FARE-FREE TRANSIT AND CRIME: CAUSAL EVIDENCE OF FREE PUBLIC TRANSPORT ON CRIME IN THE MUNICIPALITIES OF SÃO PAULO

Tarifa Zero e criminalidade: evidência causal do transporte público gratuito sobre o crime nos municípios de São Paulo

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Resumo

Este artigo estima o efeito causal do transporte público de Tarifa Zero sobre a criminalidade, explorando a adoção escalonada por onze municípios paulistas em 2023. Com um desenho de Tríplas Diferenças Empilhadas (Stacked DDD) estimado por Poisson QMLE sobre um painel de 172 municípios e hexágonos H3 de resolução 9, encontramos que o furto em vias públicas próximo a paradas de ônibus cai 24,0% após a adoção, enquanto o roubo de rua, crime confrontacional e majoritariamente noturno, não responde. A assimetria condiz com a premissa de que a vigilância passiva detém sobretudo o crime oportunista. O efeito concentra-se em dias úteis e fora dos horários de pico, quando a política adiciona fluxo de pedestres a espaços antes subutilizados. A queda também alcança a zona de transição de 200 a 600 metros (-19,7%), afastando deslocamento espacial. As estimativas implicam cerca de 740 furtos evitados por ano.

Palavras-chave: Tarifa Zero; economia do crime; transporte público; vigilância informal; diferenças-em-diferenças; Poisson.

Abstract

This paper estimates the causal effect of fare-free public transit on crime, exploiting the staggered adoption by eleven São Paulo municipalities in 2023. Using a Stacked Triple Differences design estimated by Poisson QMLE over a panel of 172 municipalities and H3 resolution-9 hexagons, we find that street theft near bus stops falls by 24.0% after adoption, whereas street robbery, a confrontational, mostly nighttime offense, does not respond. The asymmetry fits the premise that passive surveillance mainly deters opportunistic crime. The effect concentrates on weekdays and outside peak hours, when the policy adds foot traffic to previously underused spaces. The decline also reaches the 200-to-600-meter transition zone (-19.7%), ruling out spatial displacement. The estimates imply roughly 740 street thefts prevented per year, nearly a quarter of those that would occur around the stops absent the policy. These results provide the first causal evidence that fare-free transit reduces crime through informal surveillance.

Keywords: fare-free transit; economics of crime; public transportation; informal surveillance; difference-in-differences; Poisson.

Área de avaliação: Economia Regional
Classificação JEL: K42; R41; H76

1 INTRODUCTION

Crime imposes costs that extend beyond direct victimization. Violence and property crime reduce investment, depreciate real estate, and restrict safe access to urban space. Moreover, they generate high fiscal costs through intensive policing, whose expansion requires investment that most municipalities struggle to sustain (Chalfin and McCrary, 2018). Latin America concentrates a disproportionate share of global crime (Muggah and Tobón, 2018), and Brazil records millions of street theft and robbery incidents each year. This burden falls disproportionately on low-income households, which have fewer protective alternatives, a pattern documented by Britto et al. (2022) and by the Brazilian Forum on Public Safety (FBSP, 2024). To mitigate this burden without incurring the high fiscal costs of expanding police forces, the literature has turned to the tactical management of urban micro-places. Branas et al. (2018) show, in a randomized controlled trial, that the remediation of vacant lots in Philadelphia reduced gun violence by generating pedestrian flow on previously abandoned blocks. Along the same lines, Chang and Jacobson (2017) document the mirror image of this result, finding that the closure of cannabis dispensaries in Los Angeles reduced foot traffic and increased theft. The underlying mechanism is the same in both cases, since the presence of people constitutes diffuse, passive surveillance that reduces opportunistic crime. Therefore, any policy capable of altering this flow exogenously is, in principle, an instrument of public safety, even if it was not designed for that purpose.

What remains unknown is whether universal fare-free public transit changes crime rates around bus stops, through the channel of diffuse surveillance that the expansion of pedestrian flow activates near those stops. Brough et al. (2025) show, in a controlled experiment in the United States, that individual free-transit programs for welfare recipients reduce arrests among the recipients themselves. However, that result operates through the channel of individual mobility and does not capture the diffuse effect on the space around the stops, which depends on the composition of who occupies that space and at which times. Fare-free public transit ("Tarifa Zero," or TZ) is the complete elimination of fares on municipal bus networks, with the operating cost assumed by the public sector, positioning it as an instrument of urban inclusion with potential impact on that composition. The policy provides useful variation precisely because it shifts transport demand through the price channel.

This paper estimates the effect of fare-free transit on crime in the municipalities of São Paulo, exploiting the staggered adoption of the policy by eleven municipalities in the state over the four quarters of 2023. The identification design is a *Triple Difference-in-Differences* (DDD). The three dimensions are temporal variation (before and after adoption), geographic variation (treated versus never-treated municipalities), and within-municipality spatial variation (hexagons up to 200 meters from a stop versus hexagons beyond 600 meters). Municipality-by-quarter fixed effects absorb all local shocks that affect crime uniformly within the municipality. In this way, the coefficient captures only the differential between units near and far from the stops. To handle the staggered adoption, we estimate the model with the *Stacked DiD* (Baker et al., 2022), which builds clean comparisons for each adoption cohort using never-treated municipalities as controls. Consequently, we avoid the forbidden comparisons documented by Strezhnev (2023) for three-way estimators under heterogeneous adoption.

The main result is a 24.0% reduction in street theft near stops after TZ adoption. This effect is statistically significant and robust to five alternative staggered difference-in-differences estimators, to alternative functional forms, and to excluding each adoption cohort individually. These estimates imply that TZ adoption prevented approximately 740 street thefts per year in the treated municipalities, corresponding to about one quarter of the thefts that would occur around the stops in the absence of the policy.

We refer to the ring of 200 to 600 meters around the stops as the *donut*, excluded from the main estimation so as to serve as a test of spatial displacement. In that zone, crime also falls after adoption, by 19.7%. Guerette and Bowers (2009), in a meta-analysis of 102 situational-prevention studies, document that geographic displacement occurs in only 26% of the cases evaluated, with the diffusion of benefits to neighboring areas being the most frequent outcome. Our decline in the *donut* is consistent with this pattern, so that the aggregate effect constitutes a genuine reduction in the municipality's crime and not a mere redistribution between zones near and far from the stops.

