Reclaiming Neighborhoods: Violent Crime and Business Growth*

Beatriz Ribeiro†
London School of Economics

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Abstract

One in four firms in Latin America identifies crime as a major constraint on its operations. Can reducing violence help businesses grow? I study Rio de Janeiro's Pacifying Police Units, one of the largest policing interventions in the developing world, which sharply reduced violence in some of the city's most violent neighborhoods. Linking its staggered rollout to geocoded administrative records for formal establishments and workers, I compare firms in earlier- and later-treated areas. Four years after treatment, incumbent firms employed 24 percent more workers and paid 26 percent higher wage bills. Gains were concentrated among small firms, and firm entry also rose. Customer-facing firms showed no clear advantage, which argues against customer inflows as the main explanation. Among small incumbents, higher-paying firms expanded more, new hires were more educated and experienced, and wages within firms showed no detectable increase. Under benchmark assumptions, a model of local labour markets attributes most of the small-firm employment response to improved access to workers, with a smaller contribution from reduced crime-related production costs. These findings suggest that public security interventions can help small firms grow, particularly by widening the pool of workers willing to work in previously unsafe areas.

Keywords: Violent Crime, Firm Growth, Labor Supply, Policing
JEL Classification: K42, L25, O12, J23.

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†Department of Economics, London School of Economics: b.machado-ribeiro@lse.ac.uk.

1 Introduction

Violent crime is one of Latin America's most pressing problems: the region records over 100,000 homicides a year, nearly a third of the world's total, despite accounting for only 8 percent of the global population (Bisca et al., 2024). Insecurity ranks among citizens' top concerns in the region[1], and businesses also perceive it as a serious challenge. According to the World Bank Enterprise Survey, a quarter of firms in the region identify crime as a major constraint on their operations, and 9.2 percent name it as their single biggest obstacle, almost three times the share in high-income economies (World Bank, 2025).

Violent crime can constrain firms by raising operating costs, disrupting production, and lowering productivity. Yet evidence on how public security interventions affect firm growth remains scarce. At the local level, even the direction of the effect is ambiguous. A safer neighbourhood may attract workers and customers from elsewhere in the city, but it may also make residents more willing to travel outside the area for work and shopping, shifting labour supply and demand away from local firms. Understanding these responses helps establish whether costly security interventions generate local benefits beyond improvements in public safety and, more broadly, how violence constrains business activity and economic opportunity.

This paper studies how firms respond to improved security in the context of one of the largest policing interventions in the developing world: Rio de Janeiro's Pacification Policy, implemented in some of the city's most violent neighborhoods between 2008 and 2014. Using establishment-level geocoded data and exploiting the policy's staggered rollout, I show that firms operating in these areas expanded employment and their wage bills after the intervention. In particular, small incumbent firms were able to hire more workers, including more skilled ones, without raising wages, suggesting an increase in labor supply contributed to these effects. A calibrated model of local labor markets supports this interpretation, with the implied labor-supply shift accounting for roughly three-quarters of the employment gains among small firms.

Rio de Janeiro, Brazil's second-largest city, has long faced high levels of violence. Over the three decades preceding the policy, drug-trafficking gangs expanded into its poorer communities, where competition for control of drug markets and territory, along with frequent clashes with police, contributed to rising violence (Arias, 2006; Lessing, 2017). To address these dynamics, the state government launched the Pacifying Police Units (UPP)

[1] In the 2024 Latinobarómetro survey, security-related problems were the second most cited national problem (22 percent), after economic problems (42 percent). Crime alone accounted for 19 percent, up from 5 percent in 1995, with a peak of 28 percent in 2011 (Latinobarómetro, 2024).

program in 2008, establishing new police stations inside gang-controlled communities and staffing them with newly recruited officers trained in community-oriented policing. By 2014, the program had expanded to cover 39 areas and over 800,000 residents, more than 10 percent of Rio's population. Its rollout coincided with the city's preparations to host several matches during Brazil's 2014 FIFA World Cup and the 2016 Summer Olympics, raising the stakes of visible security gains. Existing work documents substantial declines in violence in treated areas (Bellégo and Drouard, 2024; Ferraz et al., 2023) and a weakening of criminal groups (Magaloni et al., 2020; Cano and Ribeiro, 2016). These localized reductions in violence create a unique opportunity to study how firms respond to changes in security in their immediate surroundings.

A central challenge in studying this question is establishing a causal link between violent crime and firm outcomes. Neighborhoods with high levels of violent crime differ from other parts of the city in economic conditions that also shape business activity. Changes in those conditions can affect both violence and firms, making it difficult to isolate the effect of crime. I address this challenge by exploiting the spatial and temporal variation created by the policy's staggered rollout across the city, comparing earlier-treated areas with areas that had not yet been treated but would receive a UPP later, using an event-study approach. Consistent with the parallel-trends assumption underlying this comparison, changes in key firm and crime outcomes do not predict treatment timing, and the event studies show no systematic pre-treatment trends. I also find no evidence that the program displaced violence into areas awaiting treatment.

A second challenge is measuring exposure: this requires precise information on both where violence occurs and where firms are located. I address this by combining two detailed sources of information. First, I use geocoded information from the *Relação Anual de Informações Sociais* (RAIS), a matched employer–employee dataset covering all establishments employing formal workers in Brazil.[2] Second, I use official shapefiles marking the boundaries of the territories targeted by the Pacification Policy and records of their treatment dates. I use these data to identify an initial sample of 4,879 establishments[3] located across all areas targeted by the policy and follow their employment and wages before and after the policy's implementation in each area.

My results on crime corroborate prior evidence that the UPP program reduced local violence: six months after the policy, the estimated decline in the local homicide rate was

[2]Because RAIS comprises administrative records on the universe of establishments employing formal labor, I observe firms of all sizes and across all business sectors, a key advantage over surveys, which may cover only selected firm sizes or sectors.

[3]The sample contains 20,454 establishment-year observations from 2003 to 2013, an average of 1,859 active establishments per year.

equivalent to approximately 56 percent of the pre-treatment rate, and the effect persisted over time. In response to these changes, incumbent firms expanded employment and their wage bills. One year after treatment, the estimated effects are 0.03 and 0.04 log points for employment and the wage bill, respectively, and they grow substantially over time: four years later, they reach 0.22 and 0.23 log points. The effects are concentrated among firms with fewer than five employees at baseline, suggesting that larger firms may be better able to shield themselves from the costs of operating in high-crime areas. Importantly, the effects remain similar when restricting to firms observed every year between 2007 and 2013, and treatment does not significantly change the probability that a firm leaves the panel, making selective attrition an unlikely explanation.

Furthermore, these changes do not appear to be driven by other changes occurring alongside the policy. Adding separate year effects for each of Rio's five planning areas leaves the estimated employment and wage-bill gains positive and similar in magnitude, addressing broader shifts across different parts of the city. To account for the possibility that preparations for the World Cup and Olympics affected firms near the venues, I also allow trends to vary with different measures of distance to sporting venues. The gains persist and are not larger in areas closer to the venues. Finally, firm growth also does not appear to be purely driven by formalization of employment relationships: the share of new hires without a formal job in the previous year does not change significantly with treatment.

Beyond incumbent growth, I also document that firm entry rises in treated areas, with no clear increase in exits, reinforcing the idea that improved security generated a net benefit of operating in these areas. Aggregating employment and wage bills across all firms within each treated area yields positive but imprecisely estimated effects of 9.1 and 14.7 percent, respectively. These estimates suggest possible aggregate gains beyond a reallocation of activity among existing firms in treated communities.

I then proceed to examine the mechanisms underlying these effects. First, I consider the possibility that the observed gains were driven by safer conditions drawing new customers to shop in treated areas, increasing demand for local businesses, an effect previously documented in studies from the United States (Fe and Sanfelice, 2022; Greenbaum and Tita, 2004). If so, customer-facing firms, which depend on in-person patronage, should benefit more than other businesses. However, across several definitions of customer-facing businesses, I find no clear advantage in employment growth or entry. These results consistently offer little support for an inflow of customers as the main explanation.

Second, I investigate whether changes in the labour market are the key channel through which improved security benefits firms, drawing on the matched employer–employee

structure of the data. My empirical analysis reveals that, among small incumbents, higher-paying firms expand more and new hires become more educated and experienced, while wages within firms show no detectable increase. These patterns suggest that safer conditions may have widened the pool of workers willing to take jobs in treated areas, an interpretation consistent with evidence from the United States that more-educated households are more responsive to changes in crime when deciding whether to leave a city, and that skilled workers sort toward cities with improving amenities (Cullen and Levitt, 1999; Diamond, 2016).

Finally, I develop a model of local labor markets to formalize the mechanisms and quantify the relative contribution of each channel. In this model, improved security can shift demand for customer-facing firms and expand the pool of workers willing to take jobs in treated areas. Moreover, the model allows security to affect firms directly through a "crime tax" that captures disruptions and other production costs faced by firms operating in high-violence environments. I calibrate the model using my reduced-form estimates. Under the benchmark calibration, improved labour supply accounts for most of the employment growth among small incumbents, while relief from the crime tax corresponds to an approximately 2.2 percent productivity gain.

Overall, my findings suggest that reducing violent crime can generate improvements beyond its immediate benefits for public safety. My results also imply that better security may raise the returns to policies that help firms recruit and grow, such as investments in transport connections to these neighborhoods. Such investments can lower commuting costs but may have limited effects on hiring if workers still avoid unsafe areas. Crime reduction may therefore have important complementarities with other policies that support local economic development.

Related Literature. This paper is closely related to several strands of literature. Research on the economic consequences of criminal activity documents detrimental effects on human capital accumulation (Monteiro and Rocha, 2017; Koppensteiner and Menezes, 2021; Borker, 2021), labor market outcomes (Velásquez, 2020; Bindler and Ketel, 2022; Melnikov et al., 2025), and entrepreneurship and business performance (Rosenthal and Ross, 2010; Bernal et al., 2024; Rozo, 2018; Utar, 2024). A related literature examines how organized crime shapes economic development, revealing that criminal organizations in both developed and developing countries distort local economies through violence, extortion, and market control (Fenizia and Saggio, 2024; Slutzky and Zeume, 2024; Mirenda et al., 2022; Brown et al., 2024; Melnikov et al., 2025; Battiston et al., 2024; Melnikov et al., 2026).[4]

My setting and data allow me to extend the existing evidence on how violent crime affects

[4]For a recent review of the effects of organized crime, see Tobón et al. (2025).

firm growth in three ways. First, linking geocoded establishments to the boundaries and start dates of UPP areas lets me measure exposure at the neighbourhood level and compare areas pacified at different times within the same city, rather than relying on violence measured across municipalities or metropolitan labour markets, as in Rozo (2018) and Utar (2024), respectively. Second, I study a broader set of firms than these studies that focus only on manufacturing plants. My data include small service businesses, allowing me to explore the role of demand and labour-supply effects. Third, employer–employee links let me look inside firm growth: who firms hire, where new hires previously worked, and whether wage changes occur within firms or through workers sorting across them.

This paper is also connected to a broader literature examining barriers to firm growth in developing countries (Tybout, 2000). This work highlights, for instance, weak management practices (Bloom and Van Reenen, 2010; Bloom et al., 2010; Bloom et al., 2026), unreliable electricity (Fried and Lagakos, 2023), and limited access to skilled workers (Engbom et al., 2025). I contribute by documenting exposure to violent crime as another important local constraint, and providing evidence that it can limit small firms' growth by making it harder to hire workers.

Finally, this study extends the evidence on the effects of Rio de Janeiro's pacification policy. Much of this work examines the policy's effects on crime, policing, and criminal governance (Cano et al., 2012; Bellégo and Drouard, 2024; Ferraz et al., 2023; Magaloni et al., 2020). Other studies document gains in students' learning (Peçanha, 2024) and birth outcomes, accompanied by more prenatal visits (Lautharte, 2021), as well as increases in nearby residential property prices (Frischtak and Mandel, 2012). Manoel (2019) also finds that the policy increased the number of firms in pacified areas. I contribute to this literature by providing an assessment of the policy's economic impact, examining how improved security reshapes firm growth and local labour markets. By following individual establishments and their workers, I distinguish incumbent growth from entry and exit and identify the roles of hiring, worker composition, and wages in firms' responses.

This paper proceeds as follows. Section 2 describes the setting of Rio de Janeiro and explains in detail the pacification policy. Section 3 presents the data, and Section 4 outlines the empirical strategy based on the staggered rollout of UPPs. Section 5 presents the main results, Section 6 examines the mechanisms, and Section 7 develops the theoretical framework and quantification exercises. Section 8 concludes.

2 Setting

This section describes the setting of this study: high-crime areas in Rio de Janeiro and how violence affects the lives of residents, workers, and firms. It then introduces the Pacifying Police Units (UPP) program. Its staggered rollout provides the local variation in violence used to isolate the effect of violent crime.

2.1 Violence in Rio de Janeiro

Rio de Janeiro is home to about 6.7 million people and generates roughly 4 percent of national GDP (IBGE, 2024; IBGE, 2023). Despite its economic importance, the city has long faced high levels of violence, with recent estimates showing a homicide rate of 20.4 per 100,000 residents (IPEA and FBSP, 2025), just above the regional rate for Latin America of 19.7 and nearly seven times the average rate among OECD countries (OECD, 2021; United Nations, 2025).

The city's high levels of violence have been linked to the presence of armed criminal groups (Arias, 2006; Barcellos and Zaluar, 2014). Following the cocaine boom of the 1980s, drug trafficking gangs emerged within the prison system and gradually expanded their presence across the city's poorer communities, or *favelas*.[5] These areas offered favorable conditions for drug gangs to establish a foothold: limited state presence, proximity to wealthier consumer markets, and geography that made them difficult for police to access and easy to defend. Drug-trafficking revenues enabled gangs to acquire weapons and ammunition, helping them enforce territorial control through armed violence (Barnes, 2022).

As gangs expanded territorial control, competition over drug markets and territory between different groups led to an escalation of violence across the city (Lessing, 2017). Over time, three major drug-trafficking groups emerged and consolidated control over *favelas*: Comando Vermelho (CV), Amigos dos Amigos (ADA), and Terceiro Comando Puro (TCP). Other parts of the city came under the control of militias. Unlike the trafficking groups, militias in this period primarily charged for protection and controlled local services rather than relying on retail drug sales, resembling the protection rackets of Italian mafia organizations. By 2010, drug gangs and militias controlled most *favelas*, and neutral areas were rare (Barcellos and Zaluar, 2014; Pope, 2023).

Within controlled territories, drug gangs exercise authority through both economic activity

[5] *Favelas* are informal urban settlements, often characterized by irregular land occupation or street layouts and limited access to basic public services (IBGE, 2011). In Rio, many occupy difficult terrain, including steep hillsides (Terry et al., 2013).

and coercion. Their core business focuses on retail drug sales, but reports also document gangs engaging in extortion and land exploitation practices on a smaller scale (Monteiro et al., 2022). Beyond these activities, gangs shape everyday life in *favelas* by regulating circulation, imposing checkpoints and curfews, and using violence to enforce territorial control. These practices also aim to discourage residents from sharing information with police, including about gang members' whereabouts (Barnes, 2022).

This dynamic has contributed to the widespread fear of violence around the city. In a 2018 survey of Rio de Janeiro residents, 92 percent feared that they or a relative would be hit by a stray bullet, and 87 percent feared being murdered (FBSP, 2018). Such fear is likely to shape where people choose to go. Nationally, more than half of Brazilians reported feeling unsafe walking alone at night. In addition, 57 percent avoided leaving or arriving home late, and half avoided places with few people around (IBGE, 2022). For local firms, these mobility concerns may affect both customer demand and the supply of workers willing to take jobs in areas exposed to violence.

2.2 The Pacification Policy

To address these dynamics, the state of Rio de Janeiro launched the Pacifying Police Units (UPP) program in 2008, introducing a permanent police presence inside gang-controlled communities with the aim of weakening armed groups and reducing violence. Before the program, police responses to organized crime largely relied on sporadic raids, which frequently led to violent confrontations and contributed to the high levels of violence in these communities. In 2007, police killed a then-record 1,330 people across Rio de Janeiro state (Human Rights Watch, 2009). The UPP program marked a major shift from sporadic raids toward sustained police deployment inside *favelas*.

