Do Place-Based Crime Reduction Policies Work?: Evidence from the West Philadelphia Promise Zone*

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Abstract

This paper studies the effect of the West Philadelphia Promise Zone Initiative on weekly violent crime rates in a high-crime area of West Philadelphia, where a series of public safety and quality of life improvement grants were dispersed from 2014 to 2019. Results using a difference-in-differences analysis with tract, year, and week fixed effects along with cluster-robust and bootstrapped standard errors provides causal evidence of a reduced rate of violent crime, primarily assaults and aggravated assaults. Multiple specifications of a Synthetic Control Model predict that crime would have trended significantly upward in the Promise Zone area had the zone not been established. Dynamic difference-in-differences and propensity score matching are also employed, finding similar results and effect sizes. By the end of 2019, the Promise Zone ultimately descends to the average level of violent crime experienced across Philadelphia. A cost-benefit analysis indicates that the crime reducing effects of the Promise Zone may offset the cost of federal grant investment in public safety in the zone.

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1 Introduction

A specific, declining area in Philadelphia home to approximately 2% of the city's population incurs over $100 million annually in tangible and intangible costs[1] generated by violent crime victimization (Heaton, 2010). In 2014, this neighborhood, which contains eight full census tracts and over 30,000 residents, 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 like violent crime. Through this program, over $70 million in grant funding was dispersed from 2014 to 2019 (Stoker and Rich, 2020). This research assesses whether the zone designation and associated public spending was successful in reducing violent crime in West Philadelphia.

This paper fills a niche in the literature regarding place-based policy analysis in that there is no other published research that studies the Philadelphia Promise Zone explicitly. Previous analyses done by community groups in Philadelphia provide descriptive and correlative statistics, but do not perform their analyses using modern causal inference techniques. By releasing large sums of grant money into the communities within the Promise Zone, especially aiding in the provision of diversion programs for at-risk youth and criminal offenders returning to the community, we should observe 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 reductions in violent crime which compounds over the years. Therefore, an event study analysis will be employed in addition to difference-in-differences techniques.

2 BCJI and The Promise Zone

2.1 BCJI

In 2012, the "Byrne Criminal Justice Innovation" (BCJI) was created as part of the Obama Administration's "National Neighborhood Revitalization Initiative". Born out of the 1968 Crime Bill and

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

named after the slain officer Edward Byrne, the BCJI uses a "place-based" approach to coordinate federal monetary and technical support with local authorities to provide training and planning toward the revitalization of a neighborhood.

The BCJI, which gave a grant to the neighborhood of Mantua that ran from 2012 to 2016, is an important factor to understand in detail. This Innovation focused on data-driven policing strategies in coordination with community residents and leaders to study criminogenic areas in Mantua. For six months, Mount Vernon Manor planned and coordinated with the police department, the US Attorney's Office, and a research partner from Drexel University. Any community intervention planned had to be motivated by evidence and data 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 same data system this paper utilizes.

Specifically, the Innovation aims to develop "hot-spot" policing strategies, data-driven solutions to crime prevention, and engagement with local community leaders. The general philosophy of this program is that reductions in violent crime "require collaboration among partners in the criminal justice system, service providers, and the communities they serve."[2] This program used a competitive grant system in support of partnerships between the local government and non-profit organizations in the area. The Mount Vernon Manor Community Development Corporation (MVCDC), a group given a half-million dollar grant through the BCJI, reports a 21% reduction in Part 1 offenses from 2011-2016.[3] At one particular crime hotspot on the corner of 34th and Haverford Avenue, MVCDC reports a 65% reduction in 911 calls, and a reduction in arrests down to zero.

The programs facilitated through the BCJI program that are of direct interest to this study are: [4]

1. Crime Prevention Through Environmental Design

2. A Community-led Public Safety Committee

[2] https://bja.ojp.gov/program/byrne-criminal-justice-innovation-bcji-program/overview

[3] This non-profit organizes joint-action by the police, other local non-profits, and the community in addressing crime. See http://www.mvmcdc.org/programs/public-safety/ for more info.

[4] Much of the information in this section, including this list, were obtained from Stokes (2020) from the book "Innovations in Community-Based Crime Prevention"

3. More programs for youth, including a delinquent mentorship program
4. Community and police trust-building activities
5. Creation of a school-based youth court

Stokes (2020) provides an in-depth analysis of this program along with the Promise Zone. The author argues that the early stages of the BCJI were contentious for several reasons. First, the Philadelphia Police Department 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 set up a budget for police overtime in Mantua. Secondly, since Mount Vernon Manor was the beneficiary of the grant and the benefactor of its allocation in the area, this made things more difficult for city planners. Ultimately this resulted in a two-year long repression of trust between PPD and MVM, which was resolved when a new police captain took over 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. For this reason, I consider the "treatment" effect in Mantua (tracts 109 110) to begin in 2014 with the rest of the Zone, not 2012.[5]

2.2 The West Philadelphia Promise Zone

As of January 2014, much of West Philadelphia, including Mantua and its surrounding neighborhoods, is contained in one of these zones. One of the requirements for the designation of the West Philadelphia Promise Zone is the presence of some form of a pre-existing place-based program. In this case, an active grant from the BCJI qualified the area. The other requirement in establishing a Promise Zone was that the area had to exhibit an elevated need for federal support. The West Philadelphia area experienced a poverty rate around 50%, numerous and abundant abandoned homes, high crime rates, and low rates of educational attainment. The goals of each Promise Zone differed between cities, but were outlined specifically in each case. The City of Philadelphia states that the designation was created to "Ensure that the ZIP code a person is born in does

[5] I also include robustness checks where I consider the treatment for Mantua beginning in 2012.

not determine their future."[6] Zones do not receive extra funding outright, but the governmental and non-governmental organizations within them receive fast-tracked approval for federal grants (Stoker and Rich, 2020).

This essentially expanded the strategies, programs, and initiatives in Mantua to the rest of West Philadelphia. This expansion of location along with the preferential treatment on federal grants given to community organizations in West Philadelphia turned the BCJI efforts into a much more substantial force in Philadelphia, being able to serve more neighborhoods. To facilitate the goals of the Promise Zone, the Mayor's Office of Community Empowerment and Opportunity (CEO) was established in 2014. Administered by the CEO, the Promise Zone Initiative involves the coordination of resources toward improved local education, public safety, housing, economic opportunity, and public health. Each one of these improvements is spearheaded by a specialized organization with approximately thirty total organizations partnering with the City. These include community development corporations such as Mt. 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 universities such as Drexel University and the University of Pennsylvania. For example, Drexel University coordinates the "improved education" goal, while Mt. Vernon works with the Philadelphia Police Department to improve public safety and the Housing Authority to improve housing access. There are also a plethora of explicit goals outlined by the Mayor's office. First, they intended to create jobs by facilitating matching processes for employees to employers, incentivizing more businesses to enter the area, and providing job training programs for residents. Second, they aimed to broadly increase economic activity by incentivizing more public and private investments. They intend to improve education levels by directing funds toward better, more accessible pre-school programs. They intend to reduce violent crime by implementing "community-oriented policing strategies", removing blighted greenery, and maintaining vacant lots. They also have some less specifically outlined goals, such as reducing poverty and encouraging healthy eating.[7] Therefore, the primary goal of this paper is to study the effect of the zone on violent crime since that is the type of crime specifically outlined by the Mayor's Office.

[6] https://www.phila.gov/programs/west-philadelphia-promise-zone/

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

For reference, Mantua houses approximately 6000 residents, while the area spanned by the Promise Zone houses approximately 35,000 residents. The Promise Zone added several new community groups to the fold, including CEO, the Public Housing Authority, the parks and rec department, and others. Since the northern half of University City is included in the zone, this meant that the University City District security apparatus along with Drexel University's police department were folded into the public safety initiatives of the Byrne program.

The timing of the treatment is another issue that needs to be acknowledged. Unlike a specific police patrol intervention that begins at time T and can be clearly measured, there are many grants which began being facilitated into the Promise Zone to fund a plethora of programs that ought to reduce violent crime. While the Promise Zone was designated and "began" in January of 2014, it takes time for grants to be allocated and programs to be created. For example, the Face Forward 2 program, one of the public safety "treatments" that provides diversion programs for hundreds of 14 to 24 year old delinquents living in the Promise Zone, did not begin until January of 2015. It is also important to consider which grants would and would not theoretically affect violent crime: for example, I do not suspect that the "Action for Early Learning Grant", which provided \$4.7 Million in 2014 to support Pre-K through 3rd grade teaching programs would have any effect on crime by 2019 (though it certainly opens the door for crime research in the mid 2020s, when its age cohort will be reaching adulthood).

It is crucial to be as clear as possible in understanding what the West Philadelphia Promise Zone *is* and what it *is not*, or else any identification strategy used could be faulty. The Promise Zone is not like the Empowerment Zones, the Enterprise Zones or the Renewal Communities of the past. It is also not like the newer Opportunity Zones. To understand the mechanism through which the zone may affect the area, it is important to know the "when, what, who, and where" of grants and initiatives that are coordinated in the zone. In its first year, the Promise Zone garnered \$25 million in grants. In 2016, Drexel University, through the Promise Zone, was able to secure a \$30 million grant from the Department of Education (a sum reserved specifically for Promise Zones) for seven Promise Zone area public schools. The goal of that money, distributed over five years, is to support "cradle to career education strategies" in the area. As of 2017, the zone had garnered \$69 million in grants from eight different federal agencies.[8] Smaller, more local initiatives are also

[8] https://generocity.org/philly/2017/12/13/samantha-porter-west-philadelphia-promise-zone/

supported through the zone with sums in the tens of thousands of dollars as opposed to millions.

Upon a direct request for information, Americorps Vistas working for Philadelphia who coordinate grants through the Promise Zone kindly provided a list of all grants from 2014-2020. The full version of the tabular list as provided by the staff is available in the Appendix, along with the infographics they provided. There are several grants of interest that should affect violent crime, some of which are public safety focused, which treat crime more directly; others are education, quality of life, and economic opportunity based, which should still have an effect on the margin. According to Becker and Mulligan (1997), not only does education make people less prone to crime due to the increased opportunity cost it creates, but it also makes individuals less impatient and more risk averse. Renewal of vacant lots and removal of blight reduces violent crime as well according to Branas et al. (2018). That being said, it is not clear if there were any grants specifically focused on removing blight, even though this goal was laid out explicitly by the facilitators of the Promise Zone.

Aside from the tens of millions of dollars in grants that attempt to improve quality of life generally, (which should marginally reduce crime as well), we can observe that \$3.38 million in grants were dedicated explicitly 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. While this is a small number of people in absolute terms, it is large in scale relative to the offending population and the population of the zone. While it is difficult to tract individuals directly, one can imagine that some of the crimes (particularly assaults) recorded in the pre-treatment period were committed by individuals eventually involved in these two grant programs. Treatments such as this should have a negative effect on crime rates.

Much of the crime literature argues that making an "outside option" more desirable relative to committing crimes makes people less likely to be involved in crime. This is also supported by economic theory, since these programs make youth and adult offenders being released from detention more desirable job candidates and give them extra-curricular activities that make them less likely to return to crime. Most of these grants focus on recidivism. In a meta-analysis of Youth Diversion Programs, (one of the "treatments" included in the Promise Zone grants), Wilson and Hoge (2013) that these programs are effective in reducing recidivism. That being said, we expect that more broadly speaking, the zone itself establishing an initiative to create more community cooperation with police will also reduce violent crime by helping to better catch criminals outside the groups treated directly with grant-based programs.

With these facts in mind, it is clear that the primary limitation of this paper is that there are dozens of grants with varying monetary amounts being dispersed at different times over the 2014-2019 period. The purpose of paper, then, is not to study any individual grant or program, but to analyze an over-arching effect of the Promise Zone itself, treating its overall establishment and the many grants/programs it facilitates as a unified treatment. By using falsification tests, robustness checks, and multiple identification strategies, this paper aims to compensate for the lack of any unique isolated causal mechanism within the West Philadelphia Promise Zone.

3 Context

Becker (1968) pioneered the empirical study of crime by developing one of the first mathematical models of its determinants. He theorized intuitively that crime was incentivized by expected reward to the criminal, and disincentivized by a higher risk of being caught and/or greater sanction if caught. Freeman (1999) expands on this literature by arguing that, while sanctions for a crime (such as imprisonment) may disincentivize crime, new criminals simply replace imprisoned criminals in the "market" of crime. The alternative method of crime-reduction he offers is increased economic opportunity. If individuals have a better "outside option", primarily through gainful employment, they will have no need to commit crimes. Weisburd et al. (2006) examines whether the practice of hotspot policing simply pushes crime "around the corner". The Philadelphia Promise Zone helps facilitate hotspot policing strategies, and it is plausible that criminals would simply go elsewhere to commit crimes if there is a heightened police presence. However, the author uses a spatial econometric analysis to argue that crime not only *does not* move around the corner, but there are positive spillover effects in crime deterrence to areas outside the hotspot. The relevance of this literature cannot be understated, as the Promise Zone not only seeks to deter crime through "hotspot" policing, but also through increased economic opportunity for locals.

Crime occurring more often in cities is a fact often taken for granted in the 21st century, but it is also a well-studied empirical phenomenon. 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".[9] In line with Becker, they find that approximately a quarter of crime variation can be explained by higher possible reward due to more concentrated wealth in cities. However, they claim that at most, a fifth of crime variation can be explained by lower risk of apprehension in cities. Moving beyond Becker's model, they claim that one-third to one-half of the additional crime in cities can be explained by the more concentrated presence of "female-headed" households.

There is much debate in the literature regarding the effectiveness of place-based policies, which varies depending on the issue the policy is aiming to solve. For many types of place-based policies, the premier literature generally argues that they are ineffective in meeting their goals. Well-known federally guided place-based policies includes Empowerment Zones, Enterprise Communities, Renewal Communities, (all of which were designated from 1993 to 2000) and most recently Opportunity Zones, designated in 2017. For the former three, the overall effectiveness is generally considered minimal and the cost-effectiveness is often called into question. A premier example is, Glaeser and Gottlieb (2008) which undertakes an extensive study of the federal government's use of localized policy to assist specific regions and neighborhoods through the Empowerment Zone program. This program created eight different urban zones across the country that provided tax and regulatory waivers to firms along with block grants for infrastructure spending. The authors point to Busso and Kline (2008), who study neighborhood level employment and housing market effects from 1994-2000. They find that Empowerment Zone neighborhoods experience an average 5% reduction in poverty rates, and 4% reduction in unemployment rates. These areas also experience a mild increase in housing prices and rents. That being said, this program cost $3 billion and only created 27,000 jobs and approximately $1 billion in increased output over five years. Thus, Glaeser and Gottlieb (2008) argue that empowerment zones are cost-inefficient. They also analyse other major place-based programs in the United States, particularly the Appalachian Regional Commission, urban renewal projects, and enterprise zones. For the former two, they find no discernible effect.

On the other hand, 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

[9] 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.

rates, similar to Glaeser and Gottlieb (2008), but ultimately argue that Empowerment Zones were modestly cost-efficient.

Enterprise Zones, which focus specifically on spurring business, are praised as cost-efficient in Glaeser and Gottlieb (2008), but criticized in other 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. The largest place-based policy initiative since the Empowerment Zones of the 1990s are the Opportunity Zones of 2017, some of which are contiguous with the Promise Zone. Sage et al. (2021) find no evidence of land value appreciation in Opportunity Zones. Chen et al. (2020), studying their effect on housing prices, conclude that the outside investment spurred by the zones does not raise demand for housing in those areas (implying that there is no perceived amenity improvement in the neighborhood). Freedman et al. (2021) find little to no effect on poverty, employment, or earnings. With this in mind, it is unlikely that the establishment of Opportunity Zones in 2017 would confound a study of the Promise Zone. 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 some level of economic surplus is created, it does little to benefit the actual *residents* of these areas.