The analysis of temporal heterogeneity reveals that the effect concentrates outside peak hours during the week. The weekday ATT reaches about −32% off-peak, whereas on weekends the reduction does not attain statistical significance, with residual support from Saturdays, and during peak hours the effect is estimated with less precision. This pattern rules out the hypothesis of a mechanism driven solely by the concentration of passengers at peak hours. On the contrary, the picture reinforces the premise of informal surveillance operating at the margin. This holds because the TZ policy attracts pedestrian flow precisely in the periods when the space used to remain idle, maximizing its deterrent potential.

This paper makes three contributions to the literature. First, it presents the first causal evidence of the effect of fare-free transit on crime rates. Despite the rapid growth of these policies in Brazil and Europe, no prior study has causally estimated their effect on crime. Grimes et al. (2024) proposed a protocol for evaluating fare-free transit in Kansas City whose primary outcome is health-related, listing crime only as a possible unintended effect, so that no causal estimate has been published. Rodrigues et al. (2024), who exploit the same policy variation used here, focus on formal employment and carbon emissions and do not examine crime.

The second contribution is one of design. The literature on crime and public transit faces a recurring identification challenge, insofar as aggregate municipal effects, whether economic, policing, or demographic variations, are confounded with the local effects of the stops. To circumvent this problem, we combine three sources of variation in a *Stacked Triple Differences* estimator (Hsieh, 2026). The temporal dimension separates the period before TZ adoption; the geographic dimension compares the 11 treated municipalities with the 161 never-treated ones in the state; and the within-municipality spatial dimension distinguishes hexagons near stops from distant hexagons within the same municipality and quarter. This third level is central because, for a hexagon near a stop to be affected by the policy, the change must occur precisely in that physical space and not merely in the municipality as a whole. The simultaneous comparison of multiple treated cities at different moments, combined with this within-municipality control, makes it possible to separate the informal-surveillance channel from the contextual variations that affect the entire municipality.

On the empirical side, we build a panel of approximately 39,000 H3 resolution-9 hexagons covering 172 São Paulo municipalities, the largest hexagonal panel for causal crime analysis in Brazil in terms of the number of municipalities. Carvalho and Guerra (2025) validated this spatial unit in Fortaleza and showed that cells at this resolution aggregate fragments of street segments in a way that reduces measurement error without diluting local effects. Our application extends this validation to a setting of small and medium-sized cities with heterogeneous road-infrastructure coverage, showing that the granularity remains adequate in contexts distinct from those of large metropolises.

The remainder of the paper is organized as follows. Section 2 develops the theoretical mechanism linking pedestrian flow, informal surveillance, and crime; Section 3 presents the data and the construction of the hexagonal panel; Section 4 develops the identification strategy and the Stacked DDD estimator; Section 5 reports the results, including the event study, the temporal heterogeneity, the absence of spatial displacement, and the aggregate magnitude; and Section 6 concludes with implications for public policy.

2 THEORETICAL MECHANISM

2.1 INFORMAL SURVEILLANCE, PEDESTRIAN FLOW, AND CRIME

The mechanism connecting pedestrian flow to crime starts from Jacobs's (1961) concept of *eyes on the street*, according to which urban streets are safe when people are present for their own reasons. This collective presence constitutes a diffuse, passive form of surveillance that potential offenders cannot easily circumvent. Safety emerges not only from the presence of people but from the diversity of uses throughout the day, with commerce, residences, and services ensuring continuous movement. Surveillance therefore operates in a strictly incidental manner. The inhibition of opportunistic crime does not stem from a deliberate effort of mutual protection among individuals, but rather from the cumulative effect generated by the simple, continuous occupation of urban space. Cohen and Felson (1979) formalize this intuition in Routine Activity Theory. According to the authors, a criminal event requires the simultaneous convergence of a motivated offender, a suitable target, and the absence of a guardian capable of inhibiting it. More people on the street suppress this third element, reducing the probability that any offender-target pair occurs without witnesses. Felson (1994) deepens this argument by showing that guardians operate through presence, not through the intention to protect. In this way, passersby who would not intervene directly nonetheless alter the offender's calculus by the mere fact of being there.

Diffuse recognition among regular users of the same space amplifies this mechanism. Milgram (1972) coined the term *familiar strangers* to describe the low-intensity recognition that emerges among people who share routines. Examples include passengers on the same bus or regulars at the same street corner who have never exchanged a word. In a complementary way, Zahnow and Corcoran (2024) frame this phenomenon as characteristic of any city with public transit, present both in metropolitan neighborhoods and in smaller cities. The vacant-lot remediation evidence of Branas et al. (2018) offers one of the cleanest causal tests of the Jacobsian mechanism. The estimated reduction in gun violence concentrated in the times and places where the intervention most increased pedestrian flow, consistent with an informal-control channel.

Crime is not distributed uniformly across urban space. Weisburd et al. (2012) document that, in every city studied, crime concentrates in a small fraction of street segments. In parallel, Weisburd (2015) formalized this regularity as the law of crime concentration. According to this law, approximately five percent of micro-places account for fifty percent of incidents. This concentration reflects the uneven geography of criminal opportunity, with Brantingham and Brantingham (1995) distinguishing two types of criminogenic places. Crime generators are places where people converge for reasons unrelated to crime, such as bus stops, schools, and shopping centers. In doing so, they incidentally create situations in which potential offenders and victims share the same space. Crime attractors, by contrast, draw offenders who already know the opportunities available there. Bus stops function primarily as generators, drawing users out of a need to commute. In this way, they generate co-presence between offenders and victims incidentally to the ordinary use of the space. However, with a history of frequent victimization, they can acquire the characteristics of attractors, as the location's reputation guides the movement of opportunistic offenders. Transport nodes are part of the everyday activity space of a substantial share of the urban population. In this direction, Bernasco and Block (2009) show that offenders select targets near the nodes of their own activity space, so that the nodes lying along their habitual routes become preferred targets.

