

Evidence of Donor Bias in Chicago Police Stops

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This study provides empirical evidence that private donations to police departments can influence the spatial allocation of discretionary enforcement. Using a quasi-experimental design in Chicago, we find that after 7-Eleven sponsored a police foundation gala, investigatory stops, particularly pedestrian stops, increased near its stores, with no robust corresponding change in vehicular stops and no detectable change in crime. The increase fell disproportionately on Black pedestrians, either through race-neutral enforcement applied to a racially skewed pedestrian population or through race-differentiated policing decisions. These findings suggest that even modest private donations can distort the allocation of public law enforcement resources toward donor-affiliated locations.

Introduction

Private donations to public police departments have long raised questions about their influence on law enforcement behavior. Policing is often framed as a public service—an institution that enforces laws impartially to maintain order and ensure safety. Yet, behind the scenes, a different economic reality is increasingly at play: thousands of police departments in the United States and Canada receive hundreds of millions of dollars annually in private donations from businesses,

industry groups, and even individuals to fund operations, equipment purchases, or community engagement work (15, 18). Scholars of policing have cautioned that corporate gifts to police may create biases or de facto obligations, generating transparency problems and a private co-optation of a public service (8, 15). However, to date, the evidence linking private donations to changes in police behavior is very limited.

Why might private donations influence police behavior? Donations can be seen as a form of gift exchange that, while legal, can often create pressures (implicit or explicit) to return the favor (7). Psychology scholars argue that cooperation is a critical component of human behavior, and reciprocity plays a key role in maintaining cooperation in contexts where people expect to encounter others repeatedly (14). According to the economics of reciprocity, humans give gifts expecting a material benefit from their actions, and the gift recipient has incentives to return the favor even if it is costly for them and yields neither present nor future material rewards (4, 5). Evidence of this positive reciprocity has been documented in many trust or gift exchange experiments, and results have been consistently independent of the amount of money at stake (4, 5, 10, 13).

We theorize that private donations to police may create what we call “donor bias,” a shift in discretionary enforcement toward donor-affiliated locations. This bias could arise through multiple channels. Donations may induce feelings of reciprocity among officers aware of a benefactor’s contributions (4, 5, 19). They may also shape organizational priorities, directing attention toward donor properties (12, 17). Alternatively, donations may operate as a form of rent-seeking, enabling firms to secure preferential access to public safety resources (10). We do not adjudicate between these mechanisms empirically. We note, however, that the modest size of the donation we study (\$10,000) makes a narrowly transactional quid pro quo an unlikely sole explanation, and points toward a reciprocity-based “donor bias” channel, we develop this more in our Discussion section. Our data identify changes in enforcement patterns but not the motivations or internal processes that produce them. Instead, we test whether enforcement behavior shifts in ways consistent with donor influence, regardless of the underlying pathway.

To test our theory, we use data on corporate donations to the Chicago Police Department (CPD) and on police investigatory stop data between January 2016 and February 2020. We identify a Chicago Police donor with physical locations across the city to evaluate whether police officers

changed their behavior after a donation. We focused on donations through public sponsorship to the Chicago Police Foundation's (CPF) True Blue Gala (TBG), as it allowed us to test the effect of a donation whose sponsor the foundation widely advertised in newsletters to police officers. Donations to the TBG are publicized on the CPF and CPD's social media pages, as well as in the Fraternal Order of Police's magazine. Additionally, CPD officers attend the gala each year, either as guests or to provide demonstrations for non-police attendees. Attending officers are exposed to information about the gala's donors and have stated that they share it with their peers on the force.

We systematically assessed every corporate sponsor of the TBG between 2016 and 2024 against four design requirements: (1) a sufficient number of physical locations to generate statistical power for block-level analysis, (2) city-wide geographic distribution rather than spatial concentration in one area, (3) a one-time donation permitting clean pre/post identification, and (4) a walk-up retail format relevant to pedestrian enforcement (see Table S3 in the Appendix for the full assessment). Among all sponsors, most have a single Chicago location (e.g., Eli's Cheesecake, Hyatt Regency), are concentrated downtown (LAZ Parking), or donated in multiple consecutive years (BMO Harris Bank, Cigna). 7-Eleven is the only sponsor satisfying all four requirements, and it was identified as the focal case in our pre-registration prior to estimation. We emphasize that this is a structural constraint of our research design, not a selective choice: the block-level quasi-experimental approach requires a donor with the specific spatial characteristics described above, and 7-Eleven is the only TBG sponsor that meets these requirements.

The 7-Eleven retail chain provided an ideal case to test our hypothesis on the effects of donations to police, given the wide distribution of their stores across the city, and the fact that 7-Eleven only sponsored the police gala once during our period of study. Using a pre-registered quasi-experimental design and unique block-level data, we leverage the temporal timing of the donation (November 2017) and the geographic distribution of 7-Eleven stores. We compared blocks with 7-Eleven stores to blocks with major intersections (after ensuring that the two groups had comparable crime rates and socio-demographic characteristics). We also used different comparison groups and specifications to assess whether our findings were robust to these changes (see Methodological Appendix). We estimate whether changes in police investigatory stop patterns by race occurred

after 7-Eleven sponsored the TBG in November 2017.¹

We subject these findings to extensive robustness checks and they are robust to changes across models, specifications, and different control groups. We estimate fixed-effects Poisson models that provide multiplicative (percentage-change) effects, test alternative control groups (other convenience stores), conduct temporal and spatial placebo tests, examine whether crime changed at treated locations, and verify that no small subset of blocks drives the results. These analyses, reported alongside the main results and in the Methodological Appendix, consistently support our core findings.

Testing our theory on the entire pool of donors to the TBG was not possible due to the dearth of publicly available data on officer behavior. For example, Citadel LLC founder and CEO Kenneth Griffin has donated millions of dollars to the Chicago Police Department, which may have shaped officer perceptions or behavior toward himself or the company. Citadel LLC's headquarters, however, is in a downtown Chicago office building with a low crime rate. If Citadel LLC were to receive any indirect reciprocity from the police for their donations, it would likely appear through officers with knowledge of individual Citadel employees or through officers' overall attitudes toward the hedge fund. Empirically examining such an effect would require detailed data on officer behavior and perceptions that do not exist for evaluation.

Beyond this data constraint, the expected mechanism of donor influence differs fundamentally across donor types. A hedge fund like Citadel, whose headquarters has no walk-up retail interface, would plausibly influence officer attitudes or political goodwill rather than street-level enforcement near a specific location, an effect that would require survey or interview data on officer perceptions, not stop counts. Different donor types generate different forms of expected reciprocity, and the empirical strategy required to identify each form differs accordingly. A null result for a downtown office tenant such as Citadel would therefore be uninformative rather than reassuring: the design tests street-level retail enforcement, a channel that does not operate at a 9 headquarters with no walk-up retail interface.

We estimate 7-Eleven donated \$10,000 U.S. dollars as part of their TBG sponsorship. While the amount of this donation was modest, donations to the TBG are highly publicized. Sponsors' logos

¹The preregistered analysis plan is available on the Center for Open Science Archive at <https://osf.io/ye9uk/>.

are prominently displayed and directly acknowledged by police leadership and rank-and-file police officers. Sponsorship of the TBG operates as a symbolic signal of allegiance. As previous behavioral literature has shown, individuals tend to reciprocate gifts independent of the amount, especially when there is the possibility of receiving future benefits from their benefactors, such as in the case of stable relations between corporate donations and police departments. In this case, police officers might be more likely to reciprocate the 7-Eleven donation because it was personally observable, identity-affirming, and a public signal of solidarity from the corporation to the police.

Police officers attending the TBG often describe feeling “supported” or “backed up” by the sponsor’s participation in the gala. Chicago Police Superintendent Larry Snelling stated:

“These gestures may seem small, but they go a long way with our officers, trust me (. . .) The support you have shown for our officers here today, and that you continually show for our officers, it lets me know there’s a guarantee out there that our officers are always going to have someone who has their back (. . .) And that is very important moving forward. Because we know that you have our backs, we will always have yours (. . .)”²

The gala format donation enables officers to interact with donors and facilitates the circulation of sponsorship images across police stations. The visibility of the TBG sponsors provides a particular setting where police officers share photos of the event with their peers, reaching police officers who did not attend the gala. Therefore, we conceptualize the 7-Eleven donation as a public signal of solidarity that could shape discretionary enforcement decisions in the locations where officers encounter the donor in everyday patrol environments.

The 7-Eleven TBG donation occurred alongside longer-standing community-police initiatives, such as providing police officers with coupons for free beverages under the “Operation Chill” program, which has been used to cultivate positive relationships between officers and 7-Eleven stores. Together, 7-Eleven’s contributions to the police created a high-visibility pro-police signal within Chicago that could potentially trigger a reciprocity behavioral pattern from police officers

²Chicago’s Police Superintendent, Fraternal Order of Police Chicago Lodge 7 Magazine. November, 2023. Page 48–50. <https://s3.us-east-1.amazonaws.com/NEP/Chicago+FOP+Documents/Newsletters/2023/NOV23.pdf>

to 7-Eleven stores.

Our estimations assessing the effect of small donations on police stop patterns are a critical first step in the systematic evaluation of gift-giving to police departments. Regardless of whether one views investigatory stops as beneficial public safety tools or intrusive encounters, the core finding that private donations can systematically alter where and how police deploy their discretionary authority raises fundamental questions about the impartial allocation of public law enforcement resources. If donations shape enforcement geography, then policing intensity becomes partly a function of private donations rather than public need, a distortion of the public mandate, regardless of one's normative view of stops themselves.

Results

Table 1 shows the main findings of our estimations assessing the effect of the 7-Eleven donation on police investigatory stops, controlling for lagged block-level crime. We present results across two control groups: blocks with major intersections (Panel A) and blocks with other convenience stores (Panel B). We treat major-intersection blocks as our primary control group because the empirical evidence supports the parallel pre-trends assumption in both stops and crime prior to the donation (see Figures 1- 3; and Figure S2 in the Appendix). Other convenience store blocks are included as a secondary sensitivity analysis rather than as the primary identification strategy: their pre-trends do not converge to zero prior to the donation (see Figures S3 and S4 in the Appendix), but the pedestrian-stop effect persists even under this less favorable comparison.

To contextualize the magnitude of these estimates, the pre-donation quarterly mean of all investigatory stops at 7-Eleven blocks was 0.89 per block. In Panel A, blocks with a 7-Eleven within 330 ft³ experienced an increase of 0.612 additional stops per block per quarter ($p < 0.001$), representing a 69% increase over the pre-donation baseline (see Table S1 for all baseline descriptive statistics in the Methodological Appendix). Across all 626 treated blocks, this translates to approximately 383 (0.612×626) additional investigatory stops per quarter. Although this is a small share of city-wide stops, the additional stops are concentrated entirely within treated blocks, which constitute fewer than 2% of all Chicago blocks. The effect is therefore geographically concentrated in a small

³330 feet is the length of a standard Chicago block; see the Data & Method section

slice of the city's blocks. As we anticipated, pedestrian stops drive the effect (0.551 additional per block-quarter, a 76% increase over the pre-donation pedestrian stop mean of 0.72), and vehicular stop coefficients are an order of magnitude smaller. Panel B confirms the pedestrian stop finding using other convenience stores as an alternative control group: pedestrian stops increase by 0.620 per block-quarter ($p < 0.001$). The consistency of the pedestrian stop effect across two independent control groups strengthens confidence that the finding reflects a behavioral response to the donation rather than an artifact of control group selection.

The negative vehicular stop coefficient in Panel B does not indicate a decline in vehicular stops at 7-Eleven blocks; it reflects a contemporaneous increase in vehicular stops at the other-convenience-store control blocks over the same period, visible as the positive 0.211 ($p < 0.001$) placebo coefficient on vehicular stops in Table 5. We do not highlight this Panel B vehicular result because it is not robust to the choice of control group and, more importantly, because Table 5 shows the change seems to be occurring at the control units rather than at 7-Eleven blocks. In other words, the negative Panel B coefficient might be an artifact of the control group selection rather than a treatment effect at 7-Elevens. This is consistent with our preregistration, which hypothesized that the donation would affect stops tied to retail crime; a vehicular stop is unlikely to fall into that category.

The fixed-effects Poisson specification (Table 2), which estimates multiplicative rather than additive effects and also controls for lagged crime, confirms these patterns. In Panel A, the Poisson model yields an incidence rate ratio (IRR) of 1.47 for pedestrian stops ($p < 0.001$), indicating a 47% increase relative to control blocks. Panel B produces an even larger IRR of 1.92 ($p < 0.001$) when using other convenience stores as the comparison. Vehicular-stop estimates do not reproduce the pedestrian-stop pattern: they are statistically insignificant in Panel A and do not show a consistent or interpretable pattern in Panel B. We therefore do not interpret the vehicular estimates as evidence of a donor-induced shift in traffic enforcement.

