REStat NBER Summer Institute Fellowship Committee
Cambridge, MA 02138

March 06, 2023

Hello

Thank you for considering my application for the Review of Economics and Statistics Fellowship for the NBER Summer Institute. Genuinely, having the opportunity to attend and participate in the NBER Summer Institute meetings would be a dream. I've had a bit of an unconventional path to get to my current position, constantly trying to work my way upward on the track to situate myself in this profession. As such, this is an opportunity I am very excited by.

To the best of knowledge, I am the only formerly incarcerated person working in applied microeconomics. I try and make the most of the experiences I have had before, after, and during my years in prison in informing the questions I ask as I pursue a research agenda in the economics of crime. In my five years since being released from prison, I ave completed a Bachelors degree at a state university in rural Pennsylvania and a Masters degree and PhD at West Virginia University (defended February 07, 2023). I am now preparing to start a two and a half year post-doc with Bocconi University's CLEAN unit next month, where I will continue to pursue my research agenda centered on the interplay between urban violence and the markets for illegal drugs.

I would love the chance to attend the crime or law and economics sessions, but genuinely the chance to attend any would be a blessing. I am eager to expand my network and to be exposed to research at the forefront of these fields. Further, I do believe that my unique perspective and insights could be valuable to others working in this area.

I have attached a current copy of my CV along with a copy of my job market paper, "Gentrification, Gun Violence, and Drug Markets", which was recently published in *Journal of Economic Behavior and Organization*. Current projects that I am working on include an exploration of the displacement and general equilibrium effects of purely supply side interventions in the markets for illegal drugs (co-authored with Aaron Chalfin), an analysis of the impacts of the introduction of a right to counsel for the accused in 19th century London (co-authored with Bryan McCannon), and a very early stage project looking at the exportation of the Sicilian Mafia to America and its effects on local economic development. I want to build my career in the field of economics of crime and law and economics, so I know that I will make the most of this opportunity if it is afforded to me.

Thank you for taking the time to review my application materials. More information can be found on my personal website: zachporreca.github.io. If you have any questions or would like any other information, please do not hesitate to reach out.

Best regards,

Zachary Porreca

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ZACHARY PORRECA

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ACADEMIC POSITIONS

Bocconi University
Post-Doctoral Research Fellow *Spring 2023-present*
Affiliations: Researcher at CLEAN Research Unit
Fellow at BAFFI CAREFIN Centre

EDUCATION

West Virginia University
Doctor of Philosophy, Economics *Spring 2023*
Fields: (Public and Urban/Regional Economics)
Master of Arts, Economics *Summer 2022*

Bloomsburg University
Bachelor of Arts, Economics *Spring 2020*

PUBLICATIONS

"Gentrification, Gun Violence, and Drug Markets"

(*Journal of Economic Behavior and Organization*, 2023)

"Synthetic Difference-In-Differences Estimation With Staggered Treatment Timing"

(*Economics Letters*, 2022)

"Assessing Ocean Temperature's Role in Fishery Production"

(*Journal of Bioeconomics*, 2021)

WORKING PAPERS

"The Right to Counsel: Criminal Prosecution in 19th Century London" *(under review)*

with Bryan McCannon

"Three Golden Balls: Pawn Shops and Crime" *(under review)*

with Bryan McCannon and Zachary Rodriguez

PRESENTATIONS

2023 Johannes Kepler University, Linz *(scheduled)*
2023 Masaryk University *(scheduled)*
2022 Bocconi University, Crime Before Christmas Workshop
2022 Southern Economic Association Conference
2022 Ethics, Law, and Social Science of Self-Defense and Firearms Workshop at Saint Anselm College
2022 Slovak Economic Association Meeting (presented virtually)
2022 Eastern Economic Association Conference
2022 Institute for Humane Studies Graduate Conference
2022 Association of Private Enterprise Education Conference
2022 Texas Economics of Crime Workshop
2021 24 Hour Conference on Global Organized Crime
2021 North American Association of Fisheries Economists Forum (COVID-19 Webinar Series)
2019 Bloomsburg University College of Liberal Arts Symposium

TEACHING EXPERIENCE- CLASSROOM

West Virginia University

ECON 201: Introductory Microeconomics (Instructor of Record) (Online and asynchronous)	Spring 2023
ECON 225- Elementary Business and Economics Statistics (Instructor of Record)	Fall 2022
ECON 711- Advanced Microeconomic Theory II (Teaching Assistant)	Spring 2022
ECON 701- Advanced Microeconomic Theory I (Teaching Assistant)	Fall 2021
Undergraduate Research Mentor Advancing Diversity in Geriatric Research program	Summer 2021
ARE 187- Energy Economics (Teaching Assistant)	Fall 2020

TEACHING EXPERIENCE- WORKSHOP

Center for Free Enterprise Summer Empirical Workshop

Text Analysis and Data Basics in R — Summer 2022

University of Sassari and University of Cagliari

Text Analysis with Economic Applications Workshop — Spring 2022

HONORS, AWARDS, AND GRANTS

Institute for Humane Studies Fellowship (2023)	Junior Summer Research Fellowship
Mercatus Center Fellowship (2022)	Oskar Morgenstern Fellow
Institute for Humane Studies Fellowship (2022)	Publication Accelerator Grant
APEE Young Scholars Program (2022)	
Center for Free Enterprise Grant (2021)	For purchase of proprietary data set
Institute for Humane Studies Fellowship (2021)	Daniel Searle Fellow
Phi Kappa Phi Graduate Fellowship (2020)	
Adam Smith Award in Economics (2020)	Bloomsburg University award
Phi Kappa Phi Honor's Society Inductee (2019)	
Margaret Minner Memorial Scholarship (2019)	
Appointed "Lead Environmental Advisor to Mayor's Office" (2019)	Affidavit from mayor of Bridgeport, PA

PROFESSIONAL EXPERIENCE

Sustainability Advisory Committee — 2018-2020
Appointee — *Bridgeport, Pennsylvania*

AMS Law, P.C. — 2019-2020
Legal Assistant — *Norristown, Pennsylvania*

MEMBERSHIPS

American Economic Association, Eastern Economic Association, Association of Private Enterprise Education, North American Association of Fisheries Economists

TECHNICAL SKILLS

R, LaTeX, Stata, Textual Analysis, Spatial Analysis, Causal Inference, High Performance Computing

INVITED EVENTS AS A PARTICIPANT

Institute for Humane Studies Summer Seminar Series Summer 2022

American Institute for Economic Research Spring 2022
Harwood Graduate Colloquium

VOLUNTEERING ACTIVITIES

Rocksteady Boxing	Boxing lessons to improve mobility in Parkinson's Disease patients
Schuylkill River Banks	Projects to revitalize the areas along the urban Schuylkill River
APBP	Matching and sending books to prison inmates across Appalachia

SERVICE TO THE PROFESSION

Ad Hoc Journal Referee	*Applied Economics*; *Marine Resource Economics*; *Journal of Regional Analysis and Policy*
R Package Contributor	In bacondecomp package, implementation of fixest::feols processes
R Repository Author	Implementation of staggered treatment timing for Synth-DiD Estimation

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Gentrification, gun violence, and drug markets

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ABSTRACT

I demonstrate the linkage between gentrification and gun violence. As the underlying mechanism, I focus on gentrification's displacement effect on local drug markets. Using two-way fixed effects differences-in-differences estimators, I show the gentrification of one block increases violence across the surrounding neighborhood. I find that some 2400 (8%) of Philadelphia's shootings between the years 2011 and 2020 can be attributed to spillover effects from the gentrification of drug blocks. This effect is nearly ten times stronger than that observed on blocks without high levels of drug crime. This study also contributes a new empirical measurement of gentrification drawn primarily from property sales, along with building, zoning, and alteration permit issuance and utilizes a novel nearest-neighbor network approach to identify spatial spillover effects.

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1. Introduction

Gentrification and rapid urban development change the landscape of a city, bringing new amenities and new residents into traditionally neglected neighborhoods. However, this development also has the potential to bring with it spatial spillovers and negative externalities, as crime may be pushed away from newly developed blocks outward into the surrounding neighborhood. In this paper, I seek to address this question empirically: does gentrification create spatial spillovers that cause increases in crime in the surrounding, yet to be redeveloped, neighborhood?

Across urban America, competition among criminal actors and organizations visibly manifests itself through incidents of gun violence. Despite the existence of incentives for criminals to coordinate with one another, organized outcomes often to fail to emerge. I argue that this failure to obtain cooperative outcomes is a product of the instability of officially unenforceable property rights in illegal criminal markets. Gentrification contributes to this instability, as the redevelopment of city blocks that once supported such illicit markets pushes criminals towards more intensive competition for the remaining blocks that are capable of sustaining their illegal activity.

In this study I demonstrate the linkage between gentrification and gun violence. Using city-block level data from Philadelphia, I specify a two-way fixed effects differences-in-differences estimator to empirically validate the relationship between the gentrification of one block and levels of gun violence across the surrounding neighborhood. I find a robust result showing that on average gentrification increases levels of gun violence in neighbor blocks. This effect is more pronounced when the gentrified block has a history of drug crime, with an average increase of nearly nine shootings in the surrounding neigh-

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borhood. Back of the envelope calculations suggest that roughly 5800 shootings, or 21% of the city's shootings across the ten year study window can be attributed to spillover effects from gentrification, while 2400 (or 8%) can be attributed to the gentrification of drug blocks. I find that gentrification has a statistically and economically significant effect on rates of gun violence in the neighborhood surrounding a newly gentrified block. I then document a potential mechanism for this effect, showing that following a block's gentrification drug crime tends to increase on surrounding blocks and decrease on the newly gentrified block. Intuitively, this indicates that drug crime that existed on a block pre-gentrification is pushed into the surrounding neighborhood by the new development. This in turn increases the intensity of competition in the surrounding neighborhood and leads to the increases in gun violence documented herein.

