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

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Abstract

Extensive research finds that place-based investment reduces crime. This has lead practitioners to propose investments as an alternative to police-centered crime policies. Using smartphone location data, we explore one channel linking local investment to crime—that policing is endogenous to the built environment. Exploiting quasi-experimental variation in HUD rules designating Qualified Census Tracts (QCTs), we find place-based investments also increase police presence. These increases can more than explain observed investment-induced violent crime reductions. Police increase patrols more in neighborhoods with more Black residents and older housing. Training deep learning models on Google Street View images, we find that QCT-induced reductions in physical disorder explain very little of the observed increases in policing or reductions in crime—failing to support broken windows theories of crime reduction. Our findings highlight the importance of understanding law-enforcement responses to local development before framing economic investments as a substitute for policing.

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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 appears 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, 2015; 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., 2022a) and more foot traffic can deter potential offenders (Jacobs, 1961; Branas et al., 2018; Farrington and Welsh, 2002). Additionally, 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. Di Tella and Schargrodsky 2004; Braga and Bond 2008; Braga et al. 2019; Weisburst 2019b; 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 the 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 the 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 crime-deterring activities by police (Branas et al. 2018). To the extent that reductions in physical disorder lower supervisors' 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 with reduced 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 the 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.

[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).

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 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 American Community Survey (ACS) demographic and housing characteristics (including median household income and a poverty rate that determines a tract's eligibility), 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 conditions, 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 11 cities with geocoded 311 call data, QCT-spurred development reduced 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 the 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 significantly enhance perceptions of safety, beauty, and wealth in QCTs compared to similar non-QCTs. Additionally, 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 the 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 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

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

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 caps, census tracts with income and poverty rates that would qualify them for QCT status are not designated as such. Figure 1 plots the distribution of the income and poverty criteria between the QCTs and eligible but non-selected tracts. There is a 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 in the same time period, but never selected ("eligible but non-selected").[3] Appendix Table A1 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]

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

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 1 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

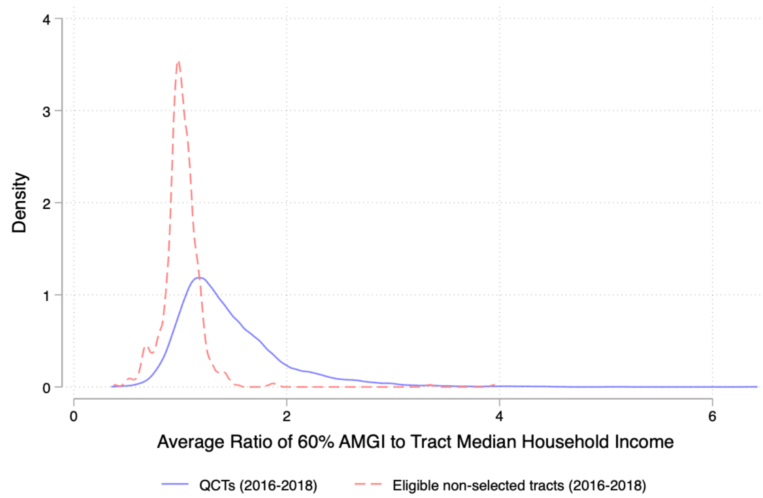
[3]Despite possible other empirical strategies to studies the impact of LIHTC housing (e.g. Baum-Snow and Marion 2009; Schwartz et al. 2006; Diamond and McQuade 2019), exploiting the population cap of QCT is best suited to evaluate this research question. Exploiting the discontinuity in HUD's QCT designation formula does not leave us enough statistical power given our focus on 18 cities. We discuss results for property-level analysis in Section A5 in the Appendix. Still, we argue that property-level analysis might not be most appropriate in our setting, as the location of new LIHTC housing can be endogenous to the existing stock of LIHTC properties.

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

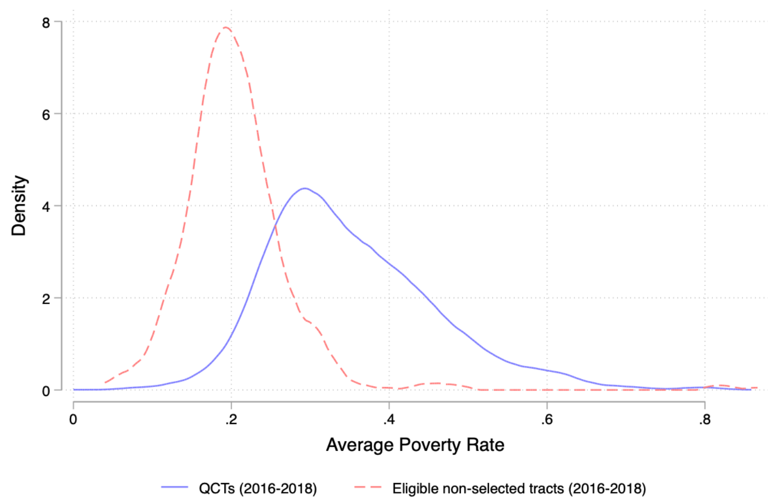
college-educated residents, and a lower share of occupied housing units.

Figure 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 eligible but non-selected tracts. Both measures are averaged from 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 reweigh eligible non-selected tracts based on a set of covariates, and an 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 1 as well as the number of LIHTC units placed in service between 2015 and 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 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 4, we examine the relationship between QCT-spurred investments and differential changes in neighborhood outcomes, specifically policing and crime relative to broader city trends.

[5]In section A6 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.

Table 1: Summary Statistics

	(1) QCTs		(2) Eligible, non-selected tracts		(3) 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	

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 a 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 a 5-day presence and is moving 50 mph or less. We identify 8,136 and 6,577 patrol officers having at least one "shift" in the 18 cities in 2017 and 2019, respectively. We match likely officers' pings during patrol to census tracts, 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, and we present some results in Appendix A2. Panel D of Table 1 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 A1 provides detailed explanations of these data sources.

[7] In Appendix A6, 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.

4 Results

4.1 QCT Status and Change in Neighborhood Environment

We start our analysis in Table 2 by demonstrating that our approach can replicate existing findings on the positive impact of QCT-spurred investment on both the physical and social environment of neighborhoods (Baum-Snow and Marion, 2009; Freedman and McGavock, 2015; Ellen et al., 2016).

Columns 1 and 2 demonstrate that QCT-spurred investment improves infrastructure investment and the 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 the 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 A4 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] 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

[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.

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: Effect of QCT status on neighborhood physical and socioeconomic environment

	Physical Disorder		Socioeconomic Environment	
	(1) Stock of LIHTC units	(2) Street Light Repair Request	(3) Jobs (E>3333)	(4) 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, and 95% confidence intervals are reported in the square brackets.

4.2 Do police respond to QCT status?

4.2.1 Policing Time

Table 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 an 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.

[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.

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 A3, we show that increased police patrol in QCTs primarily occurs during the evenings. In Appendix A4, we demonstrate that increased police time is driven by increased patrol frequencies rather than changes in officer size or racial composition. Appendix A6 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 A6, and find overall consistent patterns across most specifications.[11]

4.2.2 Role of Police in the Investment-Crime Relationship

Columns 2 and 3 of Table 3 report the reduced-form effect of QCT status on the number of violent and property crimes per 1000 jobs, an outcome measure that 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 consistent with Diamond and McQuade (2019) at similar levels of geography, suggesting that any negative bias associated with the misspecification 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, the number of jobs, comes from the LODES data that is available annually, in contrast to ACS 5-year estimates. Appendix Table A16 shows similar results using the ACS 5-year population estimate as the denominator.

Table 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 ($\text{arsinh}y = ln(y + \sqrt{y^2+1})$), 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, and 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] 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

[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; 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).

this residual change 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 A3 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 experience a differential change in police patrol relative to the city. However, there is a 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 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 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."

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 varies depending on the share of the Black population in a QCT relative to its city.