The increase in investigatory stops is not evenly distributed across racial groups. In the TWFE specification (Table 1, Panel A), the increase was largest in absolute terms for Black individuals (0.411 additional stops per block-quarter, an 83% increase over the pre-donation mean of 0.50) compared to White (0.099, a 53% increase over baseline of 0.19) and Hispanic (0.089, a 47% increase over baseline of 0.19) individuals. Because the additive model may mechanically produce

larger coefficients for groups with higher baseline stop rates, we also examine the fixed-effects Poisson estimates (Table 2) to assess whether the proportional increase differs across groups. The Poisson IRRs confirm that the proportional increase is larger for Black pedestrians (IRR = 1.56, a 56% increase) than for White (IRR = 1.35, 35% increase) or Hispanic (IRR = 1.34, 34% increase) pedestrians, even after controlling for local crime dynamics. This proportional disparity—a 21 percentage-point gap between Black and White pedestrians—indicates that the spatial reallocation of enforcement toward 7-Eleven locations disproportionately affected Black pedestrians, after accounting for differences in both baseline stop rates and crime. Panel B reinforces this pattern with even larger effects: the Poisson IRR for Black pedestrian stops is 2.04, compared to 1.73 for White and 1.60 for Hispanic pedestrians.

Several features of the data help interpret this finding. First, 7-Eleven blocks do not have a higher share of Black residents than control blocks (6.5% vs. 7.0%, Table 8), ruling out a simple mechanical explanation based on neighborhood demographics. Second, the null effect on vehicular stops—which are less discretionary and less spatially tied to retail locations—suggests that the increase reflects a targeted change in pedestrian enforcement near donor stores, not a generalized increase in all policing.

We note, however, that we cannot definitively distinguish whether officers applied enforcement in a race-neutral manner to a population of pedestrians that was disproportionately Black, or whether enforcement decisions at these locations were themselves race-differentiated. This distinction is central to a long-standing debate in the policing literature. The salience of race in Chicago police enforcement is not merely an analytical preference but an institutional fact. In 2017, the U.S. Department of Justice concluded that the Chicago Police Department engaged in a ‘pattern or practice’ of constitutional violations, including racially disparate use of force and stops (1). This investigation led to a federal consent decree that explicitly requires CPD to monitor and address racial disparities in stops, searches, and use of force. Despite this mandate, recent independent analyses have found that racial disparities in CPD stops have continued to increase since the consent decree took effect (2). Whatever one’s theoretical priors about the source of these disparities, the federal consent decree establishes that race-disaggregated analysis of CPD enforcement is methodologically appropriate and institutionally required.

One body of scholarship argues that racial disparities in police stops reflect systemic patterns of racialized enforcement, including the disproportionate surveillance of Black communities through practices like stop-and-frisk (3, 9), and that administrative data on police-civilian encounters systematically undercount racial bias because they condition on a biased sample of interactions (11). An alternative perspective holds that observed racial disparities in enforcement largely reflect the spatial concentration of crime in neighborhoods that are disproportionately Black, such that officers responding to crime patterns rather than racial animus would still produce racially disparate outcomes (6). Our study does not adjudicate between these views. What we can establish is that the donation shifted the spatial allocation of enforcement, and that the resulting pattern of stops fell disproportionately on Black pedestrians. Whether this reflects preexisting racial patterns in discretionary enforcement being amplified by the spatial reallocation, or a race-neutral increase applied to locations with racially skewed pedestrian populations, the consequence for Black pedestrians is the same, and the underlying distortion of public enforcement by private donations is unchanged.

Table 1: TWFE Estimates with Lagged Crime Control

	All Races	Black	White	Hispanic
Panel A. 7-Elevens vs. Major Intersections				
All Investigatory Stops				
Treat $\times$ Post	0.612*** (0.128)	0.411*** (0.099)	0.099*** (0.026)	0.089** (0.028)
Pedestrian Stops				
Treat $\times$ Post	0.551*** (0.117)	0.379*** (0.092)	0.095*** (0.025)	0.066** (0.020)
Vehicular Stops				
Treat $\times$ Post	0.061* (0.027)	0.032* (0.015)	0.004 (0.007)	0.023 (0.016)
Panel B. 7-Elevens vs. Other Stores				
All Investigatory Stops				
Treat $\times$ Post	0.358* (0.148)	0.220 (0.118)	0.127*** (0.033)	-0.003 (0.033)
Pedestrian Stops				
Treat $\times$ Post	0.620*** (0.136)	0.431*** (0.109)	0.123*** (0.032)	0.055* (0.027)
Vehicular Stops				
Treat $\times$ Post	-0.262*** (0.034)	-0.211*** (0.026)	0.004 (0.006)	-0.058*** (0.015)

Notes: All models include lagged block-level crime as a control, block and time fixed effects, and standard errors clustered at the block level. The time unit is the three-month quarter, leaving 16 quarterly periods. Panel A compares 7-Eleven blocks to blocks with major intersections (17,072 observations, 1,067 blocks, 626 treatment, 441 control). Panel B compares 7-Eleven blocks to blocks with other convenience stores (34,656 observations, 2,166 blocks). Race-specific models are estimated separately; “All Races” includes stops of individuals classified as other or unknown race. Negative vehicular coefficients in Panel B reflect an increase in vehicular stops at other-convenience-store control blocks (see Table 5), not a decrease at 7-Eleven blocks. We therefore do not interpret the Panel B vehicular estimates as a treatment effect.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Fixed-Effects Poisson Estimates with Lagged Crime Control

	All Races	Black	White	Hispanic
Panel A. 7-Elevens vs. Major Intersections				
All Investigatory Stops				
Treat × Post	0.323*** (0.094)	0.389** (0.133)	0.257* (0.112)	0.216 (0.113)
IRR	1.38	1.48	1.29	1.24
Pedestrian Stops				
Treat × Post	0.387*** (0.107)	0.447** (0.146)	0.299* (0.130)	0.291* (0.132)
IRR	1.47	1.56	1.35	1.34
Vehicular Stops				
Treat × Post	0.087 (0.126)	0.089 (0.187)	0.025 (0.195)	0.088 (0.188)
IRR	1.09	1.09	1.03	1.09
Panel B. 7-Elevens vs. Other Stores				
All Investigatory Stops				
Treat × Post	0.500*** (0.067)	0.584*** (0.090)	0.471** (0.155)	0.280** (0.087)
IRR	1.65	1.79	1.60	1.32
Pedestrian Stops				
Treat × Post	0.651*** (0.078)	0.714*** (0.098)	0.547** (0.189)	0.471*** (0.113)
IRR	1.92	2.04	1.73	1.60
Vehicular Stops				
Treat × Post	0.088 (0.078)	0.252* (0.114)	0.111 (0.138)	−0.032 (0.116)
IRR	1.09	1.29	1.12	0.97

Notes: All models are fixed-effects Poisson specifications with lagged block-level crime as a control. The time unit is the three-month quarter, leaving 16 quarterly periods. Panel A compares 7-Eleven blocks to blocks with major intersections; the full Panel A sample contains 1,067 blocks and 17,072 block-quarter observations, with the conditional Poisson retaining between 8,928 and 14,288 observations depending on the outcome. Panel B compares 7-Eleven blocks to blocks with other convenience stores (34,656 observations before zero-dropping). All models include block and quarter fixed effects; standard errors are clustered at the block level. Blocks with zero stops across all periods are dropped from the conditional Poisson likelihood. IRR = $\exp(\text{coefficient})$. Race-specific models are estimated separately.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To assess whether the increased enforcement produced public safety benefits, we estimate our main specification using block-level crime counts as the outcome. We find no statistically significant change in crime at 7-Eleven blocks following the donation (Table 3). Estimates are small and become further attenuated when controlling for lagged crime. This null result serves two purposes. First, it provides additional support for the parallel trends assumption: crime did not change differentially at treated blocks, ruling out the possibility that increased stops were a response to rising crime. Second, it indicates that the reallocation of enforcement toward donor stores did not generate detectable public safety gains.

We next estimate the same specification using police enforcement actions and charges as the outcome (Table 3). The Chicago Police Department defines enforcement actions as either arrests, administrative notice of ordinance violations, or citations. The dataset also has an "other" category which includes actions like verbal warnings. While these stops had no detectable impact on crime, enforcement actions increased significantly at 7-Eleven blocks following the donation. Separately, arrests also saw a significant increase. In order to determine whether this increase in enforcement resulted in serious offenders being arrested, we also looked at the charges they faced. Neither felonies nor misdemeanor charges saw a significant increase. Warrant arrests, which were typically for defendants who violate the terms of their pre-trial release, saw an increase only significant at the 0.1 level. We also combined all three types of charges (Felonies, Misdemeanors, and Warrants) that might be considered more serious into a single category and still found no effect. The only category of charge that saw a significant increase was ordinance violations and citations. These are low level charges that include traffic tickets, citations for having small amounts of marijuana (prior to legalization in Illinois but decriminalized in Chicago), or consuming alcohol on a public street or sidewalk. Additionally, the increase in stops did not result in an increase in contraband found, such as illegal drugs or weapons. Examining the descriptive shares directly underscores this point: among treated blocks, the share of stops resulting in contraband found decreased from 1.8% in the pre-donation period to 1.4% post-donation, while in control blocks the same share rose from 1.4% to 2.5% over the equivalent period.⁴ The additional stops induced by the donation were thus disproportionately unproductive on the dimension of contraband recovery. There was a significant increase in the number of stops resulting in no enforcement action taken. Taken together,

⁴Percentages computed over block-quarters with at least one stop.

these results indicate that the observed rise in enforcement reflects a shift in police activity toward donor-affiliated locations rather than a response to underlying changes in crime, and increases in enforcement activity are largely driven by the policing of low-level crime and infractions.

Table 3: Effects of Donation on Crime and Enforcement Outcomes

	treatpost
Crimes	0.125 (0.125)
All Enforcement Actions	0.112*** (0.031)
Arrests	0.036* (0.014)
Felonies Charged	0.005 (0.008)
Misdemeanors Charge	0.012 (0.012)
Warrant Arrests	0.007+ (0.004)
Felonies, Misdemeanors, and Warrants	0.023 (0.016)
Ordinance Violations and Citations	0.061** (0.019)
Contraband Found	0.001 (0.009)
Stops with No Enforcement Action	0.520*** (0.114)

Notes: All models are estimated using TWFE. The sample includes 17,072 observations across 1,067 blocks (626 treatment, 441 control blocks) and 16 quarterly time periods (2016Q2–2020Q1). Standard errors are clustered at the block level.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 1 presents the event study estimate for stops with no enforcement actions for all racial groups comparing blocks with a 7-Eleven to major-intersection blocks across 16 three-month periods before

and after the November 2017 donation.

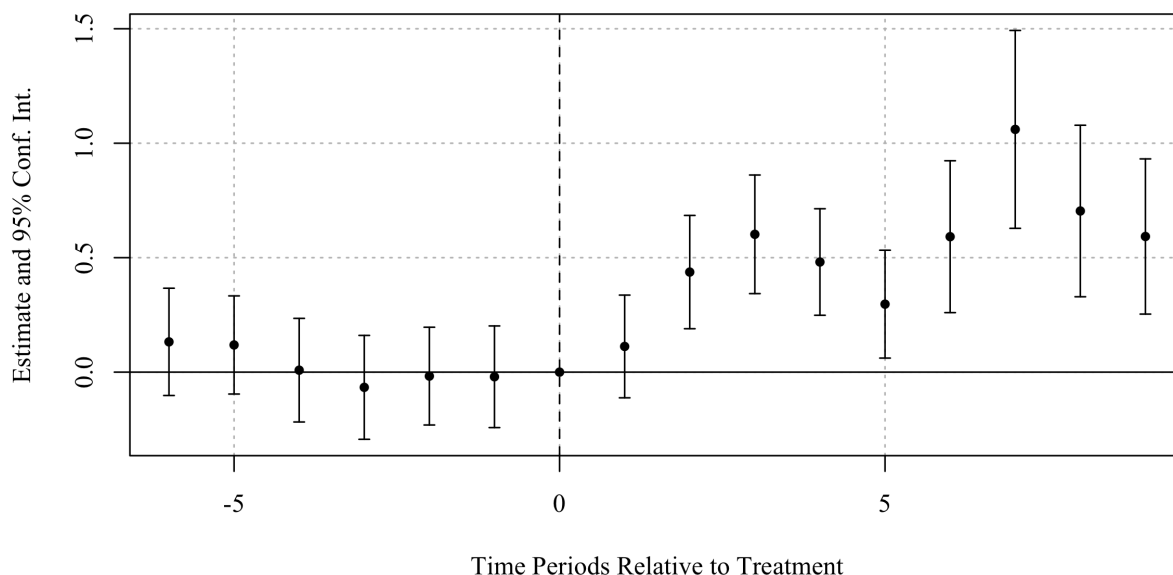


Figure 1: Event Study Analysis Stops with No Enforcement: 7-Elevens vs. Major Intersections

Notes: We included 1,067 blocks, 626 blocks with a 7-Eleven, and 441 control blocks. Aggregated in three-month time periods, leaving 16 quarterly periods (2016Q2-2020Q1), leaving all models with 17,072 observations. All models include block and time-fixed effects. Clustered standard errors at the block level. Confidence intervals at 95% level.