Austin et al. (2018) represents a major departure from some of the earlier literature regarding place-based policies. The authors find, in regard to place-based economic opportunity type policies, programs tailored to specific locations are more effective than large-scale transfers that do not account for local circumstances. This is particularly relevant regarding the Promise Zone, since the zone does not involve a general transfer of funds (e.g. more funding for police or more money for schools across the board) but instead involves specific programs facilitated and crafted by and with collaboration from locals, taking into account the culture and circumstances of the neighborhoods in the Zone.

Kitchens and Wallace (2021) examines 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[10], about $50,000 on average. Similar to this paper, they explore potential mechanisms but do not nail down any specific mechanism through which property values increase.

[10] Using matching, they find a more modest effect of 3-5 percent.

Additionally, they find no reduction in crime that can be causally linked to the LA Promise Zone designation.

The only literature 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 interaction between the three (information that has been invaluable to the production of this paper). It also provides a useful descriptive analysis of changes in rates of crime in various hotspots across Mantua. In general, the author finds that through the BCJI, and mostly after the Promise Zone took effect, Mantua moved from being a high-crime neighborhood to a more "average" neighborhood in Philadelphia. That being said, there are two primary ways in which my research builds on and fills the gaps in Stokes (2020). First, the analysis will use a much more specific identification strategy: instead of descriptive analyses, I perform a difference-in-differences analysis that controls for time trends, seasonal variation, and location fixed effects. Secondly, while Stokes analyzes 2012-2016, this paper studies 2010-2019. Thirdly, this paper studies the entire geographical area in the promise zone, while also providing robustness checks which include, but are not limited to, testing the BCJI treatment specific to Mantua and its effect on the Promise Zone as a whole.[11]

4 Data and Empirical Method

4.1 Data

Figure 1, 2, and 3 shows the entire area studied, including the boundaries of the Promise Zone, with each tract colored by the average weekly level of violent crime during a given year. These figures provide a reference point for how violent crime is located within Philadelphia around the beginning, middle, and end of the period studied. Note that while Center City seems to get more violent, the area of the West Philadelphia Promise Zone became less violent over time. Parks and census tracts with fewer than 200 residents[12] at any point during the period studied were excluded. Figure 4 provides a close-up view of the treated area. It is bounded by the Schuykill River to the

[11] Something to keep in mind, however, that will be discussed more later, is that even Stokes argues that the treatment did not *really* become very effective until the Promise Zone was created. The robustness checks in Section 6 attest to this.

[12] As one can imagine, there is a lot of overlap in these two conditions.

east, Girard Avenue to the north, 48th street to the west, and sansom street to the south. Using the establishment of the Promise Zone Initiative as a reference point for a difference-in-differences analysis, this paper studies the effect of these changes on violent crimes in the West Philadelphia Promise Zone.

Crime data for Philadelphia from January 2006 to December 2019 was obtained from the "Open Data Philly"[13] tool provided by the local government of Philadelphia. This API continuously refreshes a data-set 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 is occurring. To be clear, though, the recorded time is the time at least one officer was dispatched to the location, not the exact time of the crime. Shapefiles of the City of Philadelphia's census tract and neighborhood composition were also downloaded, allowing individual crime occurrences to be mapped and grouped by census tract. In addition to this, annual population estimates and demographic information from the US Census Bureau were obtained through the American Community Survey at the census tract level for 2010 to 2019. This paper uses the ACS 5-year estimates from the Census Bureau [14] of racial composition (% black, white, and hispanic), percentage of individuals who have not completed high-school, percentage of individuals living in poverty, percentage of individuals who are married, For the purpose of this paper, the over one million observations in this longitudinal crime data were concatenated down to approximately 200,000 average week-year-tract observations of crime. They are used later as a falsification test in the appendix. Crimes are categorized as follows:

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

[14] 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.

Violent Crime Types[15]	Occurrences
Robbery w/ Firearm	820
Robbery w/o Firearm	1121
Rape	334
Criminal Homicide	83
Arson	134
Aggravated Assault w/ Firearm	626
Aggravated Assault w/o Firearm	1686
Other Assaults	5755
Other Sex Offenses [16]	336

Figure 17 shows the average weekly violent crime rate by year within tracts in and outside the promise zone. Figure 18 shows the same trend, but quarterly. This figure suggests a downward trend in violent crime in the pre-period which flattens out in most of Philadelphia, but continues to fall sharply in the Promise Zone until 2019, where it seems to spike across the city.

One possible confounder within the Promise Zone is that Census Tract 90 contains Drexel University and its many on-campus resident students. While all tracts in the promise zone have relatively high rates of violent crime, low educational attainment, and large African-American communities, Tract 90 has a very low crime rate, a median age of 21 (expected of a college campus), high rates of high-school completion (once again, expected of a college campus) and a majority white-community, with Asian-Americans being the largest minority group. While the level of measured poverty is similar to the rest of the zone, this is due to the low incomes and ages of full-time college students. While the Promise Zone happens to cover Tract 90, it is not likely that the BCJI or the Promise Zone were placed there for the benefit of the college students of Drexel University, but the residents of the other seven tracts.

Another problem is that the choice of this particular area, (even given its pre-qualification with the BCJI, which is also endogenous), is endogenous to the problems the area is having, such as its violent crime. An area that "selects into" the policy treatments of the Promise Zone may

[16]It is worth noting that while Justifiable Homicide and Negligent Homicide are given distinction within the database, there are no recorded occurrences in the Promise Zone during the period studied.

[16]Non-commercial sex-crimes that are not rape.

be systematically different from other area of Philadelphia (Kuehn, 2014). Neumark and Young (2019), in a re-analysis of a previous enterprise zone study, suggest identifying control areas that are similar to the zones but where the policies did not apply. Similar to O'Keefe (2004) and Elvery (2009), I use propensity score matching to find tracts in Philadelphia with similar levels of violent crime and other covariates that are causally linked to violent crime. However, those studies only use pre-treatment variables to perform their matching. As Neumark et al. (2014) points out, some studies perform their propensity score matching on variables both before *and* after the treatment was established.

Two matching algorithm techniques were employed to find a set of tracts with which to compare to the tracts within the Promise Zone. For the first matching specification, they were matched on the following covariates for the pre-2014[17] period: % of the tract's inhabitants that are married, % of the tract's inhabitants who did not complete high school, % of inhabitants living in poverty, % of inhabitants that are African-American, % of inhabitants that are Caucasian, and % of inhabitants that are Hispanic.

For the second matching specification, matching was done on the aforementioned covariates, but for the full period. It is generally considered incorrect to match on outcomes, even in the pre-period. In other words, the chosen covariates should be relevant enough to the outcome that they find a control group with similar pre-treatment levels of the outcome variable. Table 1 displays the pre-treatment means of several demographic covariates that have been shown to affect violent crime, along with the violent crime rate. The poverty rate is particularly important since the Promise Zone's programs may reduce crime through their effect on poverty. Note that when Tract 90 is removed, the pre-2014 difference in demographic characteristics between the eventual Promise Zone and the rest of Philadelphia becomes even more stark. The exceptions are the marriage rate, the percent of Hispanic residents, and the poverty rate. These are to be expected though, as the Drexel University campus is filled with college-aged students who are unmarried, predominantly white, and have little to no income.

Tracts 152, 168, 172.01, 202, 243, 283, and 364 and were found to be good matches for the seven Promise Zone tracts when matched on pre-2014 covariates. Parallel trends are displayed at both the

[17]Even though the regressions ultimately allow half a year for "uptake" of the treatment, the designation as a Promise Zone was announced in January of 2014, making it a function of pre-2014 indicators.

Table 1: Comparing Covariates within and outside the Zone.

	Untreated	Promise Zone	Promise Zone (w/out Tract 90)
Weekly V. Crimes per 100k	58.90	74.77	82.65
% No High-School or No GED	19.60	23.00	25.72
% Poverty	25.67	46.12	44.19
% Black	43.45	73.33	82.73
% White	38.41	17.31	10.20
% Hispanic	10.93	3.22	3.11
% Married	29.71	16.20	17.81

year and quarter level in Figures 6 and 7. Note that the trends begin to diverge more substantially after the treatment has had time to take effect. Since much of the funding facilitated through the Promise Zone involves projects with uptake and incubation periods, we should expect the treatment to be strongest once several quarters have passed after implementation. Also note, in addition to relatively parallel pre-treatment trends in violent crime, the pre-treatment *levels* of violent crime are very similar between the treated and matched control units. First, this demonstrates a successful matching algorithm, but Kahn-Lang and Lang (2020) argue that similar levels in the pre-treatment outcome variable can substantially reduce bias in Difference-in-Differences estimation. Without similar pre-treatment levels of violent crime, one could argue that there are pre-existing conditions that may cause differences in trends later in the sample period. By finding similar tracts, the estimator is made more robust.

When matched on covariates only for the full period studied, the tracts chosen are 69, 167.01, 168, 201.01, 249, 284, and 9891. Figures 8 and 9 depict the yearly and quarterly trend comparison. It is difficult to argue that there is a parallel pre-trend when measured annually, but the quarterly trend depicts the Zone and the matched tracts being relatively intertwined. Regardless, these figures tell the same story as the previous ones: even for a comparison group with similar levels and fluctuations of violent crime, the trends seem to diverge after the establishment of the Promise Zone. This is not to say that the Promise Zone caused crime to increase in unrelated areas, but that it seems to have buffered its area against an increase in crime that seemed to occur in many comparable tracts throughout the city. The counter-factual claim being made through these figures is that violent crime would have either leveled out or risen in the Promise Zone area had it not received these grants, as opposed to decreasing as it did.

4.2 Model

A difference-in-differences model is employed, of the form

$$\begin{aligned} Crime_{it} = \beta_1 Zone_i + \beta_2 (After)_t + \beta_3 Zone_i \times (After)_t \\ + A_{it} + \alpha_i + \gamma_t + \mu_i \end{aligned} \tag{1}$$

where *Crime* is the crime rate per 100k residents in a specific week in a given year in a tract. *Zone* is a dummy variable equal to 1 for any census tract fully lying within the boundaries of what would be the Promise Zone at any time in the period studied. *After* is a dummy variable equal to 1 for any week starting June 1st until the end of the period studied, (any obs. where $Date \geq$ *6/1/2014*), which allows for a six month buffering period for the grants in the zone to take effect.[18] The interaction term is the basis of the difference-in-differences analysis, equal to 1 only for week-years in tracts where the Promise Zone had been in effect for at least six months. α_i represents a time-invariant tract fixed effect, while γ_t includes year and week-of-year fixed effects. These fixed-effects are included to account for time-invariant characteristics in tracts, along with unobservable city-wide trends from year-to-year regarding crime. A_{it} is a matrix of the 5-year ACS estimates discussed in Section 4.1, which are measured at the tract-year level. These variables include the % of individuals with no high school education, % living under the poverty line, % African-American, % White, % Hispanic and % married. Andresen and Malleson (2013) demonstrates that crimes vary by month and season, so week fixed-effects are included to control for weekly variation in crime rates; this is particularly important since there are more weeks in the pre-period than the post-period (for example, the week of Christmas has more robberies and fewer assaults, but fewer Christmas weeks are recorded in the post-period). μ_i is the error term, which is cluster-robust at the tract level for all regressions unless otherwise noted.

5 Results

Table 4 displays the results of the first matching model. In all four specifications, the treatment is negative as expected, but is only significant at the 10% level when clustering or wild cluster

[18] Weeks are categorized as 7 day intervals beginning January 1st of each year, with the 365th day of the year being counted as the 53rd week, which in 2008, 2012, and 2016 included the 366th day due to leap years. Week fixed-effects account for the shortness of the 53rd week and lack of later weeks in 2020.

bootstrapping the standard errors in the fully specified model[19]. This treatment coefficient works out to approximately 35 fewer violent crimes per year in each tract compared to the matched tracts in this specification.

Table 5 displays the results from the second matching model. Once again, the treatment effect (on treated) is negative in all four specifications. In this case, the fully specified model is significant at the 1% level even when wild cluster bootstrapping is performed. Matching on covariates from the entire period The treatment coefficient works out to approximately 30 fewer violent crimes per year in each tract compared to the matched tracts in this specification.

That being said, much of the matching literature cautions against interpreting a treatment effect in a matched sample as being the average treatment effect in a generalizeable sense. Instead, we can interpret 6 and 7 as providing a more generalizeable effect of the Promise Zone grants. These tables display full-sample regressions without matching, where the seven or eight treated tracts are compared with the more than 360 untreated tracts in the sample. Note that the effect size is stronger when tract 90 is excluded. All of these results these results hold up to wild cluster bootstrapping of the standard errors and varying specifications with or without controls or fixed effects. What they suggest is that the Promise Zone reduced violent crime by about 9 per week per 100k population in each tract. Given an average population around 3500 during the post-period, this works out to approximately 15 fewer violent crimes in each tract per year.

6 Possible Mechanism

As mentioned previously, this paper studies the effect of the Promise Zone as a whole, rather than any specific grant. However, it is useful to attempt to disentangle the mechanism through which the Promise Zone reduces violent crime in order to better inform public policy and future Promise Zone initiatives. One goal of the Zone was to reduce poverty. Figure **??** shows poverty falling, which suggests that the Zone may be reducing poverty, but without parallel trends Difference-in-Differences is not useful. One could argue that the Zone could be reducing violent crime through a reduction in poverty, but poverty is a control variable in every set of results. Table 8 suggests that the Promise Zone has no effect on poverty rates, but this should be taken with a grain of salt.

[19]HC3 Robust Standard Errors demonstrate significance at the 5% level, this is discussed in the appendix

Another major goal is to improve education within the Zone. Figure **??** suggests that the high-school completion rate within tracts in the Zone increased substantially, to levels similar to the rest of Philadelphia in the post-period. Figure **??**, which excludes tract 90 where Drexel University lies, makes an even stronger case for this.

Another argument could be that the Zone is reducing property crimes that may be complementary to According to 9, there is also no discernible effect of the Promise Zone on property crimes

7 Robustness Checks

7.1 Synthetic Control Method

I employ a Synthetic Control Method to offer an alternative counter-factual to the Zone had the treatment not occurred. Synthetic control is particularly useful as a robustness check against difference-in-differences in the case of this research, since graphically it may seem that the treatment did nothing to affect an already downward-trending violent crime rate. The argument I make in this paper is that violent crime would have *stopped* trending downward and possible trended upward without the Promise Zone's establishment in 2014. Kaul et al. (2018) find that there are a few major flaws that hurt the legitimacy of certain synthetic control models. They demonstrate both theoretically and empirically that limiting outcome lags provides more accurate and meaningful counterfactual models than using many or all pre-treatment outcomes as predictors. They argue that researchers should limit themselves to one pre-treatment outcome variable: either the average, following Abadie and Gardeazabal (2003) and Abadie et al. (2015), or the last pre-treatment outcome.

To create this synthetic control, I aggregated the 8 tracts within the Promise Zone (including Tract 90) into one unit, comparing it on yearly pre-2014 covariates to all other tracts in Philadelphia. I also included the 2013 violent crime rate as suggested by Kaul et al. (2018). Figure 11 displays the path plot from the synthetic control specification. Note that the synthetic Promise Zone is able to match the real Promise Zone nearly perfectly from 2010 to 2013. They depart upon treatment uptake, after which we observe a substantial uptick in the rate of violent crime within the zone.