In Table 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. Moreover, predominantly Black neighborhoods often correlate with socioeconomically disadvantaged neighborhoods, suggesting that this result might also have implications for how police react to poorer neighborhoods receiving investments.

Table 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, and 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 greater 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 differ by the housing characteristics from the 2013-2017 American Community Survey using the same sub-sampling strategy.

Results in Table 4 are generally in line with the idea that QCT status has a 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), so it is reasonable to think that police might respond differently to changes in the environment in different cities. To explore this, Appendix Figure A5 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 the 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.

4.3 How does QCT status change policing and crime?

Our findings indicate that QCT-spurred investments alter the local physical and socioeconomic environment, increase police presence, and reduce crime rates. This raises a key question: why does police presence increase in QCTs? In this section, we aim to distinguish the impact of environmental changes from demographic shifts in the local population.

To achieve this, we use Google Street View (GSV) images to observe changes in the physical environment in QCTs versus non-selected tracts over time. Google regularly updates street view images of the same location and avoids capturing images of people, which helps isolate environmental influences by focusing on static elements rather than dynamic human activities.

We apply deep learning models to these street view images to predict crime rates and police hours for each census block in QCTs and eligible non-selected tracts. We then examine the impact of QCT status on predicted police presence and crime over time, comparing this with the effect of QCT status on actual outcomes. If the impacts on predicted and actual outcomes are similar, it suggests that environmental factors may primarily drive these changes. Conversely, if the impact on predicted outcomes is much smaller, it implies that demographic changes may play a larger role.

4.3.1 Training Deep Learning Model

For our prediction task, we compiled a panel of street view images from the Google Street View static API, collecting street view panoramas of census blocks in the 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).[14] For each street view panorama, we downloaded two images with two headings to capture the horizontal view of the housing. Each census block was assigned the nearest street view panorama to the block's centroid, excluding panoramas that 1) are not within the corresponding census block and are more than 50 meters away from the block's

[14] We select 3 years before and after 2018, as not all street views are captured every year. If a location has multiple street view images, we used the one closest to 2018.

centroid, and 2) could not be downloaded from the Street View API. This process resulted in a dataset of 622,353 unique images across 18 cities.

We predict outcomes for street view images by training neural network models using the Residual Network architecture (ResNet), a canonical computer vision model for image recognition (He et al., 2016). Specifically, we fine-tune three separate ResNet-50 (or ResNet-18) models, which are pre-trained on the Places365 dataset, to perform our image regression task.[15] Each model is trained to predict a distinct set of key outcomes: 1) urban perception scores, 2) police hours, and 3) crime indices reflecting the total costs of crime. For all models, we implement data augmentation to increase the diversity of the training data. This involves cropping each image at the four corners and the center, then applying horizontal flips, resulting in 10 variations per image. This approach has significantly improved model performance.

The training dataset for urban perception scores comes from the publicly available Place Pulse 2.0 dataset, a widely-used dataset to study how individuals perceive urban appearance (Dubey et al., 2016). This data includes 110,988 street view images from 56 cities globally and contains over 1 million pairwise comparisons from over 80,000 online participants. Participants rated images along six dimensions: safe, lively, boring, wealthy, depressing, and beautiful. Specifically, they were shown two random street view images and asked questions such as "which place looks safer (or livelier, more boring, wealthier, more depressing, more beautiful)?" These pairwise comparisons were converted into scores using the Microsoft TrueSkill ranking algorithm. For our training, we focus exclusively on 27,784 images from 13 US cities, as our prediction task is tailored to images in US cities.

To predict crime and policing outcomes, we construct separate training and testing datasets by calculating police hours and crime indices within the radii of 100m, 200m, and 300m of all downloaded street views. These data were divided into 60% for training, 20% for validation, and 20% for testing. Police presence is measured as the total hours of all likely phones present within each panorama radius per year, again using an arsinh transformation to address skewness. Crime indices are constructed by calculating a weighted sum of the estimated relative costs of major property and

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

violent crime categories, multiplied by their respective crime counts.[16]

For policing and crime outcomes, we train separate models for each city using that city's image to account for unique local patterns and characteristics. However, for urban perception, we train a single model due to a lack of a comprehensive dataset encompassing all 18 US cities in this study.

The performance of the trained models is robust across all three outcomes. For urban perception, the model achieves a correlation coefficient of 0.52 between the predicted and actual scores for "safe." Performance on other dimensions is slightly lower: 0.5 for beautiful, 0.47 for lively, 0.43 for wealthy, 0.37 for depressing, and 0.30 for boring. Consequently, our later analyses focus on the predicted scores for "safe", "beautiful", "lively" and "wealthy", given the more reliable predictions for these dimensions.

Appendix Tables A18 and A19 present the correlation coefficients between predicted and actual labels in the testing data. The model's performance improves when predicting police hours and crime indices within larger radii. Performance varies by city, for example, police hours correlation coefficients range from 0.394 in San Francisco to 0.686 in Austin within a 300m radius, and crime indices range from 0.354 in Austin to 0.844 in Seattle within the same radius. Overall, the pooled correlation coefficient across cities is 0.69 for police hours and 0.76 for crime indices within the 300m radius, indicating a strong model fit for the fine-tuned ResNet models. Additionally, Appendix Tables A20 and A21 show that street view predictions of crime or police hours explain significantly more variation in actual crime or police hours than demographic variables alone.

4.3.2 Impact of QCT Status on Predicted Outcomes

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 in Qualified Census Tracts (QCTs) to those in eligible but non-selected tracts. We first examine the impact of QCT status on predicted urban perception scores to determine if QCT status significantly alters the local physical

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

environment. Table 5 suggests predicted scores on perceived safety, beauty, and wealthiness increase in QCTs relative to similar non-selected tracts. This finding is consistent with earlier results indicating a reduction in 311 call requests in QCTs, suggesting that QCTs indeed experience improvements in the local physical environment.

Table 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, and 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 6 presents the image-level results for police hours. Columns 1-3 indicate an increase in actual police hours nearby for blocks in QCTs, relative to blocks located in non-QCTs, with the 300m estimate showing a 12% increase in police hours, closely aligning with tract-level estimates. Conversely, the estimates for predicted police hours, shown in columns 4-6, though positive, are considerably smaller. This suggests that direct police responses to environmental changes were minimal. Instead, the observed increase in police presence in QCTs is likely driven by changes in civilian activities, such as increased 911 calls, or by changing directives from police leadership correlated with the place-based investment (e.g. policing that is supportive of a mayoral revitalization initiative).

Table 6: Effect of QCT status on predicted police hours

	Demeaned arsinh(Hour) (Actual)			Demeaned Predicted arsinh(Hour)		
	(1)	(2)	(3)	(4)	(5)	(6)
	100 m	200 m	300 m	100 m	200 m	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 (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, and 95% confidence intervals are reported in the square brackets.

Similarly, Table 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 in predicted crime indices for blocks in QCTs relative to non-selected tracts. Thus, while investments in QCTs significantly impact the local physical environment, the observed changes in crime rates are primarily driven by shifts in local population dynamics and changes in law enforcement practices, rather than direct responses to the environment itself.

Table 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	-9.287	-135.900	85.425	269.866	561.799
	[-18.537,105.259]	[-164.777,146.202]	[-442.674,170.874]	[42.877,127.974]	[84.890,454.843]	[163.557,960.041]
	(31.570)	(79.305)	(156.466)	(21.701)	(94.346)	(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	-78.718	-460.550	-51.363	9.397	-140.726
	[-277.407,132.684]	[-411.742,254.307]	[-1065.397,144.296]	[-191.601,88.875]	[-253.298,272.092]	[-702.226,420.775]
	(104.617)	(169.914)	(308.601)	(71.552)	(134.031)	(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, a n d95% confidence intervals are reported in the square brackets.