Figure 2 presents event study estimates comparing blocks with a 7-Eleven to major-intersection blocks across 16 three-month periods before and after the November 2017 donation. Consistent with the difference-in-difference estimates presented in Table 1, the results show a clear post-treatment increase in all investigatory stops—particularly pedestrian stops—at blocks with 7-Eleven. The first column reveals that, across all racial groups, the donation effect begins to rise immediately after November 2017 and becomes statistically significant by the second post-donation quarter. These increases persist and grow in magnitude over time, suggesting a sustained behavioral response by police rather than a temporary spike. The second column shows that the increase in stops is most pronounced for Black individuals, who experienced a steep and steady rise in all investigatory stops and pedestrian stops after the donation. In contrast, the increases in Whites and Hispanics are smaller and less precise, with many post-donation coefficients overlapping zero and larger confidence intervals.

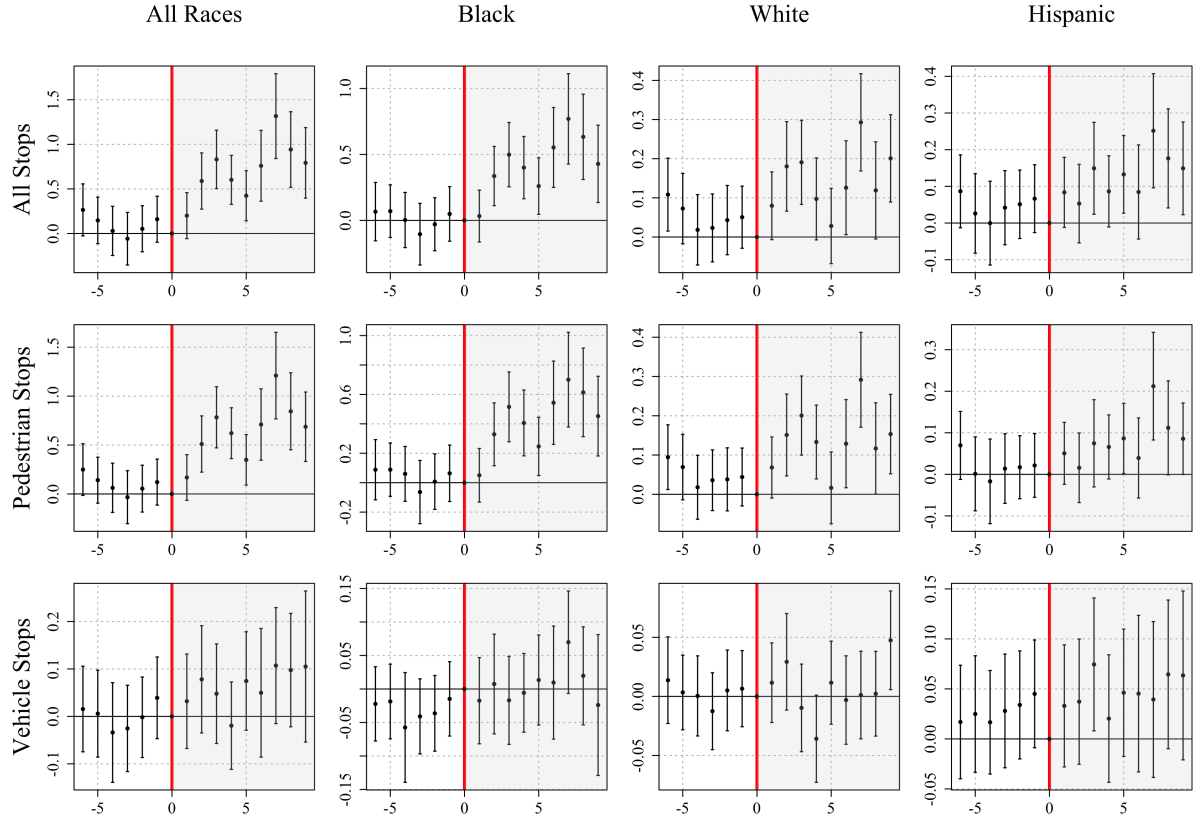


Figure 2: Event Study Analysis: 7-Elevens vs. Major Intersections

Notes: We included 1,067 blocks, 626 blocks with a 7-Eleven, and 441 control blocks. Aggregated in three-month time periods, leaving 16 quarterly periods (2016Q2-2020Q1), leaving all models with 17,072 observations. All models include block and time-fixed effects. Clustered standard errors at the block level. The y-axis is the coefficient estimate with confidence intervals at 95% level and the x-axis is time periods relative to treatment.

Figure 3 presents the event-study for the fixed-effects Poisson models.

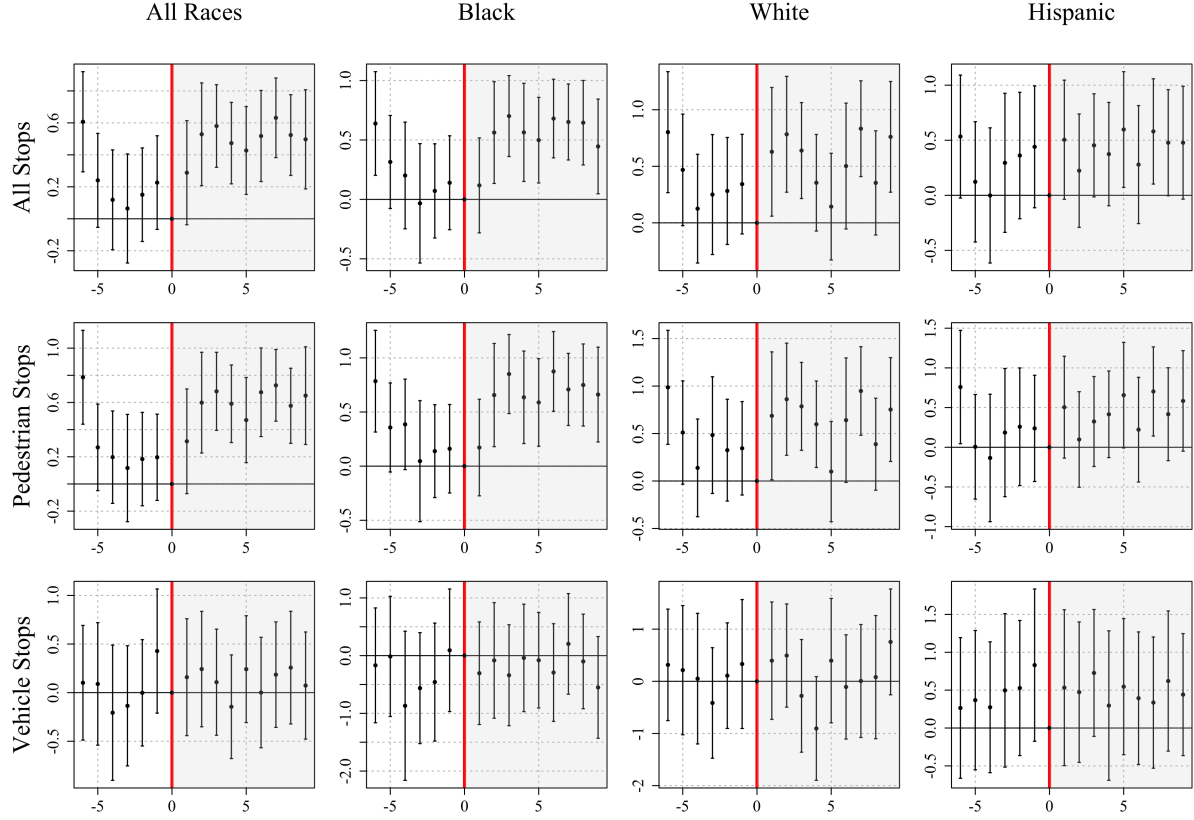


Figure 3: Event Study Poisson Analysis: 7-Elevens vs. Major Intersections

Notes: We included 1,067 blocks, 626 blocks with a 7-Eleven, and 441 control blocks. Aggregated in three-month time periods, leaving 16 quarterly periods (2016Q2-2020Q1), leaving all models with 17,072 observations. All models include block and time-fixed effects. Clustered standard errors at the block level. The y-axis is the coefficient estimate with confidence intervals at 95% level and the x-axis is time periods relative to treatment.

Across all panels, vehicle stops exhibit no meaningful change over time for any group, reinforcing that street-level pedestrian encounters drive the observed effects.

Table 4 reports a joint F-test of the pre-treatment event-study coefficients, providing empirical support for the plausibility of the parallel trends assumption. In the standard four-quarter window ($t = -4$ to $t = -1$), we find no statistically significant evidence of differential pre-donation trends for any of our main outcomes: $p = 0.555$ for all investigatory stops, $p = 0.668$ for pedestrian stops, and $p = 0.361$ for Black pedestrian stops. In the extended six-quarter window, p-values for all stops and pedestrian stops are marginally significant ($p = 0.051$ and $p = 0.046$), driven by the two earliest periods, which carry the most estimation uncertainty; for the Black pedestrian stops, the F-test result remains statistically non-significant ($p = 0.248$).

Table 4: Joint F-Test of Pre-Treatment Trends

Outcome	Window: $t = -4$ to $t = -1$		Window: $t = -6$ to $t = -1$	
	$F(4, 1066)$	p -value	$F(6, 1066)$	p -value
All Investigatory Stops	0.755	0.555	2.095	0.051
Pedestrian Stops	0.593	0.668	2.147	0.046
Black Pedestrian Stops	1.088	0.361	1.314	0.248

Notes: Each cell reports the Wald F -statistic and p -value from a joint test that all pre-treatment event-study coefficients are simultaneously equal to zero, using cluster-robust standard errors clustered at the block level. Panel A sample: 1,067 blocks (626 treatment, 441 control), 17,072 observations, 16 quarterly periods (2016Q2–2020Q1). The left panel uses the four quarters immediately preceding the donation ($t = -4, -3, -2, -1$), the standard pre-trend window. The right panel extends to all six available pre-treatment quarters ($t = -6, \dots, -1$); marginal results in the extended window for All Stops and Pedestrian Stops are driven by the two earliest periods, which carry the most estimation uncertainty.

Table 5 presents a placebo test comparing other convenience store blocks to major intersection blocks, replacing the 7-Eleven treated group with blocks containing other convenience stores while keeping the same control group and estimation period. If our main findings reflected a general pattern of increased enforcement near all convenience stores rather than a response specific to the 7-Eleven donation, we would expect similar positive coefficients here. Instead, we find no significant increase in pedestrian stops for any racial group, and vehicular stops show a significant increase, the opposite pattern from our main results. This confirms that the pedestrian stop effect identified in Tables 1 and 2 is specific to 7-Eleven blocks and is not a generic feature of convenience store locations.

Table 5: TWFE Estimates: Other Convenience Stores vs. Major Intersections

	All Races	Black	White	Hispanic
All Investigatory Stops				
Placebo Treatment	0.167 ⁺	0.090	−0.008	0.081 [*]
	(0.102)	(0.083)	(0.025)	(0.026)
Pedestrian Stops				
Placebo Treatment	−0.044	−0.053	−0.009	0.016
	(0.093)	(0.075)	(0.025)	(0.022)
Vehicular Stops				
Placebo Treatment	0.211 ^{***}	0.143 ^{**}	0.001	0.065 ^{***}
	(0.028)	(0.024)	(0.003)	(0.011)

Notes: We included 19,785 blocks (18,243 other convenience stores and 1,542 major intersections). Aggregated in three-month time periods, leaving 16 quarterly periods (2016Q2-2020Q1), leaving all models with 316,560 observations. All models include lagged block-level crime as a control and block and time-fixed effects. Clustered standard errors at the block level.

⁺ $p < 0.1$, ^{*} $p < 0.05$, ^{**} $p < 0.01$, ^{***} $p < 0.001$

A nonparametric permutation test provides strong evidence that our results are not a statistical artifact. We randomly reassigned the donation indicator, scrambling which blocks were labeled as treated and which as control, 100 times while holding all other aspects of the data and model constant, and re-estimated our main TWFE specification for each permutation. The permutation reassigns treatment among the 1,067 blocks in our analytic sample (626 treated and 441 control), preserving the total number of treated and control units in each draw, so that the null distribution reflects the same sample composition and fixed-effect structure as the main analysis. Figure 4 shows the resulting null distribution of placebo treatment effects by racial group; the vertical red line represents the actual treatment effect from our main specification. Across all 100 permutations, the actual observed effect exceeded every one of the placebo estimates for all investigatory stops, pedestrian stops, and Black pedestrian stops, yielding a permutation p-value below 0.01 for each outcome. Because this test makes no distributional assumptions, it provides nonparametric confirmation of our main result that is robust to concerns about standard error misspecification or overdispersion.