Another potential issue is when the weights in the model heavily favor a small number of

Table 2: SCM Predictor Weights

Predictors	Weights
% Black	0.139
% White	0.15
% Hispanic	0.149
% Married	0.136
% Low Education	0.146
% Poverty	0.141
2013 V. Crime Rate	0.139

covariates or units in relation to the total available; this is a problem often caused in itself by using too many pre-treatment outcomes (Mcclelland and Gault, 2017; Kaul et al., 2018). Table 2 demonstrates a healthy balance among the covariate weights.

It is possible that the split seen in the path plot at 2014 could be the result of telling the Synth package to to match from 2010 to 2013. To check this, I change the pre-treatment years for which the model is optimizing from 2010:2013 to 2010:2014. If the Synthetic Zone departs from the real one in 2015 instead, then that would be evidence of bias in the synthetic control. This path plot is shown in Figure 12. The departure between the synthetic control and the real Promise Zone still occurs in 2014 despite telling Synth to optimize until 2014. Additionally, this synthetic zone follows a similar upward trend to the original SCM.

As an additional check, I use the pre-treatment average of violent crime as a predictor, following Abadie and Gardeazabal (2003) and Abadie et al. (2015). The plot is shown in Figure 13 and provides a nearly identical outlook to the original synthetic control. Taken together, these synthetic control models strongly suggest that, had the Promise Zone not been established in 2014, violent crime within the zone would have trended significantly upward.

7.2 Event Study Specification: Plotting the Marginal Effect Quarter by Quarter

Since the treatment strength may vary over time based on its nature, as discussed previously in the paper, I specify the event-study, or "Dynamic Difference-in-Difference" model

$$\begin{aligned} Crime_{it} &= \beta_1(Zone_i \times Quarter_t) \\ &+A_{it} + \alpha_i + \gamma_t + \mu_i \end{aligned} \tag{2}$$

where *Quarter* is a factor with 40 levels, each of them being a quarter in the sample time.[20] The first quarter of 2014 is excluded as the reference category. A_{it} is a matrix of the demographic control variables used in previous models. γ_i is a matrix of quarter-year (e.g. Q1 2010, not simply Q1) fixed effects, week of year, and year fixed effects and α_i is a tract fixed effect. Quarter-year fixed effects control for seasonality in crime trends and are especially important since a specific quarter (Quarter 1 of 2014) is being used as our reference category. By adding quarter-year fixed effects, the effect will not be biased by seasonality. The benefit of this type of model is that it allows us to better understand a treatment with a "staggered" rollout. The programs facilitated by the Promise Zone do not all begin in 2014, and even those that do may take time to have an effect. Therefore, I use this model to interact tracts within the zone with each quarter of the study period. If parallel trends in the pre-period are present, we should observe no significant effects in the pre-period but significant effects in the post-period.

Figure 14 shows the results of this event-study specification. Note that in the 16 quarters in the pre-period, there are no significant effects from the Promise Zone at the 5% significance level. While not proving parallel trends outright, this adds further credibility to the parallel trends assumption. In the 23 quarters of the post-period, eight quarters experience a significant negative effect on violent crime from the Promise Zone.

I perform the same event study format, truncating the sample down to the 7 Zone tracts and 7 matched tracts, using both of the Propensity Score Matching models. Figure 15 presents the results in Matching Model 1, where tracts are matched on pre-2014 demographic controls. Figure 16 displays the same, but for Matching Model 2. In both of these event study figures, there are

[20]Similar results are found in a year and monthly specification, but quarters are chosen since monthly variation in crime is extremely noisy, and yearly variation is not very informative.

quarter-years before the establishment of the Promise Zone where there is a "treatment" effect. This makes parallel trends for the matched models a bit more difficult to accept. That being said, the effect being picked up in the pre-periods of the event-study models becomes substantially stronger and more significant in the post-period. Since the comparison group is only seven other tracts in each model, the event study becomes more susceptible to these types of problems.

7.3 Using the establishment of the BCJI as an additional treatment

I use the establishment of the Byrne Criminal Justice Initiative as an additional treatment, quantifying it as any tract in the Promise Zone 6 months into the establishment of the BCJI and onward, and the treatment effect of the Promise Zone stays significant and of the same magnitude as it did previously. Column (1) of 11 demonstrates these results. Note that there is significant overlap in the two treatment variables, since one extends from 2012 to 2019, and the other extends from 2014 to 2019. This means that the reference group is untreated tracts from 2010 to mid-2012. The non-significance of the BCJI treatment demonstrates that there is no significance change in weekly crime from before the BCJI, but essentially splitting the treatment variable into two variables removes the significance on the "real" Promise Zone treatment. This should not be alarming since the BCJI treatment that starts in mid-2012 does not actually treat the entire zone. For that reason, in column (2), I respecify a more "true" model where the BCJI time dummy is interacted with the two tracts in the neighborhood of Mantua. Here, the treatment effect retains its significance at the 1% level and is robust to wild cluster bootstrapping. In column (3) I change the BCJI treatment to cease as soon as the Promise Zone treatment becomes active. In other words, the BCJI treatment variable extends from June 1st of 2012 to May 31st of 2014. Finally in column (4), I use the "between" treatment time, but only consider tracts 108 and 109 which comprise Mantua once again, where the BCJI grant was sent in 2012. Once again, we see no significant effect from the BCJI, even when focused on the tracts that it directly affected. Note that the magnitudes of coefficients and significance is almost identical to what is found in 7, and the BCJI treatment remains insignificant across all specifications. This further demonstrates that the Byrne Criminal Justice Innovation, as established in 2012, is not biasing the results, and bolsters the point made by Stokes (2020) that the goals of the BCJI were not effective until the establishment of the Promise Zone.

8 Cost-Benefit Analysis of Crime Reducing Policies

Like any policy with tangible benefits, it is crucial to analyze it in comparison to its costs. For this back-of-the-envelope calculation, I assume that the "cost" of reducing violent crime occurrences by 13% is simply the cost of the public safety grant interventions in the Promise Zone, $3.38 Million.

An Executive Summary from Rebecca Rhynhart, City Controller of Philadelphia, states that a single year of 10 percent homicide reductions in the city would net the city approximately $13 million per year. Of course, the focus of this study (and the $3.38 million spent) is on this much smaller area of 30,000 or so residents. If we assume that the Promise Zone comprises approximately 2% of the city's population (which it does, approximately), then we could assume that that $13 million would be about $260,000 in the area of the Promise Zone. If we multiply that amount by the five years in which the zone has been in effect, we get a $1.3 million tax benefit to the city in exchange for $3.38 million in expenditures.

If we are only to consider the tax benefit to the city, then it would seem like a net loss of value of occurring. However, tax benefit to the city is not the only kind of surplus created from the reduction in violent crime. According to the National Institute of Justice, a murder creates approximately $1 million in tangible costs, and $2 million in intangible costs (1996 numbers). If even four murders are prevented by the implementation of the public safety grants which the Promise Zone facilitated, the interventions will have paid for themselves. If we were to assume that there are 9 fewer violent crimes per week per 100,000, then approximately 3 fewer are occurring weekly within the zone. In a year, this is 156 fewer violent crimes, the vast majority of which are assaults, which create costs about $10,000 each. Even if all crimes were assaults, this would be a tangible cost reduction of $1.56 million per year. Needless to say, some of these violent crimes are worse than assaults, which cause even greater tangible and intangible costs for the city and its residents.

Looking at each violent crime type individually, as in Table 12, we see that the primary violent crime reducing effects of the zone are in regard to aggravated assault and simple assault. 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." Aggravated assault is "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm." Using the framework laid out by the US Office of Justice Programs (OJP), the tangible cost of an aggravated assault is approximately $20,000 in 2008 dollars, with estimates up to $100,000 when including intangible costs.[21] Mathematically, the coefficient on Aggravated Assault works out to 30 fewer aggravated assaults per year across the entire zone. This means approximately $600,000 tangible dollars saved and $3 million in societal cost prevented per year from the reduction in aggravated assault alone. The approximately 68 fewer simple assaults occurring per year prevent approximately $68,000 in tangible costs, and $680,000 in intangible costs. Altogether, these grants may have saved the city almost a million real dollars and almost four million dollars in intangible value derived from the suffering and long-term costs to the victims of these crimes. These results are more of a lower-bound estimate, since I am only considering the effect within seven tracts, excluding tract 90.

As mentioned, the caveat of this analysis is that I assume the only expenditure toward reducing violent crime is from the Public Safety Grants.There is no doubt that there is likely a spill-over effect from other quality of life grants into the realm of violent crime. That being said, the public safety interventions of the zone provide a substantial level of return, both tangible and intangible, at a relatively low cost. Violent crime is extremely costly and different from property crime in that the damage done to a victim's body, psyche, family members psyche's, and more create lasting damage and costs that may recur over many years.

9 Discussion

Each model points to the same general conclusion: the establishment of the Promise Zone reduced violent crime rates within the treated area relative to the rest of Philadelphia and the matched comparison groups, and the effect becomes stronger over time. There are three general trend observations made in this paper: first, violent crime trended downward all across Philadelphia from 2010 to 2014. Second, beginning in 2014, violent crime began leveling out in Philadelphia overall while it rose in tracts with similar characteristics to the tracts within the Promise Zone. Third, the tracts within the Promise Zone continued to trend downward in regard to violent crime despite the levelling out in Philadelphia and the increase in similar areas. Together with the event

[21] https://www.ojp.gov/pdffiles1/nij/grants/254010.pdf

study analysis, this makes a compelling case for the general effectiveness of the Promise Zone in reducing violent crime rates.

It is important to point out that many advocates for the program and groups involved with its implementation, such as the MVCDC, report decreased crime rates in the areas where they perform their outreach. This is perfectly in line with the results from this study. Violent crime is reduced on average by 13% in tracts in the Promise Zone. In that case, not only are these results statistically significant, they are practically significant and have implications for the city as a whole. It is hard to pinpoint every single effect sponsored by the Promise Zone that would not have occurred without it, but clearly the Mayor's Office and the many community groups and citizens involved are meeting their goal of reducing violent crime in West Philadelphia and making it a more livable place.

A key limitation of this research is that it uses a more aggregated approach to looking at the Promise Zone as a whole, which makes it difficult to parse out the individual effect of individual interventions. In that way, I cannot claim one particular mechanism through which crime is reduced. That being said, further research can be done focusing on the "hot-spot" strategies employed by the community outreach groups. Smaller, more pinpointed diff-in-diff analyses can be performed based on specific grants that went out at various times, particularly at a finer grid level rather than the census tract level.

Based on results from multiple causal inference methods, this research suggests that while violent crime trended upward in similar areas to those in the Promise Zone and plateaued in most of Philadelphia (in the post-treatment period), the Promise Zone experienced significant reductions in violent crime attributable to this program. These results suggest that Promise Zones can be an effective place-based policy in terms of reducing violent crime. It is important to note that since the Promise Zone program provides federal coordination and fast-tracking of grants, but does not guarantee any specific set of grants, that 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 (2021) indicate that not all Promise Zone programs reduce violent crime. Given the robust violent crime-reducing effects of the Philadelphia Promise Zone, policymakers interested in reducing violent crime should look to the Philadelphia Promise Zone implementation for guidance.

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Figures and Tables

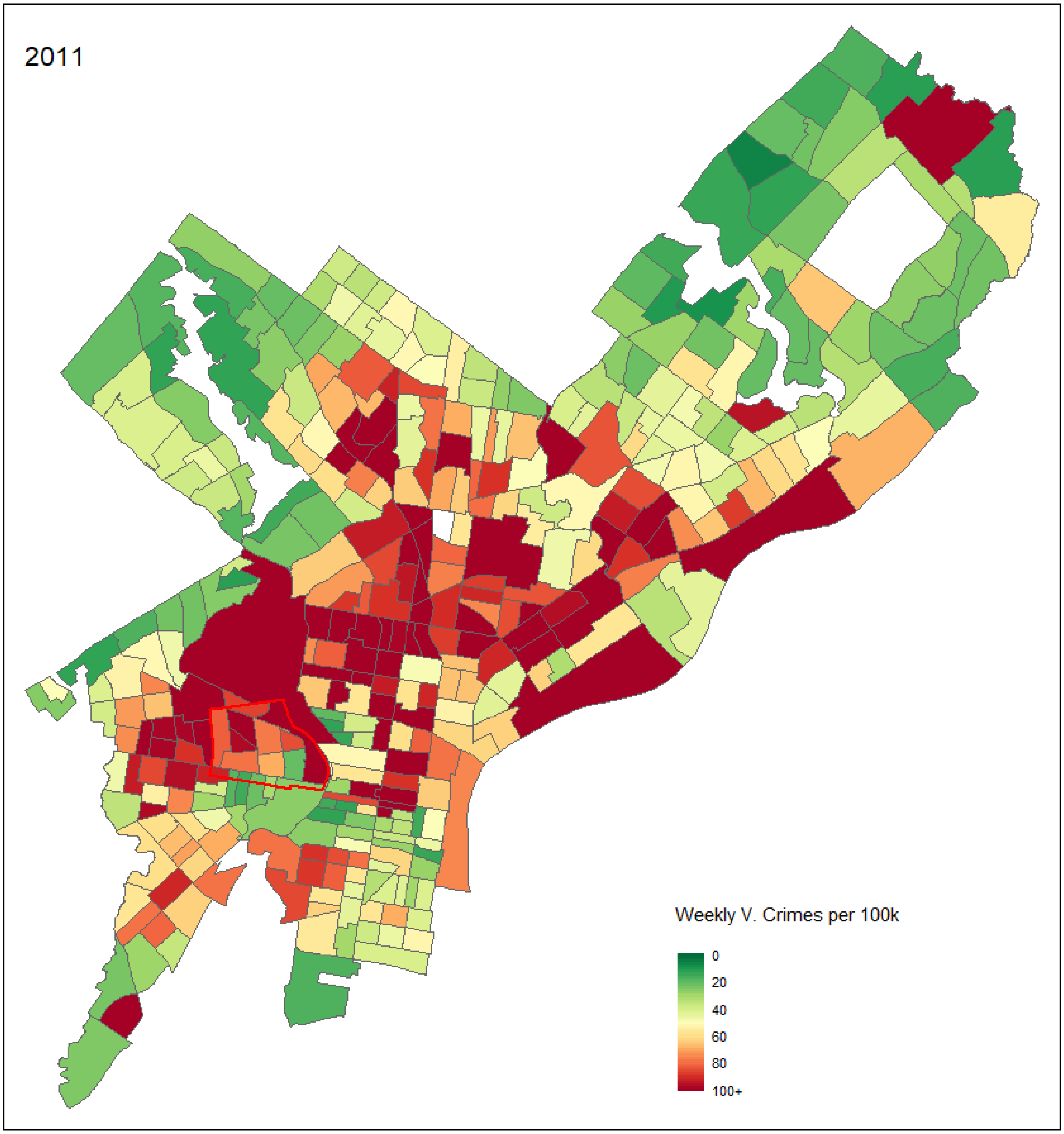

Figure 1: Philadelphia Tracts: Colored by Violent Crime in 2011
Red border indicates West Philadelphia Promise Zone.