The policy implementation consisted of two stages. First, coordinated operations, often supported by military forces, removed armed criminal groups from targeted communities, allowing police forces to enter these areas. These operations were followed by the establishment of new permanent police stations (UPPs) within these territories, usually within a year of the initial police deployment. Officers assigned to UPPs were newly recruited and trained in community-oriented policing, reflecting the program's emphasis on restoring state authority and sustaining reductions in criminal activity. Between 2007 and 2014, employment at the Rio de Janeiro Military Police grew by nearly 9,000, an increase of 22 percent.[6]

[6]Author's calculations from RAIS: employment at the same Military Police employer rose from 38,954 in 2007 to 47,627 in 2014, a net gain of 8,673. This is total employer growth, not the number of officers assigned

The first UPP station opened in the small community of Santa Marta in December 2008. The Rio de Janeiro state government formally established the UPP framework in January 2009, and extended it to the communities of Cidade de Deus and Jardim Batam the following month.[7] Between 2009 and 2014, the program gradually intervened in an additional 36 areas, covering a total of over 800,000 residents, with thirty-eight new police stations installed. Over this period, the program reached roughly one area every two months.[8] Figure 2 summarizes the rollout over time. The largest expansions occurred in 2010 and 2011, when 13 and seven new areas were treated, respectively.

The policy was rolled out as Rio de Janeiro prepared to host matches during the 2014 FIFA World Cup and the 2016 Summer Olympics. The placement of UPPs eventually reflected these priorities: targeted communities were concentrated in the eastern part of the city, closer to tourist areas and major transport routes. Figure 1 shows the spatial distribution of the treated areas and the locations of sporting venues and the city's two airports. But these events alone did not determine which communities the program targeted: it focused primarily on those controlled by drug gangs, particularly those with a history of violent confrontation with police (Magaloni et al., 2020).

The pacification policy was associated with significant reductions in violence and criminal activity. Previous work has analyzed the evolution of crime indicators, finding significant declines in the incidence of violent crimes such as robberies and homicides in treated communities (Bellégo and Drouard, 2024; Ferraz et al., 2023; Magaloni et al., 2020). Importantly, survey evidence also indicates that the program reduced the visible presence of criminal groups. For example, data collected in early-treated UPP areas indicate that roughly 80% of residents reported seeing fewer individuals openly carrying firearms after the program's implementation (Cano and Ribeiro, 2016). Descriptive trends in homicide rates across treated areas are shown in Figure A4 in Appendix A.3, and I reproduce the causal estimates for homicide rates at the end of Section 4.

to UPPs.

[7] Rio de Janeiro State Decree No. 41,650 of January 21, 2009 created the UPP within the state military police. Under Brazil's Constitution (art. 144, § 6), military and civil police answer to state governors rather than municipal governments.

[8] Maré was occupied by security forces in 2014 in anticipation of a UPP, but no unit was installed there during the rollout (PMERJ, 2015). It is the additional area in the count of 39.

Figure 1: UPP Policy Areas in Rio de Janeiro

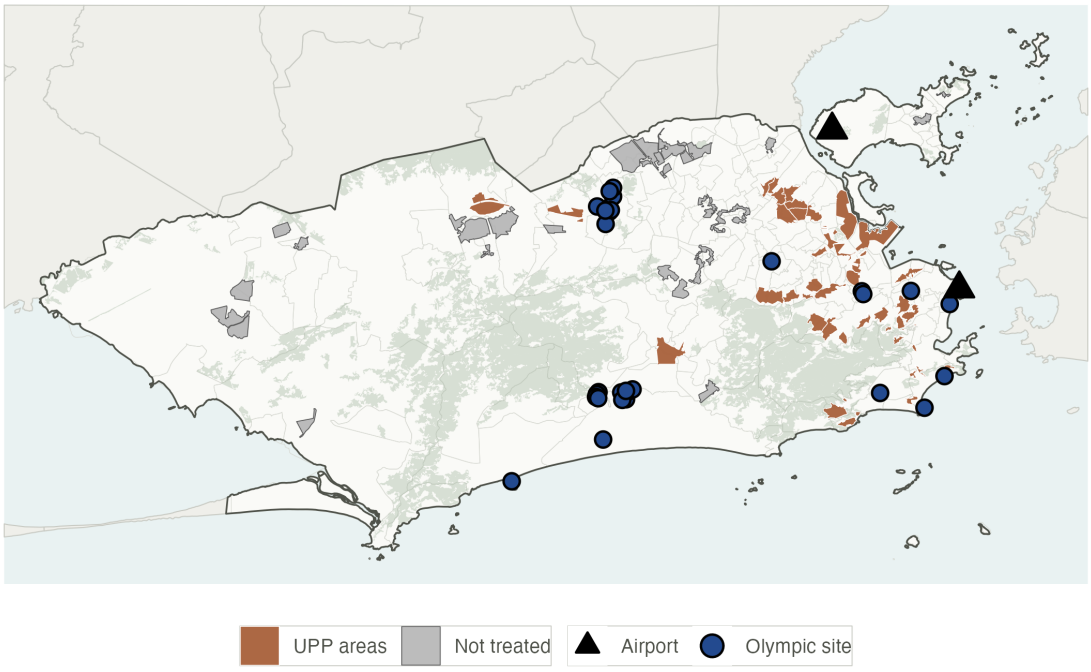
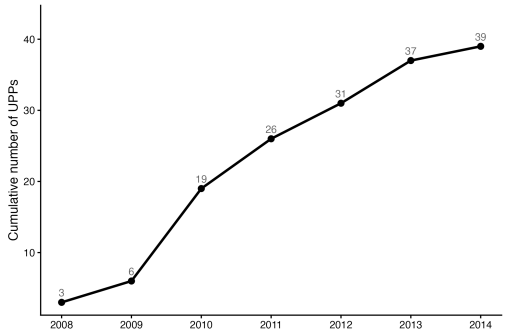
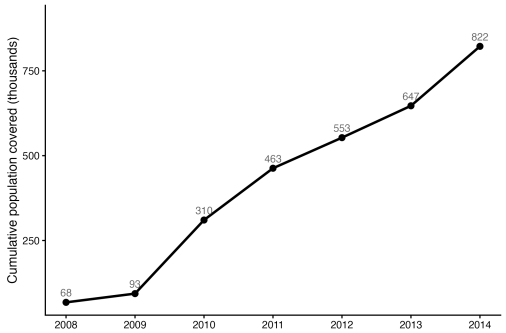


Figure 2: Rollout of the UPP Program

(a) Number of UPP Units



(b) Population Covered



Notes: The figure shows the cumulative rollout of UPP units between 2008 and 2014 and the population covered by the program.

3 Data

To study local firms' responses to the pacification policy, I combine administrative employer–employee records for Rio de Janeiro with information on UPP boundaries, implementation dates, and local crime.

Firms. To study firm outcomes, the main data source is the *Relação Anual de Informações Sociais* (RAIS), a matched employer–employee administrative dataset collected annually by the Brazilian Ministry of Labor. RAIS covers the universe of formal workers and establishments that employ them in the country, providing detailed establishment characteristics, including addresses and 5-digit sectors of activity. In this paper, I explore data from 2003 to 2019 for the city of Rio de Janeiro and geocode establishment addresses within this sample, successfully matching over 95% of the observations to coordinates within the city.[9] Since production and revenue information is not available in RAIS, the main outcomes explored from this dataset are the number of employees and the total establishment wage bill. Because more than 90% of establishments belong to single-establishment firms, I use the terms "establishment" and "firm" interchangeably throughout the paper.

Policy and crime data. To identify firms and workers exposed to the intervention, I use shapefiles delineating the 39 territories covered by the intervention. These maps were made available by the Rio de Janeiro Institute of Public Security (ISP). The program boundaries do not coincide with neighborhood boundaries. Instead, they follow the historical boundaries of favelas controlled by criminal groups. Henceforth, I refer to these territories as "UPP areas" or "treated areas". ISP also records the dates of both stages of the intervention: the initial police operation and the subsequent opening of the UPP station. The earliest recorded first-stage date is July 2008 in Batam. Throughout the analysis, I assign each area's treatment year using the date of the initial operation. ISP also provides population estimates per UPP area and baseline and monthly counts of crime incidents by category through 2021 at the UPP-area level, as well as crime counts for the rest of the city at the police-precinct level.

Sample construction. The main sample of interest consists of all establishments located within the UPP areas between 2003 and 2013. For the analysis of incumbent firms, I focus on establishments that operated in treated areas prior to the program and exclude firms with median baseline employment above 32 workers, the 95th percentile of the firm-size distribution, so that the estimates are not driven by a small number of unusually large

[9] Over the 2003–2019 period, approximately 28,000 establishment–year observations are located within treated communities, corresponding to an average of about 43 establishments with employees per UPP area per year.

establishments. For these firms, I follow employment, wages, and survival outcomes over time. To examine aggregate business dynamics, I also construct a UPP-area-by-year panel by aggregating firm-level data. Descriptive statistics for the main sample are reported in Table A1 in Appendix A.5.

Workers. RAIS worker records also contain rich demographic information, including gender, age, ethnicity, and education. Given the matched nature of the data, I can track workers across jobs and construct their employment histories to measure experience in the formal sector. I use this worker-level information to examine mechanisms in Section 6.

4 Empirical Strategy

The implementation of the UPP policy provides a distinctive setting to study the effects of reducing criminal activity on local businesses for several reasons. First, the program was large in scale, covering over 10% of the city's population and half of the people living in areas under the direct influence of criminal organizations in Rio de Janeiro.

Second, the program was associated with significant reductions in violence and criminal activity, as described in section 2. This provides a strong source of variation in local security conditions, allowing me to study the effects of reduced criminal activity on local economic outcomes.

Finally, the program was implemented within a single city and in a staggered manner, as Figure A1 in Appendix A.1 illustrates. This within-city setting helps limit concerns that treated and comparison areas are exposed to very different regional economic shocks, institutions, or labor markets, as can occur in studies that rely on variation across municipalities or regions. At the same time, the gradual rollout across communities allows me to compare changes in outcomes in early-treated areas with those in late-treated areas that had not yet received the intervention. Because all areas in the main sample were eventually selected for treatment, this comparison relies on communities that are likely to be more similar to one another than treated and never-targeted areas.

The empirical strategy therefore uses the timing of UPP implementation as a source of variation in exposure to reduced criminal activity. To estimate the causal effects, I use event-study specifications of the form:

$$y_{int} = \sum_{\ell \neq -1} \beta_\ell \mathbf{1}\{t - T_n = \ell\} + \alpha_i + \lambda_t + \varepsilon_{int}, \tag{1}$$

where y_{int} denotes the outcome for firm i in UPP area n in year t. T_n is the year of the initial police operation in area n, and ℓ indexes time relative to that year. The coefficients β_ℓ trace the dynamic effects of exposure to the policy, with $\ell = -1$ omitted as the reference period. In the annual event-study figures, I bin the tails of the relative-time distribution, grouping periods $\ell \leq -4$ and $\ell \geq 4$. The specification includes firm fixed effects α_i, which absorb time-invariant differences across firms, and year fixed effects λ_t, which absorb shocks common to all firms in a given year. For UPP-area outcomes, I use the analogous specification with y_{nt} and area fixed effects α_n.

In my empirical analysis, I focus on the period between 2003 and 2013. This window allows me to observe several years of pre-treatment trends and post-treatment outcomes for earlier-treated areas. I stop in 2013 because, by 2014, all areas in the sample had undergone an initial police operation, leaving no not-yet-treated comparison group within the sample. The early years are also the period in which the policy most clearly reduced violence. By 2015, the effects on crime had started to weaken due to fiscal constraints and staffing shortages that reduced the ability of UPP units to maintain their presence in many communities.

I estimate the event-study specification using the estimator proposed by Sun and Abraham (2021) and cluster standard errors at the UPP level. This approach is important in my setting because treatment is staggered: different UPP areas are treated in different years, and the effects of the policy may vary both across treated cohorts and over time since treatment. In this type of design, conventional two-way fixed effects event studies can produce biased dynamic coefficients when already-treated units are used as controls for later-treated units, especially if treatment effects are heterogeneous or evolve over time (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). The Sun and Abraham (2021) estimator avoids this problem by comparing each treated cohort to units that have not yet been treated.

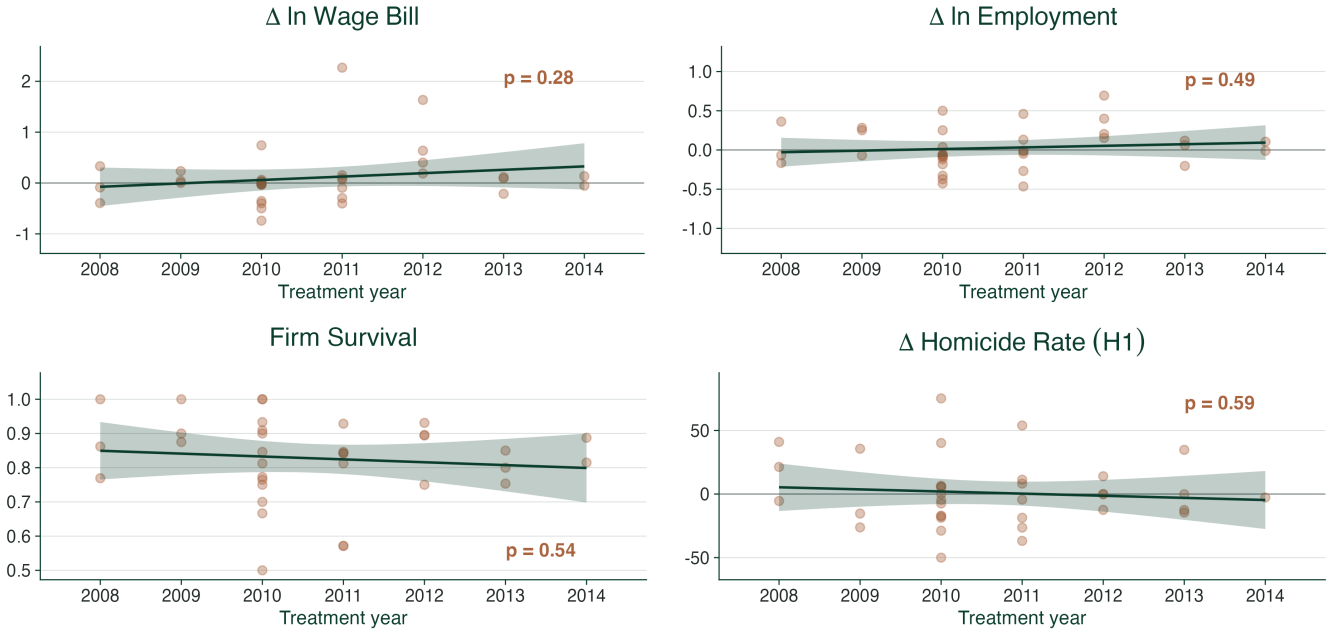
The key identifying assumption is that, in the absence of the UPP intervention, units treated at different points in time would have followed similar trends in the outcome variables. This implies that areas that had not yet received a UPP provide a reasonable counterfactual for earlier-treated areas after accounting for time-invariant differences across units and shocks common to all units in a given year. A central concern is that the timing of UPP implementation could have responded to pre-existing changes in local crime or business activity, in which case early- and late-treated areas would have been on different trajectories even before treatment.

Existing accounts emphasize that the order of occupations followed geographic and economic criteria and was strongly influenced by Rio de Janeiro's preparation for major

sporting events, rather than by particular trends in crime or business activity in these areas (Ferraz et al., 2023; Bellégo and Drouard, 2024). Consistent with this interpretation, Figure 3 plots changes in firm outcomes from 2006 to 2007 and in homicide rates from the first half of 2007 to the first half of 2008 against the year of UPP installation. If areas treated earlier were already on different trajectories, these changes would tend to vary with treatment year. The figure shows no clear relationship. Figures A2 and A3 in Appendix A.2 repeat the firm-outcome panels over longer pre-treatment windows. These comparisons likewise show no clear relationship between pre-treatment firm trends and UPP installation year.

The event-study estimates provide a complementary check using several pre-treatment periods. Coefficients close to zero and without a systematic pre-treatment trend would be consistent with earlier- and later-treated areas evolving similarly before the policy. The post-treatment coefficients then describe how outcomes changed relative to the year immediately before treatment, using not-yet-treated areas as the comparison group.

Figure 3: Changes in Firm Outcomes and Homicide Rates by Treatment Year



Notes: Each point represents a UPP area. Firm employment and wage-bill changes compare 2006 with 2007. Firm survival is the share of 2006 establishments still observed in 2007. The homicide-rate change compares the first halves of 2007 and 2008, so it may already reflect the policy in areas treated in 2008. Lines show linear fits with 95 percent confidence intervals. The *p*-values test the slope of each fit.