The empirical literature documents that transport nodes concentrate crime at rates higher than those of the surrounding blocks. Loukaitou-Sideris (1999) establishes this pattern for bus stops in Los Angeles. Later systematic reviews (Ceccato et al., 2022) confirm that transport nodes are systematically overrepresented among crime hot spots across a variety of cities and contexts. This association, however, does not permit inferring the direction of the effect, since correlational studies do not separate whether stops generate crime or whether crime tends to concentrate in places of high urban activity for reasons independent of the transit network. Ceccato and Uittenbogaard (2014) find that high density at stops is associated with more theft, consistent with more targets per square meter. Empty transit environments, by contrast, are associated with more violent crime, consistent with the absence of guardians. In addition, Zahnow and Corcoran (2021) and Gerell (2018) show that crime at hot spots changes in composition and volume over the course of the day in ways consistent with informal surveillance operating at the margin of occupancy.

Recent causal studies advance this discussion by isolating variations that shift pedestrian flows in known directions. Díaz et al. (2026c) use the staggered opening of itinerant street markets in Montevideo to show that street markets increase theft significantly but leave violent robbery unchanged. In this context, the authors attribute the pattern to concentrated commercial flow that multiplies targets without proportionally raising the number of guardians in the space. Díaz et al. (2026a) replicate this result for crowding peaks at Montevideo bus stops, finding that demand surges above the baseline volume raise theft significantly. Robbery again does not respond. Both works identify a regime in which the opportunity channel, with a greater influx of people generating more targets than guardians, produces net increases in property crime. Díaz et al. (2026b) synthesize these findings in a model with a threshold in pedestrian volume. Below the threshold, the Jacobsian mechanism of informal surveillance reduces crime; above it, opportunities multiply faster than guardians. The effect of a policy that expands demand therefore depends on the initial level of foot traffic. Places starting from low volume tend to experience a reduction in crime, conditional on the baseline volume remaining below the threshold, whereas those with high pre-existing density may face the opposite risk.

2.2 HOW FARE-FREE TRANSIT ALTERS THE COMPOSITION OF DEMAND

Removing the fare produces an increase in transport demand documented across all international and domestic experiences, with Brazilian cases recording substantial increases in the first year (Fundação Rosa Luxemburgo, 2024; Santini, 2024), in a magnitude that varies with local characteristics.

The point relevant to this paper's mechanism is not only the magnitude of the increase but its composition. Before TZ, the cost of the fare operates as a filter that restricts transit use to trips of greater immediate necessity. Vasconcellos (2018) documents that the transport voucher covers only formal workers and that fares represent a substantial share of the disposable income of informal workers. It is plausible to infer that, in smaller municipalities, the pre-TZ demand profile consisted predominantly of commuting trips to work. In parallel, lower-income groups would use transit more selectively. The marginal TZ user is, on average, a low-income worker whose prior access to the system was conditioned by the cost of the fare (Gonçalves and Santini, 2023). Eliminating the price makes possible trips previously suppressed, such as visits to relatives, health appointments, and recreational travel (Pereira et al., 2023). Bull et al. (2021) find, in a controlled experiment in Santiago, that fare-free transit increases off-peak trips by 23%. These additional trips consist predominantly of the types of travel described above. The European evidence corroborates this pattern. The additional trips generated by fare-free policies concentrate disproportionately in off-peak periods and on weekends (Cats et al., 2017; Fearnley, 2013; Pero and Mihessen, 2013).

The profile of this marginal passenger corresponds precisely to Milgram's (1972) *familiar stranger*: an individual whose recurrent presence in public space generates informal surveillance without direct verbal interaction. Zahnow et al. (2023) operationalize this mechanism using smart-card data from the Brisbane network. The authors document that stations with a higher proportion of regular users record lower crime. In this way, TZ, by converting workers who previously covered the route on foot into users with a stable commuting routine, densifies the network of mutual recognition that Jacobs associates with urban safety.

This change in the composition of demand has a direct implication for the informal-surveillance mechanism. Stops that were previously almost deserted at certain hours come to have a regular presence of people. These are the moments of greatest vulnerability to opportunistic theft, since the offender tends to choose hours and places of low occupancy. He does so precisely because the absence of witnesses reduces the perceived risk (Ceccato and Uittenbogaard, 2014; Zahnow and Corcoran, 2021). In this way, TZ, by distributing passenger volume across periods that were previously deserted, operates exactly at the margin where the informal-surveillance effect is most intense. From this argument, the expectation is that the largest impacts tend to concentrate in off-peak hours and on weekends, periods in which the margin for expanding surveillance is greater, a prediction evaluated empirically in Section 5. Conversely, a null or attenuated effect is expected during the busiest hours, since urban space was already operating at high occupancy density.

2.3 WHY SMALL AND MEDIUM-SIZED CITIES: AN AMPLIFIED MECHANISM

The treated municipalities are small and medium-sized cities. This size is not an accident of the sample but a structural feature of TZ policy in Brazil, whose adoption concentrates in smaller municipalities. Table 1 presents the eleven São Paulo municipalities that adopted TZ in 2023, distributed across four quarterly cohorts. They are local and sub-regional centers in São Paulo State, with populations between 32,000 and 166,000 inhabitants, that implemented the policy over the course of that year. This specific size strengthens the informal-surveillance mechanism through three complementary channels.

