Black Neighbors Matter: Officer Neighborhoods and Racial Differences in Policing*

Gerard Domènech-Arumí†

June 5, 2024

Click here for the most recent version

Abstract

Ten years of data from a large American city reveal that Black civilians are consistently overrepresented in police street interactions. I combine property registry data, voter files, and employee records to locate officers' exact addresses and study whether racial diversity in their neighborhoods impacts racial differences in policing. I find that White (unlike Black and Hispanic) officers are geographically clustered in a few (predominately White) neighborhoods. Those living in Black neighborhoods are more productive in Black interactions (e.g., more likely to seize drugs during stops). White officers quasi-randomly exposed to a new next-door Black neighbor interact with relatively fewer Blacks and are more productive. Effects are larger among officers in White neighborhoods, with less experience, and with more affluent new neighbors. These findings suggest that segregation may amplify racial differences in policing.

Keywords: Non-labor Discrimination, Policing, Neighborhood Characteristics
JEL Codes: J15, J45, R23

*I thank Sara Bagagli, Björn Brey, Stephen Billings, Micael Castanheira, Guillaume Chapelle, Ray Fisman, Paula Gobbi, Joanne Haddad, Eric Hardy, Madina Kurmangaliyeva, Jack McDevitt, Michael Mueller-Smith, Daniele Paserman, Steven Raphael, Roman Rivera, Nathan Story, and seminar and workshop participants at ULB, ECORES, 6th Banff Empirical Microeconomics Workshop, 2023 TxECW, Boston University, 2023 WEC Jr., 2023 TWEC, UC Berkeley, CSQIEP, IRLE, Vanderbilt, UC Riverside, University of Maryland AREC, UAB, and University of Zaragoza for comments, feedback, and help. This project was reviewed and approved by the Comité d'Ethique (Institutional Review Board) of the Université Libre de Bruxelles. All errors are my own.

†ECARES, Université Libre de Bruxelles. Av. F. D. Roosevelt 42, 1050 Brussels, Belgium. E-mail: gerard.domenech.arumi@ulb.be

1 Introduction

What drives racial differences in policing? For 67% of Americans, the answer is racial bias (Horowitz et al. 2019). Across America, Black civilians are significantly more likely than Whites to be stopped, searched, arrested, or subjects of police force (Edwards et al. 2019, Fryer Jr 2019). Given these numbers and recent cases of extreme police violence, such as the episodes that resulted in the deaths of George Floyd and Tyre Nichols, it is unsurprising that many Black Americans exhibit mistrust in police institutions and officers' proportionality when using force (Ang et al. 2024, Horowitz et al. 2019). In academia, the views on the matter are more nuanced, but the rising consensus and evidence point toward some degree of bias in the police Goncalves and Mello 2021, Fryer Jr 2019, Weisburst 2022).

Racial segregation could plausibly be a driver, perhaps through influencing racial bias. The idea that contact with individuals from different ethnic or social groups positively shapes attitudes towards these outer groups is not new (Allport et al. 1954), and several recent papers have provided evidence for it (e.g., Bazzi et al. 2019, Bursztyn et al. 2021, Rao 2019). Thus, racial segregation — which limits inter-group contact in daily interactions — could impact racial differences in policing.

Testing this hypothesis is challenging. It requires, at least, information on police officers' interactions with civilians and, critically, where they live. Most policing-related datasets do not even contain any information on officers and, due to confidentiality and security reasons, none includes addresses.

In this paper, I use ten years of police street stops from a large American city and officers' exact addresses retrieved by linking multiple administrative data to study whether the racial composition of officers' neighborhoods causally impacts racial differences in their policing.

As in other contexts, my data reveals that Black Americans are overrepresented in street stops. The city (City) Police Department (PD) Field Interactions (FI) data contains detailed information on the characteristics of all PD street interactions from 2011 to 2020, including officers' names and the race of the pedestrians or motorists stopped. Black Americans account for 60% of all individuals in FIs, even though less than 25% of City's population is Black.

I link multiple administrative data to find the exact addresses of PD officers. I obtained officers' full names and residence ZIP codes from the PD, public employee records, and a website collecting information concerning that PD through public sources and Freedom of Information Act (FOIA) requests. I combined these with voter files and the city and

state (State) property registries to locate their exact addresses. The amount of information gathered about PD's officers is substantial and sensitive (e.g., names and addresses of law enforcement personnel), which is why the city of this study is left undisclosed.

White (in contrast to Black and Hispanic) officers are geographically clustered in a few neighborhoods. These neighborhoods are predominantly White. For illustration, the average White officer in City lives in a block with a Black population of 11%. That is slightly more than the average White person in that city (who lives in a block with just under 10% of Black civilians), but less than half the exposure compared to the average individual.

White officers residing in Black neighborhoods do not stop Blacks at a differential rate, but their Black interactions are (up to 20%) more productive — in the Knowles et al. (2001) sense: more likely to result in an arrest, summons, or weapons or drugs found or seized during an interaction; outcomes suggesting officers are trying to reduce criminal activity. These results are consistent with exposure to out-group individuals in neighborhoods impacting racial differences in policing through a shift in attitudes, but also with residential sorting on unobserved characteristics. For instance, more baseline favorable attitudes toward Blacks could drive residential choices of White officers and their enhanced performance in Black police interactions.

I exploit quasi-random variation in officers' next-door neighbors turnover to causally identify the effects of exposure to individuals of different races in local environments on racial differences in policing. Officers may self-select into where they live initially, but they cannot control who their *new* next-door neighbors are. I construct a panel of addresses and their owners in City since 2010 and use owners' names to identify changes in property ownership involving a change in race. My identification strategy compares the over-time variation in outcomes from officers living close (within 100 meters in the baseline) to a dwelling that transitioned from non-Black to Black owner-occupied with those from other officers of the same race living in the same census tract without such exposure. The variation is highly granular.

White officers with a new Black neighbor stop relatively fewer Blacks. On average, their Black interactions decrease by 15 percentage points (pp). They also become more productive. Their Black stops are more likely to result in arrests, summons, and drugs found or seized. By these measures, they become 10pp (60%) more productive. Finally, there is suggestive evidence they also become less "violent," as the likelihood of frisks and searches decreases by up to 7pp.

Event study designs display no evidence of differences in outcomes' trends before the arrival of a new Black neighbor, and results prove to be robust to several sensitivity

tests. Among others, these include alternatively inferring owners' race from a sample of mortgages, exploring distinct treatment distances, variations in the definition of events, and re-estimating empirical specifications with alternative estimators introduced in recent entries of the two-way fixed effects literature (De Chaisemartin and d'Haultfoeuille 2022a).

A closer investigation of the mechanisms driving quasi-experimental results reveals that officers in "Whiter" neighborhoods, with less experience, and more affluent new neighbors (relative to them) react more strongly. These heterogeneities could indicate that the (large) magnitudes documented in the quasi-experimental exercise may partly result from the inherently positive selection in any subsample of owners (e.g., homeowners are wealthier than non-homeowners), an interpretation in line with the evidence provided by Carrell et al. (2019). They could also indicate that local environments impact racial differences in policing through mechanisms other than racial attitudes. For instance, increased inter-group contact in the home neighborhood could make White officers more proficient at extracting and interpreting signals from individuals in their out-group when patrolling, resulting in enhanced productivity.

This paper primarily contributes to the broad literature studying racial disparities in the criminal justice system, with a particular focus on policing outcomes. Extensive work has documented that Black civilians are more likely than Whites to be involved with the criminal justice system and investigated the underlying reasons, with a recurrent hypothesis in the background: racial bias.[1] In the specific context of policing outcomes, the literature departs from the observation that racial disparities in policing do not necessarily imply bias, as, for example, crime might be more prevalent in certain neighborhoods, which may happen to be predominantly Black. Therefore, an unbiased officer trying to prevent or reduce criminal activity would optimally make more stops in these neighborhoods, naturally resulting in more Black interactions (simply because the population at risk of being stopped will be disproportionately Black). With this line of reasoning, some papers have developed statistical tests to detect racial bias by looking at outcomes (hit rates) conditional on an interaction and, at times, provided evidence against bias (e.g., Anwar and Fang 2006, Coviello and Persico 2015, Knowles et al. 2001, Persico and Todd 2008, Stashko 2022). Other papers have employed similar methodologies or settings with quasi-experimental variation and documented evidence consistent with it. Using New York street stops data, Fryer Jr (2019) and Gelman et al. (2007) find that Blacks are significantly more likely to be stopped by the police even after controlling for a rich set of covariates. They argue that their results, even if not definite proof of bias, are most consistent with some form of taste-based discrimination (Becker 1957). Building on Grogger

[1] See Rehavi and Starr (2014), Arnold et al. (2018), or Lofstrom and Raphael (2016), among many others.

and Ridgeway (2006) and exploiting variation in street visibility conditions, Horrace and Rohlin (2016) find that Black drivers are significantly more likely to be stopped in Syracuse, NY. Other studies (some with quasi-experimental variation) using traffic stops in Boston, Florida, and an undisclosed State also find systematic racial differences not easily reconciled with statistical discrimination models alone (Antonovics and Knight 2009, Goncalves and Mello 2021, West 2018). Weisburst (2022) and Hoekstra and Sloan (2022) document racial differences in arrests and use of force using plausibly exogenous variation in officers' race from 911 calls. Most studies do not find significant differences in the most extreme uses of force (Johnson et al. 2019, Fryer Jr 2019), with some notable exceptions in which police institutions (like unions) may play a key role (e.g., Cunningham et al. 2021, Legewie 2016, Mekawi and Bresin 2015). A more recent strand in this literature investigates other causes explaining racial disparities in policing. Diversity in police departments (or their lack thereof) appears to be an important factor (Ba et al. 2021, McCrary 2007, Rivera 2022).

My contribution to this literature is threefold. First, by linking officers to an exact address, I can characterize their residential patterns and the demographics of their neighborhoods with great detail. My findings indicate they substantially differ based on officers' race, as also documented in Ba et al. (2022). Second, I provide novel descriptive associations between officer neighborhood demographics and the outcomes of police interactions, which hint at a possible link between the two. Finally, the quasi-experimental setting exploited in the paper allows me to go beyond associations and conjectures and make plausibly causal claims. My results support the hypothesis that officer local environments influence their policing, potentially with racial bias as a mediating mechanism.

This paper also contributes to the extensive literature on neighborhoods and segregation. Neighborhoods influence short- and long-term outcomes (Chapelle et al. 2023, Chyn and Katz 2021). Through peer effects and other mechanisms, they affect education and labor market outcomes (Bayer et al. 2008, Laliberté 2021, Oreopoulos 2003, Åslund et al. 2011), marital status and mobility (Chetty and Hendren 2018, Chetty et al. 2016; 2014), perceptions (Domènech-Arumí 2022; 2023), the propensity to engage in drugs use, commit crimes and recidivism (Billings and Schnepel 2020, Billings et al. 2019), and more. Segregation is typically associated with negative outcomes. Blacks in segregated neighborhoods fare worse in terms of education and labor market outcomes (Billings et al. 2013, Cutler and Glaeser 1997), experience higher levels of poverty and inequality (Ananat 2011), and suffer a deterioration in mental and physical health (Ludwig et al. 2013). My main contribution to this literature is to study and provide evidence for a new outcome affected by local environments: racial differences in workplace interactions.

The final contribution is to the literature on social and ethnic interactions. The con-

tact hypothesis posits that social interactions between individuals of different groups can favorably shift attitudes towards those in out-groups (Allport et al. 1954). Many recent studies have provided supporting evidence for it. In Indonesia, contact between individuals of different ethnic groups increased inter-group marriage (Bazzi et al. 2019). In India, interactions between rich and poor students made the former more pro-social and less likely to discriminate against the poor (Rao 2019). In South Africa, White students randomly assigned a Black roommate in a college dorm became less prejudiced towards Blacks (Corno et al. 2019). In the US and the specific context of the police, exposure to minority peers in the academy reduces officers' future propensity to arrest minority civilians (Rivera 2022). Other studies find mixed support. In France, increased ethnic diversity in apartment buildings led to social anomie and lower provision of local public goods (Algan et al. 2016). In two cross-country studies, Desmet et al. (2009) and Desmet et al. (2020) found that linguistic fractionalization was associated with less redistribution and public goods provision. I contribute to this literature by studying how exposure to out-group individuals in the immediate neighborhood can potentially affect the racial attitudes of a specific (but socially relevant) group of individuals: police officers. Although it is difficult to rule out all alternatives (due to the unobservable nature of attitudes), this paper's findings are consistent with the contact hypothesis.

2 Institutional background

2.1 Police interactions and their possible outcomes

There are several types of police interactions, which may result in different outcomes. An **observation** occurs when an officer observes, without physically interacting with, an individual or group of individuals for intelligence purposes (e.g., investigating gang activity in a neighborhood). An **encounter** occurs when an officer directly interacts with an individual, also on the grounds of investigative reasons. Encounters are consensual, and individuals can walk away or not identify themselves. A **stop** (or "terry stop") occurs when the officer temporarily detains a pedestrian or motorist. During the stop, the individual is not free to leave.[2] A **frisk** (or "pat down") occurs when, during a stop, the officer passes their hands over the pedestrian in search of hidden **weapons**, **drugs**, or other items. These may be seized. A "reasonable suspicion" based on facts or circumstances suggesting that the individual is involved in criminal activity must justify the stop and possible frisk.

[2]Abusing language, and for the sake of brevity, sometimes I will call police interactions "stops," even when they do not involve a stop as defined here.

A **search** occurs when the officer goes through part or all of an individual's property (including the cellphone or motor vehicle) and looks for specific items or evidence related to a criminal investigation. Searches must be justified by "probable cause" — grounded reasons to believe an individual was involved in a crime. If an individual is identified as a suspect, observed committing a serious crime (felony), or has an active warrant, police interactions may result in individuals being apprehended or taken into custody (**arrest**). When the offense is a misdemeanor (e.g., possession of a lower-class substance without intent to distribute or a traffic violation), interactions may result in the officers issuing a **summons** or **citation**, a formal requirement to appear in a designated court or government office at a specified time and date.

2.2 City demographics

The city of this study (City) is a large American town with over half a million inhabitants. The majority of its citizens are White, but the Black population exceeds 20%. Segregation is high and above the median relative to other large metropolitan areas, but it is not among the top segregated cities in the country (Other & Belonging Institute 2020).

3 Data

3.1 Field Interactions (FIs)

The Field Interactions (FI) data records all street interactions between the PD and private individuals from 2011 through 2020. The data contains detailed characteristics of the interactions, including the exact date, their location, demographic information about the individuals stopped (e.g., race, gender, age), and a written report describing the interaction (available after 2015). Unlike similar datasets for other cities (e.g., New York City), the FI dataset also includes the names and employee identifiers of the officers responsible for the FIs, thereby enabling the possibility of merging it with external sources. The FI data contains information on approximately 165 thousand interactions. In this study, I restrict the sample to only the interactions for which I observe the ZIP code, the race of all individuals involved, and carried by officers matched to an address (see Section 3.3 below). The baseline sample contains approximately 155 thousand FIs.

Table 1 presents summary statistics of police interactions. Most FIs (75%) involve just one civilian. 16% involve two, and 9% involve three or more. 60% of all civilians in interactions are Black. That figure is large (given the city's demographics) but in line with

Table 1: FIs summary statistics

	Mean
Number of individuals in FI	1.413
Share Black in FI	0.598
Share White in FI	0.232
Share Hispanic in FI	0.158
Share male in FI	0.888
Stop	0.579
Frisk	0.327
Search	0.186
Productive	0.188
Weapon found	0.035
Drugs found	0.027
Arrest	0.070
Summons issued	0.112
Male officer	0.941
White officer	0.782
Black officer	0.135
Hispanic officer	0.070
Asian officer	0.013
Neighborhood 1	0.372
Neighborhood 2	0.242
2011-13	0.504
2014-16	0.306
2017-20	0.189
N	155854

Notes: *Share Black* and *Share Male* denotes the share of Black or male individuals stopped during the FI. *Frisked* is an indicator taking the value of one if at least one of the individuals stopped was frisked. *Productive* is an indicator taking the value of one if the FI resulted in an *Arrest* or *Summons* issued or in *Drugs* or *Weapons* found. *Neighborhood 1* and *Neighborhood 2* are indicators taking the value of one if the FI occurred in those neighborhoods. The first neighborhood contains 8% of the city population, and over 50% of its residents are Black. The second neighborhood contains 15% of the city population, and 45% of its residents are Black. *2011* through *2020* are indicators taking the value of one if the stop occurred in that specific year.

other contexts. For example, Fryer Jr (2019) reports that 58% of all civilians stopped in New York City from 2003 to 2013 were Black. Most interactions involve males (89%) and are carried by male (94%) and White (78%) officers.[3] A stop occurs in almost 60% of the interactions. Conditional on a stop, frisks and searches occur 60 and 30% of the time, respectively. 19% of the interactions are "productive," defined as resulting in an arrest or summons, or in weapons or drugs found. Interactions occur throughout the city, but they are more common in specific neighborhoods — two (predominantly Black) neighborhoods concentrate over 60% of the FIs, despite only containing 23% of the total city population. Finally, there is a downward trend in the number of FIs over time, perhaps due to the increased public scrutiny of police interventions (Devi and Fryer Jr 2020, Shi 2008).

[3]These statistics are expected since, as in other contexts, White males are overrepresented in the PD (Ba et al. 2022).

3.2 Police officers

I obtained individual data on police officers from a website gathering information about the PD through several FOIA requests. Their data contains information on over 3,500 police officers and civilian employees working (or that recently worked) at the PD, including unique employee identifiers, full names, and ZIP codes of residence. Only about 2,500 officers ever participated in a FI. Attention is restricted to this subsample. The data also includes racial information for all active officers at the beginning of 2020 (80% of the sample). I obtained the race for most of the remaining officers from other public records (10%) or by predicting their race based on their name (7%) (see the Appendix B for details).

3.3 Other administrative data and address assignment

I use the following datasets to assign officers to an exact address:

1. **Property registry (State and City).** Contains detailed information on the universe of properties in the state and city. The key variables are the location of the property (geocoded address), the owner's name, and whether they occupy their property. The State data is a snapshot as of January 2021, while the City data is a yearly panel for 2010-2021.

2. **Voter files.** Contain the full names and addresses of all registered voters in the state as of late 2022.

3. **City public employee data.** Contains information on all public employees in City. Critically, it includes a unique employee identifier (the same as in the police data) and the ZIP code of residence in 2010-2021.

Combining them, I could assign over 2,200 officers (almost 90% of the target sample) to an exact address. The first step used officers' full names and residence ZIP codes in the police data to find their current addresses in the state voter files. I then used the assigned addresses and their names to match them to the property registry, which, combined with the other employee records, allowed me to obtain the address histories for the subset of officers in City. Appendix B describes this process in greater detail. Table A1 shows no significant differences in observable characteristics between the matched officers and their respective populations.

4 Descriptive facts

4.1 Officer neighborhoods



Figure 1: Distribution of officers across the city, by race

Notes: Each point in the map represents the location of an officer's home in City (with added noise). To keep the city undisclosed and protect officers' privacy, map locations are randomly shuffled within two kilometers of true locations, the plot axes are randomly flipped (so that the north in the map may not correspond to the north of the city), and the map scale is undisclosed and distorted. Different colors denote different races.

White officers live in White neighborhoods. Figure 1 displays officers' home addresses across City and provides the first hints of segregation varying with officers' race. There are four main clusters of White officers at the city borders, with the two most prominent on the West and East of the map. Non-White officers are not visibly spatially clustered, but the figure indicates that Black officers predominantly reside in the city's central areas.