Overall, we provide suggestive evidence regarding the mechanisms through which policing and crime responses are influenced by QCT-spurred investments, using deep learning models applied 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 directly attributable to these environmental changes. Importantly, our analysis underscores that using Google Street View images enables us to have a more nuanced understanding of how place-based policies affect the physical space and dynamics of neighborhoods.

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 increased patrol in QCTs that received 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. Moreover, by training deep learning models on Google Street View images, we find that QCT-induced reductions in physical disorder explain very little of the observed increases in policing or reductions in crime, challenging the broken windows theories of crime reduction. 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 a 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). 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 echo recent recommendations by the Council on Criminal Justice (CCJ) on violence reduction, emphasizing the importance of a holistic response to crime problems.[17]

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). 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 the advantage of measuring police presence consistently across jurisdictions. Efforts to harmonize criminal justice data across jurisdictions, such as those by the Criminal Justice Administrative Records System (CJARS), are crucial for comprehensive policy evaluation.

Several important caveats apply to our study. We can only measure the medium-term effect of local development on police presence due to the limited availability of

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

smartphone location data. Many believe that place-based investments address the root causes of crime in the long term by increasing economic and educational opportunities, community cohesion, and access to social services for residents, potentially reducing crime. Whether place-based investments and policing are substitutes in the long term remains an open question that requires further investigation.

Additionally, we examine one type of place-based intervention. More research is needed to understand how police respond to other types of place-based programs that either directly reduce physical disorder (e.g. vacant lot cleanup) or similarly facilitate economic investment (e.g. opportunity zones). Regardless of program type, this paper underscores the importance of evaluating the general equilibrium effect of changing neighborhood environments.

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 attention to the neighborhood (Chalfin et al. 2022a), we lack sufficient data to measure how police interact with residents in these changing environments. This limits our ability to determine whether increased police presence translates to better policing practices, specifically, whether this increased policing means more calls for service or higher rates of arrests, as well as the implication on community well-being and social welfare. More research is needed to investigate how police responses affect residents' lived experiences (Cho et al., 2023; Chalfin et al., 2022b; Weisburst, 2019a).

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Appendix

A1 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 1 summarizes the number of LIHTC projects and units that were placed in service in a tract between 2018 and 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.[18] We aggregate these block-level statistics to tract-level to estimate changes in neighborhood composition from 2017 to 2019.[19]

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.[20] 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.[21] In Panel C of Table 1, we

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

[19]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.

[20]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.

[21]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

present summary statistics for the number of crimes per 1000 jobs, comparing QCTs to eligible, non-selected tracts.

311 Call Data We collect geocoded 311 call 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 1 also shows that on average, QCTs have 25 street light repair requests in a year relative to 16 street light repair requests in eligible non-selected tracts.

A2 Validity Check of Police Presence Measure

In this section, we present various validity checks for smartphone measures of police presence from Chen et al. (Forthcoming). First, in Appendix Figure A1, we demonstrate a high correlation between the observed number of patrol officer devices in US cities and FBI estimates of police force size. Second, we probabilistically impute each device's "race" based on its home census block's racial composition. We then compare each department's imputed racial composition with their reported racial composition from 2016 Law Enforcement Management and Administration Statistics (LEMAS). Appendix Figure A2 shows 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 (except for Asian, as Asians account for only 2.5% of the police force across the cities in LEMAS). Third, our GPS-based measure of neighborhood police presence strongly correlates with downstream measures of police actions, such as stops and arrests. In Appendix Figure A3, we observe a correlation ranging from 0.44 (Washington, DC) to 0.68 (Austin, TX) among six cities where we collected geocoded arrest data. Additionally, Appendix Figure A4 shows similar positive and significant correlations for police stops in nine cities with

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 A16 demonstrates that the results are quantitatively similar when calculating per capita crimes using ACS five-year estimate as the denominator, and Appendix Table A17 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.

publicly available geocoded records.

A3 Heterogeneity by Time

In Appendix Figure A6, we explore how the increased patrol in QCTs is distributed over time to provide a more nuanced 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 A5, we similarly compute officer-hours spent in a census tract during various time periods: daytime (7 am - 6 pm), nighttime (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, is not precisely estimated and there is a smaller, 11% increase 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.

A4 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 Columns 6 and 7 of Appendix Table A5, 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 the 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.[22] Column 9 of Appendix Table A5 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 A6, we decompose the total police time spent in a census tract into the time 1) when officers move at an average speed of at least 1 mph, indicative of a "drive-through" (i.e. "a short visit") at a place, or 2) when officers move at an average speed below 1 mph, indicating a "longer visit".[23] 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. Columns 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 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

[22] 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.

[23] 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 an average speed below (at least) 1 mph in a tract.

by increasing their local presence. This could be driven by individual officer decisions, 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.

A5 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 the 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 changes in police presence within a treatment radius with those in a 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 A7, 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.

A6 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 A8 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 is more likely to belong to non-patrol officer movement compared to those departing from community stations.[24] We find a slightly attenuated point estimate for this measure, suggesting that our results are not solely driven by other police officers who do not perform regular patrol duties.

Appendix Table A9 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 is 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 variations may represent actual change in each station's policing intensity, or/and simply smartphone sampling variation across different years. To in-

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

vestigate 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 station in 2017 with replacement, such that the number of pings from each station in 2017 matches those numbers 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 A10 presents the results where we compare the 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 of each station 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 A15 and A11, 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 to 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 A12 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 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

A7 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. [25]

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 concerns about 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).[26] Panel (b) of Appendix Figure A7 reveals that with 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 estimates 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 A13 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 providing a lower bound of the estimates while the inverse probability weighting estimators provide an upper bound. What this implies is that the inverse probability weighting approach emphasizes the theoretical issue of the 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, which are typically geographically more proximate to the QCTs. In line with Appendix Table A3, the fact that we are seeing a larger (despite more imprecise) point estimate suggests that the equally

[25] 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 A8. While panel (a) of Appendix Figure A8 suggests that all leave-out estimates are almost the same as 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 the estimate. Still, we see that the original estimate without excluding any tracts in the main paper is similar in magnitude to the estimate using the full sample with all metropolitan tracts.

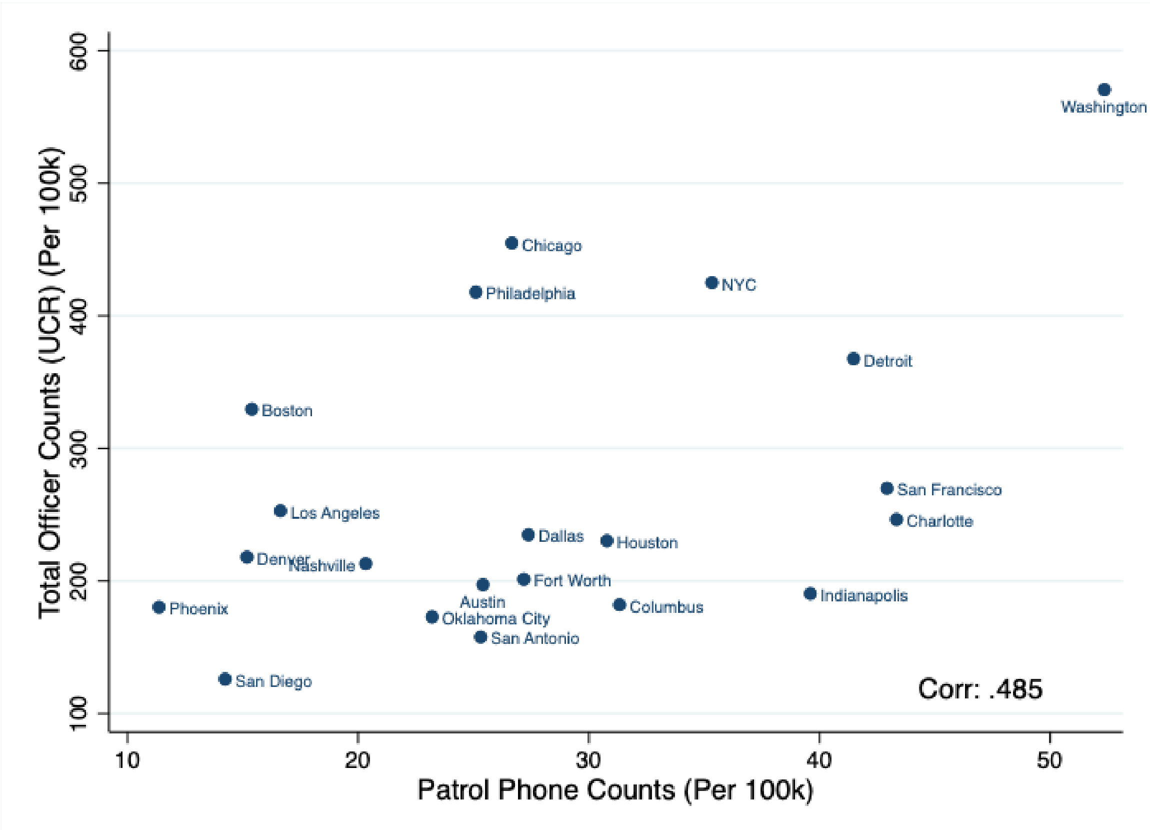
[26] The five cities are Boston, Columbus, El Paso, Indianapolis, and Oklahoma City.