Table 1 – Treated Municipalities: Fare-Free Transit in 2023

Municipality	Mesoregion	Pop. 2022	TZ Cohort	REGIC
Porto Feliz	Sorocaba	56,497	2023Q1	Local Center
Piedade	Sorocaba	52,970	2023Q1	Local Center
Jales	São José do Rio Preto	48,776	2023Q2	Sub-regional Center B
Tietê	Piracicaba	37,663	2023Q3	Local Center
São Caetano do Sul	Grande ABC	165,655	2023Q4	Regional Capital C
Itapetininga	Itapetininga	157,790	2023Q4	Sub-regional Center A
Lins	Araçatuba	74,779	2023Q4	Sub-regional Center B
Monte Mor	Campinas	64,662	2023Q4	Local Center
Santa Isabel	São Paulo	53,174	2023Q4	Local Center
Capão Bonito	Itapeva	46,337	2023Q4	Local Center
Ibaté	São Carlos	32,178	2023Q4	Local Center

Notes: Population according to the 2022 Demographic Census (IBGE). TZ cohort indicates the quarter in which fare-free transit was implemented. REGIC = Region of Influence of Cities (IBGE, 2018), indicating the municipality's urban hierarchy. São Caetano do Sul is part of the São Paulo Metropolitan Region (ABCD) but operates a municipal bus network independent of the SPTrans system. Monte Mor is part of the Campinas Metropolitan Region. The remaining municipalities do not belong to formal metropolitan regions. Three municipalities were excluded from the analysis sample: Santo Antônio de Posse (TZ since July 2021), Tambaú, and Taquarituba (TZ in 2022, with no bus-stop data available).

First, in smaller urban contexts, each additional user represents a proportionally larger increment in the occupancy of the spaces around the stops. This occurs because the marginal gain in surveillance decreases with the initial level of foot traffic. One person added to a stop with ten users per hour changes the perception of surveillance far more intensely than when added to a stop with a hundred. TZ, by substantially expanding demand from a lower initial level, operates exactly at the margin of highest return for the *eyes on the street* mechanism. This argument connects directly to the threshold model of Díaz et al. (2026b), according to which municipalities starting from low volume remain, after TZ, in the regime where each additional passenger contributes to surveillance. Despite the expected increase in demand, the absolute passenger volume in municipalities of 30,000 to 166,000 inhabitants remains well below the saturation levels observed in the dense urban centers where Díaz et al. (2026b) identify the opportunity regime. The risk of crossing the threshold is therefore low in this context.

Second, the concentration of crime in smaller cities is proportionally more intense relative to the number of available micro-places. Gill et al. (2017) document that, in American suburbs and medium-sized cities (average population of 85,000 inhabitants), about 2% of street segments concentrate 50% of crimes. This proportion is even more extreme than the 4 to 6% documented in metropolises by Weisburd (2015). Chainey et al. (2019) confirm this pattern in 42 Latin American cities, documenting that 2.1 to 3.5% of street segments account for 50% of crimes, with the concentration most pronounced in smaller cities. Thus, the São Paulo cities in our panel fit the profile in which intervention at a small number of transport nodes can affect a disproportionately large share of total crime. Bus stops appear recurrently among the high-concentration micro-places in these contexts.

Third, in communities with a more restricted set of public-transit users, the *familiar strangers* mechanisms operate with greater frequency and intensity. This occurs because the potential number of repeated co-presences is greater for any pair of individuals. TZ, by expanding the number of regular users in municipalities where the habitual base was previously more restricted, densifies this network of mutual recognition. This process raises the surveillance value of each additional passenger.

Taken together, these three arguments imply that the effect of TZ on crime should be more readily observable in the context studied. However, the mechanisms are generalizable. They operate in any context in which public transit expands the routine presence of people in previously underused spaces, including in neighborhoods of larger cities with low foot traffic at certain hours or on specific lines.

3 DATA

3.1 DATA SOURCES AND PANEL CONSTRUCTION

Crime data come from the São Paulo State Public Safety Secretariat (SSP-SP), which publicly releases georeferenced police incident reports with the coordinates of the reported crime location. The sample covers 18 quarters, from 2022Q1 to 2026Q2, ensuring at least four pre-treatment and nine post-treatment quarters for each Fare-Free transit adoption cohort ($K_{pre} = 4$, $K_{pos} = 9$).

The primary variable of interest is street theft, defined as theft without violence committed in public thoroughfares, including pickpocketing, bag snatching, and similar takings in open spaces. In addition, three complementary indicators were constructed, including street robbery, vehicle theft, and vehicle robbery. A composite indicator broadens the scope of the analysis, the aggregate crime indicator being the sum of the six types of incident recorded by SSP-SP as relevant to this study, including street theft and robbery, vehicle theft and robbery, as well as intentional bodily injury and other lower-frequency crimes in the panel.

The coordinates of the bus stops are obtained from *OpenStreetMap* (OSM) via the *Overpass* API for 174 municipalities, totaling 9,398 stops, with two municipalities lacking adequate OSM coverage (Piedade and Monte Mor) receiving stops from the GTFS published by ARTESP. As a result, the final set has 9,522 stops in 176 municipalities, all reprojected to SIRGAS 2000 / UTM Zone 23S (EPSG:31983) for computing metric distances. The Fare-Free transit municipalities were identified from Santini (2024), with start dates verified individually against municipal decrees and local press records, and the eleven 2023 adopters form four staggered adoption cohorts, as in Table 1. However, Tambaú and Taquarituba, which adopted the policy in 2022, do not have bus-stop data registered in the consulted sources, so they fall outside the set of stops and the panel. In parallel, Santo Antônio de Posse operated a first Fare-Free transit experience from July 2021, later interrupted, re-adopting the policy in February 2024. For this reason, the municipality is treated as a late adopter and excluded together with the post-2023 adoptions, as detailed below.

The unit of analysis is the H3 hexagon at resolution 9, with an approximate diameter of 174 meters and an area of ~0.105 km². This scale is coherent with the mechanism studied, since Fare-Free transit operates through informal surveillance at the neighborhood level, and not only on the street segment immediately adjacent to the stop, with cells of ~6 ha revealing clear patterns of theft and robbery without excessively diluting hot spots (Malleson et al., 2019). Moreover, resolution 9 is the one adopted by Carvalho and Guerra (2025) in a quasi-experimental study of crime in Fortaleza, so that the choice is comparable to the Brazilian causal-crime literature. However, street segments, the reference unit in geospatial crime studies (Weisburd, 2015), would be heterogeneous in size across the 176 municipalities of the panel and produce an excess of zeros above 89% in the sample cells, making identification unfeasible at that scale.