Table 2: Racial composition of officers' neighborhoods

Officer Race: Residing in:	White State	White City	Black State	Black City	Hisp. State	Hisp. City
	(1)	(2)	(3)	(4)	(5)	(6)
Share White in:						
ZIP	0.64	0.50	0.37	0.25	0.47	0.35
	(0.26)	(0.24)	(0.27)	(0.18)	(0.28)	(0.22)
Tract	0.73	0.64	0.35	0.23	0.47	0.35
	(0.23)	(0.25)	(0.29)	(0.20)	(0.29)	(0.25)
Block Group	0.75	0.68	0.36	0.24	0.49	0.37
	(0.23)	(0.26)	(0.31)	(0.24)	(0.31)	(0.28)
2010 Block	0.81	0.74	0.39	0.27	0.53	0.41
	(0.22)	(0.25)	(0.33)	(0.28)	(0.33)	(0.29)
Share Black in:						
ZIP	0.14	0.22	0.36	0.44	0.25	0.32
	(0.17)	(0.19)	(0.22)	(0.19)	(0.22)	(0.21)
Tract	0.09	0.13	0.38	0.47	0.25	0.33
	(0.14)	(0.16)	(0.24)	(0.22)	(0.23)	(0.23)
Block Group	0.08	0.11	0.39	0.48	0.24	0.31
	(0.14)	(0.16)	(0.27)	(0.25)	(0.26)	(0.27)
2010 Block	0.06	0.09	0.38	0.48	0.23	0.31
	(0.13)	(0.16)	(0.29)	(0.27)	(0.26)	(0.26)
Share Hispanic in:						
ZIP	0.11	0.17	0.18	0.22	0.20	0.24
	(0.11)	(0.11)	(0.10)	(0.07)	(0.16)	(0.12)
Tract	0.09	0.12	0.17	0.22	0.20	0.24
	(0.11)	(0.13)	(0.11)	(0.09)	(0.16)	(0.14)
Block Group	0.08	0.12	0.17	0.21	0.18	0.22
	(0.12)	(0.14)	(0.14)	(0.14)	(0.18)	(0.17)
2010 Block	0.06	0.08	0.13	0.16	0.16	0.20
	(0.10)	(0.12)	(0.12)	(0.12)	(0.17)	(0.16)
Share Asian in:						
ZIP	0.08	0.08	0.05	0.05	0.05	0.05
	(0.07)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
Tract	0.07	0.07	0.05	0.05	0.05	0.05
	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.05)
Block Group	0.06	0.07	0.04	0.04	0.06	0.05
	(0.08)	(0.09)	(0.06)	(0.06)	(0.08)	(0.07)
2010 Block	0.05	0.06	0.04	0.03	0.04	0.04
	(0.08)	(0.08)	(0.07)	(0.06)	(0.07)	(0.06)
N	1550	850	450	300	200	150

Notes: The sample includes all matched officers with at least one FI. Neighborhood demographics are obtained from Table P2 in the 2010 Decennial Census (for census blocks) and Table B03002 in the 2019 American Community Survey (rest of geographies). *Share White*, *Share Black*, and *Share Asian* include the share of individuals that self-identify as non-Hispanic and only a specific race (White, Black, or Asian) in the geography. *Share Hispanic* includes the share of individuals that self-identify as Hispanic and any race. Officer numbers rounded to the nearest fiftieth. Asian officers' neighborhood demographics are not displayed due to the small number of observations. Standard deviations in parenthesis.

Looking at neighborhood demographics, Table 2 shows that 60% of the officers live in City. Among these, a majority reside in only three neighborhoods (concentrating 30% of the total population and an above-average Black representation). However, White officers live in these neighborhoods' "Whiter" areas. For example, even though 22% of the population in the ZIP code of the average White officer in the city is Black, only 13% of the population in their census tract belongs to that ethnic group. The same figures are 11 and 9% when looking at the census block group or census block, respectively, suggesting their closest neighbors are unlikely to be Black.[4] Still, despite being more segregated than the average individual in the city (who, for example, lives in a block group with a Black population of over 20%), they are less segregated than the average White (who lives in a block group with a Black population under 10%).

Black and Hispanic officers live in more diverse neighborhoods, with higher shares of Blacks in the ZIP, census tract, block group, or block. For instance, the average Black and Hispanic officer lives in a ZIP code with a Black population of 44 and 32%, respectively. These figures are above the city average and, in contrast to White officers, exhibit lower variation across administrative levels — i.e., Black and Hispanic officers' immediate neighbors are similar to the average neighbors in their ZIP of residence.

4.2 Officer neighborhoods and racial differences in policing

Raw correlations. White officers in Black neighborhoods interact with relatively fewer Blacks and are more productive in Black stops. The top left panel in Figure 2 indicates the presence of a mild negative relation (slope of -3pp, $p < 0.05$) between the share of Blacks in officers' neighborhoods (census block group),[5] and the share of Blacks in their FIs.[6] The top-right panel reveals a stronger positive association (13pp, $p < 0.01$) between the racial composition of officers' neighborhoods and their productivity. For example, the figure indicates that only about 15% of the Black stops from officers in neighborhoods with a Black population under 10% end up being productive, as opposed to a productivity nearing 20% for officers in neighborhoods with a Black population over 30%.

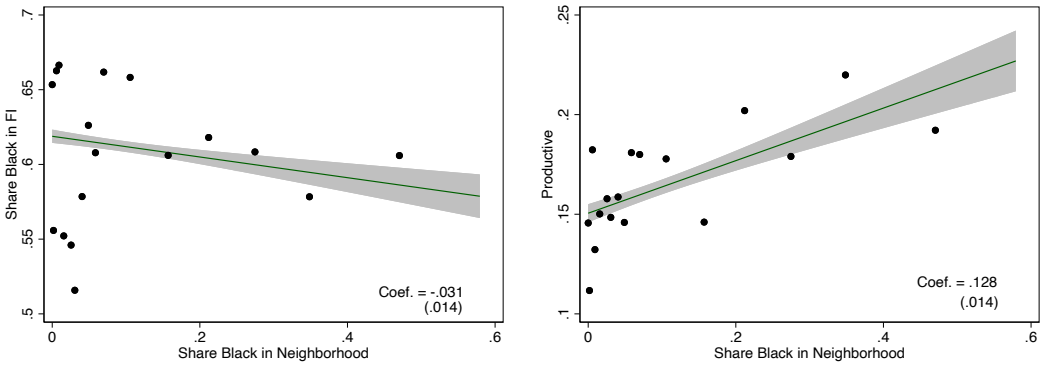
Black officers in Black neighborhoods interact with fewer Blacks in FIs, and their FIs are not differentially productive. In contrast, Hispanic officers in Black neighborhoods

[4]Census blocks, census block groups, and census tracts contain, on average, 800, 1,500, and 4,000 individuals, respectively.

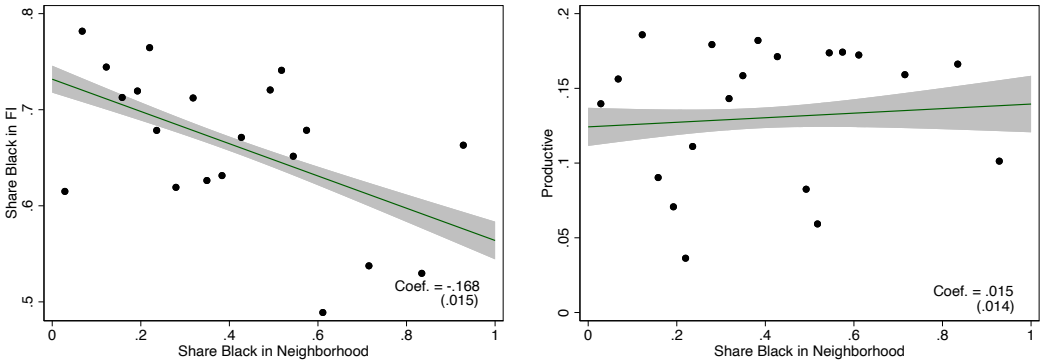
[5]The census block group is the smallest administrative unit with demographic information available yearly. Demographics are retrieved from the American Community Survey.

[6]For consistency with the rest of the paper, the baseline sample excludes officers not residing in City, as these cannot be used in the analysis from Section 5. I replicate the main descriptive results with the full sample of officers in Appendix C.

Panel A. White officers



Panel B. Black officers



Panel C. Hispanic officers

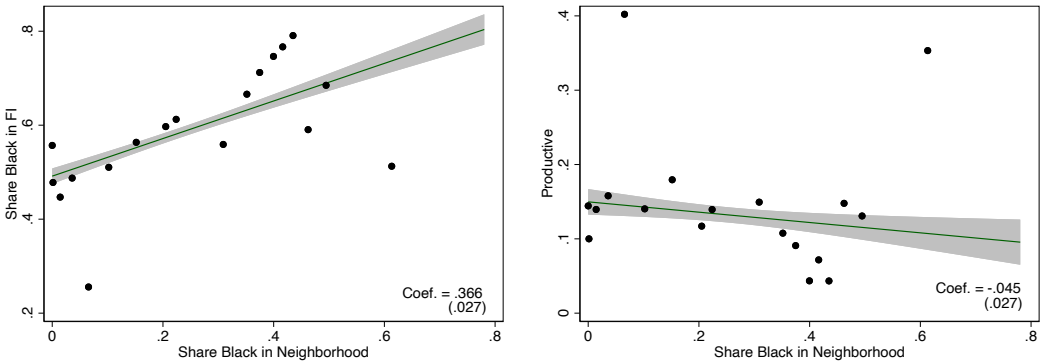


Figure 2: Neighborhoods and racial differences in FIs, by officers' race

Notes: The sample includes all FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers. Each panel subsample is divided into 20 equal-sized groups (vingtiles). Each point in the plot corresponds to the y-variable unconditional mean in a bin plotted against the x-variable unconditional mean in that same bin. The green line shows the best linear fit estimated on the underlying data using OLS. Gray areas denote 95% confidence intervals. OLS estimates and standard errors are reported in the bottom-right corner of the plot. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Share Black in Neighborhood* is the share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey.

interact with significantly more Blacks and, if anything, are less productive in Black stops ($p > 0.1$).

Figure 2 provides the first piece of (visual) evidence linking neighborhoods and racial differences in policing. A shortcoming of these raw correlations is that they do not account for crucial observable characteristics. For instance, a key variable that explains the racial composition of FIs is the racial composition of the neighborhoods in which they take place — i.e., officers will interact with more Blacks if they patrol Black areas. A standard regression analysis can account for that.

Regression analysis. I estimate the model below by ordinary least squares (OLS):

$$Y_{it} = \beta ShrBlackN_{i(o)t} + X_{it}^{'}\gamma + \epsilon_{it} \tag{1}$$

where Y is an outcome of interest (i.e., the share of Black individuals in a police interaction) for FI i, taking place in year t. *ShrBlackN* denotes the share of the Black population in the neighborhood (census block group) of officer o (the officer in charge of i) in year t. The coefficient on this variable, β, is the main parameter of interest. X is a vector of controls and a constant that captures FI i's specific circumstances. These contain officers' tenure at the PD and may include year and FI ZIP code fixed effects — the latter account for differences in the racial composition of FI neighborhoods. Officer's residence ZIP code or census tract fixed effects may also be included. Standard errors are clustered at the officer's tract-year level. Tables A2 and 3 show the regression results.

Table 3: Neighborhoods and outcomes of all-Black FIs

	Arrest		Summons		Drugs		Weapon		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. White officers												
Share Black in	0.003	0.008	0.010**	0.017**	0.003**	0.006**	0.005**	0.014***	0.010	0.013	-0.000	0.028**
Neighborhood	(0.004)	(0.008)	(0.004)	(0.008)	(0.001)	(0.003)	(0.002)	(0.005)	(0.008)	(0.015)	(0.007)	(0.012)
DV Mean	0.060	0.060	0.096	0.096	0.019	0.019	0.029	0.029	0.330	0.330	0.186	0.186
N	38743	38743	38743	38743	38743	38743	38743	38743	38743	38743	38743	38743
B. Black officers												
Share Black in	0.001	-0.007	0.001	0.013**	0.000	-0.001	0.003**	-0.001	0.018**	0.038***	0.002	0.019**
Neighborhood	(0.002)	(0.005)	(0.004)	(0.006)	(0.001)	(0.002)	(0.001)	(0.001)	(0.007)	(0.012)	(0.006)	(0.010)
DV Mean	0.043	0.043	0.083	0.083	0.014	0.014	0.021	0.021	0.358	0.358	0.174	0.174
N	9245	9245	9245	9245	9245	9245	9245	9245	9245	9245	9245	9245
C. Hispanic officers												
Share Black in	-0.010**	-0.010	0.011	0.044	-0.002	0.015	-0.002	0.002	0.013	0.064	-0.017	0.017
Neighborhood	(0.004)	(0.011)	(0.008)	(0.029)	(0.002)	(0.011)	(0.002)	(0.010)	(0.010)	(0.044)	(0.010)	(0.030)
DV Mean	0.045	0.045	0.084	0.084	0.018	0.018	0.027	0.027	0.351	0.351	0.168	0.168
N	4165	4165	4165	4165	4165	4165	4165	4165	4165	4165	4165	4165
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X		X		X

Notes: The sample includes all FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the city. Columns 5-8 restrict the sample to all-Black FIs only. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Weapon* and *Drugs* are indicators taking the value of one if the FI resulted in weapons or drugs found. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. *Share Black in Neighborhood* is the standardized share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Regression results confirm the presence of significant differences in the racial composition of police interactions depending on officers' race and where they live. First of all, Table A2 shows that, on average, Black officers stop more Blacks than White and Hispanic officers (67% vs. 61% and 58%). Columns 1-4 in Table A2 investigate whether variation in the racial demographics of officers' residence neighborhoods can explain any of these baseline differences. Panel A provides weak supporting evidence for White officers; those in blocks with more Blacks interact with relatively fewer Blacks (up to 1pp less, $p > 0.1$).[7] Panel B provides stronger evidence for Black officers, who interact with significantly fewer Black civilians when residing in Black areas (up to 2.5pp less). That difference almost vanishes when accounting for FI or officers' neighborhood characteristics (Columns 2-3) but persists when comparing officers within a census tract (Column 4). Finally, evidence of neighborhood differences is also strong among Hispanic officers (Panel C); those in Black neighborhoods interact with *more* Black civilians (up to 6pp, Column 1). However, that difference vanishes, and even reverts, when including FI ZIP or census tract fixed effects in the regressions. These suggest that Hispanic officers in Black neighborhoods are more likely to patrol Black areas, which explains the positive slope previously observed in the bottom left panel of Figure 2.

Black interactions from White officers living in Black neighborhoods are more productive. Columns 5 and 6 show that officers in blocks with more Blacks are 1.5pp (10%) more productive. That is the raw difference without accounting for disparities in officers' performance across neighborhoods. When doing so, Columns 7 and 8 reveal that these differences grow to 3pp (20%). Panel A in Table 3 shows that positive differences across all productivity subcomponents drive these effects: Black interactions from White officers in blocks with more Blacks are more likely to result in arrests (up to 0.8pp, $p > 0.1$), summons issued (1.7pp), or in drugs or weapon found (0.6pp and 1.4pp, respectively). Columns 9-12 suggest these productivity increases are coupled with mild increases in the "violence" of interactions, as frisks and searches — forms of low-intensity violence — are more prevalent (1.3pp, $p > 0.1$, and 3pp, respectively).

Black and Hispanic officers residing in Black neighborhoods are not systematically more productive in Black interactions. Columns 5-8 in Panels B and C in Table A2 display no significant differences in the productivity of all-Black stops among officers of these two races. The same panels in Table 3 exhibit differences for some outcomes among Black officers (e.g., those in Black blocks are 1pp more likely to summons), but these do not

[7] In this and the upcoming tables, the variable *Share Black in Block Group* is standardized. Therefore, the coefficients ought to be interpreted as the effect of increasing the share of the Black population in officers' block groups from 10% (mean in the sample) to 28% (SD of 0.18).

appear to be robust and are sensitive to the inclusion or lack thereof of census tract fixed effects. Black and Hispanic officers in Black neighborhoods interact with relatively fewer Blacks, and when they do, interactions are not differently productive.

The previous results provide suggestive evidence that local environments matter. White officers more exposed to Black individuals in their neighborhoods are somewhat less likely to interact with Black civilians in police stops. Conditional on a Black interaction, these are up to 3pp more productive — more likely to result in contraband found or in a summons. These differences in productivity are achieved without significant differences in frisks and searches (i.e., thereby, with a higher frisk and search success rate).

Different mechanisms can reconcile the previous findings. Endogenous sorting is one of them. A first explanation is that social interactions at the local level between White officers and out-group (Black) neighbors make them less prejudiced (Allport et al. 1954). It is also possible that these interactions teach them some skill about out-group individuals that enhances their productivity. For example, they could get better at extracting and interpreting signals. Finally, sorting is another strong contender. White officers *choosing* to reside in Black neighborhoods are likely to differ from those that choose not to in unobservable characteristics. For example, they may be more tolerant or hold more positive attitudes toward Black communities, which could explain their residence location choice and the higher productivity in Black interactions. The first two stories, albeit through different mediating mechanisms, imply a causal link between neighborhoods and police outcomes. The latter does not.

City's housing market is slack, which should significantly alleviate endogeneity concerns, especially in specifications exploiting within-tract variation (Bayer et al. 2008). However, ultimately, only a plausible exogenous shift in the local racial composition can fully address all concerns and disentangle the effects of local environments from those arising from selection in sorting.

5 Quasi-experimental approach: new neighbor design

5.1 Overview and identification

I exploit quasi-random variation in the racial composition of next-door neighbors to address endogeneity. The basic premise, illustrated in Figure A1, is that officers may choose their initial location within a neighborhood based on their preferences. However, once settled, they cannot control or predict whether their immediate neighbors move out and who subsequently moves in. Variation in the race of their close neighbors is plausibly

exogenous to them. Identification hinges on that *within-neighborhood* variation.

The focus of this section is on White officers living in City. The geographic restriction is due to the fact that I can only observe changes in ownership in City property registry data, from which I could construct an address-owner-year panel from 2010 to 2021. For State, I only observe a cross-section as of January 2021. The focus on White officers responds to three reasons. First, I do not have a clear hypothesis on how Black officers would react to increased local exposure to individuals from their in-group.[8] Second, most FIs (almost 80%) are carried by White officers — consequently, they are the most relevant group to study. Finally, the sample of Black (and Hispanic) officers is significantly smaller, so precision is a concern in such a demanding research design.

Figure 3 shows the distribution of dwellings with a non-Black-to-Black ownership transition in the city from 2011 to 2020.[9] There are transitions throughout the whole period and across the entire city. However, their concentration is slightly higher in the south (of the figure), where the Black population is also higher.

I estimate the following model:

$$Y_{it} = \tau NewBlackNeigh_{i(o)t} + X_{it}^{'}\gamma + \epsilon_{it} \tag{2}$$

where $NewBlackNeighbor$ is an indicator taking the value of one if a new Black owner moved in a previously non-Black-owned and occupied dwelling "next-door" (within 100 meters in the baseline) to the residence of officer o, conducting FI i, between 2011 and t. X includes a constant, tenure, year fixed effects, and geography (ZIP or census tract) or officer fixed effects. The remaining variables are defined as before. Standard errors are also clustered at the officer census tract-year level. The concerns brought up by the new literature on two-way fixed effects apply in this setting (see De Chaisemartin and d'Haultfoeuille (2022b) for a recent review). Therefore, τ is estimated using the imputation estimator introduced in Borusyak et al. (2024).

The identification assumption is that, within a census tract and conditional on a set of covariates, exposure to a new "next-door" neighbor of a different race is quasi-random. Table A3 provides the first piece of quantitative supporting evidence for identification by showing that, within a census tract, officers exposed to a new Black neighbor are not observationally different from those unexposed. A threat to identification would occur if Black owners relocating next to officers' homes were systematically different in unobservables from those relocating in other parts *of the same census tract*. The spatial

[8]For completeness and the interested reader, I investigate and provide the results from this exercise in Appendix C.

[9]The majority of these "events" are White-to-Black transitions, as an Appendix table makes clear later.

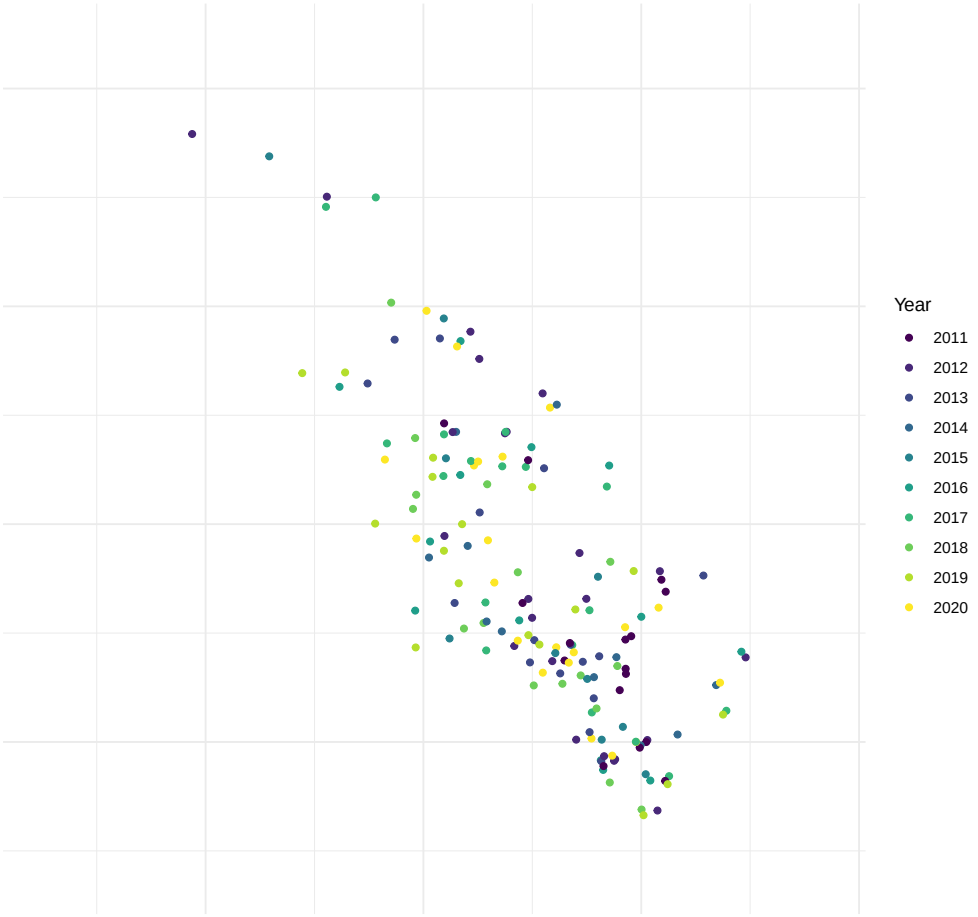


Figure 3: Distribution of Black events in City (2011-2020)

Notes: Each point in the map represents the location of a "Black event" — a change in property ownership involving a change in race, from non-Black to Black. Only owner-occupied properties are considered. Darker colors denote earlier events. Lighter colors represent more recent events. To keep the city undisclosed, the plot axis is randomly flipped, and the map scale is undisclosed and distorted (as in Figure 1).

granularity in the research design, combined with the slackness of the housing market (i.e., when moving, it is possible to target a neighborhood but difficult to choose an exact address), makes this scenario (albeit impossible to rule out) rather unlikely.[10]

5.2 Results

White officers exposed to new Black neighbors interact with fewer Black civilians in FIs. These fewer interactions are more productive. Panel A in Table 4 shows τ estimates from

[10]This is the same identification strategy and logic followed in Bayer et al. (2022). A key difference is that they exploit variation at the census block level. My sample size is too small for such a design.