While I am not the first to explore the relationship between gentrification and crime, this paper is indeed the first to study the impact of gentrification on crime displacement. O'Sullivan (2005) provides a theoretical model positing a causal relationship between exogenously falling crime rates and an influx of higher income residents. The author went further, to assert that gentrification is self-reinforcing in that the influx of higher income residents will further decrease crime rates, which further incentivizes an even greater displacement of low income residents in favor of those with a higher income. Similarly, Ellen et al. (2019) finds that reductions in crime rates invite increases in rates of wealthy and educated residents moving into the central city. While these works examine how crime effects the residential decisions that lead to gentrification, they do no consider how gentrification can effect existing neighborhood dynamics. As such, I examine the effect that gentrification will have on crime rates on the city's "frontiers"; blocks that are newly beginning the process of gentrification. My approach is unique in its focus on gentrification's impact on high drug crime blocks. Gentrification causes increasingly tense competition in concentrated drug markets. This competition in turn leads to increasing levels of neighborhood violence.

Numerous authors have empirically explored the relationship between gentrification and crime. Papachristos et al. (2011) regresses homicide and robbery rates on a distinct proxy for gentrification: the number of coffee shops in a neighborhood. The authors find that their markers of gentrification are associated with declining homicide rates, but increasing robbery rates. Smith (2012) utilizes a similar approach, regressing gang related homicides on neighborhood demographic factors, coffee shops, and an indicator for the demolition of public housing. The author finds decreases in homicides as markers of gentrification grow more prevalent. More recently, Autor et al. (2017) finds that rapid gentrification precipitated by the end of rent control in Cambridge, Massachusetts led to significant decreases in the overall crime rate. My result instead utilizes a novel nearest-neighbor network approach to focus on the spatial spillovers of crime wrought by gentrification. While I similarly find that gentrification reduces drug crime on the immediate gentrified block, there is a spatial spillover which causes adjacent non-gentrified areas to experience spikes in both drug crime and shootings.

Neighborhood, census tract, or other artificially drawn borders and boundaries do not necessarily capture the reality of a city. A city is a collection of streets, the lattice-work of their intersections creating the individual blocks that serve as the base level points from which change across a city can be observed. Violence, gentrification, and crime begin as extremely localized phenomena. As such, I examine block level variation in relevant markers and leverage this as a unique setting for identification: taking block level gentrification as an exogenous treatment so as to allow plausibly causal inferences to be made regarding the effect that gentrification has on gun violence.

This study provides a counter-intuitive result that runs against the major current of gentrification and crime related literature. Gentrification imposes a trade-off between the positive externalities of increased economic activity and the negative externality of increased gun violence in the surrounding community. These spikes in new real estate interest push violence outward and into the surrounding neighborhood. Much of this increase in violence may be able to be explained as the result of competition in the illegal drug markets that exist within pockets of the urban environment. The peculiarities of this sort of competition have been largely unexamined in the literature. As such, I offer a new insight into the spatial dynamics of gun violence with implications for urban planning, development, and policing.

Skaperdas (2001), Varese (2010), and Skarbek (2016) have all posited similarly that organized criminal governance is more likely to arise in settings in which official forms of governance fail to provide for the interests of all sub-populations. However, the presence of such an official failing does not guarantee that the organized outcome of criminal governance will arise. Skaperdas (2001) provides a reason for the failure to cooperate: that criminal competition more closely resembles an "arms race", where market share can be taken through violent means that decrease overall market welfare. In this study, I provide empirical backing to Skaperdas's proposition. I demonstrate that in illegal drug markets, lacking official governance, a destabilizing shock leads to a ramping up of this "arm's race" which in turn causes eruptions in violence as the market finds a new cooperative equilibrium.

What is often viewed as an improvement to an area is able to cause increases in violence. I theorize that the failure to cooperate in this competitive environment is a product of the ability of a criminal organization to gain market share through aggressive and militaristic tactics. The likelihood that all firms across a market should elect not to engage in such strategies decreases as the physical extent of the market to be competed for decreases. Exogenous shocks such as gentrification are responsible for this market concentration and give rise to the increases in gun violence observed.

Several other recent studies have taken a similar approach in examining spatial dimensions of competition among criminal organizations.[15] DeAngelo (2012) builds a theoretical model of spatial competition, and with this model predicts that

[15] Skaperdas (1992) and Polo (2005) provide important theoretical contributions regarding violence as a competitive strategy among criminal organizations and in settings absent formal property rights.

increases in law enforcement can deter lower productivity criminals' entrance into the market. Sobrino (2019) examines Mexican drug cartel violence as a competition for control over lucrative territory. She finds empirically that violence coincided with the entrance of additional cartels into a municipality. Castillo et al. (2020) also examines Mexican cartel violence, modeling municipal level competition as a contest for revenue. They find that scarcity caused by cocaine seizures led to significant increases in levels of violence. Bruhn (2021) examines gang presence across the city of Chicago. He finds that as a gang enters new territory, levels of crime (including violent crime) in that area increase. While these studies focus on the entrance and exit of competitors and the impact of supply shocks on the illegal market, I document the impact that changes in the availability of officially unenforceable territorial claims has on the dynamics of competition.

This paper is the first to document gentrification's displacement effect on urban crime. This result, while running contrary to related literature that posits a negative relationship between gentrification and crime, suggests that the extension of urban development to low income areas may create negative externalities as drug and violent crime is displaced into increasingly concentrated adjacent yet to be redeveloped neighborhoods. I further contribute a new easily replicable empirical measurement of gentrification, drawn primarily from property sales along with building, zoning, and alteration permit issuance. This new measurement is able to capture gentrification at its narrowest and most realistic resolution: the individual block level.

2. Hypothesis

The markets for illegal goods are notably absent formal regulation. There is no protection of property rights and no legal enforcement of contracts or territorial claims. This unregulated market allows criminal organizations to compete for profit both through the traditional vehicles of price and quantity as well as through competitive strategies that are unparalleled in traditional market dynamics. The forcible seizure of a rival's assets or territory is a very real possibility.

Particular areas of a city are more capable than others of sustaining a drug market. This may be due to physical characteristics, ease of customer access, or even simply a lack of formal oversight (Eck, 1994). Because of the inherent limits to the potential area of a drug market, any shock that reduces its total area will be a disruptive and destabilizing force that increases the value of the city blocks that make up the remainder of the viable set for the market. An organization that loses its territory will likely attempt to secure new territory elsewhere in the remaining blocks, rather than exit the market entirely. This can lead to increases in violence through either the direct engagement of a rival organization in an attempt to seize territory through military effort or by the increase in tension that can arise from the closer spatial proximity of rival organizations.

Gentrification, which entails the arrival of new residents of higher socio-economic standing to a traditionally lower income neighborhood, is this sort of destabilizing force. Ellen et al. (2019) found that gentrified neighborhoods grow wealthier, more educated, exhibit higher rates of home ownership, and experience significant racial demographic changes. This can be inferred to represent a replacement of many of the neighborhood's original residents; thus making it more difficult for a criminal organization to find the level of community support necessary to operate openly. Further, empirical evidence has demonstrated that gentrification leads to increased policing and the adoption of more punitive policing practices (Laniyonu, 2017). It is reasonable to believe that increases in policing would deter criminal activity, as famously predicted by Becker (1968). Regardless of the actual mechanism at play, it is reasonable to assume that gentrification will make a block less suitable for drug competition. As such it is expected that the gentrification of a block within a drug market will push criminal organizations to the surrounding neighborhood, with the potential to cause spikes in violence.

Due to the geographic limitations on the portions of a city in which a drug market is able to be sustained, it is expected that a displaced criminal organization will be forced to find new territory within the immediate surrounding blocks as this area has already proven itself as capable of sustaining drug activity. Local knowledge, some level of community support, and access to a proven clientele contribute to this locality constraint and increase the likelihood that the gentrification of former drug blocks will lead to violence in the surrounding neighborhood.

3. Data

3.1. Description of data

The City of Philadelphia has made the majority of the data utilized in this study publicly available through their OpenDataPhilly portal. I downloaded Shapefiles representing the city's Census Block Groups, neighborhoods, and street corner intersections independently and mapped them to one another. I further utilized the Philadelphia Police Department's (PPD) crime incidents data set, the city's Department of Licensing and Inspections' (DLI) building and zoning permits data set, and the city's Office of Property Assessment's (OPA) property data set. Data from the years 2010–2020 are used.[1]

The OPA property assessment data includes geocoded locations of each property and the date of that property's last sale. Each property is mapped to the city block it is found on.

[1] Only the years 2011–2020 were actually made use of in the study, so as to limit the study period to an even ten years. 2010 data was used solely for determining lags and deltas of variables.

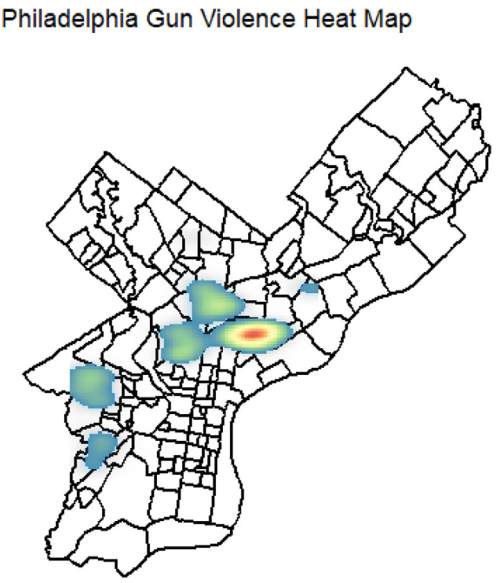


Fig. 1. Heat map depicting the city's highest gun violence clusters.

Table 1
Summary statistics for block level variables. Note that each observation in the table is for an individual block.

Statistic	N	Mean	St. Dev.	Min	Max
Number of Shootings	251,130	0.109	0.461	0	69
Number of Drug Crimes	251,130	0.386	2.244	0	163
Property Sales	251,130	0.174	0.653	0	32
Building, Zoning, and Alteration Permits	251,130	1.967	5.269	0	461

The DLI building and zoning permit data contains records of each permit issued within the city. The permits consist of structural alterations, demolitions, new construction, and change of zoning status permits. Each record has the date of the permit's issuance, it's permit type, and the geocoded location of the property that the permit pertains to. Each permit is mapped and assigned to its constituent city block.