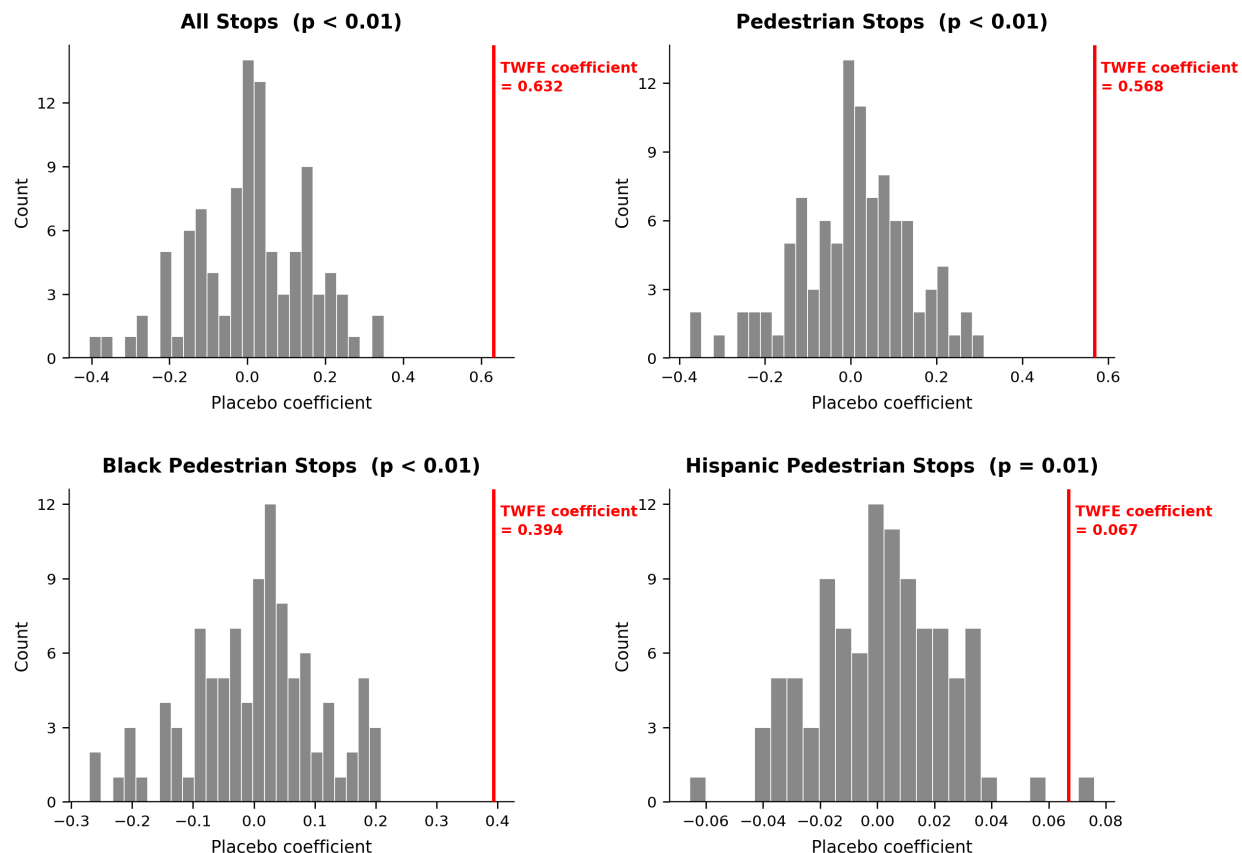


Figure 4: Placebo Distribution of Estimated Treatment Effects from 100 Random Assignments

Notes: 100 permutations randomly reassigning treatment using our main specification (Equation 1). The red line shows the actual effect reported in our TWFE analysis. Clustered SEs at block level.

Additionally, we estimated our main specification and Poisson model clustering at the police beat level to account for spatial dependence across blocks within the same patrol area. Table 6 and Table 7 show the results of Panel A. In our block dataset there are 95 unique police beats. Beat-clustered standard errors are approximately 25-35% larger than block-clustered errors, which is expected given the fewer clusters (95 beats vs. 1067 blocks). Table 6 shows that all key results in our main specification remain statistically significant. Table 7 shows that White Pedestrian Stops loses significance with beat clustering ($p=0.06$), but all other key outcomes remain significant.

Table 6: Coefficient Estimates TWFE with Block- and Beat-Level Standard Errors

Outcome	Coef	Block SE	Beat SE	Block sig	Beat sig
All Stops	0.612	0.128	0.169	***	***
Ped Stops	0.551	0.117	0.157	***	***
Black Ped	0.379	0.092	0.120	***	**
White Ped	0.095	0.025	0.031	***	**
Hispanic Ped	0.066	0.020	0.023	**	**

Notes: Coef = coefficient on Treat $\times$ Post with lagged crime control. SE = standard error. Includes 1,067 blocks and 17,072 observations. Beat-level clustering uses 95 unique police beats. Beat-clustered SEs are approximately 25–35% larger than block-clustered SEs, as expected given the reduction from 1,067 to 95 clusters.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Coefficient Estimates Poisson with Block- and Beat-Level Standard Errors and IRRs

Outcome	Coef	Block SE	Beat SE	IRR	Block sig	Beat sig
All Stops	0.323	0.094	0.116	1.38	***	**
Ped Stops	0.387	0.107	0.128	1.47	***	**
Black Ped	0.447	0.146	0.157	1.56	**	**
White Ped	0.299	0.130	0.157	1.35	*	(ns)
Hispanic Ped	0.291	0.132	0.140	1.34	*	*

Notes: Coef = coefficient on Treat $\times$ Post with lagged crime control. IRR = exp(coefficient). Includes 1,067 blocks and 17,072 observations. Beat-level clustering uses 95 unique police beats. White Pedestrian Stops loses significance with beat clustering ($p \approx 0.06$); all other key outcomes remain significant.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Discussion

Our findings provide the first empirical evidence that even modest private donations to police departments can alter the spatial allocation of discretionary enforcement. A single \$10,000 sponsorship to a police foundation gala was associated with a 47% increase in pedestrian investigatory stops (Poisson IRR = 1.47) at the donor’s retail locations, with no robust corresponding change in vehicular stops and no detectable change in crime. The proportional increase was larger for Black

pedestrians (56%) than for White (35%) or Hispanic (34%) pedestrians, indicating that the reallocation of enforcement disproportionately affected Black individuals, whether through race-neutral policing of a disproportionately Black pedestrian population at these locations, or through race-differentiated enforcement decisions. Regardless of one's normative assessment of investigatory stops, this finding reveals that private donations can distort the distribution of a core public service, directing enforcement resources toward the properties of benefactors rather than allocating them based on public safety considerations alone.

The stopping patterns we observed are consistent with reciprocity behavior triggered by the symbolic weight of the donation rather than a transactional motivation. Due to the donation's amount, the donation's impact is unlikely to stem from its monetary value. Instead, the TBG sponsorship served as a public signal identifying 7-Eleven as a pro-police corporation at a moment of intense public scrutiny of the Chicago Police Department. The 2017 donation occurred in the wake of the Laquan McDonald shooting, the subsequent Department of Justice investigation, and the August 2017 ACLU consent decree, a period in which officers publicly described feeling under siege from media criticism and political opposition. In this context, a corporate sponsorship of the TBG carried outsized symbolic weight, signaling solidarity with police at a time when such solidarity was scarce and politically costly. Officers attending the event described being "filled with gratitude" and feeling that "someone's still in our corner, still backing the blue".⁵ The donation thus functioned not merely as a financial contribution but as an identity-affirming signal that, amplified through the FoP magazine, TBG photographs, and peer-to-peer sharing, reached officers who had never attended the event. In our study, the donor is visible and physically present across officers' daily patrol environments through 116 stores, creating the conditions for spatially observable reciprocity patterns. The retail geographic distribution across the city provided a unique setting to test whether a donation to the police could translate into observable enforcement behavioral patterns.

While previous literature has focused primarily on large-scale corruption or overt quid pro quo exchanges (10, 13), our study demonstrates how behavioral shifts can emerge from legal, public forms of giving. This underscores a key insight from the psychology and economics of reciprocity: the size of the gift is not necessarily proportional to its influence. If officers respond measurably to

⁵Fraternal Order of Police Chicago Lodge 7 Magazine. November, 2019. Page 40. <https://online.flipbuilder.com/prsn/wkgl/mobile/index.html#p=40>

a \$10,000 donation, it is reasonable to hypothesize that the effects of far larger donations—many of which reach into the hundreds of thousands or even millions of dollars (18)—may be even more significant and potentially more difficult to detect using existing public data.

Nonetheless, several limitations of our study merit attention. First, our findings are specific to the case of 7-Eleven’s one-time donation to the Chicago Police Foundation in 2017. We do not claim that these results generalize to all corporate donors, all police departments, or all cities. Generalizing from this case to other donors or cities therefore cannot be assumed without comparable granular data, a theoretically specified mechanism, and quasi-experimental identification.

An important question is whether the effects we document are unique to 7-Eleven or generalizable to other sponsors of the True Blue Gala. We therefore systematically evaluate all corporate sponsors of the True Blue Gala between 2016 and 2024 (see Table S2 for the full list of sponsors in the Appendix) to assess whether our block-level design can be applied more broadly. Among all sponsors, 7-Eleven is the only firm that satisfies the four conditions required for our quasi-experimental design: (1) a sufficient number of locations to generate statistical power (116 stores yielding 626 treated blocks), (2) city-wide geographic distribution rather than spatial concentration, (3) a one-time donation enabling a clean pre/post comparison, and (4) a walk-up retail format relevant to pedestrian enforcement. Most other sponsors either have a single Chicago location (e.g., Eli’s Cheesecake, Hyatt Regency), are spatially concentrated (e.g., LAZ Parking, GCM Grosvenor), or donate in multiple consecutive years, precluding identification of a discrete treatment (e.g., BMO Harris Bank). Table S3 in the Appendix reports this assessment for all sponsors with a physical presence in Chicago. Crucially, 7-Eleven was identified *ex ante* in our pre-registration based on these design criteria. The analysis therefore evaluates a single pre-specified hypothesis on the only donor that permits the design.

Second, while we detect a sustained behavioral response following the donation, we cannot observe officers’ internal motivations or the exact mechanisms through which donor bias manifests, whether through departmental norms, direct instructions, or implicit psychological reciprocity. These remain important areas for future research, especially using more granular data on officer assignments and attitudes.

More broadly, the theoretical expectations for donor influence likely differ substantially across

firms and industries. A convenience store chain like 7-Eleven, whose stores face street-level crime and benefit directly from visible police presence, has clear incentives for enforcement-related reciprocity. But other corporate donors may seek entirely different forms of influence. A beverage or food company might donate to cultivate political goodwill that helps resist regulatory action, such as proposed sugar taxes or zoning restrictions. A technology firm might sponsor police events to position itself favorably for procurement contracts. A trucking or logistics company might seek lenient treatment on traffic violations or commercial parking enforcement. These examples illustrate that donor bias is unlikely to be a uniform phenomenon. The form reciprocity takes will depend on what the donor needs from the police and what the police have the discretion to provide. Our study captures one specific mechanism—increased pedestrian enforcement near retail locations—while the broader landscape of donor influence almost certainly encompasses subtler and less empirically tractable forms of reciprocity.

Furthermore, we find no evidence that the increased enforcement at 7-Eleven locations reduced crime, suggesting that the reallocation of policing resources did not produce public safety benefits for the surrounding community. This finding is consistent with research showing that discretionary investigatory stops, particularly those focused on low-level offenses, often do not generate meaningful crime reductions (16).

Beyond policing, this study contributes to a growing literature on the consequences of private financing of public institutions. Research on school-supporting nonprofits has documented how private donations to public schools can reproduce and amplify resource inequalities, redirecting public services toward donor-preferred priorities (13). Similar dynamics have been documented for libraries and other publicly funded civic institutions. Our findings extend this literature to law enforcement, where the consequences of private influence on resource allocation are particularly weighty for civil liberties and public safety. The case of policing thus represents a theoretically and socially significant test of how private philanthropy can alter the conduct of public agencies, and the methodological approach we employ of combining granular spatial data with quasi-experimental identification may prove useful for studying private influence in other public-service contexts.

Finally, this study opens a new empirical question about the consequences of philanthropic support for public agencies. Our finding that a single modest donation can generate a detectable and

sustained shift in enforcement patterns suggests that donor bias may arise wherever discretionary authority intersects with private funding. However, establishing whether it does requires the same kind of granular, spatially identified data that made this study possible. We do not claim that our results extend to other agencies, cities, or donors; what we demonstrate is that the phenomenon is empirically real, measurable, and consequential in the specific case we examine. Future research should investigate whether similar dynamics arise in other settings, what conditions amplify or suppress them, and what theoretical frameworks best account for variation across donor types, public agencies, and institutional contexts.

Data and Methods

To examine the effects of private donations on policing behavior, we leveraged the sponsorship of 7-Eleven to the Chicago Police Foundation’s (CPF) True Blue Gala in 2017. Using block-level data from Chicago between January 2016 and February 2020, we evaluate the effect of 7-Eleven donations on Chicago Police activity. To this end, we gathered data from five main sources.

Donations

Collecting donation data is complex since most police departments do not publicly disclose their donors and donation amounts. To overcome this challenge, we focus on CPF’s public information about True Blue Gala (TBG) sponsorship. We collected TBG sponsorship data from 2016 to 2024 using archived versions of the CPF website through the Internet Archive’s Wayback Machine and photographic materials posted by the CPF. TBG was canceled in 2020 due to COVID-19. Therefore, no sponsorship happened that year. Additionally data for 2022 was not available.

To isolate the effect of donations from other confounding factors, we focused on retail companies sponsoring TBG to leverage their physical presence across the city. Table [S2](#) in the Methodological Appendix shows all the companies that have a physical presence in Chicago and that have sponsored the TBG between 2016 and 2024. We selected the 7-Eleven 2017 donation for two reasons. First, it only sponsored the TBG one time during our period of study, allowing us to estimate the effect of one unique donation. Second, 7-Eleven has many stores across the city ($N = 116$), which allows us to have enough statistical power to capture an effect.

