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Essays in the Economics of Crime and Policing

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Doctor of Philosophy in Management

by

Yilin Zhuo

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ABSTRACT OF THE DISSERTATION

Essays in the Economics of Crime and Policing

by

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This dissertation investigates the role of space and institutional structures in shaping criminal justice contact through three essays. In Chapter 1, co-authored with Keith Chen, Katherine Christensen, Elicia John, and Emily Owens, we use smartphone location data to track on-shift movement of police officers in the 21 largest US cities, enabling us to construct and examine police presence—what it means for an area to be "policed"—without relying on a department's cooperation. We find that police spend significantly more time in non-white neighborhoods, a disparity that persists even after controlling for population density, socioeconomic factors, and crime rates. Disparities in police presence also predicts a large share of observed racial disparities in downstream police actions such as arrests and stops. Importantly, our data facilitates cross-city comparisons, revealing unique issues leading to disparities across cities.

In Chapter 2, co-authored with Keith Chen and Emily Owens, we show how

policing could be endogenous to place-based investments effective at reducing crime, using the smartphone-based measure of policing. Exploiting the variation in Qualified Census Tract (QCT) status due to administrative rules under the Low-Income Housing Tax Credit (LIHTC) program, this study finds that police patrols increase by 13.5% in QCTs compared to non-selected but similar tracts. These increases can more than explain observed investment-induced violent crime reductions. This research challenges the notion that place-based investments can significantly reduce crime without considering the broader equilibrium effects on policing patterns.

In Chapter 3, also co-authored Keith Chen and Emily Owens, we train a convolutional neural network on Google Street View images to explore the relationship between physical space, perceived safety, and actual crime rates. The study aims to identify specific urban features that influence safety perceptions and the discrepancies between perceived and actual safety. By integrating generative AI tools, this research provides a new framework that could help identify physical features that could help potentially mitigate perceived safety and crime, providing actionable insights for urban planners and policymakers.

The dissertation of Yilin Zhuo is approved.

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2024

To my family, who have been always there for me.

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"Smartphone Data Reveal Neighborhood-Level Racial Disparities in Police Presence" (with M. Keith Chen, Katherine L. Christensen, Elicia John and Emily Owens), *The Review of Economics and Statistics*, 2023

INTRODUCTION

Crime and policing are multifaceted issues influenced by a myriad of structural, neighborhood, and individual-level factors. Starting with Becker (1968)'s seminal work on the economic approach to crime, which posits that individuals engage in criminal activities based on rational calculations of expected benefits and costs, research on crime and policing has expanded to encompass broader social and environmental considerations. This includes theories such as Social Disorganization Theory, which posits that crime rates are higher in neighborhoods with weak social institutions and poor community cohesion; Broken Windows Theory, which suggests that visible signs of disorder and neglect encourage further crime and antisocial behavior; and Crime Prevention Through Environmental Design (CPTED), which aims to reduce crime by modifying the built environment to enhance natural surveillance and access control.

Policing is a central component for crime reduction, but its role extends far beyond that, encompassing the maintenance of social order and fostering community relationships. Policing strategies are shaped by decisions at multiple levels—jurisdictional, community, and individual. At the jurisdictional level, police chiefs set overall priorities and policies, such as emphasizing hot spot policing or community policing. At the community level, supervisors implement these priorities by coordinating patrols and allocating resources effectively. At the individual level, police officers make on-the-ground decisions during patrols, influenced by their perceptions of safety and community needs. Neighborhood socioeconomic conditions also affect civilian demand for policing.

Traditional studies in the economics of crime and policing have often focused on analyzing police records. These studies have been instrumental in understanding the potential causes of criminal activity and the effectiveness of law enforcement responses. However, they are typically limited to single-city case studies due to data constraints, which restrict their generalizability and scope.

The advent of big data and novel data collection methods, such as smartphone location data, image data and machine learning techniques, has opened new avenues for policing and crime research. These tools enable a more granular analysis of policing patterns, evaluation of neighborhood interventions on crime and policing, and quantifying the role of physical environments on public safety.

This dissertation leverages these advancements to provide new insights into three areas: neighborhood disparities in police presence, the impact of place-based policies on policing and crime, and the influence of built environment on crime and safety perceptions.

Chapter 1, a joint work with Keith Chen, Katherine Christensen, Elicia John, and Emily Owens, investigates neighborhood-level racial disparities in police presence, by mapping the neighborhood movement of nearly ten thousand officers across 21 of America's largest cities using anonymized smartphone data. We find that police spend 0.36% more time in neighborhoods for each percentage point increase in Black residents, which persists after controlling for density, socioeconomic, and crime-driven demand for policing. Moreover, this chapter highlights the importance of cross-city comparisons and the role of departmental structures in shaping these disparities, and suggests that patterns of police presence has important implications on the racial disparities observed in downstream police actions.

Chapter 2, co-authored with Keith Chen and Emily Owens, extends this analysis to examine the impact of place-based policies, specifically neighborhood investments through the Low-Income Housing Tax Credit (LIHTC) program, on neighborhood police presence. By exploiting quasi-experimental variation in HUD rules designating Qualified Census Tracts (QCTs) that receive more neighborhood investments, we find place-based investments increase police presence by 13.5%. This increase is large enough to explain all observed crime reductions in QCTs, challenging the assumption that neighborhood investments alone can reduce crime. These findings highlight the importance of understanding law-enforcement responses to local development before framing economic investments as a substitute for policing.

Chapter 3, a work in progress also co-authored with Keith Chen and Emily Owens, employs deep learning models to analyze the relationship between physical environment, crime and safety perceptions. By training a convolutional neural network model on Google Street View images, this chapter shows that built environment has large predictive power over actual crimes. Additionally, the use of generative AI tools helps identify specific urban characteristics that influence the gap between perceived safety and actual crime.

In summary, this dissertation provides a multifaceted exploration of policing, neighborhood investments, and urban safety, utilizing innovative data sources and analytical methods. Our data and new approach are well suited to further research on US policing and crime, specifically to understand patterns in police presence and track policy implementation for public safety.

CHAPTER 1

Smartphone Data Reveal Neighborhood-Level Racial Disparities in Police Presence

1.1 Introduction

According to FBI statistics, Black people in America were arrested at twice the rate of White people in 2019 (OJJDP Statistical Briefing Book 2019). A large literature explores the causes of racial disparities in police enforcement actions, such as stops, searches, and arrests, including differences in socioeconomic status, criminal activity, and biased decision making by police officers (Banaji et al. 2021; Banks et al. 2006; Hoekstra and Sloan 2022; Rucker and Richeson 2021). Disparities in police enforcement are highly consequential for impacted civilians, but may not fully reflect disparities in the entirety of what it means for a person, or an area, to be "policed."

In this paper, we provide evidence on the following question: do police departments differentially patrol the more heavily Black, Hispanic, or Asian neighborhoods in their cities? A priori, police presence can either help, or harm, communities. Police presence can deter crime. It can also influence when and where crime is officially recorded. Finally, police presence is necessary for a stop, search or arrest to occur. Thus, detailed information on the neighborhoods where officers work during their shifts and on how the racial composition of neighborhoods varies both across and within cities can identify sources of disparities in later criminal justice outcomes. Unfortunately, few departments record detailed data on where officers are during their shifts, and even fewer make it available to researchers in a standardized way.

We use anonymized smartphone location data to identify and track the movements of individual police officers on patrol in 21 of the largest cities in the United States. We measure police presence as the total number of officer-hours spent in a census block group (a "neighborhood" with roughly 1,000 residents) over a ten-month period (Feb 2017 - Nov 2017), when the officer is moving through a neighborhood at 50 mph or less. These data identify where police spend their time and allow us to evaluate spatial patterns of policing at scale while protecting officer privacy.

Using these data, we quantify how patterns of socioeconomic status, crime, social capital, and race relate to local police presence within and across these cities. While we do not evaluate whether such resource allocation is socially optimal, we document the following facts: (1) Officer presence tends to be higher in non-White neighborhoods, both within and between cities, and there is a large amount of cross-city heterogeneity in this result, (2) Black neighborhoods have the highest officer presence, and though (3) the disparity in officer presence in Asian neighborhoods can be fully explained by neighborhood characteristics, (4) over two thirds of the increased police presence in more Black and Hispanic neighborhoods cannot be explained by neighborhood characteristics.

Generally, geographic analysis of policing at the sub-city level has measured policing in one of two ways. Researchers have studied downstream measures—outcomes of police officer and civilian interactions—and upstream measures—departmental decisions that are made before a civilian interaction (e.g., patrol assignments). Our research builds on a growing literature that examines the role of upstream measures of policing (e.g., in Chicago (Ba et al., 2021), in Dallas (Weisburd, 2021), in an English police department (Vomfell and Stewart, 2021), and in Milan (Mastrobuoni, 2019)). We extend these single city studies in two key ways. First, our smartphone dataset allows us to examine actual police presence in neighborhoods, rather than beat assignment or patrol car locations; this allows us to observe the potentially non-trivial amount of time officers spend outside their assigned patrol locations or their patrol car (Weisburd, 2021). Second, because our smartphone dataset is independent of city-level decisions to collect or release data (Goel et al., 2017), we can extend the single-city analyses that typify existing upstream studies to better understand policing within and across 21 of America's largest cities.

Our neighborhood-level analysis of GPS location data shows that police officers spend more time in places with larger Black, Hispanic, or Asian populations both between and within cities. While controlling for variation in socioeconomic status, social disorganization, and violent crime reduces these disparities, it does not eliminate the disparity in officer time spent in more Black or Hispanic vs. more White neighborhoods. This suggests that social interventions targeted at the "root causes" of crime may be unlikely to eliminate the racial and ethnic disparities we observe in American policing and confirms qualitative and historical research on upstream police presence across America (Hinton 2016; Rios 2011; Sharkey 2018), and patterns observed at the city level (Carmichael and Kent 2014).

While still descriptive, we also explore whether differences in police presence are associated with the racial composition of officers across cities. In contrast with existing single-department studies (e.g., Hoekstra and Sloan 2022; Ba et al. 2021), our results suggest that the additional police presence in Black neighborhoods is higher in cities where more patrol officers are Black. However, conditional on the share of Black patrol officers, increasing the share of Black front-line supervisors, who direct patrol officer activity, reduces the amount of time spent in Black neighborhoods. While not causal, this highlights the potential role of retention and promotion in police reform aimed at reducing racial disparities in the criminal justice system.

We also provide evidence that the nature of disparities in police presence differs across US cities. While racial disparities in some cities (e.g., Charlotte, NC) are largely associated with spatial differences in socioeconomic status (e.g., income, education, and civic engagement) others persist after controlling for these factors, and for spatial patterns of violent crime (e.g., Austin, TX).

Our work complements existing spatial analyses of downstream measures of policing, which have found that police engage in more enforcement actions in Black neighborhoods (Geller et al. 2014; Ba et al. 2021; Pierson et al. 2020). In the six cities (including New York City) with publicly available geocoded arrest data, we connect our upstream measures of neighborhood police presence to downstream arrests within that neighborhood. We then separate observed neighborhood arrest disparities into two parts: percent differences in officer-hours spent in a neighborhood and percent differences in arrests per officer-hour spent in that place. We find that differences in where officers spend time explain roughly 55% of the Black-White disparity in neighborhood arrests, conditional on neighborhood characteristics. Officers' higher propensity to make an arrest, conditional on being in a relatively more Black neighborhood, explains 45% of these disparities.

Taken as a whole, our findings suggest that disparities in exposure to police in the

US are associated with both structural socioeconomic disparities and discretionary decision making by police commanders and officers. This study provides novel data on police-civilian interaction to enable additional analyses of the factors driving these observed disparities in hopes of developing policy interventions to mitigate them. Finally, our police presence measure provides a new benchmark against which downstream police actions like stops and arrests may be objectively evaluated.

1.2 Methods

1.2.1 Data

The smartphone location data used in this study were provided by Safegraph and can now be obtained from Veraset, a company that aggregates anonymized location data from a suite of smartphone applications.[1] The smartphone data records "pings" denoting where a specific smartphone is located at a particular point in time. Pings are logged at irregular time intervals, whenever a participating smartphone application requests location information. The modal time between consecutive pings associated with a device is roughly 10 minutes. Our smartphone data covers more than 50 million smartphones, spanning the continental US, in a 10-month period from February 2017 to November 2017. While the dataset contains geolocation information from only a subset of all smartphones, previous studies have found it highly representative of the United States on numerous demographic dimensions (Chen et al. 2021).

We link the smartphone data to two other data sources: 1) police station location

[1]For more information on the Veraset data, see `https://datarade.ai/data-providers/veraset/profile`.

data published by the Department of Homeland Security, verified with each city's open data portal and google maps data, and 2) building rooftop geofence data provided by Microsoft, enabling us to associate each police station's latitude-longitude location to a geofence that delineates the convex hull of a building's rooftop boundary. To identify patrol officers in local city neighborhoods, we include police stations categorized as patrol stations, as headquarters, or as unspecified police facilities, resulting in a total of 316 stations across 21 of America's largest cities. A description of other data sources and data cleaning process can be found in Appendix A.1. It is important to note the selection of the cities in our sample was based on jurisdictional population and the physical construction of police buildings. Our sample was not determined by the investment the department chose to make in electronic monitoring of officers, or a department's decision to release the data publicly or enter into a research agreement with external parties (see Goel et al. (2017) for a discussion of these issues in the context of measuring police bias).

1.2.2 Measuring Police Presence

We infer whether a smartphone belongs to an officer by linking smartphone data to police stations' geofences in several steps. First, if a specific smartphone is observed in a police station geofence at least five days in a month, we identify it as belonging to a police employee in that month. We next infer each smartphone user's "home" as the smartphone's modal Geohash-7 (a 152m $\times$ 152m grid) that does not include a police station. We identify two home locations, for the early and the latter half of the year, to account for a potential summer move. Then, we identify patrol officers by looking for a specific pattern: leaving home, traveling to a police station, moving around the city (without returning home), returning to the police station, and then going home. The movements of that smartphone between the first and the last station visits are assumed to be the actual locations of a patrol officer while working a "shift." We require that shifts are bracketed by home visits that are no more than 24 hours apart, and are no shorter than four hours.[2] Under this definition, our officer smartphone GPS data sample consists of 9,833 officers that have at least one shift, with a mean shift length of 8.08 hours.[3]

To measure police presence in all census block groups ("neighborhoods") within the city's jurisdiction, we look at officers' smartphone pings when officers are "on shift" and outside of police stations, in a month during which the device appears in a police station on at least 5 days. We conceptualize police presence in a city neighborhood as the number of officer-hours spent in the neighborhood. Specifically, we match police officers' ping locations to block groups, exclude pings moving faster than 50 mph, and assign the duration of each ping as half of the time between its previous and next ping.[4] We then compute the sum of officer-hours from all officers' pings observed in the block group across the ten-month period. The resulting estimate of where police spend time on patrol is highly non-uniform, and as our later regression analysis will confirm, is strongly correlated with demographics in ways that produce large racial disparities.

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[2]All results in our analysis are highly robust to limiting our sample to 8 to 12 hour shifts, requiring shifts bracketed by home visits no longer than 18 hours and excluding shifts with long hours spent within the police stations. These results are available on request.

[3]Figure A.1 in the Online Appendix displays the temporal and spatial pattern of pings for one likely LAPD officer.

[4]Using other constructs of police presence yields qualitatively and quantitatively similar results. Replications of our analysis using the number of distinct officer shifts, alternate (or no) speed thresholds, are available on request.

1.2.3 Validity Check

Our study focuses on America's largest cities. While our data do not capture the universe of police officers in a city, our estimates of the number of officers in a city satisfy many tests of face validity as a measure of police presence. The number of patrol officer devices that we observe across US cities is highly correlated with FBI estimates of police force size ($\rho = 0.98$ for total count measures, $\rho = 0.49$ for per capita measures).[5] Further, we can probabilistically impute each device's "race" using its home census block's racial composition. There is essentially a one for one unconditional relationship between the imputed racial composition of the police departments in our sample and the racial composition reported by the department in the 2016 Law Enforcement Management and Administration Statistics (LEMAS); conditional on the racial composition of the city, a one percentage point increase in our estimate of the percent of the police force that is White (Black, Hispanic, Asian) is associated with a 0.6 (0.7, 0.9, 0.6) percentage point increase in the reported percent of the force that is White (Black, Hispanic, Asian) in the LEMAS. [6]

We conduct an additional residence-based validity check in New York City, in which public records provide summary data on where NYPD officers live at the zip code level. We compare the NYPD's official record of the number of officers who live in a zip code with our smartphone-based estimate of the number of officers that

[5] Appendix Figure A.2 plots the specific values for each city.

[6] Appendix Figure A.3 plots the raw data. The p-value testing whether the slope between the smartphone GPS measures and LEMAS measures of officer racial composition is equal to 1 is 0.45 for Black, 0.91 for White, and 0.11 for Hispanic. The slopes between the two estimates for the share of Asian is significantly different from 1, though Asians account for only 2.5% of the police force across the cities in LEMAS. Table A.1 in the Appendix further reports the correlation conditional on each city's racial composition.

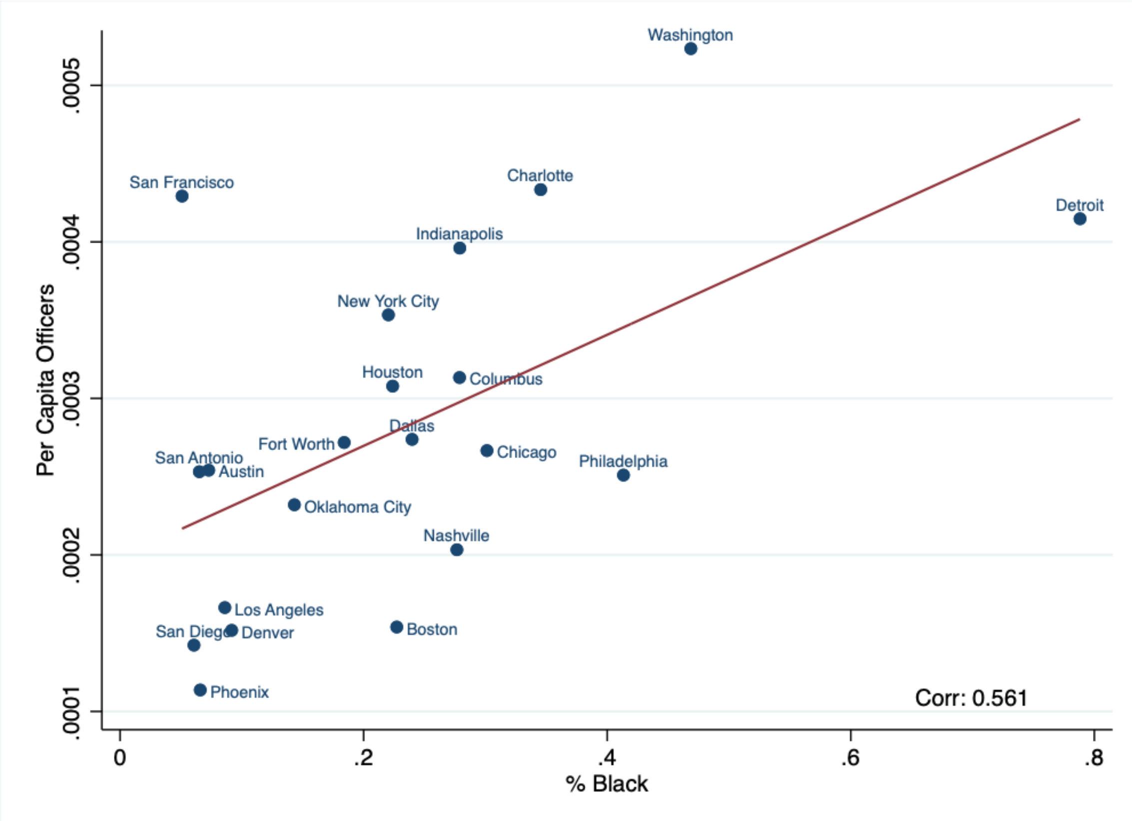
"live" in that same zip code. There is a strong and positive correlation ($\rho = 0.71$) between official NYPD records and our smartphone-based measures.[7]

There is a well-established positive correlation between the fraction of a city population that is Black and the number of sworn police officers per capita (Carmichael and Kent 2014; Stults and Baumer 2007). A basic test of construct validity is whether we observe a similar pattern in our data. Figure 1.1 plots per capita patrol officers (i.e. smartphones that have at least one "shift") against the share of Black population in the 21 cities, replicating the positive correlation between the fraction of city residents who are Black and our measure of total officers per capita. Our GPS-based measure of police presence also has significant predictive power on downstream measures of police actions, such as stops and arrests. After adjusting for nonlinearity, the correlation between our measure of police presence and the number of arrests—which we observe in six cities—ranges from 0.44 (Washington) to 0.68 (Austin). Similar positive and significant correlations for police stops for nine cities with publicly available geocoded records are observed as well. [8]

[7]Figure A.4 in the Appendix plots the zip code level data.

[8]Appendix Figures A.5 and A.6 plot these city by city graphs.

Figure 1.1: Correlation Between % Black and Officers per capita in a City



Notes: Per capita officers is defined as the number of likely patrol officers on "shift" (identified with smartphone data) divided by the city population (2013-2017 American Community Survey estimate). We identify patrol officers on "shift" by looking for a specific pattern in smartphones that visit police stations at least 5 days in a month: Leaving "home", traveling to a police station, moving around the city (without returning home), returning to the police station, and then going home. The correlation coefficient between the two measures is reported.

1.3 Results

1.3.1 Neighborhood Correlates of Police Presence

Understanding how police provide services to people from different racial groups is important from both an equity and an efficiency perspective, and our data are uniquely suited to provide new evidence on this issue. Within each neighborhood, we use 2013-2017 American Community Survey (ACS) data to estimate the percent of neighborhood residents who report being in a particular racial or ethnic category. Table A.2 in the Appendix shows summary statistics for police presence measures as well as neighborhood correlates.

Table 1.1 presents our estimates of the spatial determinants of policing in America's largest cities. Our smartphone GPS data reveal a strong relationship between the racial and ethnic composition of a neighborhood and police presence. In the largest cities in America, police spend 3.6% more time in places where the fraction of residents who are Black is 10 percentage points higher, 5.2% more time in places where the share of Hispanic residents is 10 percentage points higher, and 3.7% more time in a place where the share of Asian residents is 10 percentage points higher. [9]

Why do these disparities exist? Differences in where police spend their time can reflect decisions made by individual officers - who ultimately decide where they will go on the job - and department-level directives on patrol assignments. Both involve an assessment, by department or officer, of the residential "need" for police presence in an area. Applicable departmental policies, officer decisions, and residential demand

[9] $arsinh(y) = ln(y + \sqrt{y^2+1}) \approx ln(2y) = ln2 + lny$; hence the interpretation of β is similar to a log-transformation. A 10 pp increase in % Black (Hispanic, Asian) is associated with a $e^{0.35\times0.1} - 1 = 3.6\%$ ($e^{0.505\times0.1} - 1 = 5.2\%$, $e^{0.35\times0.1} - 1 = 3.7\%$) increase in police hours.

for police presence can all be related to the racial composition of a neighborhood. We use a multivariate OLS regression framework to provide insight into why police may tend to spend more time in places with relatively more Asian, Black, and Hispanic residents.

In column 2, we include city fixed effects. Conditioning on geography differences out any preference of officials in cities with different residential racial compositions for a particular type of policing, that may contribute to observed disparities (e.g., departments in cities with larger Black populations encouraging officers to make aggressive Terry stops or use predictive policing, see Meares 2015 or Brayne 2020). City fixed effects also address concerns that our results are driven by a correlation between a city's racial distribution and the accuracy of our smartphone data. Focusing on variation within cities almost doubles the estimated extra time officers spend in more Black neighborhoods, and reduces the differential policing of more Asian and Hispanic neighborhoods by 15-19%. [10]

We next introduce proxies for residential demand. If officers spend more time in places where there are more people, variation in population density that is correlated with race may contribute to spatial differences in policing. Residents may request that officers respond to crimes, and in particularly disadvantaged neighborhoods, police officers may be one of the few remaining providers of any social service that people need (Lum 2021). Racial disparities in police presence may therefore stem from racial inequity in the quality of non-policing institutions.

We draw on existing social science literature to approximate components of resi-

[10] In the Online Appendix A.4, we show that the relationship between exposure to police presence and the composition of the block group that is Black or Hispanic is highest during the middle of an officer's shift.

dential demand for polic presence. A lack of educational opportunity and well-paying jobs are established root causes of crime (Messner and Rosenfeld 1997). Of course, neighborhoods where residents have low incomes but high social capital (i.e. high degrees of social cohesion and community engagement) are places where police rarely need to respond to acts of violence or property destruction (Sampson and Raudenbush 1999). Following Martin and Newman (2015), we measure social capital using the fraction of 2010 census forms returned by residents. Finally, police officers go where violent crime exists. We estimate the crime-driven demand for policing based on the location of homicides known to the police. While imperfect and sparse, police records of homicides are generally thought to be the most accurate, in the sense that reporting of homicides is unlikely to be as influenced by police presence as reporting of other types of crime, and victimization data suggests that variation in homicides is highly correlated with variation in other crimes (Levitt 1998). We calculate the distance from the neighborhood center to the closest homicide in 2016, treating these rare events as an extreme expression of underlying social issues, implicitly assuming both that crime is spatially clustered and that this distance is negatively correlated with exposure to other types of crime. Additionally, we control for the number of homicides in 2016, by neighborhood, to account for potential variation in crime rates.[11]

In column 3 of Table 1.1, we condition our estimates of local police presence in different types of U.S. neighborhoods on measures of density, socioeconomics, social cohesion, and violence. Differential residential demand for police presence, some of

[11] Alternative measures of demand for policing, specifically using additional years of homicide data and 311 calls for service in New York City (Shah and LaForest 2021) are explored in the Online Appendix A.2 - none lead to substantively different conclusions.

which is created by decisions made in other policy domains, explains approximately 35% of the disparate exposure of people living in relatively Black neighborhoods, 33% of the disparate exposure of people living in relatively Hispanic neighborhoods, and can explain all of the additional exposure of people living in relatively Asian neighborhoods—even suggesting that more Asian neighborhoods have less police presence than one might expect based on social conditions.[12] The residual correlation between racial composition and police presence in column 3 reflects decisions at the police command, and officer level.

Diversifying the officer ranks is one city-level policy that is central to many police reform efforts. With this in mind, we compare how disparities in police presence vary with the racial composition of a city's police force. We do this in two ways: including the mean-centered interaction between the share of Black residents and the share of police officers that are Black in column 4, and interactions with both the share of police supervisors and patrol officers that are Black in column 5.[13] Column 4 suggests the additional exposure to police in Black neighborhoods is only slightly larger in cities with a larger share of Black officers; while this cross-city comparison is not necessarily inconsistent with existing work, it stands in contrast to single city studies finding that Black officers spend less time in Black neighborhoods (Ba et al., 2021). Further, column 5 implies that, conditional on the composition of patrol officers, there may be less police presence in Black neighborhoods when more

[12] In the Online Appendix A.3, we show that our findings are qualitatively identical when we model police presence during non-working hours (excluding weekday 9 am - 5 pm), and in New York City when we exclude census block groups in tourist destinations. In both of these situations, the demographics of residents may differ from the demographics of the ambient population.

[13] Appendix Figure A.8 reveals substantial cross city variation in the share of Black police officers and supervisors, and a meaningful difference between the share of Black officers and supervisors, despite a high correlation between the two measures.

front-line supervisors are Black – though this effect is not statistically significant at conventional levels.

Table 1.1: Disparities in Neighborhood Police Exposure

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Police Exposure in a Census Block Group: arsinh(Hours)				
% Black	0.350***	0.512***	0.333***	0.346***	0.343***
	(0.0328)	(0.0354)	(0.0481)	(0.0509)	(0.0528)
BG % Black X Police: % Black				0.0985	0.959
				(0.307)	(0.886)
BG % Black X Supervisor: % Black					-0.804
					(0.805)
% Hispanic	0.505***	0.404***	0.270***	0.242***	0.221***
	(0.0343)	(0.0365)	(0.0566)	(0.0593)	(0.0603)
% Asian	0.360***	0.294***	-0.0566	-0.0756	-0.0695
	(0.0735)	(0.0787)	(0.0828)	(0.0844)	(0.0847)
Log Population			0.418***	0.431***	0.457***
			(0.0211)	(0.0219)	(0.0225)
% College Graduates			1.079***	1.129***	1.151***
			(0.0680)	(0.0704)	(0.0713)
Median Household Income (1K)			-0.00423***	-0.00418***	-0.00396***
			(0.000396)	(0.000405)	(0.000408)
Census Form Return Rate			-1.308***	-1.352***	-1.417***
			(0.127)	(0.132)	(0.135)
Distance to nearest 2016 homicide (km)			-0.120***	-0.115***	-0.112***
			(0.00665)	(0.00734)	(0.00755)
Homicide Count 2016			0.203***	0.205***	0.204***
			(0.0199)	(0.0206)	(0.0211)
Observations	23,682	23,682	22,521	20,961	20,112
R-squared	0.010	0.104	0.167	0.152	0.156
City FE	No	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of exposure disparities among census block groups i (BGs) in 21 of the largest US cities: $arsinh(Hour_i) = \beta_0 + \beta_1 X_i + \epsilon_i$. The dependent variable is police hours observed in a BG (excluding pings moving faster than 50 mph), transformed into arsinh values. % Black, Police: % Black and Supervisor: % Black are mean-centered. Household income is measured in thousands of dollars, census return rates range from 0-1. Robust standard errors are reported in parentheses. Results are qualitatively and quantitatively similar to running all regressions with log dependent variable and dropping zero-valued observations, or clustering at the tract level, and are available on request. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

More specifically, relative to a city with the mean number of Black officers and supervisors and conditioning on social conditions, a 10 percentage point increase in the number of Black officers would mean that a 1 percentage point increase in the share of residents who are Black is associated with a 0.43% increase in police presence.[14] If there were a simultaneous 53.3 percentage point increase in Black supervisors, we would observe no relationship between the fraction of neighborhood residents who are Black and the time police spend in that neighborhood.[15] While correlational in nature, our findings suggest that efforts to hire more Black police officers, without parallel efforts to retain and promote those officers, may not reduce disparities in how the public is policed.