Equation 2 with variations in the battery of fixed effects included throughout specifications. Coefficients range from −15pp in Column 1 (with only year fixed effects) to −16pp in Column 4 (where officer fixed effects are included). My preferred specification is Column 3, which includes year and officer census tract fixed effects (and thus mirrors the toy example and design from Figure A1). It shows a reduction in Black interactions of 15pp (25% of the mean). Finally, Panel B in the same Table shows an 10pp (over 60%) increase in the productivity of all-Black FIs in my preferred specification (Column 4).

Table 4: White officers' new Black neighbors and racial differences in FIs

Panel A. Racial composition of interactions

	Share Black in FI			
	(1)	(2)	(3)	(4)
New Black neighbor	-0.148***	-0.161***	-0.154***	-0.160***
	(0.008)	(0.009)	(0.011)	(0.011)
DV Mean	0.612	0.612	0.612	0.612
N	67437	67437	67437	67437

Panel B. Productivity of all-Black interactions

	Productive			
	(1)	(2)	(3)	(4)
New Black neighbor	0.114***	0.109***	0.100***	0.120***
	(0.010)	(0.011)	(0.015)	(0.014)
DV Mean	0.163	0.163	0.163	0.163
N	38873	38873	38873	38873
Year FE	X	X	X	X
FI ZIP FE		X	X	X
Officer ZIP FE		X		
Officer Tract FE			X	
Officer FE				X

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *New Black Neighbor* is an indicator taking the value of one if a Black individual moves into a previously non-Black-owned and occupied dwelling within 100 meters of the officer's residence. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

No differential pre-trends exist between treated and untreated officers within the same census tract. Figure 4 shows treatment effects estimates from the following staggered differences-in-differences event study design, again, applying the methodology intro-

duced in Borusyak et al. (2024):

$$Y_{it} = \sum_{h=-3}^{2} \tau_h \mathbf{1}[K_{i(o)t} = h] + X_{it}^{'}\gamma + \epsilon_{it} \tag{3}$$

where $\mathbf{1}[K_{i(o)t} = h]$ is an indicator taking the value of one if FI i carried by officer o occured in event-time h (e.g., $h = 0$ denotes the event-year of the move). X is defined as before. The top panel shows no statistically significant differences in the share of Black civilians in FI interactions between treated and untreated officers before an event. That changes after exposure to a new Black neighbor, when treated officers reduce their share of Black interactions by up to 20pp. Similarly, the bottom panel shows no significant differences in the productivity of Black interactions before the event, followed by subsequent productivity increases exceeding 20pp two years after an event.

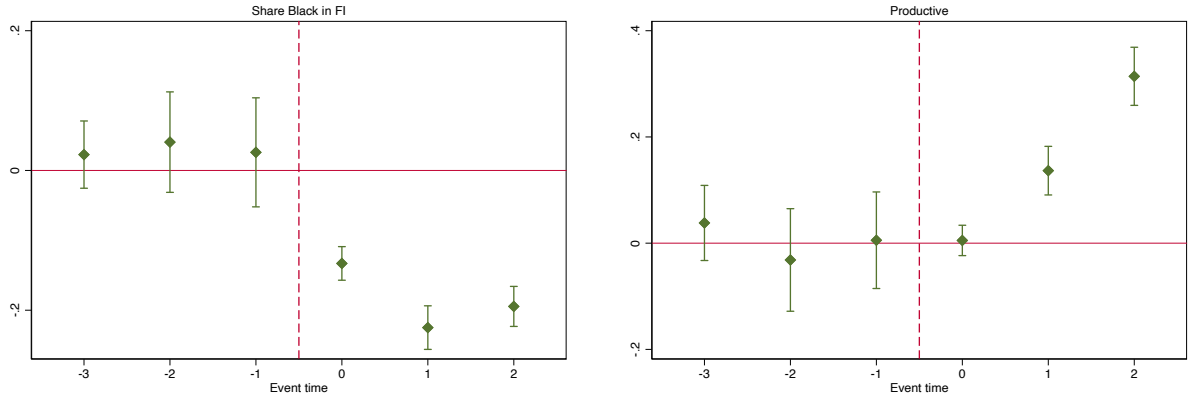


Figure 4: White officers' new Black neighbors and racial differences in FIs

Notes: The plot illustrates τ_h estimates from Equation 3. Vertical bars represent 95% confidence intervals. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. The estimation sample in the bottom plot is restricted to all-Black FIs only. Standard errors clustered at the tract-year level.

There are productivity increases across most subcomponents. Table 5 shows that arrests, summons, and drugs seized increase after treatment. Seizing weapons, if anything, becomes less likely (-3pp in the specification with officer fixed effects). The table also reveals that most productivity increases can be attributed to summons, which become 9pp more likely. The first four panels in Figure 5 show, again, no pre-trends on any of the previous outcomes analyzed. They also show that productivity increases take two to three years to materialize, sometimes following an initial "bump" (for example, in the case of arrests).

In addition to being more productive, Black interactions become less violent. Columns 9-12 in Table 5 show that frisks and searches (forms of low-intensity violence) decrease by up to 7pp. The last two panels in Figure 5 display no significant differences in the pre-trends for these outcomes (except for searches two years before an event) and provide some hints on how the time variation in frisks and searches lead to different outcomes. We see an immediate drop in frisks (and, to a lesser extent, searches) that coincides with a drop in the likelihood of arrests and contraband seized. After two years, frisks (and searches) are on the rise again, and that coincides with significant jumps in the likelihood of contraband seized, summons, and (especially) arrests. The latter sections of the paper shed more light on the mechanisms driving these changes in FI outcomes. Before that, I explore the sensitivity of the main results up until now to several robustness tests.

Table 5: White officers' new Black neighbors and outcomes of all-Black FIs

	Arrest		Summons		Weapon		Drugs		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New Black neighbor	0.022**	0.011	0.078***	0.112***	-0.003	-0.030***	0.030***	0.018***	-0.076***	-0.032**	-0.019	-0.022
	(0.009)	(0.009)	(0.010)	(0.012)	(0.004)	(0.007)	(0.004)	(0.005)	(0.019)	(0.016)	(0.017)	(0.015)
DV Mean	0.060	0.060	0.096	0.096	0.029	0.029	0.018	0.018	0.330	0.330	0.186	0.186
N	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE	X		X		X		X		X		X	
Officer FE		X		X		X		X		X		X

Notes: The sample includes all-Black FIs from White officers in the city. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Drugs* and *Weapon* are indicators taking the value of one if drugs or weapons were found during the police interaction, respectively. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. *New Black Neighbor* is an indicator taking the value of one if a Black individual moves into a previously non-Black-owned and occupied dwelling within 100 meters of the officer's residence. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

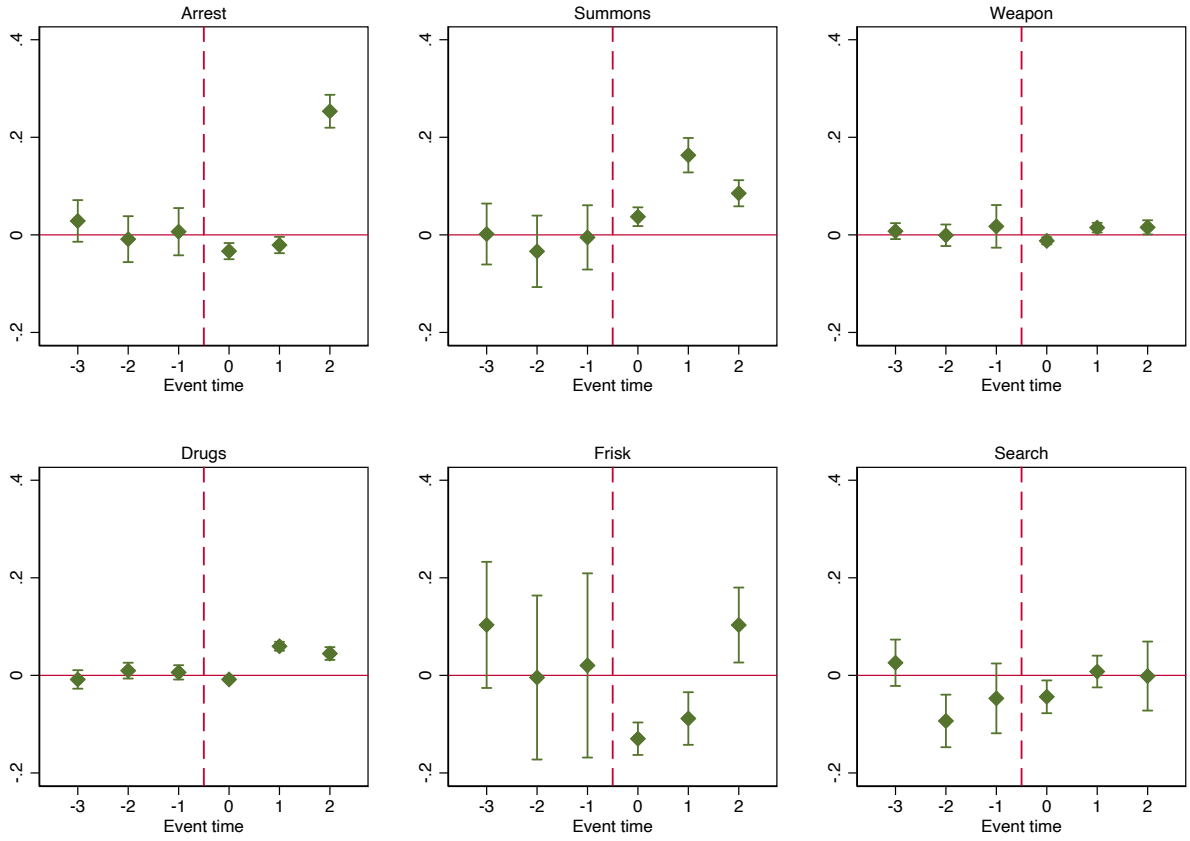


Figure 5: White officers' new Black neighbors and outcomes of all-Black FIs

Notes: The plot illustrates τ_h estimates from Equation 3. Vertical bars represent 95% confidence intervals. The estimation sample is restricted to all-Black FIs from White officers. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or a summons was issued during the police interaction. *Drugs* and *Weapon* are indicators taking the value of one if drugs or weapons were found during the police interaction, respectively. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. Standard errors clustered at the tract-year level.

5.3 Robustness

Alternative race assignment thresholds. Results are robust to alternative race assignment thresholds for new neighbors. Race information is not available in the City or State property registry, and I, therefore, rely on names and race cross-tabulations from the US Decennial Census. In my baseline results, I set a confidence threshold of 70% to assign races. These correspond to estimates in Column 3 from Table A4. Columns 1-2 in that table re-assess the effects of having a new Black neighbor applying lower race assignment thresholds (60 or 65%). Results are similar to the baseline when looking at changes in the

racial composition of FIs (the effect drops from 15 to 12pp). There are larger discrepancies in productivity (Panel B). The coefficient remains large and significant (9pp, $p < 0.01$) at the 65% threshold, but it drops sharply and (albeit remaining positive) loses significance at the lowest threshold (2pp, $p > 0.1$). Note that lowering the race assignment threshold implies a higher rate of false positives (untreated officers misclassified as treated). Columns 4 and 5 experiment with higher race thresholds (thus implying fewer false positives but more false negatives). Results yield the reverse pattern — FIs involve fewer Black civilians (up to 18pp less), and when they do, they are slightly more productive (up to 10pp more). Overall, the effects across columns are (at least qualitatively) very similar.

A caveat in the previous exercise is that changing race assignment thresholds affects the likelihood of false positives and negatives and may also introduce varying degrees of bias. The reason is that having a more Black-sounding name may correlate with several socioeconomic characteristics, which could impact how officers' interactions with these individuals affect their racial attitudes and behavior in FIs.

Race inferred from mortgage data. I use mortgage microdata from the Home Mortgage Disclosure Act (HMDA) to infer the race of new owners and replicate baseline results in Table A5. Figure A2 plots the results in an event-study format, showing no significant pre-trends in the outcomes studied (except for frisks in one of the pre-periods). The HMDA data contains information on the universe of mortgages in the United States. I retrieved all mortgage loans originating in City from 2010-21 and matched a subsample of them to a property through real estate transactions data from the state registry of deeds (see Appendix B for the details). The main advantage of using the HMDA data is that race is self-reported by mortgage applicants and is, consequently, more precise than any prediction relying on names. The main disadvantage is that it is not possible to match all property ownership changes to a loan either because (i) the transaction did not originate a loan or (ii) there were too many "similar" ownership changes or loans in a census tract in a given year to assign a loan to an ownership change confidently.

This exercise yields some differences from baseline results, but the primary messages remain: White officers with a new Black neighbor interact with relatively fewer Blacks ($-$8pp), and are more productive in Black stops (6pp). As before, productivity increases are driven by positive shifts in the likelihood of arrests (1pp, $p > 0.1$), summons (3pp), and drugs (3pp). Unlike before, the likelihood of seizing weapons also increases (5pp), and the shift in summons is significantly lower (3 vs. 8pp). Also, overall productivity increases do not (statistically) survive in the alternative specification with officer fixed effects (due to a decrease in the likelihood of summons), and, if anything, frisks and searches become more

common. Differences in the allocation of officers into the treatment and control groups (due to the limitations inherent in both approaches to assigning race to individuals) are behind these discrepancies with baseline results.

Alternative new neighbor race transitions. White-to-Black transitions drive the main results. Table A6 explores the sensitivity of baseline results to alternative "new neighbor" events. Columns 1 through 6 consider the effects of experiencing one of the following transitions in close neighbors: non-White-to-White, White-to-White, non-Black-to-Black (the baseline), White-to-Black, non-Hispanic-to-Hispanic, and non-Asian-to-Asian. Panel A shows that only White-to-Black transitions yield significant differences in the racial composition of FIs. Similarly, Panel B shows that White-to-Black transitions drive baseline productivity effects. That panel also suggests that other transitions (particularly those from a non-White to a White neighbor) might have a small impact on productivity in the opposite direction.

Alternative treatment distances. New neighbors moving in beyond 200 meters from officers' homes have little impact. Table A7 explores variations in the definition of "next-door." The baseline definition (Column 2) estimates the impact of new neighbors within 100 meters of officers' dwellings. That corresponds to a walking distance of just under a minute from officers' doorsteps. Column 1 considers an even smaller treatment distance (50 meters). Columns 3-5 consider distances of 200, 350, and 500 meters, respectively. Panel A shows that only treatment distances under 200 meters are associated with sizable changes in the racial composition of FIs (for 50 meters, the effect is negative but insignificant). Similarly, Panel B shows significant increases in productivity for distances of 100 meters and under, and small (marginally significant) decreases in the 200-350 meter range. Overall, results suggest that changes in the neighbors living farther away matter little. The smaller effects in the 50-meter treatment window for FI's racial compositions could respond to two reasons. First, the 50-meter cutoff might be missing some of the actual "next-door" (or across-the-street) neighbors. I computed distances between dwellings using their centroid as the reference point. Therefore, my approach is overstating actual distances between dwellings (as centroids will be in their interior). Second, if having a new neighbor within 100 or 200 meters impacts racial differences in policing, then classifying these treated officers as untreated will bias estimates downwards.

Appendix C shows that baseline results are robust to several additional tests. Among others, these include re-estimating specifications with alternative two-way fixed effects methods, accounting for spatial autocorrelation, or restricting to "new neighbors" that

effectively moved in the same street.

5.4 Mechanisms

Heterogeneity. Different officer subsamples respond differently to the arrival of a new neighbor. Columns 1-2 in Table 6 show that officers in ZIP codes with fewer Black population (below 10%) react with higher intensity. The relative fraction of Blacks in their FIs decreases by almost 18pp (as opposed to 7pp in the subsample of officers in "more Black" ZIPs). The productivity of their all-Black FIs is also higher (12 vs. 1pp). Similarly, Columns 3-4 show that new neighbor effects are stronger among officers with less (than seven years of) experience. They reduce their interactions with Blacks by more (20 vs. 5pp) and see larger increases in their productivity (12 vs. 1pp). Columns 5 and 6 reveal that the effects are larger among the most active officers (with more than 200 FIs over the entire sample period). They reduce the relative share of Blacks in their interactions by almost 40pp (as opposed to 3pp) and increase their productivity by 19pp (vs. 6pp). Columns 6 and 7 investigate party affiliation as a proxy for ideology (Democrat-registered officers are probably more liberal than non-affiliated or Republican-registered officers). They show that Democratic officers stop more Blacks (11pp) after treatment, and these stops are significantly more productive (12pp). In contrast, non-affiliated officers interact with significantly fewer Black civilians (22pp), and these interactions are also more productive (12pp). A caveat and possible explanation for the results on Democratic officers is the small size of that subsample — most officers are not affiliated with any party.[11] Finally, Columns 9 and 10 suggest that exposure to higher-income Black neighbors has a larger impact on FIs (racial composition and productivity). These two columns estimate the results for the subsample of officers with the closest new Black neighbor moving into a dwelling with a higher (Column 9) or lower (Column 10) assessed value than that of the officer. Relative to officers with poorer (as proxied by the value of their home) new neighbors, those with more affluent neighbors interact with significantly fewer Blacks (-19 vs. -7pp), and these interactions are more productive (12 vs. 5pp).

This exercise indicates stronger reactions to the arrival of a new Black neighbor among officers in "more White" neighborhoods, with less experience, more prolific in the number of FIs, and not affiliated to any party. Exposure effects are larger when new neighbors are more affluent.

[11] In fact, there are not enough Republican-registered officers to estimate treatment effects for that subgroup.

Table 6: Mechanisms: heterogeneity

Panel A. Racial composition of interactions

	Share Black in FI									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
New Black neighbor	-0.177***	-0.070***	-0.198***	-0.051*	-0.033**	-0.381***	0.112***	-0.223***	-0.190***	-0.069***
	(0.013)	(0.019)	(0.013)	(0.027)	(0.015)	(0.019)	(0.033)	(0.012)	(0.011)	(0.017)
DV Mean	0.623	0.597	0.620	0.602	0.548	0.648	0.569	0.624	0.658	0.573
N	39404	28033	37725	29712	24051	43386	NA	NA	31340	36097

Panel B. Productivity of all-Black interactions

	Productive									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
New Black neighbor	0.123***	0.008	0.124***	0.012	0.060***	0.185***	0.120***	0.116***	0.119***	0.052**
	(0.020)	(0.033)	(0.021)	(0.025)	(0.013)	(0.031)	(0.032)	(0.016)	(0.027)	(0.020)
DV Mean	0.164	0.163	0.179	0.144	0.191	0.151	0.131	0.165	0.175	0.152
N	23136	15737	21911	16962	12358	26515	NA	NA	19453	19420
Year FE	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X
Officer Tract FE	X	X	X	X	X	X	X	X	X	X
Sample	ShrB<10%	ShrB>10%	Exp<7y	Exp>7y	<200 FIs	200+ FIs	Democrat	Non-reg	Rich NBN	Poor NBN

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs. All outcomes and *New Black Neighbor* are as previously defined. Columns 1 and 2 restrict the sample to officers in ZIP codes with a Black population below or above 10%. Columns 3 and 4 restrict the sample to officers with less or more than seven years of experience. Columns 5 and 6 restrict the sample to officers carrying less or more than 200 FIs over the sample period. Columns 7 and 8 restrict the sample to registered Democrats or non-party affiliated officers. The subsample of Republican-registered officers is too small to estimate treatment effects. The number of observations in these two columns is undisclosed for confidentiality reasons. Columns 9 and 10 restrict the sample to officers with their closest new Black neighbor dwelling in a home with a higher or lower assessed value than theirs. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Aggregate effects. White officers with a new Black neighbor interact with more Whites. Table 7 digs deeper into the mechanisms driving the main results by looking at the aggregate number of FIs, individuals stopped, arrests, and other productivity measures. It reveals that White officers make, on average, two extra FIs per year ($p > 0.1$) and interact with five extra White civilians. Their number of Black interactions does not change significantly – if anything, it decreases. Therefore, the reduced share of Black civilians in FIs documented previously is driven by more White stops. Panel B looks at the yearly productivity of all-Black FIs. On average, in a year, a treated White officer makes 0.2 additional arrests ($p > 0.1$), issues one extra summons, and seizes drugs in 0.2 additional instances.