The PPD crime incidents data contains incident dates, locations, and a brief descriptor of the type of incident. Here, only those incidents described as "aggravated assault-firearm", "homicide- criminal", and "narcotic/ drug law violations" were kept. Each crime is mapped to the city block that it occurred on. There are a total of 25,113 individual city blocks examined over a period of ten years.

It is of note that each year roughly fifty homicides occur in the City of Philadelphia that are not firearm related. As such, as much as 3% of what are here identified as shootings may be mislabeled.[2] However, it is likely that more shootings than this go unreported each year.[3] The extent of this under-reporting is unclear, as shootings without wounded victims may not attract police attention when on average five people are shot in the city each day.

The map below shows the locations of Philadelphia's highest gun violence areas. These are those blocks experiencing a level of neighborhood shooting total greater than three standard deviations above the city-wide mean. The locations of these blocks are overlaid on a map of the city's officially designated neighborhoods, the boundaries of which are primarily the city's main surface streets (Fig. 1).

Summary statistics for these series are displayed in the table below (Table 1).

3.2. Dyads and the directional network of city blocks

From this block level data, I build a directional spatial network in order to avoid having the same incidents counted multiple times. Each block is linked to its 24 closest neighbors. This is equivalent to a two block radius surrounding each block. The 24 block radius is utilized to present the best representation of a block's immediate surrounding neighborhood while managing the high computational cost of these directional networks. The figure below depicts the manner in which response blocks will be linked to a typical signal block (Fig. 2).

[2] https://data.philly.com/philly/crime/homicides/
[3] https://www.brookings.edu/blog/up-front/2016/04/27/gun-violence-in-major-u-s-cities-is-massively-underreported/

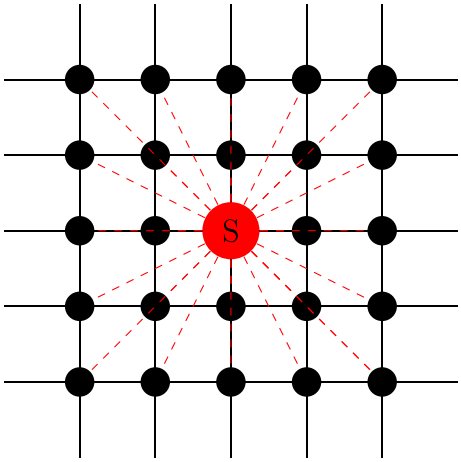


Fig. 2. The above diagram illustrates the k-nearest neighbor spatial network (with $k = 24$). Each node represents a city block. The central red node (labeled S) is the "signal" node, linked to each of the black nodes with a red dashed line representing the dyad connection. In this instance black nodes represent the response nodes of the dyads. Each dashed red line dyad connection represents a separate observation. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

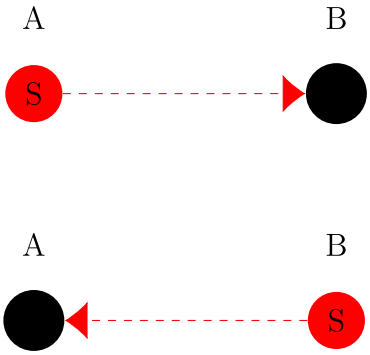


Fig. 3. This diagram illustrates the bi-directional nature of dyads in the spatial network. Block *a* links to block *b* in one dyad with *a* as the "signal node" (the block that is capable of receiving treatment) and *b* as the "response node" (the block at which treatment effect is measured). In a separate dyad, block *a* is connected to block *b* in the opposite relationship, with *b* as the "signal node" and *a* as the "response node". Each node pairing provides two separate dyad observations.

Block $a's$ link to block b is one observation, while block $b's$ link to block a is a separate observation. In total, this provides 602,712 unique linkages, herein referred to as "dyads". The first node will be referred to as the "signal block". The node it is linked to will be referred to as the "response block". Treatments will occur to the signal block, while outcomes will be measured at the response block. In practice, this means that a treatment to block a will have its effect measured at some block b in its immediate radius. The figure below depicts the bi-directional nature of these dyad linkages (Fig. 3).

In total, I index the data in dyad by year panels. Each of the 25,113 city blocks is linked to its 24 closest neighbors and charted over the course of ten years, for a total of 6,027,120 observations. As such, the primary specifications of the models specified will have over 600,000 dyad specific fixed effects. At the fine level of resolution at which data is made use of here, reliable covariate data is not available. As such, these fixed effects will be relied on to capture any time-invariant between unit variation and unit-invariant time period effects that could have been picked up by the inclusion of other covariates into the models. Census data, such as the ACS American Community Survey, which would otherwise be used to provide demographic controls are not available at this finite block level. Data for larger groupings are unreliable and offered at lengthy intervals.

The crux of my empirical strategy rests upon network link analysis, estimating average changes that occur in one node when the link's connected node is treated. To meet this purpose, a k-nearest neighbor adjacency matrix was first constructed, tying each block to its 24 closest neighbors. This square matrix has a length and a width equal to the total number of city blocks, and each row and column indexes a particular block. Every cell in the matrix has a value of zero, aside from those blocks that are among the set of k-nearest neighbors to one another. Their intersections will have a value of one (while the main diagonal, representing a block's intersection with itself will have a value of zero as well). For example, if block three and block four are neighbors, cell (4,3) and cell (3,4) will have values of one while cells (4,4) and (3,3) will have values of zero.

To generate the directional linkage dyads, each coordinate pairing with a value of one is taken as its own observation for each year. With this strategy, the average treatment effect on linked blocks is able to be efficiently estimated.

3.3. Measuring gentrification

There exists no definitive method by which gentrification should be identified quantitatively (Barton, 2014). Glaeser et al. (2020) notes that there are nearly as many different measures of gentrification as there are papers written on the subject. Each definition is at its core a product of the inherent data limitations of each individual project. Some of these different definitions and measurements are discussed in Appendix A.

Because of the narrow resolution at which I am attempting to measure gentrification and its effects, along with the peculiarities of Philadelphia itself, none of the established gentrification measures are operable or desirable. I adopt an approach to measurement similiar to that of Holm and Schulz (2017) and Glaeser et al. (2020). The "GentriMap" model of Holm and Schulz (2017) is a two pronged approach to identifying gentrification, utilizing both real estate factors (upgrades to properties and value changes) and social demographic factors as identifying criteria. The approach of Glaeser et al. (2020) is similar. The authors first identify neighborhoods that are capable of gentrifying, based on poverty rates from the first five-year American Community Survey (ACS) to occur in the window of their study. Next, to identify gentrification, the authors track growth in home rental prices; with those that have above median rental price growth rates being labeled as gentrifying.

The approach I utilize builds upon the methods described above, but instead identifies gentrification at its finest resolution at its earliest stage, at the level of an individual city block at the very beginning of redevelopment. As in the above mentioned approaches, the feasible set of blocks that are capable of gentrification are identified by demographic characteristics. Here, I make use of household income levels from the 2011-15 ACS for this purpose. The finest resolution that this data is reliably available for is the Census Block Group.[4] As such, each city block is mapped to the Census Block Group that encompasses it. Each block that is located in a block group having an average household income level below the city-wide median is labeled as having the potential to gentrify.

Of those blocks identified as having the potential to gentrify, the gentrification treatment is defined as beginning based upon real estate trends. Since home sale and rental prices (or home valuations) are not easily available for the area of study, I make use of the quantity of home sales and the number of building/renovation permits to approximate the real estate component of gentrification. Spikes in home sales are likely to be accompanied by rising prices. Helms (2003) provides justification for the use of permitting data[5], finding that trends in building permit issuance do in fact match the types and locations of properties that are predicted to experience real estate interest brought on by gentrification. Permitting data is provided at the property level before being mapped to a city-block. I use the income and housing data to create a conservative identification of gentrification.

To be labeled as gentrifying, the block in question must either have a change from the previous year in the number of new construction or building alteration permits issued that is more than three standard deviations above the city-wide mean for that year, have a change in the number of home sales that is greater than three standard deviations above the city-wide mean, or have both a change in the number of permits issued and a change in the number of home sales that each exceed two standard deviations above the city-wide mean. Limiting the gentrification label to the far-right tail of the permitting and home sale distributions has the benefit of preventing the definition from being arbitrarily applied and ensures that only relatively unprecedented shocks are able to turn this treatment indicator on. Further, as stated, for a block to be labeled as gentrifying it must also be in a Census Block Group with below-median household income as measured in the 2010 Census. The numerical values of these classification criteria are summarized in the table below (Table 2).[6]

It is evident from the above table that the necessary conditions to be labeled as gentrifying grow more stringent over time. This reflects the overall rising tide of development across the city. As more areas experience revitalization, a more significant amount of development is needed to represent a development shock to a neighborhood.

Utilizing changes in permitting and sales, rather than absolute numbers, ensures that areas of longer term sustained real estate interest do not get mislabeled as newly gentrifying. The primary results of the study are replicated with varying definitions of gentrification in the supplemental appendix. In the upcoming econometric evidence, I will suppose that once a block is labeled as having begun the gentrification treatment, it maintains the gentrification label for the remainder of the study. The map below displays the locations of gentrifying blocks in the city (Fig. 4).

Appendix B details an alternative gentrification measure where gentrification is treated as a continuous variable. Appendix D presents evidence of the validity of the treatment definition utilized, by comparing the spatial distribution of blocks labeled as gentrifying by this strategy and of locations identified as gentrified by media reports.

[4] Census Blocks mentioned here are not the same as the city blocks utilized as the basic level of observation in this study. Philadelphia has 384 Census Tracts, 1336 Census Block Groups, 18,872 Census Blocks, and 25,113 actual city blocks

[5] Using permitting data as a justification for gentrification implies a lagged effect on the treatment, as there is undoubtedly a delay between a permit being issued and any sort of residential demographic shifts. This is addressed in the treatment dynamics subsection of the robustness checks portion of this paper. Further, the primary results are replicated with a lagged treatment definition in the supplemental appendix.