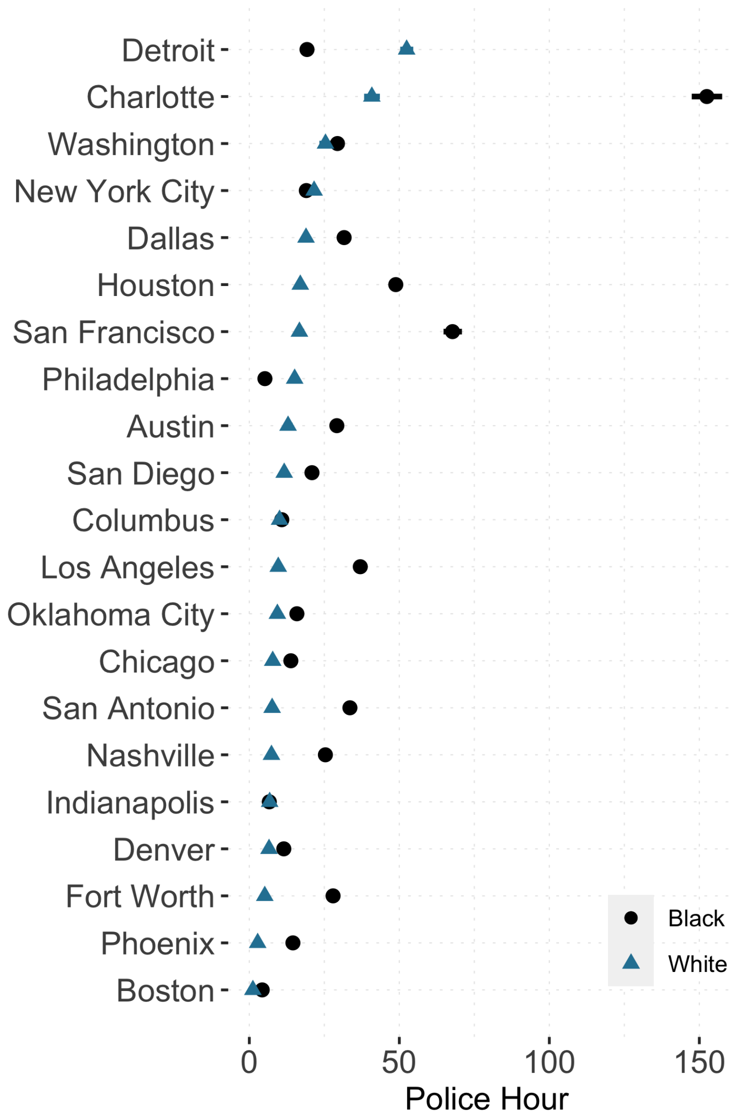
1.3.2 Cross-city variation in correlates of police presence

Our findings suggest substantial differences in the level of ambient police presence in non-White neighborhoods across the United States, and this difference is largest in Black (relative to White) neighborhoods. Given this, and the long and fraught history of the policing of Black people in the United States, in this section we focus on police presence in relatively Black versus relatively White neighborhoods. First, in Figure 1.2, we show police presence in neighborhoods with the greatest share of Black and White residents, respectively, to highlight the range of disparities in Black-White neighborhood policing across major US cities. There is little difference in the amount of time that police spend in the "most White" and "most Black" neighborhoods in Boston, but over 100 more hours of total policing in the "most Black" neighborhoods in Charlotte than in the "most White."

[14] $e^{(0.01\times(0.333+0.959\times0.1))} - 1 = 0.43\%$

[15] $0.333 + 0.0959 - (0.804 \times .533) = 0$

Figure 1.2: Police Exposure in Blackest and Whitest Neighborhoods

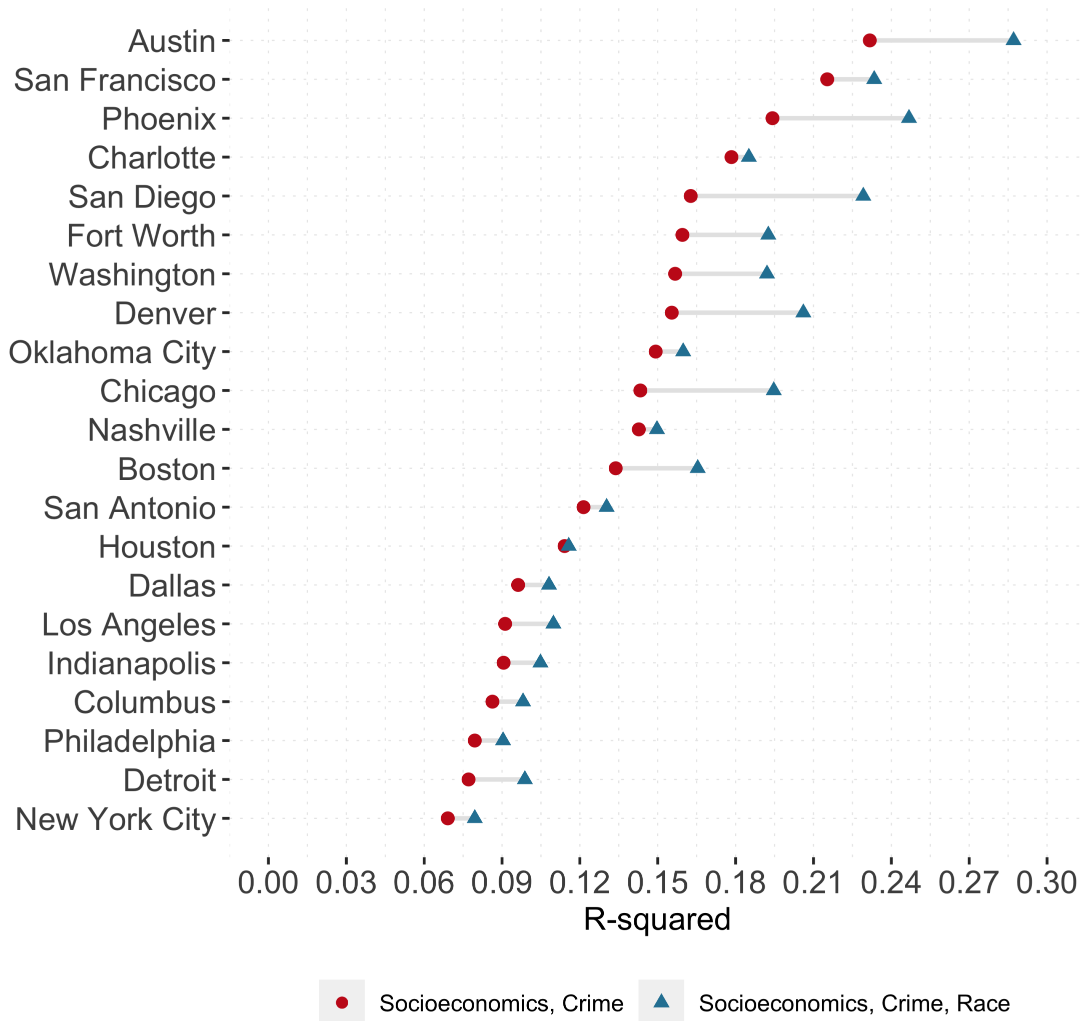


Notes: This figure plots the average police hours observed in the Blackest (Whitest) neighborhoods in a city, defined as the block groups where share of Black (White) residents is over the 95th percentile of the city's distribution. The cities are ordered by police presence in the whitest neighborhoods.

Of course, these disparities can have many sources. In Figure 1.3, we plot, for each city, how much of the spatial variation in police presence can be explained by spatial variation in our proxies for "demand" for police, and how much the explanatory power of our models increases when we add controls for racial composition. This shows the extent to which Black-White disparities in exposure to police can persist even when considering spatial differences in socioeconomic status—which may reflect historical and contemporary race-based social and economic inequality. We document substantial variation across cities in the role of this structural inequality in explaining policing disparities. For example, while Figure 1.2 reveals large differences in the ambient police exposure of Charlotte residents in the most Black and most White neighborhoods, Figure 1.3 reveals that spatial disparities in socioeconomic status explain almost all of these differences. These structural disparities in Charlotte are city-level issues that cannot be addressed solely by the city's police department. In contrast, racial disparities in police presence are absolutely smaller in Austin, but incorporating Black, Hispanic, and Asian residential patterns increases the amount of spatial variation in police presence that we can explain in that city by 27%. This suggests substantially more scope for changes in police policy to reduce criminal justice disparities in Austin, TX.[16]

[16] Appendix Figure A.9 also plots the city-specific estimate of Black-White disparity.

Figure 1.3: Variance of Police Hours Explained by Socioeconomics, Crime, and Race



Notes: This figure reports the R-squares of the following two OLS regressions for each city: (1) $arsinh(Hour_i) = \beta_0 + \beta_1 Socioeconomics_i + \beta_2 Crime_i + \epsilon_i$, and (2) $arsinh(Hour_i) = \beta_0 + \beta_1 Socioeconomics_i + \beta_2 Crime_i + \beta_3 Race_i + \epsilon_i$. *Socioeconomics* include log population, % college graduates, median household income, census form return rate. *Crime* include distance to nearest homicide and homicide count in 2016. *Race* include percent Black, percent Hispanic and percent Asian in the block group.

1.3.3 Using Police Presence to Understand Police Enforcement

The empirical observation that police are more ambiently present in Non-White neighborhoods provides support for the construct validity of our data, as this correlation has been repeatedly demonstrated at the city level (Carmichael and Kent 2014). When taken in the context of existing qualitative and legal scholarship on modern policing, this also raises equity concerns.

To quantify the extent to which racial disparities in upstream police presence are associated with disparities in one consequential downstream law enforcement action—arrest—we create three neighborhood-level measures: how much time officers spend in a given neighborhood, how many arrests are made in that neighborhood, and how many arrests are made per hour of police presence. Our measure of police presence can therefore distinguish between two very different sources of racial disparities in arrests: variation in ambient police presence that is correlated with race, and differences in behavior of officers across different neighborhood contexts.

Consistent with studies of downstream measures of policing, Table 1.2 confirms that in six cities for which we have both police presence and arrest data (New York City, Los Angeles, Chicago, Dallas, Austin, Washington), officers spend more time, and make more arrests in neighborhoods with more Black residents than the typical neighborhood in each city. Column 1 shows that the Black-White disparity in police presence is 16% larger, the Hispanic-White disparity 48% smaller, and the Asian-White disparity 6% smaller, in this set of cities that choose to make geocoded arrest data public.

While column 2 shows that officers make approximately 21% more arrests in neighborhoods where the share of residents who are Black is 10 percentage point higher, in column 3 we show that they make almost 13% more arrests per hour present.[17] We find that our proxies for neighborhood demand for police do explain part of the increased number of arrests in more Black neighborhoods, but in this sub-sample, they do not explain the increased police presence—in fact, the residual disparities increase. Whatever the source, this disparity in the propensity of an officer to make an arrest in more Black neighborhoods, while keeping other socioeconomic variables constant, explains less than half of the residual neighborhood disparity in the total number of arrests made.[18] This implies that the added time that police spend in Black neighborhoods may be a central source of Black-White disparities in arrests, in addition to an officer's decision in a particular encounter. It is outside the scope of this paper to evaluate the welfare implications of this empirical fact, which could be due to over-(or under-)policing, police using different standards to determine if people in different groups are suspicious enough to warrant an arrest, differences in unobserved criminal activity, or to differences in how police officers spend time in these neighborhoods.[19] Consistent with Meares (2015), our results

[17]The semi-elasticity is approximately equal to $e^{1.910\times 0.1} - 1 = 21\%$ ($e^{1.19\times 0.1} = 13\%$) for a 10 percentage point increase in % Black.

[18]Specifically, the elasticity of arsinh-linear model is $\beta\bar{x}\sqrt{\frac{y^2+1}{y^2}} \approx \beta\bar{x}$. Differences in the propensity of officers to make an arrest while in a relatively more Black neighborhood explain 43% ($\frac{\beta_2\bar{x}}{\beta_1\bar{x}} = \frac{0.601}{1.388}$) of the neighborhood arrest disparity, and the remaining 57% is explained by differences in the police presence across more Black versus more White neighborhood. Conditioning on socioeconomic characteristics, additional police presence in relatively more Hispanic neighborhoods explain 62% ($1 - \frac{0.433}{1.154}$) of the Hispanic-White neighborhood arrest disparity. Online Appendix Table A.7 also reveals a highly similar pattern regarding stop disparities.

[19]Police using different decision rules in more and less Black neighborhoods, while an axiomatic example of discrimination, is not necessarily illegal; Illinois v Wardlow, 528 U. S. 119 (2000) established that officers can use the predetermined designation of an area as "high crime" in determining how likely it is that someone has (or is) engaged in crime, creating a legal basis for a stop. If places with more Black or Hispanic residents are more likely to be known to police as "high crime" places, then this would lower the standard of individualized suspicion needed to make a constitutionally permissible stop.

suggest that in order to reduce disparities in criminal justice, reducing the scope for racial bias both in officers' decisions during civilian encounters and in departmental directives detailing where officers go and who they surveil may be warranted.

Table 1.2: Disparities in Neighborhood Police Exposure and Downstream Disparities

VARIABLES	(1) arsinh Hours	(2) arsinh Arrests	(3) arsinh Arrests/Hour	(4) arsinh Hours	(5) arsinh Arrests	(6) arsinh Arrests/Hour
% Black	0.431***	1.910***	1.190***	0.650***	1.388***	0.601***
	(0.0459)	(0.0398)	(0.0402)	(0.0641)	(0.0581)	(0.0584)
% Hispanic	0.211***	1.611***	1.059***	0.548***	1.154***	0.433***
	(0.0463)	(0.0422)	(0.0391)	(0.0747)	(0.0683)	(0.0659)
% Asian	0.311***	0.712***	0.233**	0.327**	0.219*	-0.154+
	(0.0932)	(0.0851)	(0.0714)	(0.100)	(0.0913)	(0.0813)
Log Population				0.510***	0.499***	-0.0599**
				(0.0309)	(0.0267)	(0.0222)
% College Graduates				1.468***	0.691***	-0.701***
				(0.0919)	(0.0850)	(0.0768)
Median Household Income (1K)				-0.00266***	-0.00401***	-0.000846*
				(0.000503)	(0.000464)	(0.000392)
Census Form Return Rate				-0.704***	-1.798***	-0.820***
				(0.168)	(0.145)	(0.143)
Distance to nearest 2016 homicide (km)				-0.143***	-0.157***	0.00876
				(0.0134)	(0.0123)	(0.0115)
Homicide Count 2016				0.230***	0.355***	0.0995***
				(0.0277)	(0.0224)	(0.0224)
Observations	12,748	12,748	12,708	12,098	12,098	12,062
R-squared	0.052	0.240	0.196	0.127	0.326	0.212
City FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of disparities in exposure, arrests, and arrests per hour among census block groups i (BGs) across 6 cities: $arsinh(Y_i) = \beta_0 + \beta_1 X_i + \epsilon_i$. Coefficient estimates of all variables in X_i are reported in the table. The six cities with publicly available geocoded arrest data are: New York City, Los Angeles, Chicago, Dallas, Austin, Washington. The dependent variables (Y_i) are: police hours observed in a BGs (excluding pings moving faster than 50 mph, mean 26.7), number of arrests in that BG (mean 40.1), and the ratio of those two measures (mean 10.2), all transformed into arsinh values. Household income is measured in thousands of dollars, census return rates range from 0-1. Robust standard errors are reported in parentheses. Results are qualitatively and quantitatively similar to running all regressions with log dependent variables and dropping zero-valued observations, or clustering at the tract level, and are available on request. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

1.4 Conclusion

We conclude by noting that a positive correlation in the provision of policing and the concentration of Black residents stands in contrast with documented spatial patterns of other institutional investment in neighborhoods with concentrated Black populations, which Derenoncourt (2022) also documents at the city level. Census tracts where more of the residential population is Black are less, not more, likely to have a large grocery store, nearby hospital, or local banking services (Walker et al. 2010; DeYoung et al. 2008; Lieberman-Cribbin et al. 2020; Yearby 2018). During the 2016 election, Chen et al. (2019) found that voting lines moved more slowly in places with larger Black populations, suggesting under-investment in polling services in places where we observe larger investments in ambient policing.

Our data are well suited to further research on policing in the United States. First, smartphone location data provide insight into officer presence in communities that traditional measures of policing cannot fully capture. Measuring officer presence informs estimates of which communities are at risk of more serious police encounters, like arrest or the use of lethal force. Second, our smartphone location data do not depend on software purchased by or developed for a particular policing agency, allowing us to map officer locations in cities across the United States using a consistent methodology. This is an advantage over technologies like Automated Vehicle Locators and body cameras, because it provides enhanced visibility into the unreported and highly discretionary activities of police officers at work. Finally, data on where officers spend their patrol time grants researchers and practitioners new abilities to understand patterns in police presence and track the implementation of departmental policies that shape the provision of public safety.

CHAPTER 2

Does Neighborhood Investment Actually Affect Crime? New Evidence from LIHTC and Smartphone-based Measures of Policing

2.1 Introduction

Place-based programs aimed at improving the local physical environment have been shown to be effective in reducing crime (Branas et al. 2020). These investments can directly address physical disorder, through vacant lot clean up and green space provision (Branas et al. 2018); alternatively, they may provide financial incentives to third parties to enhance the local built environment, such as the Low Income Housing Tax Credit and Opportunity Zone programs (Freedman and Owens 2011; Diamond and McQuade 2019). To the extent that such programs also target resources at historically under-served and marginalized populations, crime reduction through place-based, non-criminal justice, policy interventions appear to be a way to simultaneously address both social inequality broadly and socioeconomic disparities in criminal justice contact.

The literature generally argues that place-based programs work by reducing criminal propensity and/or criminal opportunities. Classic experiments (e.g. Zimbardo

1969) and more recent quasi-experimental studies (e.g. Kuo and Sullivan 2001a,b) find that reducing physical disorder lowers individual propensities for aggressive and violent behavior. Conceptually, place-based investments can alter the costs and returns to criminal behavior. Reduction in vacant lots and abandoned buildings could mean fewer opportunities for criminal activities (Cui and Walsh, 2015b; Branas et al., 2016); security cameras in new housing and on the street (Diamond and McQuade, 2019; Gómez et al., 2021), improved street lightning (Chalfin et al., 2022) and more foot traffic can deter potential offenders (Jacobs, 1961b; Branas et al., 2018; Farrington and Welsh, 2002). Finally, a better neighborhood environment could facilitate social interaction and signal that neighborhoods are being taken care of, further reducing social disorder and crime (Sampson et al. 1997).

These interpretations imply that place-based investments could serve as an alternative means of crime control that does not involve potentially disparate and costly policing, and the subsequent criminalization of civilians, as a central component. An important caveat to these interpretations is that the production of crime is multilayered and multifaceted; existing research on place and crime typically frames causal results as marginal effects, assuming all other factors are constant, and does not account for potential general equilibrium effects where environmental changes affect multiple determinants of criminal activity.

In particular, very little is known about how place-based investments may impact police, a potentially important oversight because of the strong causal relationship between policing and crime (e.g. Braga and Bond 2008; Di Tella and Schargrodsky 2004; Braga et al. 2019; Weisburd 2021). Failing to account for police responses to neighborhood investment could lead to under, or overestimates of the impact of physical disorder on individual criminal behavior, and may either under or overstate the potential of place-based investment as an alternative to law enforcement.

In this paper, we build on the literature linking changes in the physical environment to crime by quantifying an important, but previously overlooked, mechanism: changes in policing patterns in response to place-based investment. An important reason for this gap is an absence of suitable data. We use anonymized smartphone location data to address this challenge and measure police patrols in neighborhoods across 18 large US cities.

Police departments are hierarchical organizations, and decisions on where police officers spend time are made at multiple organizational levels. At the top of hierarchy, police chiefs, typically mayoral appointees, set departmental priorities regarding direct crime control and/or partnerships with local communities. Those priorities are implemented by command staff, generally holding the title of captain, who can identify which strategies best meet those goals in their specific units. The front line supervisors, typically sergeants, allocate officers across geographic beats and set priorities for officers' tasks before each work shift. While on duty, police officers are directed to respond to calls for service from local residents, on demand. During any remaining uncommitted time, police officers have discretion over where and how to patrol.

Local investments can affect decisions at all levels. At the supervisory level, chiefs will vary in their commitment to geographically-focused policing strategies; hot-spot policing and problem-oriented policing, for example, involve directing police officers to address the physical and social disorder in crime "hot spots" (Weisburd and Telep, 2014; Braga and Bond, 2008; Braga et al., 2015), and ethnographic observation documents raiding vacant lots and buildings as an important part of the crimedeterring activities by police (Branas et al. 2018). To the extent that reductions in the physical disorder lower a supervisor's perceived likelihood of criminal activities being an issue in these areas, we might observe a drop in police presence commensurate with the reduced need for police to respond to problems. Alternatively, chiefs may want to mirror the broader support for community improvement, and supervisors may increase police presence as a show of political support for the local government (the model underlying Levitt 1997).

During a work shift, there are similar varying demands on an officer's time. The potential for police to increase their patrol to respond to greater demand for police service as neighborhoods improve has been discussed in qualitative and correlational quantitative research on gentrification. Recent studies of geographic patterns of low-level police enforcement action have documented higher misdemeanor arrests and citations in gentrifying areas (Collins et al., 2021; Beck, 2020; Beck and Goldstein, 2018; Laniyonu, 2017), and that police play an important role in negotiating relationships between long time residents and new immigrants attracted by the local economic development (Huey, 2007). Police are directed to go where crime is reported, and under-invested neighborhoods with high disorder may be places where law enforcement is one of the remaining means to address immediate social problems (Wilson and Kelling 1982; Lum 2021). Finally, police officers can choose where to spend their uncommitted time, and preferences for workplaces with a better environment could increase officer presence in neighborhoods that reduce physical disorder (Ba et al., 2021).

To understand the net impact of these possible responses, we study how police presence changes in response to a specific place-based program that lends itself to causal identification—an increased rate of neighborhood investment in low-income neighborhoods identified as Qualified Census Tracts (QCTs). We estimate how policing changes from 2017 to 2019, and how these changes contribute to local crime reduction, apart from any individual response, in the 18 largest US cities.[1] Designated by the US Department of Housing and Urban Development (HUD), QCTs are low-income census tracts that could receive up to 30% larger tax incentives for construction and rehabilitation of affordable rental housing under the Low Income Housing Tax Credit (LIHTC) program. In addition to the LIHTC program, QCTs may receive investments from other place-based programs. For example, small businesses located in QCTs have priority in federal contracts under the Historically Underutilized Business Zone (HUBZone) program.

We follow Freedman and McGavock (2015) and exploit quasi-experimental variation in the QCT status generated by a HUD administrative rule that, at most, 20% of a metropolitan area population may live in QCTs. To be eligible for QCT status, tracts need to meet either HUD's income or poverty criteria. Because of this rule, in some cities census tracts with income and poverty rates that would qualify them as QCTs are not designated as such. We estimate the effect of QCT status by comparing changes in the physical environment, crime and policing from 2017 to 2019 among similar neighborhoods that are all eligible to be QCTs, but have different QCT designations due to the population cap.

Within a city, QCTs are on average more economically disadvantaged than eligible but non-selected tracts under HUD's designation rule (see section 2.2 for details). We therefore employ a doubly robust strategy from Sant'Anna and Zhao (2020) to match QCTs with eligible but non-selected tracts on a set of ACS demographic and housing

[1]The selection of the 18 cities in our sample is based on the availability of geocoded crime incident data, as well as the smartphone-based police presence data from Chen et al. (Forthcoming).

characteristics, while also accounting for city-specific changes in our outcomes. In other words, we assume that the best counterfactual for the observed change in a given QCT is the observed change in one in a different city that, prior to 2017, had the same absolute level of neighborhood features including population, age, income, college shares, and housing characteristics.

We first qualitatively replicate existing literature on the effect of QCT status on neighborhood physical and social condition, showing that this identification strategy enables us to detect meaningful neighborhood changes with sufficient statistical power in our sample. Over two years, we observe 3 more LIHTC-subsidized properties placed in service in QCTs than eligible but non-selected tracts, and for a subset of cities with geocoded 311 call data, QCT-spurred development reduces the number of requests for street light repair by 17%. We also detect changes in the socioeconomic environment in QCTs that reflect gentrification, such as an increase in the number of residents with higher earning jobs. There is also suggestive evidence of a 3% increase in street traffic measured by the total non-police visits, albeit with a noisier point estimate.

Our main results indicate a 13.5% increase in officer-hours present in QCTs relative to eligible but non-selected tracts. Consistent with Freedman and Owens (2011) and Diamond and McQuade (2019), we find a detectable, marginally significant 11% drop in violent crime rates in QCTs compared to eligible but non-selected tracts, with no significant change in property crime rates. Using estimates of police elasticity on crime from Weisburd (2021), we cannot reject the hypothesis that increased patrols can account for all of the observed violent crime reduction in QCTs. Further, relative to a regression approach, our doubly robust approach highlights suggestive evidence that increased police patrol may come at the cost of reduced patrol in eligible but non-selected tracts that border designated QCTs. This raises potential equity concerns when spatially targeted infrastructure investment coincides with "zero-sum" police allocation.

We further show that increased police presence is more pronounced in QCTs with older housing stock and a higher proportion of Black residents. This observed heterogeneity is particularly notable, as it implies potentially increased, rather than decreased, racially disparate policing in response to neighborhood development programs like LIHTC. Our central results are robust to excluding cities without binding population caps or the most weighted tracts, or employing alternative police presence measures or specifications, such as allowing differential time trends in high or low poverty tracts within a city, or different matching schemes.

Finally, we use Google Street View images to train convolutional neural network models to quantify urban appearance. Google Street View images are typically captured early in the morning to avoid images of people, providing an opportunity to potentially disentangle the environmental influences on crime and policing from local ambient population. By training our model with a publicly available urban perception dataset and predicting urban perception on our downloaded street view images, we find that QCT-spurred investments do make people more likely to perceive the built environment in QCTs as safe, beautiful and wealthy. At the same time, training the model to separately predict local policing and actual crime incidents reveals that, in the absence of changes in the ambient population, policing is predicted to weakly increase, and crime is predicted to weakly decrease in QCTs compared to similar non-QCTs. This suggests that the observed larger changes in both police presence and crime are less likely to result from direct responses to the changed infrastructure.

Taken as a whole, the finding that police respond endogenously to a changing neighborhood environment underscores the need for further investigation into the relationship between neighborhood investment and crime. Our findings do not disprove a direct link from environmental improvement to reductions in violent behavior, but rather confirm that local investment will have broad impacts. Our results imply that, in terms of understanding the factors that lead an individual to offend, estimating the crime-reduction effect of QCT-spurred development, without taking into account policing changes, could potentially overestimate the direct impact of built environment on criminal behavior. When considering policy responses to increased crime, posing investments in local infrastructure as alternatives to increased policing may therefore be misleading.

2.2 Empirical Strategy

Central to our analysis is the identification of variation in the physical environment that can be used to credibly identify its causal impact on policing. We use changes generated by the Department of Housing and Urban Development (HUD)'s Low Income Housing Tax Credit (LIHTC) program, specifically the process by which it designates certain neighborhoods as Qualified Census Tracts (QCTs).

The LIHTC program, initiated in 1987, is the largest federal housing program that subsidizes investment in affordable rental housing construction and rehabilitation for low-income households. It allocates tax credits valued at over 8 billion annually to qualified projects through state and local agencies. To be qualified for the LIHTC program, a project must have at least 20% of the tenants earning less than 50% of the area median gross income (AMGI), or at least 40% of the tenants earning less than 60% of the AMGI. To incentivize more investment in low income areas, HUD designates certain tracts as QCTs each year, and LIHTC projects located in QCTs can receive up to 30% larger tax credits. In addition to LIHTC, QCTs are also used in other place-based programs that facilitate local development, most notably programs run through the US Small Business Association.[2]

To be eligible for QCT status, a census tract must either have at least 50% of households with incomes below 60% of the AMGI or a poverty rate of 25% or more. HUD also imposes a rule that no more than 20% of a Core Based Statistical Area (CBSA) population can reside in QCTs. In CBSAs where the total population of eligible tracts exceeds the 20% limit, HUD ranks all eligible tracts from the most to least economically disadvantaged (based on the ratio of 60% AMGI to tract median household income and poverty rate). HUD then works down the list to designate QCTs until the 20% population limit is reached. This procedure means that, in some CBSAs with binding population cap, census tracts with income and poverty rates that would qualify them for QCT status are not designated as such. Figure 2.1 plots the distribution of the income and poverty criteria between the QCTs and eligible but non-selected tracts. There is significant overlap in the distribution of relative income ratios and poverty rates between QCTs and non-selected tracts, though QCTs on average have lower median household income and higher poverty rates.

Over 70% of the LIHTC projects are placed in service within 2 years after being allocated tax credits. Therefore, in our analysis, we compare QCTs that were designated in any year between 2016 to 2018, with tracts that were eligible in any year

[2]HUBzone is a program administered by US Small Business Administration (SBA). A business that is located in a Historically Underutilized Business Zone (HUB Zone) and have a certain percentage of employees that live in HUB Zones receive priority for federal contracts. QCTs are automatically HUB zones.

in the same time period, but never selected ("eligible but non-selected").[3] Appendix Table B.1 reports the number of QCTs versus non-selected tracts, and whether the population cap is binding in these 18 cities. Since we exploit cross-city variation, and both crime and policing evolve differently in each city, we demean policing and crime outcomes by city-year. Demeaning allows us to base our identification on each tract's deviation from city-level trends and whether that deviation is associated with QCT status:[4]

$$\tilde{Y}_{i,t} = \beta_0 + \beta_1 \text{QCT}_i \cdot \mathbb{1}(Year = 2019)_t + \delta_i + \gamma_t + \epsilon_{it} \tag{2.1}$$

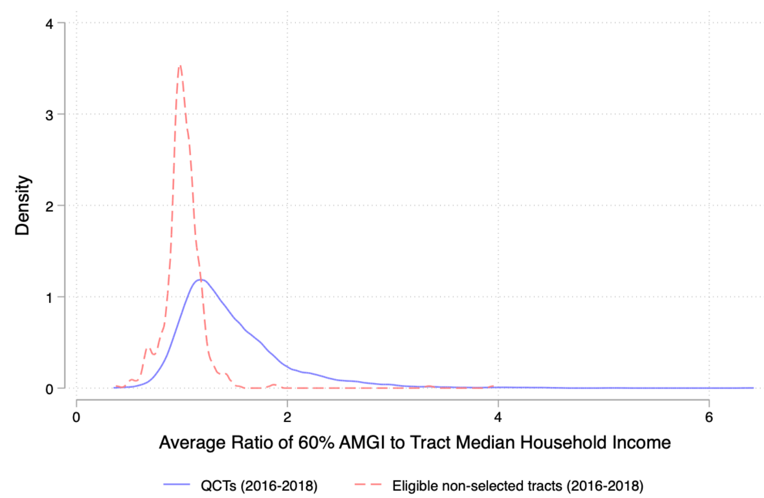
where $\tilde{Y}_{i,t} = Y_{i,t} - \bar{Y}_{c,t}$, and $\bar{Y}_{c,t}$ represents the outcome averaged across all tracts in the city c where tract i is located, δ_i denotes tract fixed effects, and γ_t represents year fixed effects. One potential concern with the above specification is that QCTs and eligible but non-selected tracts could still differ in observable characteristics. Table A.2 demonstrates that, in addition to higher poverty rates and lower median household income, QCTs have a higher concentration of minority residents, a lower share of college-educated residents, and a lower share of occupied housing units.