Table 7: Mechanisms: aggregate (officer-year level) results

Panel A. FIs and number of individuals

	Total FIs		Total Individuals		Total Black Ind.		Total White Ind.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	1.822	10.059***	1.435	10.972**	-2.933	3.102	5.142***	6.130***
	(1.977)	(3.484)	(3.129)	(4.662)	(1.932)	(3.369)	(1.296)	(1.275)
DV Mean	24.331	24.331	35.449	35.449	21.950	21.950	6.725	6.725
N	2805	2805	2805	2805	2805	2805	2805	2805

Panel B. Productivity of all-Black FIs

	Total Arrests		Total Summons		Total Weapons		Total Drugs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	0.240	-0.112	0.893**	1.930***	0.015	-0.061	0.209***	0.265***
	(0.368)	(0.430)	(0.425)	(0.493)	(0.074)	(0.107)	(0.071)	(0.068)
DV Mean	1.203	1.203	1.795	1.795	0.392	0.392	0.249	0.249
N	2342	2342	2342	2342	2342	2342	2342	2342
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X		X		X		X	
Officer Tract FE	X		X		X		X	
Officer FE		X		X		X		X

Notes: The sample includes White officers in the city. An observation is an officer-year pair. Panel A describes the total number of FIs and individuals by race (Black or White) in police interactions in a given year. Panel B describes the productivity of all-Black FIs in terms of the number of times a FI leads into arrests, summons issued, and weapons or drugs found in a given year. *New Black Neighbor* as previously defined. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

There is little evidence of officers changing the neighborhoods they patrol or moving after the arrival of a new neighbor. Columns 1-4 in Table A8 show no differences in the

demographics of neighborhoods officers patrol (proportion of Black or White population). Columns 5-6 reveal no differences in overall neighborhood crime, although Columns 7-8 suggest they patrol areas with less violent crime (0.4pp, $p < 0.05$). Unlike the results in Bayer et al. (2022) for North Carolina, Columns 9 and 10 suggest that officers' propensity to move does not change after a new neighbor of a different race arrives.

This exercise is helpful to assess the "aggregate" effects of the new neighbor treatment. However, an important caveat to bear in mind when aggregating the data at the officer-year level is that critical (contextual) information is lost in the process. For example, it is no longer possible to account for the share of Black individuals in the neighborhood of a given FI — only a yearly average or mode (e.g., for the ZIP code) can be used.

6 Caveats and discussion

Black owners are not representative. Black owners are, at least, positively selected regarding income and wealth. Therefore, the results presented in this paper speak about the effects of White officers interacting with wealthier Black individuals but may not be as informative about what happens when they interact with less wealthy individuals (such as, usually, renters).

Heterogeneity results pointed in this direction. Columns 9-10 in Table 6 suggested that the impact of a new Black neighbor was "more positive" when they were more affluent. In the lens of the contact hypothesis (Allport et al. 1954), one could interpret these findings as indicating that exposure to out-group individuals positively shifts attitudes more when these are also positively selected in other dimensions (e.g., income). That interpretation, supported by recent evidence (Carrell et al. 2019), can also help reconcile differences between descriptive and quasi-experimental results. Namely, the former investigates the association between exposure to all individuals in an ethnic group and police outcomes, whereas the latter only assesses the impact of a (positively) selected subsample. Under the presence of heterogeneity, the two effects could differ.

Racial bias or learning? This paper provides plausible evidence of the causal effects of officers' local environments on racial differences in policing. Two channels may explain these effects. On the one hand, exposure to Black civilians may improve White officers' racial attitudes. On the other hand, that same out-group exposure could make White officers more proficient in extracting or interpreting the signals they receive when encountering Black civilians while patrolling, resulting in changes in the decision to interact with them and the enhanced productivity of these interactions.

Disentangling between the two is challenging because both mechanisms are observationally equivalent (i.e., they yield the same outcomes). They are also non-mutually exclusive, so increased contact could affect outcomes through both channels. However, two results suggest the presence of (at least) some shift in attitudes. First, Table 6 revealed larger effects when new neighbors were more affluent. Given the documented negative association between income and crime, a model with learning would imply officers becoming more proficient at signal extraction when in contact with less affluent individuals, and not the opposite. Second, a text analysis exercise (in Appendix C) reveals a (positive) change in the sentiment of police narratives written by treated officers. Albeit not entirely conclusive, it provides evidence most consistent with a shift in attitudes, as the learning channel should not impact narratives' sentiment.

Effect size and comparison with the literature. Small interventions can have large effects. Table A9 summarizes some of the impacts of police interventions and training on policing outcomes documented in the (recent) literature. A general takeaway is that relatively small interventions can have large effects. For example, Dube et al. (2023) study the impact of four 4-hour sessions on situational decision-making, a training designed to help officers evaluate all possible alternatives in stressful situations and avoid rash actions. They find that, within six months of the training, discretionary arrests decreased by 23% (and more for Blacks) and officer injuries by almost 50%. Rivera (2022), a paper closer in spirit to mine, finds that higher exposure to racial minorities during the (6 months of) police academy resulted in a 16% reduction in low-level and quality arrests and a 9% increase in high-quality arrests within four years of training. Finally, Owens et al. (2018) and Weisburd et al. (2022) suggest that short procedural justice training can also substantially affect arrest rates. My results are not directly comparable (due to the different nature of outcomes and time span), but I find that treated officers make 0.2 additional arrests per year (20%, $p > 0.1$), issue 0.9 additional citations (50%), and find drugs 0.2 additional times per year (80%) (Table 7). The latter is a large effect, in part driven by these outcomes being rare events (officers find drugs 0.2 times per year on average). As with Dube et al. (2023), I find a reduction in arrests in the first year following the treatment (Figure 5). In contrast with Rivera (2022), I find that arrests increase overall (non-significantly), and, unfortunately, I cannot link arrests to court outcomes, so it is difficult to assess the overall quality of arrests. As with Owens et al. (2018) and Dube et al. (2023), I find suggestive evidence that the use of force decreases as frisks become less prevalent (Table 5).

7 Conclusions

This paper investigated the demographic characteristics of police officers' neighborhoods and their impact on racial differences in policing, in the context of a large American city. In contrast to Black or Hispanic officers, White officers are spatially clustered and tend to live in more White neighborhoods. Descriptive associations revealed that White officers in Black neighborhoods do not systematically stop fewer Blacks (who are overrepresented in police stops, as in other contexts) but are relatively more productive when they do so. Quasi-experimental variation in the race of officers' new next-door neighbors confirmed the results on productivity and further revealed an impact on the racial composition of stops (relatively fewer Black civilians stopped). The impact of a new Black neighbor was larger among officers in White neighborhoods, with less experience, and with more affluent new neighbors.

This research implies that racial segregation may be a cause and an amplifier of racial differences in policing. Segregation, by construction, hampers the contact between individuals of different groups. The reduced contact could directly impact racial attitudes or impede the acquisition of some form of productivity-enhancing knowledge. Whether through one channel or the other, this work gives policymakers another reason to carefully consider the consequences of policies impacting segregation, such as zoning or public housing. A second direct implication is that incentivizing police officers to reside in more diverse communities (for example, through subsidized housing) could be a promising policy to reduce racial disparities in policing.

References

Algan, Yann, Camille Hémet, and David D Laitin, "The social effects of ethnic diversity at the local level: A natural experiment with exogenous residential allocation," *Journal of Political Economy*, 2016, *124* (3), 696–733.

Allport, Gordon Willard, Kenneth Clark, and Thomas Pettigrew, "The nature of prejudice," 1954.

Ananat, Elizabeth Oltmans, "The Wrong Side(s) of the Tracks: The Causal Effects of Racial Segregation on Urban Poverty and Inequality," *American Economic Journal: Applied Economics*, April 2011, *3* (2), 34–66.

Ang, Desmond, Panka Bencsik, Jesse M Bruhn, and Ellora Derenoncourt, "Community Engagement with Law Enforcement after High-profile Acts of Police Violence," Technical Report, National Bureau of Economic Research 2024.

Antonovics, Kate and Brian G Knight, "A new look at racial profiling: Evidence from the Boston Police Department," *The Review of Economics and Statistics*, 2009, *91* (1), 163–177.

Anwar, Shamena and Hanming Fang, "An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence," *American Economic Review*, March 2006, *96* (1), 127–151.

Arnold, David, Will Dobbie, and Crystal S Yang, "Racial bias in bail decisions," *The Quarterly Journal of Economics*, 2018, *133* (4), 1885–1932.

Ba, Bocar A, Dean Knox, Jonathan Mummolo, and Roman Rivera, "The role of officer race and gender in police-civilian interactions in Chicago," *Science*, 2021, *371* (6530), 696–702.

Ba, Bocar, Jacob Kaplan, Dean Knox, Mayya Komisarchik, Rachel Mariman, Jonathan Mummolo, Roman Rivera, and Michelle Torres, "Who are the police? Descriptive representation in the coercive arm of government," *Working Paper*, 2022.

Bayer, Patrick, Marcus D Casey, W Ben McCartney, John Orellana-Li, and Calvin S Zhang, "Distinguishing Causes of Neighborhood Racial Change: A Nearest Neighbor Design," Technical Report, National Bureau of Economic Research 2022.

____ **, Stephen L Ross, and Giorgio Topa**, "Place of work and place of residence: Informal hiring networks and labor market outcomes," *Journal of Political Economy*, 2008, *116* (6), 1150–1196.

Bazzi, Samuel, Arya Gaduh, Alexander D Rothenberg, and Maisy Wong, "Unity in diversity? how intergroup contact can foster nation building," *American Economic Review*, 2019, *109* (11), 3978–4025.

Becker, Gary, "The economics of discrimination," 1957.

Billings, Stephen B and Kevin T Schnepel, "Hanging out with the usual suspects: Neighborhood peer effects and recidivism," *Journal of Human Resources*, 2020, pp. 0819–10353R2.

Billings, Stephen B., David J. Deming, and Jonah Rockoff, "School Segregation, Educational Attainment, and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg," *The Quarterly Journal of Economics*, 09 2013, *129* (1), 435–476.

____ **,** ____ **, and Stephen L. Ross**, "Partners in Crime," *American Economic Journal: Applied Economics*, January 2019, *11* (1), 126–50.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 2024, p. rdae007.

Bursztyn, Leonardo, Thomas Chaney, Tarek Alexander Hassan, and Aakaash Rao, "The Immigrant Next Door: Exposure, Prejudice, and Altruism," Working Paper 28448, National Bureau of Economic Research February 2021.

Carrell, Scott E, Mark Hoekstra, and James E West, "The impact of college diversity on behavior toward minorities," *American Economic Journal: Economic Policy*, 2019, *11* (4), 159–182.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Difference-in-differences estimators of intertemporal treatment effects," Technical Report, National Bureau of Economic Research 2022.

____ **and** ____ , "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey," Technical Report, National Bureau of Economic Research 2022.

Chapelle, Guillaume, Gerard Domènech-Arumí, and Paula Eugenia Gobbi, "Housing, Neighborhoods, and Inequality," *Handbook of Labor, Human Resources and Population Economics*, 2023, pp. 1–34.

Chetty, Raj and Nathaniel Hendren, "The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects," *The Quarterly Journal of Economics*, 2018, *133* (3), 1107–1162.

____ , ____ , and Lawrence F Katz, "The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment," *American Economic Review*, 2016, *106* (4), 855–902.

____ , ____ , Patrick Kline, and Emmanuel Saez, "Where is the land of opportunity? The geography of intergenerational mobility in the United States," *The Quarterly Journal of Economics*, 2014, *129* (4), 1553–1623.

Chyn, Eric and Lawrence F Katz, "Neighborhoods matter: Assessing the evidence for place effects," *Journal of Economic Perspectives*, 2021, *35* (4), 197–222.

Corno, Lucia, Eliana La Ferrara, and Justine Burns, "Interaction, stereotypes and performance: Evidence from South Africa," Technical Report, IFS Working Papers 2019.

Coviello, Decio and Nicola Persico, "An economic analysis of Black-White disparities in the New York Police Department's stop-and-frisk program," *The Journal of Legal Studies*, 2015, *44* (2), 315–360.

Cunningham, Jamein, Donna Feir, and Rob Gillezeau, "Collective bargaining rights, policing, and civilian deaths," 2021.

Cutler, David M and Edward L Glaeser, "Are ghettos good or bad?," *The Quarterly Journal of Economics*, 1997, *112* (3), 827–872.

Desmet, Klaus, Joseph Flavian Gomes, and Ignacio Ortuño-Ortín, "The geography of linguistic diversity and the provision of public goods," *Journal of Development Economics*, 2020, *143*, 102384.

____ , Shlomo Weber, and Ignacio Ortuño-Ortín, "Linguistic diversity and redistribution," *Journal of the European Economic Association*, 2009, *7* (6), 1291–1318.

Devi, Tanaya and Roland G Fryer Jr, "Policing the police: The impact of" pattern-or-practice" investigations on crime," Technical Report, National Bureau of Economic Research 2020.

Domènech-Arumí, Gerard, "Neighborhoods, perceived inequality, and preferences for redistribution: Evidence from Barcelona," *Available at SSRN 3863594*, 2022.

____ , "Neighborhoods, Perceived Immigration, and Preferences for Redistribution: Evidence from Barcelona," *Available at SSRN 4460440*, 2023.

Dube, Oeindrila, Sandy Jo MacArthur, and Anuj K Shah, "A cognitive view of policing," Technical Report, National Bureau of Economic Research 2023.

Edwards, Frank, Hedwig Lee, and Michael Esposito, "Risk of being killed by police use of force in the United States by age, race–ethnicity, and sex," *Proceedings of the National Academy of Sciences*, 2019, *116* (34), 16793–16798.

Gelman, Andrew, Jeffrey Fagan, and Alex Kiss, "An analysis of the New York City police department's "stop-and-frisk" policy in the context of claims of racial bias," *Journal of the American Statistical Association*, 2007, *102* (479), 813–823.

Goncalves, Felipe and Steven Mello, "A few bad apples? Racial bias in policing," *American Economic Review*, 2021, *111* (5), 1406–41.

Grogger, Jeffrey and Greg Ridgeway, "Testing for racial profiling in traffic stops from behind a veil of darkness," *Journal of the American Statistical Association*, 2006, *101* (475), 878–887.

Hoekstra, Mark and CarlyWill Sloan, "Does Race Matter for Police Use of Force? Evidence from 911 Calls," *American Economic Review*, March 2022, *112* (3), 827–60.

Horowitz, Juliana Menasce, Anna Brown, and Kiana Cox, "Race in America 2019," 2019.

Horrace, William C and Shawn M Rohlin, "How dark is dark? Bright lights, big city, racial profiling," *Review of Economics and Statistics*, 2016, *98* (2), 226–232.

Johnson, David J, Trevor Tress, Nicole Burkel, Carley Taylor, and Joseph Cesario, "Officer characteristics and racial disparities in fatal officer-involved shootings," *Proceedings of the National Academy of Sciences*, 2019, *116* (32), 15877–15882.

Jr, Roland G Fryer, "An empirical analysis of racial differences in police use of force," *Journal of Political Economy*, 2019, *127* (3), 1210–1261.

Knowles, John, Nicola Persico, and Petra Todd, "Racial bias in motor vehicle searches: Theory and evidence," *Journal of Political Economy*, 2001, *109* (1), 203–229.

Laliberté, Jean-William, "Long-term contextual effects in education: Schools and neighborhoods," *American Economic Journal: Economic Policy*, 2021, *13* (2), 336–77.

Legewie, Joscha, "Racial profiling and use of force in police stops: How local events trigger periods of increased discrimination," *American Journal of Sociology*, 2016, *122* (2), 379–424.

Lofstrom, Magnus and Steven Raphael, "Crime, the criminal justice system, and socioeconomic inequality," *Journal of Economic Perspectives*, 2016, *30* (2), 103–126.

Ludwig, Jens, Greg J. Duncan, Lisa A. Gennetian, Lawrence F. Katz, Ronald C. Kessler, Jeffrey R. Kling, and Lisa Sanbonmatsu, "Long-Term Neighborhood Effects on Low-Income Families: Evidence from Moving to Opportunity," *American Economic Review*, May 2013, *103* (3), 226–31.

McCrary, Justin, "The effect of court-ordered hiring quotas on the composition and quality of police," *American Economic Review*, 2007, *97* (1), 318–353.

Mekawi, Yara and Konrad Bresin, "Is the evidence from racial bias shooting task studies a smoking gun? Results from a meta-analysis," *Journal of Experimental Social Psychology*, 2015, *61*, 120–130.

Oreopoulos, Philip, "The long-run consequences of living in a poor neighborhood," *The quarterly journal of economics*, 2003, *118* (4), 1533–1575.

Other & Belonging Institute, "Most to Least Segregated Cities," 2020.

Owens, Emily, David Weisburd, Karen L Amendola, and Geoffrey P Alpert, "Can you build a better cop? Experimental evidence on supervision, training, and policing in the community," *Criminology & Public Policy*, 2018, *17* (1), 41–87.

Persico, Nicola and Petra E Todd, "The hit rates test for racial bias in motor-vehicle searches," *Justice Quarterly*, 2008, *25* (1), 37–53.

Rao, Gautam, "Familiarity Does Not Breed Contempt: Generosity, Discrimination, and Diversity in Delhi Schools," *American Economic Review*, March 2019, *109* (3), 774–809.

Rehavi, M Marit and Sonja B Starr, "Racial disparity in federal criminal sentences," *Journal of Political Economy*, 2014, *122* (6), 1320–1354.

Rivera, Roman, "The effect of minority peers on future arrest quantity and quality," *Available at SSRN 4067011*, 2022.

Shi, Lan, "Does oversight reduce policing? Evidence from the Cincinnati police department after the April 2001 riot," *Journal of Public Economics*, 2008.

Stashko, Allison, "Do Police Maximize Arrests or Minimize Crime? Evidence from Racial Profiling in U.S. Cities," *Journal of the European Economic Association*, 07 2022, *21* (1), 167–214.

Weisburd, David, Cody W Telep, Heather Vovak, Taryn Zastrow, Anthony A Braga, and Brandon Turchan, "Reforming the police through procedural justice training: A multicity randomized trial at crime hot spots," *Proceedings of the National Academy of Sciences*, 2022, *119* (14).

Weisburst, Emily K, ""Whose help is on the way?" The importance of individual police officers in law enforcement outcomes," *Journal of Human Resources*, 2022, pp. 0720–11019R2.

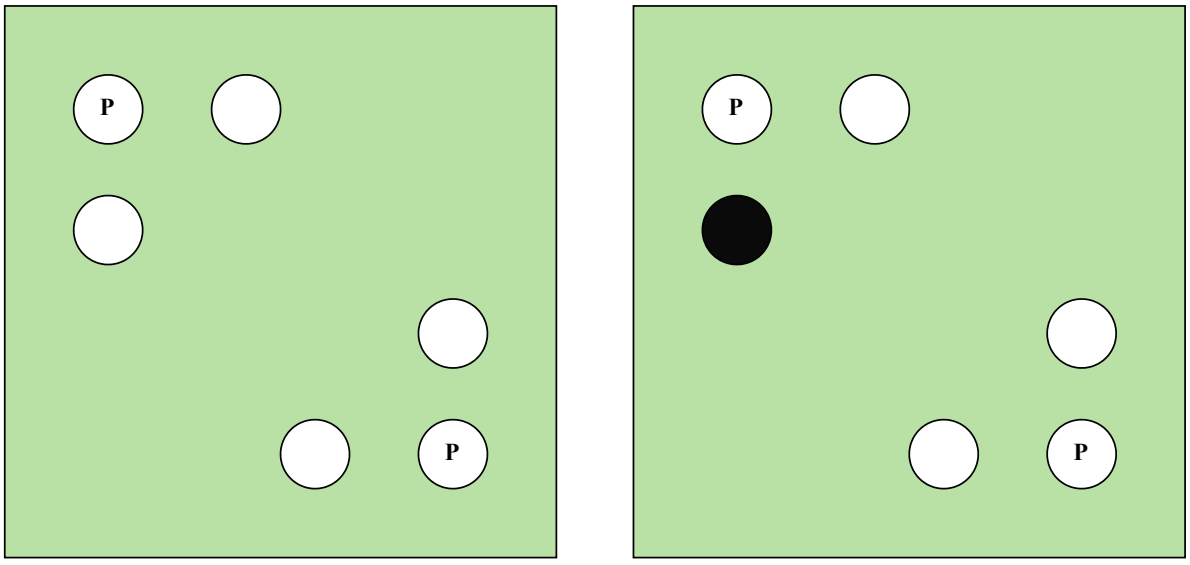
West, Jeremy, "Racial bias in police investigations," *Unpublished*, 2018.