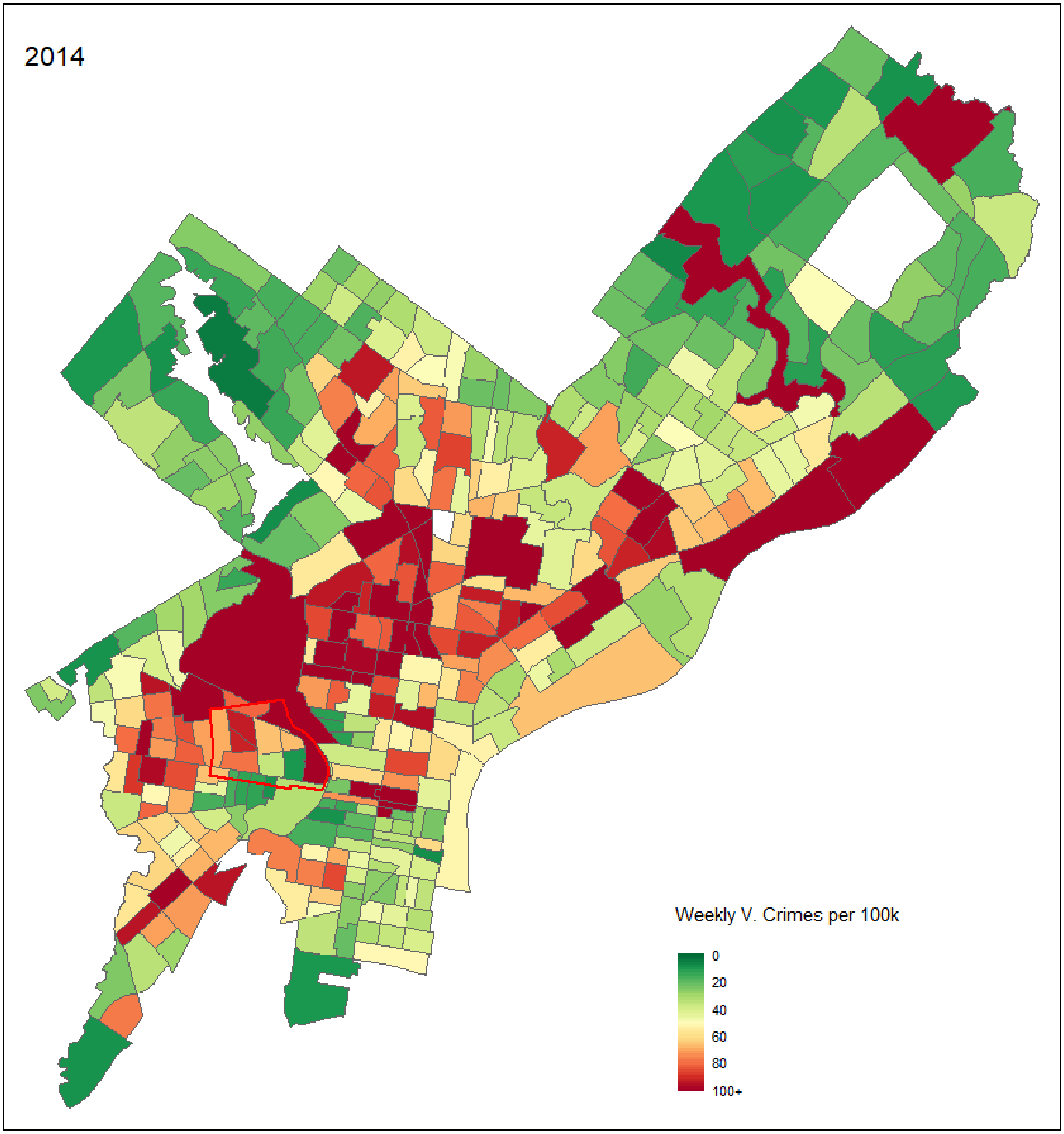

Figure 2: Philadelphia Tracts: Colored by Violent Crime in 2014
Red border indicates West Philadelphia Promise Zone.

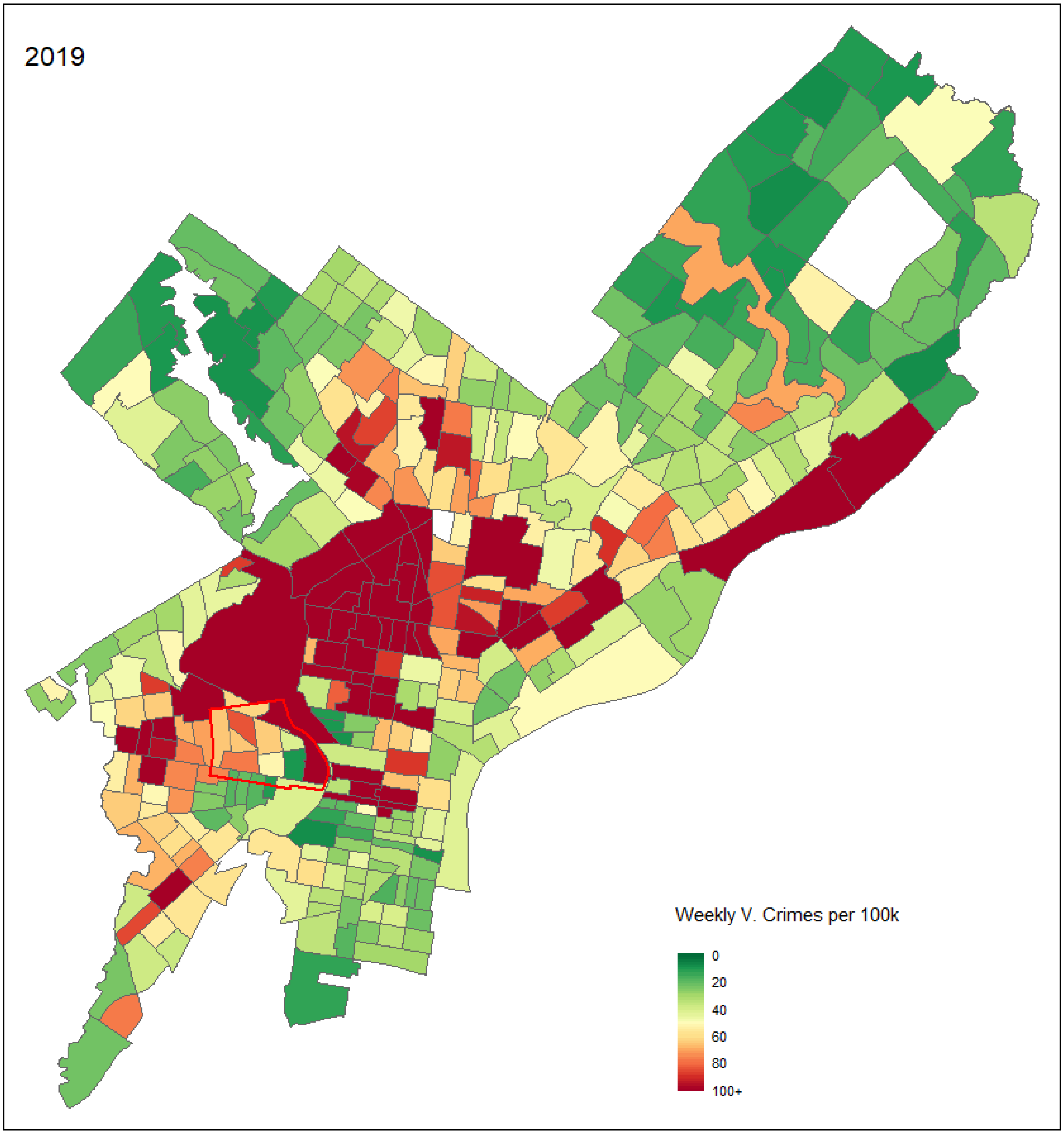

Figure 3: Philadelphia Tracts: Colored by Violent Crime in 2019
Red border indicates West Philadelphia Promise Zone.

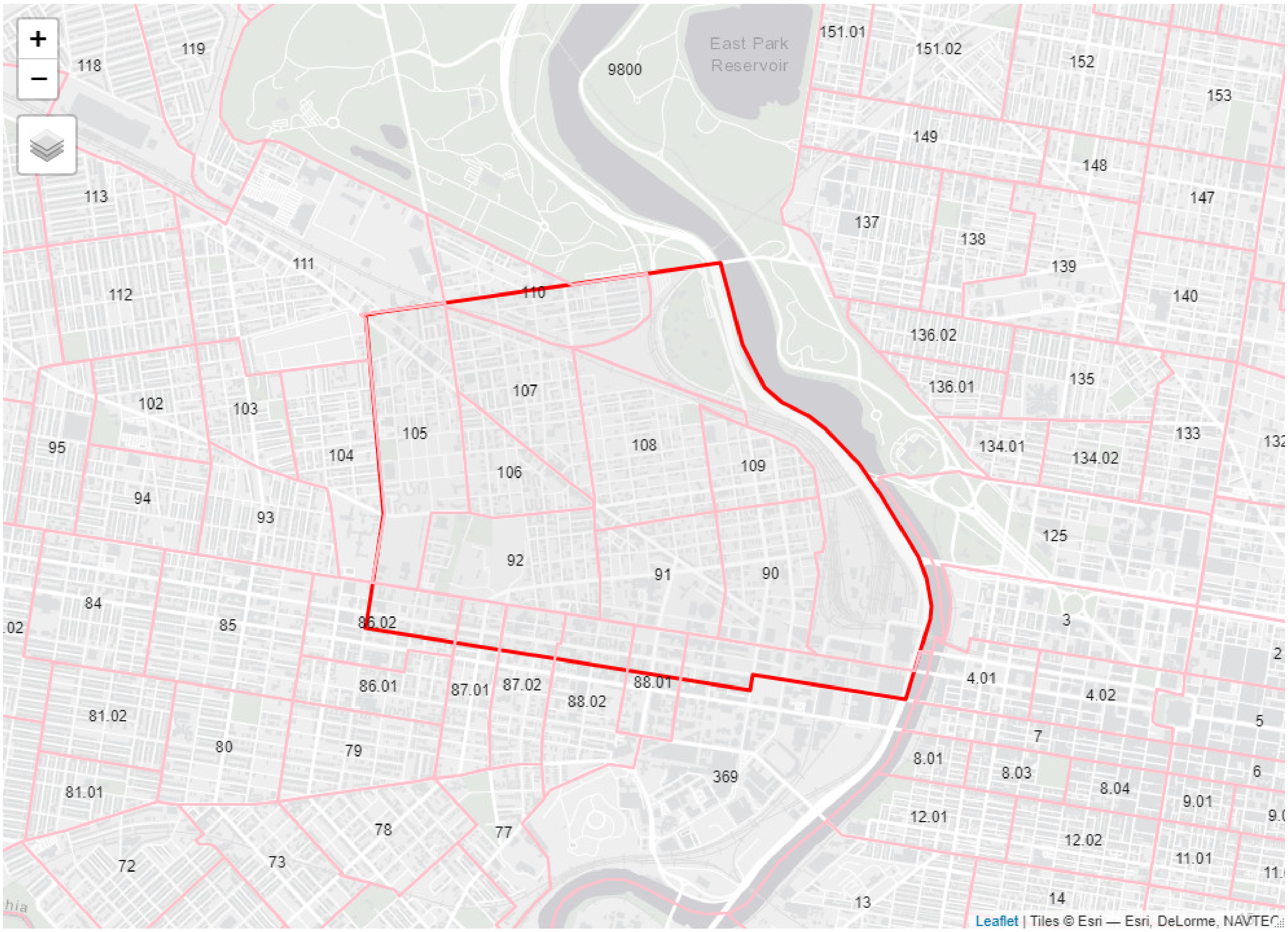

Figure 4: The Immediate Area of the Promise Zone by Tract
Red border indicates Promise Zone. Pink borders and numbers indicate individual census tracts.

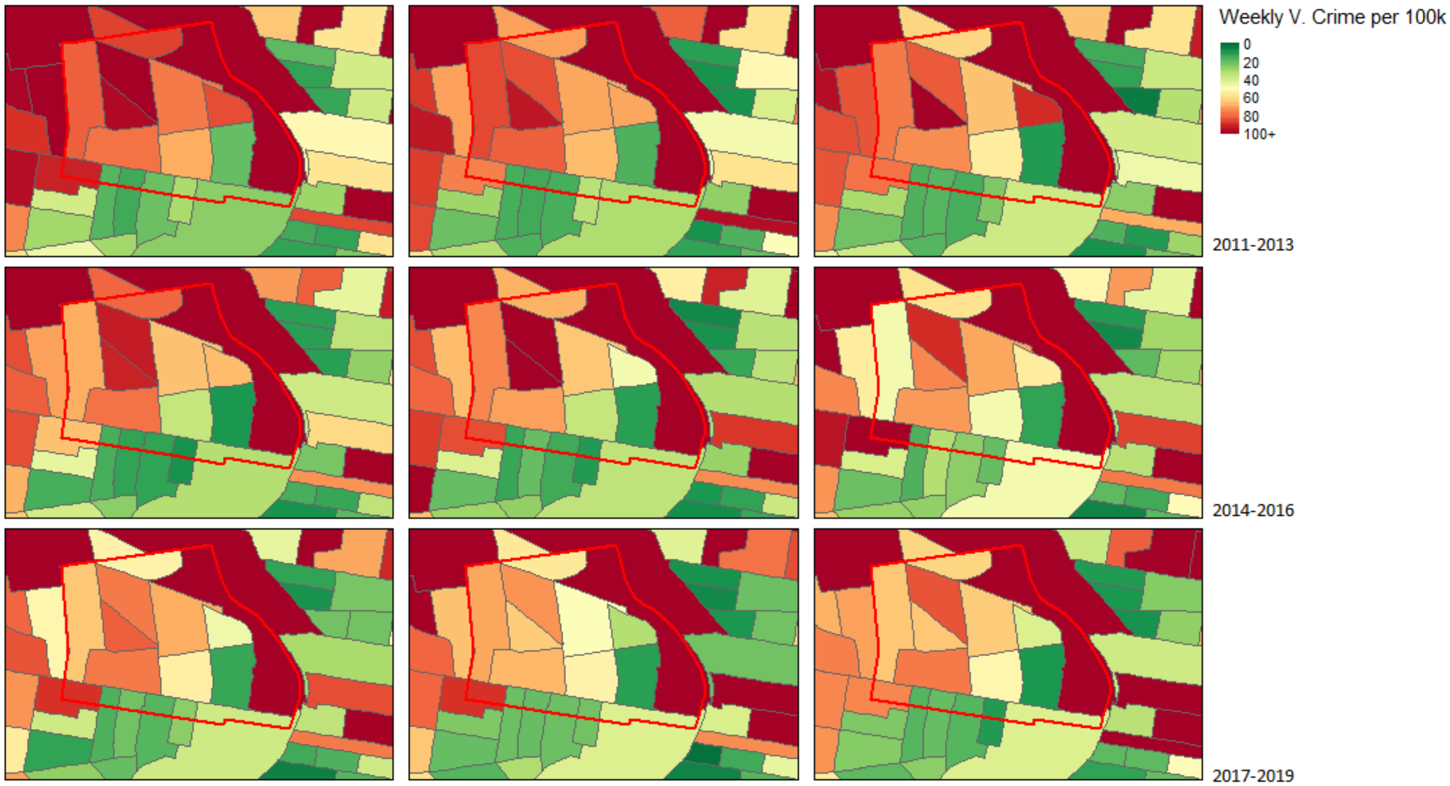

Figure 5: Violent Crime in the Immediate Area
Red border indicates Promise Zone.