[3]Despite possible other empirical strategies to studies the impact of LIHTC housing (e.g. Baum-Snow and Marion 2009; Schwartz et al. 2006; Diamond and McQuade 2019), exploiting the population cap of QCT is best suited to evaluate this research question. Exploiting the discontinuity in HUD's QCT designation formula does not leave us enough statistical power given our focus on 18 cities. We discuss results for property-level analysis in Section B.4 in the Appendix. Still, we argue that property-level analysis might not be most appropriate in our setting, as location of new LIHTC housing can be endogenous to existing stock of LIHTC properties.

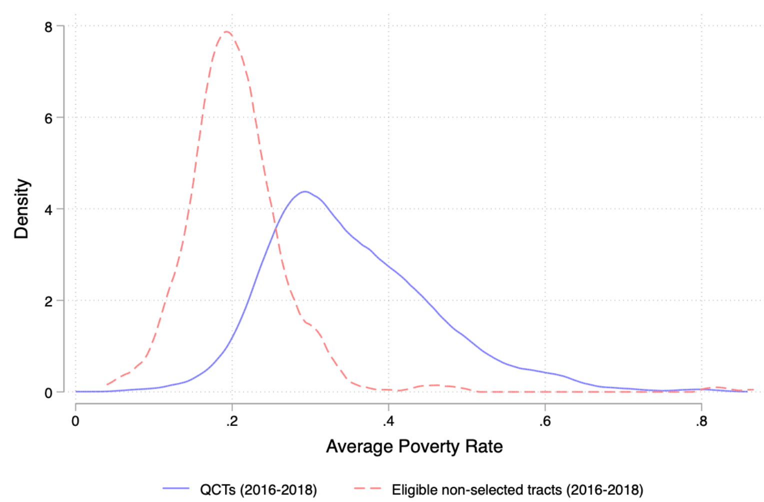
[4]This demeaning also accounts for city differences in the year to year change of smartphone sampling rates.

Figure 2.1: Kernel densities of tract income and poverty criteria for QCTs and eligible but non-selected tracts

(a) Income Criterion



(b) Poverty Criterion



Notes: This figure displays kernel densities of the tract's average ratio of 60% AMGI (Area Median Gross Income) to tract median household income (panel a) and tract poverty rates (panel b) for QCTs and for eligible but non-selected tracts. Both measures are averaged across 2016 to 2018.

We improve upon this first difference approach by using the doubly robust estimation proposed by Sant'Anna and Zhao (2020), which conditions tracts on baseline neighborhood features so that eligible but non-selected tracts better resemble QCTs in different cities. This framework combines an inverse probability weighting (IPW) approach that estimates the probability of receiving QCT status (i.e. propensity score) to reweight eligible non-selected tracts based on a set of covariates, and a outcome regression approach that models outcome change as a function of the same set of covariates among non-selected tracts. Sant'Anna and Zhao (2020) shows that this estimator is consistent if either the propensity score model or the outcome regression model is correctly specified (i.e. doubly robust). We match, and regression-adjust, tracts based on all demographic and housing variables listed in panel A of Table A.2 as well as the number of LIHTC units placed in service between 2015 to 2017, assuming that the best counterfactual for a given QCT is one that has the same absolute levels of population, income and poverty rates, college attendance, racial and age composition, and housing characteristics.[5] While conventionally referred as a "doubly robust difference-in-differences" estimator, the relative stability of QCT status over time means that our estimator is more accurately described as a "doubly robust difference-in-changes" estimator; for all but a small handful of tracts, there is no "pre" period during which the tract is not a QCT.[6]

Our identifying assumption is that, conditional on a tract's demographic and

[5]In section B.5 in the Appendix, we present results on alternative matching variables, e.g. on income and poverty rates only, or exclude past LIHTC units, none of which leads to substantive change in the estimates.

[6]Importantly, this feature makes matching on pre-trends in crime, an intuitively appealing strategy, problematic; if the QCT status has a causal impact on crime, than a QCT and non-QCT with identical pre-2017 trends in crime should be less, rather than more, similar on unobservables.

housing characteristics, QCT status is exogenous to a tract's differential change in police presence and crime relative to the city-level trend over time. In other words, any differential pre-trends in policing or crime in tracts that are affected, or not affected, by the population caps are shared by all tracts in the same city. Under this empirical framework, in section 2.4, we examine the relationship between QCT-spurred investments and differential changes in neighborhood outcomes, specifically policing and crime relative to broader city trend.

Table 2.1: Summary Statistics

	(1)		(2)		(3)	
	QCTs		Eligible, non-selected tracts		Difference	
	mean	sd	mean	sd	b	t
Panel A: Demographic and Housing Characteristics						
Total Population	4138.678	1901.276	4075.792	2199.170	-62.885	(-0.876)
Total Housing Units	1592.031	710.138	1558.083	949.001	-33.948	(-1.112)
% Owner Occupied HU	0.310	0.201	0.356	0.192	0.046	(7.128)
% Total Occupied HU	0.875	0.098	0.914	0.058	0.040	(18.057)
Median Household Income (1K)	34.811	12.257	49.117	11.918	14.306	(35.900)
Poverty Rate	0.325	0.116	0.209	0.077	-0.117	(-41.763)
% College	0.182	0.148	0.270	0.140	0.088	(18.655)
% Black	0.354	0.340	0.219	0.285	-0.135	(-13.820)
% White	0.147	0.183	0.241	0.224	0.094	(12.905)
% Hispanic	0.413	0.318	0.362	0.266	-0.052	(-5.656)
% Population Under 18	0.253	0.083	0.220	0.070	-0.032	(-13.559)
% Population Above 65	0.106	0.056	0.124	0.051	0.018	(10.477)
Panel B: Physical and Social Environment						
LIHTC Projects 2018-2019	0.053	0.273	0.028	0.206	-0.025	(-3.473)
LIHTC Units 2018-2019	6.836	48.413	2.743	22.341	-4.092	(-4.419)
Street Light Repair Request	24.579	30.417	16.361	26.672	-8.219	(-8.397)
Jobs (E > 3333)	576.039	362.491	748.332	473.159	172.293	(11.290)
Visits by Non-patrol Phones	762845.578	629054.281	740753.108	636108.696	-22092.470	(-1.045)
Panel C: Crime per 1000 Jobs						
Burglaries	17.630	17.612	9.002	13.187	-8.628	(-18.598)
Thefts	63.582	78.779	36.560	36.464	-27.021	(-17.901)
Motor Vehicle Thefts	15.359	16.292	8.008	11.648	-7.351	(-17.735)
Aggravated Assaults	17.797	17.687	9.782	13.021	-8.015	(-17.425)
Homicides	0.636	1.219	0.190	0.632	-0.446	(-17.761)
Robberies	11.891	11.937	6.137	7.812	-5.754	(-20.208)
Violent Crimes	31.882	28.133	16.833	21.112	-15.048	(-20.271)
Property Crimes	96.571	93.778	53.570	51.535	-43.001	(-21.618)
Panel D: Policing						
Police Hour	72.620	257.482	91.634	513.338	19.014	(1.180)
Police Officers	31.867	26.849	30.871	32.507	-0.997	(-0.943)
Police Shifts	141.527	168.167	142.448	275.468	0.921	(0.105)
Observations	6060		1060		7120	

2.3 Data and Measurement

Our sample includes 18 of the largest U.S. cities in 2017 (from February to November) and 2019. We use QCT designation data to determine a tract's QCT status between 2016 and 2018, and combine them with police patrol measures using smartphone location data. The smartphone data come from Veraset, a company that aggregates anonymized location data from a suite of smartphone applications. It consists of "pings" that indicate the location of a smartphone at a particular timestamp. Pings are logged whenever a participating smartphone application requests location information and thus are recorded at irregular time intervals, with an modal interval of about 10 minutes between two consecutive pings. It covers more than 50 million smartphones spanning the continental US annually. While not capturing the universe of smartphones, studies using similar smartphone location data find that the smartphone data is highly representative of the United States on numerous demographic dimensions (e.g. Chen et al. 2019; Athey et al. 2021).

We use methodologies developed in Chen et al. (Forthcoming) to identify likely police officers, and map their daily on-shift movement patterns using smartphone pings. For each month, we define a device as a likely police employee if it pings within a police station geofence at least five days in that month. To identify patrol officers among all police personnel, we look for a device's movement pattern: leaving home (defined as the most visited block other than police stations), traveling to a police station, moving around the city (without returning home), returning to the police station, and then going home. The movements of that smartphone between the first and the last station visits are assumed to be the actual locations of a patrol officer while working a "shift." We require that "shifts" are bracketed by home visits that are no more than 24 hours apart, and are no shorter than four hours.[7] We then look at officers' smartphone pings outside of police stations when officers are "on shift" in the month when the device has at least 5-day presence, and are moving 50 mph or less. We identify 8,136 and 6,577 patrol officers that have at least one "shift" in the 18 cities in 2017 and 2019, respectively. We match likely officers' pings during patrol to census tracts, and calculate ping duration as half the time between its previous and next ping, and measure police presence in a tract as the total officers-hours present in each year. Chen et al. (Forthcoming) shows that these measures satisfy many tests of face and construct validity. Panel D of Table A.2 provides summary statistics on police hours.

We supplement the analysis with additional data on LIHTC property, geocoded crime incident data, LEHD Origin-Destination Employment Statistics (LODES) - Residence Area Characteristic (RAC) data, and 311 calls data. These data allow us to quantify LIHTC units, crime, socioeconomic profiles of employed residents, and disorder-related requests. Appendix B.1 provides detailed explanation of these data sources.

2.4 Results

2.4.1 QCT Status and Change in Neighborhood Environment

We start our analysis in Table 2.2 by demonstrating that our approach can replicate existing findings on the positive impact of QCT-spurred investment on both the

[7] In Appendix B.5, we demonstrate that the results are robust to alternative definitions of police measures, including using shifts that are 8 to 12 hours long or shifts bracketed by home visits that are no more than 18 hours apart.

physical and social environment of neighborhoods (Baum-Snow and Marion, 2009; Freedman and McGavock, 2015; Ellen et al., 2016).

Column 1 and 2 demonstrate that QCT-spurred investment leads to improvements in infrastructure investment and environment. Relative to eligible and non-selected tracts, QCTs have 3.3-4.1 more LIHTC units placed in service in 2018-2019. In 11 cities, we are able to collect 311 call data on street light repairs as a proxy for static physical disorder (Wheeler, 2018).[8] Both baseline and doubly robust estimates suggest QCTs experience a significant reduction in street light repair requests, on the order of 10.5%-17%, when compared to similar non-QCT tracts.

These changes in the physical environment are accompanied by shifts in neighborhood's socioeconomic environment, particularly in the residential composition and foot traffic. In column 3, using the LODES-RAC data, we observe a significant 3.5%-5.9% increase in the number of residents with relatively high-paying jobs (i.e. jobs with monthly earnings greater than $3,333) in QCTs relative to eligible non-selected tracts. Appendix Table B.4 further indicates that socioeconomic profiles of QCT residents change in a pattern that reflects gentrification, including an increase in the number of employed residents identifying as White and holding college degrees, alongside a slight decrease in residents identifying as Black or Hispanic, and with high school diplomas.

In column 4, we utilize smartphone data to estimate changes in the ambient population in QCTs, specifically visits by non-patrol officer phones (i.e. foot traffic).[9]

[8] The cities are Austin, Charlotte, Chicago, Dallas, Denver, Detroit, Los Angeles, New York City, Philadelphia, San Francisco, and Washington.

[9] A phone is considered to "visit" a geohash-7 (roughly a street block) if it spends at least 10 minutes in that geohash-7 within a half-hour window.

While the baseline estimate suggests a substantial 17% increase in foot traffic in QCTs, the doubly robust estimate reports a less precisely estimated 3% increase in foot traffic in QCTs compared to similar eligible tracts. The differences between estimation strategies suggest that eligible non-QCTs that are most similar to QCTs experience similar increases in foot traffic over the sample period, but the "most advantaged" eligible tracts do not. In contrast, neither the marginal or "most advantaged" tracts experience differential change in other neighborhood improvement measures.

Table 2.2: Effect of QCT status on neighborhood physical and socioeconomic environment

	Physical Disorder		Socioeconomic Environment	
	(1)	(2)	(3)	(4)
	Stock of LIHTC units	Street Light Repair Request	Jobs (E>3333)	Visits by Non-patrol Phones
Panel A: DID estimator				
2019 X QCT	4.092	-0.105	0.035	0.167
	[1.524,6.660]	[-0.192,-0.018]	[0.027,0.043]	[0.136,0.198]
	(1.310)	(0.045)	(0.004)	(0.016)
Observations	7120	5682	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics				
2019 X QCT	3.251	-0.169	0.059	0.031
	[0.325,6.177]	[-0.312,-0.025]	[0.034,0.084]	[-0.019,0.081]
	(1.493)	(0.073)	(0.013)	(0.025)
Observations	7120	5682	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. Column 2 is estimated using a subsample of 11 cities that geocoded 311 data. The dependent variables in column 2-4 are demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age <18, % age >65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

2.4.2 Do police respond to QCT status?

2.4.2.1 Policing Time

Table 2.3 shows the estimated impact of QCT status on police presence, measured by the total officer-hours observed in a tract. We transform police hours using a inverse hyperbolic sine function.[10] Like foot traffic, introducing weights affects our estimates, though now we observe that, QCTs experience increased police presence relative to marginal non-selected tracts.

Specifically, when compared with other eligible tracts matched on demographics and housing characteristics, police increase their hours spent in QCTs by 13.5% from 2017 to 2019. To put this estimate in perspective, Weisburd et al. (2015) reports an average of 1100 officer-hours per week in a Dallas police beat, which is similar in size to a census tract in Dallas. Extrapolating this with our doubly robust estimate implies that QCTs receiving investments experience an average weekly increase of 149 officer-hours. In Appendix B.2, we show that increased police patrol in QCTs primarily occurs during the evenings. In Appendix B.3, we demonstrate that increased police time is driven by increased patrol frequencies rather than changes in officer size or racial composition. Appendix B.5 shows that our findings remain robust to excluding cities without binding population caps, allowing for differential time trends in high and low poverty tracts within a city, or excluding the most weighted tracts; the latter is particularly important as weighting is central to our identification. We also present results on alternative definitions of police presence, matching schemes and estimators in Appendix B.5, and find overall consistent patterns across most

[10] arsinh $y = ln(y+\sqrt{y^2+1}) \approx ln(2y) = ln(2)+ln(y)$. The interpretation of coefficient estimates is thus similar to a log-transformation.

specifications.[11]

2.4.2.2 Role of Police in the Investment-Crime Relationship

Columns 2 and 3 of Table 2.3 report the reduced-form effect of QCT status on the number of violent and property crimes per 1000 jobs, an outcome measure which reflects both crime and population size changes. [12] In line with Diamond and McQuade (2019) and Freedman and Owens (2011), the doubly robust estimates suggest that being awarded the QCT status reduces the number of violent crimes by 3.7 per thousand jobs at a 10% significance level. Property crime rates do not change significantly in QCTs relative to the non-selected tracts, which is more consistent with Freedman and Owens (2011). Notably, our estimates are in line with Diamond and McQuade (2019) at similar levels of geography, suggesting that any negative bias associated with miss-specification of the doubly robust estimator is likely to be minimal.

[11]It is worth noting that we do not find evidence that police appear to target new residents and concentrate their time spent in certain street blocks in QCTs, and this is in contrast to the finding that the distribution of foot traffic across street blocks are more concentrated in QCTs.

[12]The denominator of this measure, number of jobs, comes from the LODES data that is available annually, in contrast to ACS 5-year estimates. Appendix Table B.16 shows similar results using population from ACS 5-year estimates as denominator.

Table 2.3: Effect of QCT status on police hour and crime

	Police	Crime Per 1,000 Jobs		Police Residualized: $\Delta Crime - \Delta \hat{Crime}$	
	(1)	(2)	(3)	(4)	(5)
	Hour	Violent Crimes	Property Crimes		
Panel A: DID estimator					
2019 X QCT	0.002	0.092	0.653		
	[-0.084,0.087]	[-0.700,0.885]	[-1.526,2.832]		
	(0.044)	(0.404)	(1.112)		
Observations	7120	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics					
2019 X QCT	0.135	-3.715	-2.036		
	[0.007,0.263]	[-7.822,0.393]	[-6.746,2.675]		
	(0.065)	(2.096)	(2.403)		
QCT status				0.298	4.893
				[-9.575,10.170]	[-8.395,18.180]
				(5.037)	(6.780)
Observations	7120	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are first transformed into inverse hyperbolic sine (arsinh) values (arsinh$y = ln(y + \sqrt{y^2+1})$), and then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

This crime reduction process in QCTs could be due to both the direct impact of the built environment on individual criminal propensity, and the induced change in policing. To quantify the behavioral effect of increased police presence in QCTs on crime, we conduct a simple back-of-the-envelope calculation using crime-police elasticity estimates from Weisburd (2021)—an elasticity of -0.9 for violent crime and -0.6 for property crime, both with respect to neighborhood police presence.[13]

[13]Estimates from Weisburd (2021) are best suited to our setting as they focus on the elasticity of crime with respect to routine, neighborhood police presence in Dallas, compared to studies that estimate crime elasticity with respect to city-level police force size (e.g. Evans and Owens 2007;

Specifically, we compute the predicted change in crime in each tract that could be explained by police response to QCT status—the product of the estimated percentage change in police hours (0.135), police-crime elasticity (-0.9 or -0.6), and the tract's crime rate in 2017—and subtract this from the actual tract level crime change. We then regress this residual changes in crime rates on QCT status, using the weights generated by the doubly robust strategy.

Columns 4 and 5 report the estimated relationship between QCT status and the residual changes in violent and property crime, along with bootstrapped confidence intervals. The correlation between the residual of violent crime changes and the QCT status is not statistically distinguishable from zero. The residual change in property crime rates and QCT status shows a positive correlation, indicating that increased police presence predicts greater property crime reduction than observed, though this confidence interval is wide and includes zero. Overall, we cannot reject the hypothesis that the police response can account for all violent or property crime reduction observed in QCTs.

A natural next question is how this additional police time in QCTs is provided; unlike crime, within a city there is a fixed number of police-hours that can be allocated across space and time. Our data suggest that, on average, police presence declines in eligible but non-selected QCTs that are most similar to actual QCTs, and also experience larger increases in crime. Appendix Table B.3 illustrates this finding, and further emphasizes the role that our doubly robust weighting strategy plays in our identification. On average, eligible but non-QCT tracts do not experi-

Levitt 2002; Mello 2019), police enforcement actions (e.g. Cho et al. 2021), or increased police deployment in response to terror attacks in other countries (e.g. Di Tella and Schargrodsky 2004; Draca et al. 2011).

ence a differential change in police patrol relative to the city. However, there is an 12.3% reduction in police presence over time when we weight non-selected tracts to better match QCTs, along with a 2% increase in time in QCTs. This pattern, where the most socioeconomically disadvantaged non-QCTs appear to be most affected by the lack of QCT status, holds true for violent crime. Additionally, we examine the geographic distribution of marginal and average non-selected tracts; on average, 35% of the tracts that neighboring eligible non-selected tracts are actually QCTs, but once weighted, 52% of the adjacent tracts of eligible non-selected tracts receive QCT-based investment. Put differently, the counterfactual places in our sample are disadvantaged tracts that are physically close to QCTs, and our doubly robust probability weighting method heavily weights the most disadvantaged tracts that are even closer to other QCTs. The increased patrol time in QCTs that comes at the expense of non-QCTs could be due to officers shifting their patrol by one or two blocks, and not necessarily moving into a different "beat."

2.4.2.3 Effect Heterogeneity

By neighborhood racial composition: To explore the implications of investments in QCTs on racial disparities in policing, we examine how police response vary depending on the share of Black population in a QCT relative to its city.

In Table 2.4, we separately estimate the effects for QCTs with the share of Black residents in the top and bottom tertile within their cities, using the same doubly robust specification. To ensure an adequate sample for matching, we include all eligible but non-selected tracts in the donor pool. We find that increased police time is mostly concentrated in QCTs with larger Black populations: police spend 33%

more time in QCTs with Black share in the top tertile within their cities, compared to an 18% increase in QCTs in the bottom tertile. Notably, this suggests that in some contexts place-based investment could actually increase, rather than decrease, racially disparate criminal justice contact. Of course, the observed patterns in our data are also consistent with police being more responsive to calls for service, or other requests for police action from residents in areas with larger Black populations.

Table 2.4: Effect Heterogeneity of QCT status

	% Black		% Rental HU		% Recently Built HU		% Single HU	
	(1) Top tertile	(2) Bottom tertile	(3) Top tertile	(4) Bottom tertile	(5) Top tertile	(6) Bottom tertile	(7) Top tertile	(8) Bottom tertile
Panel A: DID estimator								
2019 X QCT	0.043	-0.056	-0.021	0.118	0.003	0.008	0.064	-0.046
	[-0.050,0.136]	[-0.159,0.046]	[-0.112,0.070]	[0.004,0.233]	[-0.103,0.109]	[-0.079,0.096]	[-0.042,0.170]	[-0.139,0.047]
	(0.047)	(0.052)	(0.046)	(0.058)	(0.054)	(0.045)	(0.054)	(0.048)
Observations	3986	2314	4444	1710	2170	5812	2238	3728
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics								
2019 X QCT	0.327	0.182	0.180	0.242	0.108	0.154	0.154	0.103
	[0.199,0.456]	[0.000,0.363]	[-0.027,0.386]	[0.003,0.481]	[-0.037,0.253]	[0.018,0.291]	[-0.033,0.340]	[-0.097,0.303]
	(0.066)	(0.093)	(0.105)	(0.122)	(0.074)	(0.070)	(0.095)	(0.102)
Observations	3986	2314	4444	1710	2170	5812	2238	3728

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are first transformed into inverse hyperbolic sine values (arsinh$y = ln(y + \sqrt{y^2 + 1})$), and then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

By neighborhood housing stock characteristics: New LIHTC housing can change the physical space of a neighborhood, and this change may be more noticeable in neighborhoods with less existing rental housing, more single family units, and older housing stocks. Furthermore, to the extent that police reporting is a public good, we might expect larger demand for police presence in QCTs that receive more LIHTC investments, as management of LIHTC properties may internalize more of the external benefits of police monitoring relative to smaller landlords (Schwartz et al. 2006). We examine the extent to which the effects of QCT status differs by the housing characteristics from the 2013-2017 American Community Survey using the same sub-sampling strategy.

Results in Table 2.4 are generally in line with the idea that QCT status has stronger impact on police presence in neighborhoods where large LIHTC developments bring more noticeable changes to the built environment. We observe a slightly larger effect in QCTs with the lowest share of rental housing units in the city (24%) compared to those in the top tertile (18%). QCTs with more recently built housing experience a significant 15% increase in total officer time, whereas QCTs in the bottom tertile show a less precise 11% increase. Finally, although the effect of QCT status on police is imprecisely estimated in both the subsample of tracts with high and low shares of single-family housing units, we detect a larger positive point estimate in QCTs with more single-family homes.

By city: The relationship between policing and the socioeconomic characteristics of residents varies across cities (Chen et al. Forthcoming), and thus it is reasonable to think that police might respond differently to changes in environment in different cities. To explore this, Appendix Figure B.1 plots the estimated effect when we iteratively exclude one city at a time from our sample. The point estimates are quantitatively similar when observations from cities other than Detroit, Los Angeles, and New York City are excluded, but are statistically indistinguishable from zero when tracts from one of these cities are excluded. This implies that police responses to local conditions in these cities are particularly important for the estimate of average police responses. Notably, excluding observations from New York City and Los Angeles reduces statistical power given they are the top two cities contributing to the largest number of tracts in our sample. On the other hand, Detroit, with its notably higher poverty rate, may elicit a stronger police response if investments in QCTs there have disproportionately large impacts on local physical environment. The relative importance of these cities for our average estimates highlights the potentially limited external validity of single-city evaluations of social programs.

2.4.3 How does QCT status change policing and crime?

In this section, we explore how QCT-spurred investments change local neighborhood and affect local policing and crime rates. We use Google Street View (GSV) images to observe how physical environment in QCTs change over time, as Google updates the street view images of the same location at regular intervals. Moreover, Google Street View images avoid capturing images of people, thus helping isolate the environmental influences on certain behaviors by focusing on static environmental elements rather than dynamic human activities.

We compile a panel of street view images from Google Street View static API, collecting street view panorama of census blocks in 18 largest US cities from two periods, before and after when 2018 LIHTC housing is placed in service: pre-period (2014-2017) and post-period (2019-2022). For each street view panorama, we download two images with two headings that depict the horizontal view of the housing. We assign each census block the nearest street view panorama to the block's centroid, excluding panorama that 1) are not within the corresponding census block and also more than 50 meters away from the block's centroid, 2) failed to download from the Street View API. This results in a dataset of 622,353 unique images across 18 cities.

We predict outcomes for street view images by training neural network models, specifically the Residual Network architecture (ResNet). ResNet is a canonical computer vision model for image recognition tasks (He et al., 2016). We fine-tuned a ResNet-50 (or ResNet-18) model that is pretrained on the Places365 dataset for our image regression task to predict three sets of key outcomes: 1) urban perception scores, 2) police hours within certain radii of the location, 3) crime indices that estimate the total costs of crime within the same radii.[14]

The training dataset for urban perception scores comes from the publicly available Place Pulse 2.0 dataset that includes 110,988 street view images from 56 cities globally, a popular dataset that is used to study how individuals perceive urban appearance (Dubey et al., 2016). This data contains over 1 million pairwise comparisons from over 80,000 online participants, rated along six dimensions: safe, lively, boring, wealthy, depressing and beautiful. Specifically, participants would be shown two random street view images and answer questions including "which places looks safer (or livelier, more boring, wealthier, more depressing, more beautiful)?" These pairwise comparison are then converted into scores using the Microsoft TrueSkill ranking algorithm. We focus exclusively on 27,784 images in the 13 US cities during

[14]The Places365 dataset includes over 1.8 million images covering 365 scene categories around the world, and is a commonly used dataset used for training deep learning models in scene recognition tasks. Fine-tuning a pre-trained Resnet-Places365 model allows us to transfer existing knowledge in similar domains to our model to improve model performance.

our training, as our prediction task is specifically tailored to images in US cities. We implement data augmentation strategies to increase the diversity of the training data, including cropping each image at the four corners and the center, and applying horizontal flips, producing 10 variations per image. This approach has effectively improved the model performance, achieving a 0.53 correlation coefficient between the predicted and actual scores on "safe." Model performance on other dimensions is slightly lower: 0.5 for beautiful, 0.47 for lively, 0.44 for wealthy, 0.36 for depressing and 0.30 for boring. As a result, we only focus on predicted scores for safety, beauty, liveliness, and wealth in later analysis given the more reliable predictions on these dimensions.

To predict crime and policing outcomes, we construct calculate the police hours and crime indices within the radii of 100m, 200m, and 300m from all 622,353 downloaded street views. These data were split into 60% for training, 20% for validation, and 20% for testing. Police presence are calculated as total hours for all likely phones present within each panorama radius for each year, using an arsinh transformation to account for the skewness of this measure.

For crime outcomes, we first calculate the number of different violent and property crimes within the radii of 100m, 200m, and 300m from the panorama location. Then, we compute a crime index to reflect the estimated cost of crime, by weighting each major property and violent crime categories with the relative costs to burglary as the label during our training and prediction.[15] We train separate models for each city using that city's image to account for unique local patterns and characteristics.

[15] $\text{Crime Index}_r = \left(\frac{67277}{13096}\right) \cdot \text{Robbery Count} + \left(\frac{87238}{13096}\right) \cdot \text{Aggravated Assault Count}_r + \text{Burglary Count}_r + \left(\frac{2139}{13096}\right) \cdot \text{Theft Count}_r + \left(\frac{9079}{13096}\right) \cdot \text{Motor Vehicle Theft Count}_r$, where $r = 100\text{m}, 200\text{m}, 300\text{m}$. Cost of crime estimates come from `https://www.rand.org/well-being/justice-policy/centers/quality-policing/cost-of-crime.html`

Appendix Table B.18 and B.19 show the correlation coefficients between predicted and actual labels in the testing data. The performance of model improves when we predict police hours and crime indices within larger radii. The model performance for testing datasets varies by city, for example, with police hours ranging from 0.394 (San Francisco) to 0.686 (Austin) within a 300 m radius, and crime indices ranging from 0.354 (Austin) to 0.844 (Seattle) within the same radius. Overall, the pooled correlation coefficient across cities achieves 0.69 for police hours and 0.76 for crime indices within the 300 m radius, indicating a strong model fit for the fine-tuned ResNet models. Moreover, Appendix Table B.20 and B.21 indicate that the street view predictions of crime or police hours alone explains significant larger variation in actual crime or police hours than demographic variables alone.

After the training and prediction phase, we estimate the impact of QCT status on these predicted outcomes using the same doubly robust specification at the image level, comparing blocks located in Qualified Census Tracts versus blocks located in eligible but non-selected tracts. We first look at the impact of QCT status on predicted urban perception scores to explore whether QCT status significantly changes local physical environment. Table 2.5 suggests predicted scores on perceived safety, beauty and wealthiness increase in QCTs relative to similar non-selected tracts. This aligns with earlier results on reduction in 311 call requests in QCTs, and suggests that QCTs indeed experience improvement in local physical environment.

Table 2.5: Effect of QCT status on predicted urban perception

	Predicted Urban Perception Scores			
	(1) Safe	(2) Beautiful	(3) Lively	(4) Wealthy
Panel A: DID estimator				
2019 X QCT	-0.015	-0.019	-0.031	-0.028
	[-0.081,0.051]	[-0.091,0.054]	[-0.079,0.018]	[-0.087,0.030]
	(0.034)	(0.037)	(0.025)	(0.030)
Observations	293534	293534	293534	293534
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics				
2019 X QCT	0.113	0.129	0.056	0.089
	[0.029,0.197]	[0.020,0.239]	[-0.016,0.129]	[0.009,0.168]
	(0.043)	(0.056)	(0.037)	(0.041)
Observations	297291	297291	297291	297291

Notes: The unit of observation is a tract-year. Each tract has one observation in 2013-2017 (pre-period) and in 2019-2022 (post-period), respectively. The dependent variables are demeaned by city-year. The covariates in panel B include tract median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017, and the block level population from 2010 census. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Next, we compare changes in observed and predicted police hours and crime indices between QCTs and eligible non-selected tracts, to explore whether changes in policing and crime come from direct changes in the physical environment, or from responses driven by changes within the local population. Table 2.6 presents the image-level results for police hours. Columns 1-3 suggest an increase in actual police hour nearby for blocks in QCTs, relative to blocks located in non-QCTs, with the 300m estimate (a 12% increase in police hours) aligning closely with the tract-level estimates on police hours. In contrast, the estimates for predicted police hours, shown in columns 4-6, though positive, are considerably smaller. This suggest that direct police response to environmental changes were minimal. Instead, the observed increase in police presence in QCTs is more likely driven by changes in civilian activities, such as increased 911 calls. Another plausible cause could be changing directives from chiefs' that are correlated with the place-based investment (e.g. policing that is supportive of a mayoral revitalization initiative).