Police Stop Data

Measuring police activity is often challenging since police departments do not make this information publicly available. To address this, we use police stop data as a proxy of police activity. We use data on investigatory stops released by the Chicago Police Department between 2016 and 2023. Investigatory stops involve temporary detentions and questioning of individuals based on “reasonable articulable suspicion” of criminal activity.

While direct data on patrol routes or deployment of officers are not publicly available, the number of stops in a given location offers a granular indicator of policing intensity when we account for criminal activity differences.

Crime Data

We leverage the geographic distribution of 7-Eleven stores across Chicago to construct a quasi-experimental setting. Since store locations are unlikely to be randomly distributed, we use block crime data published at the Chicago Data Portal to find a comparable group of blocks to isolate the effect of 7-Eleven donation on police investigatory stops.

Socio-Economic Controls

We leverage the geographic distribution of 7-Eleven stores across Chicago to construct a quasi-experimental setting. Since store locations are unlikely to be randomly distributed, we use data from the American Community Survey (ACS) five-year estimates (2016–2020) to identify a set of demographically and socioeconomically comparable blocks. Table 8 shows the differences between 7-Eleven blocks ($N = 626$) and those with major intersections ($N = 441$), suggesting that our two groups of blocks overall have no statistically significant mean differences.

Table 8: Balance Table Based on ACS 2016–2020 Characteristics

Socio-Economic Controls	Control ($N = 441$)		Treatment ($N = 626$)		Diff. in Means
	Mean	Std. Dev.	Mean	Std. Dev.	
Median Income	100,794.4	39,014.9	101,347.4	37,996.9	553.0
Share Black	0.070	0.107	0.065	0.073	−0.005
Share White	0.599	0.210	0.573	0.193	−0.026
Share Hispanic	0.200	0.207	0.214	0.219	0.014
Share Males Ages 15–25	0.065	0.058	0.064	0.052	−0.001
Share College Graduates	0.608	0.259	0.629	0.268	0.021
Share Below Poverty Line	0.104	0.078	0.103	0.067	−0.000

Notes: Data from American Community Survey five-year estimates, 2016–2020.

7-Eleven Store Location

We obtained the location of 7-Eleven in Chicago through Data Axle, a commercial directory providing business listings by year. We filtered by city and used different names to identify a 7-Eleven store (e.g., “7-Eleven”, “Seven Eleven”, “Seven-Eleven”). We obtained a comprehensive list of store locations from 2010 onwards. In cases where only approximate location was available (e.g., ZIP code centroids), we successfully re-coded all entries using full address information to obtain precise coordinates.⁶

Figure 5 shows the distribution of 7-Eleven stores and major intersections across Chicago during our study period. Besides the geographic variation that 7-Eleven offers, we selected this one-time donation to separate potential effects related to long-term relationships between private donors and police departments from the donation effect.

⁶When a store appeared in two consecutive years but is missing in between, we assumed the location remained open.

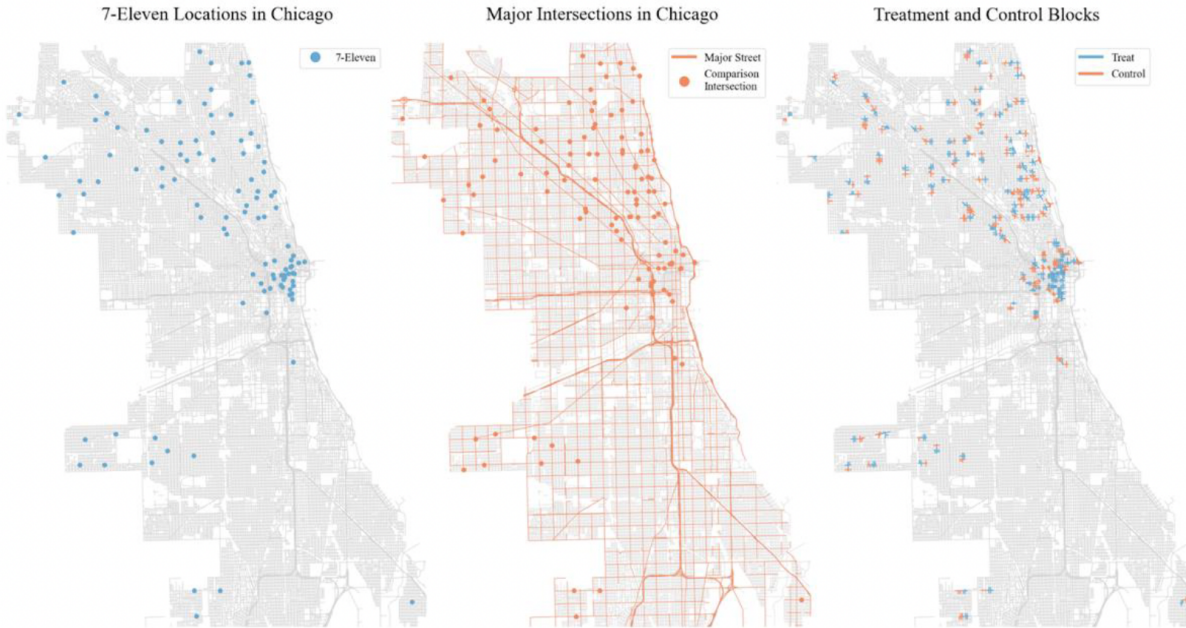


Figure 5: Distribution of 7-Eleven and Major Intersections in Chicago, 2016–2020

Empirical Strategy

Our analysis aims to estimate the effect of private donations to police departments on policing behavior. We measure private donations as 7-Eleven sponsorship of the 2017 Chicago Police TBG and police behavior as police investigatory stops in Chicago between January 2016 and February 2020. We preregistered our study after data collection and before estimating our models.⁷

We employed a two-way fixed effects model comparing blocks within 330 feet of a 7-Eleven store (treatment group) to blocks within 330 feet of a major intersection (control group). 330 feet was chosen as the radius because this is the length of a standard Chicago block. The 330-foot radius is

⁷Our pre-registration is available at OSF: <https://osf.io/ye9uk/>. The pre-registered design specified blocks within 330 feet of a 7-Eleven as the treatment group, blocks beyond 330 feet as the control group, and a difference-in-differences or two-way fixed effects estimator for pedestrian stops by race. Consistent with the pre-registration, we use blocks within 330 feet of a 7-Eleven as our treatment group. We selected blocks with major intersections as the primary comparison group from among the pre-registered control universe because they exhibit parallel pre-trends in crime (Figure S2 in the Appendix). Vehicular stops are included as a theoretically motivated null check: the pre-registration hypothesized that the donation would affect pedestrian stops, predicting no effect on vehicular stops. The pre-registration described the data as monthly panel data; we present results aggregated quarterly in the main text and report the monthly TWFE and Poisson estimates in Table S7 and Table S8 of the Methodological Appendix, where the results are fully consistent. The pre-registration specified a time frame of January 2016 to December 2018; we extended the observation window to February 2020 to capture longer-run post-donation dynamics, consistent with the pre-registered hypothesis (H3) that effects would persist "for a significant period of time."

therefore a structural feature of the design rather than a free tuning parameter: a materially smaller radius would exclude the adjacent blocks where corner-store enforcement plausibly occurs, while a materially larger one would merge non-adjacent blocks and undermine the block as the unit of analysis at which crime, demographic, and stop data are measured. Using this radius guaranteed that all adjacent blocks to a 7-Eleven would be considered treated. The control group of "Major Intersections" was created using the closest intersection of two major street (as defined by the dataset on Chicago's Open Data Portal) to each 7-Eleven with a buffer zone of 990 feet (3 blocks) to account for any spillover effects. Concretely, we estimated the following specification as our main model:

$$PoliceStops_{it}^s = \beta_0 + \beta_1 Donation_{it} + \alpha_i + \delta_t + \varepsilon_{it} \quad (1)$$

where s represents the three measures of police investigatory stops by race in a block i in period t , where t has a periodicity of three months. We estimate equation (1) using three outcome variables: (i) all investigatory police stops, (ii) pedestrian investigatory police stops, and (iii) vehicular investigatory police stops. In our preregistration, we hypothesized that the donation effect should only be observable in pedestrian stops, which would drive an effect in all investigatory stops. While a donation could increase police activity around the retail store in general, we hypothesized that police officers would only change their behavior to increase retailer protection, and a vehicular stop is unlikely to be related to retail crime.

Our donation variable is represented by a dummy variable that takes the value of one when a block has a 7-Eleven and the observation is after November 2017, when the sponsorship to the TBG happened, and zero otherwise ($Donation_{it} = \mathbf{1}[\text{block } i \text{ has a 7-Eleven}] \cdot \mathbf{1}[t \geq \text{Nov 2017}]$).⁸ We included block (α_i) and time (δ_t) fixed effects to account for time-invariant block characteristics and seasonal temporal changes. Finally, we included clustered standard errors at the block level (ε_{it}). We included in our appendix other versions of our main specification controlling for crime, showing our results are robust to changes in our model.

We estimate an event study specification with block and time fixed effects to examine the dynamic relationship between 7-Eleven donation and police stops. This approach serves two purposes. First, it allows us to assess the plausibility of the parallel trends assumption before November 2017. Second,

⁸We aggregated t every three months using November 2017 as our indicator to aggregate.

the model captures how police stops evolved after the 7-Eleven donation, enabling us to estimate period-specific effects and explore temporal dynamics of the donation's impact. Specifically, we estimated the following model:

$$PoliceStops_{it}^s = \beta_0 + \sum_{k=-m}^{k=-1} \beta_{1,k} \mathbf{1}(t - T_i = k) + \sum_{k=1}^n \beta_{2,k} \mathbf{1}(t - T_i = k) + \alpha_i + \delta_t + \varepsilon_{it} \quad (2)$$

where $\mathbf{1}(t - T_i = k)$ is an indicator equal to one when observation i in period t is k periods from the donation event date T_i (November 2017 for treated blocks), with $k = 0$ omitted as the reference period; $\beta_{1,k}$ captures the average difference in the outcome during the pre-donation period (from $k = -m$ to $k = -1$, with $m = 2, \dots, 6$), and $\beta_{2,k}$ captures the average difference in the post-donation period (from $k = 1$ to $k = n$, where $n = 2, \dots, 9$). We estimated an additional specification of model (2) to assess crime pre-trends (see Figure S2 in the Methodological Appendix).

We estimated three tests replicating our main specification to assess the likelihood of obtaining false statistically significant results. First, we changed our treated group from blocks with a 7-Eleven to blocks with other convenience stores and maintained our control group (Table 5). Second, we replicated our analysis during the pre-treatment period only, assigning a false treatment date of November 2016 and excluding the actual donation quarter (seven quarterly periods). This placebo analysis therefore uses only quarters prior to the true November 2017 donation, ensuring that any estimated effect reflects pre-existing trends rather than the true treatment (see Table S13 in the Methodological Appendix). Finally, we simulated different versions of our treatment, changing and maintaining November 2017 as the donation date. We randomly assigned Chicago blocks to a treatment and control group and estimated our model (1). We repeated this random assignment and estimation 100 times (see Figure 4 in the Methodological Appendix). We could not replicate our results with these three tests, suggesting that our estimations are not false significant effects.

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Supplementary materials

Methodological Appendix

Figures S1 to S5

Tables S1 to S13

**Supplementary Materials for
Evidence of Donor Bias in Chicago Police Stops**

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This PDF file includes:

Methodological Appendix

Figures S1 to S5

Tables S1 to S13

Methodological Appendix

1 Pre-Donation Baseline

Table S1: Pre-Donation Baseline Descriptive Statistics for Treated Blocks

Outcome	Mean	SD	Min	Max
<i>All Races</i>				
All Investigatory Stops	0.893	2.992	0	67
Pedestrian Stops	0.722	2.815	0	66
Vehicular Stops	0.171	0.627	0	8
<i>Black</i>				
All Investigatory Stops	0.497	2.541	0	61
Pedestrian Stops	0.436	2.420	0	60
Vehicular Stops	0.061	0.360	0	7
<i>White</i>				
All Investigatory Stops	0.187	0.594	0	7
Pedestrian Stops	0.153	0.548	0	7
Vehicular Stops	0.033	0.203	0	3
<i>Hispanic</i>				
All Investigatory Stops	0.191	0.671	0	11
Pedestrian Stops	0.119	0.512	0	11
Vehicular Stops	0.072	0.393	0	6

Notes: Descriptive statistics computed for the 626 treated blocks (blocks within 330 feet of a 7-Eleven store) during the pre-donation period (2016Q2–2017Q4, 7 quarterly periods, 4,382 block-quarter observations). The outcome is the number of investigatory stops per block per quarter. The high proportion of zero-valued observations reflects the granularity of block-level quarterly counts; the fixed-effects Poisson specification in Table 2 accounts for this zero-inflation by conditioning on blocks with at least one stop across all periods.

2 Context: Chicago True Blue Gala

The Chicago Police Foundation’s inaugural TBG aimed to raise funds to provide necessary equipment and relevant education for Chicago police officers, but faced significant challenges in event

promotion due to restrictive policies and logistical hurdles, including background checks on every attendee and vendor, rigorous event permitting, and venue restrictions.