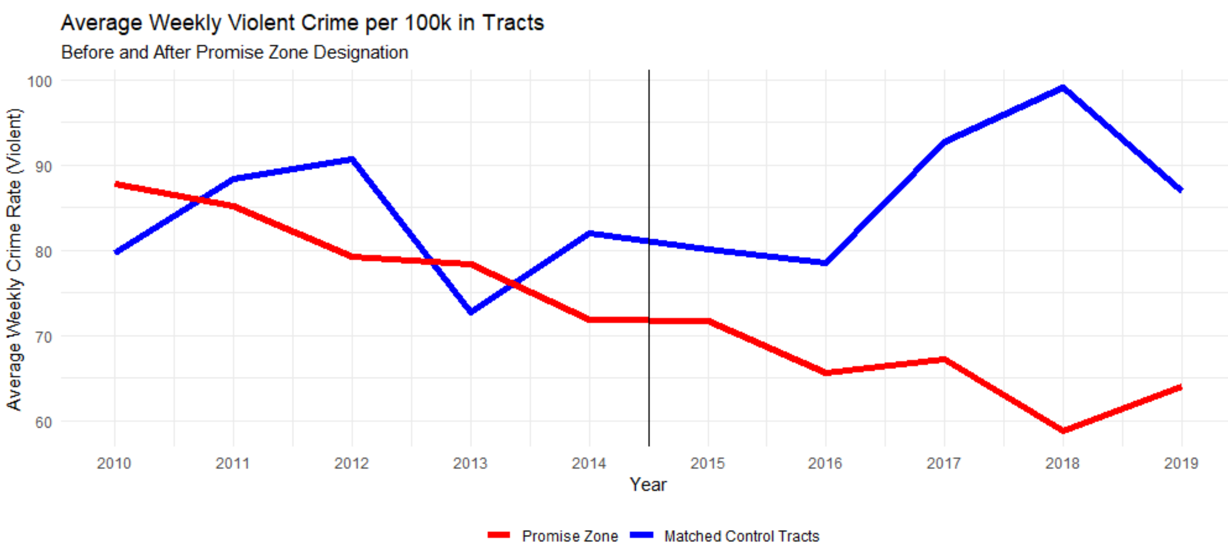

Figure 6: Yearly trends, matched on pre-2014 covariates.

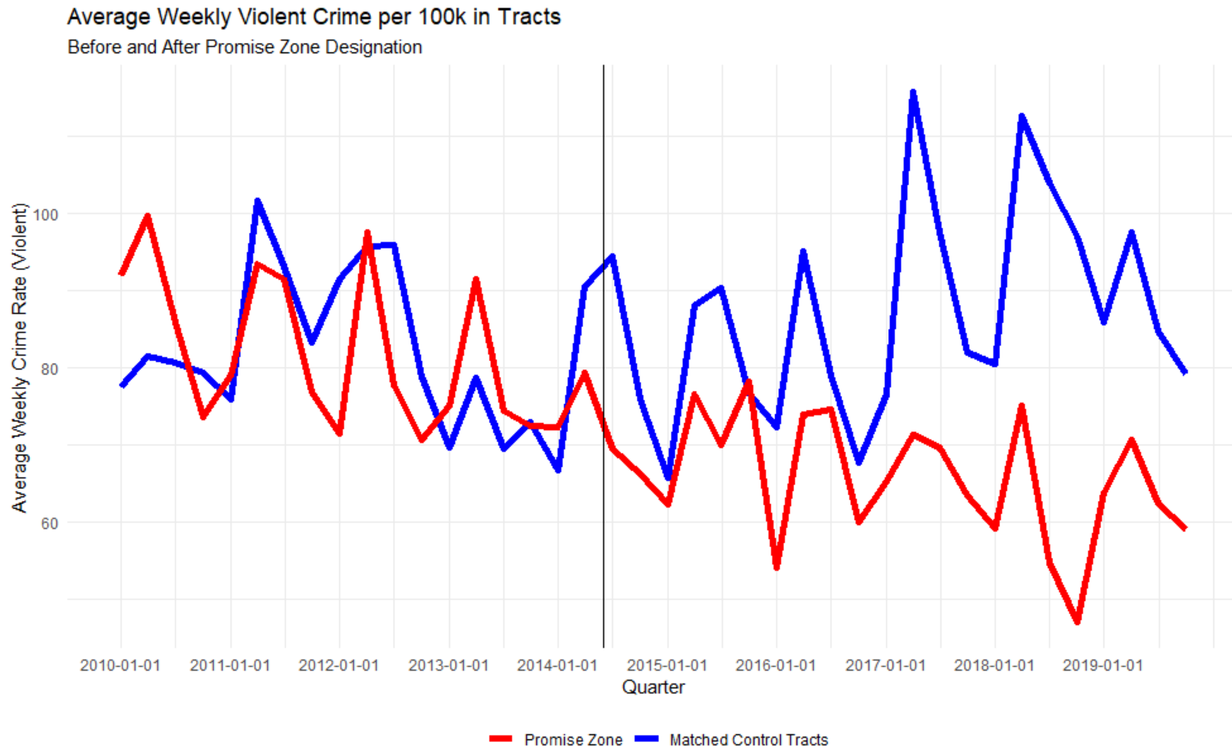

Figure 7: Quarterly trends, matched on pre-2014 covariates

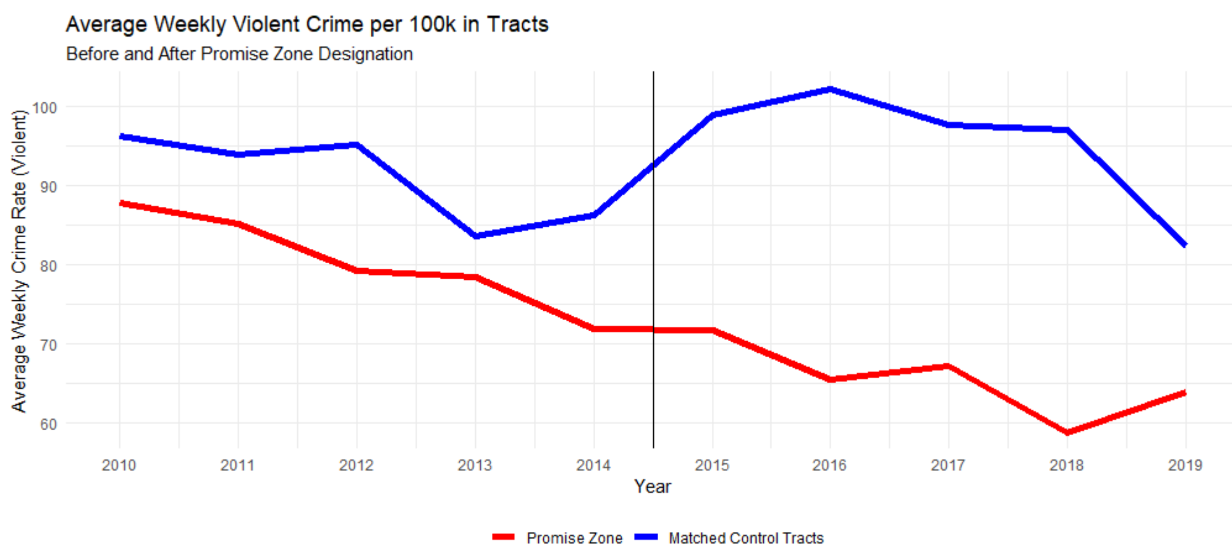

Figure 8: Yearly trends, matched on full period covariates.

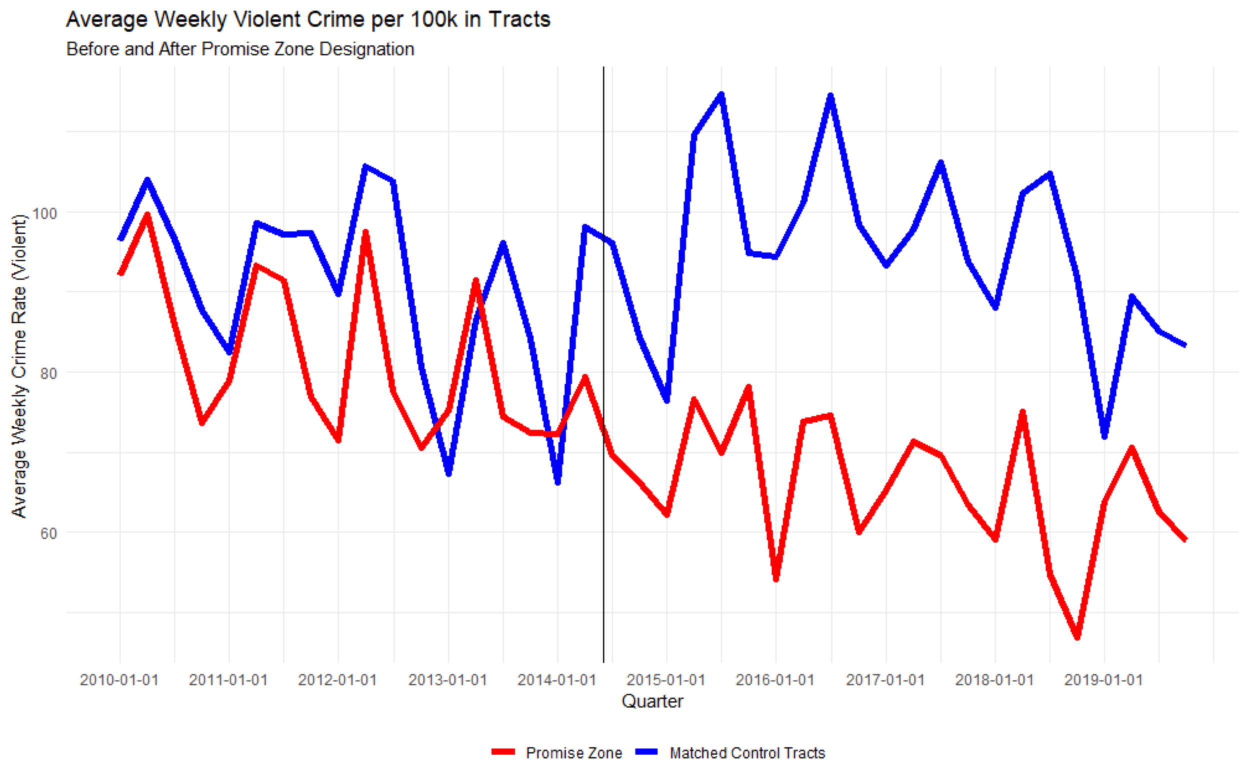

Figure 9: Quarterly trends, matched on full period covariates.