One specific threat to the interpretation of the positive firm effects is crime displacement into comparison areas. If installing a UPP pushed criminal activity into communities awaiting treatment, firms there might grow more slowly or contract before their own

UPP arrived. The estimated gains could then partly reflect worsening outcomes among comparison firms rather than growth among treated firms.

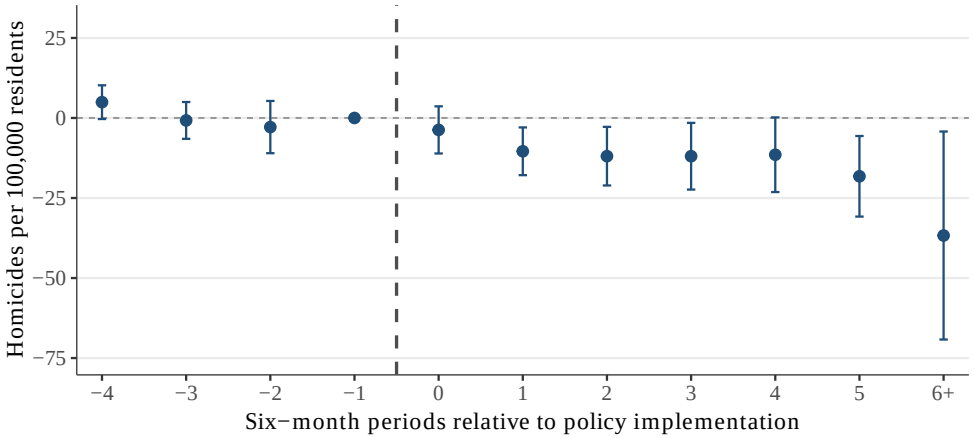
The available evidence offers little indication of large increases in violence in later-treated areas. Crime trends across treated areas, shown in Figure A4 in Appendix A.3, suggest broad reductions rather than a simple shift in violence from earlier- to later-treated communities. Figure A5 in Appendix A.4 provides disaggregated evidence: homicide levels increased between 2007 and the year before treatment in only 6 of the 38 areas, and those increases were small. Existing work also finds limited evidence that reductions in early-treated areas were offset by increases in late-treated ones (Bellégo and Drouard, 2024). This evidence does not rule out spillovers, but it makes deterioration in comparison areas a less likely explanation for the positive firm effects.

4.1 Effects on Crime

A first step is to establish that the UPP program produced a substantial and sustained reduction in local violence. Figure 4 applies the event-study specification in Equation 1 to local homicide rates[10]. The estimates turn negative after occupation: six months after the policy, the estimated decline is 10.4 homicides per 100,000 residents, approximately 56 percent of the pre-treatment rate of 18.5 for the areas contributing to this estimate. The estimates remain negative at later horizons. These patterns corroborate prior evidence that the UPP program produced large and sustained reductions in violence in treated communities (Bellégo and Drouard, 2024; Ferraz et al., 2023; Magaloni et al., 2020). The pre-treatment and spillover checks discussed above lend further support to this interpretation.

[10] I focus on homicides as the key crime indicator because they are less likely to be affected by changes in reporting behavior induced by the policy. They also represent the most extreme form of violence and therefore provide a useful measure of local security conditions.

Figure 4: Effect on Homicide Rates



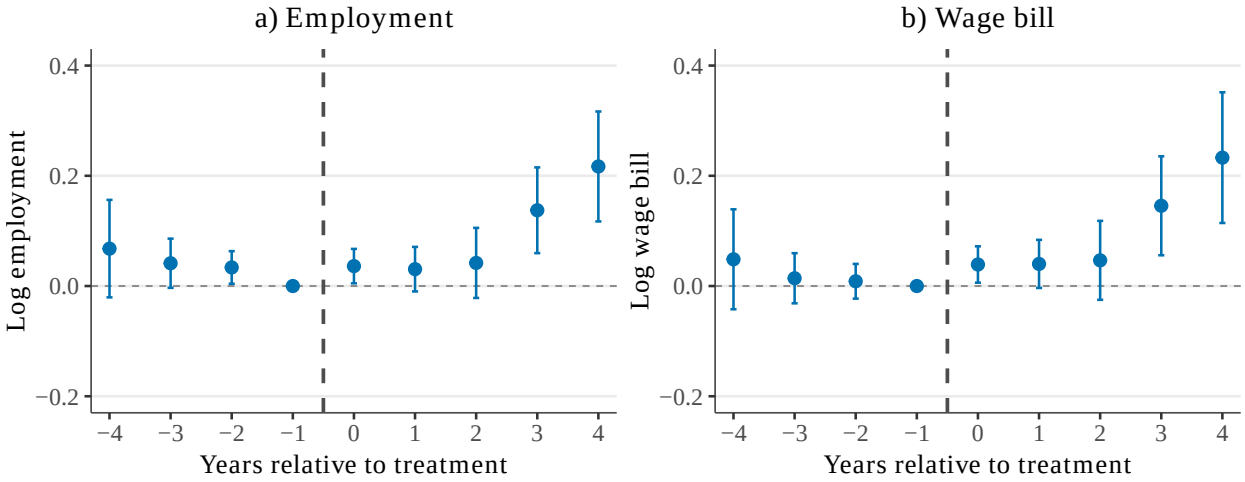
Notes: The figure reports event-study estimates for homicides per 100,000 residents in each six-month period. Event time −1 is omitted. The −4 bin pools earlier periods, and 6+ pools periods three or more years after treatment. Five treatment-semester cohorts contribute to the 6+ estimate. The specification includes UPP-area and semester fixed effects and is weighted by baseline UPP population. Bars denote UPP-clustered 95 percent confidence intervals.

5 Results

In this section, I present the paper's main findings. Using the event-study design described in Section 4, I examine how the UPP policy affected businesses in treated areas. I begin with employment and wage-bill responses among incumbent firms and how these vary with baseline firm size. I then study entry and exit before assessing total employment and wage-bill changes at the treated-area level. Sections 6 and 7 examine and quantify the mechanisms underlying these changes.

Effects on Incumbent Firms. Figure 5 plots the event-study estimates of β_ℓ from Equation 1 for the log employment and log wage bill outcomes, estimated using the unbalanced panel of incumbent firms. The results show that improved security conditions led exposed firms to hire more workers and increase their wage bills. One year after treatment, the estimated effects are 0.031 log points for employment and 0.040 log points for the wage bill. For event years four and later, the pooled estimates reach 0.217 and 0.233 log points, respectively. Table 1 reports average post-treatment effects of 0.054 and 0.060 log points, equivalent to increases of 5.5 and 6.2 percent, respectively. Using wild-cluster bootstrap standard errors, reported in brackets in Table 1, does not change the inference: both average and pooled later effects remain significant for both outcomes at the 5 percent level.

Figure 5: Effects on Incumbent Employment and Wage Bill



Notes: Estimates use the unbalanced incumbent-firm panel with firm and year fixed effects. Event time −1 is omitted. The −4 and 4 bins pool earlier and later years. Bars show UPP-clustered 95 percent confidence intervals.

The results in Figure 5 are also consistent with the parallel-trends assumption discussed in Section 4. The estimated pre-treatment coefficients are not statistically significant, and joint tests fail to reject the null that they are all zero. As an additional check that the estimated effects do not reflect pre-existing trends, Table A6 in Appendix A.10 presents the results controlling for administrative-region-by-year fixed effects[11]. The estimated effects remain positive and similar in magnitude to the main results.

Appendix A.11 shows that the estimated effects are also not driven by selective attrition. Figure A11 shows that treatment does not significantly affect the probability of a firm exiting its original UPP area, and Figure A12 and Table A7 show that the estimated effects remain similar when conditioning on a balanced panel of firms observed every year between 2007 and 2013. Figure A13 and Table A8 relax this condition to firms observed at both endpoints. The average post-treatment effects remain positive, while the five-year estimates are sensitive to one intermittently observed firm.

[11]The municipality of Rio de Janeiro is divided into five administrative planning areas, all of which contain neighborhoods covered by the UPP policy. Figure A10 in Appendix A.10 maps the planning areas and UPP boundaries.

Table 1: Effects on Incumbent Firm Growth

	Log Employment (1)	Log Wage Bill (2)
Average post-treatment effect	0.054	0.060
	(0.020)**	(0.023)**
	[0.019]***	[0.023]***
Effect at $t \geq 4$	0.217	0.233
	(0.051)***	(0.060)***
	[0.052]***	[0.060]***
Observations	12,536	12,536
Firms	2,201	2,201
UPP clusters	34	34

Notes: The table reports the estimates plotted in Figure 5. The average post-treatment effect aggregates available event-time bins 0–3 and 4+ across treated cohorts, weighting cohort-by-event-time coefficients by the number of contributing firm-year observations. The 4+ bin pools years 4 and 5. Specifications include firm and year fixed effects. UPP-clustered standard errors are in parentheses. Wild-cluster bootstrap standard errors use 999 Rademacher residual draws at the UPP level and are in brackets. Stars on each standard-error row use that row's inference method. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Interpretation. Firms in the main estimation sample employed an average of 6.56 workers in 2007, meaning that the average employment gain corresponds to approximately 0.36 additional workers per firm after treatment, or 1.59 workers in event years 4 and later. As a descriptive benchmark, among the 213 firms in areas untreated through 2013 that were observed in both 2007 and 2013, employment increased by 11.5 percent. Their real wage bills grew by 43.2 percent over the same period, likely partly reflecting increases in the minimum wage.[12] Firms in areas not yet treated were therefore growing over this period, while the estimates show that treatment generated significant additional growth in employment and the wage bill.

Job Formalization. Given the elevated informality rates in Rio de Janeiro, a relevant consideration is whether the employment and wage-bill gains reflect the formalization of previously informal employment relationships, rather than a real expansion in firm size.[13] In Table A11 in Appendix A.14, I analyze whether the treatment changed the profile of new hires in a manner consistent with this explanation, exploiting the worker-level structure of the data to track employment histories across firms. The estimates show no statistically

[12]The monthly national minimum wage rose from R$380 in April 2007 to R$678 in January 2013, a 78% nominal increase (2007 legislationand 2013 decree), or a 28% real increase. The wage-bill growth reported in the text is also measured in real terms.

[13]Note that firm formalization itself cannot account for the results because the analysis follows a fixed set of establishments that were already formally registered before the policy.

significant change in the share of new hires who held a formal job at another firm in the previous year. Thus, this test does not provide evidence that formalization of previously informal employment relationships is the main source of the employment gains, although it cannot rule out some contribution. I discuss the effects on worker composition further in Section 6.

Sporting-Events Confound. The UPP rollout preceded two major international sporting events hosted by Rio de Janeiro in 2014 and 2016, and the choice of targeted areas partly reflected concerns about improving security for visitors. Indeed, among all 39 UPP areas, three-quarters were located within 3.5 km of an Olympic venue (Table A5 in Appendix A.7)[14]. One question, therefore, is whether the estimated effects on firm growth capture benefits from proximity to these competitions rather than the consequences of crime reduction induced by the installation of UPP units. In difference-in-differences terms, the concern is that treatment timing may be correlated with other shocks affecting the evolution of the outcome variables.

Two features of the empirical setting limit this threat. First, the analysis covers 2003–2013 and therefore ends before either competition took place, not capturing direct contemporaneous effects generated by the associated influx of visitors. Second, because I avoid using never-treated areas as controls, shocks common to all areas eventually covered by UPPs are absorbed by time fixed effects. Nevertheless, preparations for the events may have affected some parts of the city more than others. If these local changes coincided with UPP implementation, they could confound the estimated effects.

I test this possibility empirically in two ways. First, I reestimate the results allowing for flexible time trends based on proximity to sporting venues. Table A2 and Figure A6 in Appendix A.6 show that the effects on employment and the wage bill remain positive and statistically significant when adding venue-distance-by-year controls. In these specifications, I consider three distance measures: the distance from each UPP area to its nearest Olympic venue, to Maracanã Stadium, and to Copacabana Arena. The latter two are prominent sites in the two zones that account for most UPP areas' nearest venues.

Second, Table A3 compares treatment effects in areas above and below the median for each of the three distance measures. Medians are calculated separately across the 34 UPP areas in the estimation sample. The specifications interact cohort-event treatment effects with an above-median-distance indicator while retaining firm, year, and distance-by-year effects. If proximity to the sporting events explained the gains, effects should be larger in areas closer to the venues. Instead, estimated employment and wage-bill gains are larger above the

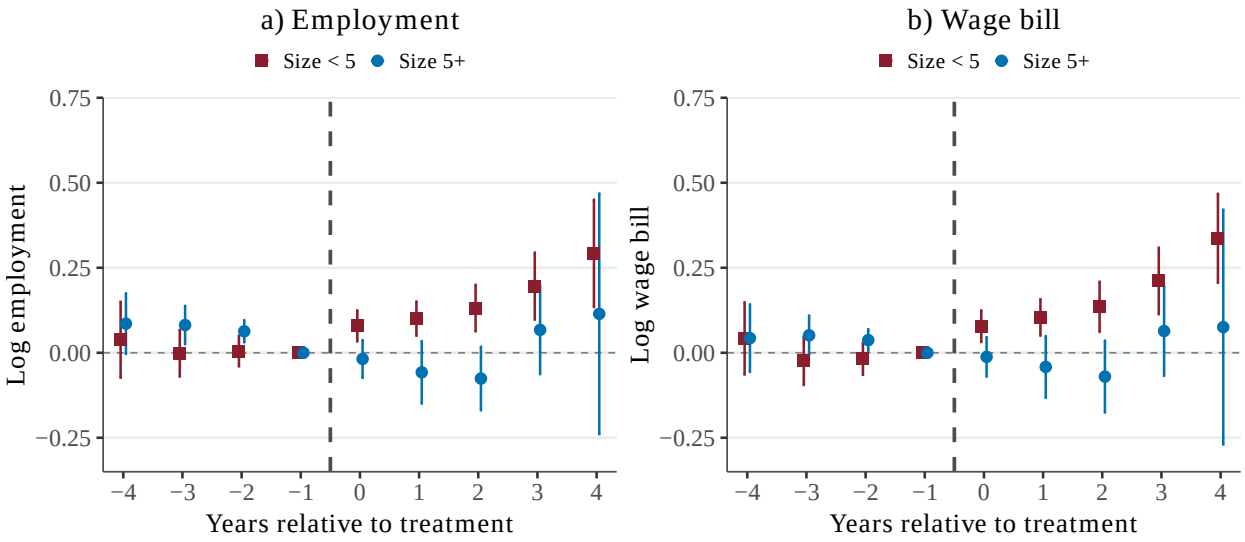
[14]Table A4 lists the closest venue to each UPP area.

median for all three distance measures. For the nearest venue, the employment effects are 0.102 log points above the median and 0.039 below it; the corresponding wage-bill effects are 0.122 and 0.050. The above-minus-below differences are statistically significant at the five percent level for employment under all three measures and for the wage bill using the nearest venue and Copacabana Arena. These comparisons offer no evidence that the gains are concentrated near sporting venues.

Taken together, the results from these exercises offer little support for a venue-proximity explanation: the gains remain positive with venue-distance controls, and they are not concentrated in the treated areas closest to the venues.

Size Heterogeneity. The average effects reported above mask substantial heterogeneity by firm size. I examine this by splitting the incumbent sample in Figure 5 at five workers in median pre-treatment employment.[15] Figure 6 plots the event studies separately for firms below five workers and those with five or more workers.

Figure 6: Effects on Incumbent Growth by Baseline Firm Size



Notes: The figure splits the incumbent sample in Figure 5 by median pre-treatment employment: fewer than five workers (red squares) and five or more (blue circles). Both groups use the same event-study specification as Figure 5. Bars show UPP-clustered 95 percent confidence intervals.