For each hexagon i, the Euclidean distance to the nearest stop is computed, classifying it into a treatment zone (*NearStop*, $d \leq 200$ *m*), a transition zone (*Donut*, $200 < d \leq 600$ *m*), and a control zone (*Far*, $d > 600$ *m*). This 200 m threshold corresponds to the pedestrian catchment radius documented in the literature, since Gerell (2018) and Ariel and Partridge (2017) operationalize zones of 150 m to 200 m in interventions on bus stops in Malmö and London, respectively, a context directly analogous to this paper's. Convergently, the buffer gradient estimated in this paper shows a stable effect between 100 m and 300 m and attenuation from 400 m onward, supporting pedestrian catchment within the immediate radius.

The panel therefore covers the 176 municipalities in the state of São Paulo with at least one registered bus stop and a population below 393,000 inhabitants. The panel contains approximately 39,000 hexagons and 18 quarterly observations, totaling about 707,000 hexagon-quarter observations. In addition, the four municipalities that adopted Fare-Free transit after 2023 (Santo Antônio de Posse in February 2024; Guararema, Ilha Comprida, and Santa Cruz do Rio Pardo in 2025) were excluded entirely from the panel. This exclusion follows from their adoption dates, which would make them appear as *never-treated* within the $K_{\text{pos}} = 9$ window of the 2023 cohorts, contaminating the control group. In this sense, the analysis sample is reduced to 172 municipalities, with 11 treated and 161 never-treated controls.

4 IDENTIFICATION STRATEGY

4.1 DESIGN, SPECIFICATION, AND ESTIMATOR

Fare-Free transit policy was adopted in a staggered manner by eleven São Paulo municipalities in four quarterly cohorts over 2023, generating two simultaneous dimensions of variation that underpin the quasi-experimental design. The first is temporal and between municipalities, since the adopters are compared with the 161 never-treated municipalities before and after policy adoption. The second is spatial and within the municipality, with hexagons near the stops (*NearStop,* $d \leq 200$ *m*) compared to distant hexagons (*Far,* $d > 600$ *m*) in the same municipality and period.

The combination of the three dimensions (temporal, geographic, and spatial) configures a *Triple Differences* (DDD) design, whose logical role was formalized by Gruber (1994). In this structure, the third difference, which is the within-municipality spatial variation, cancels any uniform municipal shock, such as policing changes, economic fluctuations, and seasonality, before comparing treated and control municipalities. In this way, the parallel-trends assumption becomes weaker than in pure DiD, as detailed in Section 5.2. Analogously, Chang and Jacobson (2017) apply a structurally analogous design with buffer zones around cannabis dispensaries, constituting the closest published analog to this paper.

The assignment of hexagons to the *NearStop* and *Far* zones is purely geographic, determined by the location of the bus stops and fixed before any TZ adoption decision. Within the same municipality and the same quarter, near and distant hexagons share the same fiscal, policing, and economic regime, so that the post-TZ differential between these cells is attributable to the increase in the flow of people around the stops, not to parallel shocks. The change in passenger flow occurs only when they effectively stop paying the fare, so that changes prior to formal adoption are unlikely in this context.

The main specification is a Poisson QMLE Stacked DDD. In the stacked design, each hexagon-quarter observation is also indexed by the subsample s of the cohort g to which it belongs, so that for hexagon i in municipality m, subsample s, cohort g, and period t, we estimate:

$$E[Y_{igst} \mid X_{igst}] = \exp\{\beta(NearStop_i \times D_{gt}) + \delta_{is} + \gamma_{mts} + \lambda_{zts}\}, \tag{1}$$

where Y_{igst} is the count of criminal incidents, X_{igst} is the full vector of regressors, $NearStop_i \in \{0,1\}$ indicates a hexagon ≤ 200 m from a stop, D_{gt} is the post-treatment indicator for cohort g in period t, δ_{is} is the hexagon × subsample fixed effect, γ_{mts} is the subsample × municipality × quarter fixed effect, and λ_{zts} is the zone (NearStop) × quarter × subsample fixed effect.

The fixed effect δ_{is} absorbs all static cell heterogeneity within each subsample, including location, land-use density, and permanent exposure to pedestrian traffic. The fixed effect γ_{mts}, in turn, absorbs every shock that affects the municipality uniformly in a given quarter within each subsample, including economic fluctuations, policing changes, and seasonality. In a complementary way, the zone × quarter fixed effect absorbs trends common to all zones near the stops in the same quarter, so that state-level shocks unrelated to the policy do not contaminate the estimate. As a result, $\hat{\beta}$ is identified by the triple difference between hexagons near and far from the stops, between treated and control municipalities, and between the periods before and after adoption. Thus, the coefficient $\hat{\beta}$ is interpreted as the average proportional effect via $\exp(\hat{\beta}) - 1$, expressing the percentage change in the expected count of incidents in the *NearStop* cells after policy adoption. In addition, standard errors are clustered by municipality ($G = 172$), following the unit of treatment variation.

Having defined the identification design, it remains to justify the multiplicative functional form adopted in Equation (1). Georeferenced crime data at the hexagon level exhibit two characteristics that make linear specifications inadequate as the main estimator. The first is the excess of zeros, since most hexagon-quarter cells record no crime, so that the conditional distribution of Y_{igst} is strongly skewed. The second is the conditional variance increasing with the mean, since in count data cells with greater exposure to crime record more incidents, with dispersion growing proportionally. A hexagon averaging ten thefts per quarter fluctuates far more than one averaging one, so that the linear model, by treating this variation as constant across all levels, produces inefficient estimates. Santos Silva and Tenreyro (2006) show, precisely, that the $\log(1 + Y)$ transformation, usually applied to linearize count data, produces inconsistent estimates under these conditions, since it compresses the variance of zeros and attenuates the signal in low-count cells, generating systematic compression bias.