[6] The supplemental appendix replicates this paper's main analysis with a separate gentrification criteria.

Table 2
Criteria for Gentrification Classification.

Year	Conditions to be met in addition to below median household income condition	# blocks newly meeting criteria
2011	ΔSales $\geq$ 2 or ΔPermits $\geq$ 12 or (ΔSales $\geq$ 1 & ΔPermits $\geq$ 8)	265
2012	ΔSales $\geq$ 2 or ΔPermits $\geq$ 12 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 8)	292
2013	ΔSales $\geq$ 2 or ΔPermits $\geq$ 13 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 9)	268
2014	ΔSales $\geq$ 2 or ΔPermits $\geq$ 14 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 9)	241
2015	ΔSales $\geq$ 3 or ΔPermits $\geq$ 15 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 10)	131
2016	ΔSales $\geq$ 3 or ΔPermits $\geq$ 16 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 11)	152
2017	ΔSales $\geq$ 3 or ΔPermits $\geq$ 17 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 10)	189
2018	ΔSales $\geq$ 3 or ΔPermits $\geq$ 18 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 11)	152
2019	ΔSales $\geq$ 3 or ΔPermits $\geq$ 19 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 13)	115
2020	ΔSales $\geq$ 3 or ΔPermits $\geq$ 18 or (ΔSales $\geq$ 2 & ΔPermits $\geq$ 12)	35
Total		1840

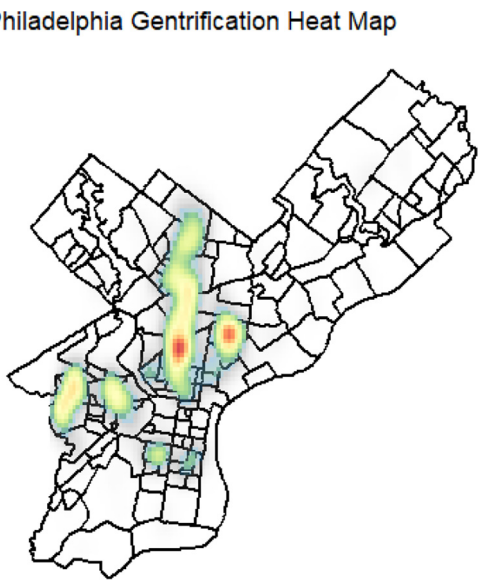


Fig. 4. Heat map depicting density of blocks that are labeled as gentrifying during the period of the study.

4. Estimation strategy and results

4.1. Primary specification

A two-way fixed effect differences-in-differences model is specified to identify the relationship between gentrification and gun violence.[7] City blocks gentrify independently of one another across time. Exploiting this variation provides an ideal setting from which to estimate gentrification's impact on violence.

This primary difference-in-differences specification is reported below.

$$Shootings_{it} = \beta_i + \psi_t + \alpha_1 Gentrified_{it} + \epsilon_{it} \quad (1)$$

Where, i indexes dyads, t indexes years, β_i is a vector of dyad fixed effects, and ψ_t is a vector of year fixed effects. $Gentrified_{it}$ is a dummy variable equal to 1 if block i has been identified as having begun gentrifying by period t.

The α_1 parameter will capture the estimated causal effect that gentrification has on each treated dyad. The two fixed effect vectors partial out the time and dyad invariant components of gun violence, allowing for the treatment parameter to capture the estimated treatment effect. A positive value for this coefficient indicates that the gentrification treatment has led to an increase in shootings on that block.

The results of fitting this primary empirical model to the data using both the total number of shootings and an Inverse Hyperbolic Sine transformation are reported below.[8] The IHS transformation of the dependent variable is introduced so as to be able to interpret the treatment effect as an elasticity and to deal with the large right tale of shootings data (Bellemare and Wichman, 2020).

[7] Additional specifications and robustness tests are included in the supplemental appendix.

[8] Estimation methods aimed at addressing the large amount of zeros in the data are reported in the appendix.

Table 3
Output from primary differences-in-differences specification from various estimation methods.

	Dependent variable:	
	Neighbor Block Shootings	
	Total	Inverse Hyperbolic Sine
Gentrified	0.021***	0.012***
	(0.003)	(.002)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.11	0.085
Observations	6,027,120	6,027,120
Adjusted R^2	0.13	0.16
AIC	7,512,901	2,248,264

Note: All Standard Errors clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Gentrification has a positive effect on levels of gun violence observed in the neighborhood surrounding the gentrified block. This effect represents an 18% increase in gun violence on blocks linked to gentrified blocks (in the primary OLS specification). This result is analyzed in detail in the robustness section of this paper (Table 3).

4.2. Common trends assumption and treatment dynamics

Given that the empirical model makes use of a staggered treatment timing difference-in-differences estimator, this estimator's unbiasedness as a causal estimator rests upon the assumption of parallel trends in outcome between both control and treatment groups (prior to those treatment blocks receiving their gentrification treatment). Following the bulk of recent literature, an event-study plot is used to visually demonstrate the average treatment effect at a number of periods before or after treatment.

This plot is made by specifying the following regression model, adapted from He and Wang (2017).

$$Shootings_{it} = \sum_{k=-9, k\neq -1}^{k=9} D_{it}^{k} \cdot \delta_k + \gamma_t + \psi_i + \epsilon_{it} \tag{2}$$

Here, the coefficients of interest are δ_k. D_{it}^{k} represents a vector of dummy variables equal to one, if the sending block of dyad i in period t is k periods away from beginning gentrification. $k = 0$ in the initial treatment period. Further, as in He and Wang (2017), $k = -1$ is omitted so that post-treatment event study estimators are relative to the period immediately before treatment.

The results are plotted in the graphic below.

Of the eight pre-treatment coefficients, only one is significant at the 95% confidence level. This result is not dissimilar from those surveyed by Roth (2022). However, the primary criteria by which the common trends assumption will be tested on is that of a joint-significance χ^2 test. All of the pre-period coefficients will be tested together against a null hypothesis of joint insignificance.

This test returns a Wald statistic of 1.41, which has a p-value of 0.18. As such, the null hypothesis cannot be rejected. There is not evidence that the assumption of common trends is violated. Gentrification's effect on gun violence is increasing over time. While this may initially seem counter-intuitive (as it should be expected that the market will re-stabilize quickly), this feature is likely due to the fact that I have measured gentrification at the point of permit *issuance*. This represents the beginning point of neighborhood change, the effects of which are likely to grow more pronounced across time. Further, it should be noted that since units are treated in all years of the study's window, less observations are available on the tails of the event study. As such, confidence intervals for these cohorts grow quite large and the precision of these estimates is questionable. Inference based off of these event study estimates should be done cautiously. However, the event study specification here is sufficient for the purpose of evaluating the parallel trends assumption.

4.3. Addressing the potential non-random assignment of the gentrification treatment

There is certainly cause to question whether the patterns of real estate investment and development that are used here to proxy for gentrification can in fact be treated as exogenous or random. Despite the above evidence of parallel trends for both treated and untreated blocks, arguments can easily be made that the blocks that gentrify are not selected at random. This would obviously impact the ability of the findings reported here to be interpreted as plausibly causal in nature. Fortunately, the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021) allows for causal identification without reliance on the assumption of parallel trends and in settings in which treatment assignment is not independent of unobserved time invariant unit-specific characteristics or unobserved time-varying global shocks. These shocks may have

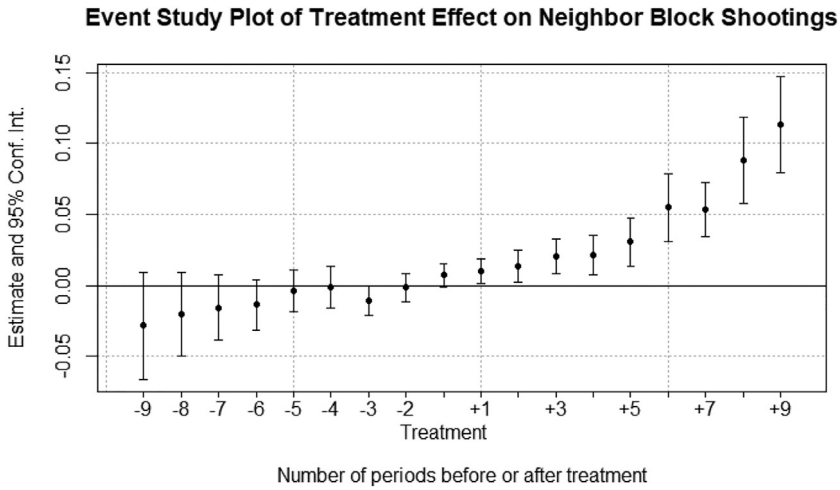


Fig. 5. Observed treatment effect by number of years pre or post treatment.

differential impacts on individual units, removing the threat of bias introduced by the omission of variables with potentially non-uniform impacts across units. Synthetic difference-in-differences estimation solely requires that the error term be uncorrelated with treatment status. This movement away from the standard additive fixed effects framework to that of interactive fixed effects allows for identification when regressors are functions of unit fixed effects or systematic time-varying unobserved characteristics (Bai, 2009).

The above event study analysis (Fig. 5) provides evidence of parallel trends between treated and control units. This suggests that eventual treatment status is not correlated with the outcome variable in pre-treatment periods. Further, the correlation between the binary treatment variable and the error term from the two-way fixed effects specification is near zero and statistically insignificant when tested using the point-biserial correlation test of Tate (1954). This test specifically addresses correlation between a continuous and binary variable. As such, the only cause for concern regarding possible non-random treatment assignment rests on the potential correlation between treatment assignment and some unobserved characteristics, ether time-varying or time-invariant. Estimation under the interactive fixed effects framework underpinning the synthetic difference-in-differences model allows for identification when treatment status is correlated with unit-specific time invariant characteristics and time-varying unobservables. For example, if a spate of development across the city is viewed as an unobserved global shock, with differential impacts on different city blocks, an interactive fixed effects framework allows for identification to still be possible even if these differential impacts are predictive of treatment status.