Table 2.6: Effect of QCT status on predicted police hours

	Demeaned arsinh(Hour) (Actual)			Demeaned Predicted arsinh(Hour)		
	(1) 100 m	(2) 200 m	(3) 300 m	(4) 100 m	(5) 200 m	(6) 300 m
Panel A: DID estimator						
2019 X QCT	0.009	0.032	0.061	-0.003	-0.001	-0.002
	[-0.039,0.056]	[-0.041,0.105]	[-0.025,0.148]	[-0.018,0.012]	[-0.054,0.051]	[-0.073,0.069]
	(0.024)	(0.037)	(0.044)	(0.008)	(0.027)	(0.036)
Observations	292586	292586	292586	292526	292526	292526
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics						
2019 X QCT	0.018	0.077	0.120	0.005	0.028	0.025
	[-0.040,0.076]	[-0.028,0.181]	[-0.010,0.250]	[-0.002,0.013]	[-0.001,0.057]	[-0.014,0.064]
	(0.030)	(0.053)	(0.066)	(0.004)	(0.015)	(0.020)
Observations	297325	297325	297325	297291	297291	297291

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are first transformed into inverse hyperbolic sine values ($\text{arsinh}y = ln(y + \sqrt{y^2+1})$), and then demeaned by city-year. The covariates in panel B include tract median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017, and the block level population from 2010 census. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Similarly, Table 2.7 reveals a consistent pattern for crime indices. The doubly robust estimates on actual crime indices, though not precisely estimated, suggest a negative effect of QCT status on cost-adjusted crime in blocks located in QCTs. In columns 4-6, we do not observe significant changes predicted crime indices for blocks located in QCTs relative to non-selected tracts. Thus, while investments in QCTs have a significant impact on local physical environment, the observed changes in crime rates primarily come from shifts in local population dynamics and changes in the law enforcement practices rather than the direct responses to environments per se.

Table 2.7: Effect of QCT status on predicted crime indices

	Demeaned Crime Index Per 1,000 Jobs (Actual)			Demeaned Predicted Crime Index Per 1,000 Jobs		
	(1) 100 m	(2) 200 m	(3) 300 m	(4) 100 m	(5) 200 m	(6) 300 m
Panel A: DID estimator						
2019 X QCT	43.361 [-18.537,105.259] (31.570)	-9.287 [-164.777,146.202] (79.305)	-135.900 [-442.674,170.874] (156.466)	85.425 [42.877,127.974] (21.701)	269.866 [84.890,454.843] (94.346)	561.799 [163.557,960.041] (203.119)
Observations	196246	196246	196246	201000	201000	201000
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics						
2019 X QCT	-72.361 [-277.407,132.684] (104.617)	-78.718 [-411.742,254.307] (169.914)	-460.550 [-1065.397,144.296] (308.601)	-51.363 [-191.601,88.875] (71.552)	9.397 [-253.298,272.092] (134.031)	-140.726 [-702.226,420.775] (286.485)
Observations	201838	201838	201838	206644	206644	206644

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. The covariates in panel B include tract median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017, and the block level population from 2010 census. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Overall, we provide suggestive evidence regarding the mechanism through which policing and crime responses are influenced by QCT-spurred investments, by applying deep learning models to Google Street View images. While these models effectively detect notable changes in perceptions of the physical environment within QCTs, they do not predict significant changes in crime and policing patterns that could be directly attributed to these environmental changes. Importantly, our analysis underscores that using Google Street View images could enable us to have a more nuanced understanding on how place-based policies could affect the physical space and dynamics of neighborhoods.

2.5 Conclusion

Existing research linking place-based investments to crime has generally attributed crime reduction to changes in civilian behavior. However, our understanding of the general equilibrium effect of local development may be incomplete without considering how police respond to the same changes. This paper studies police response to local investments in low-income neighborhoods designated as Qualified Census Tracts (QCTs). We find that, compared to eligible but non-selected tracts with similar observable characteristics, police increases patrol in QCTs that receive more investment among the 18 largest US cities from 2017 to 2019. Importantly, this increase in police presence is large enough to explain the entirety of the observed reduction in violent crime in QCTs. These results suggest that investments in place-based policies may change, but not necessarily reduce, the extent to which residents are policed, and may not necessarily lead to reduction in police expenditure.

While improving the physical environment is important in its own right to improve the lived experience of residents, the assumed reduction in violence without involving criminalization or increased policing may not be warranted (Branas et al. 2020). Rather, our results are consistent with the idea that crime reduction in places that receive new investment may be the result of complementary positive changes in environment and policing. In that sense, the general equilibrium impacts of investing in neighborhoods may lead to smaller reductions in criminal justice contact than implied by previous partial-equilibrium studies that assume constant policing. Our findings also echo recent recommendations by the Council on Criminal Justice (CCJ) on violence reduction, which highlight the importance of a holistic response to crime problems.[16]

This paper is one of the few multi-city studies of neighborhood policing. Existing studies of policing typically focus on city-level or single-city neighborhood-level outcomes (e.g. Ba et al. 2021; Blattman et al. 2021; Chalfin et al. 2021b; Cho et al. 2021). However, single city studies of policing, investment, and crime will generally vary both in the city level context and in the way in which the key police variables are measured (e.g. Chalfin et al. 2021a; Schwartz et al. 2006). The smartphone location data used in this paper has an advantage of measuring police presence consistently across jurisdictions. More efforts to harmonize criminal justice data across jurisdictions, such as projects by the Criminal Justice Administrative Records System (CJARS), are of great significance for comprehensive policy evaluation.

Several important caveats apply to our study. We are able to measure only the medium-term effect of local development on police presence, and are not able to examine the longer-term effect due to limited availability of smartphone location data. Additionally, we examine one type of place-based intervention. More research is needed to examine whether and how police respond to other types of place-based programs that either directly reduce physical disorder in a neighborhood (e.g. vacant lot cleanup) or that similarly facilitate economic investment (e.g. opportunity zones). Regardless of program type, this paper highlights the need to evaluate the general

[16] CCJ report: `https://counciloncj.org/wp-content/uploads/2022/01/VCWG-Final-Report.pdf`

equilibrium effect to a changing neighborhood environment.

Lastly, our finding that the benefits accruing to neighborhoods receiving investment may come at the cost of disinvestment in similarly disadvantaged places raises additional equity concerns with place-based policies. Although more police presence in low-income areas may signal that the neighborhood is being looked after (Chalfin et al. 2022), we are limited in our ability to measure how police interact with local residents in changing environments. More research is needed to investigate how police response affects the lived experience of neighborhood residents.

CHAPTER 3

Built Environment and Urban Safety: A Machine Learning Approach

3.1 Introduction

Urban safety is a multifaceted issue that significantly influences residents' quality of life. Numerous studies highlight the crucial role of the street environment in shaping safety perceptions and affecting crime rates. Theories in sociology and criminology have underscored the importance of the built environment in urban safety. For example, the broken windows theory suggests that visible signs of disorder increase crime (Wilson and Kelling, 1982). Natural surveillance theory posits that crime can be deterred by designing spaces to increase visibility (Jacobs, 1961a). Crime Prevention Through Environmental Design (CPTED) focuses on strategically designing physical environments to reduce crime. (Newman, 1973; Crowe and Fennelly, 2013)

Despite these theoretical frameworks, few studies have quantitatively explored the relationship between the built environment and both perceived and actual crime, primarily due to challenges in measurement of built environment. Even fewer studies have delved into the mechanisms that examine which specific environmental attributes causally contribute to both urban perceptions of safety and actual crime rates.

Moreover, while some environmental features may effectively reduce crime, they might not have the same impact on reducing fear of crime. Foster et al. (2010) highlights that environmental attributes influencing crime and the fear of crime are distinct concepts. Yet, there is limited understanding of the discrepancies between them and what environmental factors contribute to these differences.

Understanding the differences between fear of crime and actual crime is crucial for several reasons. Public support for place-based crime prevention policies often hinges more on perceived crime rates of local places rather than actual crime statistics. If people perceive their neighborhood as unsafe, they are more likely to demand stringent policing and security measures, which might not align with the reality of the crime data. Conversely, a low perception of crime in a high-crime area might lead to insufficient policy responses. Understanding this discrepancy can inform more nuanced and effective public safety strategies, dispelling misconceptions, and increasing trust in law enforcement agencies. This, in turn, can bolster the effectiveness of policies aimed at maintaining public safety and fostering a sense of security among citizens.

Advancements in big data sources and technology make it increasingly possible to address these questions. In this paper, we apply deep learning models to Google Street View to quantify and examine the relationship between the built environment and urban safety in the 18 largest US cities. Google Street View provides panoramic images that allow for a detailed examination of street-level features, offering a comprehensive and scalable way to capture the physical appearance of urban cities. Specifically, we aim to address two questions: 1) What is the predictive power of street view images, either independently or in conjunction with demographics, on actual crime? 2) How does actual crime differ from perceived safety, and can certain environmental features explain these differences?

By training a Convolutional Neural Network (CNN) model on Google Street View images, we first assess urban perception using Place Pulse 2.0 data, a dataset that provides crowd-sourced ratings on urban environments based on visual appeal and perceived safety. This model is then applied to newly downloaded images to predict urban perceptions across 18 large U.S. cities. Additionally, we create a training dataset for these newly downloaded images to evaluate how well the machine learning model predicts actual crime rates in these areas. We find that CNN-based predictions statistically explains significant variation in actual crime much more than demographic and socioeconomic characteristics.

Additionally, we use generative AI tools to explore the differential impact of various environmental features on both fear of crime and actual crime. Specifically, we generate synthetic images that modify the environment by adding or removing elements such as trash, graffiti, or both. Subsequently, these synthesized images are fed into our trained models to generate predictions of both perceived and actual crime. In this way, we aim to unravel how specific elements of the environment contribute to the disparities between fear of crime and actual crime.

Our preliminary findings suggest a nuanced relationship between environmental attributes like trash and graffiti, crime perceptions and actual crime. For example, while the presence of trash and graffiti is predicted by CNN models to increase actual crimes, graffiti may paradoxically increase the CNN model-predicted perception of safety, possibly due to its association with more liveliness and vibrancy in the local area. This highlights the critical differences between fear of crime and actual crime, suggesting that interventions aimed solely at reducing crime may not necessarily alleviate public fear.

Taken as a whole, this paper provides initial analyses of how urban environmental factors influence both actual crime and the perception of safety. It underscores the promise of leveraging advanced technologies such as deep learning and generative AI to gain insights into the complex dynamics between the built environment and urban safety, facilitating better policy design that addresses the environmental factors contributing to both actual crime and the fear of crime.

3.2 Literature Review

Previous studies have shown that environmental incivilities like litter, graffiti, and vandalism are linked to increased fear of crime. For instance, visual aspects reflecting disorder can significantly elevate fear levels among residents (Nasar and Fisher, 1993; Hanyu, 1997; Painter, 1996; Warr, 1990). These studies often rely on participants' explicit reports, potentially overlooking subtler, less conscious aspects of the environment. To address these limitations, recent advancements in visual assessment methods, including eye-tracking technology, have been employed to provide more detailed insights into the visual aspects contributing to fear of crime (Andrews and Gatersleben, 2010; Toet and van Schaik, 2012; Austin and Sanders, 2007; Herzog and Flynn-Smith, 2001; Herzog and Kutzli, 2002).

The relationship between built environment and actual crime rates has also been a subject of extensive research. Classic experiments (e.g. Zimbardo 1969) and more recent quasi-experimental studies (e.g. Kuo and Sullivan 2001a,b) find that reducing physical disorder lowers individual propensities for aggressive and violent behavior. Chalfin et al. (2021b) demonstrates that improved street lighting reduces crime, while other studies show that reducing vacant lots and abandoned buildings, and investing in low-income housing and security cameras, can also achieve similar reductions in crime (Cui and Walsh, 2015a; Branas et al., 2016; Freedman and Owens, 2011; Diamond and McQuade, 2019; Gómez et al., 2021).

However, few studies have examined the impact of environmental attributes on both fear and crime. One notable exception is Branas et al. (2018), which shows, in a randomized experiment, that cleaning up vacant lots in Philadelphia reduces both violence and fear. South et al. (2023) also find that abandoned housing interventions in Philadelphia led to substantial reductions in nearby weapons violations and gun assaults, although they do not find evidence suggesting these interventions significantly alter perceptions of neighborhood safety and time spent outside. Our research contributes to this growing literature by isolating specific features of urban spaces on both actual and perceived crime using generative AI tools. This approach allows for a more detailed examination of the causal mechanisms driving the crime-perception gap, a critical area of investigation highlighted by (Ajzenman et al., 2022).

We also contribute to a growing literature in urban planning, computer science and economics that combines big data and machine learning tools to measure and analyze the built environment. For example, Naik et al. (2016) use computer vision models to extract visual features of Google Street View and construct an index "Streetscore" to measure urban environment. They find positive relationship between Streetscore, income and population density, and a negative relationship with income inequality. Using the same set of models, Glaeser et al. (2018) shows that Google Street View images have high predictive power on neighborhood income. Deng et al.

(2022) utilizes similar Google Street View images to compute various streetscape indexes, such as the green view index and light view index, and examined their relationship with crime. Naik et al. (2017) quantifies changes in urban environment and find that neighborhoods with more colleges are associated with greater environmental improvement over time. More recent advancement includes employing deep learning models to quantify and predict urban perception (e.g. Dubey et al. 2016; De Nadai et al. 2016).

Overall, these machine learning approaches offer a nuanced understanding of how environmental factors influence crime and public safety perceptions, advancing beyond traditional field surveys and virtual audits of urban imagery (Mooney et al., 2014; Rundle et al., 2011). This research builds on this foundation also by utilizing deep learning models to quantify the built environment to a broader dataset, enhancing the external validity of these findings, and combining that with generative AI to explore the mechanisms from a causal perspective.

3.3 Data and Methods

We train convolutional neural network (CNN) models on Google Street View images, following the procedures in Dubey et al. (2016) and De Nadai et al. (2016). Our model predicts two key outcomes: 1) urban perception scores, and 2) crime indices estimating the total costs of crime within specific radii of a location. This process involves several steps. First, we use the Place Pulse 2.0 dataset, which contains Google Street View images labeled with urban perception scores. For the crime indices, we construct our own training dataset by collecting street view images and labeling each image with crime data. We then train the CNN model to learn these patterns. Once trained, the model can predict outcomes on new street view images.

3.3.1 Data

Google Street View Images. We utilize the extensive visual data from Google Street View (GSV). Google Street View images are panoramic photographs that provide immersive, 360-degree views of streets and locations and are known for its high resolution and broad coverage, making it ideal for large-scale urban environment analysis. These images form the basis of training datasets for our CNN models to predict crime outcomes. Additionally, these models are used to scale predictions of urban perception using downloaded images.

Our data collection process begins with compiling street view images from the 18 largest US cities. We initially sample panoramas by providing latitude and longitude coordinates of census block centers to an unofficial Google API.[1] This API yields essential information such as panorama ID (pano_id), date, heading, and pitch. We retain only one panorama captured between 2019 and 2022, with a preference for those closest to 2019, excluding panoramas outside the corresponding census block or more than 50 meters from the block's centroid.

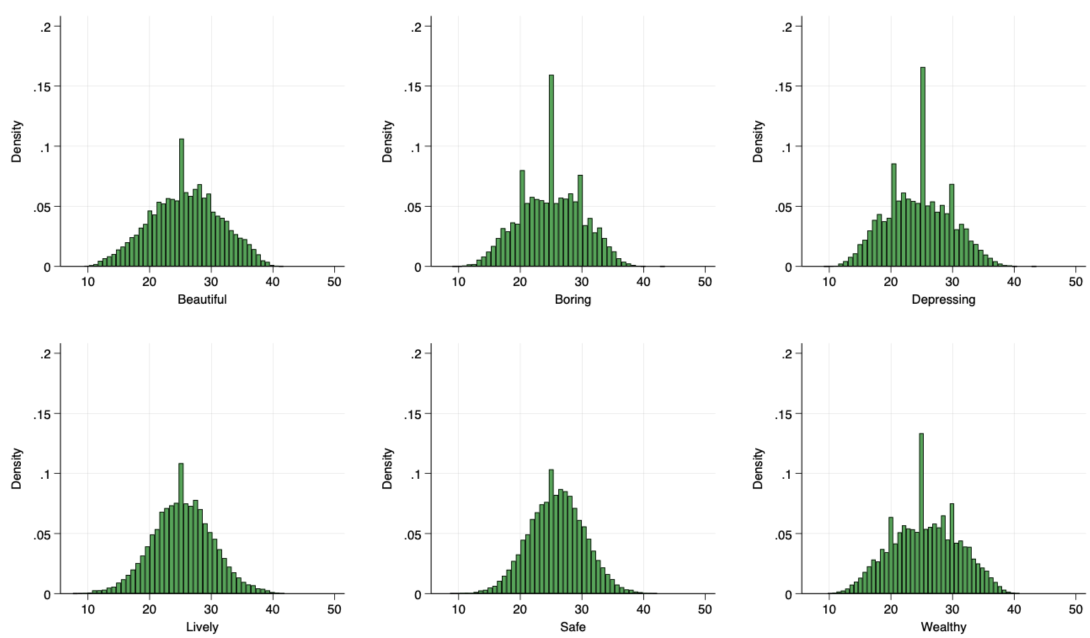
Subsequently, we download street view images using the obtained pano_id through the official Google Static Street View API. We select camera headings depicting the horizontal view of housing, setting the pitch to 0 for all images. Failed downloads are excluded from our dataset, resulting in 622,353 unique images across the 18 cities.

Urban perception data. The training dataset for urban perception scores comes from the publicly available Place Pulse 2.0 dataset, comprising 110,988 street view

[1]See `https://github.com/robolyst/streetview`

images from 56 global cities. This dataset, a popular dataset that is used to study how individuals perceive urban appearance (Dubey et al., 2016), contains over 1 million pairwise comparisons over street view images from over 80,000 online participants, rated along six dimensions: safe, lively, boring, wealthy, depressing and beautiful. Specifically, participants would be shown two random street view images and answer questions including "which place looks safer (or livelier, more boring, wealthier, more depressing, more beautiful)?" These pairwise comparison are then converted into scores using the Microsoft TrueSkill ranking algorithm. We focus exclusively on 27,784 images in the 13 US cities, as our prediction task is specifically tailored to images in US cities. Figure 3.1 illustrates the distribution of six different perceived scores for these images, with each perception score following a normal distribution with a mean centered at 25 and a standard deviation around 5.

Figure 3.1: Perception Scores Distribution: Training Dataset



Crime Data. For crime outcomes, we construct separate training and testing datasets by calculating the crime indices within the radii of 100m, 200m, and 300m from all downloaded street views. Our datasets were split into 60% for training, 20% for validation, and 20% for testing to ensure robust model evaluation and generalization.

We first collect geocoded crime incident data from 17 cities in 2019, either downloaded from open data portals or obtained through police records requests. Using this data, we calculate the number of crimes within the 100m, 200m, and 300m radii from the panorama location. Then, we computed a crime index for each location by weighting major property and violent crime categories according to their relative costs to burglary: [2]

$$\begin{aligned}
\text{Crime Index}_r = & \left(\frac{67277}{13096}\right) \cdot \text{Robbery Count}_r + \\
& \left(\frac{87238}{13096}\right) \cdot \text{Aggravated Assault Count}_r + \\
& \text{Burglary Count}_r + \\
& \left(\frac{2139}{13096}\right) \cdot \text{Theft Count}_r + \\
& \left(\frac{9079}{13096}\right) \cdot \text{Motor Vehicle Theft Count}_r, \\
& \text{where } r = 100\text{m}, 200\text{m}, 300\text{m}
\end{aligned}$$

Table 3.1 displays the summary statistics for crime outcomes, demonstrating similar statistics across training, validation, and test datasets. On average, within

[2] Cost of crime estimates come from `https://www.rand.org/well-being/justice-policy/centers/quality-policing/cost-of-crime.html`

100m of each location, there are fewer than 1 violent crime and 4.5 property crimes. For 200m, the averages are approximately 4 violent crimes and 18 property crimes, while for 300m, there are roughly 9 violent crimes and 40 property crimes.

Table 3.1: Summary Statistics of Crime Outcomes

	(1) Training		(2) Validation		(3) Test	
	mean	sd	mean	sd	mean	sd
Burglary Crime Counts, 100 m	0.623	1.623	0.625	1.559	0.622	1.555
Burglary Crime Counts, 200 m	2.495	4.236	2.505	4.223	2.497	4.189
Burglary Crime Counts, 300 m	5.545	8.206	5.542	8.111	5.536	8.133
Theft Crime Counts, 100 m	3.443	13.114	3.481	13.252	3.397	11.414
Theft Crime Counts, 200 m	13.395	35.699	13.405	35.298	13.258	32.431
Theft Crime Counts, 300 m	29.824	69.783	29.742	68.570	29.823	66.587
Motor Vehicle Theft Crime Counts, 100 m	0.488	1.315	0.499	1.316	0.490	1.314
Motor Vehicle Theft Crime Counts, 200 m	1.996	3.318	2.010	3.323	1.988	3.296
Motor Vehicle Theft Crime Counts, 300 m	4.400	6.216	4.368	6.140	4.369	6.081
Aggravated Assault Crime Counts, 100 m	0.517	1.628	0.522	1.611	0.510	1.530
Aggravated Assault Crime Counts, 200 m	2.106	4.586	2.107	4.603	2.079	4.467
Aggravated Assault Crime Counts, 300 m	4.633	8.986	4.602	8.777	4.596	8.800
Robbery Crime Counts, 100 m	0.396	1.405	0.404	1.364	0.392	1.317
Robbery Crime Counts, 200 m	1.591	3.796	1.594	3.721	1.580	3.645
Robbery Crime Counts, 300 m	3.502	7.278	3.497	7.067	3.475	6.972
Violent Crime Counts, 100 m	0.970	2.761	0.983	2.736	0.960	2.605
Violent Crime Counts, 200 m	3.936	7.978	3.940	7.956	3.899	7.741
Violent Crime Counts, 300 m	8.662	15.831	8.623	15.424	8.597	15.375
Property Crime Counts, 100 m	4.554	14.228	4.606	14.295	4.509	12.524
Property Crime Counts, 200 m	17.887	38.986	17.919	38.567	17.743	35.815
Property Crime Counts, 300 m	39.770	76.995	39.652	75.541	39.728	73.641
Crime Index, 100 m	7.000	18.212	7.094	17.894	6.928	17.081
Crime Index, 200 m	28.268	52.591	28.312	52.380	28.009	51.048
Crime Index, 300 m	62.321	105.177	62.050	102.293	61.903	102.133
Observations	364296		121518		121305	

3.3.2 Training CNN models

Our training model employs a Residual Network architecture (ResNet), a widely used deep learning model renowned for image classification tasks. Specifically, we fine-tune ResNet models pretrained on the Places365 dataset for our image regression tasks. The Places365 dataset, containing over 1.8 million images across 365 scene categories globally, is a benchmark dataset for training deep learning models in scene recognition. Fine-tuning CNNs pretrained on similar domains enables us to transfer pre-existing knowledge to our models, tailored for urban environments.

To enrich data diversity during training, validation, and testing, we implement various data augmentation strategies. Initially, we rescale all images to 448x448 pixels, then perform cropping at the four corners and center, reducing them to 224x224 pixels, and apply horizontal flips, generating 10 variations per image. During testing, predictions for each image are averaged across all 10 crops to enhance robustness. This augmentation ensures our model generalizes well across diverse urban scenes, significantly boosting performance. Our prediction task adopts a regression framework, aiming to minimize L2 loss between sample labels and model predictions.

For urban perception, due to the absence of a comprehensive dataset encompassing all 18 US cities, we trained a single model using training data, Place Pulse 2.0 data, available in 13 US cities. Labels for this model represent perception scores for images, scaled to the interval [0, 10], following De Nadai et al. (2016). ResNet18 and ResNet50 models, which represent 18 and 50 layers respectively, are both trained and used to generate predictions; this is because ResNet18 generates better performance for certain scores (e.g. safety), while ResNet50 yields superior results for others.

For models predicting crime indices, we adopt a localized approach, training separate models for each city using city-specific images. This strategy enables us to capture unique local patterns and characteristics accurately, ensuring our models reflect local urban dynamics. We use ResNet50 models to predict actual crime, as ResNet50 models outperform ResNet18, likely due to the larger dataset available for training.

Finally, for our prediction tasks, we select the best-performing model based on the highest correlation coefficient observed in the validation data.

3.3.3 Model Validation

The cross-validation results in Table 3.2 show similar performance, measured using the correlation coefficient between actual and predicted scores, for the ResNet-18 and ResNet-50 models in urban perception. The highest correlation coefficient (0.52) is for predicting "safe" scores.

Performance for "beautiful," "lively," and "wealthy" scores is slightly lower, with correlation coefficients of 0.5, 0.45-0.47, and 0.43, respectively. Predictions for "depressing" and "boring" are significantly lower, with coefficients of 0.36 and 0.27-0.30. Consequently, our subsequent analyses focus solely on predicted scores for "safe", "beautiful," "lively," and "wealthy", given the more reliable predictions on these dimensions.

Table B.19 show the correlation coefficients between predicted and actual labels in the testing data for each city. The performance of model improves when we predict crime indices within larger radii. The model performance varies by city, for example, with crime indices ranging from 0.369 (Nashville) to 0.761 (San Francisco) within the same radius. Overall, the pooled correlation coefficient across cities achieves 0.706 for crime indices within the 300 m radius, indicating a strong model fit for the fine-tuned ResNet-50 models.

Table 3.2: Correlation coefficients (ρ) between actual and predicted crime indices

	Resnet-18	Resnet-50
Beautiful	0.500	0.499
Boring	0.270	0.298
Depressing	0.356	0.367
Safe	0.524	0.520
Lively	0.452	0.467
Wealthy	0.432	0.432

Table 3.3: Correlation coefficients (ρ) between actual and predicted crime indices

City	100 m	200m	300m
Austin	0.371	0.398	0.354
Charlotte	0.421	0.411	0.485
Chicago	0.630	0.601	0.609
Dallas	0.444	0.525	0.610
Denver	0.519	0.744	0.784
Detroit	0.628	0.468	0.497
Fort Worth	0.428	0.651	0.618
Houston	0.578	0.644	0.648
Los Angeles	0.585	0.602	0.636
Nashville	0.451	0.689	0.590
New York City	0.741	0.775	0.792
Philadelphia	0.744	0.784	0.809
Phoenix	0.236	0.630	0.671
San Antonio	0.329	0.495	0.437
San Francisco	0.804	0.711	0.745
Seattle	0.809	0.806	0.844
Washington	0.509	0.599	0.554

3.4 Results

3.4.1 How effectively does the CNN model predict crime using street view images?

To assess how well our machine learning model predicts crime based on street view images relative to demographic variables, we run the following regressions on the testing sample of street view images. Specifically, we regress the actual crime index on three sets of variables: the predicted crime from street view images that captures the part of the crime that could be explained by the built environment, demographic and socioeconomic variables alone, and a combination of predicted values and demographic and socioeconomic variables. We link our street view image data to block group level demographics using 2015-2019 American Community Survey (ACS).

Table 3.4 indicate that the street view predictions of crime alone explains larger variation in actual crime than demographic variables alone. Across different radii (100m, 200m, and 300m), the CNN models' predictions explain 51.8%, 56.1%, and 60.2% of the variation in crime indices, respectively. In comparison, demographic variables alone account for only 8.6%, 15.3%, and 17.8% of the variation over the same radii.

When combining both the model's predictions and demographic variables, the R^2 values increase slightly to 59.2%, and 64.4% for crime indices within the 200m, and 300m radii, respectively. This marginal increase suggests that while demographics add some explanatory value, the built environment remains the dominant factor influencing crime patterns.

Overall, these findings highlight the critical role of the physical environment in shaping crime patterns across space. While demographic variables do contribute additional explanatory power, their impact is relatively minor compared to the built environment, implying that urban design and maintenance could play an important role in urban safety.

Table 3.4: Predictive Power on Actual Crime Index, Predicted Values vs. Demographics

	Std Crime Index, 100m			Std Crime Index, 200m			Std Crime Index, 300m		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted Std Crime Index, 100 m	1.171***		1.153***						
	(0.014)		(0.015)						
Log Population (ACS 15-19)		-0.124***	-0.082***		-0.182***	-0.101***		-0.178***	-0.083***
		(0.021)	(0.015)		(0.020)	(0.013)		(0.020)	(0.013)
Log housing units (ACS 15-19)		0.107***	0.098***		0.145***	0.119***		0.137***	0.108***
		(0.022)	(0.015)		(0.021)	(0.014)		(0.021)	(0.014)
Median HH Income (1K, 15-19 ACS)		0.001***	-0.001***		0.002***	-0.002***		0.002***	-0.002***
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
% College (ACS 15-19)		0.056*	0.053**		0.015	0.010		0.051	0.028
		(0.029)	(0.022)		(0.032)	(0.022)		(0.032)	(0.022)
Census Return Rate 2010		-0.385***	0.104**		-0.385***	-0.025		-0.442***	0.041
		(0.072)	(0.049)		(0.069)	(0.044)		(0.068)	(0.041)
Share recent built units (15-19 ACS)		-0.211***	0.151**		-0.339***	0.282***		-0.354***	0.238***
		(0.075)	(0.062)		(0.066)	(0.050)		(0.065)	(0.047)
Share units owner occupied (ACS 15-19)		-0.743***	-0.115***		-1.000***	-0.172***		-1.100***	-0.207***
		(0.026)	(0.017)		(0.025)	(0.016)		(0.025)	(0.015)
Share units occupied (ACS 15-19)		0.210***	-0.010		0.108**	-0.087**		0.085	-0.173***
		(0.053)	(0.039)		(0.051)	(0.036)		(0.054)	(0.038)
Share Age < 5 (ACS 15-19)		-0.608***	-0.139*		-0.802***	-0.231***		-1.040***	-0.427***
		(0.097)	(0.072)		(0.099)	(0.069)		(0.098)	(0.065)
Share Age Between 5 and 17 (ACS 15-19)		-0.430***	-0.059		-0.563***	-0.058		-0.678***	-0.131***
		(0.057)	(0.041)		(0.059)	(0.040)		(0.059)	(0.039)
Share Age > 65 (ACS 15-19)		0.005	-0.330***		-0.103*	-0.456***		-0.100*	-0.522***
		(0.059)	(0.043)		(0.057)	(0.040)		(0.058)	(0.039)
% Hispanic (ACS 15-19)		-0.155***	0.301***		-0.236***	0.426***		-0.257***	0.453***
		(0.038)	(0.025)		(0.046)	(0.028)		(0.046)	(0.028)
% White (ACS 15-19)		-0.312***	-0.034		-0.406***	0.020		-0.456***	-0.006
		(0.044)	(0.031)		(0.050)	(0.033)		(0.050)	(0.032)
% Black (ACS 15-19)		0.180***	0.252***		0.282***	0.317***		0.298***	0.341***
		(0.048)	(0.032)		(0.054)	(0.035)		(0.054)	(0.034)
% Other Languages (15-19 ACS)		0.291***	-0.123***		0.431***	-0.219***		0.470***	-0.249***
		(0.029)	(0.022)		(0.029)	(0.022)		(0.029)	(0.021)
Predicted Std Crime Index, 200 m				1.149***		1.119***			
				(0.014)		(0.015)			
Predicted Std Crime Index, 300 m							1.118***		1.089***
							(0.011)		(0.013)
Constant	0.034***	0.696***	0.035	0.060***	1.105***	0.275***	0.055***	1.249***	0.309***
	(0.003)	(0.081)	(0.058)	(0.003)	(0.084)	(0.056)	(0.003)	(0.084)	(0.054)
Observations	60859	57548	57548	60859	57548	57548	60859	57548	57548
R^2	0.518	0.086	0.518	0.561	0.153	0.592	0.602	0.178	0.644

3.4.2 Which environmental factors affect urban perception and predicted crime?

In Section 3.4.1, we find that the built environment could explain significantly larger variations in predicted crime than demographic and socioeconomic factors. One natural next question is, can we isolate the impact of specific environmental features on perceived and predicted crime? For instance, does the presence of elements like trash, trees, or graffiti notably influence both perceived safety and predicted crime incidences?