Police executives' and rank-and-file officers' responses to the donations provided through the TBG suggest immense gratitude and indicate a preference for reciprocation.⁹ The Chicago Fraternal Order of Police (FoP) showcased several of its annual galas in its monthly magazine and featured numerous quotes from officers in attendance. Officers frequently drew on the language of “backing the blue” to describe donations. The FoP magazine described one sergeant as “filled with gratitude” for the donations and quoted him as stating “It feels nice that someone’s still in our corner, still backing the blue.”¹⁰ Another officer positioned the donations against public framing of the police, stating “the media displays a lot of anger and outrage toward the police. . . but to know there are actually people like this backing us up. . . and support us financially—it’s a great feeling.”¹¹

Officers described being “rejuvenated” by the show of support,¹² feeling speechless and sharing pictures of the event with their peers,¹³ and otherwise described how the event made them “feel good.”¹⁴ The emotional reaction that social and evolutionary psychologists argue plays a role in developing a preference for reciprocation, gratitude, describes officers’ experiences with the TBG. Members of the Chicago Police Foundation are keenly aware of this reaction, as well, with board member Michael Distasio claiming “when I talk to officers about what we do in the foundation, they’re beyond grateful. . . they’re very happy and very grateful that there are people who still believe in them.”¹⁵

At the 2022 TBG, recently inaugurated Superintendent Larry Snelling delivered remarks that sent some of the strongest signals of reciprocation. The FoP noted that Snelling “reciprocated words of respect and appreciation toward the foundation’s leaders and volunteers” while stating that their

⁹Fong (7) uses the language of “a preference for reciprocation” to refer to the inclination of legislators to help others that have helped them in the past.

¹⁰Fraternal Order of Police Chicago Lodge 7 Magazine. November, 2019. Page 40. <https://online.flipbuilder.com/prsn/wkgl/mobile/index.html#p=40>

¹¹Fraternal Order of Police Chicago Lodge 7 Magazine. November, 2018. Page 45. <https://online.flipbuilder.com/prsn/znwh/mobile/index.html#p=46>

¹²Ibid. pp. 48.

¹³Ibid.

¹⁴Fraternal Order of Police Chicago Lodge 7 Magazine. December, 2017. Page 43. <https://online.flipbuilder.com/prsn/rqbq/mobile/index.html#p=43>

¹⁵Fraternal Order of Police Chicago Lodge 7 Magazine. October, 2023. Page 48. <https://www.chicagofop.org/news/newsletters>

“gestures may seem small, but they go a long way with our officers, trust me.”¹⁶ In his conclusion, Snelling further praised the support of the foundation and its donors, ultimately stating that “because we know that you have our backs, we will always have yours.”¹⁷ Figure S1 shows an example of promotional materials of the TBG, illustrating the donation publicity presented to Chicago police officers.

¹⁶Fraternal Order of Police Chicago Lodge 7 Magazine. November, 2023. Pages 48–50. <https://www.chicagofop.org/newsletters-archive>

¹⁷Ibid. p. 49.



Figure S1: Sponsor Recognition Materials from the 2017 True Blue Gala

Note: Images sourced from Chicago Police Foundation promotional materials and Fraternal Order of Police magazine coverage.

3 Donations for TBG (2016–2024)

Table S2 shows our donation data information showing all private corporations with physical presence across Chicago and whether they have sponsored the TBG between 2016 and 2024, excluding 2020 due to the Covid-19 pandemic and 2022 due to missing data.

Table S2: Chicago Police Foundation Gala Sponsors by Year

Company	2016	2017	2018	2019	2021	2023	2024
@ Properties	No	Yes	Yes	Yes	Yes	No	No
7Eleven	No	Yes	No	No	No	No	No
8 Hospitality	No	No	No	No	Yes	Yes	Yes
AED Professionals	No	No	No	No	Yes	Yes	Yes
Allan Reich	No	No	No	No	No	Yes	Yes
American Airlines	No	No	No	Yes	No	No	No
American Heritage Protective Services	No	No	No	No	No	No	Yes
Axon	No	Yes	Yes	Yes	Yes	No	No
BMO Harris Bank	Yes	Yes	Yes	Yes	No	No	No
Cigna	Yes	Yes	Yes	Yes	No	No	No
CNA	No	No	No	Yes	Yes	No	No
Columbia Pipe and Supply	No	No	Yes	Yes	No	No	No
ComEd	Yes	Yes	Yes	No	No	No	No
Daniel Micic	No	No	No	No	No	No	Yes
Dr. Scholls	No	No	No	No	No	No	Yes
Duchossois Capital Management	No	No	No	No	No	No	Yes
Eli's Cheesecake	No	Yes	No	No	No	Yes	Yes
Employco USA	No	No	No	No	No	No	Yes
Energy Distribution Partners	No	No	No	No	No	No	Yes
Fifth Third Bank	No	No	No	No	No	Yes	Yes
Forward Together	No	No	No	No	Yes	No	No
Four Seasons	No	No	No	No	No	Yes	Yes
Gaslight Studios	No	No	No	No	No	No	Yes
GCM Grosvenor	No	Yes	No	No	No	No	No
GPG Strategies	No	No	Yes	Yes	No	No	No
Greeley and Hansen	Yes	Yes	Yes	Yes	Yes	No	No

Continued on next page

Table S2 – continued

Company	2016	2017	2018	2019	2021	2023	2024
Hilco Global	Yes	No	No	No	No	No	No
Horizon (Therapeutics)	No	No	No	No	No	Yes	No
Hyatt Regency	No	Yes	No	No	No	No	No
Jim Kallas Calumet Hospitality	No	No	No	No	Yes	No	No
John Robak	No	No	No	No	No	Yes	Yes
JP Morgan Chase	No	No	No	No	No	No	Yes
Justine Fedak Corporate Hippie	No	No	No	No	Yes	No	No
Kevin and Sarah Bruno	No	No	No	No	No	No	Yes
Landis Family Foundation	No	No	No	No	No	Yes	Yes
Law Offices of Debra Dimaggio	No	No	No	No	Yes	Yes	No
LAZ Parking	Yes	Yes	Yes	Yes	Yes	Yes	No
Louis D'Angelo	No	No	No	No	No	No	Yes
Magellan Corporation	No	Yes	Yes	Yes	No	No	No
Maverick Hotel and Restaurants	No	No	No	No	No	Yes	Yes
MB Financial	No	Yes	Yes	No	No	No	No
Medical Shipment	No	No	No	No	Yes	No	No
Modern Luxury CS	Yes	Yes	No	No	No	No	No
Motorola Solutions	No	No	No	Yes	No	No	No
Noahs Ark	No	No	No	No	Yes	No	No
OverNorth	No	No	Yes	No	No	No	No
Pamela and Alfredo Capitanini	No	No	No	No	No	Yes	Yes
Peoples Gas	No	No	No	No	No	Yes	Yes
PNC Bank	No	No	Yes	No	Yes	No	No
Rainbow Sandals	No	No	No	No	Yes	No	No
Richard Gamble	No	No	No	No	No	No	Yes
Risk Management Solutions of America	No	No	No	No	No	No	Yes

Continued on next page

Table S2 – continued

Company	2016	2017	2018	2019	2021	2023	2024
RME (Rubino and Mesia Engineers)	No	No	No	No	Yes	No	No
RSM (RSM International)	No	No	No	No	Yes	No	No
Shotspotter	No	No	Yes	No	Yes	No	Yes
Sudler	No	No	Yes	No	No	No	No
Supreme Lobster	No	No	No	No	No	No	Yes
Taft Law	No	No	No	No	No	No	Yes
Tamar Productions	No	Yes	No	No	No	Yes	Yes
The Horton Group	No	No	No	No	No	Yes	No
The Sotos Law Firm	No	No	No	No	Yes	No	No
Third Coast Hospitality	No	No	No	No	Yes	No	No
Time Zone One	No	No	No	No	Yes	No	No
Torshen Capital Mgmt/Kay Torshen Fdn	No	No	Yes	Yes	No	No	No
Tritech/Central Square	No	No	No	Yes	Yes	No	No
Tullman Community Ventures	No	No	No	No	No	Yes	No
United Airlines	No	No	No	Yes	No	No	No
United Scrap	No	No	No	No	No	Yes	Yes
United Service Companies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
US Bank	No	Yes	Yes	Yes	No	No	No
USI	Yes	Yes	Yes	Yes	Yes	No	Yes
Verizon	No	No	No	Yes	Yes	Yes	Yes
Weber Shandwick	Yes	Yes	Yes	Yes	No	No	No
William Blair	No	No	No	No	No	No	Yes
Wintrust	No	No	No	No	No	No	Yes
Wilson Companies	No	No	No	No	No	Yes	No

Table S3 evaluates every 2017 TBG sponsor with a physical presence in Chicago against the four design requirements for our block-level difference-in-differences: (1) sufficient locations for

statistical power, (2) city-wide geographic distribution, (3) one-time donation for clean pre/post identification, and (4) walk-up retail format relevant to pedestrian enforcement. 7-Eleven is the only sponsor satisfying all four criteria. Sponsors from other years (2016, 2018–2024) face similar or more severe limitations.

Table S3: Assessment of TBG Sponsors for Quasi-Experimental Design Feasibility

Sponsor (2017)	Chicago Locations	City-Wide Spread	One-Time Donor	Walk-Up Retail	Design Feasible
7-Eleven	116 stores	Yes	Yes (2017 only)	Yes	Yes
@Properties	~10 offices	Partial (North Side)	No (2017–2019, 2021)	No (office)	No
Axon	0 retail	N/A	No (2017–2019, 2021)	No (manu- facturer)	No
BMO Harris Bank	~30–50 branches	Partial (commer- cial)	No (2016–2019)	Partial	No
Cigna	1–2 offices	No	No (2016–2019)	No (office)	No
ComEd	0 retail	N/A	No (2016–2018)	No (utility)	No
Eli’s Cheesecake	1 bakery	No	No (2017, 2023–24)	Yes	No
GCM Grosvenor	1 office	No	Yes (2017 only)	No (office)	No
Greeley and Hansen	1 office	No	No (2016–2019, 2021)	No (office)	No
Hyatt Regency	~3–5 hotels	No (downtown)	Yes (2017 only)	No (hotel)	No
LAZ Parking	~20–40 lots	No (Loop- concentrated)	No (2016–2019, 2021, 2023)	No (parking)	No
Magellan Corp.	1–2 properties	No	No (2017–2019)	No (developer)	No

Continued on next page

Table S3 – continued

Sponsor (2017)	Chicago Locations	City-Wide Spread	One-Time Donor	Walk-Up Retail	Design Feasible
MB Financial	~15–25 branches	Partial	No (2017–2018)	Partial	No
US Bank	~20–30 branches	Partial	No (2017–2019)	Partial	No
USI	1 office	No	No (2016–2019, 2021, 2024)	No (insurance)	No
Weber Shandwick	1 office	No	No (2016–2019)	No (PR firm)	No

4 Control Group Blocks

We compared 7-Eleven blocks to three groups of blocks to identify a group of blocks with similar crime dynamics to rule out potential effects in police stops due to crime dynamics. We compared (i) 7-Eleven blocks to all other blocks without a 7-Eleven, (ii) 7-Eleven blocks to all other blocks with a convenience store, and (iii) 7-Eleven blocks to all other blocks with major intersections. Figure S2 shows that the best comparison group is blocks with major intersections since these have no statistically significant difference in crime rates before the donation occurred.