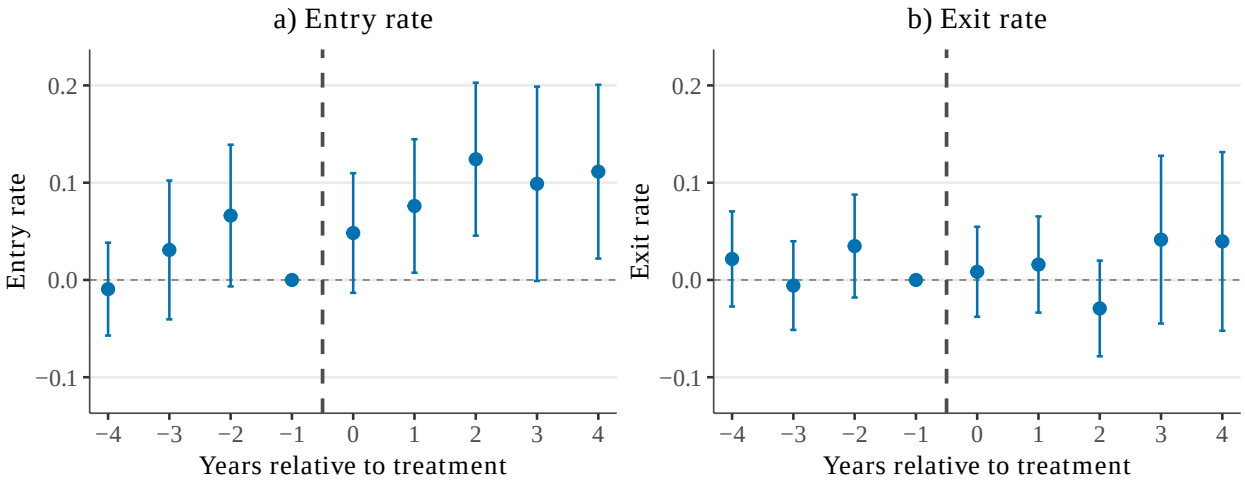
The results show that firms with fewer than five workers grow by 11.7 percent in employment and 12.5 percent in the wage bill on average following treatment, compared with statistically

[15]For each firm, baseline employment is the median of its observed employment in all available years before its UPP area's treatment. The five-worker split is motivated by the World Bank Enterprise Surveys' microenterprise convention, which covers establishments with one to four workers. Figures A14 and A15 in Appendix A.12 use four and six workers as alternative thresholds and show similar patterns.

insignificant changes of −2.7 and −2.2 percent, respectively, for firms with five or more workers.[16] These results are consistent with larger firms being less constrained by local violence: they may be better able to absorb the fixed costs of security-related investments or compensate workers for taking jobs in unsafe areas. They may also have more scope to relocate when conditions deteriorate, whereas small firms are more likely to remain tied to their neighborhoods. In that case, reducing violence would especially ease small firms' difficulty in attracting workers and expanding. These heterogeneous results are discussed further in Section 6.

Firm Entry. Beyond helping small incumbent firms grow, crime reduction may encourage local residents and entrepreneurs from elsewhere to start businesses in treated areas, or existing firms to relocate there. Figure 7 investigates this margin by estimating the area-level version of Equation 1 for firm entry and exit rates. Using an annual UPP-area panel, I define each rate as the number of firms entering or departing in year t divided by the number of firms present in $t-1$. Entrants include relocations into the area, and departures include relocations out of it.

Figure 7: Effects on Firm Entry and Exit Rates



Notes: Both rates use the number of firms in the previous year as their denominator. The population-weighted event-study specification uses all treated cohorts and UPP-area and year fixed effects. It does not include population-quintile-by-year controls. Event time −1 is omitted. Vertical bars show conventional 95 percent confidence intervals based on UPP-clustered standard errors.

The estimates show that the policy increased firm net entry. The average post-treatment entry-rate effect is 8.1 percentage points, compared with 2.2 percentage points for exits.

[16] All estimates are reported in Table A9 in Appendix A.12. The differences between the two size groups are statistically significant for both outcomes.

Appendix A.8 separates these flows: the entry increase is driven by establishments with no citywide RAIS record in the previous year, rather than relocations from elsewhere in Rio, while neither exit component shows a clear treatment effect. To probe whether the entry response reflects formalization of existing activity, Figure A8 examines the share of entrants whose workers were all absent from RAIS in the preceding year. This share does not rise significantly after treatment, suggesting that the entry effect does not simply capture a push to formalization of existing firms.

Area-Level Effects. Finally, I examine the results at the treated-area level. I explore two outcomes: total employment and the total wage bill, each summed across all active establishments in a UPP area and year. Figure A9 in Appendix A.9 plots the area-level event study. The estimates are imprecise and mostly positive after treatment, with average post-treatment effects of 0.087 log points for employment and 0.137 log points for the wage bill. These positive point estimates are consistent with gains beyond a reallocation of activity among firms within treated areas, although the confidence intervals include zero. Taken together, the evidence presented in Section 5 suggests that reducing violent crime creates better conditions for local firms to operate and grow.

6 Mechanisms

How did the reduction in violence translate into gains for firms? In this section, I examine two channels through which criminal activity can constrain local businesses: access to customers in the product market, and access to workers in the labour market. The presence of criminal organizations and the widespread threat of violent crime can alter where individuals choose to undertake everyday activities. A reduction in crime may therefore expand local businesses' pools of potential customers and workers.

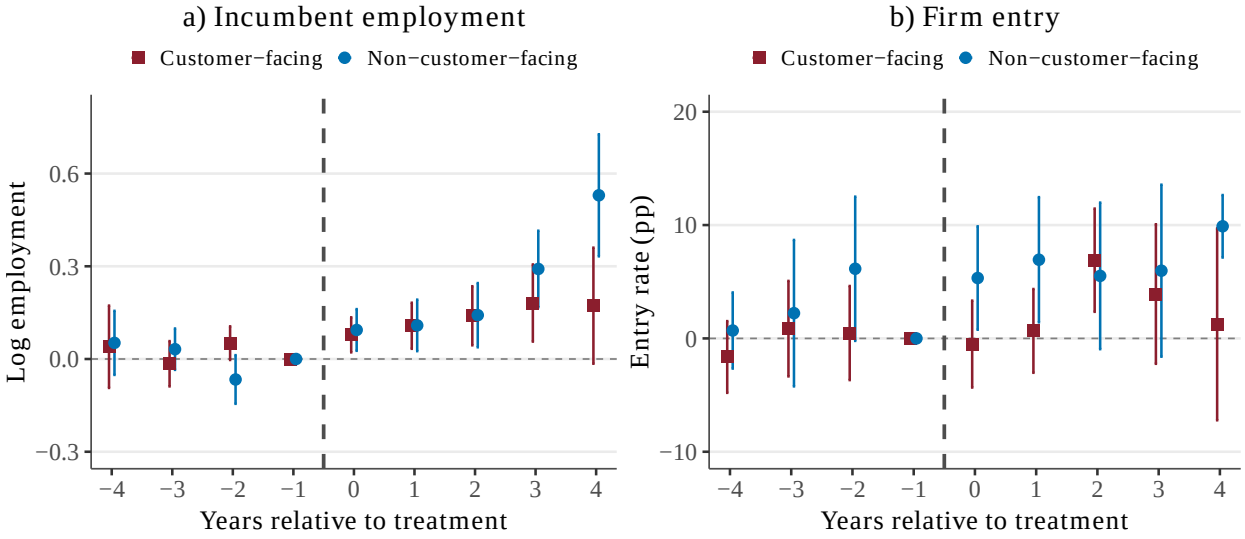
To analyse these ideas empirically, I extend the analysis in Section 5 to examine which small incumbent firms expand, which firms enter, who gets hired, and how wages respond. The empirical evidence does not support customer inflows as the main explanation, but is consistent with a story of improved access to workers. Section 7 then complements this analysis by developing a model of local labour markets that formalises these channels, quantifies the share of the firm response explained by changes in labour supply, and helps interpret the remaining response.

6.1 Access to Customers

Making treated areas safer could have drawn new customers into local businesses. Previous evidence from the United States shows that crime affects consumer visits and that violence particularly harms businesses reliant on in-person patronage (Fe and Sanfelice, 2022; Greenbaum and Tita, 2004). If this channel explained the local response to the UPP program, customer-facing firms should have benefited more than other businesses. I test this hypothesis by splitting firms into customer-facing and non-customer-facing groups using different definitions and estimating separate treatment effects.

Figure 8 presents the results using the main customer-facing definition I adopt, which classifies retail, hospitality, and personal services businesses as customer-facing, following Greenbaum and Tita (2004). Panel (a) compares employment growth among small incumbent firms in the two groups, and panel (b) compares their contributions to the area entry rate. The employment effects are similar between the groups, and the rise in entry is significant only for non-customer-facing firms, although the difference in treatment effects between the two groups is not statistically significant.

Figure 8: Customer-Facing Firms: Effects on Employment and Entry



Notes: Panel (a) reports effects on log employment among incumbent firms with median baseline employment below five. Panel (b) reports effects on area-level entry rates, defined as entrants in each sector divided by all firms present in the area in the preceding year. Entrants of any size are included. The Main classification treats retail, food and hospitality, and personal services as customer-facing. Employment specifications include firm and customer-facing-by-year fixed effects. Entry-rate specifications include UPP-area and year fixed effects and are weighted by area population. Event time −1 is omitted. Bars show UPP-clustered 95 percent confidence intervals.

Table 2 presents average post-treatment effects using the main customer-facing classification and three alternatives. Panel A reports the heterogeneity of employment effects among small incumbent firms, and Panel B reports entry-rate effects for the same classifications. The classification in Column 1 is the one shown in Figure 8. Column 2 compares manufacturing with retail businesses, and Column 3 restricts the comparison to businesses classified within the broad services sector[17]. Finally, Column 4 uses a manual classification based on the description of the five-digit industry codes available in RAIS, detailed in Appendix A.15, Table A12. Under all four definitions, customer-facing businesses show no advantage over other firms, suggesting that access to customers is not the main driver of the treatment effects.

Table 2: Employment and Entry-Rate Effects by Customer-Facing Status

	Main (1)	Manufacturing vs. Retail (2)	Services Only (3)	Manual Classification (4)
Panel A. Incumbent employment (log points)				
Treated	0.143***	0.142	0.195***	0.203***
	(0.036)	(0.134)	(0.052)	(0.047)
Customer-facing	−0.038	−0.013	−0.113**	−0.115*
	(0.047)	(0.140)	(0.052)	(0.058)
Observations	6,991	4,148	6,209	6,991
Firms	1,332	781	1,186	1,332
Panel B. Firm entry rates (percentage points)				
Treated	5.537***	2.099*	3.372***	6.309***
	(2.022)	(1.045)	(1.142)	(1.642)
Customer-facing	−2.995	−1.901	−0.773	−4.539
	(2.678)	(1.826)	(3.091)	(2.795)
Observations	680	680	680	680

Notes: Entries are average post-treatment effects. Treated is the effect for other firms. Customer-facing is the additional effect relative to other firms from a fully interacted model. Panel A uses incumbents with median baseline employment below five and includes firm and customer-facing-by-year fixed effects. Panel B uses area-level entry rates: entrants in each sector divided by *all* firms present in the area in the preceding year. Panel B includes UPP-area-by-sector and year-by-sector fixed effects and is weighted by area population. The 2014 cohort is the reference group. UPP-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Furthermore, taking the incumbent point estimates at face value suggests a negative customer-side response. One possibility is that safer travel allowed local residents to shop

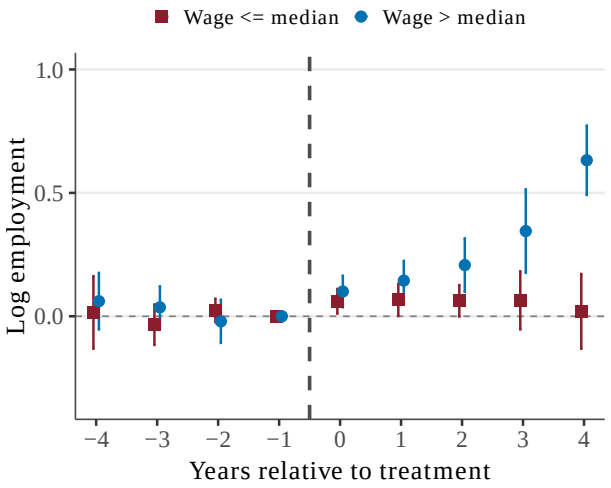
[17]Within services, the customer-facing group includes retail and wholesale trade, hotels and restaurants, education, health, and personal services. The non-customer-facing group includes transport, business and computer services, finance, and real estate.

farther from home, drawing spending away from treated areas. However, the customer-facing differential is imprecisely estimated in most specifications, and this interpretation remains suggestive.

6.2 Access to Workers

Improved security conditions can also make it easier for local firms to hire. Before the policy, the persistent threat of violence in gang-controlled communities may have deterred workers from taking jobs in these areas, limiting the ability of local firms to recruit and expand. To assess this hypothesis, I examine which firms expand after treatment, whom they hire, and how wages respond. Together, the patterns presented are consistent with improvements in labour supply driving firm growth in treated areas.

Figure 9: Employment Growth by Baseline Wage Among Small Incumbents



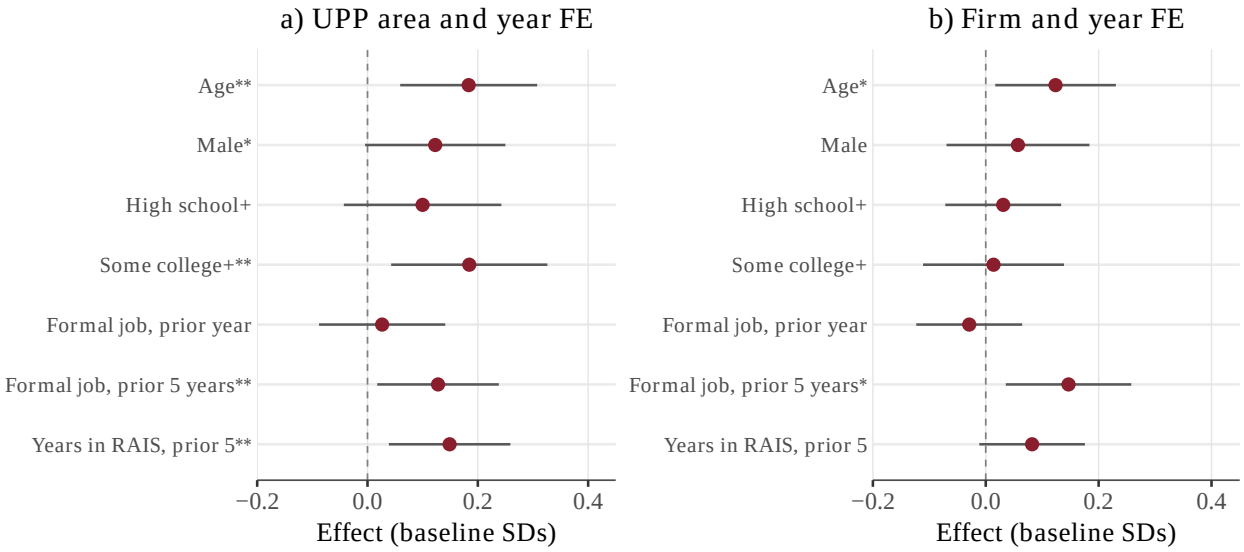
Notes: Effects on incumbent log employment by pre-treatment wage group (median baseline size below five). Firm and year fixed effects are included. Event time −1 is omitted. Bars show 95 percent UPP-clustered confidence intervals.

Figure 9 splits small incumbents by their pre-treatment average wages, showing that employment gains were considerably larger among firms paying above-median wages. This difference persists after controlling for sector-by-year fixed effects (Figure A16 in Appendix A.13), suggesting that it is not driven by sector-wide changes over time. Firm pay is one dimension of a job's attractiveness and can also proxy for productivity (Card et al., 2018). The results in Figure 9 therefore suggest that, among small firms, those offering better jobs were more constrained by crime, consistent with difficulty recruiting

qualified workers having limited their growth before treatment. Table A10 compares the two groups across several baseline characteristics. Indeed, among other differences, workers at higher-paying firms were considerably more educated: 43.1 percent had a high school education or more, versus 27.9 percent at lower-paying firms, and 12.1 percent had some college or more, versus only 2.1 percent in the other group.

Furthermore, Figure 10 examines changes in the composition of workers hired by small incumbent firms in treated areas. Panel (a) presents worker-level regressions in which the outcomes are worker characteristics and the sample is restricted to new hires.[18] The results reveal that, after treatment, new hires at these firms are generally older, more likely to be male, more educated, and have more formal work experience. In Panel (b), I additionally include firm fixed effects. The age and prior-five-year employment measures remain positive and statistically significant at the 10 percent level after multiple-testing adjustment, suggesting that some of the change in composition occurs within firms and goes beyond changes in which firms hire.