Åslund, Olof, Per-Anders Edin, Peter Fredriksson, and Hans Grönqvist, "Peers, Neighborhoods, and Immigrant Student Achievement: Evidence from a Placement Policy," *American Economic Journal: Applied Economics*, April 2011, *3* (2), 67–95.

Appendix of
Black Neighbors Matter: Officer Neighborhoods and Racial Differences in Policing

Gerard Domènech-Arumí

A Appendix figures and tables



(a) Before (b) After

Figure A1: Illustration of the new "next-door" neighbor quasi-experimental design

Notes: Each square represents a neighborhood (e.g., a census tract). Each circle represents a household. The color indicates race (White or Black). The "P" identifies households with a police officer. White officers may initially decide to locate in more "White" neighborhoods (panel a). However, once settled, it is not in their control how the neighborhood (and their neighbors) change. Panel b depicts a situation where a Black household moves next to one of the officers. The research design exploits this exogenous change (from the officers' perspective) in the race of next-door neighbors to identify the effects of local exposure to different races on officers' behavior in FIs. The comparison group contains officers of the same race from the same tract.

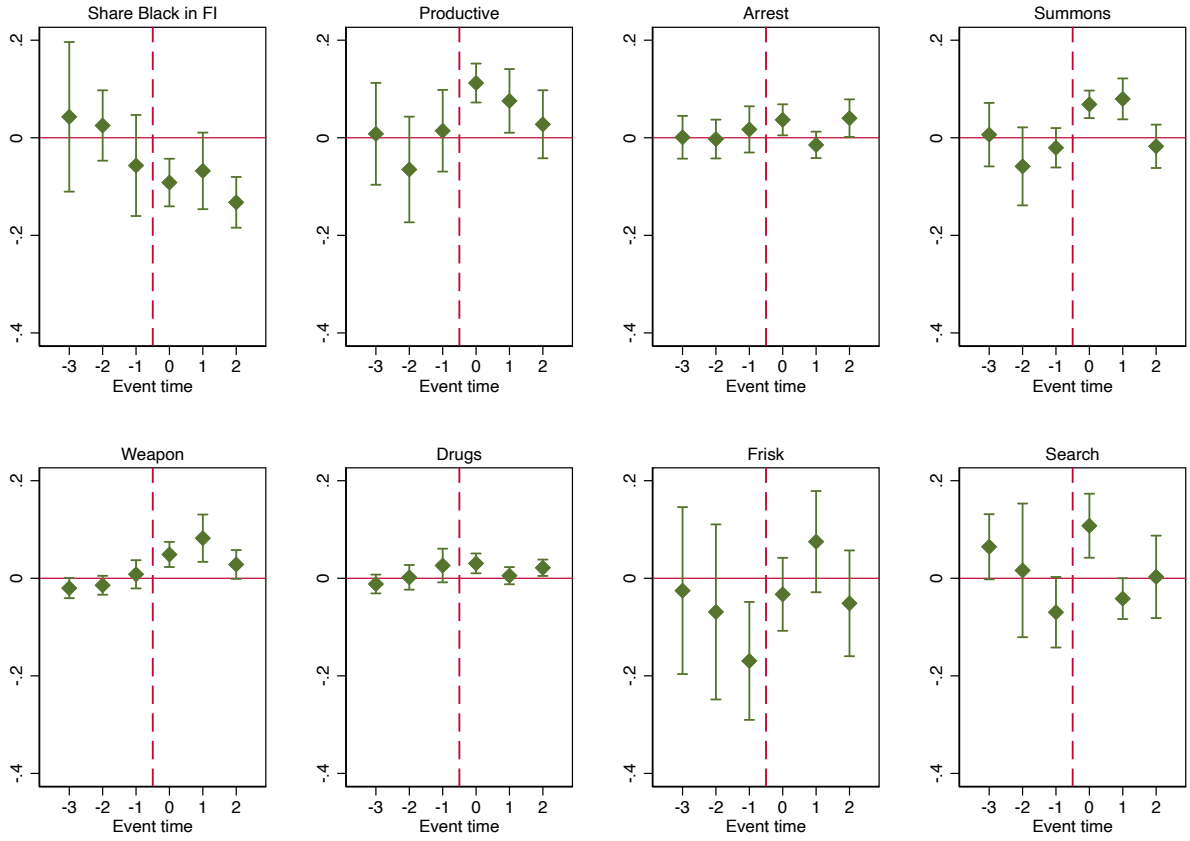


Figure A2: Robustness: neighbors' race inferred from HMDA data

Notes: The plot illustrates τ_h estimates from Equation 3. Vertical bars represent 95% confidence intervals. The estimation sample is restricted to all-Black FIs (except for the top-left plot). *New Black neighbor* as previously defined, but race is inferred from mortgage data available under the Home Mortgage Disclosure Act (HMDA). *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an *Arrest* or *Summons* issued or in *Drugs* or *Weapons* found. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. Standard errors clustered at the tract-year.

Table A1: Officers descriptive statistics

	State All	State Matched	City All	City Matched
	(1)	(2)	(3)	(4)
Panel A. Officer Characteristics				
Male	0.87	0.87	0.84	0.84
	(0.34)	(0.34)	(0.37)	(0.37)
White	0.69	0.69	0.64	0.64
	(0.46)	(0.46)	(0.48)	(0.48)
Black	0.19	0.19	0.22	0.22
	(0.39)	(0.40)	(0.42)	(0.42)
Hispanic	0.098	0.097	0.11	0.11
	(0.30)	(0.30)	(0.32)	(0.32)
Asian	0.024	0.023	0.024	0.022
	(0.15)	(0.15)	(0.15)	(0.15)
Earnings (1000s)	162.7	163.8	154.2	155.2
	(55.4)	(54.9)	(52.0)	(51.7)
Start year	1999.1	1999.3	2002.3	2002.3
	(9.63)	(9.58)	(9.34)	(9.33)
Panel B. FI Characteristics				
Number of FIs	99.9	105.6	91.1	94.7
	(487.8)	(464.3)	(418.3)	(427.4)
Individuals in FI	1.37	1.38	1.43	1.43
	(0.50)	(0.49)	(0.53)	(0.54)
Share male in FI	0.87	0.87	0.87	0.87
	(0.17)	(0.16)	(0.15)	(0.15)
Share Black in FI	0.51	0.51	0.53	0.53
	(0.30)	(0.29)	(0.28)	(0.28)
Frisk	0.29	0.29	0.30	0.30
	(0.26)	(0.25)	(0.24)	(0.24)
Search	0.17	0.17	0.16	0.16
	(0.22)	(0.21)	(0.19)	(0.19)
Weapon found	0.038	0.038	0.038	0.038
	(0.10)	(0.095)	(0.095)	(0.093)
Drugs found	0.030	0.030	0.026	0.026
	(0.090)	(0.086)	(0.071)	(0.071)
Arrest	0.079	0.081	0.073	0.073
	(0.15)	(0.15)	(0.13)	(0.13)
Summons issued	0.11	0.11	0.11	0.11
	(0.18)	(0.16)	(0.15)	(0.14)
N	2500	2250	1400	1350

Notes: Officers with at least one FI in the State (Columns 1-2) or City (Columns 3-4). Columns 2 and 4 restrict the sample to officers matched to an address. Panel A describes officers' characteristics. *Earnings* are gross earnings (1000s of USD) in 2020. *Start year* is the year the officer joined the PD. Panel B shows summary statistics of the FIs (2011-2020). Officer numbers rounded to the nearest fiftieth. Standard deviations in parenthesis.

Table A2: Neighborhoods and racial differences in FIs

	Share Black in FI				Productive			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. White officers								
Share Black in Neighborhood	-0.009 (0.011)	-0.008 (0.006)	0.001 (0.006)	-0.011 (0.012)	0.016*** (0.006)	0.015** (0.006)	0.017** (0.008)	0.030*** (0.011)
DV Mean	0.613	0.613	0.613	0.613	0.163	0.163	0.163	0.163
N	67177	67177	67177	67177	38743	38743	38743	38743
B. Black officers								
Share Black in Neighborhood	-0.026*** (0.008)	-0.009* (0.005)	-0.003 (0.006)	-0.024*** (0.007)	0.005 (0.005)	0.003 (0.005)	-0.000 (0.006)	0.005 (0.009)
DV Mean	0.665	0.665	0.665	0.665	0.130	0.130	0.130	0.130
N	14476	14476	14476	14476	9245	9245	9245	9245
C. Hispanic officers								
Share Black in Neighborhood	0.063*** (0.017)	0.024*** (0.009)	0.009 (0.014)	-0.049** (0.024)	0.001 (0.009)	0.004 (0.009)	0.017 (0.019)	0.040 (0.033)
DV Mean	0.582	0.582	0.582	0.582	0.133	0.133	0.133	0.133
N	7627	7627	7627	7627	4165	4165	4165	4165
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE		X	X	X		X	X	X
Officer ZIP FE			X				X	
Officer Tract FE				X				X

Notes: The sample includes all FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the city. Columns 5-8 restrict the sample to all-Black FIs. *Share Black in FI* is the share of Black individuals in the police interaction. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Share Black in Neighborhood* is the standardized share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table A3: Covariate balance across treatment and control White officer samples

	Treated	Control	Difference
Panel A. Officer Characteristics			
Male	0.912	0.890	0.022
	(0.049)	(0.014)	
Earnings (1000s USD)	123.473	127.901	-4.427
	(4.477)	(1.306)	
Start year	2002.011	2003.956	-1.945
	(1.575)	(0.360)	
Share White in Neighborhood	0.682	0.695	-0.013
	(0.016)	(0.004)	
Share Black in Neighborhood	0.115	0.112	0.002
	(0.013)	(0.002)	
Share Hispanic in Neighborhood	0.111	0.105	0.006
	(0.010)	(0.002)	
Share Asian in Neighborhood	0.067	0.060	0.007
	(0.010)	(0.002)	
Panel B. FI Characteristics (First Year)			
Number of FIs	11.979	14.387	-2.408
	(2.120)	(1.148)	
Individuals in FI	1.367	1.435	-0.067
	(0.119)	(0.031)	
Share male in FI	0.865	0.870	-0.005
	(0.034)	(0.006)	
Share Black in FI	0.544	0.523	0.021
	(0.042)	(0.011)	
Frisk	0.326	0.285	0.041
	(0.044)	(0.010)	
Search	0.221	0.164	0.057
	(0.040)	(0.008)	
Weapon found	0.033	0.041	-0.007
	(0.011)	(0.004)	
Drugs found	0.021	0.030	-0.009
	(0.005)	(0.004)	
Arrest	0.091	0.074	0.017
	(0.027)	(0.006)	
Summons issued	0.127	0.111	0.016
	(0.029)	(0.006)	
N	50	800	

Notes: Sample: White police officers with at least one FI in City. *Treated* officers are exposed to a new Black neighbor sometime during the sample period. *Control* officers are not. *Share White in N'hood*, *Share Black in N'hood*, *Share Hispanic in N'hood*, *Share Asian in N'hood* denote the average share of White, Black, Hispanic, and Asian population in officers' census block groups throughout the sample period. The rest of the variables are as previously defined. Outcomes in Panel B show sample means for the first year officers make a FI in the data. All variables are residualized (net of census tract fixed effects) and scaled up by their unconditional sample mean, so coefficients offer group comparisons within a census tract. Officer numbers rounded to the nearest fiftieth. Standard errors in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4: Robustness: alternative race thresholds

Panel A. Racial composition of interactions

	Share Black in FI				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	-0.119***	-0.153***	-0.154***	-0.170***	-0.177***
	(0.013)	(0.011)	(0.011)	(0.012)	(0.013)
DV Mean	0.612	0.612	0.612	0.612	0.612
N	67437	67437	67437	67437	67437

Panel B. Productivity of all-Black interactions

	Productive				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	0.019	0.087***	0.100***	0.103***	0.101***
	(0.023)	(0.014)	(0.015)	(0.017)	(0.018)
DV Mean	0.163	0.163	0.163	0.163	0.163
N	38873	38873	38873	38873	38873
Year FE	X	X	X	X	X
FI ZIP FE	X	X	X	X	X
Officer Tract FE	X	X	X	X	X
Race threshold	60%	65%	70%	75%	80%

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *New Black Neighbor* is an indicator taking the value of one if a Black individual moves into a previously non-Black-owned and occupied dwelling within 100 meters of the officer's residence. *Race threshold* denotes the threshold (%) to assign races to owners based on the likelihood of their last name belonging to a given race according to the Decennial Census. All specifications include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A5: Robustness: neighbors' race inferred from HMDA data

Panel A. Racial composition and productivity of all-Black FIs

	Shr Black in FI		Productive		Arrest		Summons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	-0.082***	-0.092***	0.059***	0.015	0.012	0.019*	0.033**	-0.020
	(0.019)	(0.022)	(0.022)	(0.029)	(0.013)	(0.011)	(0.016)	(0.027)
DV Mean	0.613	0.613	0.163	0.163	0.060	0.060	0.096	0.096
N	67221	67221	38770	38770	38770	38770	38770	38770

Panel B. Productivity and characteristics of all-Black FIs

	Weapon		Drugs		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	0.046***	0.050***	0.026***	0.030***	0.036	-0.079***	0.028	0.177***
	(0.008)	(0.008)	(0.009)	(0.008)	(0.028)	(0.025)	(0.030)	(0.019)
DV Mean	0.029	0.029	0.018	0.018	0.330	0.330	0.186	0.186
N	38770	38770	38770	38770	38770	38770	38770	38770
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X
Officer Tract FE	X		X		X		X	
Officer FE		X		X		X		X

Notes: The sample includes all FIs from White officers in the city. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an *Arrest* or *Summons* issued or in *Drugs* or *Weapons* found. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. *New Black neighbor* as previously defined, but race is inferred from mortgage data available under the Home Mortgage Disclosure Act (HMDA). Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table A6: Robustness: alternative racial transitions

Panel A. Racial composition of interactions

	Share Black in FI					
	(1)	(2)	(3)	(4)	(5)	(6)
New neighbor	0.010	-0.009	-0.154***	-0.154***	0.007	-0.008
	(0.017)	(0.011)	(0.011)	(0.011)	(0.013)	(0.017)
DV Mean	0.612	0.615	0.612	0.612	0.611	0.612
N	35288	53419	67437	67442	65300	63172

Panel B. Productivity of all-Black interactions

	Productive					
	(1)	(2)	(3)	(4)	(5)	(6)
New neighbor	-0.024	-0.023*	0.100***	0.100***	0.028*	-0.010
	(0.018)	(0.014)	(0.015)	(0.015)	(0.014)	(0.013)
DV Mean	0.174	0.166	0.163	0.163	0.164	0.164
N	20172	30930	38873	38875	37469	36514
Year FE	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X
Officer Tract FE	X	X	X	X	X	X
Transition	W-W	!W-W	!B-B	W-B	!H-H	!A-A

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *New neighbor* is an indicator taking the value of one if an individual of a given race (White, Black, Hispanic, or any race) moved into a previously owner-occupied dwelling from an owner of a different race within 100 meters of the officer's residence. *Transitions* identify the racial transitions considered. These are: *W-W* for White-to-White, *!W-W* for non-White-to-White, *!B-B* non-Black-to-Black, *W-B* White-to-Black, *!H-H* non-Hispanic-to-Hispanic, and *!A-A* for non-Asian-to.Asian. The number of observations differs across columns due to variations in the always-treated and never-treated subsamples, which depend on the racial transition considered (e.g., White transitions are more common, leading to a larger number of always-treated officers). All specifications include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A7: Robustness: alternative treatment distances

Panel A. Racial composition of interactions

	Share Black in FI				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	-0.018	-0.154***	-0.061**	-0.002	0.006
	(0.021)	(0.011)	(0.027)	(0.016)	(0.013)
DV Mean	0.612	0.612	0.612	0.612	0.612
DV SD	67437	67437	67435	67425	67368

Panel B. Productivity of all-Black interactions

	Productive				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	0.145***	0.100***	-0.029*	-0.029*	-0.010
	(0.026)	(0.015)	(0.017)	(0.015)	(0.014)
DV Mean	0.163	0.163	0.163	0.163	0.163
N	38873	38873	38873	38871	38838
Year FE	X	X	X	X	X
FI ZIP FE	X	X	X	X	X
Officer Tract FE	X	X	X	X	X
Treatment distance (m)	50	100	200	350	500

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *New Black Neighbor* is an indicator taking the value of one if a Black individual recently (after 2010) moved into a previously non-Black-owned and occupied dwelling located within a given distance (50, 100, 200, 350, or 500 meters) of the officer's residence. All specifications include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A8: Mechanisms: FI neighborhoods and officers' relocation decisions (officer-year)

	Shr Black Pop		Share White Pop		Share Crime		Share Violent Crime		Officer Moved	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
New Black neighbor	-0.015	0.013	-0.011	-0.033**	-0.000	0.002	-0.004**	-0.004*	-0.021	0.007
	(0.012)	(0.016)	(0.012)	(0.016)	(0.002)	(0.002)	(0.002)	(0.002)	(0.033)	(0.033)
DV Mean	0.274	0.274	0.410	0.410	0.053	0.053	0.053	0.053	0.214	0.214
N	2628	2628	2628	2628	2628	2628	2628	2628	2805	2805
Year FE	X	X	X	X	X	X	X	X	X	X
Officer Tract FE	X		X		X		X		X	
Officer FE		X		X		X		X		X

Notes: The sample includes White officers in the city. An observation is an officer-year pair. Columns 1-8 investigate the characteristics of the average neighborhood (ZIP code) in which FIs take place regarding demographics (share of Black or White population in the ZIP) and crime (share of total criminal incidents or violent incidents in the ZIP-year pair). Columns 1-8 sample is restricted to officer-year pairs with at least one FI. Columns 9 and 10 investigate officers' relocation decisions. Officer Moved is an indicator taking the value of one after the officer's first change of address in the sample period. *New Black Neighbor* as previously defined. Source of crime data: 2015-2020 PD incident reports. Crime statistics for 2011-2014 use the value for 2015 in the ZIP. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table A9: The effects of interventions and training on policing outcomes

Paper	**Intervention**	**Results**
Domènech-Arumí (2023)	Exposure to a new Black neighbor	Within 3 years: less Black civilians stopped (-3py, 13%), more Whites (5py, 70%), and increase in arrests (0.2py, 16%), citations (0.8py, 45%), drugs found (0.2py, 80%).
Rivera (2023)	6-month police academy training with a higher share of minority peers	Within 4 years: 1 SD Minority peers in academy causes a 16% reduction in low-level/quality arrests and a 9% of a SD increase in serious/high-quality arrests.
Dube, MacArthur, and Shah (2023)	Four 4-hour training sessions on cognitive policing (situational decision-making)	6-months after the training: 23% decrease in discretionary (unproductive) arrests. 23% decrease in use of force. Reduction in racial disparities (Black arrests -28%; White arrests -6%). Fewer officer injuries (-47%). No change in activity.
Weisburd, Telep, Vovak, Zastrow, Braga, and Turchan (2022)	5-day training on procedural justice	9 months after the training: in crime hot spots: 60% less arrests. Reduction in criminal activity.
Owens, Weisburd, Amendola, Alpert (2018)	One session training with supervisor on procedural justice	6 weeks after the training: arrests decrease by 12%, use of force decrease (16-50%).

Notes: Summary of the effects of police interventions and training on policing outcomes documented in the literature.

B Data appendix

B.1 Officer addresses

I combined multiple administrative data sources to assign officers with FIs to an exact address. 2,500 officers participated in at least one FI from 2011 to 2021. Among those, 1,400 (56%) live in City. The data sources and key variables used for the assignment are the following:

1. Officers data. Contains unique employee identifiers, full names, and ZIP code of residence.
2. State voter files. Contains full names and exact addresses as of late 2022. I obtained the data from a vendor (L2 Data).
3. State and City property registries. Contain owners' full names and exact addresses. The State data is a cross-section as of January 2021. The City data is a panel of owner-addresses for 2010-2021.
4. City public employee roster data. Contains unique employee identifiers and their ZIP code of residence from 2010 to 2021.