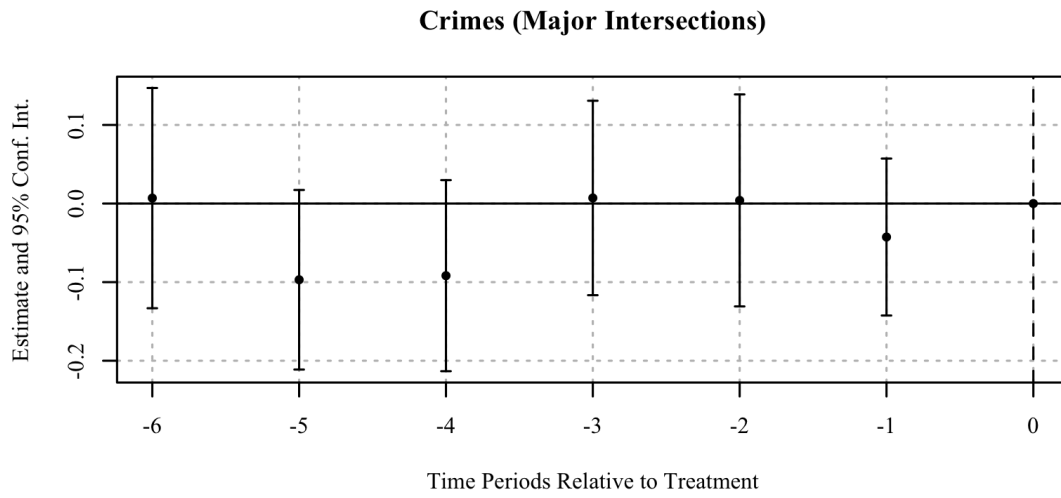
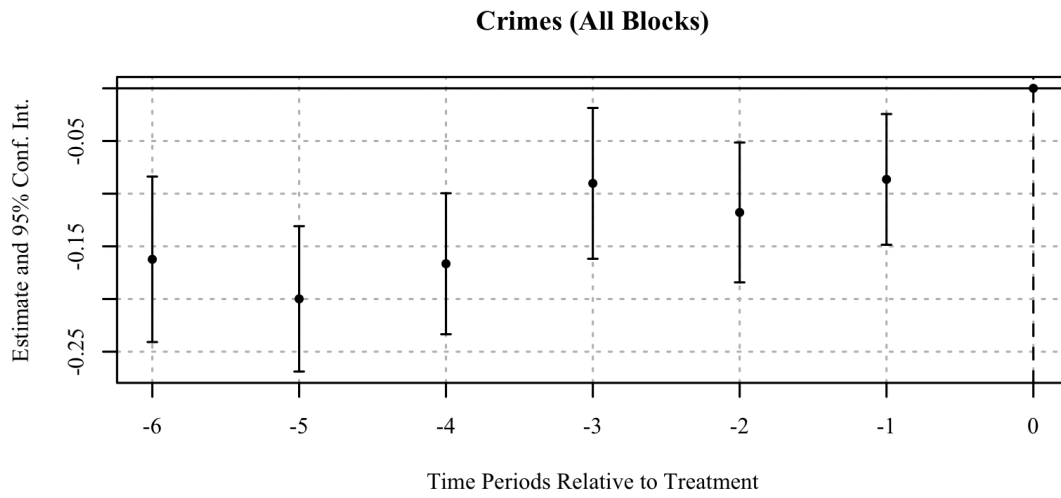


Figure S2: Crime Pre-trends: Potential Control Blocks

Once we found a group of blocks with comparable crime trends, we checked potential differences across other block socio-economic characteristics. In our main table, Table 8, we present that based on the ACS from 2016 to 2020, there were no statistically significant differences between 7-Eleven and major intersection blocks. Table S4 shows a similar trend for the years 2011 to 2015, confirming that these blocks had balanced characteristics during our study period, but these similarities have been constant over time.

Table S4: Balance Table Based on ACS 2011–2015 Characteristics

	Control ($N = 441$)		Treatment ($N = 626$)		Diff. in Means
	Mean	Std. Dev.	Mean	Std. Dev.	
Median Income	80,964.3	30,103.0	78,277.1	29,434.0	−2,687.2
Share Black	0.076	0.094	0.070	0.080	−0.005
Share White	0.619	0.201	0.588	0.195	−0.031
Share Hispanic	0.186	0.200	0.208	0.218	0.022
Share Males Ages 15–25	0.071	0.065	0.069	0.053	−0.002
Share College Graduates	0.616	0.248	0.584	0.265	−0.031
Share Below Poverty Line	0.135	0.096	0.135	0.081	0.000

Notes: Data from American Community Survey five-year estimates, 2011–2015.

5 Robustness Checks

We estimated alternative versions of our main specification (Table 1) to assess the robustness of our findings. In Table S5, we estimated the difference-in-difference models without any controls to show that our results are not sensitive to changes in the control variables. Table S6 shows the results without controls using the Poisson specification.

Table S5: TWFE Estimates without Controls

	All Races	Black	White	Hispanic
Panel A. 7-Elevens vs. Major Intersections				
All Investigatory Stops				
Treat $\times$ Post	0.632*** (0.133)	0.427*** (0.102)	0.101*** (0.026)	0.091*** (0.028)
Pedestrian Stops				
Treat $\times$ Post	0.568*** (0.121)	0.394*** (0.096)	0.097*** (0.025)	0.067*** (0.020)
Vehicular Stops				
Treat $\times$ Post	0.064* (0.027)	0.033* (0.015)	0.004 (0.007)	0.024 (0.016)
Panel B. 7-Elevens vs. Other Stores				
All Investigatory Stops				
Treat $\times$ Post	0.431** (0.152)	0.283* (0.122)	0.131*** (0.032)	0.003 (0.033)
Pedestrian Stops				
Treat $\times$ Post	0.684*** (0.140)	0.487*** (0.113)	0.127*** (0.032)	0.060* (0.027)
Vehicular Stops				
Treat $\times$ Post	-0.253*** (0.034)	-0.203*** (0.026)	0.004 (0.006)	-0.057*** (0.016)

Notes: Panel A compares 7-Eleven blocks to blocks with major intersections. The sample includes 1,067 blocks (626 treatment, 441 control), aggregated in three-month periods over 16 quarterly periods (2016Q2–2020Q1), for a total of 17,072 observations. Panel B compares 7-Eleven blocks to blocks with other convenience stores (34,656 observations, 2,166 blocks). All models include block and time fixed effects. Standard errors are clustered at the block level.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S6: Fixed-Effects Poisson Estimates without Controls

	All Races	Black	White	Hispanic
Panel A. 7-Elevens vs. Major Intersections				
All Investigatory Stops				
Treat × Post	0.330*** (0.102)	0.398** (0.144)	0.261* (0.114)	0.218+ (0.115)
IRR	1.39	1.49	1.30	1.24
Pedestrian Stops				
Treat × Post	0.393*** (0.116)	0.455** (0.159)	0.301* (0.131)	0.292* (0.134)
IRR	1.48	1.58	1.35	1.34
Vehicular Stops				
Treat × Post	0.096 (0.126)	0.099 (0.187)	0.031 (0.195)	0.092 (0.188)
IRR	1.10	1.10	1.03	1.10
Panel B. 7-Elevens vs. Other Stores				
All Investigatory Stops				
Treat × Post	0.525*** (0.074)	0.618*** (0.103)	0.481** (0.154)	0.295*** (0.089)
IRR	1.69	1.86	1.63	1.35
Pedestrian Stops				
Treat × Post	0.679*** (0.087)	0.752*** (0.114)	0.555** (0.187)	0.488*** (0.115)
IRR	1.97	2.12	1.75	1.63
Vehicular Stops				
Treat × Post	0.098 (0.079)	0.265* (0.114)	0.121 (0.139)	−0.023 (0.116)
IRR	1.10	1.30	1.13	0.98

Notes: Panel A compares 7-Eleven blocks to blocks with major intersections and includes 17,072 block-quarter observations before zero-dropping (retained observations vary by outcome: 8,928–14,288). Panel B compares 7-Eleven blocks to blocks with other convenience stores and includes 34,656 observations before zero-dropping. All models are fixed-effects Poisson specifications with block and period (quarterly) fixed effects. Standard errors are clustered at the block level. IRR = $\exp(\text{coefficient})$.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To show our results are robust to time period aggregation, we also reran our main models for Panel

A (7-Elevens vs Major Intersections) using months as the time period rather than quarters. Table S7 and Table S8 show our results are not sensitive to the time period used for aggregation.

Table S7: TWFE Estimates with Lagged Crime Control By Month (7-Elevens vs. Major Intersections)

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post	0.206*** (0.044)	0.139*** (0.034)	0.033*** (0.009)	0.030** (0.009)
Pedestrian Stops				
Treat × Post	0.183*** (0.040)	0.128*** (0.031)	0.031*** (0.008)	0.022** (0.007)
Vehicular Stops				
Treat × Post	0.022* (0.009)	0.011* (0.005)	0.002 (0.002)	0.008 (0.001)

Notes: Regression results compare 7-Eleven blocks to blocks with major intersections and includes 1,067 blocks (626 treatment, 441 control), aggregated by month over 50 months (Jan 2016–February 2020) for a total of 53,350 observations. All models include block and month fixed effects. Standard errors are clustered at the block level.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S8: Fixed-Effects Poisson Estimates with Lagged Crime Control By Month (7-Elevens vs. Major Intersections)

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post	0.305** (0.096)	0.364** (0.136)	0.250* (0.112)	0.203+ (0.111)
Pedestrian Stops				
Treat × Post	0.363*** (0.109)	0.423** (0.148)	0.269* (0.130)	0.276* (0.130)
Vehicular Stops				
Treat × Post	0.099 (0.125)	0.081 (0.190)	0.120 (0.189)	0.079 (0.185)

Notes: Regression results compare 7-Eleven blocks to blocks with major intersections and includes 53,350 block-month observations before zero-dropping (retained observations vary by outcome: 18,250–44,850). All models are fixed-effects Poisson specifications with block and period (quarterly) fixed effects. Standard errors are clustered at the block level.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S9 and Table S10 report estimates comparing blocks near a 7-Eleven vs all other blocks in Chicago. Outcomes are similar across both models compared to the control groups used in the main text.

Table S9: TWFE Estimates with Lagged Crime Control: 7-Elevens vs. All Other Blocks

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat $\times$ Post	0.638*** (0.107)	0.389*** (0.084)	0.131*** (0.020)	0.100*** (0.021)
Pedestrian Stops				
Treat $\times$ Post	0.656*** (0.100)	0.435*** (0.080)	0.124*** (0.019)	0.085*** (0.016)
Vehicular Stops				
Treat $\times$ Post	-0.018 (0.020)	-0.046*** (0.011)	0.007 (0.004)	0.015 (0.011)

Notes: Regression results compare 7-Eleven blocks to all other blocks and includes 33,774 blocks (626 treatment, 33,148 control), aggregated in three-month periods over 16 quarterly periods (2016Q2–2020Q1), for a total of 540,384 observations. All models include block and month fixed effects. Standard errors are clustered at the block level.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S10: Fixed-Effect Poisson Estimates with Lagged Crime Control: 7-Elevens vs. All Other Blocks

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post	0.432*** (0.056)	0.515*** (0.083)	0.293*** (0.067)	0.282*** (0.071)
IRR	1.54	1.67	1.34	1.33
Pedestrian Stops				
Treat × Post	0.596*** (0.065)	0.680*** (0.088)	0.345*** (0.077)	0.444*** (0.088)
IRR	1.81	1.97	1.41	1.56
Vehicular Stops				
Treat × Post	0.089 (0.072)	0.233* (0.107)	0.025 (0.117)	0.031 (0.105)
IRR	1.09	1.26	1.03	1.03

Notes: Regression results compare 7-Eleven blocks to all other blocks and includes 540,384 block-quarter observations before zero-dropping (retained observations vary by outcome: 165,664–419,184). All models are fixed-effects Poisson specifications with block and period (quarterly) fixed effects. Standard errors are clustered at the block level. IRR = $\exp(\text{coefficient})$.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S11: Fixed-Effects Poisson Estimates of Disaggregated Enforcement Outcomes

	treatpost
Crimes	0.010 (0.033)
All Enforcement Actions	0.275* (0.119)
Arrests	0.267* (0.123)
Felonies Charged	0.053 (0.218)
Misdemeanors Charge	0.120 (0.168)
Warrant Arrests	0.385 (0.265)
Felonies, Misdemeanors, and Warrants	0.130 (0.123)
Ordinance Violations and Citations	0.405+ (0.230)
Contraband Found	-0.177 (0.178)
Stops with No Enforcement Action	0.364*** (0.109)

Notes: All models are fixed-effects Poisson (conditional maximum likelihood) specifications with block and period fixed effects. The sample is Panel A (7-Eleven blocks vs. major intersection blocks), restricted to blocks with at least one occurrence of the relevant outcome across all periods; sample sizes therefore vary by outcome. Standard errors are clustered at the block level.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Additionally, we estimated two event studies comparing 7-Eleven blocks to other convenience store blocks, one using a TWFE specification (Figure S3) and the other using a Poisson specification (Figure S4). Figures S3 and S4 show that pre-donation coefficients are not centered around zero for all groups, reflecting the existence of pre-trends when using the other-convenience-store blocks

as control group.