Figure 10: Effects on the Profile of New Hires



Notes: Both panels use new hires at small incumbent firms. Coefficients are average post-treatment effects, standardized by the pre-treatment standard deviation. Horizontal bars show conventional 95 percent confidence intervals. Stars mark Benjamini–Hochberg-adjusted significance within each panel.

Finally, Table 3 presents the treatment effects on wages using worker-level regressions with UPP-area and year fixed effects. Column 1 shows a positive, statistically significant effect

[18] A new hire is a worker not employed by the same establishment in the previous year, based on a match to the previous year's nationwide RAIS records.

of 0.080 log points without worker controls. The estimate falls to 0.043 in Column 2 after controlling for worker characteristics[19], and adding firm fixed effects in Column 3 reduces it to -0.006, which is not statistically significant. The attenuation is consistent with the changes in worker composition described above and further suggests that the remaining worker-level wage gain reflects sorting into higher-paying firms rather than detectable wage increases within firms.

Table 3: Worker-Level Wage Effects Among Small Incumbents

	No Worker Controls (1)	Worker Controls (2)	Worker Controls and Firm FE (3)
Treated	0.080***	0.043***	−0.006
	(0.016)	(0.010)	(0.009)
Observations	19,155	19,155	19,155
Firms	1,565	1,565	1,565

Notes: The outcome is worker-level log wages among incumbent firms with median baseline employment of at most five. Worker controls are age, age squared, gender, education, and race. All specifications include UPP-area and year fixed effects. Column 3 also includes firm fixed effects. Standard errors clustered by UPP area and worker are in parentheses. $^{***}p < 0.01$.

Together, the growth of higher-paying firms, the more educated and experienced new hires, and the lack of detectable within-firm wage gains are consistent with increased labour supply, particularly if skilled workers are more responsive to local safety. These patterns align with evidence from the United States that local safety and other amenities shape where more-educated households choose to live. Cullen and Levitt (1999) find that households with more education are more likely to leave cities when crime rises, while Diamond (2016) shows that college-educated workers sort toward cities with improving amenities. Although residential location and workplace choice are different decisions, workers with better outside options may be more reluctant to take jobs in unsafe areas. A reduction in violence could therefore expand local firms' access to more-skilled workers.

Given the empirical results discussed above, a natural question is how safer conditions would draw in workers but not customers to treated areas. Three features of the setting can help explain this asymmetry. First, labour markets are more spatially integrated than markets for local retail. Workers commonly travel across the city for jobs, whereas consumers tend to shop close to home, and small shops in these areas may mainly serve

[19]These controls include age, age squared, gender, education, and race. Table A16 in Appendix A.16 uses the 2007–2013 sample to add two measures from workers' RAIS histories: an indicator for formal employment in the previous year and the number of years observed in RAIS over the previous four years. The wage estimates remain similar with and without these controls.

local residents. Second, exposure to violence differs. Workers enter the area daily, while customers visit only occasionally, so the threat of violence weighs more heavily on the decision to take a job than on a shopping trip. Third, it is worth noting that the customer results capture a net effect. Safer mobility allows outsiders to shop in treated areas but also allows residents to shop elsewhere, so the absence of a differential effect for customer-facing firms may reflect offsetting flows rather than no change in customer behaviour.[20]

Finally, note that these hiring patterns do not, by themselves, rule out a labour-demand response: firms facing fewer disruptions could also expand and recruit more skilled workers. Section 7 considers this possibility and quantifies relative contributions.

7 Quantification

This section develops a theoretical framework that further organizes the mechanisms discussion and extends it in two ways. First, I use it to quantify how much of the firm response a shift in labour supply can explain. Second, it helps interpret the remaining response through shifts in labour demand, including those arising from fewer disruptions and lower operating costs.

7.1 Theoretical Framework

The model presented in this section describes a single local labor market (in my application, a gang-controlled neighborhood that was part of the pacification program), in which local firms hire workers willing to take jobs in the area. Improved security can affect firms in three ways. It can change demand from customers, change the pool of available workers, and reduce the crime-related costs and disruptions associated with production.

7.1.1 Model Setup

Production and Customer Demand. Consider a neighborhood with crime intensity c. Firms located in this area belong to one of two sectors, $j \in \{CF, NCF\}$: customer-facing firms (CF), whose revenues depend on in-person patronage, and non-customer-facing

[20]This possibility is consistent with evidence from El Salvador showing that changes in local violence can alter spatial mobility and access to outside economic opportunities. Melnikov et al. (2025) show that residents living just inside gang-controlled territory are 50 percentage points more likely to also work within gang territory than individuals living only 50 meters away in non-gang-controlled areas.

firms (NCF). A firm produces with labor alone, and production is given by

$$y_j = \big(1 - \tau(c)\big) D_j(c) n_j^{\alpha}, \tag{2}$$

where n_j is employment and $\alpha \in (0,1)$ is the output elasticity of labor. The crime tax $\tau(c) \in [0,1)$ captures the loss of effective output associated with producing in a high-crime area, including disruptions, extortion, and other costs of operating. Reduced violence is therefore expected to decrease this tax by limiting those losses. The demand shifter $D(c) > 0$ captures the net effect of security on customer-facing firms' demand *relative to* other firms. I assume that non-customer-facing firms have no customer-demand shift:

$$D_{CF}(c) = D(c), \qquad D_{NCF}(c) = 1. \tag{3}$$

An inflow of customers can raise $D(c)$, but easier travel can also allow local residents to shop elsewhere, so its net change when crime falls is unrestricted.

Labor Supply. Workers supply labor to the neighborhood according to

$$N^S(w,c) = S(c) w^{\varepsilon}, \qquad \varepsilon > 0, \tag{4}$$

where $S(c) > 0$ shifts the number willing to work locally at a given wage and ε is the local labor-supply elasticity. A crime reduction can raise $S(c)$ if more outsiders accept jobs in the area, or lower it if more residents take jobs elsewhere. The model allows $D(c)$, $S(c)$, and $\tau(c)$ to respond independently to security conditions.

Firm Optimization and Equilibrium. Firms operate in a competitive market and take the common local wage w and the output price, normalized to one, as given. A type-j firm solves

$$\max_{n_j \geq 0} \big(1 - \tau(c)\big) D_j(c) n_j^{\alpha} - w n_j. \tag{5}$$

Its first-order condition gives labor demand

$$n_j(w,c) = \left[\frac{\alpha (1 - \tau(c)) D_j(c)}{w} \right]^{\gamma}, \qquad \gamma \equiv \frac{1}{1-\alpha} > 1. \tag{6}$$

Let M_{CF} and M_{NCF} denote the numbers of firms in each sector. Aggregate labor demand is

$$N^D(w,c) = \left[\frac{\alpha(1-\tau(c))}{w}\right]^{\gamma} \left[M_{NCF} + M_{CF}D(c)^{\gamma}\right]. \tag{7}$$

Equating this expression to Equation (4) yields the market-clearing wage

$$w^*(c) = \left[\frac{\{\alpha(1-\tau(c))\}^{\gamma}\{M_{NCF} + M_{CF}D(c)^{\gamma}\}}{S(c)}\right]^{1/(\gamma+\varepsilon)}. \tag{8}$$

If crime falls, an increase in $D(c)$ or a decrease in $\tau(c)$ raises firms' demand for labor and bids up the local wage. An increase in $S(c)$ expands the pool of available workers and puts downward pressure on wages. The wage response therefore depends on the balance of these shifts.

7.2 Importance of Channels

Building on this framework, I first ask how much of the employment response can be attributed to improved access to workers. Let Δ denote the change induced by treatment, let $q \equiv \Delta \ln n_{CF} - \Delta \ln n_{NCF}$ be the customer-facing employment differential, and let s be the customer-facing share of employment before treatment. The change in the customer-demand component of aggregate labor demand is $Q \equiv \ln(1 - s + se^q)$, as derived in Appendix A.17, Equation (13). Combining the market-clearing condition with non-customer-facing labor demand gives

$$\Delta \ln S = \Delta \ln n_{NCF} - \varepsilon \Delta \ln w^* + Q. \tag{9}$$

Intuitively, non-customer-facing employment growth measures how much more labor firms hire at the new wage. I subtract the employment change that would follow mechanically from the wage response along a fixed labor-supply curve, then account for the effect of customer demand on the common wage. The remainder is the shift in labor supplied to the area. Appendix A.17 gives the full derivation.

To calibrate the model, I use the estimates based on the main customer-facing classification in Column 1 of Table 2: retail, food and hospitality, and personal services are customer-facing. Focusing on incumbent firms with median baseline employment below five, the estimated employment effects are 0.143 log points for non-customer-facing firms and 0.105 for customer-facing firms, a differential of $q = -0.038$. The pre-treatment customer-facing employment share is $s = 0.584$. For the wage change, I use the separate worker-level

estimate for new hires at these small incumbent firms, controlling for worker characteristics and firm fixed effects: $\Delta \ln w^* = -0.025$ log points. This serves as a proxy for the change in the wage faced by firms when hiring.

I set the labour-supply elasticity to $\varepsilon = 2.7$, using the estimate from Sharma (2023) as a benchmark. This gives $Q = -0.022$ and an implied $\Delta \ln S = 0.190$ log points. With $\alpha = 2/3$ (so $\gamma = 3$), the corresponding labor-supply contribution is 0.100 log points: 70 percent of net employment growth in non-customer-facing firms. For customer-facing firms, customer demand makes a negative contribution, so the same 0.100 log points represent 76 percent of the *positive* contributions to employment growth. This decomposition strengthens the interpretation of the evidence in Section 6.2 that improved access to workers is a key source of small-firm growth. Crime-tax relief accounts for another 22 percent of non-customer-facing growth and 24 percent of positive customer-facing contributions. Appendix A.17 reports each contribution in log points and shows how the shares change with the assumed elasticities.

7.3 Size of the Crime Tax

In this second exercise, I measure the size of the implied crime tax. The production term $1 - \tau(c)$ measures how much output remains after crime-related costs and disruptions. Because customer demand does not shift non-customer-facing firms' revenue in the model, their labor-demand condition identifies the implied crime-tax relief, conditional on γ:

$$\Delta \ln(1 - \tau) = \Delta \ln w^* + \frac{\Delta \ln n_{NCF}}{\gamma}. \tag{10}$$

Using the same small-incumbent employment and new-hire wage estimates, and fixing $\gamma = 3$ ($\alpha = 2/3$), I obtain $\Delta \ln(1 - \tau) = 0.022$ log points. For non-customer-facing firms, $D_{NCF} = 1$, so their production function is

$$y_{NCF} = \big(1 - \tau(c)\big) n_{NCF}^{\alpha}. \tag{11}$$

Because $1 - \tau(c)$ multiplies output, the estimated change corresponds to approximately a 2.2 percent productivity gain at a fixed level of employment. Possible sources of this implied gain are discussed below in Section 7.4. The estimate and channel shares depend on the calibrated elasticities. Appendix A.17 reports sensitivity to both ε and γ.

7.4 Interpretation of the Crime Tax

The model's crime tax, $\tau(c)$, summarizes costs that prevent firms from producing at their potential with a given level of labor input. A reduction in $\tau(c)$ raises the net-of-tax production factor $1 - \tau(c)$. The decomposition above suggests that relief from this distortion may account for part of the employment response alongside improved access to workers. In this section, I discuss potential explanations for the implied reduction in the crime tax.

One possibility is that lower violence reduced disruptions to production. In this setting, turf wars between gangs and confrontations between gangs and police can interrupt business activity. As described in Section 2, areas targeted by the UPP policy had elevated levels of violent crime, including deaths caused by police intervention. During confrontations, workers may stay home because of curfews imposed by armed groups or fear of crossfire. A recent survey of residents of two gang-controlled communities in Rio illustrates this disruption: among respondents who worked and knew of a police operation in the preceding year, 60.4 percent reported being unable to work because of a police operation (Lemgruber et al., 2023). Firms can then face absenteeism, shorter operating hours, and interrupted deliveries, even when their measured labor input is unchanged. Related evidence from Brazil links violence to teacher and student absenteeism (Koppensteiner and Menezes, 2021; Monteiro and Rocha, 2017) and to weaker student performance (Peçanha, 2024).

Extortion is another possible source of the wedge: payments demanded by criminal groups reduce the return to operating locally. Evidence from El Salvador and Guatemala shows that extortion can raise business costs and discourage entry (Brown et al., 2024; Estefan and Ordoñez, 2025). In Rio, protection rackets were more central to militias than to drug-trafficking gangs, while the UPP program mainly targeted areas controlled by the latter (Pope, 2023; Magaloni et al., 2020). Reports nevertheless document extortion by drug gangs on a smaller scale (Monteiro et al., 2022), so a reduction in these payments cannot be ruled out as a contributor to the implied crime-tax relief.

8 Discussion

This paper studies how violent crime affects local businesses by examining the staggered rollout of the UPP program in Rio de Janeiro. Combining geocoded administrative data on formal establishments and workers with the timing and boundaries of the intervention, I

compare areas treated earlier and later by the policy. My results reveal that the program substantially reduced local violence: six months after treatment, the estimated decline in homicides was equivalent to 56 percent of the pre-treatment rate. Incumbent firms expanded employment and their wage bills, with average post-treatment gains of 5.5 and 6.2 percent, respectively, concentrated among firms with fewer than five workers at baseline. Firm entry also rose, with no clear increase in exits.

To understand these responses, I examine changes in customer demand and access to workers. Across several definitions, customer-facing firms show no clear advantage in employment growth or entry, offering little support for customer inflows as the main explanation. Among small incumbents, higher-paying firms expand more and new hires become more educated and experienced, while wages within firms show no detectable increase. These patterns are consistent with improved access to workers, although they do not rule out a labor-demand response. A simple model of local labor markets allows improved security to affect customer demand, labor supply, and a crime-related wedge on production. Under the benchmark calibration, the implied labor-supply shift accounts for most of the employment response among small incumbents, while relief from the production wedge corresponds to an approximately 2.2 percent productivity gain at a fixed level of employment.

These results point to a broader lesson for policies aimed at improving firm performance. Many interventions target firms directly, through credit, training, formalization, or management support (Banerjee et al., 2015; McKenzie, 2021; Bruhn and McKenzie, 2014). This paper highlights a complementary channel: improving the local environment in which firms operate. By reducing exposure to criminal activity, the policy changed the constraints and incentives facing businesses, allowing small firms to expand. Better security may also raise the returns to policies that help workers access jobs and small firms recruit. Transport improvements that reduce commuting costs to these neighborhoods are one potential complement.

My results also have implications for how the economic consequences of public security policies should be understood. Business responses can affect the sustainability of violence reduction efforts: better local economic opportunities may reinforce declines in crime, while disruption or displacement may slow recovery and weaken support for the policy. Understanding how firms respond to reductions in criminal activity is therefore important not only for measuring the full benefits of public security interventions, but also for designing policies that support durable local improvements.

Taken together, the evidence presented in this paper suggests that criminal activity affects local economic development not only through its direct costs to victims, but also by

shaping the conditions under which firms hire and operate day to day. Measuring these indirect costs is important for understanding the full economic burden of violence and for evaluating policies that improve safety in areas where firms, workers, and residents have long faced the threat of violence and instability.