I assigned over 2,200 officers (90%) to an exact address following these steps:

1. Match officers to the voter files data.
   (a) Use officers' full names to find the best match within a ZIP code.
   (b) In case of a duplicate match, try manually assigning the "right" individual in the voter data using other demographic information available (e.g., date of birth and first day of work in the PD). Less than 100 officers were assigned to an address using this procedure.

   I matched approximately 2,200 (out of 2,500) officers in this step. Many of the unmatched officers had FIs in the earlier years of the sample and were retired (in 2022). They may have moved out of state.
2. Merge the previously matched data to the State and City property registry.
   (a) Use the officers' last name and address information (street number, street name, street type, ZIP code) to assign them to a property. Retrieve the property's unique identifier.
   (b) Define an officer as a renter if the property owner's family name in the voter files address does not coincide with that of the officer.

I assigned almost all (except less than 50) officers to a property in the registry. This step gives me the most recent address for almost 90% of all officers in the data with at least one FI. Table A1 shows no significant differences in observable characteristics between matched officers and their respective populations.

3. Obtain address histories (2010-2021) for the subsample of officers in City.

   (a) Use the property registry to identify officers' tenure in the most recent address (January 2021) and identify gaps in 2010-2021 based on ownership information (name and occupancy status).

   (b) Use City employee roster data to identify officers' ZIP codes of residence in any given year. Use that information to fill the gaps. In particular, use officer full names to manually find the best match within a ZIP code in a given year.

   (c) For every officer, attempt to fill in the entire address history since 2010 (up to the three most recent addresses). Give priority to finding addresses of officers with more FIs in a given year.

   (d) For renters: assume they live in the same property as long as they reside within the same ZIP code based on the roster data.

   Officers in City are assigned to an address 80% of the years in the period 2010-2020. I cannot retrieve the full address histories for all officers in all years, but I do observe officers' current addresses in 94% of the FIs. Figure B1 summarizes the address assignment process described above in a flowchart.

B.2 Race of police officers

I assigned race to police officers following these steps:

1. Use public employee records. Racial information is available for all officers active at the beginning of 2020. These constitute approximately 80% of all officers with at least one FI in the sample.

2. If race is still unassigned, retrieve it from a database containing PD officers' internal affairs investigations published online by a news outlet. That gives me racial information for 10% of the officers.

3. If race is still unassigned, impute it from officers' last names using R's command *predictrace*. The package returns a probability of a last name belonging to a specific race based on its actual frequency in the US Decennial Census. Assign a race only when there is a confidence level above 70%. I assigned race for 7% of the officers in this step. I was not able to impute a race to 3% of the officers.

B.3 Race of owners

I assigned a race to owners in City using the R package *predictrace* described earlier. For every last name, the package returns the probability of that name belonging to a specific race based on frequencies in the US Decennial Census. Baseline results only assigned a race when the confidence level is above 70%. One robustness table shows the sensitivity of baseline results to alternative confidence thresholds.

B.4 Race of new owners inferred from HMDA data

A robustness test obtains the race of a subset of new owners combining different administrative data sources:

1. Home Mortgage Disclosure Act (HMDA) data. Contains detailed information on all mortgages issued since 2007 in the United States. The key variables in the data are: loan amount, census tract of the property, mortgage type (e.g., for a primary residence), and applicant information (race and gender). I retrieved the data on all mortgages originated in City.

2. State registry of deeds data. Universe of real estate transactions in the State since 2008. This data contains the property registry identifier and the sale price. I retrieved information on all sales in City.

3. City registry data (described earlier). Contains property unique identifiers and exact addresses. These can be used to easily merge with the registry of deeds data.

The advantage of the HMDA data is that race is self-reported and should therefore be precise. I assigned loans to transactions following these steps:

1. If there is only one transaction and one loan in a tract-year, assign the loan to the transaction.

2. If there is only one transaction in a tract-year and several loans, assign a loan to a transaction only if there is one match in terms of gender (based on applicant/s names) and value (loan value between 85 and 115% of transaction price).

3. If there are several transactions in a tract-year, assign a loan to a transaction if there is only one loan matching in terms of gender and value (as defined in the previous step).

I could assign 3,800 loans (8%) to a transaction in 2010-2021.

B.5 FI data cleaning

This subsection describes the cleaning process of the FI data and the construction of the primary outcomes used in the analysis.

Data reporting standards changed in 2015, and the variables had to be harmonized. Pre-2015, most outcomes are constructed from the raw variables *reasons for the FI* (example: "arrest warrant"), *reasons for the stop* (example: "criminal violation"), *basis* (example: "probable cause"), *FI type* (example: "frisk and search"), and *FI outcome* (example: "seizure"). These variables (except *basis*) are not available in the post-2015 reporting format. They are replaced by the variables *FI circumstance* (example: "stopped"), *Frisk in FI* (example: "yes"), *Search type in FI* (example: "vehicle search"), and *narrative*. The variable *narrative* is a string containing a report detailing the developments that occurred during the interaction written by the officer in charge of the FI.

I first constructed the following intermediate variables from *reasons for the FI* (pre-2015), and *narrative*.

- **Misdemeanor.** An indicator taking the value of one if the FI is related to a misdemeanor. That is, if *reasons for the FI* (pre-2015) or *narrative* refer to one of the following offenses: assault, break and entering, disorderly conduct, domestic assault, drug possession, driving under influence (DUI), failure to appear in court, hit and run, indecent exposure, shoplifting, malicious damage, minor in possession of alcohol, resisting arrest, trespassing, threatening to commit a crime, negligent operation of a motor vehicle, prostitution, uttering.

- **Felony.** An indicator taking the value of one if the FI is related to a felony. That is, if *reasons for the FI* (pre-2015) or *narrative* refer to one of the following offenses: arson, assault and battery with a dangerous weapon, burglary, carjacking, child endangerment, child pornography, drug possession with intent to distribute, firearm charges (e.g., unlawful possession), fraud, home invasion, impersonation, indecent assault, kidnapping, murder, sexual assault or rape, theft auto, unnatural and lascivious.

- **Warning.** An indicator taking the value of one if the FI resulted in at least one individual receiving a verbal warning. That is, post-2015, if the *narrative* contains one of the following keywords: "warning," "warned," "given a verbal." The variable cannot be constructed pre-2015.

I then constructed the main outcomes:

- **Share Black in FI.** Denotes the share of Black individuals present in a FI. Pre-2015, the race categories were Black, White, Hispanic, Asian, Native American, or Hawaiian. After 2015, a new ethnicity variable distinguished between Hispanic and Non-Hispanic, thereby making it possible to differentiate, for example, Black Hispanics from Black Non-Hispanics. For consistency, I constructed a new race variable mimicking the categories pre-2015 (i.e., Black and White Hispanics are "Hispanics").

- **Frisk.** An indicator taking the value of one if at least one individual was frisked during a FI. Pre-2015, these are identified through the variable *FI type*. Post-2015, these are identified through the variable *Frisk in FI*.

- **Search.** An indicator taking the value of one if at least one individual was searched during a FI. That is, pre-2015, if the *FI type* included a "search." Post-2015, if the *search type in FI* included "person", or "vehicle."

- **Arrest.** An indicator taking the value of one if at least one individual was arrested during a FI. That is, pre-2015, if the reason for the FI was an "arrest warrant", the reason to stop an individual was a "violation" and the FI was related to a felony; or if the FI resulted in a seizure and was related to a felony. After 2015, if the FI involved a contact, the keywords "arrest," "warrant," "taken into custody," "placed under arrest" appeared in the *narrative*, but not the keywords "released," "advised to leave," "left without incident," "no warrant," and the FI did not result in a *warning*; or if sentence fragments explicitly referring to an arrest appear (e.g., "the suspect was arrested").

- **Summons.** An indicator taking the value of one if at least one individual received a summons or court citation during a FI. That is, pre-2015, if the reason for a stop was a "violation" and the FI was not related to a felony or an arrest warrant; or if the FI resulted in a seizure and was related to a misdemeanor but not a felony; or if the FI resulted in contraband found but not to an arrest. Post-2015, if the narrative refers to a "citation," "summons," or "city ordinance violation"; or if it related to a misdemeanor, the interaction was not made for intelligence purposes, and it involved no contact with individuals with priors.

- **Weapon.** An indicator taking the value of one if officers seized at least one weapon during a FI. That is, pre-2015, if the *reasons for FI* included one of the following key terms: "armed," "weapon," "firearm," "ammunition," and "possession," "found," or "carrying"; if it resulted in a seizure and references to crimes in which offenders are more likely to possess a weapon were mentioned (e.g., "affray," "arson," "damage to property"); or if the FI resulted in the seizure of contraband but was not related to any specific crime (e.g., reason for FI of "Investigative, person"). Post-2015, if the *narrative* mentions weapons being found (e.g., "a firearm was found").

- **Drugs.** An indicator taking the value of one if officers seized drugs during a FI. That is, pre-2015. if *reasons for FI* mentioned drugs, and there was a seizure, or the words "possession" or "found" appeared; if the FI resulted in a seizure but no weapons were seized; if the FI resulted in the seizure of contraband but was not related to any specific crime (e.g., reason for FI of "Investigative, person"). Post-2015, if the *narrative* mentions drugs being found (e.g., "officers recovered a small bag of white powdery substance").

- **Productive.** An indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found, according to the previous definitions.

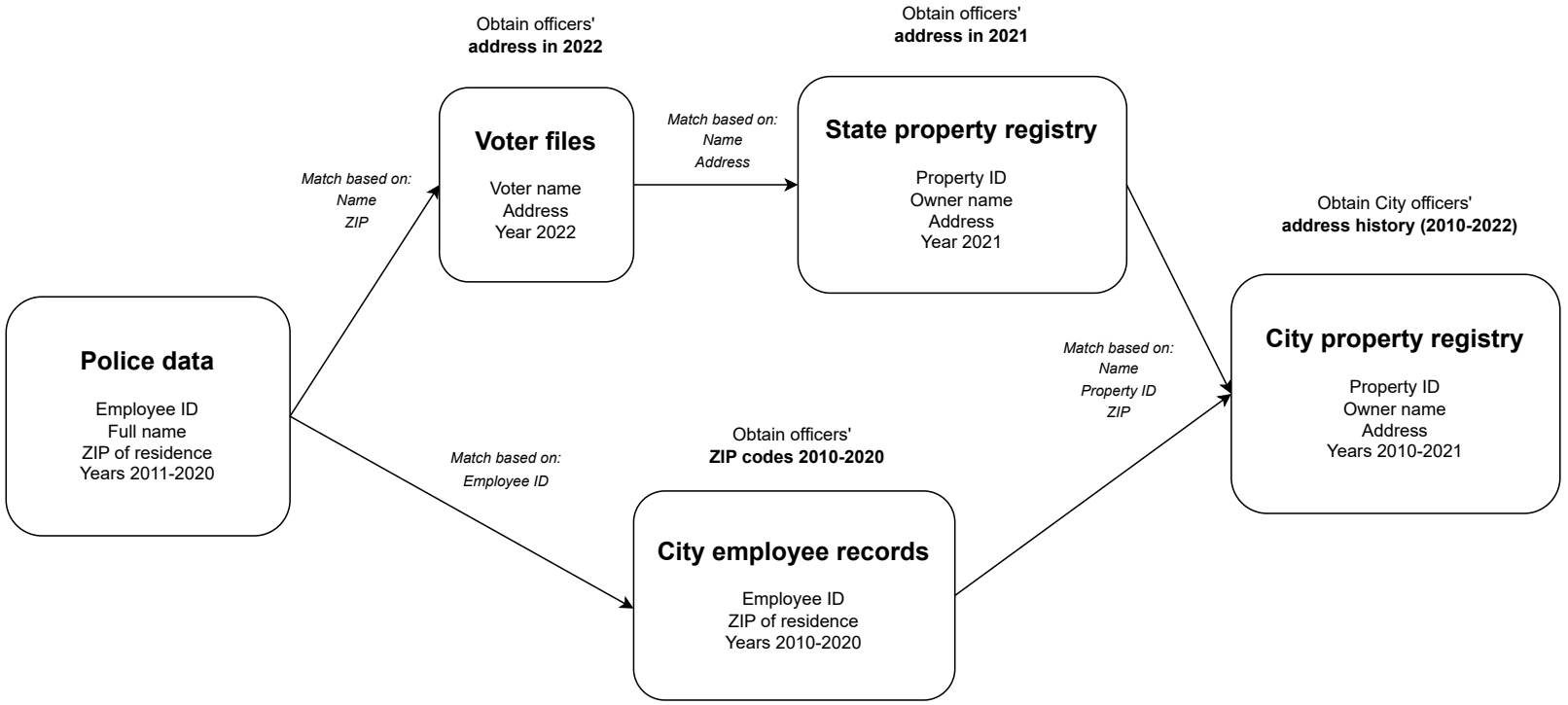


Figure B1: Officer address assignment flowchart

Notes: The flowchart describes the process followed to assign officers to an exact address. Each box depicts a data source (police data, voter files, property registry, or employee records) along with its key variables used. The outcome is an assigned address as of 2021 for officers not residing in City, and an address history (2010-2021) for officers in City.

C Additional results and robustness tests

C.1 Additional descriptive results

Outcomes test. There is suggestive evidence of racial bias in the PD based on an outcomes test à la Persico (2002). Panel A in Table C1 shows a raw gap of 4.6pp ($p < 0.01$) in the productivity of all-Black stops (6.5pp in the subsample of officers residing in the rest of the State). Given a baseline productivity rate of just under 20%, that translates into the average Black stop being approximately 25% less productive than the average White stop. Accounting for FI and officers' characteristics (Columns 2 and 6) shrinks that gap by 30% in both subsamples, down to 3-4pp, but the difference is still significant. Finally, accounting for officers' neighborhood characteristics (Columns 3-4 and 7-8) further shrinks the gap to approximately 3pp. The latter results constitute suggestive evidence of local environments' influence on officers' productivity, as the gap shrinks by an additional 0.2-0.9pp (10-20%) when including ZIP or census tract fixed effects.

There are differences in the productivity of the stops depending on officers' race. Panel B shows that the productivity gap for Whites closely mirrors the results of the entire PD, with a gap ranging between 4 and 2.5pp. Panel C shows an even larger raw gap for Black officers (exceeding 5pp). However, the gap shrinks to 2pp when accounting for differences in FI characteristics and officers' neighborhoods. Panel D shows that Hispanic officers have a raw gap of 5pp, which almost wholly closes when accounting for officers' neighborhoods (ZIP or census tract fixed effects).

Differences in productivity are present in almost all subcomponents. Panels A-C from Table C2 show that Black stops from White or Black officers are approximately 1pp less likely to result in an arrest, a summons, or drugs found. There are no clear differences in the likelihood of finding weapons. For Hispanic officers, the productivity gap is completely closed after accounting for differences in officers' neighborhoods.

Veil of darkness. The veil of darkness hypothesis poses that race is less salient during nighttime and, consequently, differences in the racial composition of stops should be lower at night when officers practice racial profiling (Grogger and Ridgeway 2006). Table C3 follows Grogger and Ridgeway (2006) to investigate this hypothesis by exploiting within-year variation in daylight hours in the subsample of stops that occurred in inter-twilight hours (5 to 10 PM). Results provide some supportive evidence of racial profiling based on the hypothesis. Blacks are 2pp ($p < 0.1$) less likely to be stopped at night. That effect is primarily driven by Black officers (who are 5pp less likely to interact with Blacks at night). Also, among the subsample of Black stops, those at night are up to 1.3pp ($p < 0.05$) more likely to result in some contraband (drugs or weapons) being seized. The effect is primarily driven by Hispanic officers.

The supporting evidence of racial profiling through the lens of the veil of darkness hypothesis is positive but weak. The reasons for these could be: (i) a small sample size (e.g., in comparison, the recent work by Pierson et al. (2020) uses data on millions of stops); (ii) the prevalence of pedes-

trians in the data (i.e., only a subsample of individuals in the FIs are motorists, and the "mask" of darkness is likely to be thinner among pedestrians); (iii) the exact time of the stop is not observed, implying measurement error in the definition of "night."

Proximity to FI neighborhoods. Officers residing close to where they police interact with relatively fewer Blacks, but there is no evidence of their Black interactions being differentially productive. The top panel in Table C4 shows how officers residing within a few kilometers of where a FI takes place interact with 3-6pp fewer Blacks (up to 10%). The last column indicates that one standard deviation increase in the distance between officers' homes and where they police (13 km) is associated with FIs with 9pp more Blacks. However, the bottom panel shows no indication that officers' proximity to FI neighborhoods matters for productivity in Black interactions. .

Heterogeneity by party affiliation. FI characteristics vary with officers' ideology (proxied by party affiliation). Results in Table C5 show some baseline differences in the racial composition and productivity of FIs by party affiliation. Relative to FIs from Republican officers, FIs from Democrats have fewer Blacks (57 vs. 62%) but are also less productive (13 vs. 20%). Non-affiliated officers interact with more Blacks than Republicans (63%) and are less productive (16%). The table also shows that officers residing in Black neighborhoods interact with significantly fewer Blacks in FIs. That is true for Democrat (up to -5pp, $p > 0.1$) and Republican (up to -7pp) officers but not among those non-affiliated to any party. Regarding productivity, results suggest differences across neighborhoods are only present for non-affiliated and, especially, Republican officers (up to 17pp more productive).

C.2 Additional robustness checks

Descriptive results with the full sample of officers. Tables C6 and C7 replicate the main descriptive results (Tables A2 and 3) with the full sample of officers. Results are qualitatively identical, and quantitative differences are statistically insignificant.

Descriptive results using with demographics from the (2010) census block. White officers in census blocks with higher Black population shares are 1pp ($p > 0.1$) more productive in all-Black interactions, a difference driven by increased success in the likelihood of finding drugs (up to 0.6pp) and weapons (up to 2pp). These are the main takeaways from Panel A in Tables C8 and C9, that replicate Tables A2 and 3. As before, Panel B in Table C8 shows that Black officers in Black neighborhoods interact with relatively fewer Black civilians, and Panel C shows that this is also the case for Hispanic officers only when accounting for differences in the census tracts where they live (Column 4). Also, columns 5-8 in the same panel and Table, and the entire Panel C in Table C8 reveal no systematic differences in Hispanic officers' productivity. However, unlike before, columns 5-8 in Panel B suggest a positive differential in the productivity of Black officers residing

in Black census blocks. Table 3 reveals that differences in the likelihood of summons primarily drive this. Overall, the results in the two sets of tables are very similar. Differences between the two are driven by the increased granularity of the census block (relative to the block group), which plays in favor of the present results in terms of reliability, and the lack of time variation, which acts against them.

Individuals with a new White neighbor as a control group. Columns 1-2 in Table C10 reproduces the main results by restricting the sample of officers to those recently exposed to a new Black or White neighbor. Coefficients are qualitative and quantitatively indistinguishable from those in the baseline.

New Black neighbors in the same street. Columns 3-4 in Table C10 replicate baseline results restricting the definition of *New Black Neighbor* to only include new neighbors in the same street as the officers. Results are qualitatively indistinguishable from the baseline, but Panel A shows a smaller effect on the racial composition of FIs (6-12 vs. 15-16pp). There are no differences concerning the impact on productivity.

COVID-19. Columns 5-6 in Table C10 show that FIs following the start of the COVID-19 pandemic are not driving the main results.

Productivity of all-White interactions. As a placebo test, Columns 7-8 in Table C10 show that White officers do not experience significant changes in the productivity of all-White interactions.

Differences in reporting standards. FI reporting standards changed in 2015, as Appendix B described. Table C11 shows no qualitative differences in outcomes when restricting the sample to FIs from either the earlier (pre-2015) or latter (post-2015) period, but there are quantitative differences in many outcomes. The reduction in the relative share of Black civilians stopped is larger post-2015 (8 vs. 17pp), and productivity gains are only significant in the most recent years (0 vs. 11pp), primarily driven by the increase in summons (-1 vs. 12pp). The coefficients on arrests, frisks, searches, weapons, and (to a lesser extent) drugs are much statistically closer. Reasons for the discrepancies in results could be a combination of (i) changes in the reporting standards and variable definitions; (ii) changes in the behavior of police officers over time (e.g., due to public scrutiny); or (iii) a shorter panel and sample.

Spatial autocorrelation. Standard errors are slightly larger when accounting for spatial autocorrelation in OLS estimates. Table C12 reproduces the main estimates on the racial composition and productivity of stops while computing the standard errors following Conley (1999), applying distance thresholds ranging from 200 meters to one kilometer. Estimates on the racial composition

of FIs remain significant at the 5% level. Estimates on productivity only become significant at the 10% level. A caveat is that this exercise does not prevent the "forbidden comparisons" discussed in the new TWFE literature (De Chaisemartin and d'Haultfoeuille 2022b).