Existing correlational research attempts to compare crime levels between areas with and without trash. For instance, Sampson et al. (1997) found that social and physical disorder, including graffiti, were associated with increased perceptions of crime. However, the correlated nature of these features complicates causal inference, which is that areas with trash often also exhibit other environmental issues like graffiti or poor housing conditions, making it difficult to isolate the causal impact of a single factor due to potential omitted variable bias.

To address this, we use generative AI to create synthetic street views. For example, by adding trash to a street view image while keeping all other elements constant. We can examine how this synthetic image altered the CNN-model based predictions of actual crime and safety perception, relative to the original image's predictions. This approach mimics a controlled experiment, allowing us to single out specific factors and examine their causal impact.

Figure 3.2: Variations of Street View Images

(a) Original Street View

(b) Add Trash

(c) Add Graffiti

(d) Add Trash and Graffiti

Specifically, for each original street view (s) , we generated four main variations: (1) the street view without trash and graffiti, (2) the street view with added trash (or the removal of trash if it was present in the original), (3) the street view with added graffiti (or the removal of graffiti if it was present in the original), and (4) the street view with both trash and graffiti added. Figure 3.2 illustrates these variations for a example street view image in Philadelphia. We run the following regression model to examine their impact of adding trash, graffiti or both:

$$Y_{i,s} = \beta_s + \beta_1 \text{Has Trash}_i + \beta_2 \text{Has Graffiti}_i + \beta_3 \text{Has Trash}_i \times \text{Has Graffiti}_i + \epsilon_{i,s} \quad (3.1)$$

where i represents a street view image under one of the four conditions, s represents the group of original street view image that this variation belongs to, and β_s represent original street image fixed effects. The inclusion of the interaction term allows us to examine whether there is any interaction effect between graffiti and trash.

Table 3.5 presents the results. The presence of trash increases the CNN model-predicted crime index by 12% within a 100-meter radius, 5% within a 200-meter radius, and 3.6% within a 300-meter radius. In comparison, the effect of graffiti is smaller; adding graffiti increases the predicted crime index by 5% within a 100-meter radius, 2.4% within a 200-meter radius, and 2.6% within a 300-meter radius. And we do not observe any interaction effect between the presence of graffiti and trash on the predicted crime index.

It is important to note that the interpretation of these coefficients should be understood as the causal impact of graffiti or trash on the part of crime as directly influenced by the environment. However, this does not necessarily translate to the causal impact on actual crime, as we do not know how these environmental changes affect human behavior that is not captured by the street view images. The models' predictions likely represent how Bayesian agents update their belief on the perceived likelihood of crime based on changes in the presence of certain environmental feature.

In comparison, Table 3.6 and Table 3.7 present results on predicted perceived safety under both ResNet-18 and ResNet-50 models. These tables reveal consistent patterns, indicating that graffiti enhances perceptions of an area being beautiful, lively, safe, and wealthy, with the most significant impact on liveliness. Interestingly, despite graffiti increasing predicted crime, it also boosts perceived safety. This paradox may be explained by graffiti's association with social activity and community presence. Graffiti could indicate a vibrant, active area with frequent human activity, leading to increased natural surveillance and a sense of safety. Additionally, graffiti may attract more visitors including artists and onlookers, reinforcing the sense of safety and community.

Conversely, the results in Tables 3.6 and 3.7 suggest more varied and less robust impacts of trash on different dimensions of perception. Trash has a significant positive impact on dimensions including beautiful, lively and wealthy, but has a null to negative impact on predicted safety.

Overall, we find that graffiti has a more positive impact on urban perception, particularly regarding perceived safety, compared to trash. The findings further suggest that environmental factors like graffiti and trash affect urban perception and crime differently. This discrepancy highlights the complex relationship between fear of crime and actual crime. Consistent with Kelling and Wilson (1982), visible signs of disorder, such as trash and graffiti, may indicate more opportunities for crime and attract criminals. However, while environmental cleanliness is crucial for perceptions of safety, the presence of community activity, even with graffiti, can positively influence perceptions of neighborhood quality. Our results indicate that crime reduction interventions must consider the nuanced ways in which environmental factors affect both actual crime and public perception. Further research is needed to explore the underlying reasons behind these distinctions.

Table 3.5: The Impact of Graffiti and Trash on Predicted Crime

	Predicted Crime Index 100m			Predicted Crime Index 200m			Predicted Crime Index 300m		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Has Graffiti	0.723***		0.671***	1.428*		1.371*	3.537**		3.287*
	(0.218)		(0.197)	(0.781)		(0.774)	(1.710)		(1.742)
Has Trash		1.826***	1.774***		3.105***	3.049***		5.028**	4.778**
		(0.596)	(0.592)		(0.672)	(0.695)		(2.162)	(2.205)
Has Graffiti & Trash			0.104			0.113			0.501
			(0.087)			(0.154)			(0.448)
Constant	15.735***	15.184***	14.848***	60.188***	59.349***	58.664***	140.046***	139.301***	137.657***
	(0.109)	(0.298)	(0.310)	(0.390)	(0.336)	(0.497)	(0.855)	(1.081)	(1.486)
Observations	360	360	360	360	360	360	360	360	360
R^2	0.959	0.963	0.964	0.986	0.988	0.988	0.989	0.989	0.989
Image FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.6: The Impact of Graffiti and Trash on Resnet18-Predicted Perception

	Predicted Beautiful		Predicted Lively		Predicted Safe		Predicted Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has Graffiti	0.143***		0.267***		0.107***		0.129***	
	(0.045)		(0.036)		(0.030)		(0.033)	
Has Trash		0.032		0.162***		-0.059**		0.185***
		(0.024)		(0.028)		(0.023)		(0.028)
Constant	24.252***	24.308***	23.811***	23.864***	24.390***	24.473***	25.317***	25.289***
	(0.022)	(0.012)	(0.018)	(0.014)	(0.015)	(0.011)	(0.016)	(0.014)
Observations	360	360	360	360	360	360	360	360
R^2	0.990	0.989	0.987	0.984	0.992	0.992	0.987	0.989
Image FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.7: The Impact of Graffiti and Trash on Resnet50-Predicted Perception

	Predicted Beautiful		Predicted Lively		Predicted Safe		Predicted Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has Graffiti	0.220***		0.220***		0.163***		0.077**	
	(0.034)		(0.035)		(0.041)		(0.035)	
Has Trash		0.047*		0.166***		0.044		0.150***
		(0.025)		(0.032)		(0.031)		(0.033)
Constant	24.463***	24.550***	24.828***	24.855***	24.474***	24.534***	25.002***	24.966***
	(0.017)	(0.013)	(0.017)	(0.016)	(0.020)	(0.015)	(0.017)	(0.016)
Observations	360	360	360	360	360	360	360	360
R^2	0.994	0.991	0.988	0.986	0.985	0.983	0.983	0.985
Image FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

3.5 Conclusion

In this paper, we apply deep learning models to Google Street View images to quantify the built environment on a large scale. We find that the built environment, as captured by these models, can explain a substantial variation in crime rates, demonstrating a stronger predictive power than demographic and socioeconomic variables. Additionally, we conduct controlled experiments using generative AI to create synthetic street view images, revealing that environmental features such as graffiti and trash significantly increase predicted crime rates. Interestingly, while graffiti is associated with higher predicted crime, it also increases perceived safety.

Our results suggest that environmental attributes influence these two phenomena differently, and underscore the crucial distinction between fear of crime and actual crime. Visible signs of disorder like trash and graffiti can create an impression of neglect and reduced community vigilance, potentially increasing opportunities for criminal activities (Kelling and Wilson, 1982). On the other hand, graffiti's dual role in increasing both perceived safety and predicted crime can be attributed to its cultural value in some contexts. Graffiti can enhance the vibrancy and appeal of urban areas, fostering a sense of community and cultural richness that makes residents feel safer, despite the associated rise in predicted crime.

Future research should explore several key areas to build on these findings. Currently, we use machine learning models to evaluate both original and synthetic images, as these models make these evaluations scalable. However, we could also involve human participants to evaluate these synthetic images for comparison and repeat this analysis.

Beyond graffiti and trash, examining the effects of other environmental interventions, such as improved street lighting, increased greenery, and better maintenance of public spaces, could provide insights into alternative policies for enhancing urban safety. This approach would allow us to compare the relative effectiveness of different environmental interventions on safety perception and crime. Additionally, while the current analysis focuses on the presence of trash or graffiti in street views, investigating how the quantity of these features impacts crime rates and safety perceptions could offer more nuanced insights into environmental influences on urban safety.

In conclusion, our research demonstrates the powerful potential of leveraging advanced technologies like deep learning and generative AI to better understand and address urban crime and safety. This framework has great potential in other research settings, such as evaluating the impact of place-based policies over time on urban perception.

The findings further underscore the importance of understanding the distinction between fear of crime and actual crime in future research. By recognizing the distinct impacts of environmental factors on both fear of crime and actual crime, policymakers can design more effective, targeted interventions that not only reduce crime rates but also enhance the perceived safety and well-being of urban residents.

Appendix A

Appendix to "Smartphone Data Reveal Neighborhood-Level Racial Disparities in Police Presence"

A.1 Other Data Sources

Census demographics data. Census block group and city characteristics data come from American Community Survey (ACS) 2013-2017 5-year estimates. We collect data on each block group's racial composition (% Black, Hispanic, and Asian), population, median household income, percent college graduates, and census form mail return rate. We also collect city level data on racial composition (% Black, White, Hispanic, and Asian).

Homicide data. Homicide data is collected by The Washington Post and covers homicide information (including latitude-longitude location, arrest decision, victim demographics) in 50 of the largest U.S. cities from 2007 to 2017 (Rich 2020). For several cities, the homicide data is not available for the whole decade: for example, in New York City, data are provided in 2016 and 2017 only (so we collected NYC homicide data between 2013 to 2016 from NYC open data portal); for San Antonio, data are only available between 2013 to 2016. The definition of homicide follows the FBI's Uniform Crime Reporting Program, including murder and non-negligent manslaughter while excluding suicides, accidents, justifiable homicides, and deaths caused by negligence. We use records of homicides to measure crime-driven demand for policing given the high accuracy of homicide reporting.

Law Enforcement Management and Administrative Statistics (LEMAS) data. The LEMAS data contains information on police officers' demographics, salaries, and functions, and agencies' duties, structures, and policies for 3499 local law enforcement agencies in 2016 (Bureau of Justice Statistics 2016). We obtain the racial composition of full time sworn officers and supervisors for 21 cities' police departments to compare with the imputed race of smartphone users. Among the 21 cities, the Indianapolis Metropolitan police department is not included in the LEMAS data, while the Phoenix and the San Antonio Police Departments have missing data on officers' and supervisors' race, respectively.

FBI Uniform Crime Report (UCR) - Law Enforcement Officers Killed or Assaulted (LEOKA) data. UCR-LEOKA data contains measures of officers that are killed or assaulted and total officer employment as of October 1st of each year at the departmental level (Kaplan 2020). We compare the police officer counts in the 2017 UCR-LEOKA data with the smartphone measure of patrol officers.

NYPD Officer Home Zip Code data. Data on NYPD police officers' home zip code comes from Bell (2016) through submission of a Freedom of Information Law (FOIL) request to the NYPD. The data reports the number of police officers that live in a specific zip code and patrol in a specific precinct. We calculate the total number of police officers that live in a zip code across all precincts to compare with the police officer counts that we infer to "live" in that zipcode from the smartphone location data.

Police Enforcement Action data. We collect 6 cities' geocoded data on police arrests in 2017 from each city's open data portal.[1] We collect geocoded data on police stops in nine cities from multiple sources, including open data portals for New York City, Philadelphia and Denver, Stanford Open Policing Project (Pierson et al. 2020) for Columbus, Nashville, Houston, San Antonio, and Oklahoma City and Ba et al. (2021) for Chicago. We collect 2017 stop data for most cities, and for cities in which 2017 data are not available, we use data closest to 2017: for Chicago, we use data in 2015; for Columbus and Oklahoma City, we use data in 2016. We match the latitude-longitude location of a police action to a census block group and aggregate the total number of stops or arrests during a year in a block group. Note that a small fraction of police action data are missing location information. While the missing records usually account for less than 5% of the observations for most cities, 13.49% of the stop records have missing location information for the Chicago Police Department.

A.2 Alternative Crime-driven Demand Measures

In this section, we explore the robustness of policing disparity estimates to alternative crime-driven demand measures. In Table A.3, we measure crime-driven demand using homicide data from 2013 to 2016, and include the average homicide count and distance to the nearest homicide between 2013 and 2016 in the regression. The estimates are quantitatively similar when using multiple years of homicide data. It is worth noting that while including information on older homicides does allow re-

[1]The 6 cities are: New York City, Los Angeles, Chicago, Dallas, Austin, Washington.

searchers to differentiate between neighborhoods without homicides in 2016, it is not obvious that police "should" do the same. Given the potential negative consequences of police interaction, particularly for young Black people (Rios, 2011), failing to update patrol patterns to reflect current, rather than past, violence may itself be a component of anti-Black bias in addition to a proxy for neighborhood demand for police.

To provide a direct measure of demand for police services as well as suspicion of criminal activity, in Table A.4, we control for the number of 311 calls in New York City where the geocoded 311 calls data are made publicly available. In the case of New York City, we do not find evidence suggesting that the number of 311 calls explain the police presence disparity in Black neighborhoods, regardless of controlling for the total number of calls (Column 3), or calls handled specifically by NYPD (Column 4), or calls handled by the nine major agencies (Column 5). In contrast, conditional on neighborhood socioeconomic characteristics, the number of 311 calls explains away roughly 60% of the enhanced police time in Hispanic neighborhoods, and all additional police time in Asian neighborhoods.

A.3 Sensitivity to Visitors' Foot Traffic

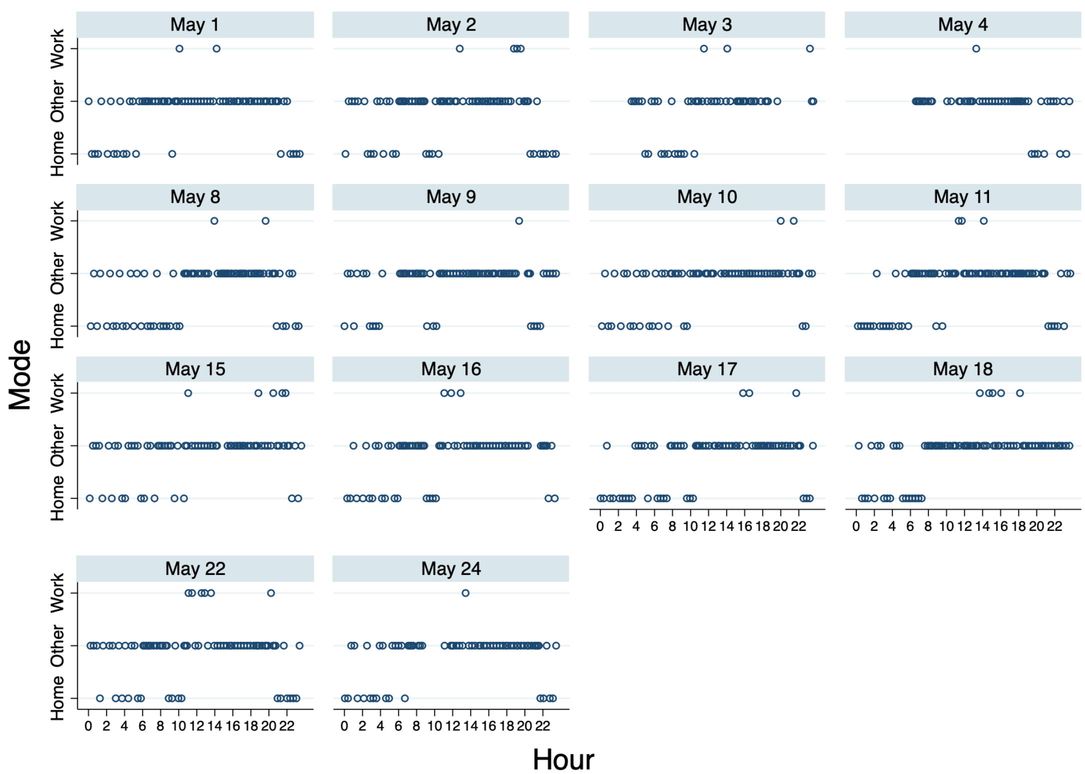
We demonstrate that our results are not sensitive to foot traffic from non-residents in two ways. First, we examine police presence during non-working hours by excluding pings between 9 am to 5 pm on weekdays, shown in Table A.5. We observe a strikingly similar pattern as in Table 1, suggesting that the estimates are not driven by daytime foot traffic. Second, we complement the above analysis by removing block groups that are likely to have large levels of visitor foot traffic in one city, New York City, that accounts for the largest number of block groups (N = 6,226) among the 21 cities. We exclude block groups in Precinct 1 (Wall Street), 6 (the West Village), 8 (Penn Station, Grand Central), 14 (Midtown South) and 18 (Midtown North). Comparing the estimates of exposure disparities where we include every block group in NYPD precincts (column 1-2) or exclude block groups in five NYPD precincts (column 3-4) in Table A.6, suggests that our results are insensitive to the exclusion of precincts with potentially high levels of non-residential foot traffic.

A.4 Disparities Over the Course of a Shift

Officers begin each shift at a station and, after receiving specific instructions about their daily tasks (in a process known as "roll call"), leave to patrol their beat with relatively little real-time oversight. Enforcement activity generally peaks midway through an officer's shift, suggesting that the way officers spend their patrol time may vary over the course of a day (Chalfin and Goncalves 2021). Appendix Figure A.7 plot how the share of time officers spend in more Hispanic and more Black places increases as their shift rolls out. The difference between how much time officers spend in more Hispanic versus Whiter places increases from the first hour of the shift through the third hour. In places where more Black people live, the disparities in police time are most pronounced halfway through a shift and then decline.

A.5 Figures and Tables

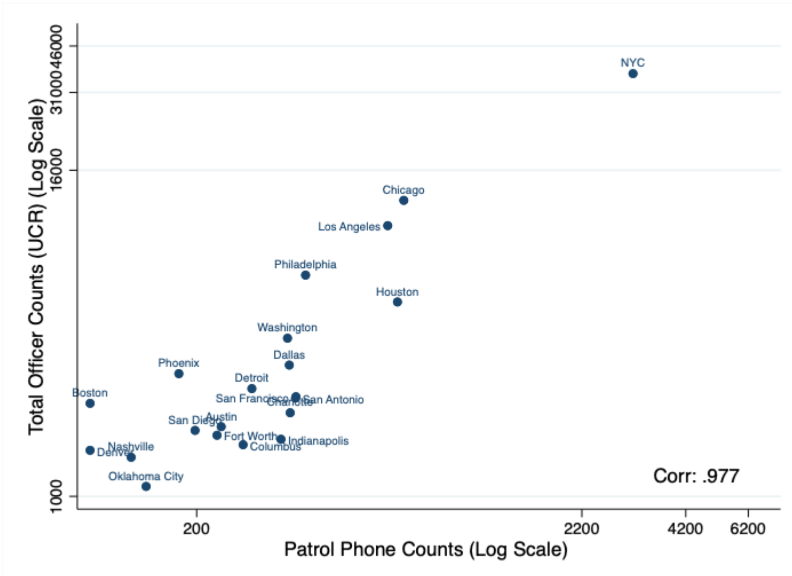
Figure A.1: Spatial Pattern of Pings of a Smartphone Observed in LAPD



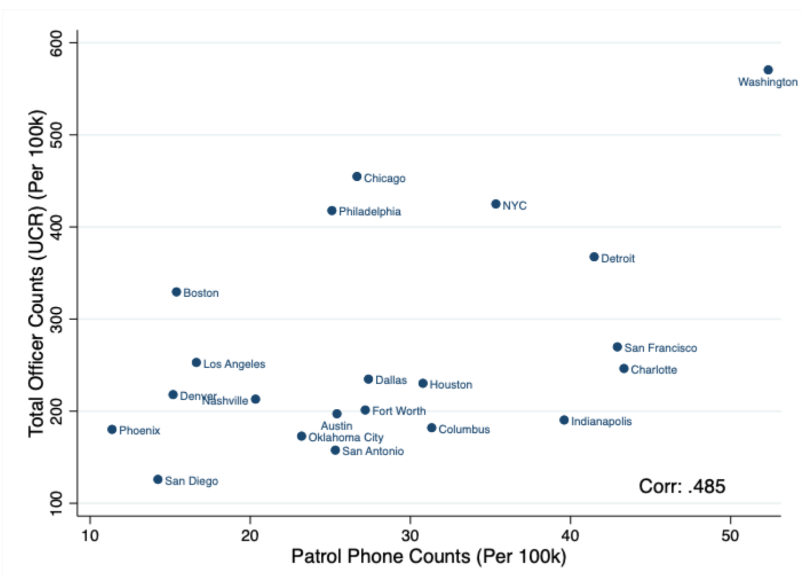
Notes: The spatial pattern of smartphone pings is categorized as either Home, Other, or Work. Smartphone is "at home" if the ping location is at the Home Geohash-7 (a 152 x 152 m grid); "at Work" if the ping location is in any police stations' building boundaries. Pings observed at locations other than "Home" and "Work" are classified as "Other".

Figure A.2: Police Officer Validation at the City Level

(a) UCR Officer Counts and Patrol Smartphone Counts

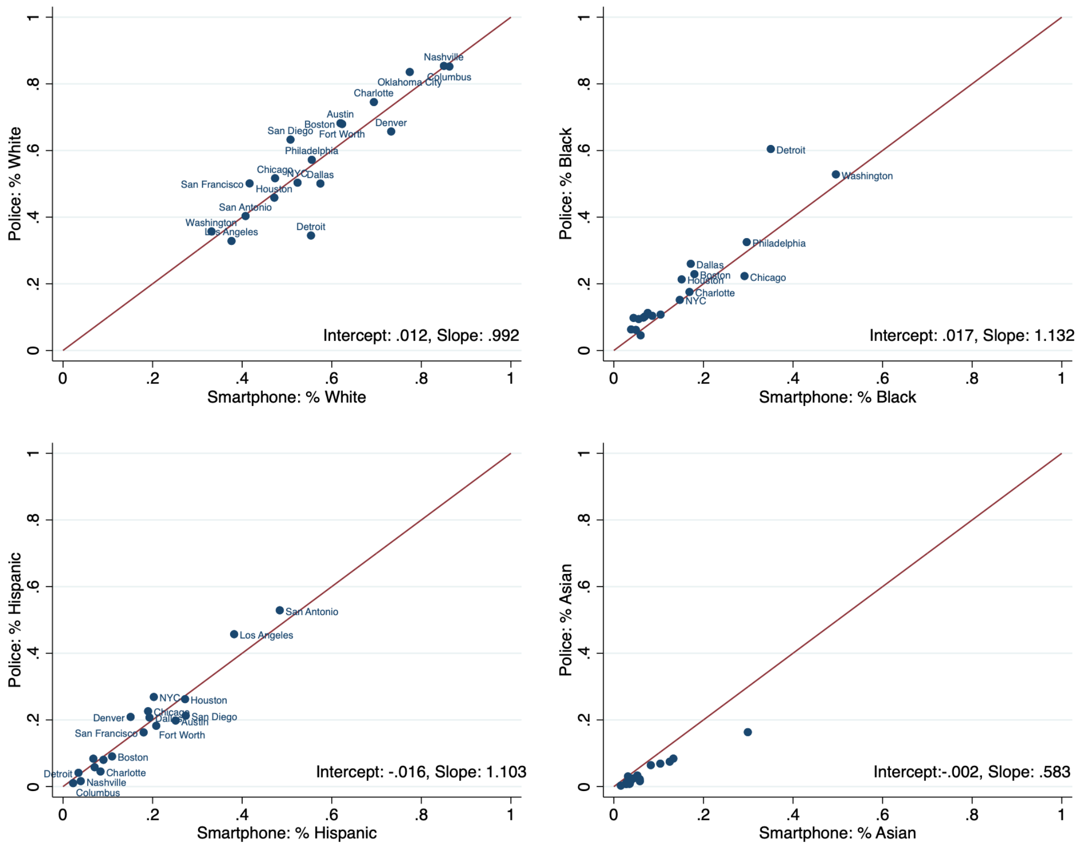


(b) UCR Officer and Patrol Smartphone (Per Capita Value)



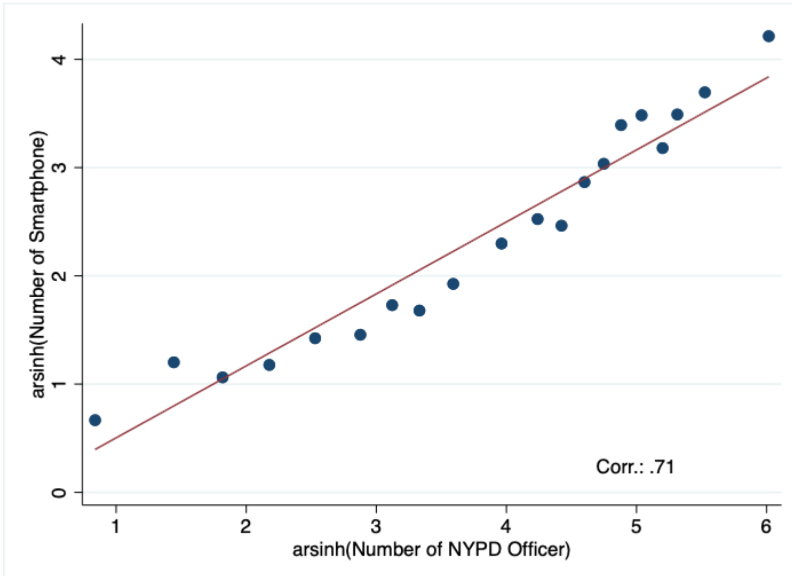
Notes: Total Officer Counts on the y-axis reports the number of officers (with arrest powers) in each city's police department on October 1st, 2017 from Uniform Crime Report (UCR) data. *Patrol Smartphone Counts* reports the number of smartphones that have at least one "shift" during 2017. Correlation coefficient between the two measures is reported.

Figure A.3: LEMAS Police Force Racial Composition vs. Smartphone Racial Composition



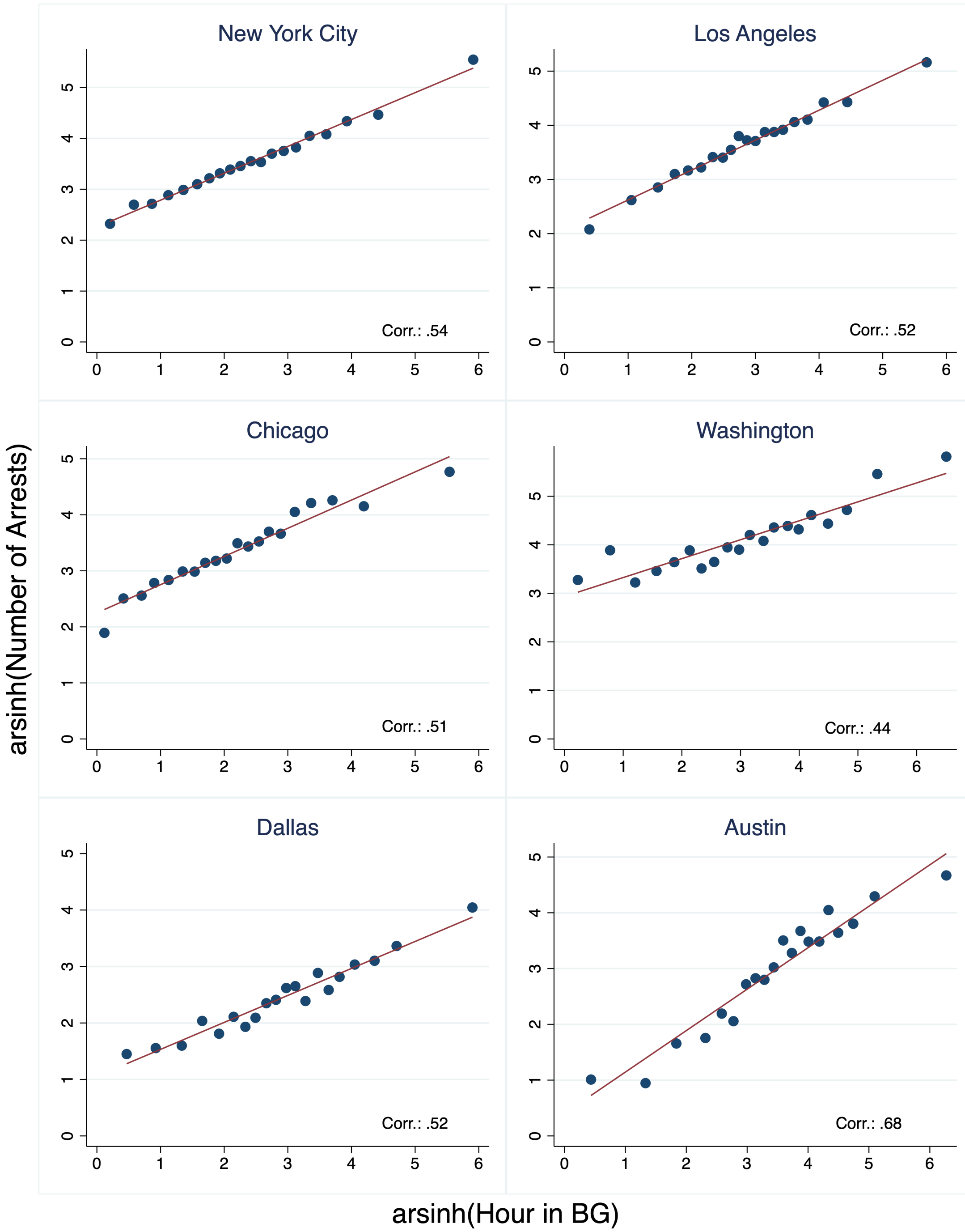
Notes: Police % White (Black, Hispanic, Asian) represents measures of racial composition of police officers from LEMAS data. Smartphone: % White (Black, Hispanic, Asian) denotes the smartphone-imputed racial composition for likely patrol officers based on home blocks.