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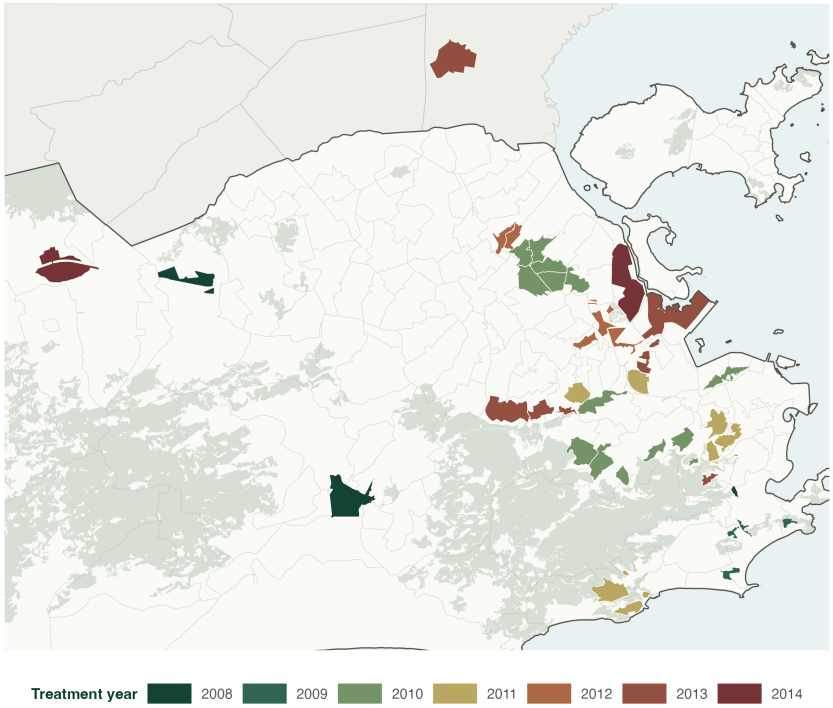
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A Appendix

A.1 Treatment Rollout Across UPP Areas

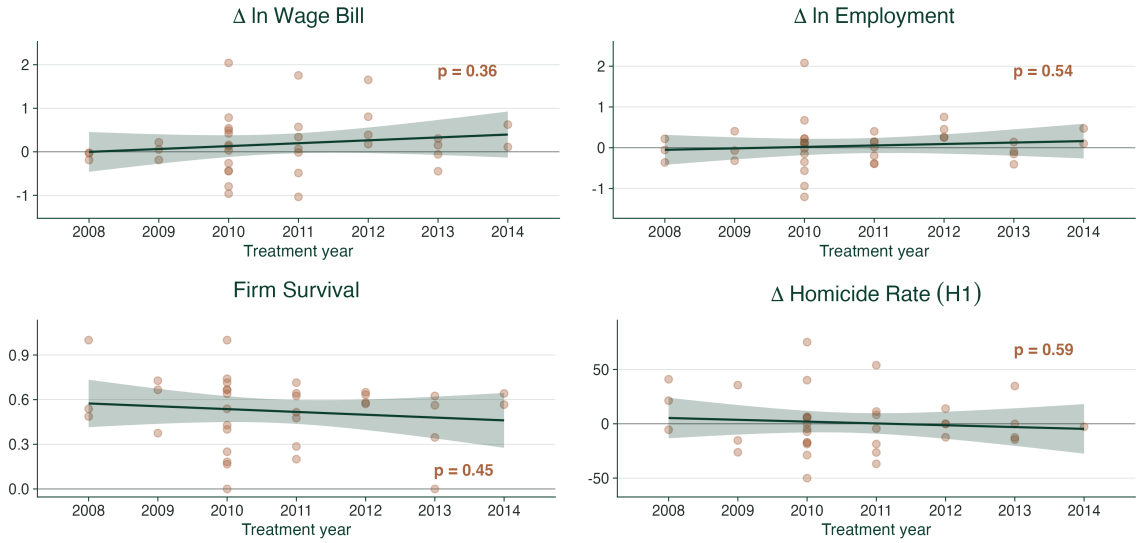
Figure A1: Treated Locations by Treatment Year



Notes: The figure colors UPP areas according to the year in which the policy was implemented.

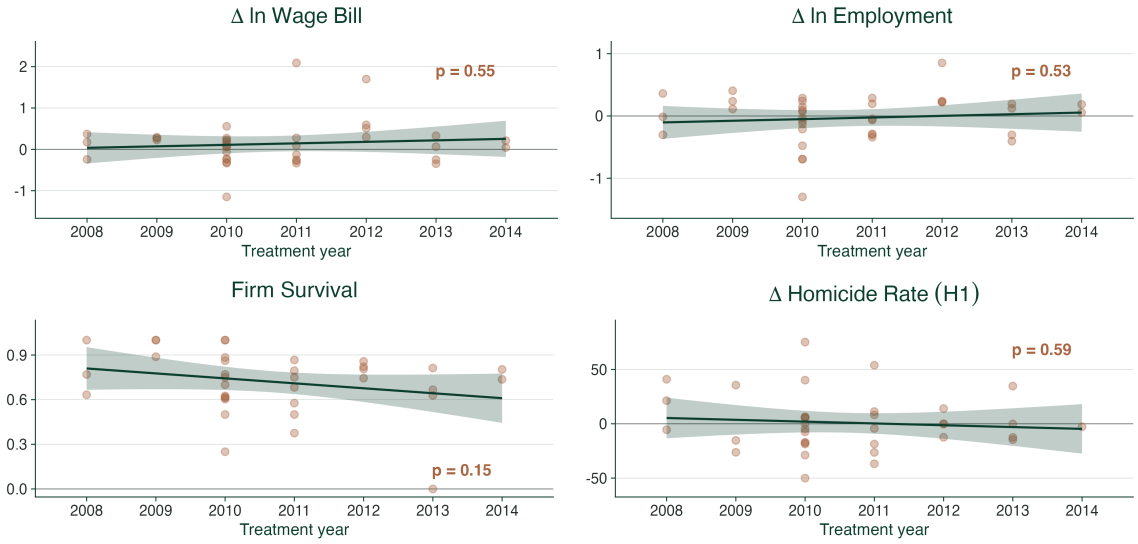
A.2 Pre-Treatment Changes and Treatment Timing: Alternative Windows

Figure A2: Pre-Treatment Changes and Treatment Timing: 2003–2007



Notes: Firm panels compare 2003 with 2007. Survival is measured among firms active in 2003. Homicides compare the first halves of 2007 and 2008. Lines show linear fits with 95 percent confidence intervals. The *p*-values test their slopes.

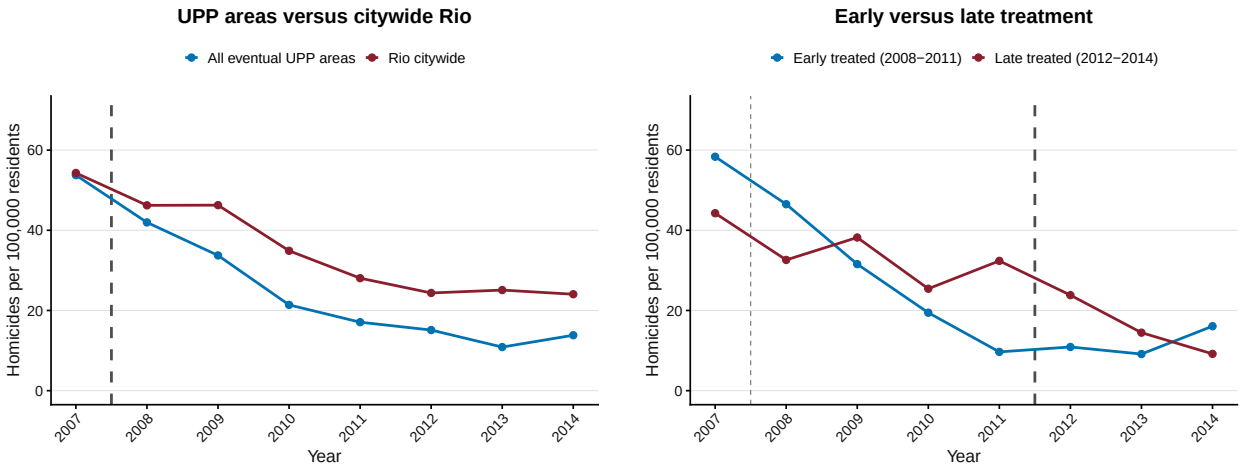
Figure A3: Pre-Treatment Changes and Treatment Timing: 2005–2007



Notes: Firm panels compare 2005 with 2007. Survival is measured among firms active in 2005. Homicides compare the first halves of 2007 and 2008. Lines show linear fits with 95 percent confidence intervals. The *p*-values test their slopes.

A.3 Homicide Trends in UPP Areas and Rio de Janeiro

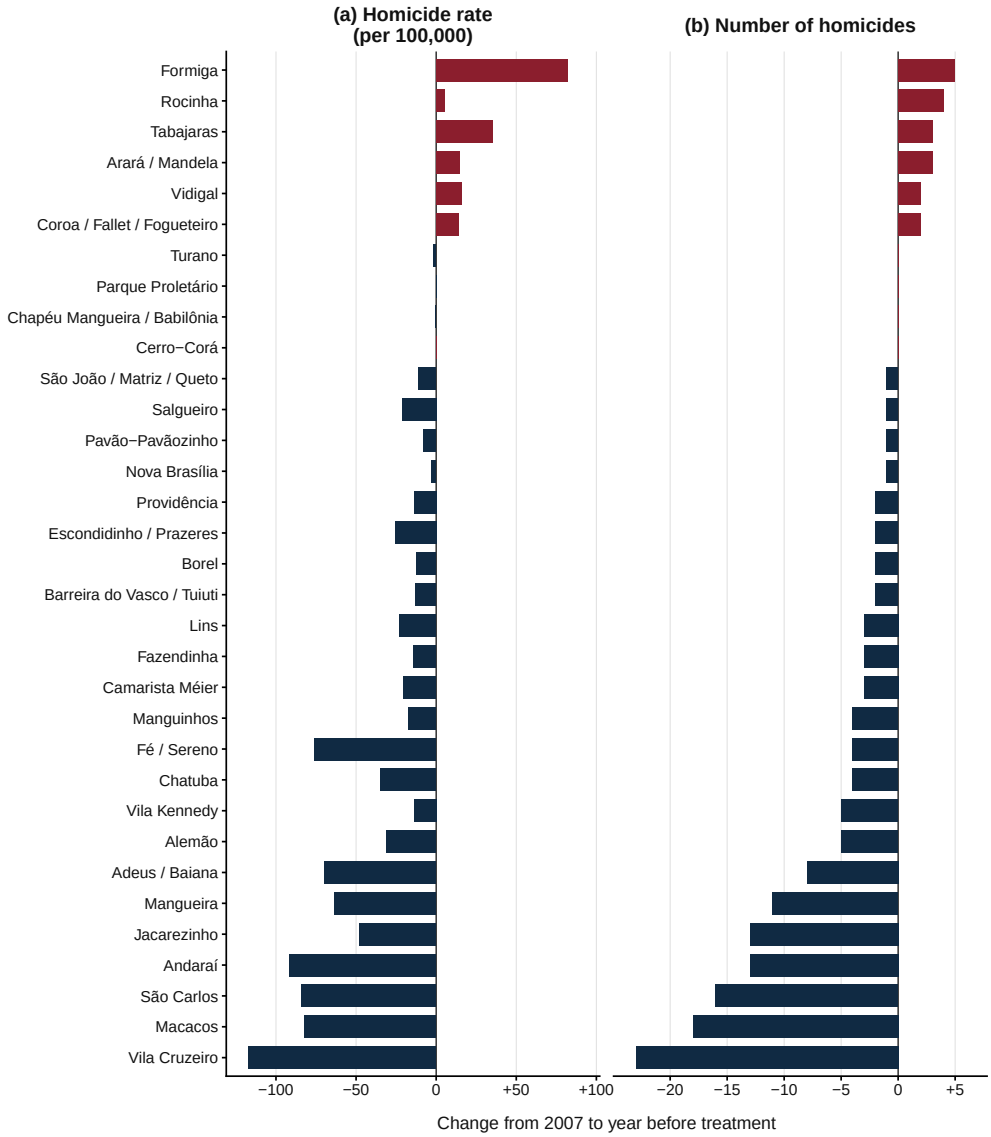
Figure A4: Homicide Rates in UPP Areas and Rio de Janeiro



Notes: Annual homicide rates are per 100,000 residents. The left panel compares UPP areas with Rio de Janeiro as a whole. The right panel compares earlier- and later-treated areas. Dashed lines mark 2008 and 2012.

A.4 Homicide Changes Before Treatment

Figure A5: Changes in Homicides Before Treatment



Notes: For each UPP area, panel (a) reports the change in the homicide rate per 100,000 residents and panel (b) the change in the number of homicides between 2007 and the year immediately preceding treatment. Positive changes are shown in red and negative changes in blue.

A.5 Descriptive Statistics

Table A1: Descriptive Statistics

	Main results sample
Employees	5.7
Wage bill (2016 BRL)	8,272
Years observed in RAIS	4.6
Service share	84.9%
Retail share	40.4%
Manufacturing share	14.7%
Firm-year observations	13,274
Firms	2,914

Notes: The table reports descriptive statistics for the incumbent-firm sample used in the main results: establishments in UPP areas. Firm-level characteristics are averaged at the firm level.

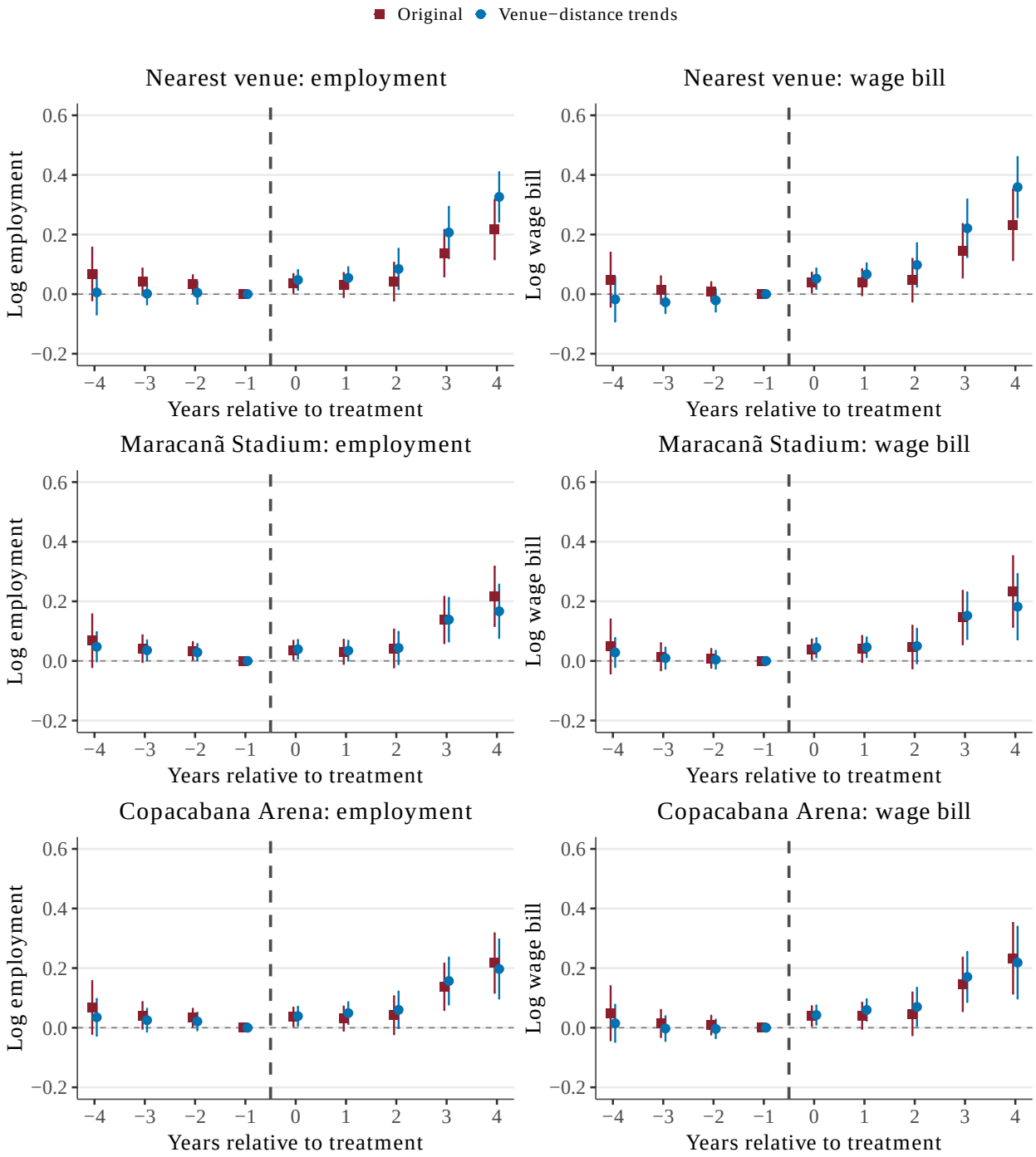
A.6 Sporting-Events Confound

Table A2: Main Growth Effects with Distance-by-Year Controls

	Nearest Venue		Maracanã Stadium		Copacabana Arena	
	Log Emp. (1)	Wage Bill (2)	Log Emp. (3)	Wage Bill (4)	Log Emp. (5)	Wage Bill (6)
Treated	0.074***	0.084***	0.048***	0.057***	0.059***	0.068***
	(0.018)	(0.020)	(0.015)	(0.016)	(0.017)	(0.018)
Observations	12,536	12,536	12,536	12,536	12,536	12,536
Firms	2,201	2,201	2,201	2,201	2,201	2,201

Notes: Columns 1–6 add venue-distance-by-year controls. Distance is measured in kilometers. Models include firm and year fixed effects. Standard errors are clustered by UPP area.