poor tracts with little funding for investments experienced greater loss when policing resources were constrained.

In Appendix Figure A9, 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.

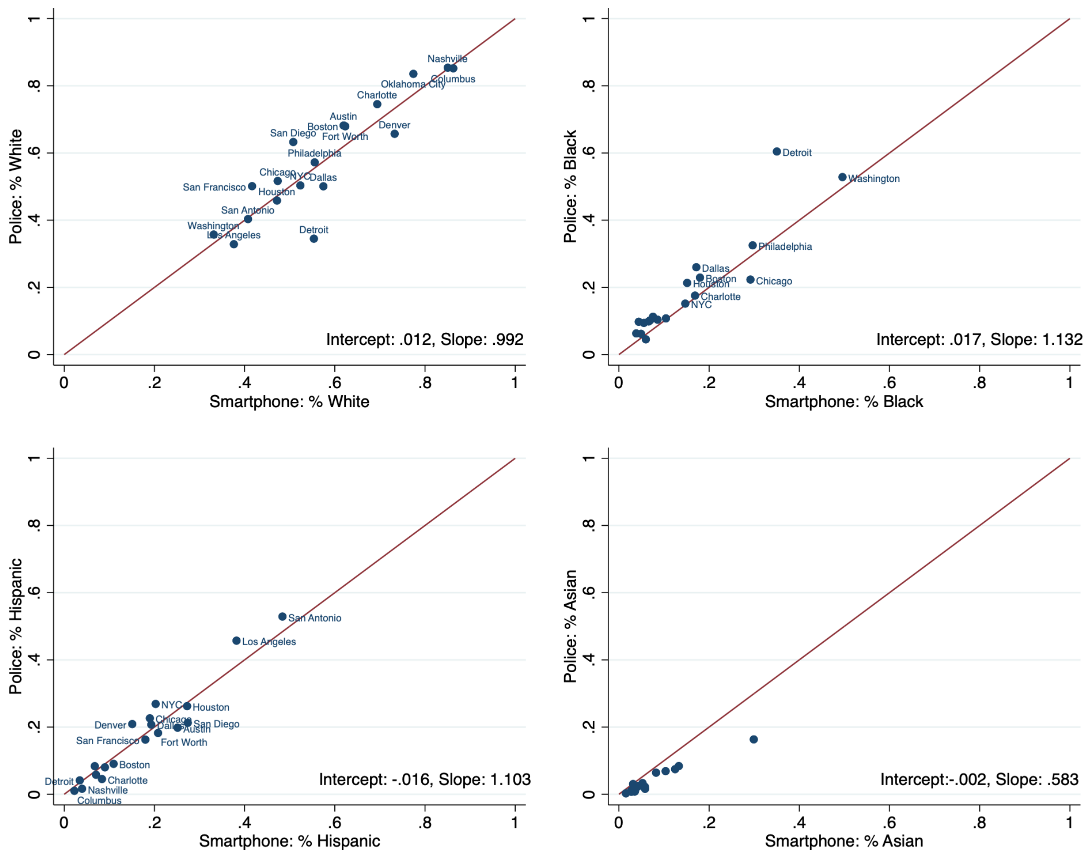
A7 Figure and Table

Figure A1: UCR officer and patrol smartphone



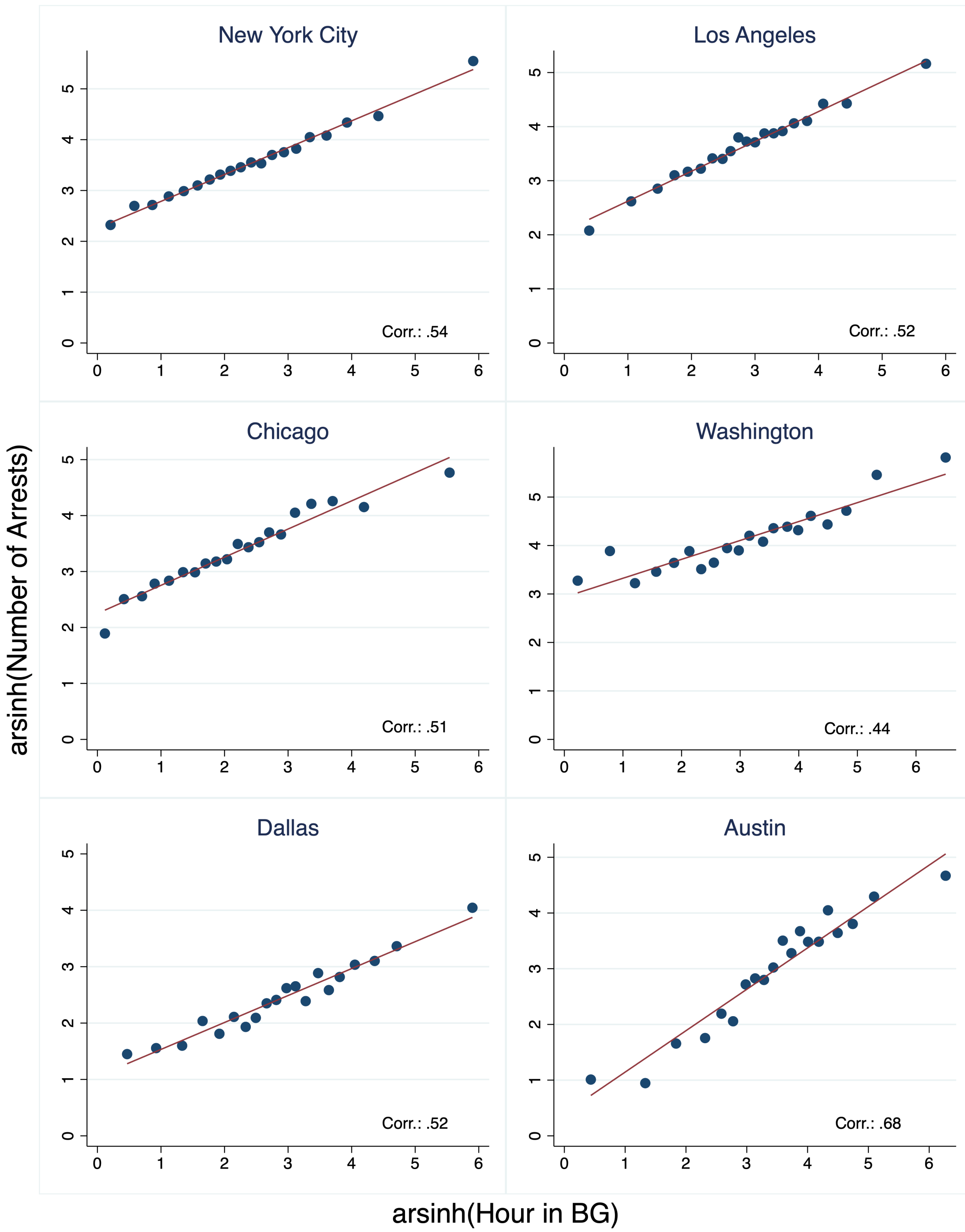
Notes: Total Officer Counts on the y-axis reports the number of officers (with arrest powers) in each city's police department on October 1st, 2017 from Uniform Crime Report (UCR) data. *Patrol Smartphone Counts* reports the number of smartphones that have at least one "shift" during 2017. Correlation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