The Poisson QMLE solves both problems by directly estimating the multiplicative model of the conditional mean, without imposing a parametric distribution on the data. The consistency of the estimator is guaranteed solely by the correct specification of $E[Y \mid X] = \exp(X'\beta)$, regardless of whether the data follow a Poisson distribution (Santos Silva and Tenreyro, 2006). Wooldridge (2023), in turn, formalizes the compatibility of Poisson QMLE with count panels under staggered adoption, showing that the estimator produces interpretable ATTs in the *Stacked DDD* context and is robust to treatment-effect heterogeneity across cohorts. Analogously, Carvalho and Guerra (2025) adopt the Poisson QMLE in a quasi-experimental study of crime in Fortaleza with the same spatial unit (H3 resolution-9 hexagons), giving our design a methodological anchor in the Brazilian causal-crime literature. The linear specification with $\log(1 + Y_{igst})$ is retained in the robustness section as a functional-form check, with the comparison between the two estimators allowing us to quantify the magnitude of the compression bias in this paper's data.

4.2 *STACKED DDD*, COMPETING ESTIMATORS, AND PARALLEL TRENDS

Under staggered adoption with treatment effects that are heterogeneous across cohorts, the two-way fixed effects (TWFE) estimator mixes valid comparisons (treated versus never-treated) with potentially biased comparisons, in which late cohorts are compared to early cohorts already treated, which act as contaminated controls. Goodman-Bacon (2021) decomposes TWFE into 2 × 2 comparisons and shows that negative weights emerge when treatment effects vary across cohorts, making the estimator a weighted average that may contradict the individual treatment effects. In a complementary way, Roth (2022) synthesizes the subsequent literature and recommends estimators robust to effect heterogeneity whenever adoption is staggered.

The Goodman-Bacon (2021) decomposition applied to this paper's data confirms that 99.2% of the TWFE weight comes from treated-versus-never-treated comparisons, with 0% negative weights. However, the *Stacked DDD* is adopted as the main estimator because it is more rigorous by construction, since, for each cohort g, a subsample is assembled containing only cohort g and the never-treated municipalities, with an event window $[-K_{\text{pre}}, K_{\text{pos}}]$ and $K_{\text{pos}} = 9$ post-treatment quarters (Baker et al., 2022). Hence, the aggregate ATT is the weighted average of the cohort-specific ATTs estimated within each clean subsample, eliminating the possibility of cross-cohort contamination even if effect heterogeneity exists.

In the context of this paper's *Triple* DiD, the *Stacked DiD* acquires a more precise interpretation. Hsieh (2026) extends the result of Baker et al. (2022) to *Triple Differences* designs with staggered adoption, showing that the *three-way fixed effects* (*3WFEDDD*) inherits, in amplified form, the same contamination problems of TWFE identified by Goodman-Bacon (2021). Similarly, Strezhnev (2023) formalizes this problem and recommends stacking or imputation approaches. The *Stacked DDD* of Hsieh (2026) resolves the issue by stacking clean cohort-specific 2 × 2 × 2 subsamples, each containing cohort g, the never-treated municipalities, and the within-municipality *NearStop* × *Far* spatial variation preserved, which is exactly the structure of our estimator. Our

Stacked DiD is therefore a *Stacked DDD* in the sense of Hsieh (2026), with the third spatial dimension operating naturally within each subsample.

Callaway and Sant'Anna (2021) propose a semi-parametric estimator that by construction avoids cross-cohort comparisons under staggered adoption, estimating group-period ATTs $ATT(g,t)$ and aggregating them with explicit weights. Nevertheless, the central argument for preferring the *Stacked DDD* in this paper is not one of asymptotic efficiency but of compatibility with the *Triple* DiD. The treatment groups in Callaway and Sant'Anna (2021) are defined at the municipality level, and the spatial dimension *NearStop* × *Far* has no natural mechanism within the CS framework to be incorporated as a source of identification. Moreover, unlike clustering over the full panel ($G = 172$), the standard bootstrap of the CS estimator resamples only the $G_{\text{treated}} = 11$ treated municipalities, which can substantially inflate the standard errors (MacKinnon and Webb, 2020) and reduce the power of the test. Accordingly, the *Stacked DDD* incorporates the third dimension directly and transparently, by stacking complete municipal subsamples with the within-municipality spatial variation preserved and clustering at the municipality over the full panel. Thus, the choice reflects the *Triple* DiD nature of the design and preserves the precision of the estimates, with the Callaway and Sant'Anna (2021) estimator retained as a robustness check in Section 5.

In the context of this paper's *Triple* DiD, the required parallel-trends assumption is that the *NearStop* − *Far* differential within the municipality evolves in parallel between treated and control municipalities in the absence of treatment. In the Poisson QMLE estimator, this assumption is formulated on the multiplicative scale of the index, so that the ratio $E[Y_{it}(0) \mid NearStop]/E[Y_{it}(0) \mid Far]$ evolves identically between treated and controls (Wooldridge, 2023), with violations on the additive scale not invalidating identification if proportionality holds. This requirement is strictly weaker than the pure-DiD assumption, since the latter would require identical crime trajectories between adopting and non-adopting municipalities, groups that differ in the fiscal and political characteristics that motivated TZ adoption. The *Triple* DiD assumption requires only that, within a municipality that would come to adopt TZ, the difference between hexagons near and far from the stops was not on a trajectory diverging from the corresponding difference in the never-treated municipalities.

This assumption is plausible given the nature of the variation exploited, since the geography of the bus stops was fixed before any TZ adoption decision and is independent of municipal crime trends, with no reason for hexagons near the stops to experience crime trends systematically distinct from those of distant hexagons in the same municipality for reasons unrelated to passenger flow. Moreover, fixed differences between zones are absorbed by the hexagon fixed effect δis, so that only trend divergences, and not level differences, violate the assumption. Section 5 presents the empirical evidence through the pre-period event study.

5 RESULTS

5.1 MAIN RESULT

Fare-Free transit adoption reduced street theft in the hexagons within 200 m of the bus stops by 24.0%, on average, in the quarters following implementation. Table 3 presents the average treatment effects on the treated (ATT) estimated by the main model, the Poisson QMLE Stacked DDD (Baker et al., 2022; Wooldridge, 2023), in the panel of 172 municipalities. The effects are estimated on hexagons in the treatment zone (*NearStop*, ≤ 200 m) of the adopting municipalities, compared with hexagons in the control zone
(> 600 m) before and after policy adoption.