Figure A6: Incumbent Growth with Venue-Distance-by-Year Trends



Notes: Red squares reproduce Figure 5. Blue circles add venue-distance-by-year controls. Bars show 95 percent UPP-clustered confidence intervals.

Table A3: Main Growth Effects Above and Below Median Venue Distance

	Nearest Venue		Maracanã Stadium		Copacabana Arena	
	Log Emp. (1)	Wage Bill (2)	Log Emp. (3)	Wage Bill (4)	Log Emp. (5)	Wage Bill (6)
Treated: below median	0.039*	0.050**	0.015	0.030	0.008	0.018
	(0.021)	(0.023)	(0.020)	(0.024)	(0.016)	(0.019)
Treated: above median	0.102***	0.122***	0.070***	0.076***	0.104***	0.114***
	(0.021)	(0.024)	(0.019)	(0.021)	(0.019)	(0.022)
Above minus below	0.063**	0.073**	0.056**	0.046	0.096***	0.096***
	(0.028)	(0.031)	(0.028)	(0.032)	(0.025)	(0.030)
Observations	12,536	12,536	12,536	12,536	12,536	12,536
Firms	2,201	2,201	2,201	2,201	2,201	2,201

Notes: Medians are computed separately for each venue across the 34 UPP areas: 2.40 km for the nearest venue, 4.43 km for Maracanã, and 9.95 km for Copacabana. Below includes distances at the median. Effects average supported post-treatment cohort-event cells within each group. Models interact the cohort-event treatment effects with the above-median indicator and retain firm, year, and distance-by-year effects. Standard errors for group effects and their difference use the full UPP-clustered covariance matrix.

A.7 Olympic-Venue Distance Robustness

Table A4: UPPs and Distance to the Nearest Olympic Venue

UPP	Year treated	Distance (km)	Nearest Olympic venue	Zone
Batam	2008	0.99	Centro Olímpico de Tiro	Deodoro
Cidade de Deus	2008	2.45	Parque Aquático Maria Lenk	Barra
Santa Marta	2008	2.59	Arena Copacabana	Copacabana
Chapéu Mangueira / Babilônia	2009	0.34	Arena Copacabana	Copacabana
Pavão-Pavãozinho	2009	0.73	Forte de Copacabana	Copacabana
Tabajaras	2009	1.31	Arena Copacabana	Copacabana
Providência	2010	0.90	Sambódromo	Maracanã
Borel	2010	2.52	Ginásio do Maracanãzinho	Maracanã
Formiga	2010	2.91	Ginásio do Maracanãzinho	Maracanã
Andaraí	2010	3.18	Ginásio do Maracanãzinho	Maracanã
Salgueiro	2010	1.30	Ginásio do Maracanãzinho	Maracanã
Turano	2010	1.58	Ginásio do Maracanãzinho	Maracanã
Macacos	2010	1.14	Estádio do Maracanã	Maracanã
Adeus / Baiana	2010	3.85	Estádio Olímpico João Havelange	Maracanã
Alemão	2010	3.82	Estádio Olímpico João Havelange	Maracanã
Fazendinha	2010	2.83	Estádio Olímpico João Havelange	Maracanã
Nova Brasília	2010	3.39	Estádio Olímpico João Havelange	Maracanã
Parque Proletário	2010	3.99	Estádio Olímpico João Havelange	Maracanã
Vila Cruzeiro	2010	4.10	Estádio Olímpico João Havelange	Maracanã
Coroa / Fallet / Fogueteiro	2011	0.91	Sambódromo	Maracanã
Escondidinho / Prazeres	2011	1.87	Sambódromo	Maracanã
São Carlos	2011	0.39	Sambódromo	Maracanã
São João / Matriz / Queto	2011	2.78	Estádio do Maracanã	Maracanã
Mangueira	2011	0.44	Estádio do Maracanã	Maracanã
Rocinha	2011	2.38	Estádio da Lagoa	Copacabana
Vidigal	2011	1.94	Estádio da Lagoa	Copacabana
Chatuba	2012	4.73	Estádio Olímpico João Havelange	Maracanã
Fé / Sereno	2012	4.54	Estádio Olímpico João Havelange	Maracanã
Arará / Mandela	2012	2.32	Estádio do Maracanã	Maracanã
Jacarezinho	2012	2.84	Estádio Olímpico João Havelange	Maracanã
Manguinhos	2012	3.68	Estádio do Maracanã	Maracanã
Barreira do Vasco / Tuiuti	2013	1.30	Estádio do Maracanã	Maracanã
Caju	2013	2.82	Estádio do Maracanã	Maracanã
Cerro-Corá	2013	3.25	Sambódromo	Maracanã
Mangueirinha	2013	10.70	Centro Olímpico de BMX	Deodoro
Camarista Méier	2013	1.93	Estádio Olímpico João Havelange	Maracanã
Lins	2013	2.42	Estádio Olímpico João Havelange	Maracanã
Vila Kennedy	2014	6.14	Centro Olímpico de Tiro	Deodoro
Maré	2014	3.69	Estádio do Maracanã	Maracanã

Notes: Distances are the shortest straight-line distances to Olympic venues. The table identifies each area's nearest venue and venue zone.

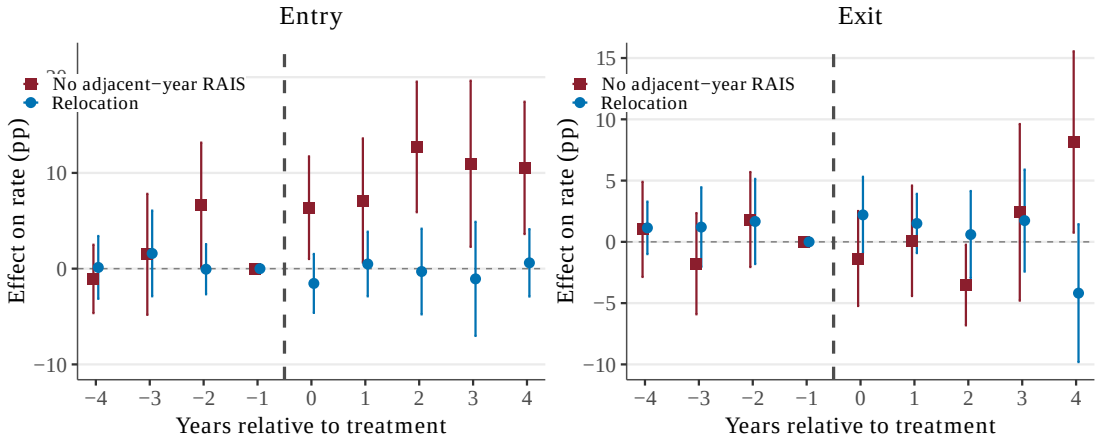
Table A5: Distribution of Distances to Olympic Venues

Distance measure	UPPs	Mean	SD	P25	Median	P75	Min	Max
Nearest Venue	39	2.692	1.875	1.309	2.521	3.534	0.344	10.702
Maracanã Stadium	39	6.101	5.255	2.799	3.959	7.900	0.440	25.536
Copacabana Arena	39	11.021	7.051	6.474	10.144	14.842	0.344	32.894

Notes: Distances are measured in kilometers to the nearest Olympic venue, Maracanã Stadium, and Copacabana Arena.

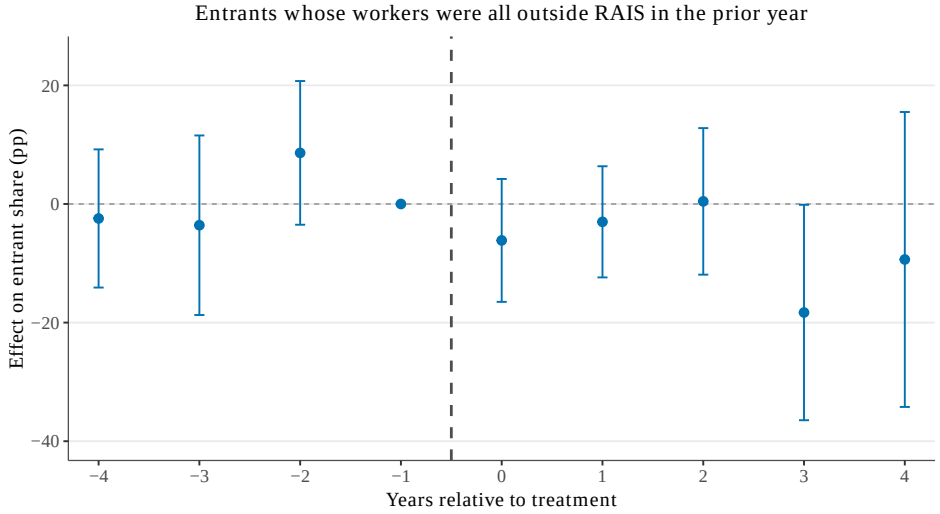
A.8 Relocation versus Other Entry and Exit

Figure A7: Relocation versus "True" Entry and Exit



Notes: The panels split entry and exit into relocations and other flows. A relocation is identified when the establishment appears elsewhere in citywide RAIS in the adjacent year. Each component is divided by the number of firms present in the UPP area in the previous year. Estimates are population-weighted with UPP-area and year fixed effects. Bars show 95 percent UPP-clustered confidence intervals.

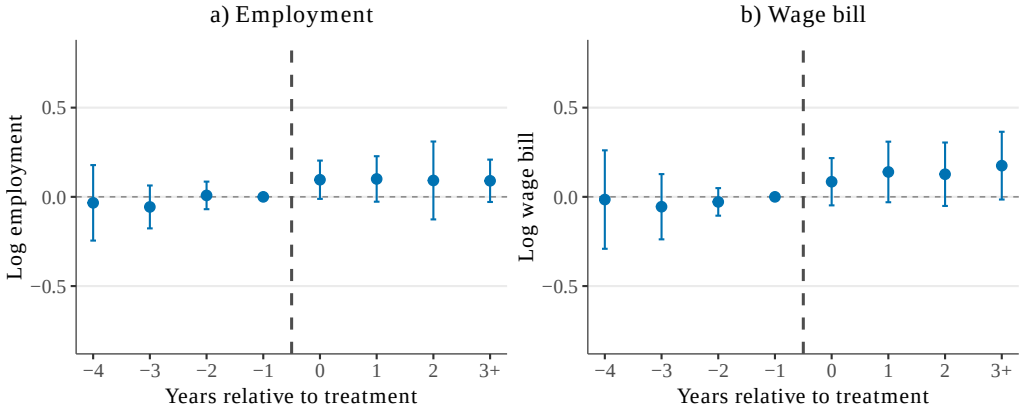
Figure A8: Entrants Whose Workers Were Absent from RAIS in the Prior Year



Notes: An entrant is present in the area in year t but absent in $t-1$. The outcome is the share of entrants whose workers were all absent from citywide RAIS in $t-1$. Area-years without entrants are excluded. Estimates are population-weighted with UPP-area and year fixed effects. Bars show 95 percent UPP-clustered confidence intervals. Prior-year absence from RAIS does not distinguish informal work from labour-market entry or missing records.

A.9 Area-Level Event Studies

Figure A9: Area-Level Effects on Employment and the Wage Bill



Notes: The outcomes sum employment and wage bills across active establishments in each UPP area. Estimates are population-weighted with UPP-area and year fixed effects. Event times three years and later are pooled into the 3+ bin; event times four years before treatment and earlier are pooled at −4. The omitted reference period is −1. Bars show 95 percent UPP-clustered confidence intervals.

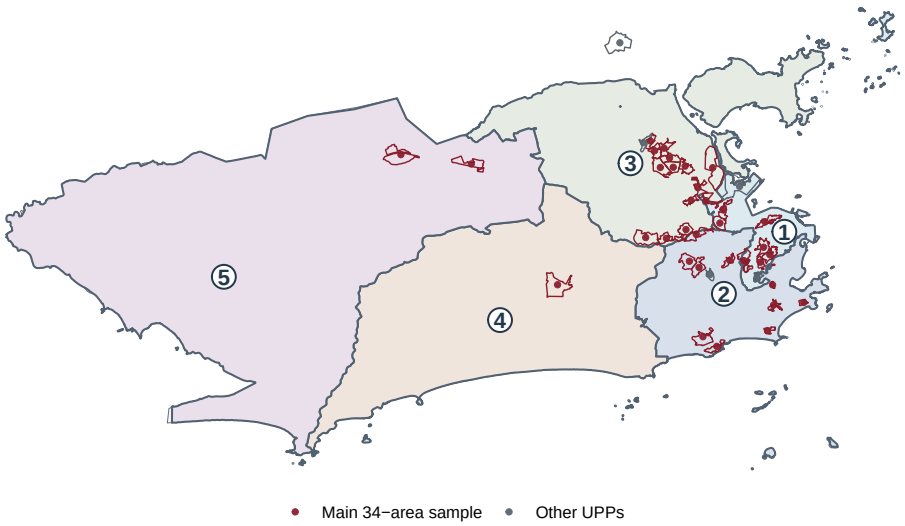
A.10 Administrative-Region Trends

Table A6: Robustness to Administrative-Region-by-Year Fixed Effects

	Main		Administrative Region × Year FE	
	(1) Log Employment	(2) Log Wage Bill	(3) Log Employment	(4) Log Wage Bill
Treated	0.115***	0.140***	0.094**	0.118**
	(0.030)	(0.033)	(0.047)	(0.053)
Percent effect	12.2%	15.0%	9.8%	12.5%
Administrative region × year FE	No	No	Yes	Yes
Observations	12,568	12,568	12,568	12,568
Firms	2,914	2,914	2,914	2,914

Notes: Models include firm, year, and UPP-population-quintile-by-year fixed effects. Columns 3 and 4 add administrative-region-by-year fixed effects, combining Planning Areas 4 and 5. Parentheses show UPP-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

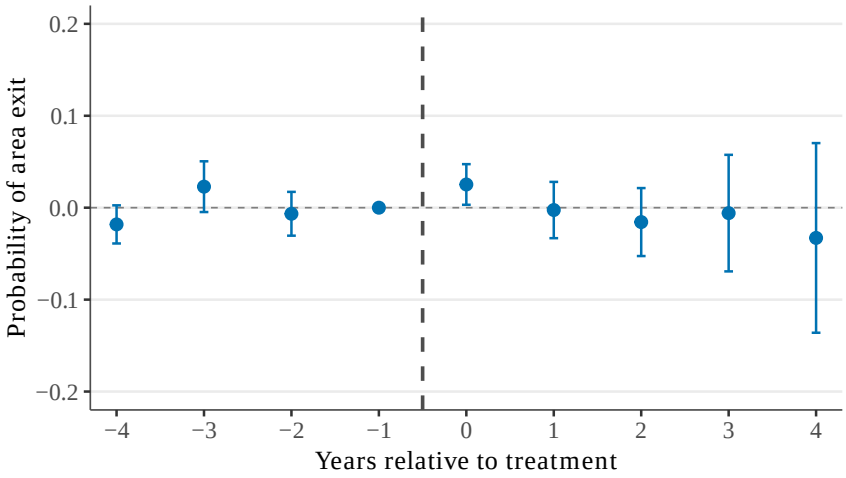
Figure A10: Rio de Janeiro's Planning Areas and UPP Boundaries



Notes: The numbered regions are Rio's five administrative planning areas. Red outlines mark UPPs in the main firm sample. Gray outlines mark the other UPPs. The administrative-region robustness specification groups Planning Areas 4 and 5.