This estimator, as extended to staggered treatment adoption settings in Porreca (2022), allows for a localized causal estimator in which treated units are compared to a never treated control group that is weighted on both the time and unit dimensions. This produces a control group for identification that is composed of those units most similar to those that are treated and that follows a time path similar to the treated units. The synthetic counterfactual's construction differs from that of the traditional synthetic control method, in which the counterfactual is built from the weighting of observed covariates. The synthetic difference-in-differences model instead builds a counterfactual from the weighting of the outcome variable. Unlike the more standard two-way fixed effects estimator used in the primary specification, there is no earlier treated versus later treated component to the estimator (Goodman-Bacon, 2021). Treated units are solely compared to a weighted composite of never treated units.

In the context of this study, treated dyads are compared to never treated dyads that are most similar in both pre-treatment periods and in the time trend those units follow in the post-treatment period. This local estimator provides a comparison between the outcomes of gentrified blocks and those blocks that themselves would be seen as "potentially gentrifiable". The results of replicating the primary specification with this estimator are reported in the table below (Table 4).

As is evident from the table above, gentrification still has a positive and statistically significant impact on neighbor block shootings. This estimate, however, does need to be interpreted with some caution as it is solely a local estimate of this effect. Its magnitude reflects the estimated difference in shootings across the entire time window that the neighbors to a gentrified block will experience relative to the neighbors of an extremely similar, yet ungentrified, block. While I will avoid drawing any conclusions about this estimator's magnitude, it does sufficiently demonstrate that even with the possible endogeneity of gentrification, the positive plausibly causal relationship found in the primary specification is sound.

4.4. SUTVA and gentrification's impact on other gentrified blocks

The stable unit treatment value assumption (SUTVA) of Rubin (1980) is essential for identification in difference-in-differences. In this setting, in which I am seeking to measure treatment spillovers onto neighbor blocks, this assumption

Table 4
Output replicating the primary specification with the synthetic difference-in-differences estimator.

	Dependent variable:
	Neighbor Block Shootings
Gentrified	0.905***
	(0.004)
Dyad Fixed Effects	✓
Year Fixed Effects	✓
Observations	6,027,120

Note: Standard errors are calculated with jackknife procedure. This is a local estimator, with only those similar dyads of the control group being used a basis for comparison. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5
Frequency of gentrified observations with a number of gentrified neighbors falling into a specific range.

Number of treated neighbors	Number of treated blocks in each category
0 to 5	246
6 to 10	378
11 to 15	535
16 to 20	518
21 to 24	163

Table 6
Dissagregation of treatment effect between neighbor blocks that have themselves previously been treated and those that have not.

	Dependent variable:	
	Neighbor Blocks Shootings	
	(Gentrified Response Block Subset)	(Non-Gentrified Response Block Subset)
Gentrified	0.011	0.016**
	(0.008)	(0.005)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.245	0.103
Observations	292,231	5,734,889
Adjusted R^2	0.13	0.13
AIC	601,560	6,799,962

Note: All Standard Errors clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

requires that the gentrification of one block does not have an effect that extends beyond its 24 block neighborhood. If this assumption is violated here, and gentrification has the same effect in inducing increases in gun violence in an even greater surrounding neighborhood, this simply would introduce a downward bias into the treatment effect estimator. As such, if there is cause to suspect that SUTVA has been violated, then the positive effect of gentrification spurring neighborhood violence that is estimated here is in fact *understating* gentrification's true impact.

It is similarly worth examining the effect that this treatment has on neighboring blocks that have themselves already been treated, Gentrification does not occur disparately across the city. As evidenced in the map displayed earlier, gentrification tends to occur in clusters. The table below shows, for the final year of this study (2020), the degree of clustering among gentrified blocks. At the end of the study, there were 1840 blocks recorded as having begun gentrifying. The table displays the frequency of a gentrified block having a number of gentrified neighbors in its 2 block radius ($k = 24$) (Table 5).

It is evident from the above table that gentrification occurs in clusters, but very few gentrified blocks are entirely surrounded by gentrified neighbors. The majority of treated blocks, have less than half of their neighbors eventually treated.

To examine the heterogeneous impact that gentrification has on other gentrified blocks versus non-gentrified blocks, the data is subset into dyads that connect to gentrified blocks and dyads that connect to non-gentrified blocks. The primary model specification was replicated on these two subsets. Here the coefficient is disaggragated into the treatment effect on those blocks that are also already gentrified and those that are not (Table 6).

There is no effect observed on the gentrified response blocks. The insignificance of this estimated effect suggests that the increases in neighborhood gun violence that stems from gentrification are limited to those neighbor blocks that are them-

selves yet to begin the redevelopment associated with gentrification. This provides evidence that the effect being captured in this paper's primary regression specification is largely occurring on non-gentrified blocks.

5. Mechanism

5.1. Resident turnover

Implicit in the earlier discussed hypothesized mechanism is an assumption that the redevelopment of urban neighborhoods brings with it a turnover of the resident population. To validate this assumption, providing a basis for the hypothesis of gentrification changing the geographic constraints of illegal drug markets, I employ data at the census block group level of geographic aggregation from the 2006–2010, 2011–2015, and 2016–2020 American Community Surveys. I then map permit issuance and home sale data its constituent census block group and ACS period. A constructed index of ethnic fractionalization is regressed on a continuous gentrification index (similar to the index used to replicate the main result of this paper in Appendix B). A positive relationship between gentrification and ethnic fractionalization implies a turnover of neighborhood residents and a growing degree of racial/ethic diversity in the neighborhood.

Criminal organizations have traditionally been able to operate in areas of ethnic homogeneity (Skaperdas, 2001). Some empirical evidence of this proposition is provided in Trawick and Howsen (2006), who document that crime rates across Kentucky are positively correlated with a county's degree of ethnic homogeneity. Criminal organizations rely on tacit community support to function. Changes to the structure of that community, including a loss of in-group identity for criminal actors, has the potential to lessen the degree of support and community deference that allowed the organization to previously flourish. In the context of the present study, increased ethnic fractionalization would presumably limit the ability of the criminal organization to entrench itself in a neighborhood to operate illegal drug markets. Subsequently, the criminal organization wishing to continue as a competitor in the illegal drug market would need to relocate to some other territory.

I follow Alesina et al. (1999) in constructing an index of ethnic fractionalization. This index measures the probability that two randomly drawn people from a particular geography belong to different ethnic groups. Higher values of this index represent higher degrees of ethnic and racial diversity in a particular geography. The index values are calculated as follows:

$$Ethnic\ Index = 1 - \sum_{h}^{H} (Race_h)^2$$
$$\forall\ h \ \in \ \{Asian, Black, Multiracial, Pacific\ Islander, White, Other\} \tag{3}$$

Here, h indexes racial groups represented in US Census ACS surveys.

The gentrification index utilized captures both of the operational components discussed earlier in constructing the binary gentrification indicator: the potential to gentrify, based on poverty rates, and the level of development as captured by permit issuance and home sales. More real estate interest in more impoverished census block groups leads to higher values on this spectrum, while less interest in wealthier census block groups leads to lower values.

The real estate development component of this index is calculated as follows.

$$Real\ Estate\ Index_{it} = \frac{\left(\frac{sales_{it}}{\bar{sales}_t} + \frac{permits_{it}}{\bar{permits}_t}\right)}{\frac{1}{N} \cdot \sum_{i=1}^{N} \left(\frac{sales_{it}}{\Delta\bar{sales}_t} + \frac{permits_{it}}{\bar{permits}_t}\right)} \tag{4}$$

This index is then multiplied by its constituent census block group's poverty rate to provide a simple continuous measure of gentrification.

$$Gentrification\ Index_{it} = Real\ Estate\ Index_{it} \cdot Poverty\ Rate_{it} \tag{5}$$

Using these two indices, a a fixed effects OLS specification allows for estimation of the relationship between gentrification and ethnic fractionalization.

$$Ethnic\ Index_{it} = \beta_i + \psi_t + \alpha_1 Gentrification\ Index_{it} + \epsilon_{it} \tag{6}$$

In this specification, i indexes census block groups and t indexes 5 year ACS survey periods. β_i and ψ_t are vectors of unit and survey period fixed effects. The results from this specification are displayed in the table below (Table 7).

These results demonstrate a positive relationship between the degree of gentrification in a particular census block group and its level of ethnic fractionalization. This provides suggestive evidence that as lower income areas are redeveloped there is a turnover of residents which would in turn likely make these areas less viable for sustaining illicit drug markets and organized criminal activity.

5.2. Drug markets

The driving presumption behind this paper's empirical strategy is that a block's gentrification makes that block no longer suitable for a drug market. There are multiple potential rationales that would support this assumption. As described above,

Table 7
Output from regressing ethnic fractionalization index on gentrification index.

	Dependent variable:
	Ethnic Fractionalization
Gentrification Index	0.056***
	(0.016)
Census Block Group Fixed Effects	✓
ACS Year Fixed Effects	✓
Mean	0.36
Observations	3795
Adjusted R^2	0.69
AIC	3888.6

Note: Standard errors are clustered at the census tract level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 8
Output from differences-in-differences estimation of gentrification's negative effect on immediate block drug crime.

	Dependent variable:
	Own Block Drug Crimes
Gentrified	-0.218***
	(0.068)
Dyad Fixed Effects	✓
Year Fixed Effects	✓
Mean	0.39
Observations	251,130
Adjusted R^2	0.56
AIC	22,504,829

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

gentrification includes a turnover of residents and as such likely reduces the original resident base that supported the illicit activity. Further, gentrification is associated with increased policing that is likely to have a deterrent effect. Therefore, my hypothesis is that the drug trade is disrupted at the gentrified signal block and displaced to the response blocks.