Figure A2: LEMAS police force racial Composition vs. smartphone racial composition



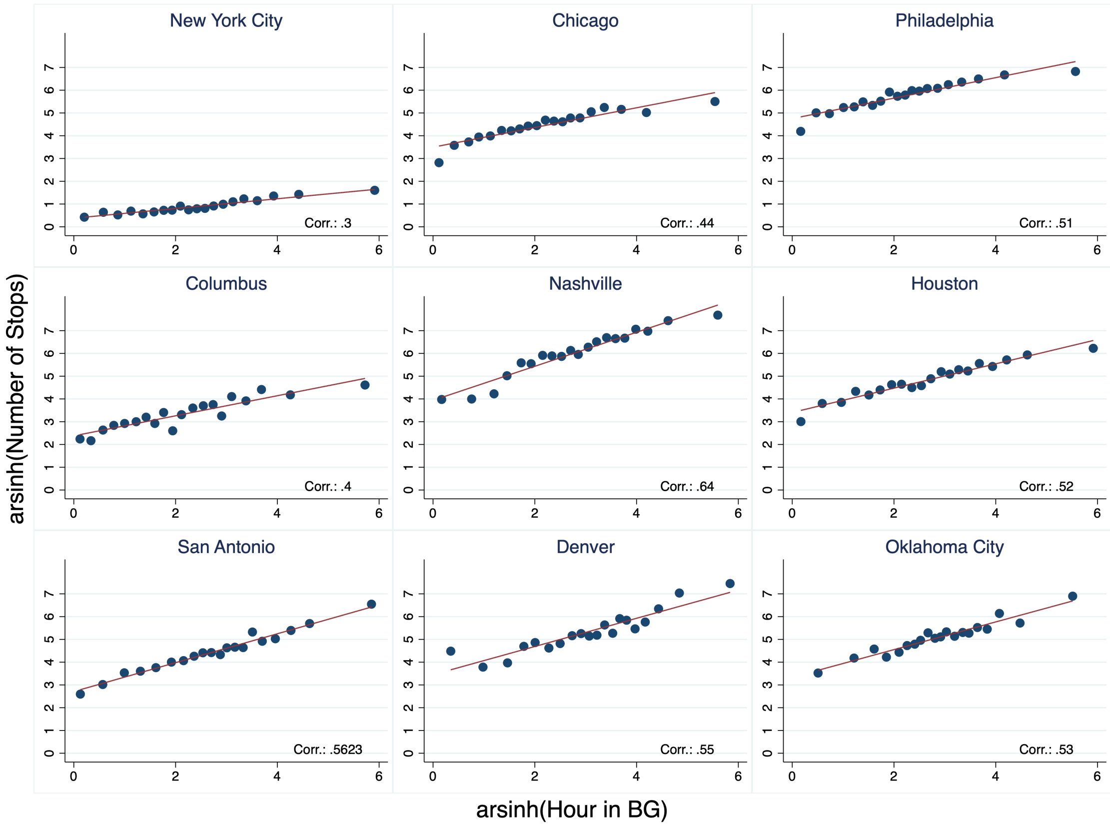
Notes: Police % White (Black, Hispanic, Asian) represents measures of racial composition of police officers from LEMAS data. Smartphone: % White (Black, Hispanic, Asian) denotes the smartphone-imputed racial composition for likely patrol officers based on home blocks.

Figure A3: Number of arrests vs. police hours across census block groups



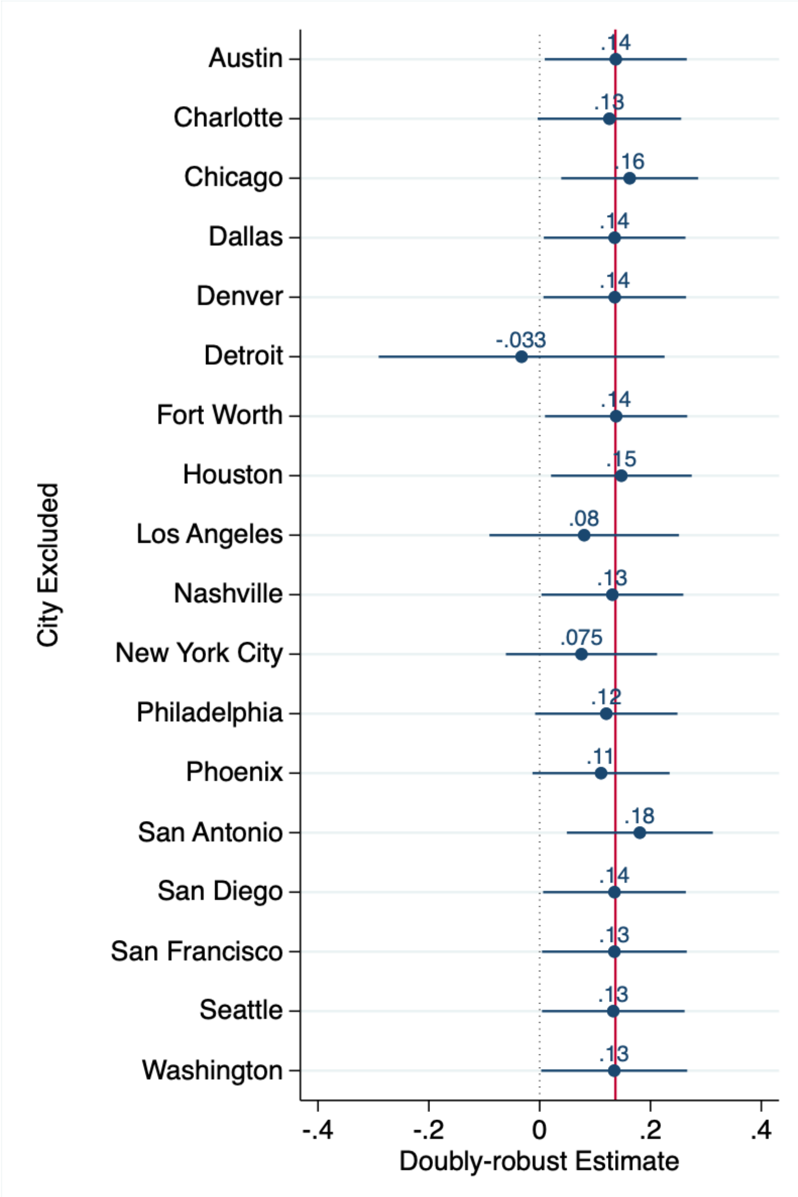
Notes: Each panel presents a binned scatter plot of the number of arrests vs. the police hours observed in the block groups, with both variables measured in arsinh values. Block groups are grouped into 20 equal size bins. Correlation coefficient between the two measures (in arsinh values) is reported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