Figure A.4: Police Officer Validation, a Residence-based Check for NYPD Officers at the Zip Code Level



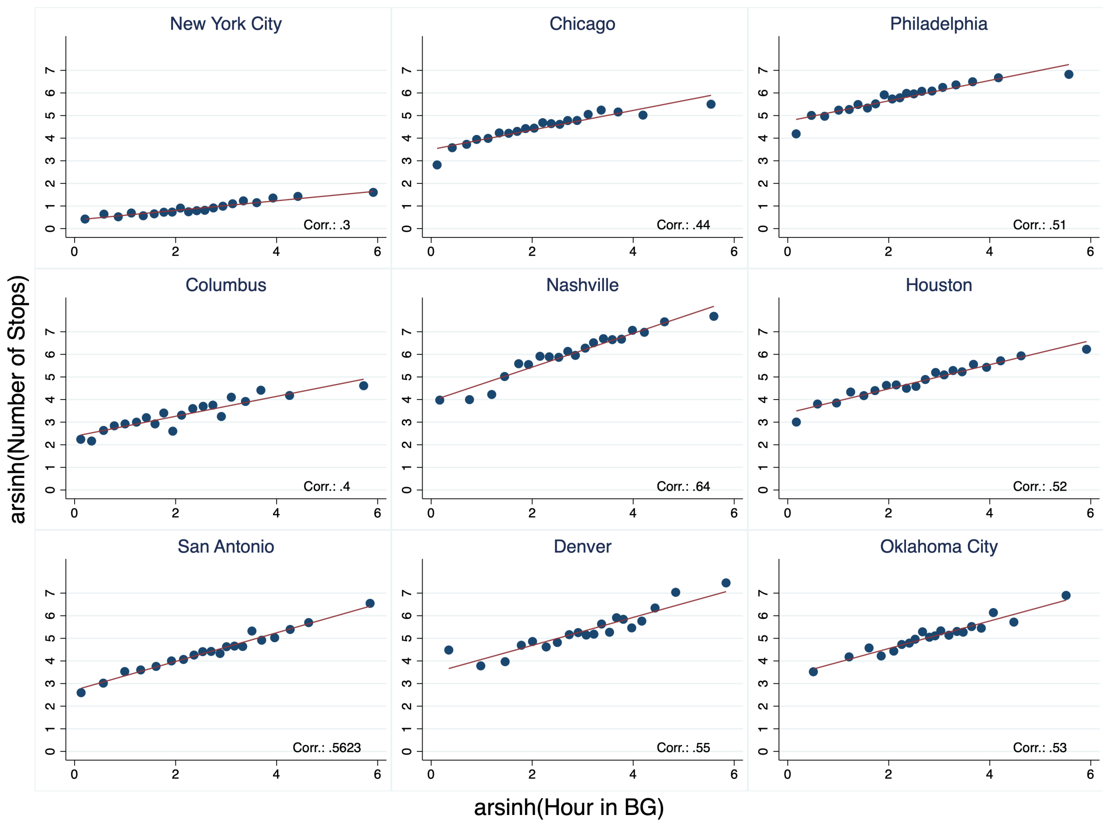
Notes: This figure presents a binned scatter plot of the number of smartphones from NYPD that we infer "live" in a zip code vs. the actual number of NYPD police officers living in a zip code, both transformed in arsinh values. We include all zip codes in the FOIL request data, with zip codes grouped into 20 equal-sized bins. Correlation coefficient between the two measures (in arsinh values) is reported.

Figure A.5: Number of Arrests vs. Police Hours Across Block Groups



Notes: Each panel presents a binned scatter plot of the number of arrests vs. the police hours observed in the block groups, with both variables measured in arsinh values. Block groups are grouped into 20 equal size bins. Correlation coefficient between the two measures (in arsinh values) is reported in each panel.

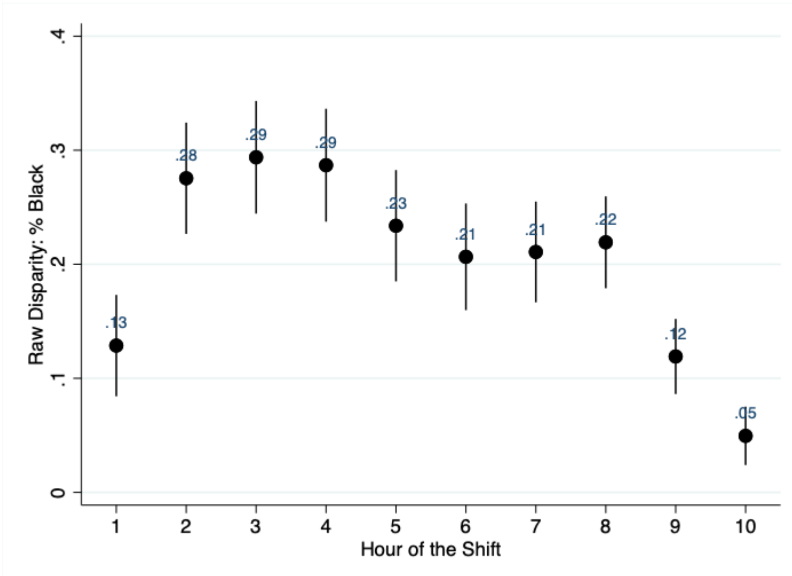
Figure A.6: Number of Stops vs. Police Hours Across Block Groups



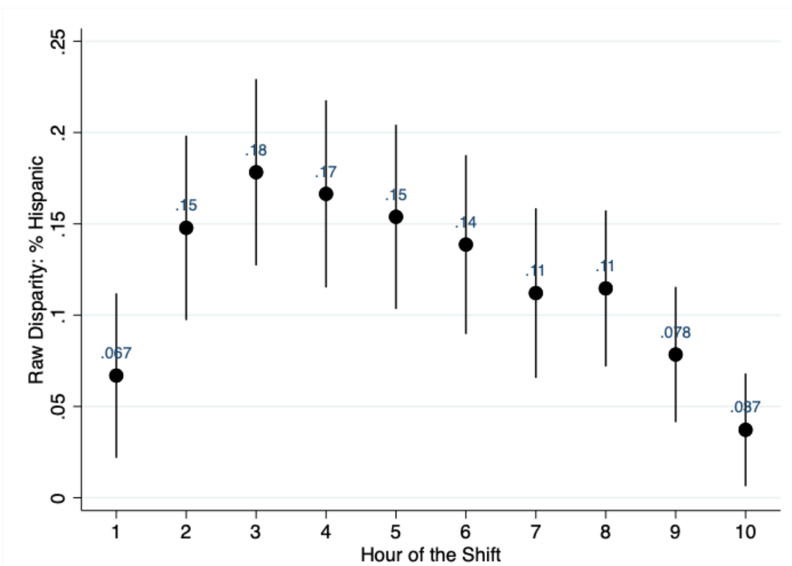
Notes: Each panel presents a binned scatter plot of number of stops vs. the police hours observed in the block groups, with both variables transformed in arsinh values. Block groups are grouped into 20 equal-sized bins. Correlation coefficient between the two measures (in arsinh values) is reported in each panel.

Figure A.7: Racial Disparity in Police Presence over the Course of a Shift

(a) Black-White Disparity



(b) Hispanic-White Disparity



Notes: Figure plots coefficients of % Black (Hispanic) share from a regression where police presence in each hour of the shift is regressed against the % Black, % Hispanic and % Asian, with city fixed effects included.

Figure A.8: Supervisor: % Black vs. Officer: % Black

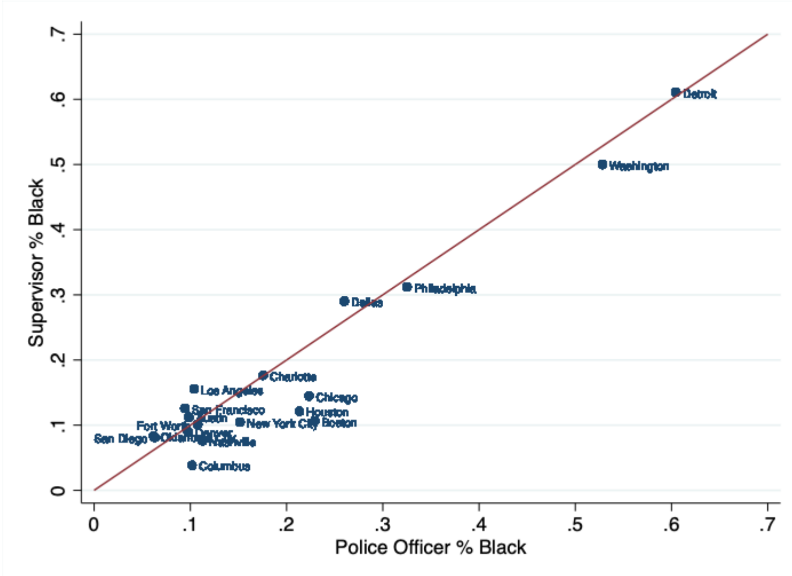
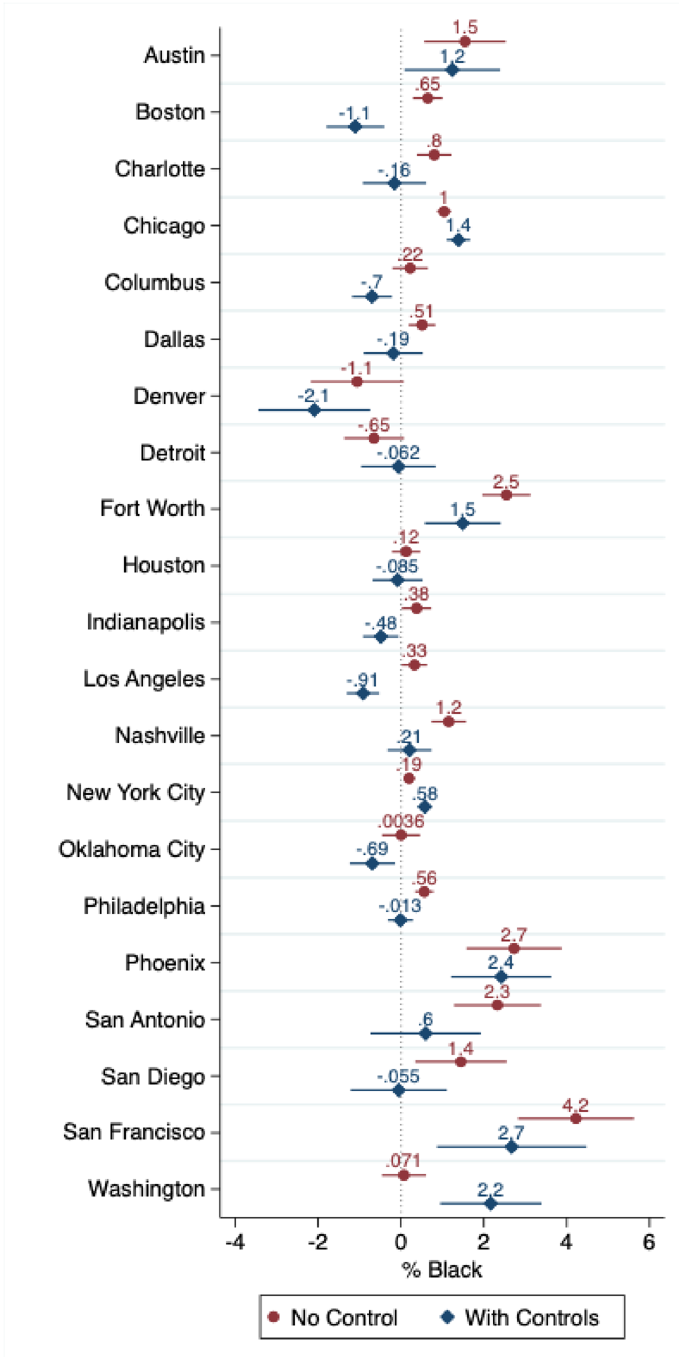


Figure A.9: City-specific Estimates of Black-White Disparity



Notes: "No Control" ("With Controls") condition plots the coefficient for % Black in the OLS regression: $arsinh(Hour_i) = \beta_0 + \beta_1 Race_i + \epsilon_i$ ($arsinh(Hour_i) = \beta_0 + \beta_1 Socioeconomics_i + \beta_2 Crime_i + \beta_3 Race_i + \epsilon_i$). *Race* include % Black, % Hispanic and % Asian. *Socioeconomics* include log population, % college graduates, median household income, census form return rate. *Crime* include distance to nearest homicide and homicide count in 2016.

Table A.1: Racial Composition: Smartphone Measure vs. LEMAS

VARIABLES	(1) Police: % White	(2) Police: % Black	(3) Police: % Hispanic	(4) Police: % Asian
Smartphone: % White	0.627*** (0.101)			
City % White	0.647*** (0.125)			
Smartphone: % Black		0.685*** (0.153)		
City % Black		0.384** (0.136)		
Smartphone: % Hispanic			0.882*** (0.196)	
City % Hispanic			0.184 (0.134)	
Smartphone: % Asian				0.587*** (0.140)
City % Asian				-0.00341 (0.125)
Constant	-0.0271 (0.0452)	-0.00701 (0.0164)	-0.0264** (0.0103)	-0.00190 (0.00309)
Observations	19	19	19	19
R-squared	0.921	0.921	0.935	0.946

Notes: Police % White (Black, Hispanic, Asian) represents measures of racial composition of police officers from LEMAS data. Smartphone: % White (Black, Hispanic, Asian) denotes the smartphone-imputed racial composition of likely patrol officers based on home blocks. City % White (Black, Hispanic, Asian) denotes the share of population that is identified as White (Black, Hispanic, Asian) in the city. Robust standard errors are reported in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table A.2: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Police Presence:					
Hour	23799	26.722	200.585	0	14707.31
arsinh(Hour)	23799	2.515	1.415	0	10.289
Number of shifts	23799	71.381	130.198	0	5320
arsinh(Number of shifts)	23799	4.277	1.245	0	9.272
Neighborhood Characteristics:					
% Black	23682	.237	.31	0	1
% Hispanic	23682	.287	.284	0	1
% Asian	23682	.084	.137	0	.983
Population	23799	1425.74	820.84	0	18369
% College Graduates	23679	.338	.251	0	1
Median Household Income (1K)	22526	62.553	38.174	2.499	250.001
Census Form Return Rate	23671	.736	.088	0	1
Distance to nearest 2016 homicide (km)	23799	1.331	1.612	.001	23.759
Homicide Count 2016	23799	.152	.472	0	7

Notes: This table provides summary statistics of police presence and neighborhood characteristic variables across block groups in the 21 cities.

Table A.3: Disparities in Neighborhood Police Exposure (Controlling for Homicides from 2013-2016, Including NYC)

VARIABLES	(1) arsinh(Hour)	(2) arsinh(Hour)	(3) arsinh(Hour)	(4) arsinh(Hour)	(5) arsinh(Hour)
% Black	0.350***	0.512***	0.276***	0.286***	0.271***
	(0.0328)	(0.0354)	(0.0480)	(0.0506)	(0.0526)
BG % Black X Police: % Black				0.0456	1.475+
				(0.306)	(0.883)
BG % Black X Supervisor: % Black					-1.340+
					(0.803)
% Hispanic	0.505***	0.404***	0.287***	0.242***	0.219***
	(0.0343)	(0.0365)	(0.0560)	(0.0587)	(0.0596)
% Asian	0.360***	0.294***	-0.0194	-0.0379	-0.0303
	(0.0735)	(0.0787)	(0.0822)	(0.0838)	(0.0841)
Log Population			0.391***	0.405***	0.433***
			(0.0209)	(0.0217)	(0.0223)
% College Graduates			1.153***	1.196***	1.219***
			(0.0676)	(0.0701)	(0.0708)
Median Household Income (1K)			-0.00416***	-0.00399***	-0.00375***
			(0.000394)	(0.000403)	(0.000406)
Census Form Return Rate			-1.224***	-1.256***	-1.317***
			(0.126)	(0.132)	(0.135)
Avg 13-16 Homicide Count			0.305***	0.307***	0.304***
			(0.0174)	(0.0182)	(0.0187)
Distance to nearest 13-16 homicide (km)			-0.157***	-0.164***	-0.163***
			(0.0106)	(0.0118)	(0.0123)
Observations	23,682	23,682	22,521	20,961	20,112
R-squared	0.010	0.104	0.173	0.159	0.164
City FE	No	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of exposure disparity among census block groups (BGs) across 21 cities (Column 1, 2, 4, 5) and within cities (Column 3, 6). The dependent variable is police hours observed in BGs (excluding pings moving faster than 50 mph), transformed in arsinh values. All race variables (including neighborhood racial composition, Police: % Black and Supervisor: % Black) are mean-centered. Household income is measured in thousands of dollars, census return rates range from 0-1. Robust standard errors are reported in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table A.4: Disparities in Neighborhood Police Exposure (Controlling for Number of 311 Calls)

VARIABLES	(1) arsinh(Hour)	(2) arsinh(Hour)	(3) arsinh(Hour)	(4) arsinh(Hour)	(5) arsinh(Hour)
% Black	0.194**	0.576***	0.570***	0.605***	0.660***
	(0.0666)	(0.0839)	(0.0797)	(0.0825)	(0.0785)
% Hispanic	0.126+	0.726***	0.266*	0.393***	0.299**
	(0.0712)	(0.106)	(0.105)	(0.109)	(0.103)
% Asian	-0.140	0.272*	0.367**	0.232*	0.0807
	(0.111)	(0.121)	(0.113)	(0.118)	(0.103)
Log Population		0.452***	0.112*	0.240***	0.0883*
		(0.0487)	(0.0459)	(0.0485)	(0.0403)
% College Graduates		1.704***	1.157***	1.462***	0.189
		(0.136)	(0.128)	(0.133)	(0.132)
Median Household Income (1K)		-0.00147*	-0.000972	-0.000866	-0.00237***
		(0.000735)	(0.000680)	(0.000706)	(0.000665)
Census Form Return Rate		-0.335	1.256***	0.851**	0.799**
		(0.250)	(0.267)	(0.274)	(0.259)
Distance to nearest 2016 homicide (km)		-0.156***	-0.123***	-0.130***	-0.0892***
		(0.0266)	(0.0247)	(0.0262)	(0.0238)
Homicide Count 2016		0.465***	0.414***	0.454***	0.374***
		(0.0867)	(0.0830)	(0.0846)	(0.0778)
asinh(311 Calls - NYPD)				0.369***	0.0717**
				(0.0244)	(0.0250)
asinh(311 Calls - HPD)					-0.0194
					(0.0119)
asinh(311 Calls - DOT)					0.326***
					(0.0237)
asinh(311 Calls - DEP)					0.0884***
					(0.0253)
asinh(311 Calls - DSNY)					-0.0605*
					(0.0237)
asinh(311 Calls - DOB)					0.0849***
					(0.0214)
asinh(311 Calls - DPR)					-0.138***
					(0.0186)
asinh(311 Calls - DOHMH)					0.184***
					(0.0195)
asinh(311 Calls - DHS)					0.300***
					(0.0149)
asinh(Total 311 Calls)			0.713***		
			(0.0320)		
Observations	6,226	5,821	5,821	5,821	5,821
R-squared	0.003	0.080	0.170	0.118	0.287

Notes: This table presents OLS estimates of exposure disparity among census block groups (BGs) in NYC. In column 5, we control for the number of calls handled by the top 9 agencies: NYPD, Housing Preservation and Development (HPD), Department of Transportation (DOT), Department of Environmental Protection (DEP), Department of Sanitation (DSNY), Department of Buildings (DOB), Department of Parks & Recreation (DPR), Department of Health and Mental Hygiene (DOHMH), Department of Homeless Services (DHS) respectively. Robust standard errors in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table A.5: Disparities in Neighborhood Police Exposure (During Non-working Hours)

VARIABLES	(1) arsinh(Hour)	(2) arsinh(Hour)	(3) arsinh(Hour)	(4) arsinh(Hour)	(5) arsinh(Hour)
% Black	0.388***	0.549***	0.383***	0.379***	0.388***
	(0.0315)	(0.0338)	(0.0457)	(0.0488)	(0.0510)
BG % Black X Police: % Black				0.00226	0.360
				(0.294)	(0.860)
BG % Black X Supervisor: % Black					-0.362
					(0.777)
% Hispanic	0.511***	0.413***	0.287***	0.226***	0.206***
	(0.0327)	(0.0348)	(0.0539)	(0.0569)	(0.0578)
% Asian	0.436***	0.300***	-0.0405	-0.0658	-0.0566
	(0.0693)	(0.0742)	(0.0784)	(0.0800)	(0.0803)
Log Population			0.409***	0.418***	0.442***
			(0.0200)	(0.0209)	(0.0215)
% College Graduates			0.981***	1.019***	1.038***
			(0.0643)	(0.0673)	(0.0682)
Median Household Income (1K)			-0.00361***	-0.00365***	-0.00345***
			(0.000374)	(0.000383)	(0.000387)
Census Form Return Rate			-1.345***	-1.372***	-1.430***
			(0.121)	(0.126)	(0.129)
Distance to nearest 2016 homicide (km)			-0.108***	-0.109***	-0.107***
			(0.00622)	(0.00694)	(0.00717)
Homicide Count 2016			0.185***	0.186***	0.187***
			(0.0193)	(0.0201)	(0.0206)
Observations	23,682	23,682	22,521	20,961	20,112
R-squared	0.011	0.106	0.167	0.143	0.148
City FE	No	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of exposure disparity among census block groups (BGs) across 21 cities (Column 1, 2, 4, 5) and within cities (Column 3, 6). The dependent variable is police hours observed in BGs (during non-working hours), transformed in arsinh values. All race variables (including neighborhood racial composition, Police: % Black and Supervisor: % Black) are mean-centered. Household income is measured in thousands of dollars, census return rates range from 0-1. Robust standard errors are reported in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table A.6: Disparities in NYC Neighborhood Police Exposure

VARIABLES	(1) arsinh(Hour)	(2) arsinh(Hour)	(3) arsinh(Hour)	(4) arsinh(Hour)
% Black	0.194**	0.576***	0.258***	0.538***
	(0.0666)	(0.0839)	(0.0665)	(0.0838)
% Hispanic	0.126+	0.726***	0.182*	0.617***
	(0.0712)	(0.106)	(0.0709)	(0.105)
% Asian	-0.140	0.272*	-0.114	0.207+
	(0.111)	(0.121)	(0.111)	(0.120)
Log Population		0.452***		0.425***
		(0.0487)		(0.0478)
% College Graduates		1.704***		1.564***
		(0.136)		(0.137)
Median Household Income (1K)		-0.00147*		-0.00237***
		(0.000735)		(0.000704)
Census Form Return Rate		-0.335		-0.204
		(0.250)		(0.251)
Distance to nearest 2016 homicide (km)		-0.156***		-0.160***
		(0.0266)		(0.0265)
Homicide Count 2016		0.465***		0.452***
		(0.0867)		(0.0857)
Observations	6,226	5,821	6,062	5,672
R-squared	0.003	0.080	0.005	0.072

Notes: This table presents the OLS regression estimates of the disparity in police presence among census block groups (BGs) in New York City. Column 1 and 2 include the full sample; column 3 and 4 exclude BGs in Precinct 1 (Wall Street), 6 (the West Village), 8 (Penn Station, Grand Central), 14 (Midtown South) and 18 (Midtown North). The dependent variable is the police hours observed in census block groups (excluding pings moving faster than 50 mph), transformed in arsinh values. Household income is measured in thousands of dollars, census form return rates range from 0-1. Robust standard errors are reported in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table A.7: Disparities in Neighborhood Police Exposure and Downstream (Stop) Disparities

VARIABLES	(1) arsinh(Hour)	(2) arsinh(Number of stops)	(3) arsinh(Stops/Hours)	(4) arsinh(Hour)	(5) arsinh(Number of stops)	(6) arsinh(Stops/Hours)
% Black	0.464***	1.322***	0.634***	0.540***	0.986***	0.467***
	(0.0426)	(0.0349)	(0.0357)	(0.0570)	(0.0482)	(0.0492)
% Hispanic	0.200***	1.069***	0.568***	0.448***	0.820***	0.396***
	(0.0484)	(0.0417)	(0.0402)	(0.0696)	(0.0601)	(0.0596)
% Asian	0.357***	0.384***	0.00793	0.264*	0.0360	-0.0526
	(0.0973)	(0.0689)	(0.0569)	(0.103)	(0.0763)	(0.0657)
Log Population				0.425***	0.329***	-0.0463*
				(0.0281)	(0.0231)	(0.0217)
% College Graduates				1.387***	0.521***	-0.521***
				(0.0839)	(0.0737)	(0.0712)
Median Household Income (1K)				-0.00341***	-0.00307***	0.00157***
				(0.000542)	(0.000448)	(0.000386)
Census Form Return Rate				-1.253***	-1.113***	0.225+
				(0.158)	(0.133)	(0.126)
Distance to nearest 2016 homicide (km)				-0.102***	-0.0869***	0.0129
				(0.0101)	(0.0117)	(0.0114)
Homicide Count 2016				0.232***	0.326***	0.116***
				(0.0248)	(0.0202)	(0.0238)
Observations	13,969	13,969	13,915	13,176	13,176	13,126
R-squared	0.033	0.762	0.663	0.097	0.774	0.660
City FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of disparities in exposure, stops, and stops per hour among census block groups (BGs) across 9 cities: New York City, Chicago, Houston, Philadelphia, San Antonio, Oklahoma City, Denver, Columbus, Nashville. All race variables (including neighborhood racial composition, Police: % Black and Supervisor: % Black) are mean-centered. Household income is measured in thousands of dollars, census return rates range from 0-1. Robust standard errors are reported in parentheses: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Appendix B

Appendix to "Does Neighborhood Investment Actually Affect Crime? New Evidence from LIHTC and Smartphone-based Measures of Policing"

B.1 Data Appendix

LIHTC and QCT Data We obtain data on annual QCT designation and LIHTC-subsidized property from the U.S. Department of Housing and Urban Development (HUD). QCT designation data indicates the QCT status of all census tracts and includes variables that HUD uses to determine a tract's QCT eligibility. The LIHTC property data covers all LIHTC-funded projects placed in service between 1987 and 2020, along with information on project location, type (construction, rehabilitation, or other), year that the project is allocated credits and placed in service, number of all units, and number of low-income units, among others. We compute the stock and flow of LIHTC projects and units placed in service in each tract-year. Panel B of Table A.2 summarizes the number of LIHTC projects and units that are placed in service in a tract between 2018-2019.

LODES-RAC Data The LEHD Origin-Destination Employment Statistics (LODES) data, published by U.S. Census Bureau, offers employment statistics based on workers' residence (origin) and workplace (destination). We use the Residence Area Characteristic (RAC) data, which provides annual statistics on job counts for workers residing in a census block, and job counts for workers in various earning, age, race, and education categories.[1] We aggregate these block-level statistics to tract-level to estimate change in neighborhood composition from 2017 to 2019.[2]

Crime Data We collect geocoded crime incident data for 18 cities in 2017 and 2019 from each city's open data portal or through open record requests. These data record the date, location, and offense category for most crime incidents.[3] We assign the location of each crime incident to a census tract, and approximate the annual crime rate by calculating the number of crimes per thousand jobs in a tract, where the denominator comes from the LODES-RAC data.[4] In Panel C of Table A.2, we present summary statistics for the number of crimes per 1000 jobs, comparing QCTs

[1]The specific RAC data we use covers all primary jobs in all segment of workforce.

[2]We prefer the LODES-RAC data over the American Community Survey data for estimating short-term neighborhood turnover because, to the best of our knowledge, only LODES-RAC data provides annual statistics on neighborhood characteristics; while publicly available ACS data only offers five-year estimates on neighborhood demographics.

[3]Note that some city agencies (e.g. Seattle and San Francisco) do not disclose location information for homicide and rape due to privacy concerns, resulting in missing data for these crimes.

[4]We use the number of jobs for all residents in a tract as the denominator due to the same reason that only LODES-RAC data provides annual statistics on neighborhood characteristics to the best of our knowledge. The number of primary jobs that a tract's residents have is highly correlated with the ACS's total population estimate ($\rho = 0.9$), making it a relevant and valid measure for changing population size. Appendix Table B.16 demonstrates that the results are quantitatively similar when calculating per capita crimes using ACS five year estimate as the denominator, and Appendix Table B.17 displays results on the crime counts in QCTs. We detect smaller and less precisely estimated effects on the number of crimes, potentially as the inflow of new residents increases return for criminal opportunities.

to eligible, non-selected tracts.

311 Call Data We collect geocoded 311 calls data that are available in 11 cities in 2017 and 2019 through each city's open data portal. These data record the date, location, and request description (or category) for 311 service requests. We focus on any requests containing"street light" in their description to measure street light repair requests. We similarly assign the location of each 311 request to a census tract to calculate the number of 311 street light repair requests in a tract. Panel B of Table A.2 also shows that on average, QCT have 25 street light repair requests in a year relative to 16 street light repair requests in eligible non-selected tracts.

B.2 Heterogeneity by Time

In Appendix Figure B.2, we explore how the increased patrol in QCTs are distributed over time to provide a more nuance picture of policing patterns throughout a day. Specifically, we plot doubly robust estimates from separate regressions when the outcome variables are arsinh-transformed police hours observed in each hour of day in a tract. Police time in QCTs increases most during late afternoon, evening and midnight, while it does not increase, or even decreases in QCTs from morning to noon.

In Appendix Table B.5, we similarly compute officer-hours spent in a census tract during various time periods: daytime (7 am - 6 pm), night time (7 pm - 11 pm, 12 am - 6 am), weekdays, and weekends. We observe a 26% increase in police presence during nighttime and a 19% increase during weekends in QCTs compared to non-selected tracts. In contrast, the effect of QCT status on daytime police hours, though positive, are not precisely estimated and there is a smaller, 11% increases in police time during weekdays. The increased police presence is thus concentrated during non-working hours when residents are more likely to be in their home tracts.