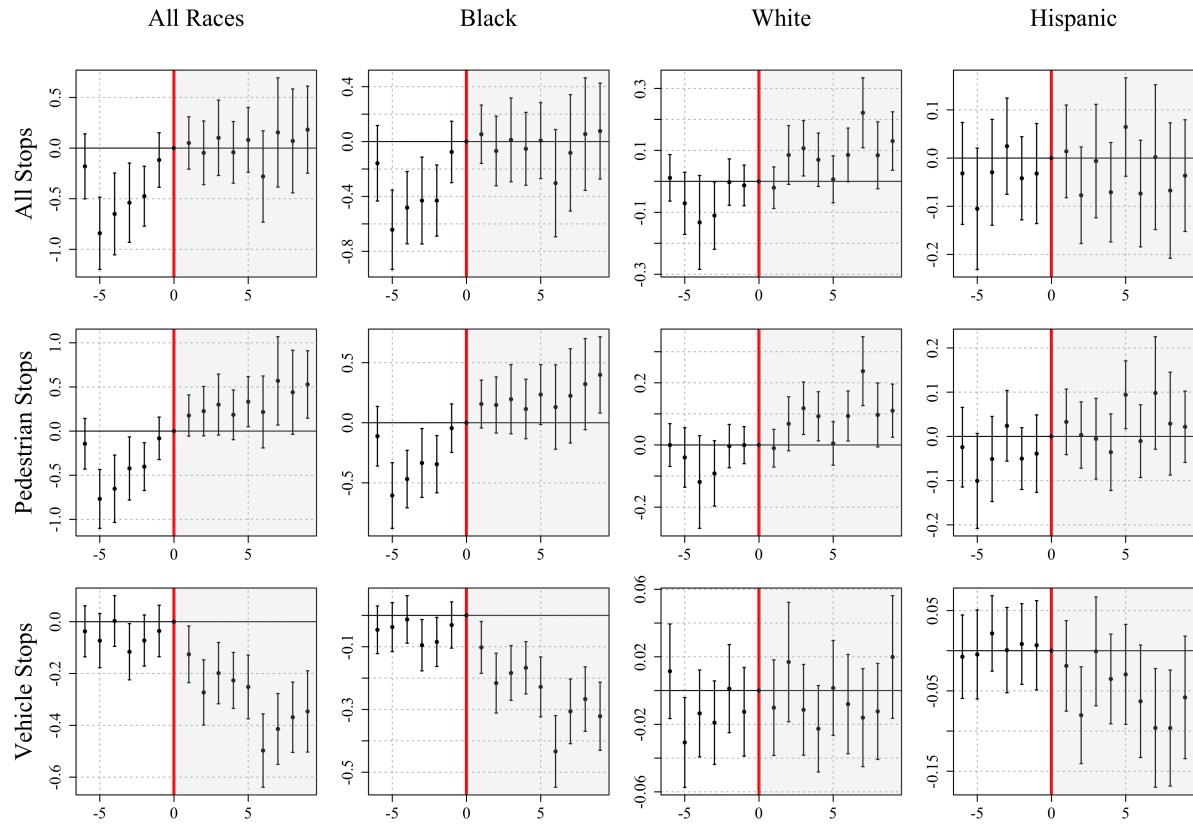


Figure S3: Event Study Analysis: 7-Elevens vs. Other Convenience Stores

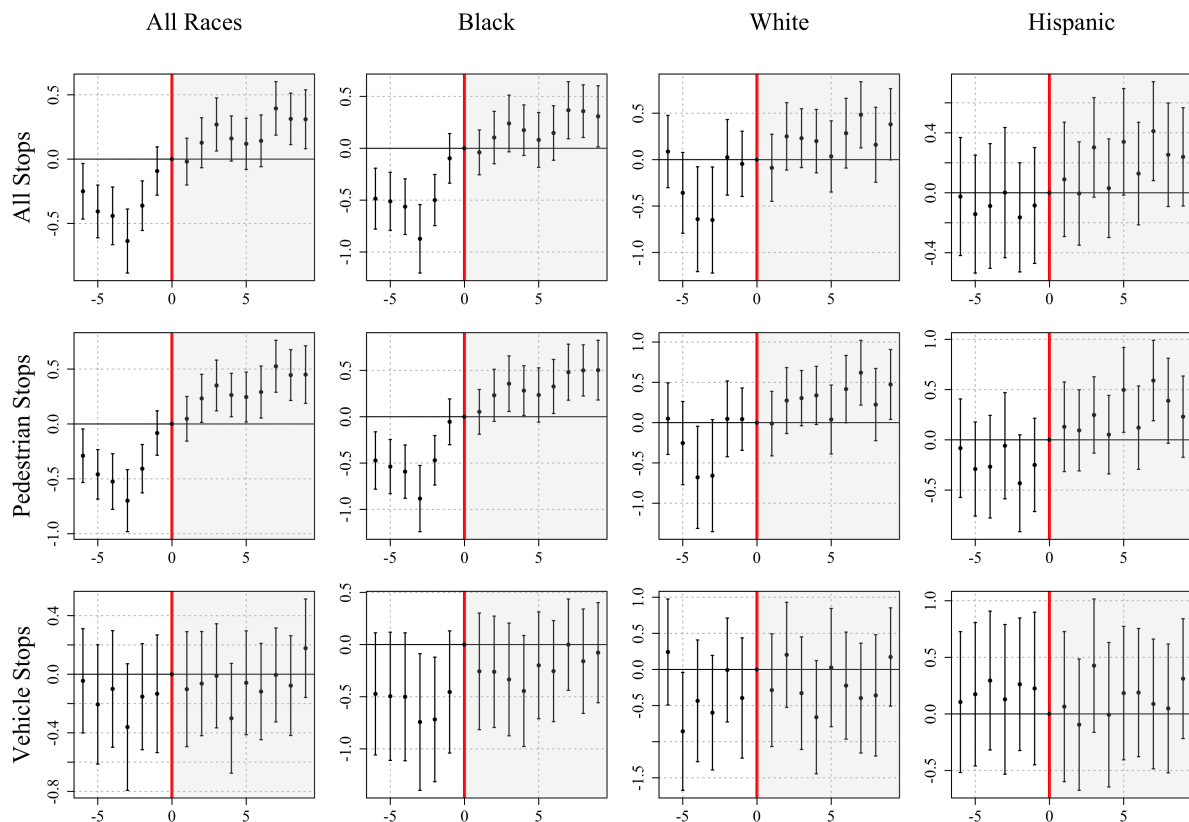


Figure S4: Event Study Poisson Analysis: 7-Elevens vs. Other Convenience Stores

6 Block-Level Heterogeneity and Spatial Robustness

To assess whether the treatment effect is driven by a small number of outlier blocks, we re-estimated our main specification (Equation 1) 626 times, each time excluding one treated block. Figure S5 shows the distribution of the resulting coefficients. The estimates range from 0.592 to 0.637 with a mean of 0.632 and standard deviation of 0.005, indicating that no single block drives the result. The treatment effect remains positive and substantively similar to the full-sample estimate regardless of which block is excluded.

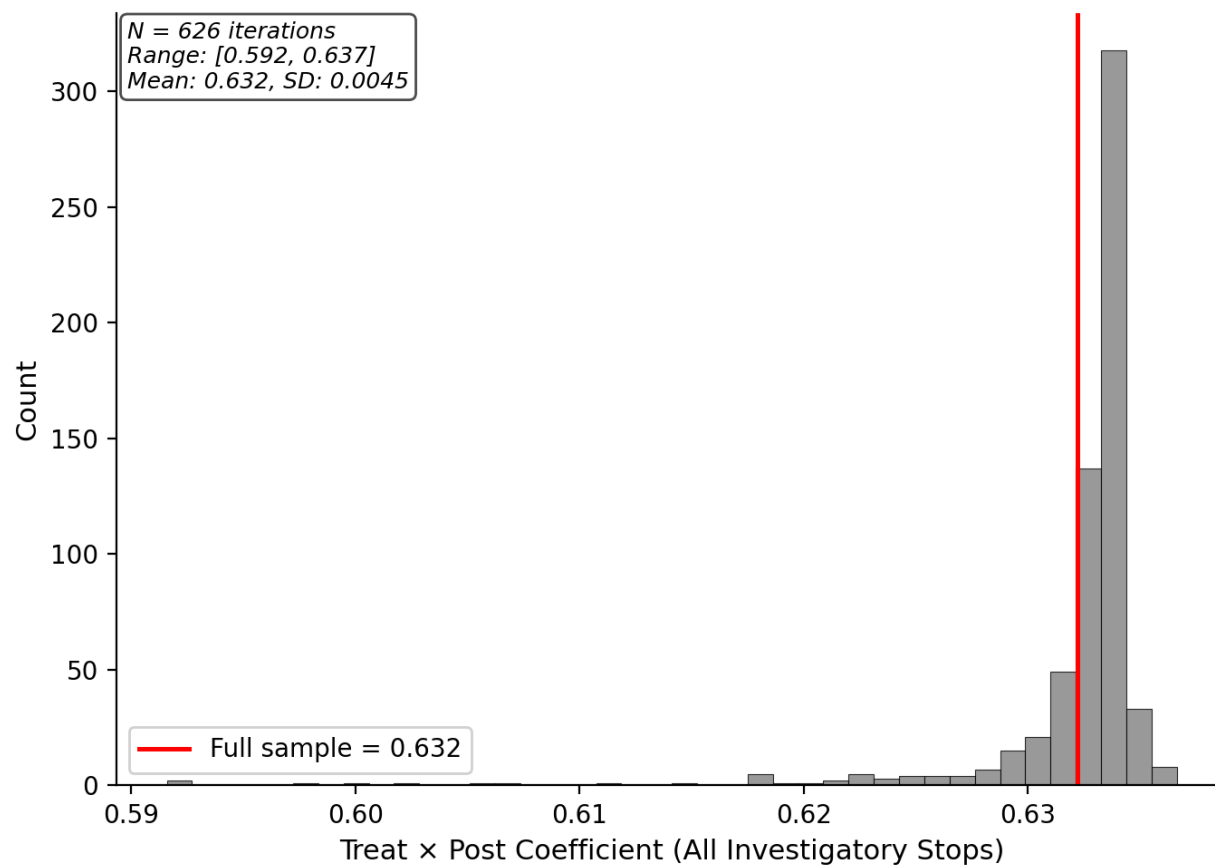


Figure S5: Leave-One-Out Sensitivity Analysis

In addition to our leave-one-out sensitivity analysis, we re-estimated our main specification excluding all blocks inside of the Loop Community area (108 treatment blocks and 42 control blocks, approximately 10–17% of each group). Table S12 reports the results. All effects remain statistically significant, supporting that the results are not driven by Loop blocks.

Table S12: Robustness: Excluding Downtown Loop Blocks

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat $\times$ Post	0.428** (0.135)	0.246* (0.104)	0.081** (0.025)	0.089** (0.031)
Pedestrian Stops				
Treat $\times$ Post	0.375** (0.122)	0.228* (0.097)	0.079** (0.024)	0.059** (0.022)
Vehicular Stops				
Treat $\times$ Post	0.054+ (0.030)	0.018 (0.016)	0.002 (0.007)	0.030+ (0.018)

Notes: Excludes 108 treatment blocks and 42 control blocks inside the Loop Community Area. Remaining sample: 518 treatment blocks, 399 control blocks, 14,672 observations. All models include block and time fixed effects. Standard errors are clustered at the block level.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7 Additional Placebo Checks

As an additional placebo test, we assigned a false treatment date prior to the actual November 2017 7-Eleven donation and re-estimated our difference-in-difference specification on pre-treatment data only. Table S13 reports the estimated placebo treatment effect across all investigatory, pedestrian, and vehicular stops, disaggregated by racial group. Table S14 shows fixed-effects Poisson estimates for the same specification. Across these placebo specifications, the estimates do not reproduce the positive post-donation pattern observed in the main analysis. The TWFE placebo estimates are negative or near zero, and the FE Poisson placebo estimates are negative for the key pedestrian outcomes. We therefore interpret these placebo tests as evidence against a pre-existing positive trend at 7-Eleven blocks. Table S15 shows fixed-effects Poisson estimates for another placebo test comparing other convenience stores to major intersections similar to Table 5 from our main results. In this case all significant results have a different direction.

Table S13: TWFE Placebo: 7-Elevens vs. Intersections, Pre-Donation Period

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post (placebo)	−0.111 ⁺	−0.069	−0.037	0.002
	(0.067)	(0.050)	(0.023)	(0.029)
Pedestrian Stops				
Treat × Post (placebo)	−0.118 ⁺	−0.079 ⁺	−0.031	−0.005
	(0.060)	(0.047)	(0.022)	(0.021)
Vehicular Stops				
Treat × Post (placebo)	0.007	0.010	−0.006	0.007
	(0.026)	(0.016)	(0.009)	(0.017)

Notes: All models include 7,469 observations (1,067 blocks × 7 pre-treatment quarterly periods: 2016Q2–2017Q4) with lagged block-level crime as a control and block and period fixed effects. Standard errors are clustered at the block level. False treatment date = November 2016; analysis restricted to pre-treatment period only.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S14: FE Poisson Placebo: 7-Elevens vs. Intersections, Pre-Donation Period

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post (placebo)	−0.186 [*]	−0.293 [*]	−0.249 ⁺	−0.052
	(0.087)	(0.116)	(0.149)	(0.167)
Pedestrian Stops				
Treat × Post (placebo)	−0.251 ^{**}	−0.368 ^{**}	−0.254	−0.010
	(0.094)	(0.117)	(0.170)	(0.190)
Vehicular Stops				
Treat × Post (placebo)	0.062	0.182	−0.229	0.156
	(0.180)	(0.337)	(0.300)	(0.284)

Notes: All models include 7,469 observations (1,067 blocks × 7 pre-treatment quarterly periods: 2016Q2–2017Q4) with lagged block-level crime as a control and block and period fixed effects. Standard errors are clustered at the block level. False treatment date = November 2016; analysis restricted to pre-treatment period only.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S15: FE Poisson Placebo: Other Stores vs. Intersections

	All Races	Black	White	Hispanic
All Investigatory Stops				
Treat × Post (placebo)	−0.131** (0.041)	−0.126* (0.045)	−0.242+ (0.143)	−0.087 (0.058)
Pedestrian Stops				
Treat × Post (placebo)	−0.130** (0.050)	−0.102+ (0.053)	−0.273 (0.177)	−0.135+ (0.080)
Vehicular Stops				
Treat × Post (placebo)	−0.032 (0.036)	−0.050 (0.044)	−0.134 (0.085)	0.026 (0.056)

Notes: All models include 316,560 observations before zero-dropping with lagged block-level crime as a control and block and period fixed effects. Standard errors are clustered at the block level.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$