Figure A4: Number of stops vs. police hours across census block groups



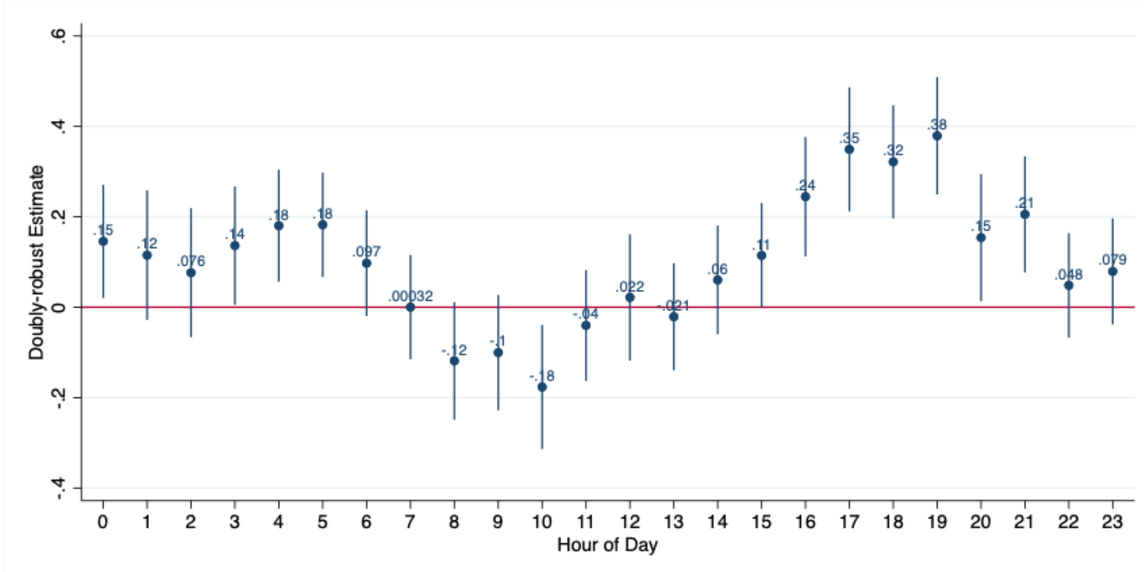
Notes: Each panel presents a binned scatter plot of number of stops vs. the police hours observed in the block groups, with both variables transformed in arsinh values. Block groups are grouped into 20 equal-sized bins. Correlation coefficient between the two measures (in arsinh values) is reported in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

Figure A5: 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.

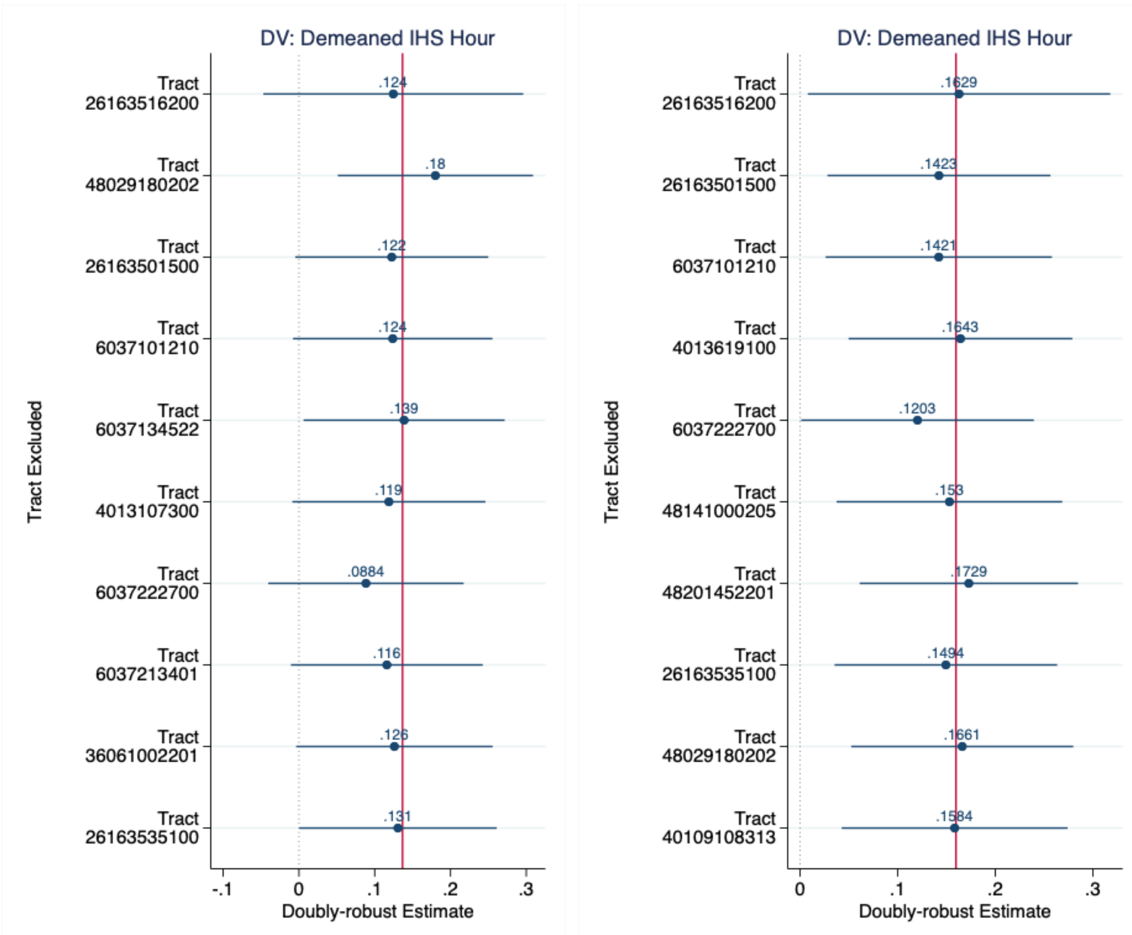
Figure A6: Effect on police hour by hour of day



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 A7: Sensitivity to most weighted tracts: leave-one-out estimates on the police hours

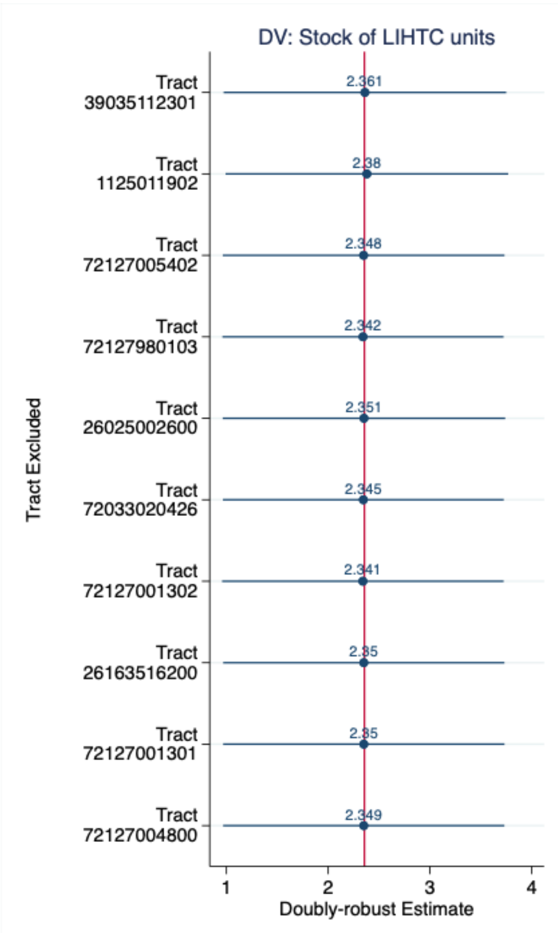
(a) Sample: 18 cities (b) Sample: 23 cities