To empirically test the accuracy of this hypothetical mechanism, the following difference-in-differences estimation model is specified and tested. This specification seeks the plausibly causal impact of gentrification on the levels of drug crime observed on a block following that block's gentrification.[9] For evidence of my hypothesized mechanism's validity, the post-gentrification treatment coefficient should be negative and significant.

$$Drugs_{it} = \beta_i + \psi_t + \alpha_1 Gentrified_{it} + \epsilon_{it} \tag{7}$$

In this case, i indexes individual blocks rather than dyads, t indexes time periods, β_i is a vector of block fixed effects, ψ_t is a vector of year fixed effects, ϵ_{it} is an error term, and α_1 is the parameter of interest. The results of this specification are reported below. As the result demonstrates, there is sufficient evidence to support the validity of this mechanism. Newly gentrified blocks do not support the same level of drug trade post-gentrification as they did pre-gentrification (Table 8).

Theory predicts that the removal of viable territory from an illegal market will increase the relative value of the remaining territory to be competed for. I have provided evidence that gentrification reduces drug crime on the immediate gentrified block, implying that this block has been removed from the set of viable territory to be competed for. This is not enough to explain how gentrification is able to instigate increases in gun violence. It needs to be seen that in the short run the gentrification of one block leads to increases in drug crime on linked neighbor blocks. As such, I will limit the treatment variable to it's effect in the immediate period of treatment[10] and replicate the same specification with i now indexing dyads and drug crime at this level as the response variable (Table 9).

In the short run there is a clear increase in neighbor block drug crime following a block's gentrification. When taken in conjunction with gentrification's effect on drug crime on the immediate block, there is a clear suggestion that gentrification pushes drug crime away from the immediate block and into the surrounding neighborhood. This provides evidence of a likely mechanism behind gentrification's positive relationship with gun violence.

[9] This specification mirrors the primary estimation strategy, for gentrification's effect on shootings.

[10] This is done by replacing the Gentrification Indicator with a new indicator that is equal to one if a given block is newly gentrified in that particular period.

Table 9
Output from differences-in-differences estimation of gentrification's short run positive effect on neighboring block drug crime.

	Dependent variable:
	Neighbor Block Drug Crimes
Immediate Year Gentrification Indicator	0.06**
	(0.023)
Dyad Fixed Effects	✓
Year Fixed Effects	✓
Mean	0.38
Observations	6,027,120
Adjusted R^2	0.56
AIC	22,408,438

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

5.3. Displacement of drug activity spurring violence

An unfortunate attribute of crime data is the inability to directly observe the motives and context behind criminal incidents. Here, this means that I am unable to directly test the assumption that it is the spatial displacement of drug activity, discussed above, that creates the increases in gun violence observed in the neighborhood of newly gentrifying blocks. However, Sobrino (2019) derives a theoretic basis for the assumption that more valuable drug market territory will see more criminal organizations seeking to enter that territory and that levels of violence will increase as more criminal organizations enter the same territory. Further, she demonstrates empirically that once a second cartel enters a municipality homicide rates in that municipality increase, continue to increase with the entry of subsequent cartels, and do not decline until all but one cartel exit the municipality (Sobrino, 2019).

Similarly, Taniguchi et al. (2011), Brantingham et al. (2012), and Bruhn (2021) all show that significant outliers in levels of violence are most likely to be observed at the borders of gang territories. Taken together, this empirical and theoretic evidence provides support to my proposition that the displacement of drug activity is a driving factor behind the uptick in gun violence observed following a block's gentrification. This displacement pushes rival criminal organizations into closer proximity to one another, bringing their informal territorial boundaries into conflict. Consequently, this in turn causes the same sort of violent response observed in other studies. Gang competition at territorial (or market) boundaries fosters increasing levels of violence at those boundaries.

5.4. Heterogeneous treatment effects: The gentrification of high drug crime blocks

Here, I analyze the heterogeneous treatment effects, providing evidence that the treatment has a significantly stronger effect on those blocks that house the illegal drug trade. It was argued earlier that gentrification makes a block less able to sustain an illegal drug market and that the loss of physical territory from such an illicit market would be a destabilizing force. That this can lead to violence should be no surprise. The territory that remains gains in value, incentivizing fiercer competition for its control. Because of this, it is expected that the treatment effect will be more pronounced when it is blocks that have been observed to have high levels of illegal drug activity that gentrify.

To begin, as mentioned previously, drug incidents from the same Philadelphia Police Department crime incidents database are mapped to their nearest city block. These incidents are listed in the city's records as "narcotic/ drug law violations". Those blocks that have a number of incidents in the previous year that exceeded three standard deviations above the city-wide mean are labeled as high drug crime areas. Once labeled a drug block that block will maintain that label for the remainder of the study. Allowing for blocks to gain this label at later time periods is meant to capture some of the dynamic nature of the movement of the drug trade across the city. A map of the location of these blocks is shown below (Fig. 6).

The table below charts the number of drug blocks both initially identified and gentrified in the city in each year. It is evident that drug blocks are both a relative rarity in the city and that their gentrification is an even rarer occurrence. Across the ten year window examined, the threshold for this classification is on average at least seven drug incidents occurring on that block in a given year (Table 10).

With this drug block indicator, the primary difference in difference specification is able to be disaggregated into the treatment effect on non-drug blocks and the effect on drug blocks.

$$Shootings_{it} = \beta_i + \psi_t + \alpha_1 NonDrugBlockGentrified_{it} + \alpha_2 DrugBlockGentrified_{it} + \epsilon_{it} \tag{8}$$

This model is essentially the same as the primary specification. However, here the treatment effect parameter is split into its constituent effects on the the two different types of treated units (Table 11).

As is evident in both specifications, the gentrification of a drug block has a highly significant positive effect on the level of gun violence exhibited in the surrounding neighborhood. Further, it is of note that the effect on drug blocks is consistently of a higher magnitude than that on non-drug blocks. Focusing on the totals results, the treatment effect is more than ten

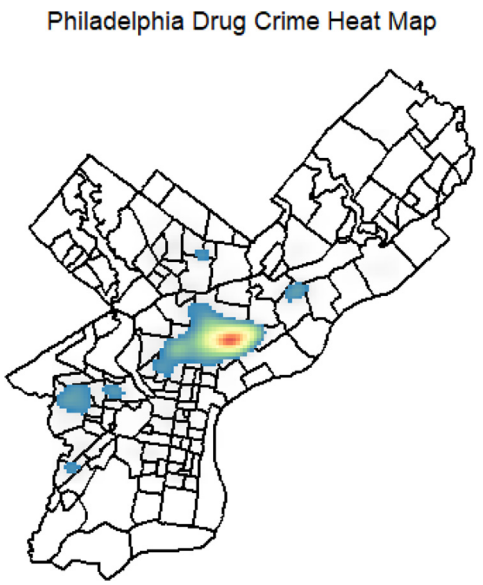


Fig. 6. Heat map depicting density of blocks designated as high drug crime areas during the period of study.

Table 10
Counts of unique blocks labeled as gentrified or as drug blocks by year.

Year	Total New Blocks Gentrified	Total New Blocks Labeled High Drug crime	Number of Drug Blocks Gentrified
2011	265	282	23
2012	292	0	24
2013	268	71	21
2014	241	62	23
2015	131	34	5
2016	152	35	13
2017	189	42	7
2018	152	49	13
2019	115	29	10
2020	35	50	4
Total	1,840	654	143

Table 11
Output from disaggregating difference and difference coefficient with various estimation methods.

	Dependent variable:	
	Neighbor Block Shootings	
	Total	Inverse Hyperbolic Sine
Drug Block Gentrification Indicator	0.116***	0.068***
	(0.016)	(0.009)
Non-Drug Block Gentrification Indicator	0.014***	0.008***
	(0.003)	(0.002)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.11	0.085
Observations	6,027,120	6,027,120
Adjusted R^2	0.13	0.16
AIC	7,512,571	2,247,992

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

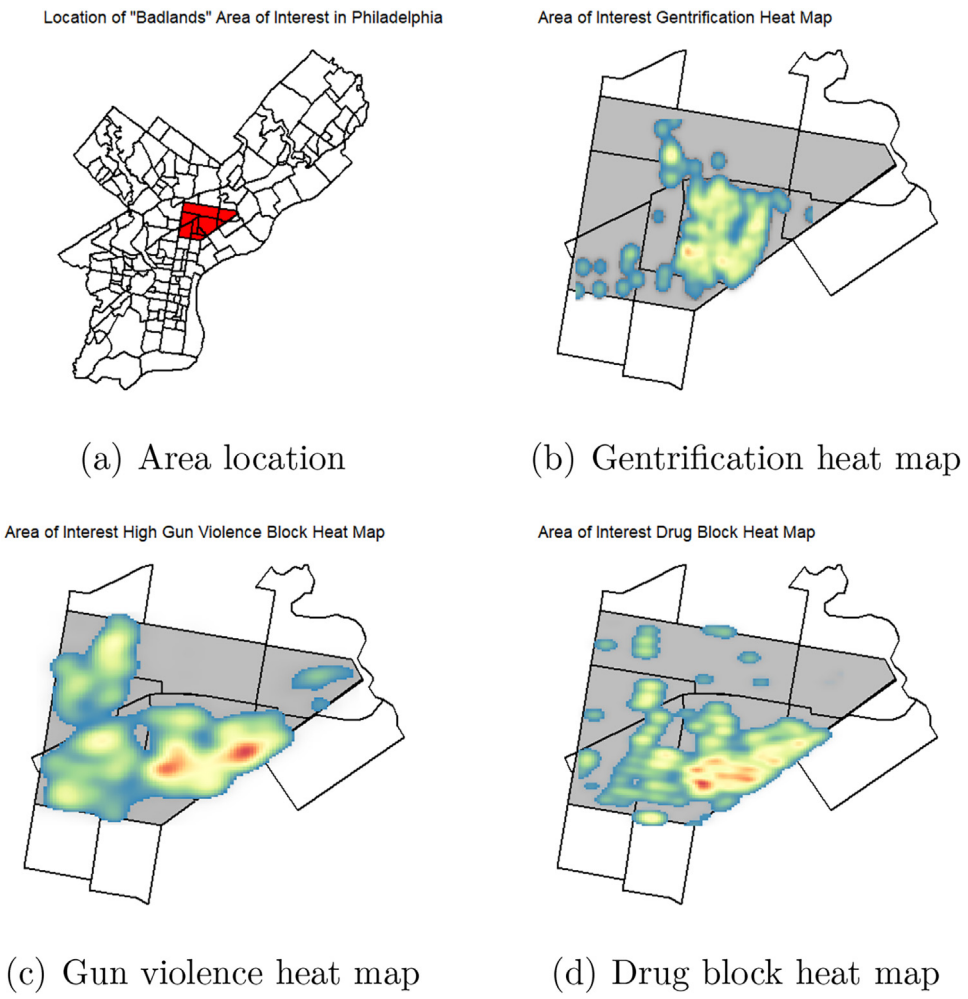


Fig. 7. Graphic depicting the Badlands are of interest's location within the city (in red) along with heat maps of gentrification, high gun violence blocks, and high drug crime blocks. For the heat maps, the area of interest is in gray. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

times as large when it is a drug block that gentrifies. Testing the equality of these two coefficients returns a Wald Chi Square of 12.38, with a corresponding *p*-value below 0.01. These two coefficients are significantly different than one another.