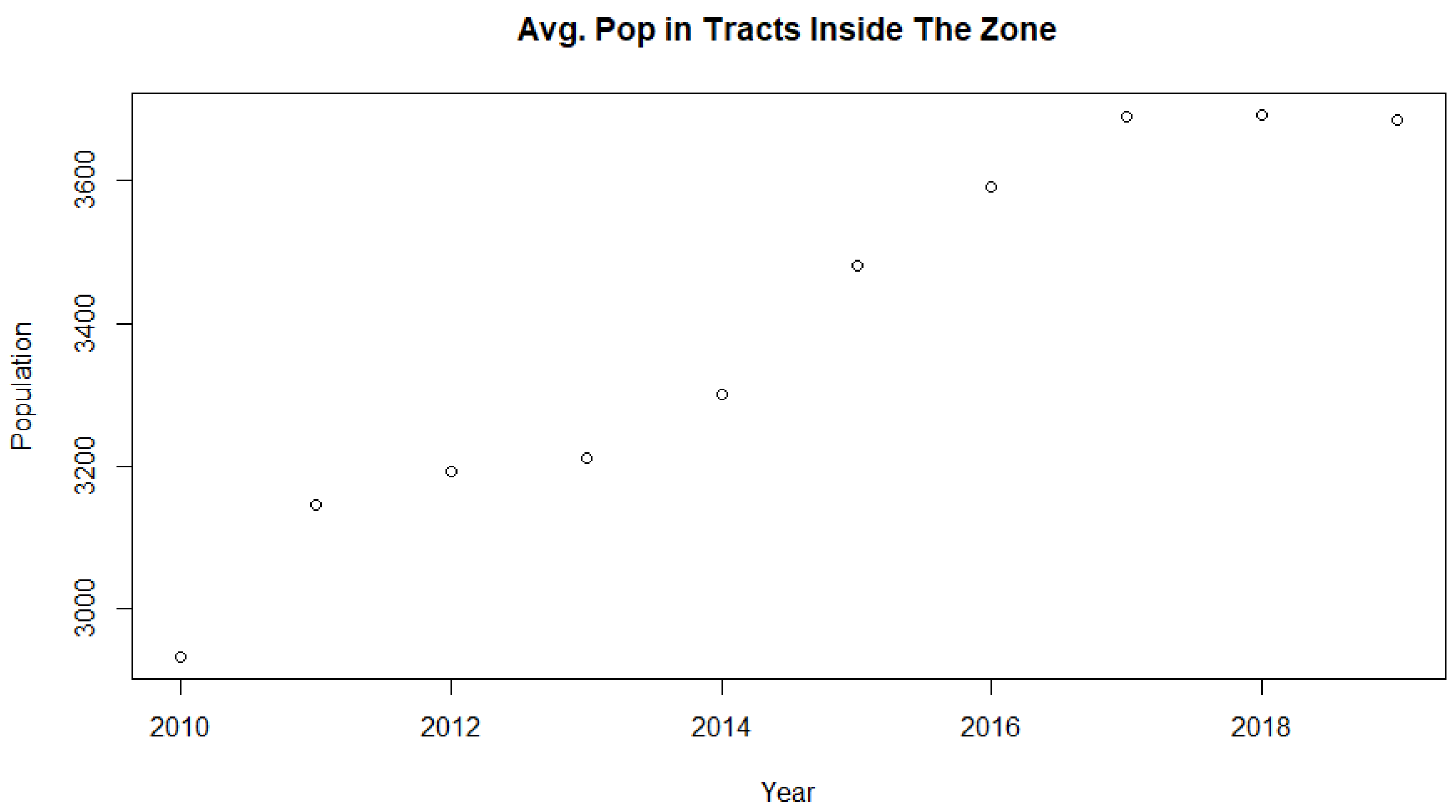

Figure 10: ACS 5-Year Estimates of Tract Population

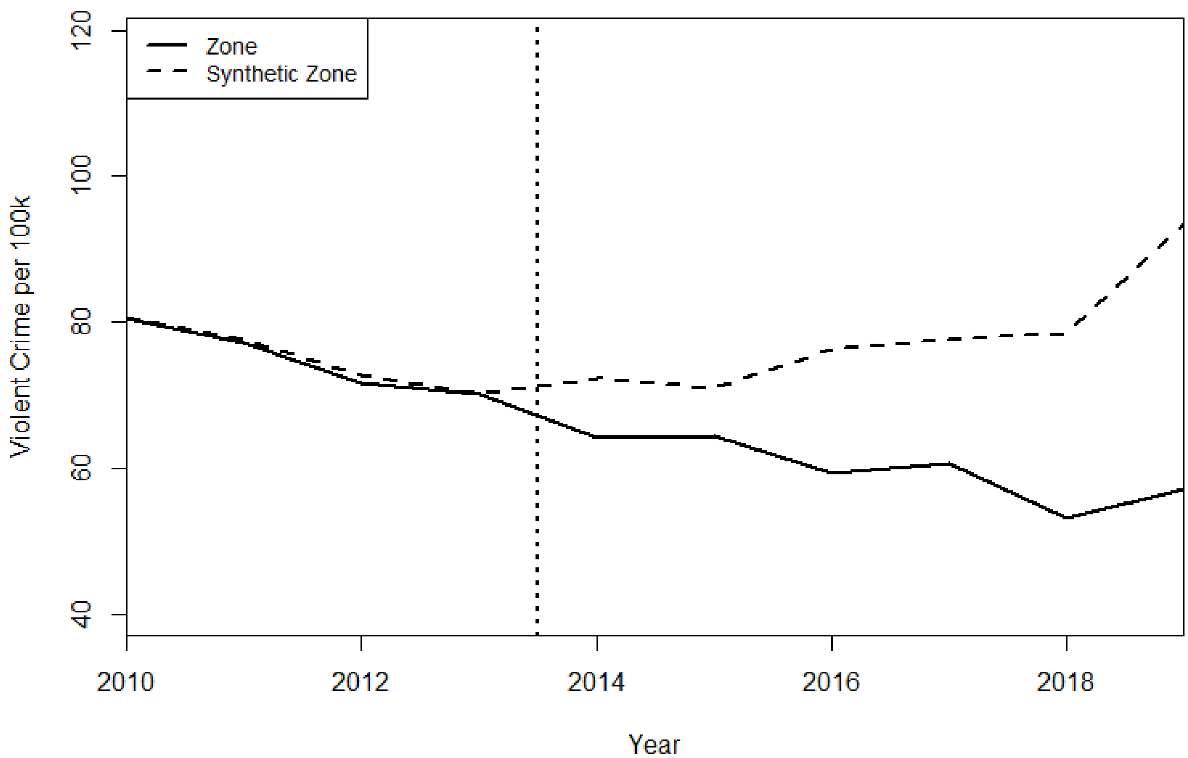

Figure 11: SCM matched on pre-2014 covariates and 2013 Violent Crime Rate

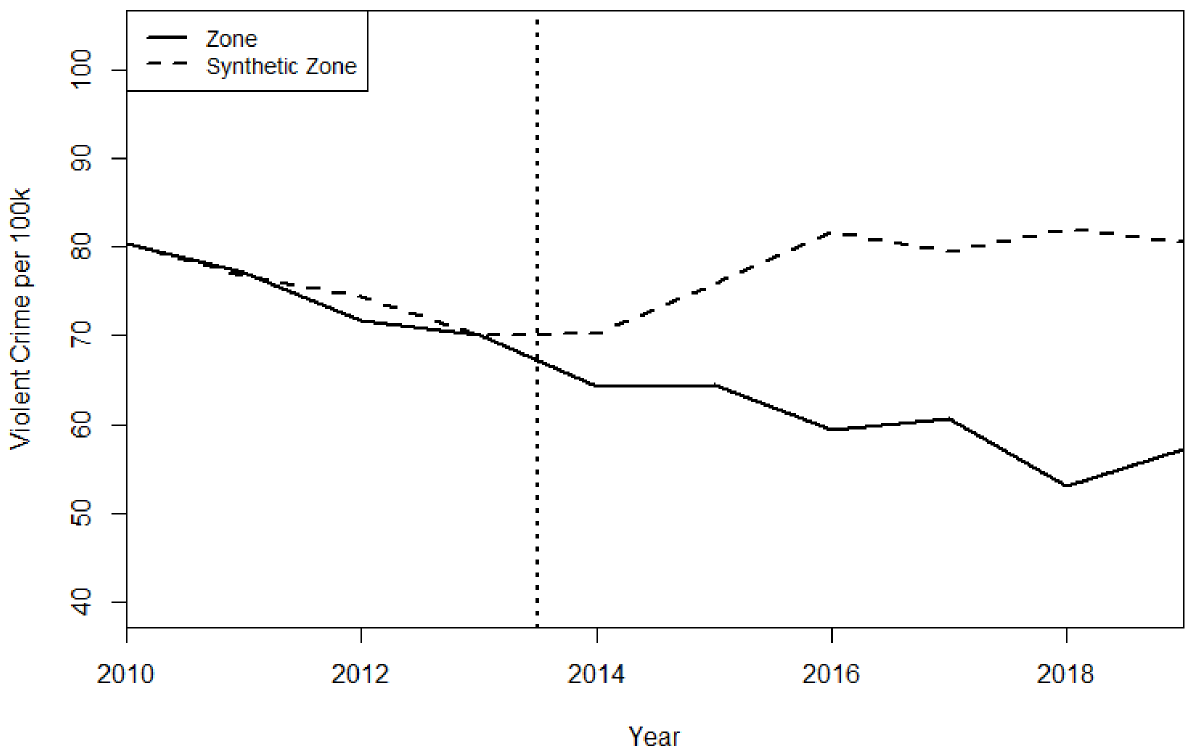

Figure 12: Robustness Check - SCM matched on pre-2015 covariates and 2013 Violent Crime Rate.

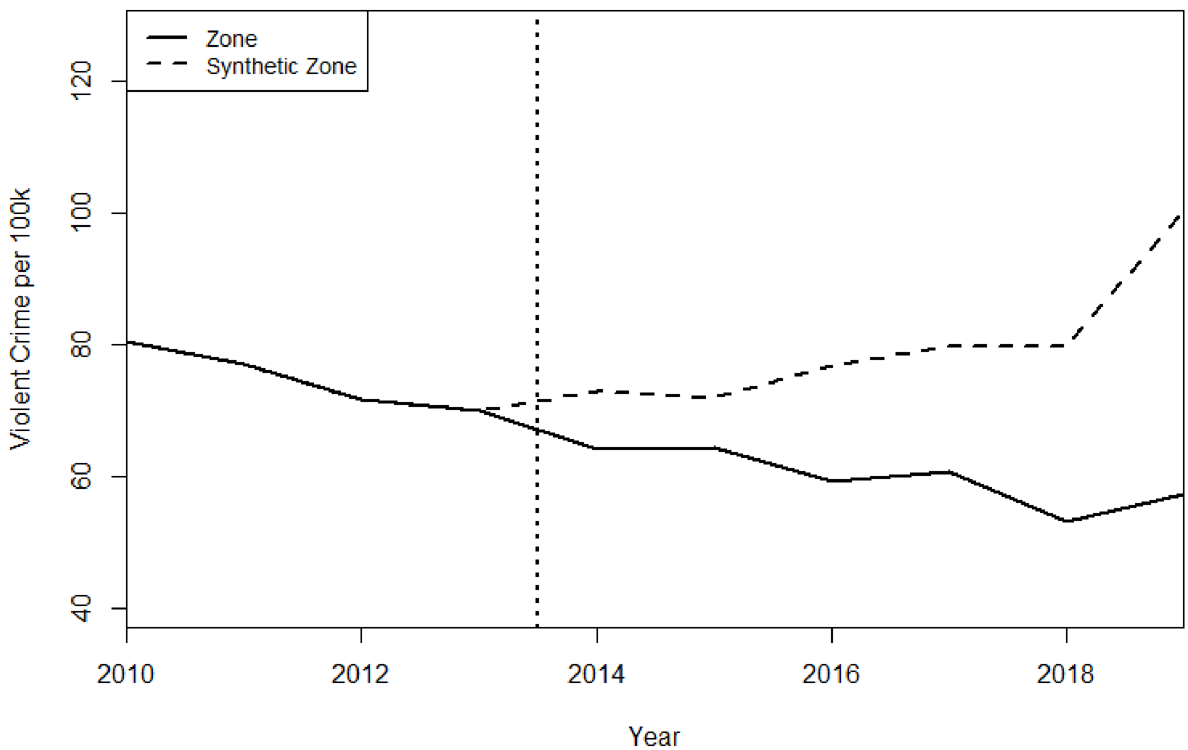

Figure 13: SCM matched on pre-2014 covariates and pre-treatment average Violent Crime rate.

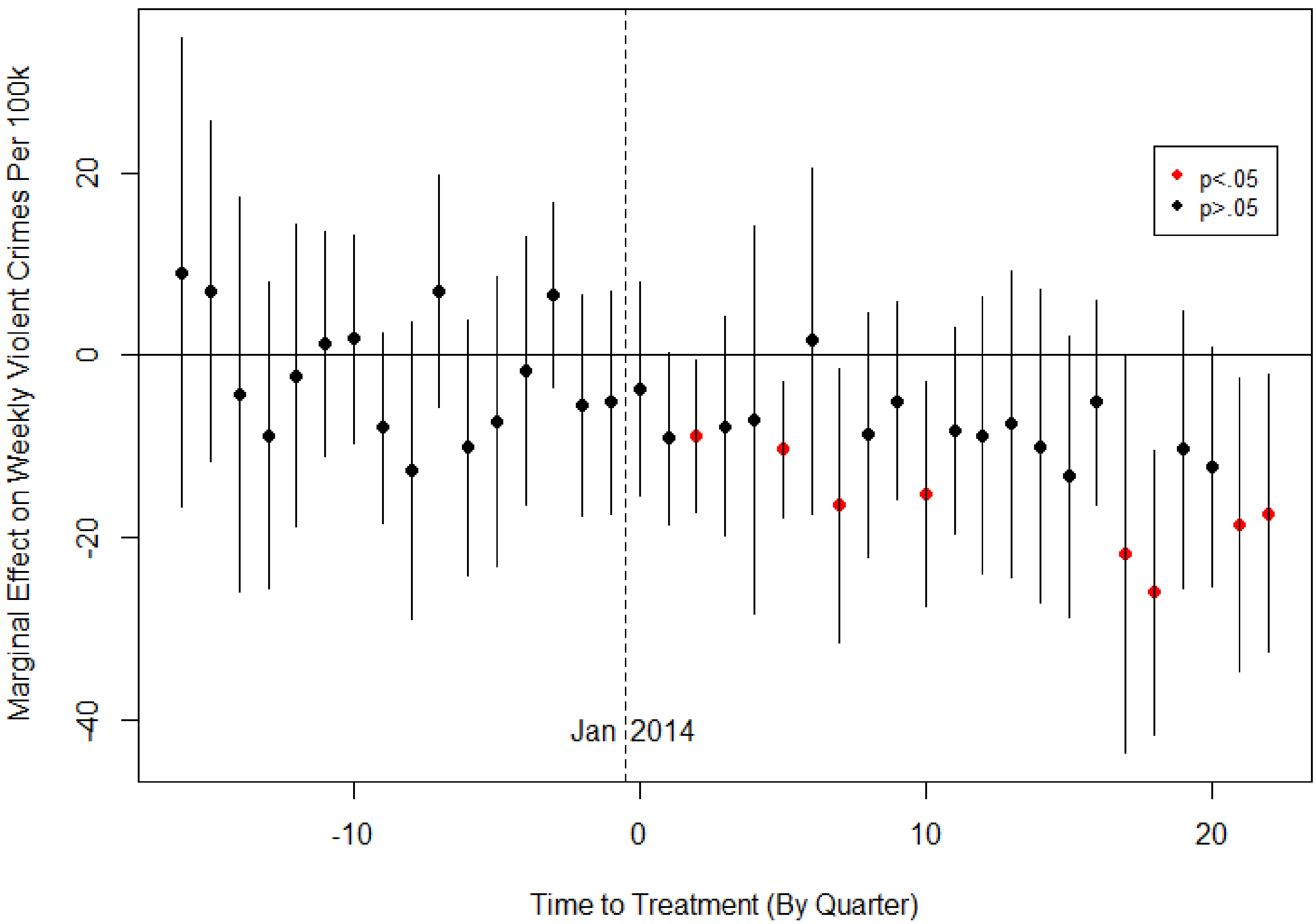

Figure 14: Event Study w/ Full Sample

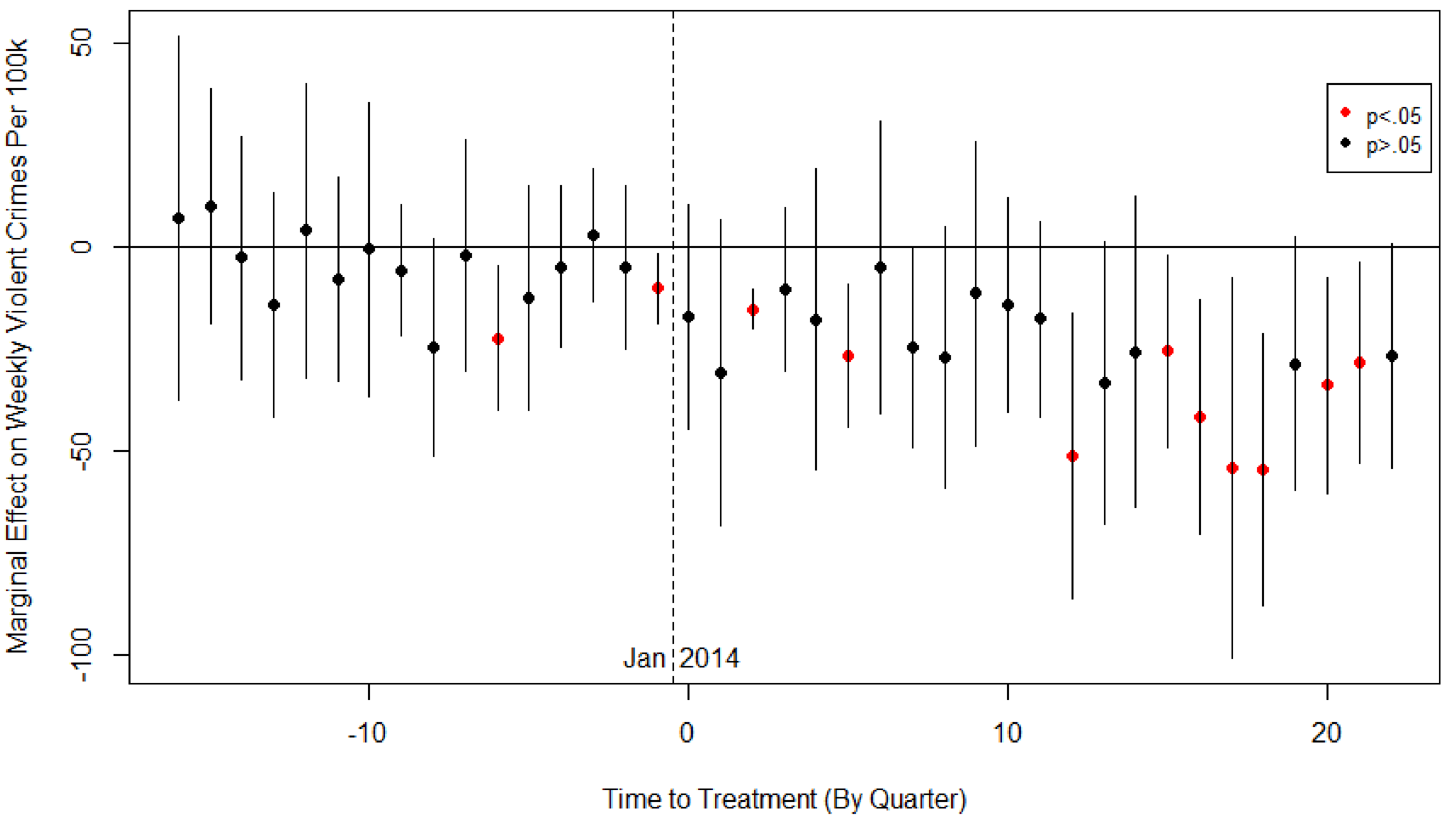

Figure 15: Event Study (Matched on pre-2014 covariates. Tract 90 Excluded.)