Table 2 – Effect of Fare-Free Transit on Crime around the Stops

	$\hat{\beta}$	SE	%	Sig.	*N*
Panel A — Simple crimes					
Street theft	-0.274	0.093	-24.0%	***	1,535,860
Street robbery	0.027	0.120	2.8%	ns	736,002

Vehicle theft	-0.195	0.119	-17.7%	ns	652,853
Vehicle robbery	-0.357	0.202	-30.0%	*	429,001
Panel B — Aggregate indicator					
Total crime	-0.175	0.111	-16.1%	ns	1,715,040

*Notes: sample of 172 SP municipalities, H3 res-9 hexagon (~174m), Poisson QMLE Stacked DDD (K_pos=9), never-treated control, Monte Mor censoring from 2025Q4, standard errors clustered by municipality (G = 172). Panel A reports the four simple property crimes; Panel B reports the aggregate indicator, the sum of the SSP-SP types included in the panel. Percentage change = exp(β) − 1. *** p < 0.01, ** p < 0.05, * p < 0.10. Source: Authors' elaboration.*

Panel A disaggregates the results by type of offense to isolate the mechanism under analysis. As the main outcome, street theft shows an estimated reduction of 24.0%. However, for street robbery, an offense with direct confrontation between offender and victim, the estimated reduction is close to zero and not significant. In this context, this contrast is consistent with the *eyes on the street* mechanism (Jacobs, 1961), since the passive surveillance of passersby inhibits the opportunistic crime that depends on the absence of witnesses. In parallel, confrontational offenses do not depend on that absence, since their planning incorporates factors beyond the perception of passive observers (Cohen and Felson, 1979; Weisburd, 2015).

Vehicle crimes, still in Panel A, are treated as secondary results, since their identification status differs from that of the main outcome. For vehicle theft, the parallel-trends test reveals a positive and significant pre-trend, a pattern indicative of mean reversion, so that the pooled estimate captures the normalization of an elevated pre-treatment period. This violation prevents a causal interpretation, with the result reported only as descriptive and excluded from the main narrative. On the other hand, vehicle robbery is defensible as an exploratory result, since its pre-trend is individually not significant and the randomization-inference placebo supports it. However, its decline concentrates at night and on weekends, a pattern that points to a mechanism distinct from commuting flow, so that the offense is explored as mechanism evidence in Section 5.2, and not as a main outcome. Both decisions are conservative, since they avoid attributing to the policy effects whose identification is more fragile.

The aggregate crime indicator, in Panel B, falls 16.1%, a non-significant magnitude. This attenuation is expected, since the aggregate incorporates street robbery, whose response is null, and carries a larger standard error than that of the simple crimes. In this way, the aggregate points in the same direction as the effect, without the precision for significance, so that identification rests on the simple crimes, in particular on street theft.

5.2 TEMPORAL HETEROGENEITY

The informal-surveillance mechanism predicts that the effect of Fare-Free transit concentrates in the periods when the policy adds users to previously underused spaces. During weekday peak hours, the stops already recorded a regular flow of workers before the policy, so that eliminating the fare adds relatively few users in that period. Off-peak, by contrast, the stops were emptier, and these are the moments of greatest vulnerability to opportunistic crime (Ceccato and Uittenbogaard, 2014; Zahnow and Corcoran, 2021). In this way, the prediction is directional, with a more pronounced effect in periods of prior underoccupancy and an attenuated one during already busy hours. Thus, each temporal cut re-estimates Equation (1), replacing the total count Y_{igst} with the count of incidents in the corresponding cut, whether by day of week (weekday, Saturday, and Sunday) or by time of day (peak and off-peak), keeping the three fixed effects and the municipality clustering.

The decomposition by time of day within weekdays, in Table 5, reinforces this reading. Off-peak, the reduction in theft is more intense and statistically significant, whereas at peak the magnitude is close but estimated with less precision. In this way, the evidence by time of day and by day of week converges, pointing to the marginal flow of pedestrians in periods of underoccupancy as the margin on which the policy acts.

Table 3 – Heterogeneity of the Effect by Time of Day, Weekdays

Period	Street theft	Vehicle robbery
Aggregate (with hour)	-0.268** (-23.5%)	-0.393* (-32.5%)
Weekday, peak	-0.287 (-25.0%)	0.401 (49.3%)
Weekday, off-peak	-0.339** (-28.8%)	-0.509* (-39.9%)

Notes: sample of 172 SP municipalities, Poisson QMLE Stacked DDD, Monte Mor censoring, standard errors clustered by municipality. Estimates obtained on the panel reconstructed from SSP-SP microdata that preserves the incident hour, discarding the ~22% of thefts without a determinable hour, a limitation inherent to the record. Therefore, the aggregate row differs marginally from the ATT of the main panel (street theft −0.274; vehicle robbery −0.357). Weekday peak = 7–9 a.m. and 5–7 p.m.; weekday off-peak = other weekday hours. Vehicle robbery is reported as a secondary exploratory result. *** p < 0.01, ** p < 0.05, * p < 0.10. Source: Authors' elaboration.

This temporal gradient helps to rule out alternatives to the surveillance channel. If the effect operated through an income channel, with resources freed after the fare exemption, the impact should appear proportionally to the volume of trips, and not concentrated off-peak. Moreover, if it operated through changes in policing, there would be no reason for the response to depend so markedly on the time of day. Thus, the concentration of the effect precisely in the periods of underoccupancy prior to Fare-Free transit points to the marginal flow of pedestrians as the active mechanism.

5.3 ABSENCE OF SPATIAL DISPLACEMENT

An alternative hypothesis to a genuine causal effect is spatial displacement, since Fare-Free transit could simply redistribute crime from the hexagons near the stops to more distant areas, without a net reduction in municipal crime. The analysis of the transition zone rejects this interpretation. Formally, the effect in the intermediate ring is estimated by the counterpart of Equation (1),

$$E[Y_{igst} \mid \mathbf{X}_{igst}] = \exp \beta_{\text{donut}} \cdot \text{Donut}_i \times D_{gt} + \delta_{is} + \gamma_{mts}), \tag{2}$$

where $Donut_i \in \{0,1\}$ indicates a hexagon in the 200-to-600 m ring of the nearest stop, with the equation estimated on the sample that excludes the *NearStop* zone, so that the comparison is between the ring and the *Far* zone (> 600 m).