It is clear that the gentrification of a high drug crime block has a greater violent spillover into the surrounding neighborhood than does the gentrification of a more typical city block. This is consistent with the hypothesized mechanism behind the story of this paper. In the next section, I focus on one particular Philadelphia neighborhood, known locally as the "Badlands", that has a reputation as housing an open-air heroin market. This neighborhood has the largest concentration of high drug crime blocks of the city, and as such based on this result is expected to demonstrate an even more pronounced increase in neighborhood violence following gentrification.

6. Case study of the "Badlands"

Within the city of Philadelphia, one particular section stands at the cross roads of the trends here-in described: the so-called North Philadelphia "Badlands" (which has shown up in red in all of the above heat maps). This section, located in the lower northeast of the city, is known locally as a hotbed of illegal drug activity and has for some time been plagued by rampant gun violence. However, despite this, the rapid gentrification of surrounding neighborhoods has led to an influx of development within this area. The maps below depict this section's location within Philadelphia, the distribution of gentrifying blocks within the area, and the distribution of high drug crime and high gun violence blocks in the area (Fig. 7).

It is of note that there are no formal boundaries to this area. As such, I have adopted a commonly held definition of its borders as described in conversations with locals.[11] This definition has the Badlands encompassing the area of north

[11] This same definition is also mirrored at https://en.wikipedia.org/wiki/Philadelphia_Badlands

Table 12
Output from primary differences-in-differences specification from various estimation methods.

	Dependent variable:	
	Neighbor Block Shootings	
	Total	Inverse Hyperbolic Sine
Gentrified Indicator	0.09***	0.05***
	(0.03)	(.015)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.30	0.22
Observations	308,880	308,880
Adjusted R^2	0.14	0.16
AIC	687,461	399,057

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 13
Output from disaggregating difference and difference coefficient with various estimation methods.

	Dependent variable:	
	Neighbor Block Shootings	
	Total	Inverse Hyperbolic Sine
Drug Block Gentrification Indicator	0.16***	0.09***
	(0.05)	(0.02)
Non-Drug Block Gentrification Indicator	0.05**	0.03**
	(0.018)	(0.011)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.30	0.22
Observations	308,880	308,880
Adjusted R^2	0.14	0.16
AIC	687,413	399,021

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Philadelphia bounded by Kensington Avenue to the east, Broad Street to the west, York Street to the south, and Hunting Park Avenue to the north.

This area is widely known for its quite visible open air drug markets, where illegal narcotics (primarily heroin and fentanyl) are sold openly on gang controlled street corners.[12] This unregulated market is operated by crews from around the city and attracts drug users from across the greater-Philadelphia region. Newer development and an influx of young professionals into the surrounding areas of Fishtown, Olde Kensington, and Northern Liberties has created a rise in housing prices and and spate of development within the traditional Badlands area. Because of these trends, and its uniquely large scale and unregulated drug market, the Badlands provides an opportunity to replicate this paper's city-wide finding in a more geographically concentrated area (Table 12).

The results from replicating the primary difference-in-differences specification and the disaggregation specification for heterogeneous treatment effects on this geographic subset are displayed in the tables below. This is simply replicating the primary analysis on a truncated data set limited to this particular section of the city (Table 13).

As is evident from these results, the same direction of the effects seen across the city are mirrored in this area; albeit with much larger magnitudes. The impact of gentrification on gun violence is felt more acutely in this neighborhood. This is consistent with the result demonstrated earlier. In a neighborhood like this, with a greater clustering of high drug blocks, gentrification's destabilizing affect is of a greater magnitude.

7. Discussion

I have demonstrated a robust positive relationship between gentrification and gun violence. Following a block's gentrification, there is a substantial increase in shootings in the surrounding neighborhood. Economic intuition provides a potential mechanism behind some of these increases. I have argued that due to a lack of enforceable territorial claims, competition in

[12] See https://www.nytimes.com/2018/10/10/magazine/kensington-heroin-opioid-philadelphia.html for a New York Times piece examining the Badlands' heroin market

illegal markets grows more violent with shocks to the total viable territory available and that gentrification serves as such a shock. However, this is not the only mechanism behind gentrification's role in spurring gun violence.

It is likely that gentrification forces intra-city migration. Residents are displaced from their long term homes, and forced into the remaining viable tracts of affordable housing. These sorts of situations, where disaffected low-income residents are forced to live in unfamiliar neighborhoods surrounded by similarly disaffected and displaced neighbors, have the potential to cause excessive tension. That this can give rise to explosions in gun violence is not surprising.

Despite these computational and data limitations, I have presented a readily replicable measure of gentrification at an extremely narrow spatial resolution. Further, through leveraging a massive networked data set, I have been able to generate robust significant estimates of the effect that gentrification has on levels of gun violence.

All of the coefficients estimated are at the dyad level and as such are of seemingly small magnitudes. However, when considering that each of these estimates is of the average effect on the single linkage between a newly gentrified block and each of its neighbors, these numbers add up quickly to a startling total. Over the ten year window of the study, back of the envelope calculations suggest that the city experienced some 5800 shootings that can be attributed to gentrification. This means, that of the 27,000 shootings that occurred across the city during this time period, around 21% may have been spillover effects of gentrification.

Exploring the mechanism posited by this study's economic intuition, the estimator was significantly higher. The gentrification of drug blocks accounted for roughy 2400 additional shootings during the ten year time span. This attributes some 8% of the city's gun violence across the decade to gentrification giving rise to instability in the city's illicit drug markets.

These estimates are striking. Each episode of gun violence has the potential to forever impact the lives of victims, perpetrators, families, and community members at large. As such, in light of this unintended impact, it is crucial that urban development occur responsibly and intentionally. Forced displacement, through pricing out residents, has very real effects on the surrounding neighborhood.

Future research would do well to examine the natural limitations to where illegal drug markets can feasibly exist. Models that are able to predict the spatial movements of these markets across time can help to prevent these high levels of violence from continuing. Further, this result suggests that police resources could be better utilized in the neighborhoods surrounding newly developed blocks. City policy may benefit from efforts that seek to stave off this sort of violent spillover effect through deployment of officers and social workers into areas that are likely experiencing significant population displacements. It is likely that displacement will give rise to volatility and violence. It is reasonable that resources be deployed proactively alongside the forces of development in an attempt to prevent this sort of community violence.

Funding

Not applicable

Availability of data and material

All data is freely available from OpenData Philly and the National Historical Geographic Information System (Manson et al. 2021). Code and cleaned data is available upon request.

Code Availability

All econometrics were conducted in R. Code utilized by the author is available upon request.

Declaration of Competing Interest

Not applicable

Data availability

Data will be made available on request.

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Appendix A. Discussion of other gentrification definitions

First and foremost, there exists no overarching consensus regarding the method by which gentrification should be identified quantitatively (Barton, 2014). Authors have employed novel identification strategies, such as utilizing the number of

coffee shops in a given neighborhood[13] (Papachristos et al., 2011). Easton et al. (2019) have stated that the difficulties in developing a quantitative identification strategy for gentrification is primarily a product of insufficient data. They posit that gentrification is the *displacement* of lower income residents, and that neighborhoods undergoing gentrification can be identified by demographic changes, real estate trends, and survey data. However, the authors concede that this sort of data is limited in its usefulness and may not necessarily capture all of this sort of resident displacement.

Helms (2003) provides a thorough model of real estate dynamics as a product of gentrification. His model's framework provides much of foundation on which this study's quantitative definition of gentrification rests. Using data from Chicago to test his theoretical model, Helms (2003) finds that trends in building permit issuing do in fact match the types and locations of properties that are predicted to experience real estate interest brought on by gentrification. This finding supports this study reliance on building/ renovation permit and home sale data to serve as a marker of gentrification.

Holm and Schulz (2017) provide a robust statistical model for identifying gentrification. As is typical of this literature, the authors concede that data availability is the largest limitation to the quantitative identification of gentrification. Their "GentriMap" model includes both real estate factors (upgrades to properties and value changes) and social demographic factors as identifying criteria.

This study embodies a similar approach to that of the "GentriMap" model, subject to the data limitations. Sales counts are used in place of value changes. This proxy is a reasonable substitution, given data limitations. An increase in sales reflects an increase in demand which in turn should imply an increase in property valuations. Permit data is used in the same manner as in other gentrification models. Median household incomes are utilized as a demographic factor indicative of a block's ability to become gentrified -as most definitions agree that low income household displacement is a necessary condition.

Several other recent papers have put forth measurements of gentrification. Dragan et al. (2020) made use of 5-year ACS data at the Census Tract level (the third smallest level of Census data aggregation) to identify potentially gentrifying blocks based in part on changes in demographic characteristics captured by these surveys. This data was augmented by administrative data identifying the addresses of individuals, to track their movement across the city over the study period. Ellen et al. (2019) used confidential census data to identify household demographic characteristics and residency, to track high-income movements into the city that are associated with gentrification. This analysis takes place at the Census Tract level.