Alternative two-way fixed effects methods. Table C13 tests the sensitivity of the results to alternative methodologies recently developed in the fast-expanding two-way fixed effects (TWFE) literature. The first row provides the results of estimating Equation 2 with a standard OLS. The second row reports the (baseline) results obtained using the imputation estimator from Borusyak et al. (2024). Finally, the third and fourth rows report the results obtained from applying the methodologies introduced in De Chaisemartin and d'Haultfoeuille (2022a) and Sun and Abraham (2021), respectively.[1] Despite some differences in the magnitude and significance of coefficients, the four methodologies deliver a consistent message: exposure to a new Black neighbor is associated with a reduction in the share of Black individuals in FIs (up to 15pp) and with an overall increase in the productivity of Black interactions (up to 10pp). Also, all signs (except with one estimator) are identical across columns.

The De Chaisemartin and d'Haultfoeuille (2022a) estimator yields the highest discrepancy between methodologies. Differences possibly arise because *did_multiplegt*, the command to implement the De Chaisemartin and d'Haultfoeuille (2022a)'s TWFE estimator, is less flexible than the others. In particular, it does not allow for the inclusion of FI ZIP and officer census tract fixed effects simultaneously, so coefficients are estimated only including the former. That makes them not directly comparable with the rest, as the variation exploited for their computation comes from different sources (from between as opposed to within census tracts).

Finally, the major differences between Borusyak et al. (2024) and Sun and Abraham (2021) come from the latter being (slightly) more precisely estimated. An explanation could be the presence of serial correlation between groups and over time, a setting in which Sun and Abraham (2021) is a more efficient estimator (De Chaisemartin and d'Haultfoeuille 2022b).

C.3 Additional quasi-experimental results

Interaction details and White stops. Table C14 investigates more closely the changing dynamics of FI interactions following exposure to a new Black neighbor. Columns 1-2 reveal a decrease in observations of up to 8pp. Columns 3-4 indicate an overall increase in consensual encounters of up to 15pp, although that effect appears to be sensitive to the fixed effects included in the empirical design for Black stops (showing a decrease of 5pp in the specification with officer fixed effects). Columns 5-6 reveal an overall reduction in the share of FIs with stops. That effect is highly heterogeneous depending on the nature of the interaction: stops increase up to 5pp in Black interactions and decrease up to 12pp in White interactions. Columns 7-10 indicate an overall decrease in the violence of interactions, with reductions in frisks and searches, more prevalent in White stops.

[1] I did not use the estimator developed in Callaway and Sant'Anna (2021), as it does not allow for time-varying covariates. Accounting for the variation in the racial composition of FI neighborhoods is paramount.

Finally, Columns 11-12 reveal an overall increase in the productivity of interactions of up to 6pp, driven by productivity increases in Black stops (of up to 12pp). The overall productivity of White stops remains unchanged.

Sentiment analysis. I test whether the sentiment in police narratives changes among officers with a new Black neighbor. If the reduction in the share of Black stops and the relative increase in the productivity of those are due to positive shifts in the attitude of White officers toward Blacks (contact hypothesis), that change in attitudes could be reflected in the tone of police narratives. Specifically, when comparing narratives describing "similar" situations, treated officers' narratives might display a more positive sentiment in Black interactions.

To test this hypothesis, I cleaned the narratives by removing punctuation, stop words, and some location and other narrative-specific words (e.g., names in narratives were replaced with "xxx"), and obtained the roots of all remaining words. Figure C1 illustrates the most common words in the cleaned narratives with a word cloud. Some of these are: "observ," "vehicl," "state," "male," "drug," "black," or "arrest" — all align well with what one would expect to find in this type of literature. I then classified words into "positive" or "negative" using the Bing lexicon (Hu and Liu 2004), and computed each narrative's *sentiment* by subtracting the number of negative words from the positive word count.

Table C15 reports the OLS estimates of the following model:

$$Sentiment_{it} = \beta_1 NewBlackNeigh_{i(o)t} + \beta_2 BlackStop_{it} + \beta_3 NewBlackNeigh_{i(o)t} \times BlackStop_{it} + X_{it}^{'}\gamma + \epsilon_{it} \quad (4)$$

Treated officers write narratives with a more positive tone, as β_1 estimates range from 0.4 to 0.9 (up to almost 50% of a SD in *Sentiment*). The first four columns, however, suggest that treated White officers' sentiment in All-Black stops, relative to all-White stops, is, if anything, more negative (up to 0.4 points) — even though the overall effect of the treatment ($\beta_1 + \beta_3$) remains positive. Columns 5-8 indicate that these effects on the sentiment of Black interactions can be attributed in part to differences in the type of interactions. By restricting the sample to only "productive" interactions, β_3 estimates shift their range to -0.01-0.1 ($p > 0.1$). Overall, results suggest that exposure effects to a new Black neighbor improve the sentiment of *all* narratives, but there is no evidence of a differential shift in Black stops.

An alternative channel in which results are driven by officers' getting better at extracting or interpreting signals from out-group individuals (a learning hypothesis) should not impact the sentiment of narratives. Therefore, albeit not fully conclusive, the results presented here are most consistent with the contact hypothesis.

The effect of a new Black neighbor on Black officers. Table C16 shows that Black officers exposed

to a new Black neighbor also stop relatively fewer Blacks in FIs (4pp). When they do stop Black civilians, these stops are not differentially productive. Figure C2 digs deeper into these averages by analyzing the productivity subcomponents, the FI characteristics, and pretrends. It shows that the likelihood of finding drugs increases in the latter years following exposure to a new Black neighbor, an effect counterbalanced by a decrease in summons. Arrests increase in the earlier years and later decrease, and there is little variation in the likelihood of finding weapons. In terms of violence, frisks decrease overall, and searches remain stable. A caveat is that some outcomes (e.g., *Frisk*) exhibit differential pretrends.

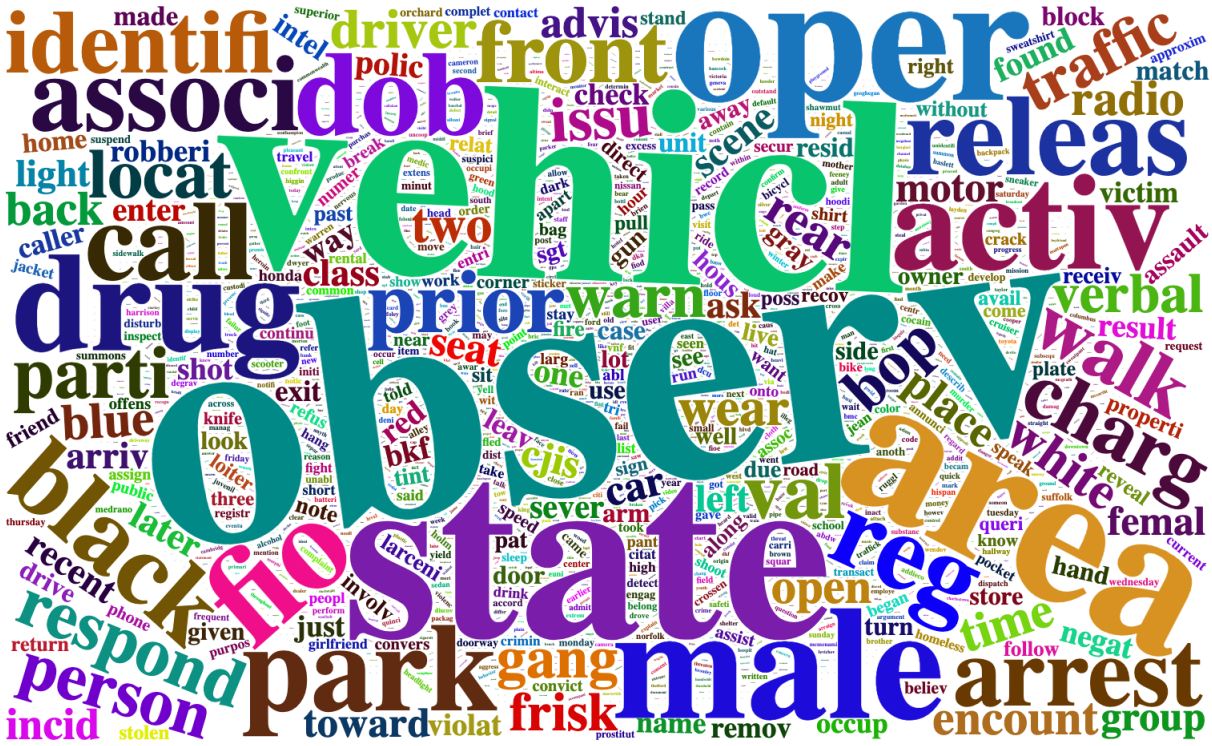


Figure C1: Word cloud of police narratives

Notes: Word cloud of police narratives (available in FIs after 2015). The figure displays the roots of the most common words in police narratives after pre-processing: removing punctions, stop words, and some location and other narrative-specific words (e.g., the original narratives replaced offender names with "xxx").

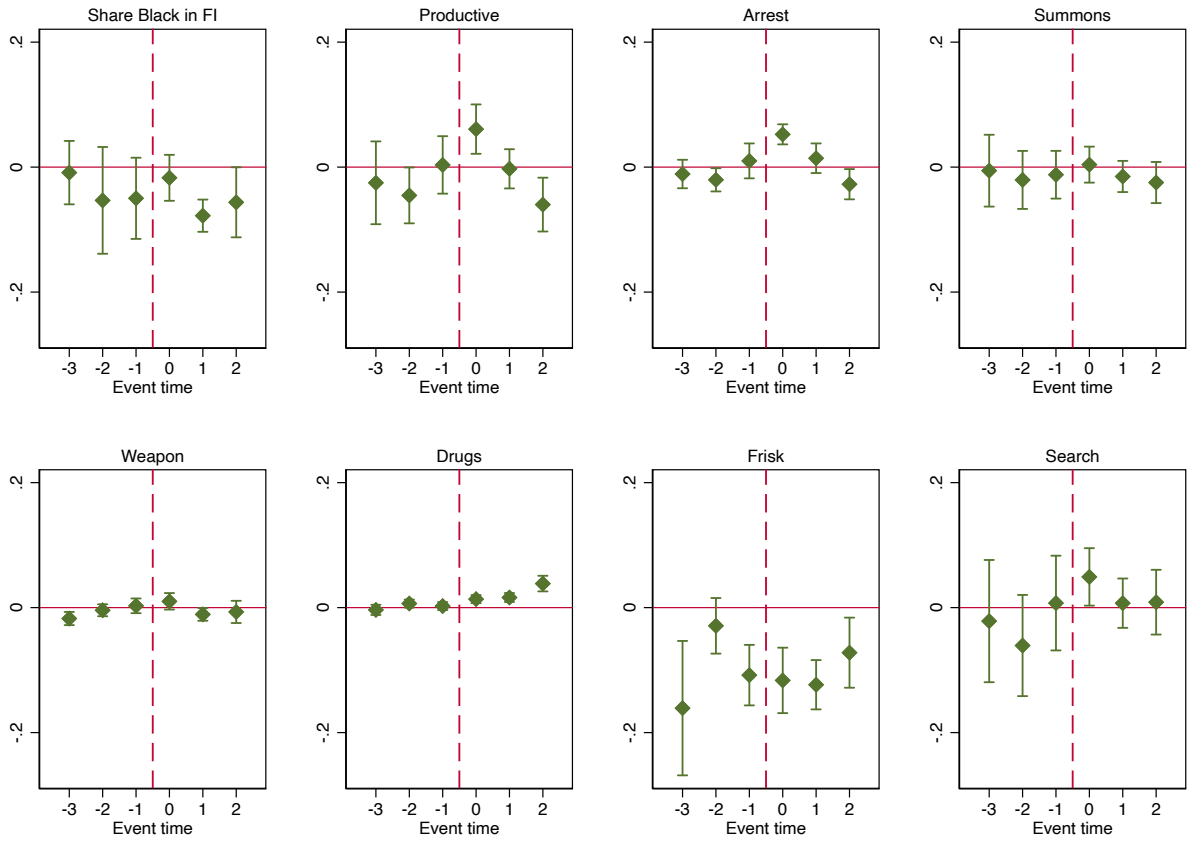


Figure C2: Black officers' new Black neighbors and racial differences in FIs

Notes: The plot illustrates τ_h estimates from Equation 3. Vertical bars represent 95% confidence intervals. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Drugs* and *Weapon* are indicators taking the value of one if drugs or weapons were found during the police interaction, respectively. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. Standard errors clustered at the tract-year.

Table C1: Productivity of all-Black stops

	Productive							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. All officers								
Black stop	-0.046***	-0.030***	-0.030***	-0.028***	-0.065***	-0.043***	-0.034***	-0.032***
	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
DV Mean	0.175	0.175	0.175	0.175	0.184	0.184	0.184	0.184
N	84765	84765	84765	84765	138965	138965	138965	138965
B. White officers								
Black stop	-0.041***	-0.028***	-0.028***	-0.026***	-0.066***	-0.045***	-0.034***	-0.031***
	(0.007)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)
DV Mean	0.180	0.180	0.180	0.180	0.190	0.190	0.190	0.190
N	62660	62660	62660	62660	108838	108838	108838	108838
C. Black officers								
Black stop	-0.061***	-0.041***	-0.034***	-0.026***	-0.059***	-0.038***	-0.027***	-0.021***
	(0.010)	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)
DV Mean	0.149	0.149	0.149	0.149	0.148	0.148	0.148	0.148
N	13737	13737	13737	13737	19181	19181	19181	19181
D. Hispanic officers								
Black stop	-0.050***	-0.011	-0.005	0.001	-0.046***	-0.018*	-0.011	-0.006
	(0.014)	(0.010)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)
DV Mean	0.160	0.160	0.160	0.160	0.168	0.168	0.168	0.168
N	7099	7099	7099	7099	9241	9241	9241	9241
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE		X	X	X		X	X	X
Officer ZIP FE			X				X	
Officer Tract FE				X				X
Sample	City	City	City	City	State	State	State	State

Notes: Sample of all (Panel A), White (Panel B), Black (Panel C), or Hispanic (Panel D) officers in the city (Columns 1-4) or state (Columns 5-8). *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Black stop* is an indicator taking the value of one if all individuals in the FI were black. It takes the value of 0 if none of the individuals in the FI were Black. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C2: Productivity of all-Black stops, by outcome

	Arrest		Summons		Drugs		Weapon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. All officers								
Black stop	-0.015***	-0.014***	-0.013***	-0.013***	-0.008***	-0.007***	-0.002*	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
DV Mean	0.067	0.067	0.101	0.101	0.024	0.024	0.031	0.031
N	84765	84765	84765	84765	84765	84765	84765	84765
B. White officers								
Black stop	-0.014***	-0.012***	-0.013***	-0.013***	-0.007***	-0.006***	-0.002	-0.001
	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
DV Mean	0.069	0.069	0.104	0.104	0.025	0.025	0.032	0.032
N	62660	62660	62660	62660	62660	62660	62660	62660
C. Black officers								
Black stop	-0.018***	-0.012**	-0.023***	-0.014**	-0.014***	-0.009***	-0.006**	-0.003
	(0.005)	(0.005)	(0.007)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)
DV Mean	0.052	0.052	0.093	0.093	0.021	0.021	0.025	0.025
N	13737	13737	13737	13737	13737	13737	13737	13737
D. Hispanic officers								
Black stop	-0.012	-0.003	0.001	0.005	0.001	0.001	-0.001	0.001
	(0.008)	(0.008)	(0.007)	(0.007)	(0.004)	(0.004)	(0.005)	(0.005)
DV Mean	0.066	0.066	0.088	0.088	0.022	0.022	0.029	0.029
N	7099	7099	7099	7099	7099	7099	7099	7099
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X

Notes: Sample of all (Panel A), White (Panel B), Black (Panel C), or Hispanic (Panel D) officers in the city. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Weapon* and *Drugs* are indicators taking the value of one if the FI resulted in weapons or drugs found. *Black stop* is an indicator taking the value of one if all individuals in the FI were black. It takes the value of zero if none of the individuals in the FI were Black. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C3: Veil of darkness: FI racial composition and outcomes in inter-twilight hours

	Share Black in FI		Arrest		Summons		Drugs		Weapon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. All officers										
Night	-0.018*	-0.018*	0.005	0.007	0.012	0.013*	0.006*	0.007**	0.002	0.001
	(0.011)	(0.011)	(0.008)	(0.008)	(0.008)	(0.008)	(0.003)	(0.003)	(0.005)	(0.005)
DV Mean	0.615	0.615	0.057	0.057	0.093	0.093	0.018	0.018	0.028	0.028
N	12200	12200	7837	7837	7837	7837	7837	7837	7837	7837
B. White officers										
Night	-0.014	-0.015	0.006	0.005	0.010	0.011	0.003	0.004	0.004	0.003
	(0.013)	(0.013)	(0.010)	(0.010)	(0.009)	(0.009)	(0.004)	(0.004)	(0.007)	(0.006)
DV Mean	0.613	0.613	0.060	0.060	0.096	0.096	0.019	0.019	0.029	0.029
N	9170	9170	5844	5844	5844	5844	5844	5844	5844	5844
C. Black officers										
Night	-0.051***	-0.050**	-0.014	-0.016	-0.013	-0.016	0.000	0.001	-0.002	-0.006
	(0.019)	(0.020)	(0.014)	(0.013)	(0.015)	(0.015)	(0.002)	(0.002)	(0.008)	(0.008)
DV Mean	0.665	0.665	0.043	0.043	0.083	0.083	0.014	0.014	0.021	0.021
N	1948	1948	1420	1420	1420	1420	1420	1420	1420	1420
D. Hispanic officers										
Night	0.032	0.021	0.024	0.037	0.101***	0.110***	0.043**	0.052**	-0.003	-0.003
	(0.033)	(0.032)	(0.042)	(0.042)	(0.031)	(0.034)	(0.020)	(0.022)	(0.025)	(0.029)
DV Mean	0.582	0.582	0.045	0.045	0.084	0.084	0.018	0.018	0.027	0.027
N	970	970	519	519	519	519	519	519	519	519
Year FE	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X		X

Notes: Sample of all (Panel A), White (Panel B), Black (Panel C), or Hispanic (Panel D) officers in the city. Only FIs after 2015 (the time of the stop was not reported before) during inter-twilight hours (5 to 10 PM) are included. Columns 3-10 only include Black stops. All outcomes are as previously defined. *Night* is an indicator taking the value of one if the stop occurred after civil twilight (approximately – I only observe hour intervals). All regressions control for officers' tenure. All regressions include time of the day fixed effects. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C4: Distance to policing neighborhoods and racial differences in FIs

	Share Black in FI						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Within Distance	-0.037***	-0.035***	-0.027***	-0.033***	-0.033***	-0.059***	
	(0.014)	(0.012)	(0.009)	(0.008)	(0.008)	(0.014)	
Distance (Km)							0.089***
							(0.017)
DV Mean	0.613	0.613	0.613	0.613	0.613	0.613	0.613
N	67062	67062	67062	67062	67062	67062	67062
	Productive						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Within Distance	-0.002	0.024	0.007	-0.002	-0.013	-0.025**	
	(0.012)	(0.015)	(0.011)	(0.009)	(0.009)	(0.012)	
Distance (Km)							0.017
							(0.019)
DV Mean	0.163	0.163	0.163	0.163	0.163	0.163	0.163
N	38715	38715	38715	38715	38715	38715	38715
Year FE	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X
Range	Same ZIP	<2km	<3km	<5km	<7km	<10km	

Notes: The sample includes all FIs from White officers in the city. *Share Black in FI* is the share of Black individuals in the police interaction. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Within Distance* is an indicator taking the value of one if the officer resides within a given distance (in the same ZIP, or within 2 to 10 Km) to the FI neighborhood (centroid of the FI ZIP code). *Distance (Kms)* is the standardized distance (in Kms) between the officers' neighborhood (centroid of the ZIP) and the FI neighborhood. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C5: Neighborhoods and racial differences in FIs – by officers party affiliation

	Share Black in FI						Productive					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Share Black in Neighborhood	-0.014 (0.015)	-0.053 (0.032)	0.001 (0.008)	0.005 (0.017)	-0.070*** (0.020)	-0.041** (0.018)	0.015 (0.012)	-0.008 (0.038)	0.014* (0.007)	0.025** (0.013)	0.046 (0.030)	0.174*** (0.064)
DV Mean	0.569	0.569	0.627	0.627	0.619	0.619	0.132	0.132	0.163	0.163	0.198	0.198
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X		X		X
Party	D	D	N	N	R	R	D	D	N	N	R	R

Notes: The sample includes all FIs from White officers in City. Columns 7-12 restrict the sample to all-Black FIs. *Share Black in FI* is the share of Black individuals in the police interaction. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Share Black in Neighborhood* is the standardized share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey. Officers party affiliation can be Democratic (D), Republican (R), or None (N). The number of observations is undisclosed for confidentiality reasons. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C6: Neighborhoods and racial composition and productivity of FIs (All officers)