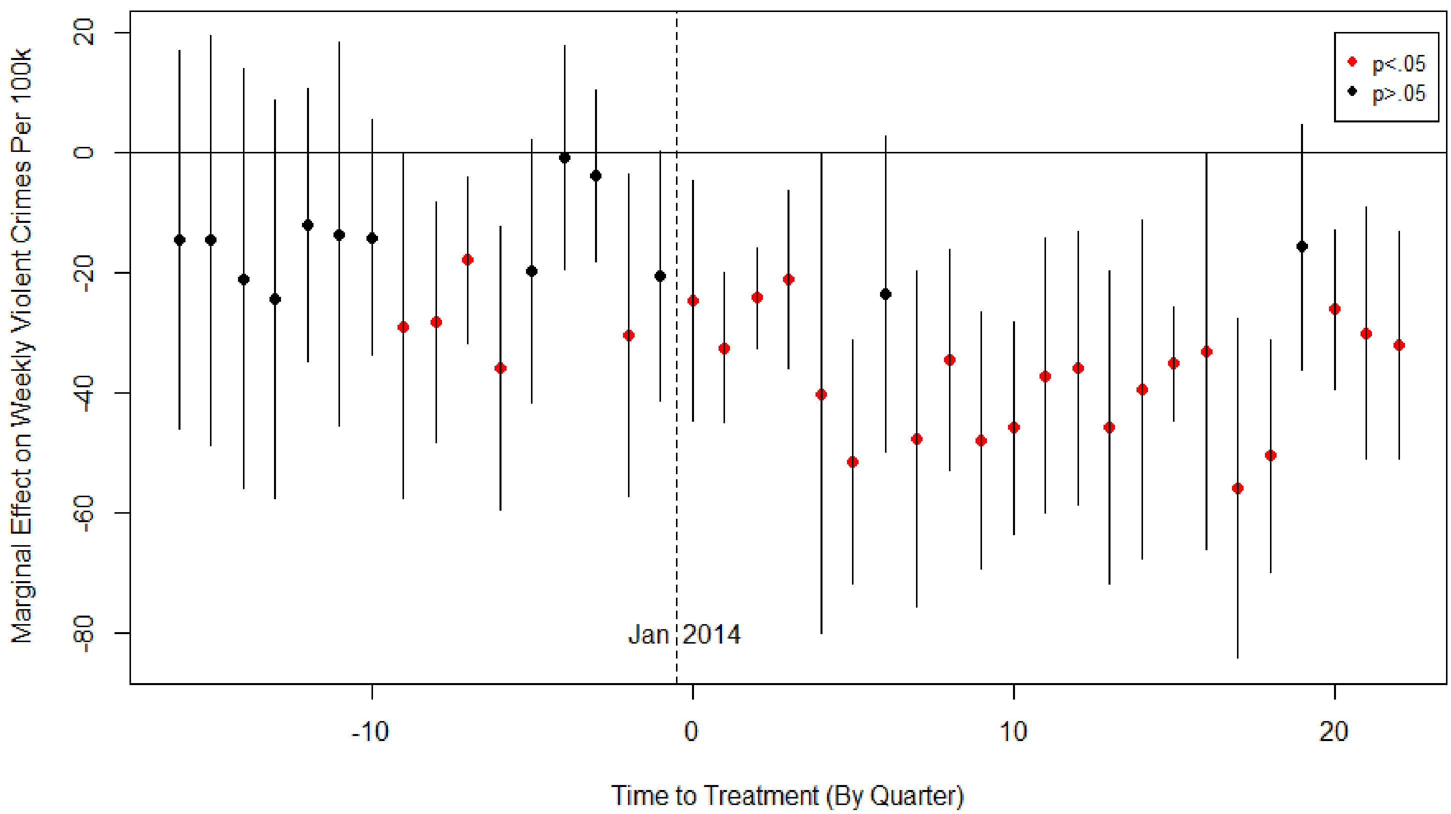

Figure 16: Event Study (Matched on full period covariates. Tract 90 Excluded.)

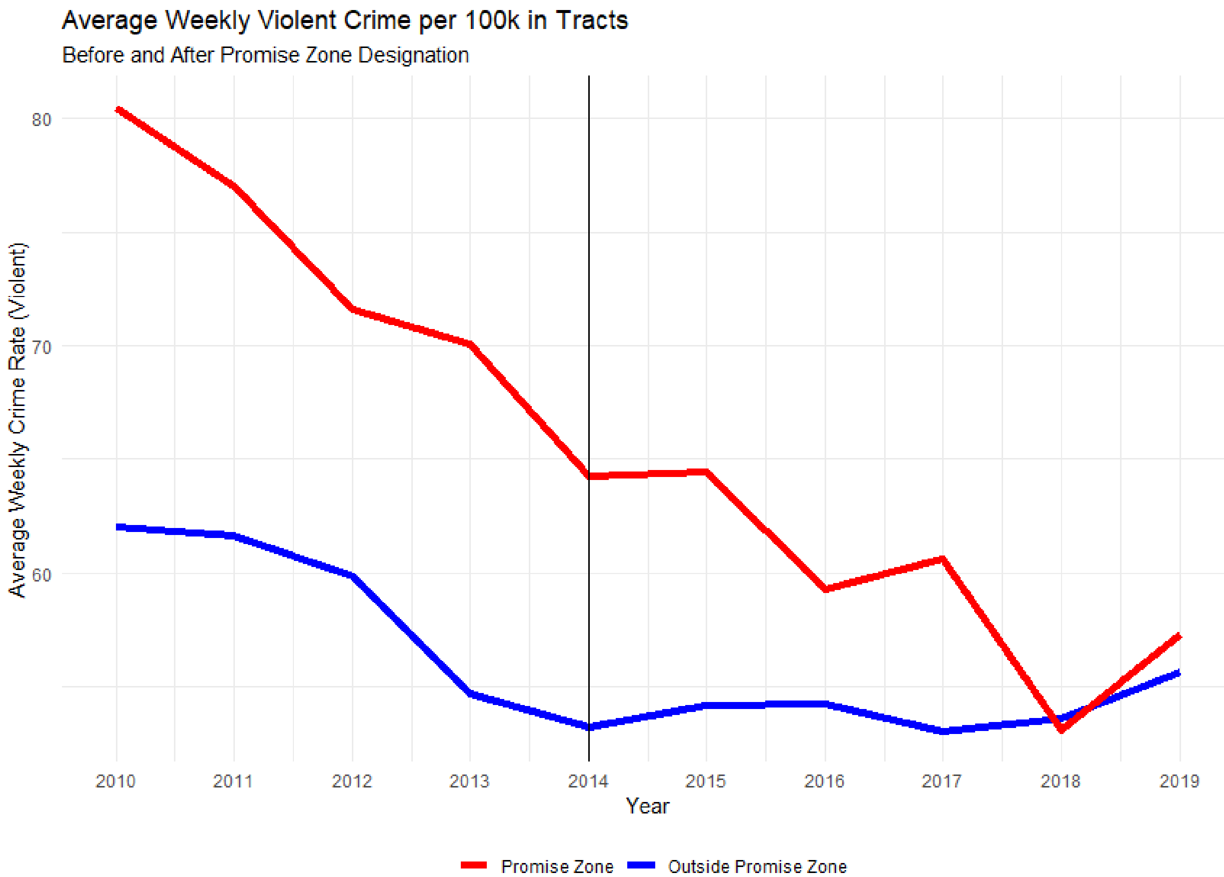

Figure 17: Yearly Trends. Full Sample

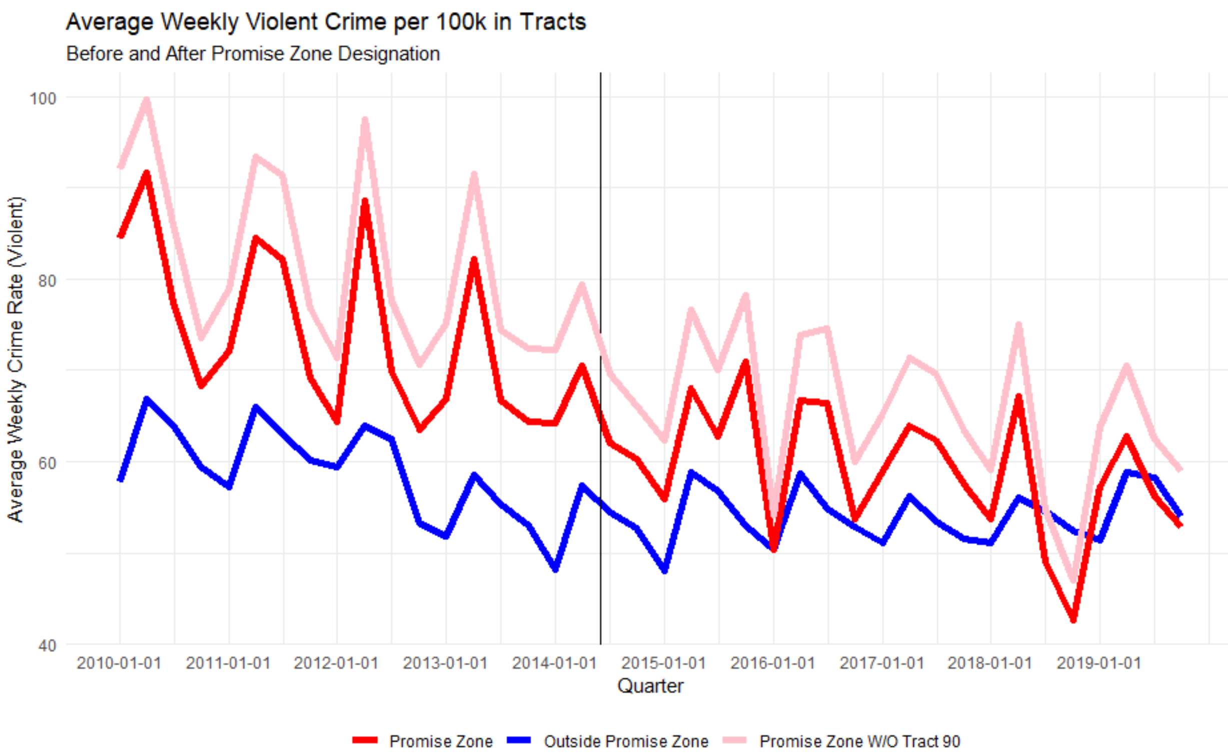

Figure 18: Quarterly Trends. Full Sample

Table 3: Summary Statistics (Tract-Week-Year Observations)

Statistic	N	Mean	St. Dev.	Min	Max
Zone	189,403	0.022	0.147	0	1
After	189,403	0.557	0.497	0	1
Population	189,403	4,174	1,688	242	9,510
% Black	189,403	44.351	35.372	0	100
% Hispanic	189,403	11.827	17.097	0	91.7
% White	189,403	36.093	31.800	0	100
% Married	189,403	28.797	12.553	1.2	66.9
% Low Education	189,403	18.250	11.041	0	62.9
% Poverty	189,403	26.339	15.616	0	92
V. Crimes per 100k	189,403	56.442	62.532	0	1,991
Homicides per 100k	189,403	0.397	3.877	0	329
Assaults per 100k	189,403	32.395	42.405	0	1,202
Agg. Assaults per 100k	189,403	10.673	23.526	0	1,874
Robberies per 100k	189,403	9	20	0	481
Rapes per 100k	189,403	1.452	7.373	0	413
Arson per 100k	189,403	0.616	5.128	0	413

Table 4: Violent Crime in Matched Tracts (Matched on Pre-2014 covariates)

	Dependent variable:			
	Weekly Violent Crimes per 100k Pop.			
	(1)	(2)	(3)	(4)
After	11.901**	18.755***	6.528	15.737*
	(5.871)	(7.207)	(3.450)	(6.814)
Zone	2.012	2.092		
	(7.623)	(7.481)		
% Low Education	0.843*	1.001**	0.188	0.208
	(0.480)	(0.497)	(0.312)	(0.390)
% Black	−0.241	−0.273	−0.370	−0.580
	(0.733)	(0.712)	(0.701)	(0.643)
% White	−0.845	−0.878	−1.121	−1.301
	(0.986)	(0.932)	(1.274)	(1.236)
% Married	−1.104*	−1.053*	−0.210	−0.099
	(0.601)	(0.626)	(0.542)	(0.532)
% Hispanic	1.459	1.100	3.120**	2.877**
	(1.402)	(1.453)	(0.877)	(0.780)
% Poverty	0.151	0.209	0.319	0.460
	(0.196)	(0.179)	(0.294)	(0.256)
After x Zone	−27.051**	−27.658***	−21.252*	−21.699*
	(10.509)	(10.662)	(10.546)	(10.346)
				[0.09]*
Week FE?	X	X	X	X
Tract FE?			X	X
Year FE?		X		X
Observations	7,292	7,292	7,292	7,292
R^2	0.065	0.069	0.136	0.142
Adjusted R^2	0.057	0.060	0.127	0.132
Residual Std. Error	56.736	56.663	54.580	54.438

Note:S.E. are Cluster-Robust at the subclass (pair) level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 5: Violent Crime in Matched Tracts (Matched on full sample covariates)

	Dependent variable:			
	Weekly Violent Crimes per 100k Pop.			
	(1)	(2)	(3)	(4)
After	3.631	11.999*	6.076	10.556**
	(7.480)	(6.296)	(5.048)	(3.632)
Zone	−12.548***	−13.053***		
	(3.971)	(3.571)		
% Low Education	0.567***	0.368**	0.252	−0.119
	(0.127)	(0.179)	(0.160)	(0.197)
% Black	−0.191	−0.276	0.538	0.133
	(0.253)	(0.271)	(0.412)	(0.604)
% White	−0.466	−0.640	−0.222	−0.446
	(0.382)	(0.437)	(0.423)	(0.386)
% Married	−1.343***	−1.460***	0.060	0.020
	(0.485)	(0.495)	(0.326)	(0.339)
% Hispanic	0.502	0.769	0.269	0.218
	(1.164)	(1.330)	(0.673)	(0.881)
% Poverty	−0.146	−0.122	−0.262	−0.233
	(0.268)	(0.288)	(0.171)	(0.163)
After x Zone	−21.030**	−20.787**	−16.712***	−17.907***
	(8.338)	(8.485)	(4.149)	(3.562)
				[0.006]***
Week FE?	X	X	X	X
Tract FE?			X	X
Year FE?		X		X
Observations	7,283	7,283	7,283	7,283
R^2	0.081	0.083	0.112	0.115
Adjusted R^2	0.073	0.074	0.103	0.105
Residual Std. Error	59.939	59.910	58.958	58.888

Note:S.E. are Cluster-Robust at the subclass (pair) level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 6: Violent Crime w/ Demographic Controls (All tracts.)

	Dependent variable:			
	Weekly Violent Crimes per 100k Pop.			
	(1)	(2)	(3)	(4)
After	1.800**	1.925**	2.024**	1.911**
	(0.806)	(0.806)	(0.815)	(0.805)
Zone	15.016*		−13.724	
	(8.467)		(8.527)	
% Low Education			0.781***	0.027
			(0.236)	(0.118)
% Black			0.420**	0.122
			(0.205)	(0.219)
% White			0.352*	0.073
			(0.197)	(0.223)
% Married			−1.012***	0.141
			(0.235)	(0.088)
% Hispanic			0.153	−0.075
			(0.201)	(0.211)
% Poverty			0.427**	−0.061
			(0.167)	(0.078)
After x Zone	−9.645***	−9.420***	−12.214***	−8.706***
	(2.831)	(2.823)	(4.068)	(2.842)
				[0.01]**
Week FE?	X	X	X	X
Tract FE?		X		X
Year FE?	X	X	X	X
Observations	189,403	189,403	189,403	189,403
R^2	0.008	0.365	0.167	0.367
Adjusted R^2	0.008	0.364	0.167	0.366
Residual Std. Error	62.292	49.887	57.087	49.810

Note:S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 7: Violent Crime w/ Demographic Controls (All tracts except 90.)