Autor et al. (2017) use spatial exposure to the buildings that end their rent control status as a measure of gentrification. This is done on the basis of some literature the authors cite, that establish the abatement of rent control as being associated with numerous changes in neighbor housing stock quality and pricing. Glaeser et al. (2020) puts forth both a continuous and a binary gentrification measure, built on 5 year ACS survey data and rental price data. That analysis is conducted at the zip-code level.

As is evident, this study is not alone in offering forth an empirical measure of gentrification. Nor is the measurement strategy utilized herein disparate from that utilized in the bulk of the literature. However, this study differs in that it measures gentrification at the finest resolution available, while using solely open access freely available data.

Appendix B. Continuous definition of gentrification

It is worth briefly examining a potential continuous measure of gentrification, in which gentrification exists on a spectrum. More real estate interest on more impoverished blocks leads to higher values on this spectrum, while less interest of wealthier blocks leads to lower values.

The real estate component is represented by the following index.

$$Real\ Estate\ Index = \frac{\left(\frac{\Delta sales_{it}}{\Delta \overline{sales}_t} + \frac{\Delta permits_{it}}{\Delta \overline{permits}_t}\right)}{\frac{1}{N} \cdot \sum_{i=1}^{N} \left(\frac{\Delta sales_{it}}{\Delta \overline{sales}_t} + \frac{\Delta permits_{it}}{\Delta \overline{permits}_t}\right)} \tag{1}$$

This index value is block specific. It is then multiplied by its constituent Census Block Group's poverty rate to provide a simple continuous measure of gentrification.

$$Gentrification\ Index = Real\ Estate\ Index \cdot Poverty\ Rate \tag{2}$$

This measure has a mean of 0.032, a standard deviation of 1.05, and ranges from -10.07 to 9.3. Higher values are associated with higher degrees of gentrification.

Replacing the binary gentrification treatment indicator from the initial specification with this continuous measure creates a simple OLS fixed effects model which yields the following results (Table B.1).

$$shootings_{it} = \beta_i + \psi_t + \alpha_1 Gentrification\ Index_{it} + \epsilon_{it} \tag{3}$$

[13] Such a strategy would not be useful for a city such as Philadelphia which embodies a striking divide between residential and commercial neighborhoods. As such, this method is not replicated in this study

Table B.1
Continuous model of gentrification's effect.

	Dependent variable:
	Neighbor Block Shootings
Continuous Gentrification Measure	0.0007**
	(0.0003)
Dyad Fixed Effects	✓
Year Fixed Effects	✓
Mean	0.11
Observations	6,027,120
Adjusted R^2	0.13
AIC	7,512,447

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Appendix C. Addressing Non-standard Distribution of Shootings

As is to be expected, the distribution of shootings is heavily clustered at zero. The vast majority of blocks and linkages do not experience any shootings. Typically, this is addressed with with a Tobit model. However, due to the large number of fixed effects used in the empirical model, this is extremely computationally costly and not feasible. This sort of estimation would rely on specialty atypical maximization routines that would still provide inconsistent estimations of the variance (Henningsen, 2020, August 4).

As such, a random effects Tobit model is specified here, the results of which are reported in the table below (Table C.1).

The disaggregation of this coefficient, as shown earlier with OLS estimators in the heterogeneous treatment effects section, is displayed below (Table C.2).

Table C.1
Random Effects Tobit Model.

	Dependent variable:
	Number of Shootings
Gentrification Indicator	0.11***
	(0.0008)
Log Sigma Mu	-0.62***
	(0.0007)
Log Sigma Nu	-0.78***
	(0.00002)
Dyad Random Effects	✓
Year Random Effects	✓
Mean	0.11
Observations	6,027,120
Log Liklihood	9,419,719

Note: Standard errors are clustered at the sending block level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.2
Random Effects Tobit Model- disaggregation of primary treatment estimator.

	Dependent variable:
	Number of Shootings
Drug Block Gentrification	0.40***
	(0.003)
Non-Drug Block Gentrification	0.08***
	(0.0009)
Log Sigma Mu	-0.62***
	(0.0007)
Log Sigma Nu	-0.78***
	(0.00002)
Dyad Random Effects	✓
Year Random Effects	✓
Mean	0.11
Observations	6,027,120
Log Liklihood	9,419,083
Standard Errors	Clustered at Census Block Group Level

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.3
Output from replicating primary difference and difference specification with hurdle approximation.

	Dependent variable:	
	Pr(Number of Shootings>0) Logit	Number Shootings When Number of Shootings>0 OLS
Gentrification Indicator	0.017	0.06***
	(0.028)	(0.022)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.08	1.37
Observations	2,391,250	483,363
Adjusted R^2	0.12	0.14
AIC	2,627,291	1,548,094
All Standard Errors Clustered at Census Block Group Level		

Note: Logit Adjusted R^2 is Squared Correlation. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.4
Output from disaggregating difference and difference coefficient with hurdle approximation.

	Dependent variable:	
	Pr(Number of Shootings>0) Logit	Number Shootings When Number of Shootings>0 OLS
Drug Block Gentrification Indicator	0.18***	0.19**
	(0.068)	(0.060)
Non-Drug Block Gentrification Indicator	-0.001	0.04*
	(0.027)	(0.022)
Dyad Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Mean	0.08	1.37
Observations	2,391,250	483,363
Adjusted R^2	0.12	0.14
AIC	2,627,269	1,548,063
All Standard Errors Clustered at Census Block Group Level		

Note: Logit Adjusted R^2 is Squared Correlation. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Now, addressing the fixed effects issue, a hurdle model is approximated. This will provide two separate estimators: one, a logit estimator of the probability of the shooting outcome variable not being equal to zero and the other a differences-in-differences OLS estimator ran solely on the subset of the data in which the number of shootings is greater than zero (Tables C.3 and C.4).

As above, the disaggregated coefficient is reported as well.

Appendix D. Validating the Gentrification Definition with Newspaper Text Analysis

To validate the measure of gentrification utilized in this study, I began by downloading the text of every newspaper article referencing gentrification from the *Philadelphia Inquirer* and the *Philadelphia Daily News* published between 2010 and 2020. This yielded 663 articles. These articles were then parsed by a script to extract any complete address or intersection. Limiting this to those location's within the city limits left 68 specific locations. These were then geocoded using the Google Maps API, and assigned to their corresponding city blocks, as was done with the bulk of this paper's data.

It should be noted that the points extracted from these newspapers typically came from restaurant reviews or articles relating to well established previously gentrified neighborhoods. The newspaper references stem more from the long term effects of gentrification, not the first wave of initial investment that is used as the treatment in this study. Gentrification in Philadelphia is largely an outward moving process, expanding away from Center City. As is evident in the map below (displaying the 68 newspaper generated points superimposed over the previously shown gentrification heat map) these newspaper points are clustered in the areas between my early stage gentrification treatment locations and the city's central business district (Figs. D.1 and D.2).

To evaluate whether it is likely that these two spatial distribution's arose from the same underlying data generating process, a bivariate Ripley's K function is used to analyze the two spatial point processes. The procedure for evaluating the independence of two spatial distributions, as detailed by Dixon (2002) and Rossiter (2021), entails generating the K measure of how many observations of one type are observed at increasing radii surrounding an observation of the other type. This line is then plotted against a theoretical Ripley's K generated by a homogeneous Poisson process, which represents complete spatial randomness (CSR) or independence of the two distributions. This result is plotted below.

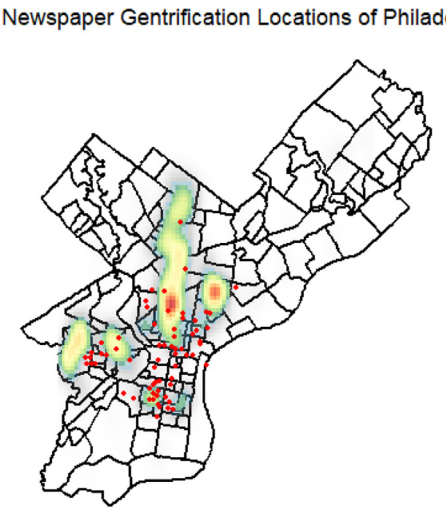


Fig. D.1. Blocks identified as gentrified from newspapers superimposed over the treatment gentrification heat map used in this study.

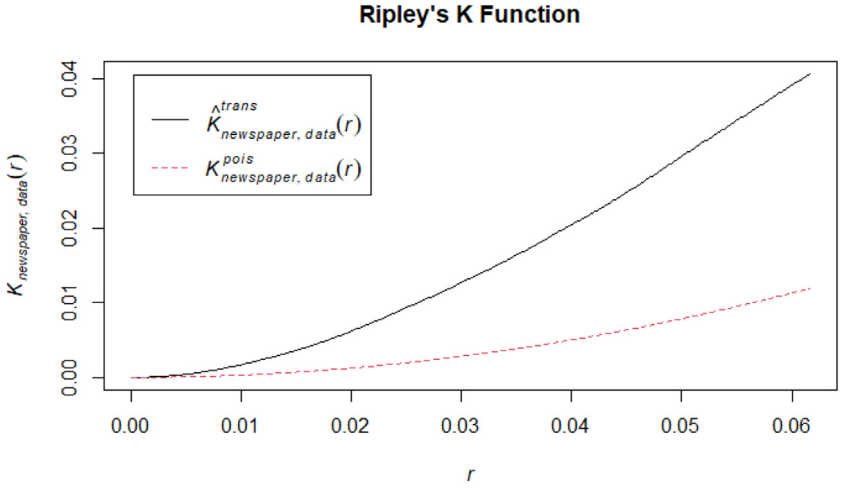


Fig. D.2. Plotting of Ripley's K function, the Poisson (CSR) process is in red and the empirically observed K function is in black. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

As is evident from this figure, the empirically observed K function is consistently above the Poisson process line. This means that within every radius[14] surrounding a given observation of a newspaper generated gentrified block there are more blocks identified by my empirical gentrification definition than would be at random. This is indicative of the two spatial distributions not being independent of one another, and provides some measure of validation for the empirical definition used herein.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jebo.2023.01.017

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[14] The units of radius here are in hundredths of a longitude-latitude square. As such, 0.06 is around one kilometer in size.

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