	Share Black in FI				Productive			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. White officers								
Share Black in Neighborhood	-0.001	-0.003	0.008	-0.002	0.013**	0.012**	0.014*	0.022**
	(0.010)	(0.006)	(0.006)	(0.011)	(0.006)	(0.005)	(0.007)	(0.010)
DV Mean	0.594	0.594	0.594	0.594	0.161	0.161	0.161	0.161
N	115610	115610	115610	115610	65126	65126	65126	65126
B. Black officers								
Share Black in Neighborhood	-0.018***	-0.010**	-0.006	-0.022***	0.003	0.002	-0.002	0.002
	(0.006)	(0.004)	(0.005)	(0.007)	(0.004)	(0.004)	(0.006)	(0.009)
DV Mean	0.664	0.664	0.664	0.664	0.128	0.128	0.128	0.128
N	20227	20227	20227	20227	12888	12888	12888	12888
C. Hispanic officers								
Share Black in Neighborhood	0.062***	0.028***	0.011	-0.051**	-0.010	-0.008	0.018	0.047
	(0.014)	(0.007)	(0.014)	(0.023)	(0.010)	(0.009)	(0.020)	(0.031)
DV Mean	0.554	0.554	0.554	0.554	0.144	0.144	0.144	0.144
N	9892	9892	9892	9892	5146	5146	5146	5146
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE		X	X	X		X	X	X
Officer ZIP FE			X				X	
Officer Tract FE				X				X

Notes: The sample includes all FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the state. Only FIs after 2015 are included (the time of the stop was not reported before). Columns 5-8 restrict the sample to all-Black FIs. All outcomes are as previously defined. *Share Black in Neighborhood* is the standardized share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C7: Neighborhoods and outcomes of all-Black FIs (All officers)

	Arrest		Summons		Drugs		Weapon		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. White officers												
Share Black in Neighborhood	0.003	0.008	0.007*	0.009	0.003**	0.003	0.004**	0.010**	-0.006	0.034**	0.003	0.026**
	(0.003)	(0.007)	(0.004)	(0.007)	(0.001)	(0.003)	(0.002)	(0.005)	(0.008)	(0.015)	(0.006)	(0.011)
DV Mean	0.059	0.059	0.096	0.096	0.020	0.020	0.030	0.030	0.364	0.364	0.189	0.189
N	65126	65126	65126	65126	65126	65126	65126	65126	65126	65126	65126	65126
B. Black officers												
Share Black in Neighborhood	0.002	-0.006	-0.001	0.009	-0.000	-0.000	0.003**	-0.000	0.015**	0.037***	0.008	0.020**
	(0.002)	(0.004)	(0.003)	(0.006)	(0.001)	(0.002)	(0.001)	(0.001)	(0.007)	(0.011)	(0.005)	(0.009)
DV Mean	0.042	0.042	0.083	0.083	0.015	0.015	0.020	0.020	0.366	0.366	0.175	0.175
N	12888	12888	12888	12888	12888	12888	12888	12888	12888	12888	12888	12888
C. Hispanic officers												
Share Black in Neighborhood	-0.012***	-0.004	0.002	0.042	-0.004**	0.008	-0.007**	0.003	0.016*	0.067*	-0.019*	0.017
	(0.004)	(0.011)	(0.007)	(0.027)	(0.002)	(0.011)	(0.003)	(0.009)	(0.010)	(0.039)	(0.010)	(0.026)
DV Mean	0.051	0.051	0.088	0.088	0.021	0.021	0.032	0.032	0.351	0.351	0.178	0.178
N	5146	5146	5146	5146	5146	5146	5146	5146	5146	5146	5146	5146
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X		X		X

Notes: The sample includes all-Black FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the state. All outcomes are as previously defined. *Share Black in Neighborhood* is the standardized share of Black population in officers' census block group in the year of the FI obtained from the American Community Survey. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C8: Neighborhoods and racial differences in FIs (2010 Census Block)

	Share Black in FI				Productive			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. White officers								
Share Black in 2010 Block	-0.019 (0.012)	-0.016* (0.008)	-0.003 (0.008)	0.000 (0.011)	0.011 (0.008)	0.010 (0.007)	0.010 (0.008)	0.013 (0.016)
DV Mean	0.613	0.613	0.613	0.613	0.163	0.163	0.163	0.163
N	66950	66950	66950	66950	38565	38565	38565	38565
B. Black officers								
Share Black in 2010 Block	-0.026*** (0.007)	-0.008* (0.005)	-0.003 (0.007)	-0.015** (0.007)	0.015*** (0.004)	0.013*** (0.004)	0.014** (0.006)	0.019*** (0.006)
DV Mean	0.665	0.665	0.665	0.665	0.130	0.130	0.130	0.130
N	14378	14378	14378	14378	9204	9204	9204	9204
C. Hispanic officers								
Share Black in 2010 Block	0.053*** (0.014)	0.022*** (0.006)	0.023* (0.011)	-0.023 (0.018)	-0.003 (0.009)	-0.000 (0.008)	0.000 (0.015)	0.006 (0.024)
DV Mean	0.582	0.582	0.582	0.582	0.133	0.133	0.133	0.133
N	7527	7527	7527	7527	4149	4149	4149	4149
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE		X	X	X		X	X	X
Officer ZIP FE			X				X	
Officer Tract FE				X				X

Notes: The sample includes all FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the city. Columns 5-8 restrict the sample to all-Black FIs. *Share Black in FI* is the share of Black individuals in the police interaction. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *Share Black in 2010 Block* is the standardized share of the Black population in officers' census block in the 2010 Decennial Census. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C9: Neighborhoods and outcomes of all-Black FIs (2010 Census Block)

	Arrest		Summons		Drugs		Weapon		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. White officers												
Share Black in 2010 Block	0.000	-0.001	0.008	0.010	0.002*	0.006**	0.007***	0.020***	0.014	0.051***	0.002	0.022
	(0.004)	(0.015)	(0.005)	(0.009)	(0.001)	(0.003)	(0.002)	(0.004)	(0.009)	(0.019)	(0.007)	(0.016)
DV Mean	0.060	0.060	0.096	0.096	0.019	0.019	0.029	0.029	0.330	0.330	0.186	0.186
N	38565	38565	38565	38565	38565	38565	38565	38565	38565	38565	38565	38565
B. Black officers												
Share Black in 2010 Block	0.004*	-0.000	0.008**	0.019***	0.002*	-0.001	0.003***	0.000	0.014**	0.031**	0.013*	0.020**
	(0.002)	(0.003)	(0.003)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.006)	(0.012)	(0.007)	(0.008)
DV Mean	0.043	0.043	0.083	0.083	0.014	0.014	0.021	0.021	0.358	0.358	0.174	0.174
N	9204	9204	9204	9204	9204	9204	9204	9204	9204	9204	9204	9204
C. Hispanic officers												
Share Black in 2010 Block	-0.011***	-0.005	0.008	0.008	-0.002	-0.002	-0.003	-0.002	0.024***	0.050*	-0.003	0.010
	(0.004)	(0.013)	(0.007)	(0.019)	(0.002)	(0.008)	(0.002)	(0.007)	(0.008)	(0.028)	(0.007)	(0.020)
DV Mean	0.045	0.045	0.084	0.084	0.018	0.018	0.027	0.027	0.351	0.351	0.168	0.168
N	4149	4149	4149	4149	4149	4149	4149	4149	4149	4149	4149	4149
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE		X		X		X		X		X		X

Notes: The sample includes all-Black FIs from White (Panel A), Black (Panel B), or Hispanic (Panel C) officers in the city. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Weapon* and *Drugs* are indicators taking the value of one if the FI resulted in weapons or drugs found. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. *Share Black in 2O1O Block* is the standardized share of Black population in officers' census block in the 2010 Decennial Census. All regressions control for officers' tenure. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C10: Robustness: additional sensitivity tests

Panel A. Racial composition of interactions

	Share Black in FI					
	(1)	(2)	(3)	(4)	(5)	(6)
New Black neighbor	-0.142***	-0.158***	-0.067***	-0.120***	-0.160***	-0.164***
	(0.010)	(0.012)	(0.011)	(0.008)	(0.011)	(0.011)
DV Mean	0.615	0.615	0.614	0.614	0.613	0.613
N	63355	63355	65949	65949	66428	66428

Panel B. Productivity of interactions

	Productive							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	0.101***	0.120***	0.109***	0.117***	0.118***	0.133***	0.015	-0.014
	(0.016)	(0.014)	(0.014)	(0.010)	(0.016)	(0.015)	(0.016)	(0.022)
DV Mean	0.162	0.162	0.163	0.163	0.163	0.163	0.201	0.201
N	36667	36667	38159	38159	38388	38388	24078	24078
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X
Officer Tract FE	X		X		X		X	
Officer FE		X		X		X		X
Test	Exposed to NN		NN in Same Street		FIs pre-COVID		Only All-White FIs	

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only, except in columns 5-6. All outcomes are as previously defined. *New Black Neighbor* as previously defined. Columns 1-2 restrict the sample of control individuals to those exposed to a new White neighbor. Columns 3-4 restrict the definition of *New Black Neighbor* to officers exposed to a new neighbor in the same street. Columns 5-6 drop all FIs after March 1st, 2020. Columns 7-8 investigate productivity effects on the subsample of all-White interactions. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table C11: Robustness: change in data reporting

Panel A. Racial composition and productivity of all-Black FIs

	Shr Black in FI		Productive		Arrest		Summons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	-0.075***	-0.174***	-0.006	0.109***	0.004	0.006	-0.011	0.121***
	(0.013)	(0.014)	(0.015)	(0.018)	(0.008)	(0.018)	(0.015)	(0.011)
DV Mean	0.596	0.646	0.157	0.177	0.045	0.090	0.110	0.069
N	45409	22008	25995	12874	25995	12874	25995	12874

Panel B. Productivity and characteristics of all-Black FIs

	Weapon		Drugs		Frisk		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	-0.006	-0.013	0.006**	0.031***	-0.104***	-0.107***	-0.048	-0.024
	(0.006)	(0.009)	(0.003)	(0.004)	(0.021)	(0.025)	(0.031)	(0.025)
DV Mean	0.029	0.030	0.021	0.014	0.358	0.272	0.205	0.145
N	25995	12874	25995	12874	25995	12874	25995	12874
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X
Officer Tract FE	X	X	X	X	X	X	X	X
Data Source	2011-15	2015-20	2011-15	2015-20	2011-15	2015-20	2011-15	2015-20

Notes: FIs from White officers in the city. Odd columns use the subsample of FIs from 2011-15 (old reporting system). Even columns use the subsample of FIs from 2015-20. All outcomes are as previously defined. *New Black neighbor* as previously defined. All specifications include tenure as a control. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C12: Robustness: accounting for spatial autocorrelation

Panel A. Racial composition of interactions

	Share Black in FI				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	-0.153**	-0.153**	-0.153**	-0.153**	-0.153**
	(0.063)	(0.063)	(0.062)	(0.062)	(0.062)
DV Mean	0.612	0.612	0.612	0.612	0.612
N	66813	66813	66813	66813	66813

Panel B. Productivity of all-Black interactions

	Productive				
	(1)	(2)	(3)	(4)	(5)
New Black neighbor	0.114**	0.114**	0.114**	0.114**	0.114**
	(0.055)	(0.054)	(0.055)	(0.055)	(0.056)
DV Mean	0.162	0.162	0.162	0.162	0.162
N	38477	38477	38477	38477	38477
Year FE	X	X	X	X	X
FI ZIP FE	X	X	X	X	X
Officer Tract FE	X	X	X	X	X
Conley SE Dist Cutoff (m)	200	350	500	750	1000

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. *Share Black in FI* denotes the share of Black civilians in the FI. *Productive* is an indicator taking the value of one if the FI resulted in an arrest or summons issued or in drugs or weapons found. *New Black Neighbor* is an indicator taking the value of one if a Black individual moves into a previously non-Black-owned and occupied dwelling within 100 meters of the officer's residence. All specifications include tenure as a control. Different columns report Conley standard errors with varying distance cutoffs, from 200 meters to one kilometer (Conley 1999). Estimates are computed by OLS with the Stata command *acreg* (Colella et al. 2023). *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table C13: Robustness: alternative TWFE estimation methods

		Conditional on All-Black in FI						
	Share Black in FI	Productive	Arrest	Summons	Weapon	Drugs	Frisk	Search
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS	-0.154***	0.101*	0.023	0.080**	-0.003	0.029*	-0.075**	-0.018
	(0.060)	(0.052)	(0.037)	(0.040)	(0.008)	(0.016)	(0.034)	(0.026)
Borusyak, Jaravel and Spiess 2021	-0.154***	0.100***	0.022**	0.078***	-0.003	0.030***	-0.076***	-0.019
	(0.011)	(0.015)	(0.009)	(0.010)	(0.004)	(0.004)	(0.019)	(0.017)
De Chaisemartin and D'Haultfoeuille 2022	-0.104	0.009	-0.041	0.054	-0.033	-0.020	-0.062	0.004
	(0.097)	(0.068)	(0.042)	(0.052)	(0.031)	(0.020)	(0.067)	(0.078)
Sun and Abraham 2021	-0.153***	0.100***	0.023***	0.078***	-0.003***	0.029***	-0.075***	-0.019***
	(0.007)	(0.005)	(0.005)	(0.006)	(0.001)	(0.002)	(0.004)	(0.004)

Notes: The sample includes all FIs from White officers in the city. Columns 2-10 restrict the sample to all-Black FIs. *Arrest* and *Summons* are indicators taking the value of one if at least one individual was arrested or summonsed during the police interaction, respectively. *Drugs* and *Weapon* are indicators taking the value of one if drugs or weapons were found during the police interaction, respectively. *Frisk* is an indicator taking the value of one if the interaction included a frisk. *Search* is an indicator taking the value of one if the interaction included a search. The first, second, third, and fourth-row estimates are obtained using *reghdfe*, *did_imputation* (Borusyak et al. 2024), *did_multiplegt* (De Chaisemartin and d'Haultfoeuille 2022a), and *eventstudyinteract* (Sun and Abraham 2021), respectively. All estimators include year and FI ZIP fixed effects. All, except De Chaisemartin and d'Haultfoeuille (2022a) in the third row, also include officer census tract fixed effects. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table C14: Mechanisms: FI details

Panel A. All interactions

	Observed Only		Encounter		Stop		Frisked		Search		Productive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New Black neighbor	-0.060***	-0.025	0.124***	0.036**	-0.046***	-0.012	-0.135***	-0.106***	-0.054***	-0.061***	0.061***	0.052***
	(0.015)	(0.018)	(0.021)	(0.016)	(0.016)	(0.019)	(0.014)	(0.013)	(0.015)	(0.014)	(0.013)	(0.013)
DV Mean	0.188	0.188	0.544	0.544	0.561	0.561	0.302	0.302	0.181	0.181	0.179	0.179
N	68910	68910	68910	68910	68910	68910	68910	68910	68910	68910	68910	68910

Panel B. Black interactions

	Observed Only		Encounter		Stop		Frisked		Search		Productive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New Black neighbor	-0.077***	-0.003	0.138***	-0.054***	0.015	0.053***	-0.076***	-0.032**	-0.019	-0.022	0.100***	0.120***
	(0.018)	(0.017)	(0.027)	(0.018)	(0.017)	(0.020)	(0.019)	(0.016)	(0.017)	(0.015)	(0.015)	(0.014)
DV Mean	0.182	0.182	0.552	0.552	0.578	0.578	0.330	0.330	0.186	0.186	0.163	0.163
N	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873	38873

Panel C. White interactions

	Observed Only		Encounter		Stop		Frisked		Search		Productive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New Black neighbor	-0.033	-0.048**	0.126***	0.132***	-0.122***	-0.084***	-0.174***	-0.167***	-0.094***	-0.107***	0.015	-0.014
	(0.020)	(0.022)	(0.026)	(0.019)	(0.022)	(0.027)	(0.017)	(0.028)	(0.017)	(0.025)	(0.016)	(0.022)
DV Mean	0.199	0.199	0.552	0.552	0.527	0.527	0.252	0.252	0.176	0.176	0.201	0.201
N	24078	24078	24078	24078	24078	24078	24078	24078	24078	24078	24078	24078
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X	X	X	X	X
Officer Tract FE	X		X		X		X		X		X	
Officer FE		X		X		X		X		X		X

Notes: The sample in Panel A includes all FIs from White officers in the city. Panel B restricts the sample to all-Black FIs only. Panel C restricts the sample to all-White FIs only. *Observed Only* is an indicator taking the value of one if the FI involved an observation (without direct contact). *Encounter* is an indicator taking a value of one if the FI included a consensual encounter. *Stop* is an indicator taking the value of one if the FI included a stop. *Encounter* and *Stop* are not mutually exclusive. The rest of the outcomes are as previously defined. *New Black Neighbor* as previously defined. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C15: Mechanisms: sentiment analysis

	Sentiment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Black neighbor	0.640***	0.886***	0.418**	0.472**	0.875***	0.862***	0.852***	0.780*
	(0.090)	(0.125)	(0.184)	(0.184)	(0.197)	(0.325)	(0.319)	(0.423)
Black stop	0.139***	0.151***	0.030	0.033	0.045	0.044	-0.090	-0.095
	(0.045)	(0.045)	(0.045)	(0.047)	(0.094)	(0.098)	(0.101)	(0.106)
New Black neighbor		-0.369**		-0.100		0.019		0.115
× Black stop		(0.158)		(0.150)		(0.303)		(0.285)
DV Mean	-0.943	-0.943	-0.943	-0.943	-1.331	-1.331	-1.331	-1.331
DV SD	1.813	1.813	1.813	1.813	2.159	2.159	2.159	2.159
N	15823	15823	15823	15823	3130	3130	3130	3130
Year FE	X	X	X	X	X	X	X	X
FI ZIP FE	X	X	X	X	X	X	X	X
Officer Tract FE	X	X			X	X		
Officer FE			X	X			X	X
Sample	Full	Full	Full	Full	Prod	Prod	Prod	Prod

Notes: All-Black or all-White FIs from White officers in the city. Columns 1-4 include all FIs in which the narrative is available. Columns 5-8 restrict the sample to *productive* interactions, as previously defined. *Sentiment* is the result of subtracting the number of negative words from the positive word count, with words classified as positive or negative using the Bing sentiment lexicon (Hu and Liu 2004). *New Black Neighbor* is an indicator taking the value of one if a Black individual moves into a previously non-Black-owned and occupied dwelling within 100 meters of the officer's residence. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year level in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table C16: Black officers' new Black neighbors and racial differences in FIs

Panel A. Racial composition of interactions

	Share Black in FI			
	(1)	(2)	(3)	(4)
New Black neighbor	-0.006	-0.025*	-0.035***	-0.021
	(0.011)	(0.013)	(0.013)	(0.019)
DV Mean	0.678	0.678	0.678	0.678
N	13977	13977	13977	13977

Panel B. Productivity of all-Black interactions

	Productive			
	(1)	(2)	(3)	(4)
New Black neighbor	-0.000	-0.000	0.002	0.048***
	(0.012)	(0.013)	(0.014)	(0.018)
DV Mean	0.127	0.127	0.127	0.127
N	9097	9097	9097	9097
Year FE	X	X	X	X
FI ZIP FE		X	X	X
Officer ZIP FE		X		
Officer Tract FE			X	
Officer FE				X

Notes: The sample in Panel A includes all FIs from Black officers in the city. Panel B restricts the sample to all-Black FIs only. All outcomes are as previously defined. *New Black Neighbor* as previously defined. Specifications without officer fixed effects include tenure as a control. Standard errors clustered at the tract-year in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

References

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 2024, p. rdae007.

Callaway, Brantly and Pedro H.C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230. Themed Issue: Treatment Effect 1.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Difference-in-differences estimators of intertemporal treatment effects," Technical Report, National Bureau of Economic Research 2022.

____ and ____, "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey," Technical Report, National Bureau of Economic Research 2022.

Colella, Fabrizio, Rafael Lalive, Seyhun Orcan Sakalli, and Mathias Thoenig, "acreg: Arbitrary correlation regression," *The Stata Journal*, 2023, *23* (1), 119–147.

Conley, Timothy G, "GMM estimation with cross sectional dependence," *Journal of Econometrics*, 1999, *92* (1), 1–45.

Grogger, Jeffrey and Greg Ridgeway, "Testing for racial profiling in traffic stops from behind a veil of darkness," *Journal of the American Statistical Association*, 2006, *101* (475), 878–887.

Hu, Minqing and Bing Liu, "Mining and summarizing customer reviews," in "Proceedings of the tenth ACM SIGKDD international conference on Knowledge discovery and data mining" 2004, pp. 168–177.

Persico, Nicola, "Racial profiling, fairness, and effectiveness of policing," *American Economic Review*, 2002, *92* (5), 1472–1497.

Pierson, Emma, Camelia Simoiu, Jan Overgoor, Sam Corbett-Davies, Daniel Jenson, Amy Shoemaker, Vignesh Ramachandran, Phoebe Barghouty, Cheryl Phillips, Ravi Shroff et al., "A large-scale analysis of racial disparities in police stops across the United States," *Nature human behaviour*, 2020, *4* (7), 736–745.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.