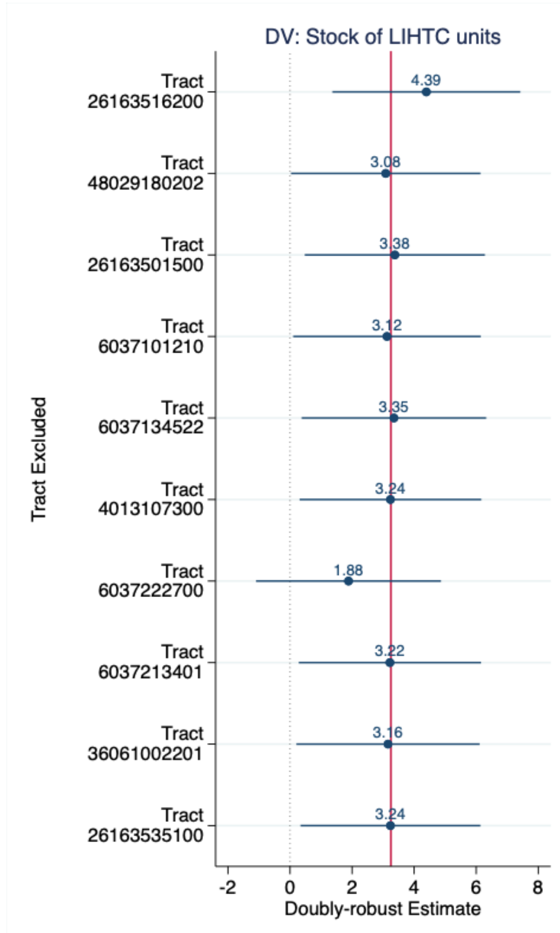
Notes: This figure displays doubly robust estimates for the effect of QCT status on police hour (demeaned and in arsinh values), in which we iteratively exclude one of the top ten most weighted tracts in our sample. The red line indicates the original doubly robust estimate when no tract is excluded. 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 A8: Sensitivity to most weighted tracts: leave-one-out estimates on the stock of LIHTC unit

(a) Sample: all US metro tracts



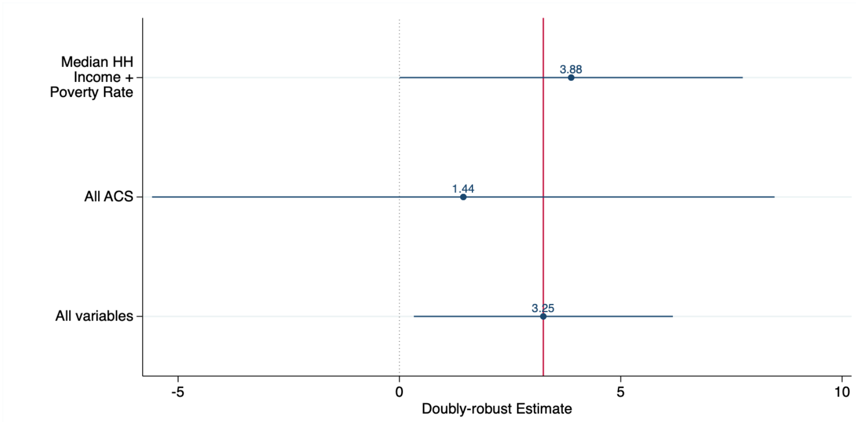
(b) Sample: 18 cities



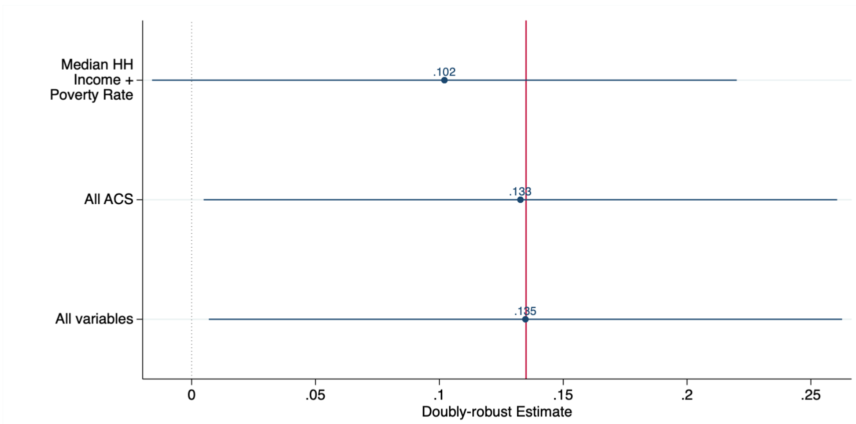
Notes: This figure displays doubly robust estimates for the effect of QCT status on the stock of LIHTC unit in a tract, in which we iteratively exclude one of the top ten most weighted tracts in our sample. The red line indicates the original doubly robust estimate when no tract is excluded. Panel (a) 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 A9: Sensitivity to matching variables

(a) Stock of LIHTC units



(b) Demeaned arsinh(Hour)



(c) Demeaned Violent Crimes per 1,000 Jobs

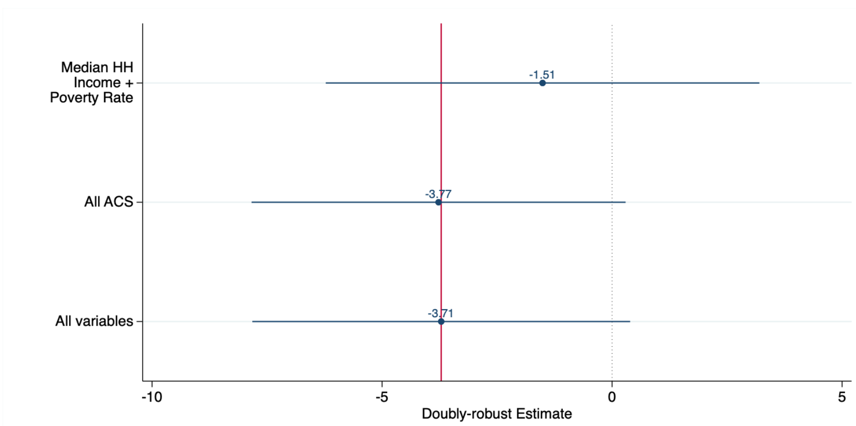


Table A1: QCTs and non-selected tracts in 18 cities

	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 A2: 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 A3: 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 A4: Effect of QCT status on tract composition and street traffic

	DV: Residence Area Characteristics (Demeaned)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	% White	% Black	% Asian	% Hispanic	% Less HS	% College	N. Jobs	Jobs (E<1250)	Jobs (E:1251-3333)	Jobs (E>3333)	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, and 95% confidence intervals are reported in the square brackets.

Table A5: 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, and 95% confidence intervals are reported in the square brackets.

Table A6: Effect of QCT status on police movement characteristics

	DV: Police Movement Characteristics (Demeaned)			
	(1) arsinh(Hour: Short Visit)	(2) arsinh(Hour: Long Visit)	(3) Mean Speed	(4) 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, and 95% confidence intervals are reported in the square brackets.

Table A7: 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, and 95% confidence intervals are reported in the square brackets.

Table A8: 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, and 95% confidence intervals are reported in the square brackets.

Table A9: Log-transformation of police hours

	DV: Demeaned log(Police Hour), dropped NA values						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	dm_log_hour_in_tr	dm_log_hour_day	dm_log_hour_nite	dm_log_hour_wkdy	dm_log_hour_wknd	dm_log_N_officers_in_tr	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, and 95% confidence intervals are reported in the square brackets.

Table A10: Comparing change in actual police presence in 2019 with the synthetic police presence in 2017

	DV: arsinh(Police Activities)						
	(1) Hour	(2) Hour: Day Time	(3) Hour: Night Time	(4) Hour: Weekday	(5) Hour: Weekend	(6) Officer	(7) 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, and 95% confidence intervals are reported in the square brackets.

Table A11: 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, and 95% confidence intervals are reported in the square brackets.

Table A12: Effect of QCT status on policing, excluding cities without binding population caps

	DV: Demeaned arsinh(Police Activities)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Hour	Hour: Day Time	Hour: Night Time	Hour: Weekday	Hour: Weekend	arsinh(Officer)	arsinh(Shifts)	Frac. Days with Police Presence	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, and 95% confidence intervals are reported in the square brackets.

