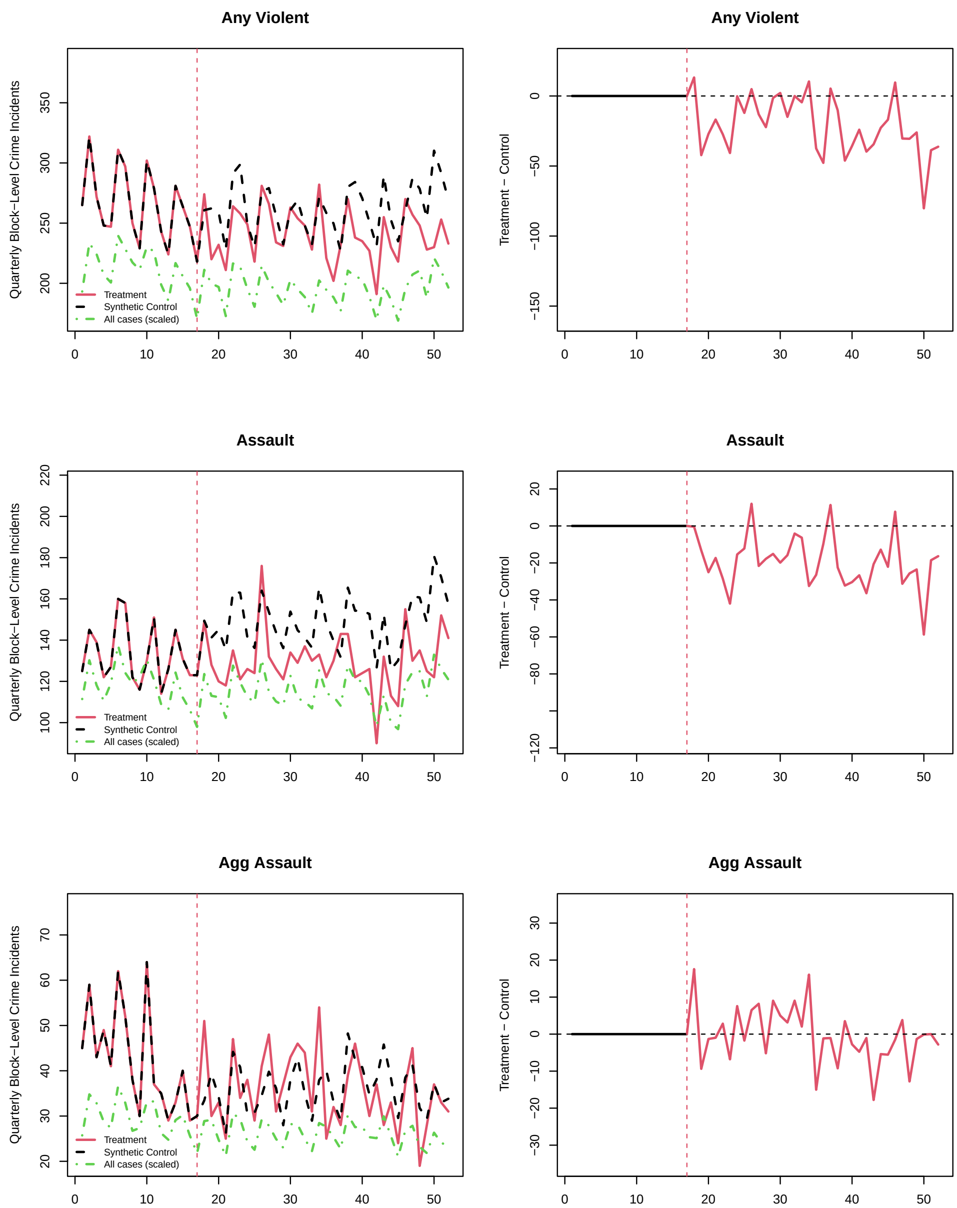


Figure A.2: Block-level: Agggravated assaults with firearms and robberies.