Table 4 – Absence of Spatial Displacement: Effect in the Transition Zone (200–600 m)

	$\hat{\beta}donut$	SE	%	Sig.
Street theft	-0.220	0.061	-19.7%	***
Total crime	-0.190	0.062	-17.3%	***

Notes: sample of 172 SP municipalities, Poisson QMLE Stacked DDD (K_{pos}=9), never-treated control (*Far* > 600m), Monte Mor censoring, clustering at the municipality. The *donut* zone (200–600m) is estimated relative to the *Far* hexagons (> 600m). A negative coefficient indicates a decline in crime also in the intermediate zone, ruling out spatial displacement. *** p < 0.01, ** p < 0.05, * p < 0.10. Source: Authors' elaboration.

Table 6 reports the effect on hexagons 200–600 m from the stops (*Donut*), compared with the control zone (> 600 m) within the same municipality. For street theft, the ATT of the *donut* zone is −19.7%, with the aggregate crime indicator also showing a significant reduction in the same band. Thus, if there were displacement, the coefficient of the transition zone would be positive, so that the negative sign indicates that the reduction is genuine and that the benefit propagates beyond the immediate vicinity of the stops. This pattern corresponds to the diffusion of benefits documented in the situational-prevention literature, and not to crime displacement (Guerette and Bowers, 2009).

5.4 MAGNITUDE OF THE AGGREGATE EFFECT

To translate the estimates into absolute terms, the number of street thefts prevented per year in the eleven treated municipalities was calculated. The procedure applies the estimated ATT to the average crime count per hexagon per quarter in the treatment zone during the pre-Fare-Free transit period, extrapolated over the total hexagons of the zone and over the four quarters of the year, as in Equation (3).$\bar{C}_{\text{near}}$

As a result, for street theft, the procedure yields approximately 513 thefts prevented per year in the treatment zone (≤ 200 m from the stops), as shown in Table 7. Moreover, Table 6 documents that the reduction extends to the transition zone, adding about 227 thefts prevented per year, so that the aggregate impact across the two zones totals approximately 740 street thefts prevented per year in the eleven treated municipalities.

The relative magnitude of this number requires a causally coherent reference, which is the counterfactual expectation $Y(0)$ of theft in the treatment zone in the absence of the policy. The zone within 600 m of the stops concentrates 76% of the street theft of the eleven treated municipalities, so that the surroundings of the stops are where this crime actually occurs. In that zone, the 740 prevented thefts amount to about 22.8% of the counterfactual expectation of 3,246 thefts per year, that is, the policy eliminates nearly a quarter of the thefts that would occur there without Fare-Free transit. In this direction, taking as reference the total theft of the eleven treated municipalities, across all zones, the reduction corresponds to about 17.7%. In a complementary way, the gross decline observed in the annual levels of the zone is 762 thefts, a value that practically coincides with the causal estimate, validating the calculation through an independent route.

Table 5 – Crimes Prevented per Year

Outcome	*NearStop* (≤200m)	*Donut* (200–600m)	Total
Total crime	469	256	724
Theft (passerby)	513	227	740

Notes: calculation from the Poisson QMLE ATT by outcome: $N_{\text{zona}} \times \bar{C}_{\text{zona}} \times [1 - \exp(\hat{\beta})] \times 4$, where N_{zona} is the total hexagons of the zone in the 11 treated municipalities, $\bar{C}_{\text{zona}}$ is the average number of incidents per hexagon per pre-TZ quarter and $1 - \exp(\hat{\beta})$ is the proportional reduction. *NearStop* zone ≤ 200 m; *Donut* zone 200–600 m. Source: Authors' elaboration.

6 CONCLUSION

This paper presents the first causal evidence of the effect of Fare-Free transit on crime, exploiting the staggered adoption of the policy by eleven São Paulo municipalities in 2023. Estimation by Poisson QMLE in a *Triple Difference Stacked* design shows that street theft falls 24.0% in the hexagons near the bus stops after the fare exemption. Street robbery, a confrontational offense that does not depend on the absence of witnesses, does not respond to the policy, a contrast consistent with the *eyes on the street* mechanism that rules out non-specific channels, such as policing or income, which would reduce both offenses. In this way, the differential pattern between theft and robbery is not a limitation of the result but the testable prediction of the diffuse-surveillance channel that Fare-Free transit activates around the stops.

The effect is strong on weekdays and estimated with greater precision off-peak, precisely the periods in which the policy adds flow to previously underused spaces. At peak hours the magnitude is close but less precise, with the difference in significance between periods reflecting above all statistical power, as discussed in the results section. This temporal gradient reinforces the reading that the policy acts at the margin of informal surveillance, adding people precisely when the space used to remain idle. The decline is not limited to the immediate vicinity, since the *donut* zone of 200 to 600 meters also recedes 19.7%, which rules out the spatial-displacement hypothesis and indicates a genuine reduction in the municipality's crime. Aggregating the two zones, Fare-Free transit adoption prevented approximately 740 street thefts per year, which amounts to about a quarter of the thefts that would occur around the stops in the absence of the policy.

Crime reduction is only one of the benefit channels of Fare-Free transit, so that the estimate produced here represents a lower bound on the gains of the policy. The gains in mobility, access to employment, emissions, and health remain unquantified in this paper. The contribution is therefore to quantify the crime channel with causal rigor, a result relevant to the debate over expanding Fare-Free transit in Brazil, whose adoption is growing among small and medium-sized municipalities.

Some limitations delimit the reach of the results and point to the future agenda. External validity is restricted to smaller municipalities, so that extrapolation to large metropolises, with distinct flow and crime dynamics, requires caution. Moreover, the SSP-SP administrative records are subject to possible underreporting, although there is no reason for it to vary differentially between zones near and far from the stops. Vehicle crimes, by violating parallel trends, were treated only as descriptive evidence and do not support a causal interpretation. Future research may investigate the heterogeneity of the effect across different types of municipality and the persistence of the response over longer horizons, as new adoption cohorts accumulate post-treatment quarters.

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