Table A13: 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, and 95% confidence intervals are reported in the square brackets.

Table A14: 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, and 95% confidence intervals are reported in the square brackets.

Table A15: 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, and 95% confidence intervals are reported in the square brackets.

Table A16: Effect of QCT status on crime per 1000 residents (ACS estimates of population as denominator)

	DV: Crime Per 1000 Residents (ACS, demeaned)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Violent Crimes	Robberies	Aggravated Assaults	Homicides	Property Crimes	Burglaries	Thefts	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, and 95% confidence intervals are reported in the square brackets.

Table A17: 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, and 95% confidence intervals are reported in the square brackets.

Table A18: 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 A19: 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 A20: 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***		1.159***						
	(0.014)		(0.014)						
Log Population (ACS 15-19)		-0.096***	-0.097***		-0.145***	-0.131***		-0.150***	-0.128***
		(0.015)	(0.011)		(0.015)	(0.010)		(0.015)	(0.010)
Log housing units (ACS 15-19)		0.102***	0.124***		0.140***	0.167***		0.142***	0.170***
		(0.015)	(0.011)		(0.015)	(0.010)		(0.015)	(0.010)
Share recent built units (15-19 ACS)		-0.242***	0.130**		-0.389***	0.246***		-0.410***	0.189***
		(0.072)	(0.058)		(0.063)	(0.047)		(0.063)	(0.044)
Share units owner occupied (ACS 15-19)		-0.676***	-0.183***		-0.885***	-0.243***		-0.968***	-0.287***
		(0.022)	(0.014)		(0.022)	(0.013)		(0.022)	(0.012)
Share units occupied (ACS 15-19)		0.166***	0.006		0.070	-0.038		0.070	-0.098***
		(0.048)	(0.035)		(0.048)	(0.033)		(0.049)	(0.034)
% College (ACS 15-19)		0.218***	-0.117***		0.256***	-0.203***		0.323***	-0.210***
		(0.026)	(0.019)		(0.027)	(0.020)		(0.027)	(0.019)
Census Return Rate 2010		-0.491***	0.094**		-0.528***	-0.044		-0.610***	0.012
		(0.067)	(0.046)		(0.066)	(0.042)		(0.065)	(0.040)
Share Age $<$ 5 (ACS 15-19)		-0.521***	-0.125*		-0.722***	-0.238***		-0.914***	-0.405***
		(0.096)	(0.068)		(0.098)	(0.067)		(0.098)	(0.063)
Share Age Between 5 and 17 (ACS 15-19)		-0.397***	-0.106***		-0.517***	-0.116***		-0.601***	-0.175***
		(0.055)	(0.038)		(0.056)	(0.037)		(0.056)	(0.036)
Share Age $>$ 65 (ACS 15-19)		0.090	-0.291***		-0.010	-0.419***		-0.004	-0.485***
		(0.057)	(0.041)		(0.056)	(0.039)		(0.056)	(0.038)
% Hispanic (ACS 15-19)		-0.183***	0.296***		-0.250***	0.433***		-0.264***	0.466***
		(0.038)	(0.025)		(0.045)	(0.027)		(0.045)	(0.027)
% White (ACS 15-19)		-0.366***	-0.058*		-0.456***	-0.003		-0.501***	-0.028
		(0.043)	(0.030)		(0.048)	(0.032)		(0.048)	(0.031)
% Black (ACS 15-19)		0.106**	0.246***		0.217***	0.327***		0.241***	0.360***
		(0.049)	(0.031)		(0.053)	(0.034)		(0.053)	(0.033)
		(0.030)	(0.021)		(0.029)	(0.021)		(0.030)	(0.021)
Predicted Std Crime Index, 200 m				1.149***		1.126***			
				(0.014)		(0.014)			
Predicted Std Crime Index, 300 m							1.118***		1.093***
							(0.011)		(0.012)
Constant	0.034***	0.702***	-0.010	0.060***	1.059***	0.168***	0.055***	1.187***	0.187***
	(0.003)	(0.083)	(0.055)	(0.003)	(0.083)	(0.053)	(0.003)	(0.083)	(0.051)
Observations	60859	60199	60199	60859	60199	60199	60859	60199	60199
R^2	0.518	0.082	0.543	0.561	0.145	0.605	0.602	0.170	0.655

Table A21: 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 arsinh(Hour), 100 m	1.222***		1.168***						
	(0.011)		(0.013)						
Predicted arsinh(Hour), 200 m				1.191***		1.110***			
				(0.006)		(0.007)			
Predicted arsinh(Hour), 300 m							1.172***		1.087***
							(0.005)		(0.006)
Log Population (ACS 15-19)		-0.010	-0.027***		-0.045**	-0.054***		-0.097***	-0.094***
		(0.012)	(0.010)		(0.019)	(0.014)		(0.023)	(0.016)
Log housing units (ACS 15-19)		-0.013	0.038***		-0.049***	0.070***		-0.061***	0.117***
		(0.012)	(0.010)		(0.019)	(0.013)		(0.022)	(0.015)
Share recent built units (15-19 ACS)		0.020	0.068*		-0.089	0.097*		-0.127	0.262***
		(0.045)	(0.036)		(0.069)	(0.052)		(0.083)	(0.061)
Share units owner occupied (ACS 15-19)		-0.521***	-0.132***		-1.004***	-0.341***		-1.276***	-0.428***
		(0.014)	(0.012)		(0.021)	(0.017)		(0.025)	(0.019)
Share units occupied (ACS 15-19)		-0.020	0.072***		-0.101*	0.021		-0.132**	0.161***
		(0.033)	(0.028)		(0.052)	(0.039)		(0.062)	(0.045)
% College (ACS 15-19)		0.451***	0.021		0.908***	0.016		1.162***	-0.002
		(0.018)	(0.015)		(0.028)	(0.022)		(0.034)	(0.026)
Census Return Rate 2010		-0.522***	-0.071*		-1.309***	0.044		-1.732***	-0.201***
		(0.043)	(0.037)		(0.070)	(0.055)		(0.084)	(0.063)
Share Age < 5 (ACS 15-19)		-0.592***	-0.200***		-1.111***	-0.359***		-1.499***	-0.435***
		(0.063)	(0.053)		(0.099)	(0.077)		(0.118)	(0.090)
Share Age Between 5 and 17 (ACS 15-19)		-0.734***	-0.244***		-1.458***	-0.449***		-2.019***	-0.668***
		(0.038)	(0.032)		(0.060)	(0.047)		(0.072)	(0.055)
Share Age > 65 (ACS 15-19)		0.033	-0.109***		0.129**	-0.190***		0.114*	-0.311***
		(0.036)	(0.031)		(0.057)	(0.045)		(0.068)	(0.052)
% Hispanic (ACS 15-19)		-0.027	0.080***		-0.065*	-0.047*		-0.103**	0.035
		(0.024)	(0.020)		(0.036)	(0.028)		(0.044)	(0.033)
% White (ACS 15-19)		-0.131***	-0.153***		-0.270***	-0.384***		-0.409***	-0.420***
		(0.030)	(0.024)		(0.046)	(0.035)		(0.056)	(0.041)
% Black (ACS 15-19)		0.228***	-0.060**		0.517***	-0.198***		0.666***	-0.233***
		(0.030)	(0.024)		(0.046)	(0.035)		(0.056)	(0.042)
		(0.022)	(0.018)		(0.035)	(0.027)		(0.043)	(0.032)
Constant	-0.222***	1.998***	0.015	-0.262***	3.804***	0.337***	-0.300***	5.226***	0.506***
	(0.013)	(0.054)	(0.048)	(0.010)	(0.085)	(0.068)	(0.010)	(0.103)	(0.080)
Observations	63615	62927	62927	63615	62927	62927	63615	62927	62927
R^2	0.364	0.116	0.374	0.472	0.183	0.488	0.510	0.214	0.530