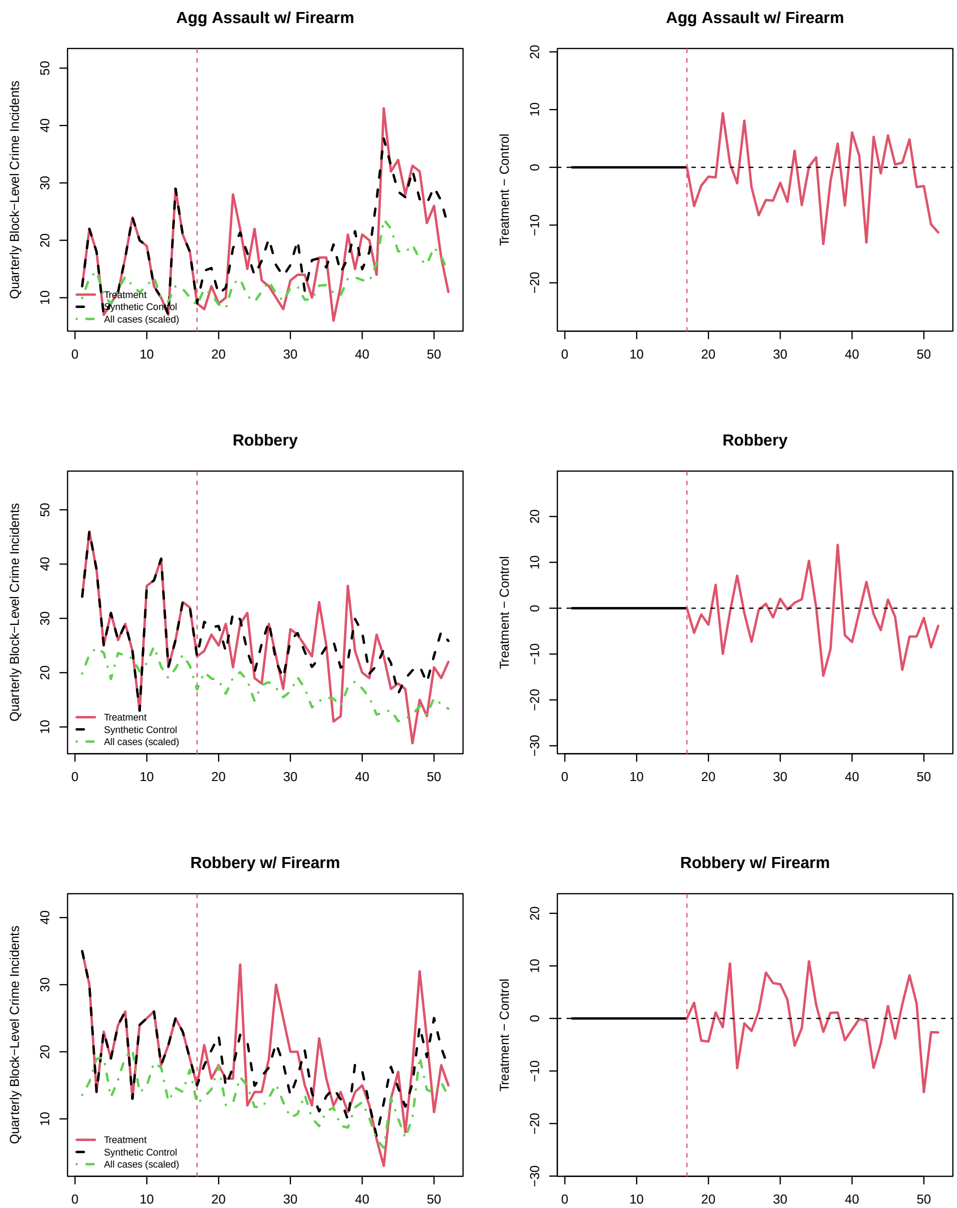
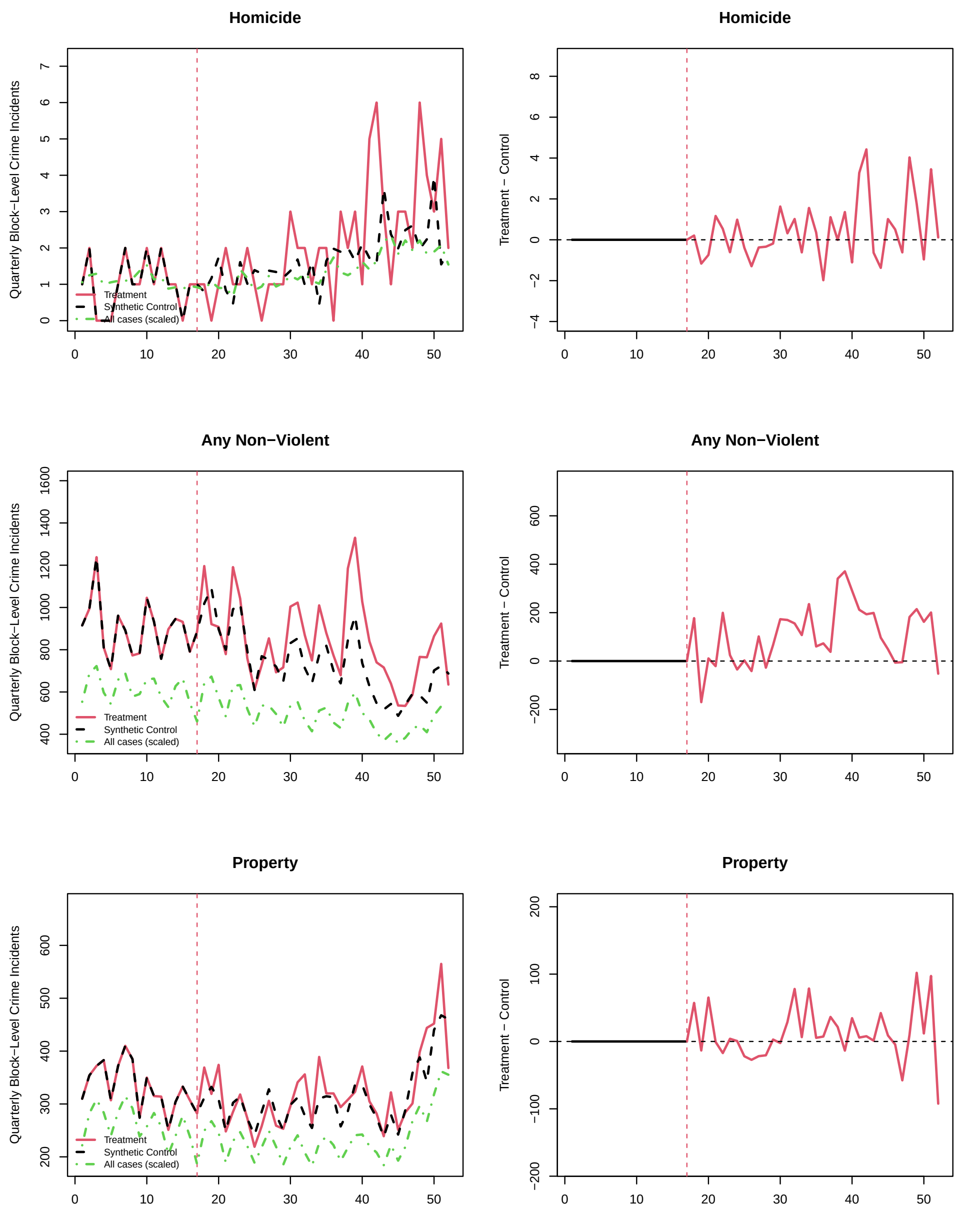