B.3 Police Activities

In this section, we examine how other aspects of police activities change to understand police behavior driving more time spent in QCTs. While we are not able to observe specific officer actions, we can measure various dimensions of policing that are indicative of policing styles using smartphone location data.

In Column 6 and 7 of Appendix Table B.5, we do not find evidence of an increase in the number of unique officers visiting QCTs. However, we observe a 10% increase in the number of "shifts" (i.e. unique daily visits) taking place in QCTs. This suggests that the increase in police presence is not mainly driven by more officers responding to specific events in QCTs, but is more likely due to officers patrolling QCTs more frequently. These increased patrol frequencies translate into a 2.6 percentage point (9.2%) increase in the fraction of days with police presence in QCTs, again indicating that increased police presence is not solely driven by long visits to QCTs on specific days to respond to particular events, but rather by an increase in daily patrol frequency.

To examine whether the change in policing time reflects change in patrol assignments, especially assignment of officers from different racial backgrounds, which could be particularly relevant for police-civilian interaction (Ba et al. 2021), we calculate the absolute difference between the racial composition of residents (White, Black, or Hispanic) and imputed racial composition of officers in the same tract.[5] Column 9 of Appendix Table B.5 reveals that receiving QCT status is not associated with a significant change in officer's race composition in response to changes in the racial composition of residents.

While we do not find evidence of changes in officer demographics in QCTs, the way that officers patrol in these areas could change in response to local investments. In Table B.6, we decompose the total police time spent in a census tract into the time 1) when officers move at an average speed at least 1 mph, indicative of a "drive-through" (i.e. "a short visit") at a place, or 2) when officers move at a average speed below 1 mph, indicating a "longer visit".[6] Column 1 of panel B reports a 15.5% increase in police time during relatively short visits, compared to a less precise 9.2% increase in officer time during longer visits. Column 3 and 4 indicate an increase in the average speed of officer phone pings, with a smaller and less precise increase in speed when weighted by each ping's duration. While suggestive, these patterns are more in line with the idea that police officers have more car-based patrols in QCTs than out-of-car investigations that might involve slower movement. QCT residents are thus more likely to experience increased ambient police presence rather than a

[5]The distance measure in a tract is computed as: $\sum_r |\text{Share of Residents of race } r - \text{Share of Officers of race } r|$, where r could be White, Black, or Hispanic. Tract-level racial composition in 2017 (2019) comes from the 2013-2017 (2015-2019) American Community Survey estimates. We impute an officer's race based on the officer phone's home census block group's racial composition using 2013-2017 (2015-2019) ACS estimates, respectively. The average percentage of White (Black, Hispanic) officers present in a tract is weighted by each officer's time spent in a tract. We do not use the LODES-RAC data to impute race because LODES-RAC data does not differentiate between non-Hispanic White and White Hispanic Americans.

[6]To approximate a ping's speed, we calculate the Haversine distance between a smartphone ping to its previous ping, and divide this distance by the time since the previous ping. We then calculate the average speed for police officers' ping using all surrounding pings within the 5-minute window to smooth out this measure. Police hours on long (short) visits in a tract are the total time for all pings with the average speed below (at least) 1 mph in a tract.

greater number of direct police contacts.

Taken as a whole, our preferred estimates under the doubly robust specification suggest that police respond to improvement in neighborhood physical infrastructure by increasing their local presence. This could be driven by individual officer decisions, or departmental-level decisions, or both. Importantly, our results provide less support to the idea that increased police presence is solely dependent on more response to calls, to the extent that this will significantly increase police time when they pay relatively longer visits in QCTs.

B.4 Property-level Analysis

Our main empirical strategy exploits cross-sectional quasi-experimental variation in the rates of development in QCTs relative to eligible but non-selected tracts due to HUD-imposed population cap. In this section, we examine the impact on police patrols around LIHTC properties placed in service in 2018, following the specification in Asquith et al. (2021). We compare change in police presence within a treatment radius with those in larger, "control" radius (i.e. a "ring" difference-in-differences approach). Examining what happens around LIHTC projects is a fundamentally different question than the central analysis in this paper, in that it explores the local response to the construction of a rental property, rather than the overall impact of QCT status on neighborhood investment, which includes increased LIHTC construction and funding from other place-based programs. That said, in Appendix Table B.7, we provide geographically disaggregated results that mirror our tract-level causal identification by estimating how police patrol changes around LIHTC construction in QCTs, eligible QCTs that are dropped, and tracts that are not eligible to be QCTs.

As in our tract-level estimates, when choosing a treatment radius of 250 meters and control radius of 600 meters, we observe that LIHTC construction in disadvantaged neighborhoods without greater development incentive (i.e. dropped QCTs) is associated with significantly less police presence, compared to housing construction in QCTs with relatively more development incentives. In comparison, there are either no significant, or much smaller differences in the property-level estimates for QCTs compared to dropped QCTs when using a treatment radius of 0.25 miles (approximately 400 meters) or 0.5 miles.

Unlike tract-level estimates, the property-level estimates indicate a general decline in police presence in the immediate vicinity of 2018 LIHTC properties. This could be attributed to various factors, including varying levels of investments at different geographic scales, and differences in the location studied. Moreover, since LIHTC properties tend to be spatially clustered, using a larger radius might introduce bias from spatial spillover effects. We also do not employ the strategy of considering future LIHTC housing as the control group, as the construction of the future LIHTC properties can be endogenous to existing LIHTC properties, as discussed in Voith et al. (2022). Given these concerns, we think that property-level analysis may not be most appropriate in this context.

B.5 Robustness

In this section, we present additional analyses to examine the robustness of the results on police presence. First, we show that our results are not sensitive to alternative definitions of police presence. Appendix Table B.8 displays quantitatively similar estimates when we measure police time using only 8 to 12 hour shifts (instead of any shifts longer than 4 hours), excluding pings that move faster than 25 mph (rather than 50 mph), using shifts bracketed by home visits no longer than 18 hours (as opposed to 24 hours). Point estimates are slightly attenuated and less precise when excluding shifts with long hours spent within the police stations, which is reasonable, to the extent that phones that spend longer time in police stations are more likely to belong to police officers rather than non-police phones that visit police stations frequently. We also exclude on-shift movement departing from police headquarters, as this movement are more likely to belong to non-patrol officer movement compared to those departing from community stations.[7] We find a slightly attenuated point estimate for this measure, suggesting that our results are not solely driven by other police officers that do not perform regular patrol duties.

Appendix Table B.9 indicates that our estimates remain quantitatively similar to those when using log-transformed police activity measures and excluding observations with zero values. This addresses concerns raised by Chen and Roth (2023) regarding the scale-dependency of estimated effects when using log-like transformations with zero-valued outcomes.

How much does the change in where police officers spend time driven by changes in the relative contribution of stations to our sample of smartphone pings from 2017 to 2019? These variation may represent actual change in each station's policing intensity, or/and simply smartphone sampling variation across different years. To investigate how changes in the proportion of pings from different police stations affect our main doubly robust estimates, we resample pings during shifts from each police stations in 2017 with replacement, such that the number of pings from each station

[7]That said, the presence of any police officers, irrespective of rank or duty, are meaningful for the public safety surveillance in neighborhoods.

in 2017 matches those number in 2019. Using these resampled pings, we construct a measure of synthetic police presence for 2017. In other words, this measure ensures that each station's relative contribution of smartphone pings is the same as in 2019, assuming that sampled pings are representative of the broader movement patterns of the station. Table B.10 presents the results where we compare change in actual police presence in 2019 with the synthetic police presence in 2017. Our findings indicate a 37% reduction in the doubly robust of police hours, primarily driven by reduced effect during day time. On the other hand, the effect of QCT-spurred development on nighttime police presence remains largely unchanged. This suggests that change in police presence is not solely driven by the change in the relative contribution for each stations to the sample, though there is suggestive evidence that change in police presence reflects more station-level changes in police activities rather than variations in officers' activities within a station during the daytime.

While our main specification assumes that any differential trends in policing or crime between QCTs and non-selected tracts are driven by city-level trends, in Appendix Table B.15 and B.11, we allow for differential time trends in the high and low poverty tracts within the same city. Specifically, we compare each tract's poverty rate with the city median and classify each tract within each city as high or low poverty, and further demean the policing and crime outcomes by city-year-high (low) poverty pairs. We find that the point estimates on crime and policing remain quantitatively similar under this specification, though the estimate of the total police time is less precise due to a decreased effect during daytime. We also re-estimate the effect using only cities with binding population caps to address the concern that cities with binding population caps may be less comparable to those without. Appendix Table B.12 shows that the estimates using this subsample remain fundamentally unchanged.

One notable feature of the doubly robust estimates is that tracts that are among the most economically distressed receive the largest weights in estimation, since without the population cap, these tracts would mostly likely to be awarded QCT status. We further check the sensitivity of our doubly robust estimates to the most-weighted tracts. Specifically, we re-estimate the effect on police hours by iteratively excluding one of the top ten tracts receiving the largest weights. Panel (a) of Appendix Figure B.3 shows that these leave-one-out estimates are still quantitatively similar to the original estimate, though for a few excluded tracts, zero is included in the 95% confidence interval. The reduction in the statistical precision when excluding the heavily weighted tracts is not surprising; given that with only 18 cities, our sample is limited in the number of eligible but non-selected tracts and excluding the poorest tracts leaves us a less good sample of matched counterfactuals. [8]

Though the analysis sample in the main paper covers only 18 cities, which are cities with available data for both smartphone-based police presence and crime, we can alleviate some concern of small sample sizes using an extended sample where we include five more cities where we have only smartphone-based police presence data (but not crime data).[9] Panel (b) of Appendix Figure B.3 reveals that, with

[8]To see this more clearly, we compare the leave-one-out doubly robust estimates on the stock of LIHTC units, estimated using a sample with all tracts in the US metropolitan area, versus using the current sample with only 18 cities in Appendix Figure B.4. While panel (a) of Appendix Figure B.4 suggests that all leave-out estimates are almost the same with the estimate obtained without excluding tracts, panel (b) suggests that under a much smaller sample, it is more likely that excluding one tract could have a larger impact on the exact point estimate and reduce the statistical precision of estimate. Still, we see that the original estimate without excluding any tracts in the main paper are similar in magnitude to the estimate using the full sample with all metropolitan tracts.

[9]The five cities are: Boston, Columbus, El Paso, Indianapolis and Oklahoma City.

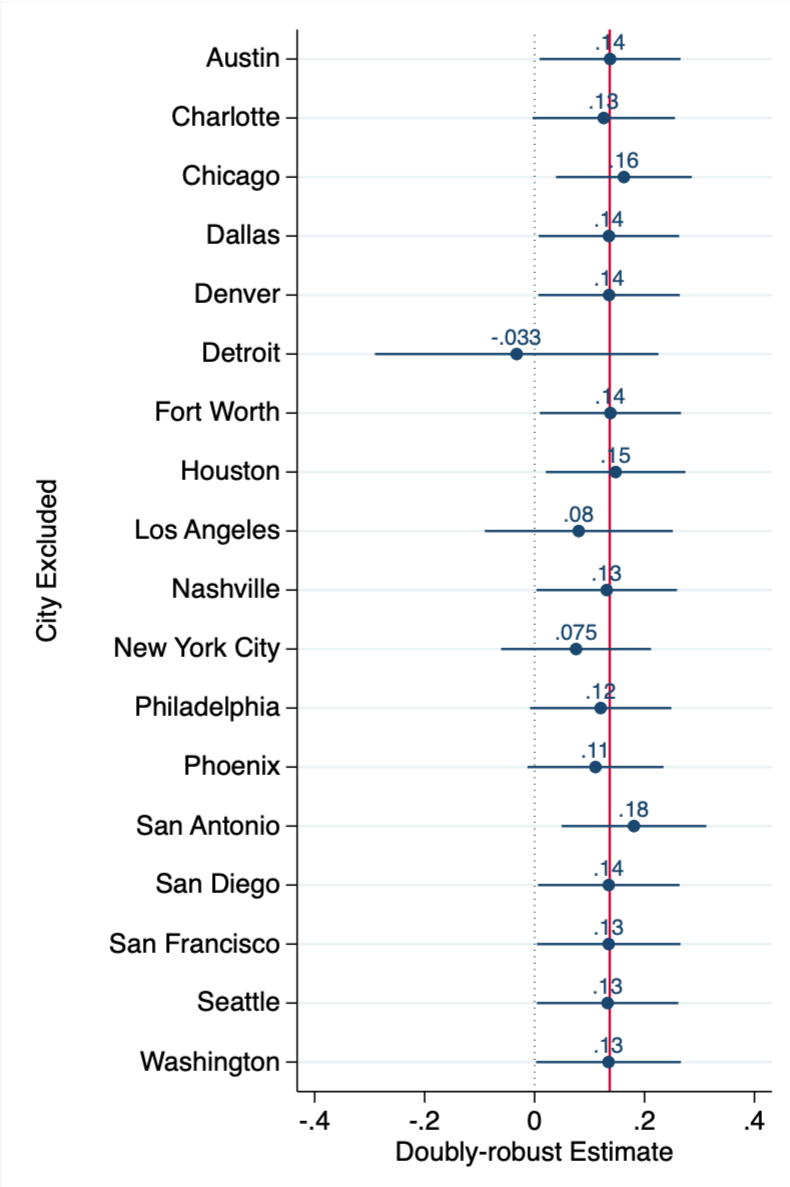
an increased number of eligible but non-selected tracts, most leave-one-out doubly robust estimates are statistically significant and quantitatively close to both the original doubly robust estimate under this sample as well as the estimate using the sample with 18 cities.

Finally, we investigate the sensitivity of our estimates to alternative matching schemes and estimators. Appendix Table B.13 presents other estimators in addition to the doubly robust estimators. We find that, the point estimates for LIHTC units, policing and crime vary under different estimators, with the outcome regression estimators provide a lower bound of the estimates while the inverse probability weighting estimators provide a upper bound. What this implies is that, the inverse probability weighting approach emphasizes the theoretical issue of finite distribution of policing resources. Compared to the outcome regression approach that weights each tract equally, the inverse probability weighting approach weights non-selected tracts that are most economically disadvantaged most heavily, which are typically geographically more proximate to the QCTs. In line with Appendix Table B.3, the fact that we are seeing a larger (despite more imprecise) point estimate suggests that the equally poor tracts with little funding for investments experienced greater loss when policing resources were constrained.

In Appendix Figure B.5, we present the estimates when matching tracts with alternative sets of variables, such as only using HUD's QCT designation rule—median household income and poverty rates, and all ACS variables on demographics and housing while excluding past LIHTC units. We find that the estimates on police hours are quantitatively similar across different matching schemes, while estimates on violent crime rates and LIHTC units are less precise under specific matching schemes. Nevertheless, the general patterns align with the main estimates.

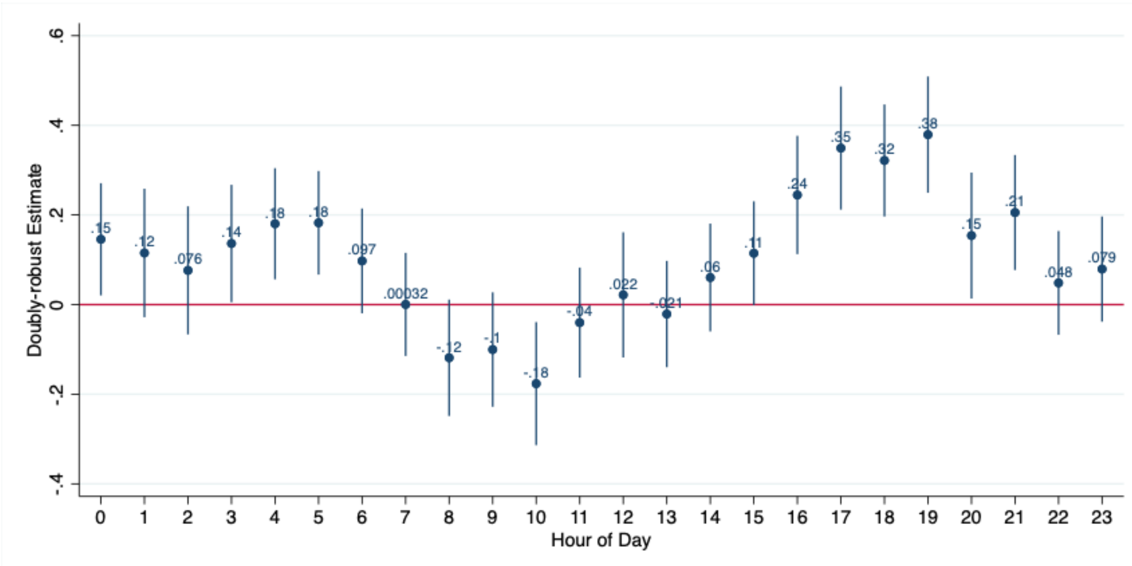
B.6 Figure and Table

Figure B.1: Effect heterogeneity by city



Notes: This figure displays doubly robust estimates for the effect of QCT status on police hour (demeaned and in inverse hyperbolic sine (arsinh) values), in which we iteratively exclude one city in our sample. The red line indicates the original doubly robust estimate (0.135) when no tract is excluded.

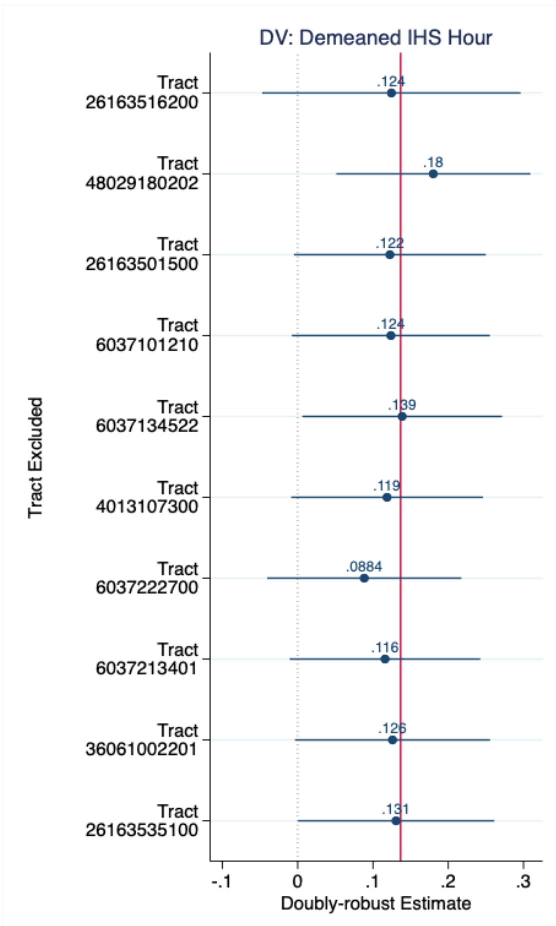
Figure B.2: Effect on police hour by hour of day



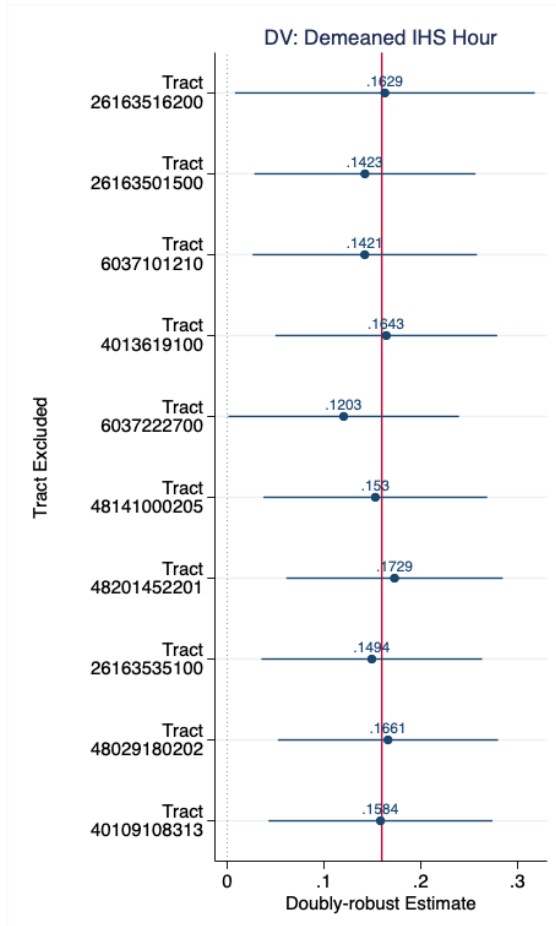
Notes: This figure displays coefficients from separate regressions when the outcome variables are arsinh-transformed police hours observed in each hour of day in a tract, demeaned by city-year.

Figure B.3: Sensitivity to most weighted tracts: leave-one-out estimates on the police hours

(a) Sample: 18 cities



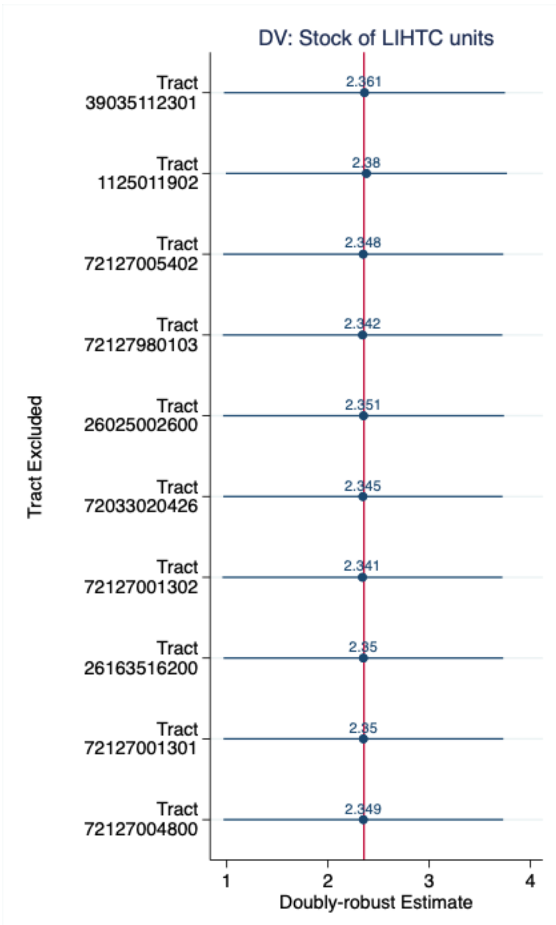
(b) Sample: 23 cities



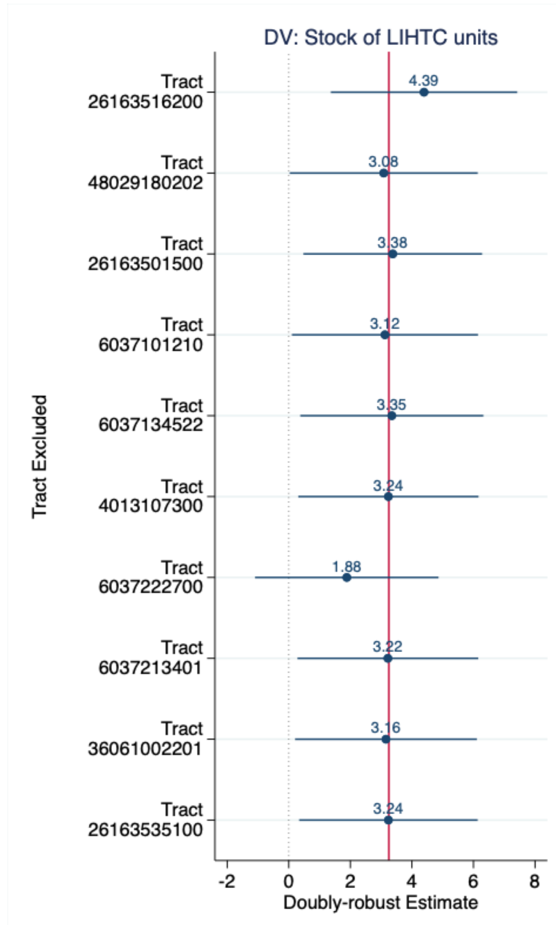
Notes: This figure displays doubly robust estimates for the effect of QCT status on police hour (demeaned and in arsinh values), in which we iteratively exclude one of the top ten most weighted tracts in our sample. The red line indicates the original doubly robust estimate when no tract is excluded. Panel (a) uses the same sample in the main paper (i.e. all QCT-eligible tracts in 18 cities) for estimation, while panel (b) adds five more cities (Boston, Columbus, El Paso, Indianapolis and Oklahoma City) with available smartphone-based police presence data to the sample in addition to the existing 18 cities.

Figure B.4: Sensitivity to most weighted tracts: leave-one-out estimates on the stock of LIHTC unit

(a) Sample: all US metro tracts



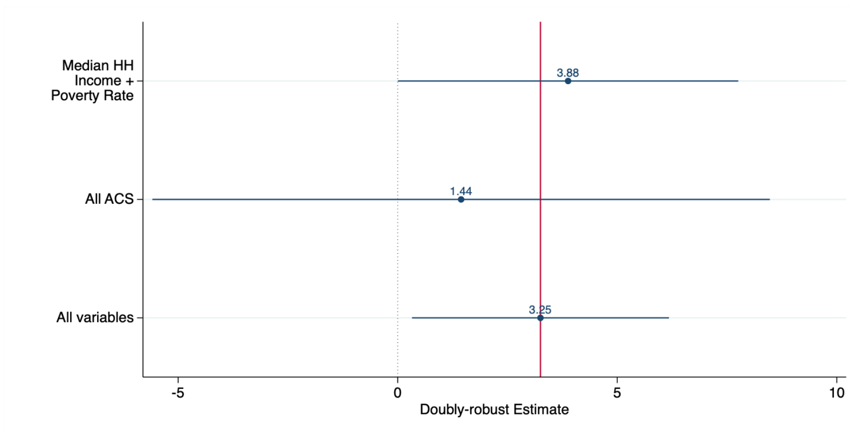
(b) Sample: 18 cities



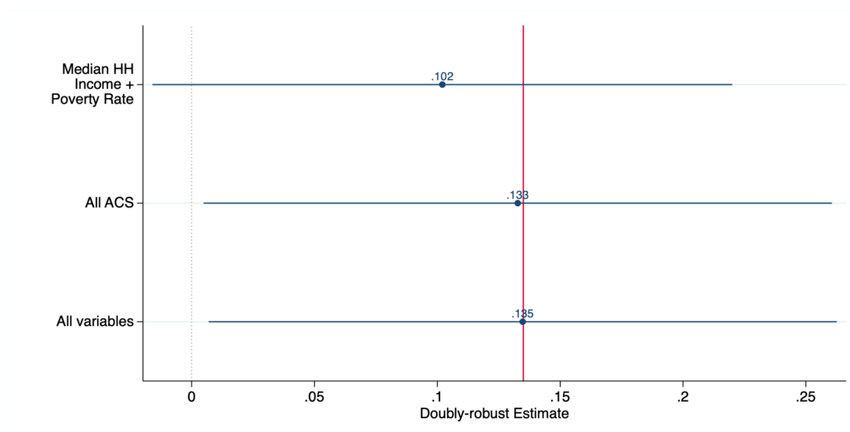
Notes: This figure displays doubly robust estimates for the effect of QCT status on the stock of LIHTC unit in a tract, in which we iteratively exclude one of the top ten most weighted tracts in our sample. The red line indicates the original doubly robust estimate when no tract is excluded. Panel (a) uses all QCT-eligible tracts in the US metropolitan areas for estimation, while panel (b) uses the same sample in the main paper (i.e. all QCT-eligible tracts in 18 cities).

Figure B.5: Sensitivity to matching variables

(a) Stock of LIHTC units



(b) Demeaned arsinh(Hour)



(c) Demeaned Violent Crimes per 1,000 Jobs

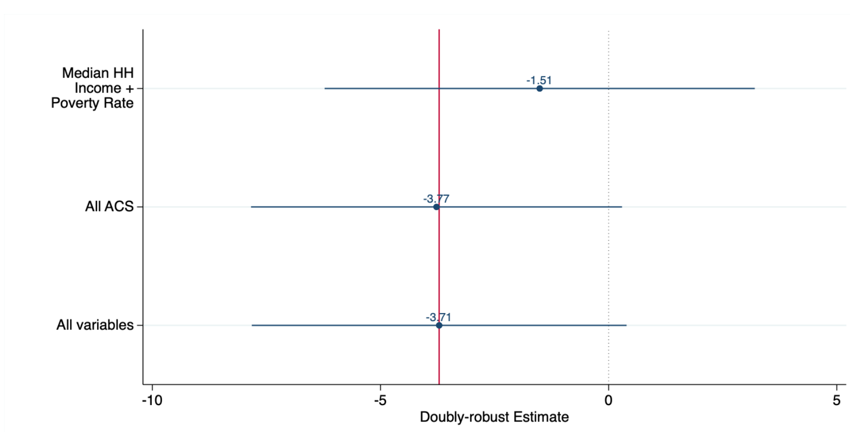


Table B.1: QCTs and non-selected tracts in 18 cities

	Num. QCTs	Num. of eligible, non-QCT tracts	Poverty Rate (QCTs)	Poverty Rate (non-QCT tracts)	Population Cap Binding in CBSA
Austin	59	0	.361		1
Charlotte	60	8	.344	.234	1
Chicago	416	8	.342	.194	1
Dallas	146	8	.337	.254	1
Denver	53	0	.302		0
Detroit	247	17	.431	.323	1
Fort Worth	65	5	.36	.21	1
Houston	198	31	.352	.226	1
Los Angeles	373	147	.354	.219	1
Nashville	57	0	.357		1
New York City	661	267	.343	.191	1
Philadelphia	230	8	.351	.149	1
Phoenix	136	13	.406	.24	1
San Antonio	109	8	.345	.234	1
San Diego	67	7	.327	.143	1
San Francisco	56	4	.237	.083	1
Seattle	25	0	.338		0
Washington	78	0	.303		0

Table B.2: Number of QCTs in the top and bottom tertile of neighborhood characteristics

City	Num. QCTs with % Black		Num. QCTs with % Rent HU		Num. QCTs with % Recently Built HU		Num. QCTs with % Single HU		Num. Eligible
	Top tertile	Bottom tertile	Top tertile	Bottom tertile	Top tertile	Bottom tertile	Top tertile	Bottom tertile	Non-QCT tracts
Austin	32	10	36	7	15	24	6	34	0
Charlotte	35	3	44	2	19	34	2	34	8
Chicago	225	76	215	67	50	366	98	148	8
Dallas	74	27	60	32	29	104	48	55	8
Denver	26	8	26	3	14	35	12	20	0
Detroit	83	75	87	74	10	237	75	84	17
FortWorth	32	20	29	5	12	41	23	23	5
Houston	91	47	99	26	35	149	47	90	31
LosAngeles	171	98	213	22	68	305	67	168	147
Nashville	30	8	36	0	19	27	4	31	0
NewYorkCity	328	117	474	4	118	543	32	357	267
Philadelphia	107	34	100	42	53	177	86	68	8
Phoenix	63	23	84	7	32	103	21	69	13
SanAntonio	33	53	47	13	19	84	31	38	8
SanDiego	34	14	46	1	13	54	8	40	7
SanFrancisco	35	8	35	14	10	46	18	27	4
Seattle	16	1	20	0	10	6	1	17	0
Washington	50	6	42	8	29	46	12	31	0
Total	1465	628	1693	327	555	2381	591	1334	531

Table B.3: Change in demeaned hour and crime by QCT status

	QCT	Eligible non-QCT tracts	
	Weight = 1	Unweighted	Weighted
Δ Demeaned arsinh Hour	.012	.0097	-.123
Δ Demeaned Violent Crime Per 1,000 Jobs	-.434	-.546	3.280
Mean. Fraction of Adjacent Tracts are QCTs		0.354	0.524

Notes: This table shows the change in demeaned arsinh police hour and violent crime per 1,000 jobs for QCT and eligible but non-selected tracts (both unweighted and weighted under the doubly robust estimator). The final line reports both the unweighted mean and weighted mean of the fraction of adjacent tracts that are QCTs for the non-selected tracts.