	Dependent variable:			
	Weekly Violent Crimes per 100k Pop.			
	(1)	(2)	(3)	(4)
After	1.780**	1.907**	1.995**	1.894**
	(0.809)	(0.808)	(0.816)	(0.807)
Zone	22.918***		−6.879	
	(4.560)		(6.214)	
% Low Education			0.729***	0.023
			(0.241)	(0.119)
% Black			0.390*	0.120
			(0.207)	(0.219)
% White			0.332*	0.073
			(0.198)	(0.223)
% Married			−1.031***	0.139
			(0.236)	(0.088)
% Hispanic			0.139	−0.078
			(0.201)	(0.212)
% Poverty			0.456***	−0.059
			(0.170)	(0.079)
After x Zone	−10.888***	−10.635***	−13.325***	−9.876***
	(2.906)	(2.922)	(4.518)	(2.977)
				[0.013]**
Week FE?	X	X	X	X
Tract FE?		X		X
Year FE?	X	X	X	X
Observations	188,884	188,884	188,884	188,884
R^2	0.009	0.364	0.168	0.366
Adjusted R^2	0.009	0.363	0.167	0.365
Residual Std. Error	62.305	49.946	57.094	49.868

Note:S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 8: Poverty w/ Demographic Covariates (All tracts.)

	Dependent variable:			
	poverty			
	(1)	(2)	(3)	(4)
After	−0.153**	−0.060	−0.027	−0.066
	(0.065)	(0.052)	(0.054)	(0.050)
Zone	20.631***		10.112**	
	(3.211)		(4.047)	
% Low Education			0.492***	0.265***
			(0.054)	(0.048)
% Black			−0.072	0.062
			(0.051)	(0.069)
% White			−0.099**	−0.102
			(0.050)	(0.083)
% Married			−0.692***	−0.225***
			(0.056)	(0.040)
% Hispanic			0.031	0.049
			(0.047)	(0.091)
After x Zone	2.945	3.087	1.445	3.200*
	(2.022)	(2.026)	(2.129)	(1.898)
Week FE?	X	X	X	X
Tract FE?		X		X
Year FE?	X	X	X	X
Observations	189,403	189,403	189,403	189,403
R^2	0.046	0.887	0.720	0.896
Adjusted R^2	0.045	0.886	0.720	0.896
Residual Std. Error	15.259	5.267	8.258	5.039

Note: S.E. are Cluster-Robust at the tract level for all regressions. *p<0.1; **p<0.05; ***p<0.01

Table 9: Property Crime w/ Demographic Covariates (All tracts.)

	Dependent variable:			
	propcap			
	(1)	(2)	(3)	(4)
After	2.321*	2.268*	2.620**	2.266*
	(1.301)	(1.286)	(1.291)	(1.309)
Zone	−0.575		−18.091	
	(9.325)		(17.008)	
% Low Education			−0.748	0.109
			(0.565)	(0.277)
% Black			−1.580***	−0.245
			(0.571)	(0.727)
% White			−0.924*	−0.114
			(0.551)	(0.698)
% Married			−2.866***	−0.151
			(0.675)	(0.238)
% Hispanic			−1.160***	−0.268
			(0.417)	(0.624)
% Poverty			−0.024	−0.556***
			(0.349)	(0.212)
After x Zone	−1.805	−1.477	−14.897**	−1.241
	(3.926)	(3.922)	(5.976)	(4.899)
Week FE?	X	X	X	X
Tract FE?		X		X
Year FE?	X	X	X	X
Observations	189,403	189,403	189,403	189,403
R^2	0.013	0.555	0.100	0.555
Adjusted R^2	0.012	0.554	0.099	0.554
Residual Std. Error	99.472	66.873	94.988	66.806

Note:S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 10: % Low Education w/ Demographic Covariates (All tracts.)

	Dependent variable:			
	lessthanhighschool			
	(1)	(2)	(3)	(4)
afterpromise	−0.055	0.009	−0.003	0.004
	(0.046)	(0.038)	(0.037)	(0.035)
mantua	3.240		−0.703	
	(3.410)		(3.321)	
percentblack			−0.063	−0.024
			(0.046)	(0.038)
percentwhite			−0.168***	−0.149***
			(0.047)	(0.036)
percentmarried			0.137***	0.001
			(0.032)	(0.031)
percenthispanic			0.223***	0.023
			(0.050)	(0.047)
poverty			0.291***	0.114***
			(0.037)	(0.022)
afterpromise:mantua	−0.338	−0.310	−0.217	−0.301
	(1.653)	(1.652)	(1.414)	(1.561)
Week FE?	X	X	X	X
Tract FE?		X		X
Year FE?	X	X	X	X
Observations	189,403	189,403	189,403	189,403
R^2	0.024	0.903	0.669	0.910
Adjusted R^2	0.024	0.903	0.669	0.910
Residual Std. Error	10.907	3.435	6.356	3.311
Note:				*p<0.1; **p<0.05; ***p<0.01
				Std. Errors Clustered at tract level.

Table 11: Including BCJI as a Treatment

	Dependent variable:			
	Weekly Violent Crimes per 100k pop.			
	Addt'l Treatment	Mantua	Between	Mantua Between
	(1)	(2)	(3)	(4)
Date $\geq$ 6/1/2012	-2.792^{***}	-2.853^{***}		
	(0.820)	(0.815)		
5/31/2014 $\geq$ Date $\geq$ 6/1/2012			-2.792^{***}	-2.876^{***}
			(0.820)	(0.824)
Date $\geq$ 6/1/2014	1.524^{*}	1.558^{*}	-1.268	-1.305
	(0.817)	(0.814)	(1.238)	(1.241)
Date $\geq$ 6/1/2012 x Zone	-3.104			
	(3.326)			
Date $\geq$ 6/1/2012 x Mantua		1.121		
		(2.466)		
5/31/2014 $\geq$ Date $\geq$ 6/1/2012 x Zone			-3.104	
			(3.326)	
5/31/2014 $\geq$ Date $\geq$ 6/1/2012 x Mantua				-2.991
				(6.346)
Date $\geq$ 6/1/2014 x Zone	-5.351	-6.896^{***}	-8.455^{***}	-6.714^{***}
	(3.368)	(2.521)	(2.786)	(2.284)
	[0.192]	[0.032]**	[0.007]***	[0.02]**
Week FE?	X	X	X	X
Tract FE?	X	X	X	X
Year FE?	X	X	X	X
Demographic Controls?	X	X	X	X
Observations	189,403	189,403	189,403	189,403
R^2	0.367	0.367	0.367	0.367
Adjusted R^2	0.366	0.366	0.366	0.366
Residual Std. Error	49.808	49.808	49.808	49.808

Note: S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed at the tract level with 9999 iterations. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 12: Disaggregated Violent Crimes

	Dependent variable: Weekly Violent Crimes per 100k Pop.						
	Violent	Homicide	Assault	Agg. Assault	Robbery	Rape	Arson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After	1.911**	0.021	2.447***	−0.140	−0.205	−0.0004	−0.024
	(0.805)	(0.051)	(0.611)	(0.291)	(0.272)	(0.114)	(0.083)
After x Zone	−8.706***	−0.078	−5.355**	−2.333***	−0.784	−0.089	0.151
	(2.842)	(0.203)	(2.218)	(0.853)	(1.044)	(0.335)	(0.213)
	[0.01]**		[0.041]**	[0.026]**			
Tract FE?	X	X	X	X	X	X	X
Week FE?	X	X	X	X	X	X	X
Year FE?	X	X	X	X	X	X	X
Demographic Ctrls?	X	X	X	X	X	X	X
Observations	189,403	189,403	189,403	189,403	189,403	189,403	189,403
R^2	0.367	0.013	0.254	0.120	0.146	0.024	0.013
Adjusted R^2	0.366	0.010	0.253	0.118	0.144	0.021	0.011
Residual Std. Error	49.810	3.857	36.662	22.091	18.706	7.294	5.099

Note: S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed with 9999 iterations at the tract level.. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 13: Disaggregated Violent Crimes (Matched on Pre-2014 Covariates)

	Dependent variable: Weekly Violent Crimes per 100k Pop.						
	Violent	Homicide	Assault	Agg. Assault	Robbery	Rape	Arson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After	15.737*	0.132	12.102*	2.201	−0.287	1.487*	0.158
	(6.814)	(0.322)	(5.740)	(1.764)	(1.576)	(0.747)	(0.236)
After x Zone	−21.699*	−0.393	−15.788**	−3.587	−1.984	−0.256	−0.013
	(10.346)	(0.342)	(6.291)	(1.920)	(1.927)	(0.364)	(0.449)
	[0.09]*		[0.028]**				
Tract FE?	X	X	X	X	X	X	X
Week FE?	X	X	X	X	X	X	X
Year FE?	X	X	X	X	X	X	X
Observations	7,150	7,150	7,150	7,150	7,150	7,150	7,150
R^2	0.118	0.016	0.084	0.058	0.049	0.020	0.016
Adjusted R^2	0.108	0.004	0.074	0.047	0.037	0.008	0.005
Residual Std. Error	60.692	5.617	45.650	28.131	21.743	8.534	7.658

Note: S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed with 9999 iterations at the tract level.. This is included as a robustness check and discussed in the appendix. *p<0.1; **p<0.05; ***p<0.01

Table 14: Disaggregated Violent Crimes (Matched on Full Period Covariates)

	Dependent variable: Weekly Violent Crimes per 100k Pop.						
	Violent	Homicide	Assault	Agg. Assault	Robbery	Rape	Arson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After	10.556**	−0.114	8.512***	0.973	−0.733	1.015	0.131
	(3.632)	(0.259)	(2.275)	(2.672)	(1.514)	(0.688)	(0.372)
After x Zone	−17.907***	0.317	−14.330***	−2.183	−0.945	−0.301	0.143
	(3.562)	(0.314)	(2.744)	(2.041)	(1.247)	(0.509)	(0.323)
	[0.006]***		[0.006]***				
Tract FE?	X	X	X	X	X	X	X
Week FE?	X	X	X	X	X	X	X
Year FE?	X	X	X	X	X	X	X
Demographic Ctrls?	X	X	X	X	X	X	X
Observations	7,283	7,283	7,283	7,283	7,283	7,283	7,283
R^2	0.115	0.016	0.127	0.045	0.067	0.021	0.015
Adjusted R^2	0.105	0.005	0.117	0.034	0.056	0.009	0.004
Residual Std. Error	58.888	5.830	43.415	28.586	20.911	9.108	5.425

Note: S.E. are Cluster-Robust at the tract level for all regressions. The square bracket contains the p-value from a Webb Six-Point Method Wild Cluster Bootstrap that is performed with 9999 iterations at the tract level.. This is included as a robustness check and discussed in the appendix. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Appendix A

.1 Regarding Causal Inference and Standard Errors

One of the problems with standard error clustering is that there is no consensus over either the proper degrees of freedom to use, or the type of heteroskedasticity-robust standard errors a project such as this should be employing. This is even demonstrated by the idiosyncracies among default degrees of freedom and heteroskedasticity calculations done in R and Stata (this project uses R). It is reasonable to expect that regressors and errors would be correlated among within-tract observations. This is why Austin and Small (2014) and Abadie and Spiess (2021) both argue that, when building a model on propensity score matched data, cluster-robust standard errors at the *pair* membership level should be employed. Since, after matching, each tract is very similar to its partner in the subclass, we would expect the errors to be correlated within each pair, as opposed to more specifically in each tract. Pair membership is determined by subclass in the Matching algorithm. For example, never treated Tract 67 was paired with eventually treated Tract 106. Ho et al. (2007) demonstrate that matching can be used to process data, after which you can use standard regression techniques you "would have used anyway". That being said, errors are ultimately likely correlated across tract borders. On one hand, it could be argued that since tracts themselves are not particularly chosen for treatment, and it is in fact a square area of Philadelphia that received this designation based on factors inherent to the area as a whole, the standard errors provided by clustering at the tract (or pair) level may be too large. Clustering alternatively on the binary variable of "Zone" would cause a regression with many thousands of observations to have 1 degree of freedom[1], making it nearly impossible to discern significance on the treatment effect. There is a general consensus that clustering at the tract level may give overly conservative estimates of the standard errors if errors are not correlated within tracts. Abadie et al. (2017) demonstrate that if treatment is not cluster-specific, then clustered standard errors may be too conservative, making rejection of the null unattainable even if there is a "real" treatment effect. After including tract fixed effects and tract-level demographic controls, it is not likely that the errors need a tract-level correction. For that reason, I also employ alternative standard errors,

[1] Generally speaking, while non-clustered SEs use n-k degrees of freedom to determine p-value, clustered SEs with G clusters result in G-1 (or G-k) degrees of freedom, resulting in larger p-values. Cameron and Miller (2015)

which are hetero-skedasticity robust. These standard errors provide p-values that are significant at the %1 level for the treatment effect in all models, matched and unmatched.

Another problem with clustering is that it can be unreliable in instances where there are fewer than fifty clusters in the dataset. In this case, with only fourteen tracts and pairwise clustering, there are only seven clusters. According to Cameron and Miller (2015), when there are more regressors than clusters – these models have anywhere from 60 to 69 regressors– the cluster-robust variance matrix becomes rank-deficient. Despite this, individual analysis of regression coefficients can still be performed. Cameron and Miller (2015) claims that having few clusters does not necessarily make regression coefficients imprecise if there are many observations per cluster, but the standard errors are likely to become biased downward. For the most robust standard errors when there are fewer than ten treated clusters, they recommend following the Webb six-point method of wild cluster bootstrapped standard errors from Webb (2014).[2]

In any case I tested with standard heteroskedasticity robust and cluster-robust standard errors on all models (the latter of which are displayed in the tables), and included Webb Six-Point Wild Cluster Bootstrapped standard errors on any fully specified model showing significance. In all cases, the hetero-robust errors were the least conservative, often giving p-values substantially smaller than the cluster-robust and bootstrapped errors (typically one magnitude of confidence smaller, e.g. 0.05 to 0.01). On the other side were the bootstrapped errors, which were extremely conservative and generally increased p-values by one magnitude (e.g. 0.05 to 0.1).

.2 Grant Information

The following are the verbatim[3] descriptions of each grant:

- Face Forward 2: *This grant will provide diversion programs for 208 youth from 14 to 24 years in lieu of adjudication for delinquency and expungement services. Other services include case management services, academic and work aptitude assessment, career planning services, GED preparation, academic assistance, and certified vocation training. Subgrantee Juvenile Justice*

[2]Even in the full sample regression with 374 clusters, having only 7 to 8 clusters being treated also falls under the "too few clusters" problem as explained by Cameron and Miller (2015). Therefore wild cluster bootstrap standard errors are utilized in those models as well.

[3]Once again, I would like to thank Nicole Teufel and Brooke Garcher for providing this descriptive writeup of the grants.

Center Family Services operates 1/3 of this grant in the Promise Zone for Promise Zone residents. The remainder of the grant is administered in Camden, NJ (VOA) and Chester, PA (Boys and Girls Club of Chester).

- Training to Work 1 and 3: *Provide 250 to 275 returning offenders ages 18 and up with GED preparation and testing services, academic and work aptitude assessment, career planning services, and certified vocation training. These are accompanied by life skills/job preparation training, community service projects, and supportive services including, as needed, substance abuse treatment/counseling; family counseling; housing assistance; legal assistance; mental health; medical; child care and child support arrangements, and others.*

- Our Town: *Project supports theatre performances to heighten community awareness around the impact of traumatic experiences and how it affects individual decision making. The resident interventions are designed to foster the natural, human tendency to group together to seek safety and solace after traumatic incidents, reduce acute traumatic stress among survivors, and increase awareness of community-based resources.*