Figure A.3: Block-level: Homicide, non-violent crimes, and property crimes.



A.2 Expanded Explanation of ACS 5-year Demographic Variables

For the race variables, 'B02001_002' and 'B02001_003' estimate the number of *White* and *Black* individuals, respectively. 'B01001_001' (*Pop*) is an estimate of the total tract population.

For the *Hispanic* variable, I used 'B03002_012'. This variable is the estimate of the number of individuals who are of Hispanic or Latino origin. This is not a "racial" variable; individuals in the "White" or "Black" groups may also be Hispanic.

For the *Income* variable, I used 'B19301_001'. This variable is the estimate of the tract-level per capita income earned in the last 12 months in the given year's dollars.

For the *Male15to21* variable, I used 'B01001_006' (15 to 17yrs), 'B01001_007' (18 and 19yrs), 'B01001_008' (20yrs), and 'B01001_009' (21yrs). Each of these variables estimates the number of males of a certain age within the tract. Summing these together by tract-year provides a measure of Males 15 to 21.

For the *Single Mother* variable, I used 'B09005_005'. This variable used to be the estimate of children under 18 living in a home with a female householder who has no husband present. Recently, the language was changed from husband to spouse/partner. Regardless, this is the variable for determining the number of children living in homes with a single mother. For the "rate", I divide this by 'B09005_001', which is the number of children under 18 in the tract.

For the educational attainment variables, I used 'B06009_002' through 'B06009_06'. This variable is the estimate for the number of individuals *25 and older* categorized by their highest level of educational attainment. No high school diploma or GED, diploma or GED, some college, bachelor's degree, and graduate or professional degree. The "rate" variable for these is divided by 'B06009_001', which is the population 25 and older; this is not to be confused with 'B01001_001', which is the total population.

For individuals who have not worked in the last 12 months, I used 'B23022_025' and 'B23022_049', which are males and females, respectively, within the "universe" of 16 to 64 year olds. For the "rate" variable, I divided by 'B23022_001', which is the number of individuals 16 to 64.