Table B.4: Effect of QCT status on tract composition and street traffic

	DV: Residence Area Characteristics (Demeaned)										
	(1) % White	(2) % Black	(3) % Asian	(4) % Hispanic	(5) % Less HS	(6) % College	(7) N. Jobs	(8) Jobs (E<1250)	(9) Jobs (E:1251-3333)	(10) Jobs (E>3333)	(11) Real Estable Jobs
Panel A: DID estimator											
2019 X QCT	0.002	-0.002	-0.000	-0.001	-0.001	0.003	0.005	-0.009	0.015	0.035	0.007
	[0.000,0.005]	[-0.004,-0.000]	[-0.001,0.001]	[-0.002,0.001]	[-0.003,0.000]	[0.001,0.005]	[-0.001,0.011]	[-0.021,0.002]	[0.006,0.023]	[0.027,0.043]	[-0.020,0.033]
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.006)	(0.004)	(0.004)	(0.014)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics											
2019 X QCT	0.004	-0.007	0.002	-0.004	-0.002	0.007	0.007	-0.006	-0.002	0.059	0.006
	[0.000,0.008]	[-0.012,-0.002]	[0.001,0.004]	[-0.007,0.000]	[-0.006,0.001]	[0.003,0.011]	[-0.004,0.019]	[-0.030,0.017]	[-0.017,0.013]	[0.034,0.084]	[-0.028,0.040]
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.006)	(0.012)	(0.008)	(0.013)	(0.017)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. Outcome variables in column 7-11 are first log-transformed, then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.5: Effect of QCT status on police activities

	DV: Police Activities and Characteristics (Demeaned)								
	(1) Hour	(2) Hour: Day Time	(3) Hour: Night Time	(4) Hour: Weekday	(5) Hour: Weekend	(6) arsinh(Officer)	(7) arsinh(Shifts)	(8) Frac. Days with Police Presence	(9) Diff. in Officer and Resident Race
Panel A: DID estimator									
2019 X QCT	0.002	0.009	-0.026	-0.016	0.021	-0.023	0.015	-0.004	-0.018
	[-0.084,0.087]	[-0.082,0.100]	[-0.129,0.077]	[-0.101,0.070]	[-0.079,0.122]	[-0.056,0.010]	[-0.052,0.082]	[-0.018,0.010]	[-0.048,0.012]
	(0.044)	(0.046)	(0.053)	(0.044)	(0.051)	(0.017)	(0.034)	(0.007)	(0.015)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7114
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics									
2019 X QCT	0.135	0.062	0.260	0.113	0.190	-0.010	0.102	0.026	-0.023
	[0.007,0.263]	[-0.066,0.189]	[0.106,0.414]	[-0.011,0.237]	[0.036,0.343]	[-0.060,0.041]	[0.001,0.202]	[0.003,0.049]	[-0.086,0.040]
	(0.065)	(0.065)	(0.078)	(0.063)	(0.078)	(0.026)	(0.051)	(0.012)	(0.032)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7114

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.6: Effect of QCT status on police movement characteristics

	DV: Police Movement Characteristics (Demeaned)			
	(1)	(2)	(3)	(4)
	arsinh(Hour: Short Visit)	arsinh(Hour: Long Visit)	Mean Speed	Wgt. Mean Speed
Panel A: DID estimator				
2019 X QCT	0.010	-0.032	0.395	0.133
	[-0.067,0.086]	[-0.136,0.072]	[0.037,0.754]	[-0.104,0.371]
	(0.039)	(0.053)	(0.183)	(0.121)
Observations	7120	7120	7114	7114
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics				
2019 X QCT	0.155	0.092	1.443	0.086
	[0.036,0.273]	[-0.054,0.238]	[0.743,2.142]	[-0.228,0.401]
	(0.061)	(0.074)	(0.357)	(0.160)
Observations	7120	7120	7114	7114

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.7: Property-level analysis: "ring" difference-in-differences

	Outcome: Δ asinh(Hour)								
	QCT			Dropped QCTs			Ineligible		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Within 250m of 2018 site	-0.014			-0.458			0.002		
	[-0.071,0.043]			[-0.744,-0.172]			[-0.085,0.089]		
	(0.029)			(0.139)			(0.044)		
Within 0.25 mi of 2018 site		-0.015			0.005			-0.037	
		[-0.065,0.036]			[-0.201,0.211]			[-0.104,0.030]	
		(0.026)			(0.103)			(0.034)	
Within half mi of 2018 site			-0.048			-0.094			-0.068
			[-0.086,-0.010]			[-0.208,0.021]			[-0.108,-0.027]
			(0.019)			(0.058)			(0.021)
Observations	2622	8198	20264	180	573	2142	1092	4290	15303
R^2	0.447	0.327	0.252	0.356	0.349	0.254	0.413	0.335	0.244
Fixed Effects	LIHTC site	LIHTC site	LIHTC site	LIHTC site	LIHTC site	LIHTC site	LIHTC site	LIHTC site	LIHTC site
Control Ring	400m	Half Mi	1 Mi	400m	Half Mi	1 Mi	400m	Half Mi	1 Mi

Notes: The unit of observation is a census block. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.8: Alternative measures of police presence

	DV: Demeaned arsinh(Police Hour)				
	(1) Use 8-12 Hour shifts	(2) Exclude pings below 25 mph	(3) Home-Home interval <= $18h$	(4) Remove shifts in PD >= $3h$	(5) Remove shifts from HQ
Panel A: DID estimator					
2019 X QCT	0.007	-0.002	-0.011	-0.006	0.006
	[-0.091,0.104]	[-0.089,0.085]	[-0.098,0.075]	[-0.098,0.086]	[-0.082,0.094]
	(0.050)	(0.044)	(0.044)	(0.047)	(0.045)
Observations	7120	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics					
2019 X QCT	0.156	0.133	0.134	0.116	0.119
	[0.019,0.293]	[0.007,0.260]	[-0.002,0.269]	[-0.030,0.262]	[-0.008,0.246]
	(0.070)	(0.065)	(0.069)	(0.075)	(0.065)
Observations	7120	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. Outcome variables in column 7-11 are first log-transformed, then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.9: Log-transformation of police hours

	DV: Demeaned log(Police Hour), dropped NA values						
	(1) dm_log_hour_in_tr	(2) dm_log_hour_day	(3) dm_log_hour_nite	(4) dm_log_hour_wkdy	(5) dm_log_hour_wknd	(6) dm_log_N_officers_in_tr	(7) dm_log_N_shifts_in_tr
Panel A: DID estimator							
2019 X QCT	0.007	0.018	-0.009	-0.013	-0.018	-0.025	0.012
	[-0.081,0.096]	[-0.079,0.115]	[-0.132,0.114]	[-0.102,0.077]	[-0.149,0.113]	[-0.057,0.008]	[-0.055,0.079]
	(0.045)	(0.049)	(0.063)	(0.046)	(0.067)	(0.017)	(0.034)
Observations	7114	7112	7050	7114	6988	7114	7114
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics							
2019 X QCT	0.139	0.065	0.309	0.116	0.210	-0.008	0.104
	[0.006,0.272]	[-0.072,0.201]	[0.117,0.501]	[-0.015,0.246]	[0.007,0.413]	[-0.058,0.042]	[0.003,0.204]
	(0.068)	(0.070)	(0.098)	(0.067)	(0.104)	(0.026)	(0.051)
Observations	7114	7112	7050	7114	6988	7114	7114

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. Outcome variables in column 7-11 are first log-transformed, then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.10: Comparing change in actual police presence in 2019 with the synthetic police presence in 2017

	DV: arsinh(Police Activities)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Hour	Hour: Day Time	Hour: Night Time	Hour: Weekday	Hour: Weekend	Officer	Shifts
Panel A: DID estimator							
2019 X QCT	-0.039	-0.038	-0.071	-0.049	-0.048	-0.024	-0.002
	[-0.113,0.035]	[-0.123,0.047]	[-0.164,0.022]	[-0.125,0.027]	[-0.141,0.045]	[-0.056,0.008]	[-0.059,0.055]
	(0.038)	(0.043)	(0.047)	(0.039)	(0.047)	(0.016)	(0.029)
Observations	7120	7120	7120	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics							
2019 X QCT	0.085	0.001	0.211	0.062	0.134	-0.000	0.077
	[-0.049,0.219]	[-0.160,0.163]	[0.070,0.352]	[-0.079,0.202]	[-0.005,0.273]	[-0.050,0.050]	[-0.009,0.163]
	(0.068)	(0.082)	(0.072)	(0.072)	(0.071)	(0.026)	(0.044)
Observations	7120	7120	7120	7120	7120	7120	7120

Notes: This table compares change in actual police presence in 2019 with the synthetic police presence in 2017, where we resample pings during shifts from each police stations in 2017 with replacement, such that the number of pings from each station in 2017 matches those number in 2019. The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. Outcome variables in column 7-11 are first log-transformed, then demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age <18, % age >65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.11: Effect of QCT status on policing, outcomes demeaned by city-high poverty tracts-year

	DV: arsinh(Police Activities), demeaned by city-high poverty tracts-year								
	(1) Hour	(2) Hour: Day Time	(3) Hour: Night Time	(4) Hour: Weekday	(5) Hour: Weekend	(6) Officer	(7) Shifts	(8) Frac. Days with Police Presence	(9) Diff. in Officer and Resident Race
Panel A: DID estimator									
2019 X QCT	-0.010	-0.007	-0.015	-0.025	0.026	-0.025	-0.004	-0.000	-0.025
	[-0.095,0.076]	[-0.098,0.084]	[-0.118,0.088]	[-0.111,0.060]	[-0.074,0.127]	[-0.058,0.008]	[-0.070,0.063]	[-0.015,0.014]	[-0.055,0.005]
	(0.044)	(0.046)	(0.053)	(0.044)	(0.051)	(0.017)	(0.034)	(0.007)	(0.015)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7114
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics									
2019 X QCT	0.093	0.012	0.246	0.070	0.166	-0.016	0.058	0.021	-0.030
	[-0.033,0.219]	[-0.114,0.137]	[0.092,0.399]	[-0.053,0.193]	[0.013,0.319]	[-0.065,0.034]	[-0.040,0.157]	[-0.001,0.044]	[-0.095,0.034]
	(0.064)	(0.064)	(0.078)	(0.063)	(0.078)	(0.025)	(0.050)	(0.012)	(0.033)
Observations	7120	7120	7120	7120	7120	7120	7120	7120	7114

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.12: Effect of QCT status on policing, excluding cities without binding population caps

	DV: Demeaned arsinh(Police Activities)								
	(1) Hour	(2) Hour: Day Time	(3) Hour: Night Time	(4) Hour: Weekday	(5) Hour: Weekend	(6) arsinh(Officer)	(7) arsinh(Shifts)	(8) Frac. Days with Police Presence	(9) Diff. in Officer and Resident Race
Panel A: DID estimator									
2019 X QCT	-0.005	-0.008	-0.016	-0.023	0.018	-0.025	0.008	-0.005	-0.018
	[-0.092,0.081]	[-0.099,0.084]	[-0.120,0.088]	[-0.109,0.064]	[-0.083,0.119]	[-0.058,0.008]	[-0.060,0.075]	[-0.020,0.009]	[-0.048,0.013]
	(0.044)	(0.047)	(0.053)	(0.044)	(0.052)	(0.017)	(0.034)	(0.007)	(0.015)
Observations	6576	6576	6576	6576	6576	6576	6576	6576	6572
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics									
2019 X QCT	0.134	0.049	0.294	0.112	0.200	-0.011	0.097	0.028	-0.017
	[-0.001,0.269]	[-0.086,0.184]	[0.133,0.454]	[-0.019,0.243]	[0.040,0.359]	[-0.065,0.044]	[-0.008,0.202]	[0.002,0.053]	[-0.082,0.047]
	(0.069)	(0.069)	(0.082)	(0.067)	(0.081)	(0.028)	(0.054)	(0.013)	(0.033)
Observations	6576	6576	6576	6576	6576	6576	6576	6576	6572

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.13: Other estimators provided by DRDID

	(1) Stock of LIHTC unit	(2) Demeaned IHS Hour	(3) Demeaned violent rates	(4) Demeaned property rates
dripw	3.299	0.409	-7.205	-11.391
	[-3.049,9.647]	[-0.275,1.092]	[-21.426,7.016]	[-32.629,9.847]
	(3.239)	(0.349)	(7.256)	(10.836)
drimp	3.251	0.135	-3.715	-2.036
	[0.325,6.177]	[0.007,0.263]	[-7.822,0.393]	[-6.746,2.675]
	(1.493)	(0.065)	(2.096)	(2.403)
reg	2.912	0.084	-0.161	2.561
	[-0.573,6.398]	[-0.073,0.242]	[-3.530,3.207]	[-2.473,7.595]
	(1.778)	(0.080)	(1.719)	(2.569)
ipw	3.930	0.753	-25.539	-100.839
	[1.206,6.654]	[-0.818,2.323]	[-75.062,23.984]	[-285.039,83.361]
	(1.390)	(0.802)	(25.267)	(93.981)
stdipw	5.924	0.244	-8.311	-31.216
	[3.695,8.153]	[0.067,0.421]	[-24.821,8.199]	[-44.687,-17.745]
	(1.137)	(0.090)	(8.424)	(6.873)
sipwra	3.224	0.125	-2.666	-2.298
	[0.157,6.291]	[-0.011,0.261]	[.,.]	[-7.243,2.648]
	(1.565)	(0.069)	(.)	(2.523)
Observations				

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates used for matching include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. "drimp" denotes Sant'Anna and Zhao (2020)'s improved doubly robust DiD estimator based on inverse probability of tilting and weighted least squares and is the estimator used in the main paper; "dripw" represents Sant'Anna and Zhao (2020)'s doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares; "reg" stands for the outcome regression DiD estimator; "stdipw" stands for the inverse probability weighting DiD estimator with stabilized weights; "ipw" refers to the inverse probability weighting DiD estimator as in Abadie (2005); "sipwra" refers to inverse-probability-weighted regression adjustment. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.14: Effect of QCT status on crime per 1000 jobs

	DV: Crime Per 1000 Jobs (Demeaned)							
	(1) Violent Crimes	(2) Robberies	(3) Aggravated Assaults	(4) Homicides	(5) Property Crimes	(6) Burglaries	(7) Thefts	(8) Motor Vehicle Thefts
Panel A: DID estimator								
2019 X QCT	0.092	-0.067	0.168	0.004	0.653	0.229	0.282	0.143
	[-0.700,0.885]	[-0.488,0.355]	[-0.399,0.734]	[-0.080,0.088]	[-1.526,2.832]	[-0.455,0.912]	[-1.433,1.996]	[-0.513,0.799]
	(0.404)	(0.215)	(0.289)	(0.043)	(1.112)	(0.349)	(0.874)	(0.335)
Observations	7120	7120	7120	7000	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics								
2019 X QCT	-3.715	-1.122	-1.701	-0.614	-2.036	2.748	-4.453	-0.331
	[-7.822,0.393]	[-2.257,0.013]	[-4.261,0.858]	[-1.192,-0.037]	[-6.746,2.675]	[1.259,4.237]	[-8.402,-0.504]	[-1.692,1.030]
	(2.096)	(0.579)	(1.306)	(0.295)	(2.403)	(0.760)	(2.015)	(0.694)
Observations	7120	7120	7120	7000	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The dependent variables are demeaned by city-year. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.15: Effect of QCT status on crime per 1000 jobs, outcomes demeaned by city-high poverty tracts-year

	DV: Crime Per 1000 Jobs (Demeaned by city-high poverty tracts-year)							
	(1) Violent Crimes	(2) Robberies	(3) Aggravated Assaults	(4) Homicides	(5) Property Crimes	(6) Burglaries	(7) Thefts	(8) Motor Vehicle Thefts
Panel A: DID estimator								
2019 X QCT	0.510	0.301	0.197	0.003	1.042	0.488	0.415	0.139
	[-0.284,1.304]	[-0.122,0.723]	[-0.370,0.764]	[-0.080,0.086]	[-1.163,3.246]	[-0.195,1.171]	[-1.319,2.149]	[-0.521,0.798]
	(0.405)	(0.215)	(0.289)	(0.042)	(1.124)	(0.348)	(0.884)	(0.336)
Observations	7120	7120	7120	7000	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics								
2019 X QCT	-3.542	-1.021	-1.680	-0.602	-1.450	3.004	-4.030	-0.424
	[-7.642,0.558]	[-2.152,0.109]	[-4.244,0.884]	[-1.176,-0.027]	[-6.181,3.280]	[1.519,4.488]	[-7.947,-0.113]	[-1.818,0.970]
	(2.092)	(0.577)	(1.308)	(0.293)	(2.414)	(0.757)	(1.999)	(0.711)
Observations	7120	7120	7120	7000	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age < 18, % age > 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.16: Effect of QCT status on crime per 1000 residents (ACS estimates of population as denominator)

	DV: Crime Per 1000 Residents (ACS, demeaned)							
	(1) Violent Crimes	(2) Robberies	(3) Aggravated Assaults	(4) Homicides	(5) Property Crimes	(6) Burglaries	(7) Thefts	(8) Motor Vehicle Thefts
Panel A: DID estimator								
2019 X QCT	-2.082	-0.907	-1.179	0.124	-11.870	-0.342	-10.193	-1.335
	[-9.926,5.763]	[-5.190,3.375]	[-4.754,2.396]	[0.092,0.156]	[-41.564,17.825]	[-1.220,0.536]	[-36.476,16.090]	[-4.134,1.464]
	(4.001)	(2.184)	(1.823)	(0.016)	(15.145)	(0.448)	(13.405)	(1.428)
Observations	7120	7120	7120	7000	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics								
2019 X QCT	-1.224	-0.274	-0.578	-0.142	-1.963	0.784	-2.816	0.069
	[-2.440,-0.009]	[-0.721,0.173]	[-1.244,0.088]	[-0.319,0.035]	[-4.834,0.908]	[0.243,1.325]	[-5.456,-0.177]	[-0.414,0.552]
	(0.620)	(0.228)	(0.340)	(0.090)	(1.465)	(0.276)	(1.347)	(0.246)
Observations	7120	7120	7120	7000	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.17: Effect of QCT status on crime counts

	DV: Demeaned arsinh(Crime Count)							
	(1) Violent Crimes	(2) Robberies	(3) Aggravated Assaults	(4) Homicides	(5) Property Crimes	(6) Burglaries	(7) Thefts	(8) Motor Vehicle Thefts
Panel A: DID estimator								
2019 X QCT	0.059	0.055	0.057	0.023	0.034	0.084	0.017	0.080
	[0.015,0.104]	[-0.010,0.120]	[0.000,0.115]	[-0.029,0.076]	[0.004,0.065]	[0.017,0.150]	[-0.020,0.054]	[0.012,0.148]
	(0.023)	(0.033)	(0.029)	(0.027)	(0.016)	(0.034)	(0.019)	(0.035)
Observations	7120	7120	7120	7000	7120	7120	7120	7120
Panel B: Doubly-robust DID estimator, matching on demographic and housing characteristics								
2019 X QCT	-0.004	0.013	-0.010	-0.066	0.005	0.132	-0.044	0.016
	[-0.059,0.050]	[-0.105,0.130]	[-0.078,0.058]	[-0.204,0.071]	[-0.042,0.052]	[0.034,0.230]	[-0.106,0.018]	[-0.067,0.099]
	(0.028)	(0.060)	(0.035)	(0.070)	(0.024)	(0.050)	(0.032)	(0.042)
Observations	7120	7120	7120	7000	7120	7120	7120	7120

Notes: The unit of observation is a tract-year. Each tract has one observation in 2017 (pre-period) and in 2019 (post-period), respectively. The covariates in panel B include median household income, poverty rate, log population, log housing units; share units owner occupied, share units occupied, % College, % Black, % Hispanic, % age $<$ 18, % age $>$ 65 from 2013-2017 ACS, and the number of LIHTC units placed in service between 2015 and 2017. Robust standard errors clustered at the tract level are reported in parentheses, 95% confidence intervals are reported in the square brackets.

Table B.18: Correlation coefficients (ρ) between actual and predicted arsinh(Hour)

City	100 m	200m	300m
Austin	0.644	0.656	0.686
Charlotte	0.544	0.584	0.607
Chicago	0.482	0.544	0.576
Dallas	0.501	0.566	0.553
Denver	0.502	0.578	0.621
Detroit	0.517	0.500	0.477
Fort Worth	0.527	0.585	0.619
Houston	0.445	0.500	0.517
Los Angeles	0.507	0.535	0.530
Nashville	0.504	0.609	0.647
New York City	0.554	0.611	0.621
Philadelphia	0.400	0.489	0.528
Phoenix	0.269	0.379	0.484
San Antonio	0.539	0.634	0.666
San Diego	0.449	0.577	0.614
San Francisco	0.311	0.390	0.394
Seattle	0.524	0.603	0.619
Washington	0.519	0.606	0.626

Table B.19: Correlation coefficients (ρ) between actual and predicted crime indices

City	100 m	200m	300m
Austin	0.371	0.398	0.354
Charlotte	0.421	0.411	0.485
Chicago	0.630	0.601	0.609
Dallas	0.444	0.525	0.610
Denver	0.519	0.744	0.784
Detroit	0.628	0.468	0.497
Fort Worth	0.428	0.651	0.618
Houston	0.578	0.644	0.648
Los Angeles	0.585	0.602	0.636
Nashville	0.451	0.689	0.590
New York City	0.741	0.775	0.792
Philadelphia	0.744	0.784	0.809
Phoenix	0.236	0.630	0.671
San Antonio	0.329	0.495	0.437
San Francisco	0.804	0.711	0.745
Seattle	0.809	0.806	0.844
Washington	0.509	0.599	0.554

Table B.20: Predictive Power on Actual Crime Index, Predicted Values vs. Demographics

	Std Crime Index, 100m			Std Crime Index, 200m			Std Crime Index, 300m		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted Std Crime Index, 100 m	1.171***		1.153***						
	(0.014)		(0.015)						
Log Population (ACS 15-19)		-0.124***	-0.082***		-0.182***	-0.101***		-0.178***	-0.083***
		(0.021)	(0.015)		(0.020)	(0.013)		(0.020)	(0.013)
Log housing units (ACS 15-19)		0.107***	0.098***		0.145***	0.119***		0.137***	0.108***
		(0.022)	(0.015)		(0.021)	(0.014)		(0.021)	(0.014)
Median HH Income (1K, 15-19 ACS)		0.001***	-0.001***		0.002***	-0.002***		0.002***	-0.002***
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
% College (ACS 15-19)		0.056*	0.053**		0.015	0.010		0.051	0.028
		(0.029)	(0.022)		(0.032)	(0.022)		(0.032)	(0.022)
Census Return Rate 2010		-0.385***	0.104**		-0.385***	-0.025		-0.442***	0.041
		(0.072)	(0.049)		(0.069)	(0.044)		(0.068)	(0.041)
Share recent built units (15-19 ACS)		-0.211***	0.151**		-0.339***	0.282***		-0.354***	0.238***
		(0.075)	(0.062)		(0.066)	(0.050)		(0.065)	(0.047)
Share units owner occupied (ACS 15-19)		-0.743***	-0.115***		-1.000***	-0.172***		-1.100***	-0.207***
		(0.026)	(0.017)		(0.025)	(0.016)		(0.025)	(0.015)
Share units occupied (ACS 15-19)		0.210***	-0.010		0.108**	-0.087**		0.085	-0.173***
		(0.053)	(0.039)		(0.051)	(0.036)		(0.054)	(0.038)
Share Age < 5 (ACS 15-19)		-0.608***	-0.139*		-0.802***	-0.231***		-1.040***	-0.427***
		(0.097)	(0.072)		(0.099)	(0.069)		(0.098)	(0.065)
Share Age Between 5 and 17 (ACS 15-19)		-0.430***	-0.059		-0.563***	-0.058		-0.678***	-0.131***
		(0.057)	(0.041)		(0.059)	(0.040)		(0.059)	(0.039)
Share Age > 65 (ACS 15-19)		0.005	-0.330***		-0.103*	-0.456***		-0.100*	-0.522***
		(0.059)	(0.043)		(0.057)	(0.040)		(0.058)	(0.039)
% Hispanic (ACS 15-19)		-0.155***	0.301***		-0.236***	0.426***		-0.257***	0.453***
		(0.038)	(0.025)		(0.046)	(0.028)		(0.046)	(0.028)
% White (ACS 15-19)		-0.312***	-0.034		-0.406***	0.020		-0.456***	-0.006
		(0.044)	(0.031)		(0.050)	(0.033)		(0.050)	(0.032)
% Black (ACS 15-19)		0.180***	0.252***		0.282***	0.317***		0.298***	0.341***
		(0.048)	(0.032)		(0.054)	(0.035)		(0.054)	(0.034)
% Other Languages (15-19 ACS)		0.291***	-0.123***		0.431***	-0.219***		0.470***	-0.249***
		(0.029)	(0.022)		(0.029)	(0.022)		(0.029)	(0.021)
Predicted Std Crime Index, 200 m				1.149***		1.119***			
				(0.014)		(0.015)			
Predicted Std Crime Index, 300 m							1.118***		1.089***
							(0.011)		(0.013)
Constant	0.034***	0.696***	0.035	0.060***	1.105***	0.275***	0.055***	1.249***	0.309***
	(0.003)	(0.081)	(0.058)	(0.003)	(0.084)	(0.056)	(0.003)	(0.084)	(0.054)
Observations	60859	57548	57548	60859	57548	57548	60859	57548	57548
R^2	0.518	0.086	0.518	0.561	0.153	0.592	0.602	0.178	0.644

Table B.21: Predictive Power on arsinh(Hour), Predicted Values vs. Demographics

	arsinh(Hour), 100m			arsinh(Hour), 200m			arsinh(Hour), 300m		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted ihs(Hour), 100 m	1.222***		1.163***						
	(0.011)		(0.013)						
Log Population (ACS 15-19)		-0.010	-0.022		-0.011	-0.036**		-0.030	-0.056***
		(0.015)	(0.014)		(0.023)	(0.018)		(0.027)	(0.021)
Log housing units (ACS 15-19)		-0.029*	0.017		-0.130***	0.020		-0.200***	0.041**
		(0.015)	(0.014)		(0.024)	(0.018)		(0.028)	(0.021)
Median HH Income (1K, 15-19 ACS)		0.001***	-0.000		0.002***	0.000		0.002***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
% College (ACS 15-19)		0.299***	0.025		0.670***	0.005		0.929***	0.104***
		(0.022)	(0.019)		(0.036)	(0.028)		(0.043)	(0.033)
Census Return Rate 2010		-0.446***	-0.011		-1.140***	0.185***		-1.492***	-0.027
		(0.045)	(0.038)		(0.072)	(0.056)		(0.087)	(0.065)
Share recent built units (15-19 ACS)		0.089*	0.096**		0.068	0.146***		0.098	0.322***
		(0.047)	(0.039)		(0.071)	(0.055)		(0.084)	(0.064)
Share units owner occupied (ACS 15-19)		-0.599***	-0.150***		-1.162***	-0.398***		-1.463***	-0.439***
		(0.016)	(0.013)		(0.024)	(0.019)		(0.029)	(0.022)
Share units occupied (ACS 15-19)		-0.029	0.075**		-0.165***	0.024		-0.261***	0.114**
		(0.036)	(0.031)		(0.056)	(0.043)		(0.068)	(0.050)
Share Age $<$ 5 (ACS 15-19)		-0.679***	-0.255***		-1.344***	-0.499***		-1.813***	-0.582***
		(0.066)	(0.056)		(0.103)	(0.081)		(0.124)	(0.094)
Share Age Between 5 and 17 (ACS 15-19)		-0.840***	-0.269***		-1.679***	-0.492***		-2.313***	-0.696***
		(0.041)	(0.035)		(0.064)	(0.051)		(0.077)	(0.059)
Share Age $>$ 65 (ACS 15-19)		-0.010	-0.148***		0.048	-0.258***		0.000	-0.408***
		(0.037)	(0.032)		(0.058)	(0.046)		(0.069)	(0.054)
% Hispanic (ACS 15-19)		-0.004	0.084***		-0.035	-0.046		-0.075*	0.027
		(0.025)	(0.020)		(0.038)	(0.029)		(0.045)	(0.034)
% White (ACS 15-19)		-0.079***	-0.132***		-0.170***	-0.356***		-0.286***	-0.391***
		(0.030)	(0.025)		(0.047)	(0.036)		(0.057)	(0.043)
% Black (ACS 15-19)		0.279***	-0.055**		0.600***	-0.191***		0.756***	-0.251***
		(0.031)	(0.025)		(0.048)	(0.037)		(0.057)	(0.043)
% Other Languages (15-19 ACS)		0.260***	-0.163***		0.610***	-0.208***		0.819***	-0.264***
		(0.022)	(0.019)		(0.035)	(0.028)		(0.043)	(0.033)
Predicted ihs(Hour), 200 m				1.191***		1.106***			
				(0.006)		(0.007)			
Predicted ihs(Hour), 300 m							1.172***		1.086***
							(0.005)		(0.006)
Constant	-0.222***	2.039***	0.082	-0.262***	4.020***	0.456***	-0.300***	5.607***	0.693***
	(0.013)	(0.057)	(0.052)	(0.010)	(0.089)	(0.072)	(0.010)	(0.108)	(0.085)
Observations	63615	60231	60231	63615	60231	60231	63615	60231	60231
R^2	0.364	0.120	0.374	0.472	0.190	0.489	0.510	0.221	0.532

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