A.11 Firm Attrition and Survivor Panel

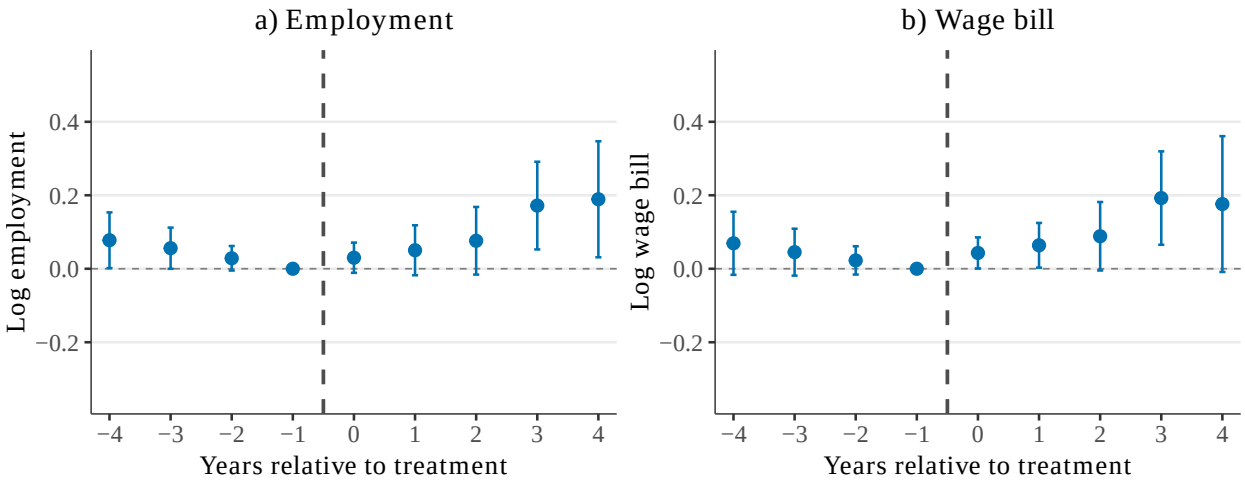
Figure A11: Effects on Exit from the Original UPP Area



Notes: Exit equals no positive formal employment in the original UPP area during the next five years, so relocation outside the area counts as exit. Because the outcome looks five years ahead, pre-treatment coefficients may reflect post-treatment years. Bars show 95 percent UPP-clustered confidence intervals.

The following figure and table repeat the main incumbent-firm specification for firms observed in every year from 2007 through 2013.

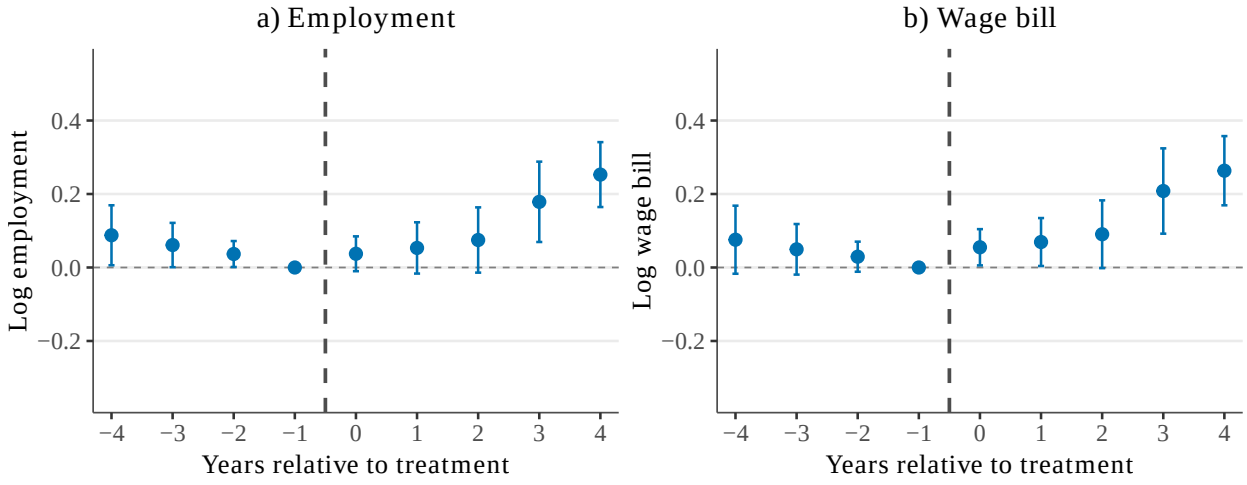
Figure A12: Effects on Incumbent Growth among Firms Observed Annually, 2007–2013



Notes: Firms are observed annually from 2007 through 2013. Earlier observations remain where available. The regression includes firm and year fixed effects. Bars show 95 percent UPP-clustered confidence intervals.

The next figure and Table A8 relax the balanced-panel restriction: firms must be observed in both 2007 and 2013, but can be absent in intervening years. This adds 27 firms. The average post-treatment estimates remain similar, but the $t = 5$ estimates rise sharply. One added firm in the 2008 cohort accounts for nearly all of that change: it had one worker in 2007, no eligible observations in 2008–2011, and 24 workers in 2013. Excluding this firm reduces the $t = 5$ estimates to 0.074 for employment and 0.037 for the wage bill. The endpoint-only estimates therefore do not establish sustained growth among continuously operating incumbents.

Figure A13: Effects on Incumbent Growth among Firms Observed in 2007 and 2013



Notes: Firms are observed in 2007 and 2013 but may be absent in intervening years. All available observations are retained. Bars show 95 percent UPP-clustered confidence intervals.

Table A7: Effects on Incumbent Growth among Firms Observed Annually, 2007–2013

	Log Employment (1)	Log Wage Bill (2)
Average post-treatment effect	0.074	0.086
	(0.030)**	(0.030)***
	[0.028]***	[0.029]***
Effect at $t \geq 4$	0.189	0.176
	(0.081)**	(0.094)*
	[0.078]***	[0.093]**
Joint pre-treatment p-value	0.195	0.445
Observations	5,500	5,500
Firms	545	545

Notes: Firms are observed annually from 2007 through 2013. Earlier observations remain where available. Models include firm and year fixed effects. Parentheses show UPP-clustered standard errors. Brackets show wild-cluster bootstrap standard errors using 999 Rademacher draws. Stars use the corresponding inference method. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Effects on Incumbent Growth among Firms Observed in 2007 and 2013

	Log Employment (1)	Log Wage Bill (2)
Average post-treatment effect	0.081	0.099
	(0.029)***	(0.030)***
	[0.028]***	[0.028]***
Effect at $t = 5$	0.172	0.153
	(0.080)**	(0.081)*
	[0.080]***	[0.079]**
Joint pre-treatment p-value	0.020	0.072
Observations	5,678	5,678
Firms	572	572

Notes: Firms are observed in 2007 and 2013 but may be absent in intervening years. Models include firm and year fixed effects. Parentheses show UPP-clustered standard errors. Brackets show wild-cluster bootstrap standard errors using 999 Rademacher draws. Stars use the corresponding inference method. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

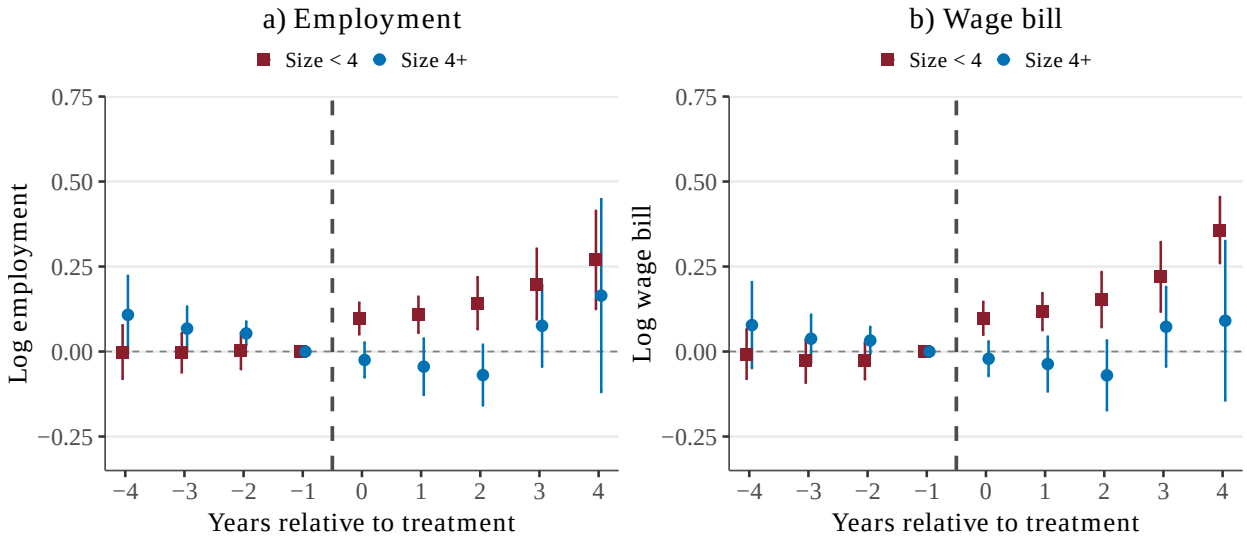
A.12 Size Heterogeneity

Table A9: Effects on Incumbent Growth by Baseline Firm Size

	Log Employment (1)	Log Wage Bill (2)
Average post-treatment effect		
Size < 5	0.111^{***}	0.118^{***}
	(0.024)	(0.025)
Size ≥ 5	-0.027	-0.022
	(0.039)	(0.040)
Size < 5 minus size ≥ 5	0.138^{***}	0.141^{***}
	(0.050)	(0.049)
Effect at event time 5		
Size < 5	0.268	0.245
	(0.186)	(0.158)
Size ≥ 5	0.111	0.088
	(0.168)	(0.170)
Size < 5 minus size ≥ 5	0.157	0.157
	(0.344)	(0.313)
Joint pre-treatment p, size < 5	0.912	0.187
Joint pre-treatment p, size ≥ 5	0.004	0.121
Observations	12,536	12,536
Firms	2,201	2,201

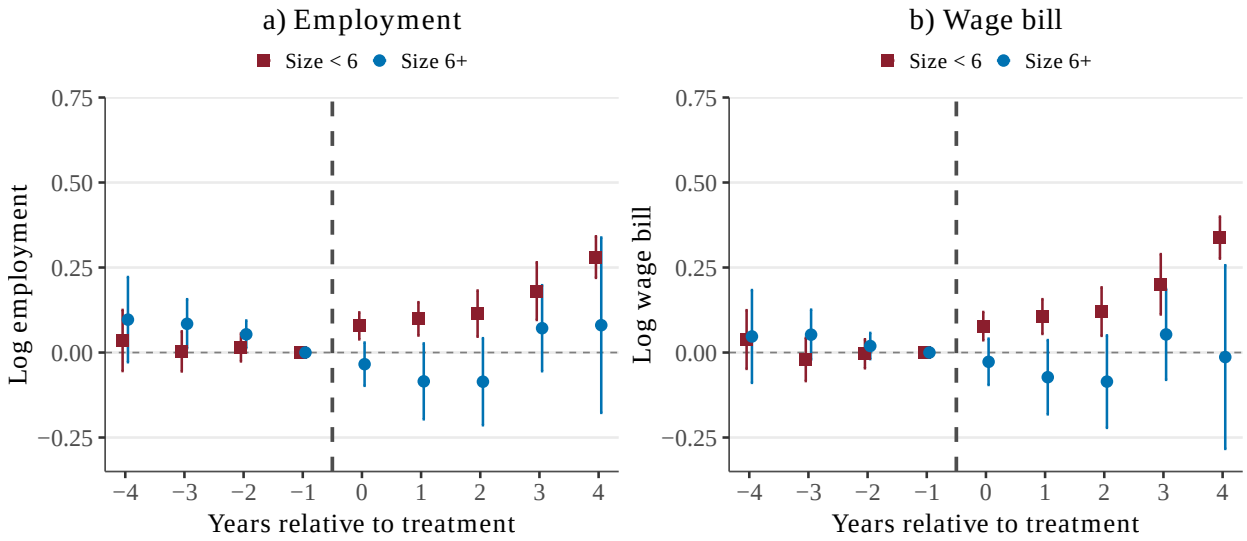
Notes: Firms are split by median pre-treatment employment. The interaction is the difference between the two groups' average post-treatment effects from a fully interacted model. Parentheses show UPP-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A14: Incumbent Growth by Size: Four-Worker Cutoff



Notes: Firms are split at a median pre-treatment employment of four workers. The specification matches Figure 5. Bars show 95 percent UPP-clustered confidence intervals.

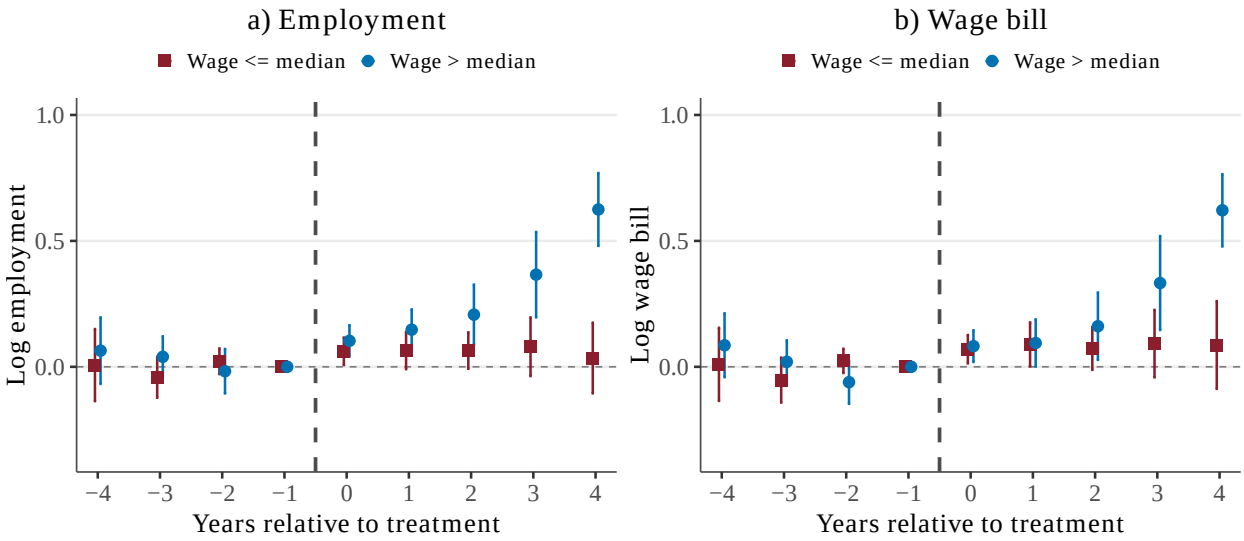
Figure A15: Incumbent Growth by Size: Six-Worker Cutoff



Notes: Firms are split at a median pre-treatment employment of six workers. The specification matches Figure 5. Bars show 95 percent UPP-clustered confidence intervals.

A.13 Baseline-Wage Heterogeneity

Figure A16: Incumbent Growth by Baseline Wage with Sector-by-Year Effects



Notes: This figure adds sector-by-year fixed effects to Figure 9. Sectors are retail, other services, and manufacturing/other non-services. Bars show 95 percent UPP-clustered confidence intervals.

Table A10: Characteristics of Small Incumbents by Pre-Treatment Wage

	Wage $\leq$ median	Wage $>$ median	p-value
Panel A. Firm characteristics			
Mean baseline wage (2016 R$)	802	1,476	< 0.001
Median baseline wage (2016 R$)	804	1,202	< 0.001
Mean baseline employment	2.01	2.20	< 0.001
Median baseline employment	2	2	1.000
Mean years observed in RAIS, 2003–2013	5.47	5.04	0.009
Panel B. Sector			
Retail (%)	54.1	38.2	< 0.001
Non-retail services (%)	37.6	46.8	< 0.001
Manufacturing (%)	6.3	10.7	0.004
Construction/other non-services (%)	2.0	4.3	0.019
Panel C. Workforce characteristics			
Mean worker age (years)	33.6	37.1	< 0.001
Male workers (%)	62.5	71.1	< 0.001
High school or more (%)	27.9	43.1	< 0.001
Some college or more (%)	2.1	12.1	< 0.001
College degree or more (%)	1.0	9.2	< 0.001
Firms	652	680	

Notes: Firms have fewer than five workers at baseline and are split at the median pre-treatment average wage (R$953 in 2016 prices). Worker-row p-values are clustered by firm.

A.14 Informality

Table A11: Effect on Prior Formal Employment Among New Hires

	2004–2013		2007–2013	
	No Controls	Controls	No Controls	Controls
Treated	-0.008	-0.012	-0.005	-0.010
	(0.015)	(0.015)	(0.014)	(0.014)
Baseline mean (2007)	0.190	0.190	0.190	0.190
Worker-years	23,714	23,714	18,133	18,133
Workers	23,014	23,014	17,719	17,719
Firms	1,738	1,738	1,454	1,454

Notes: The outcome indicates whether a new hire held a formal job at another establishment in the previous year. A zero does not distinguish informal work from nonemployment. Models include UPP-area and year fixed effects. Controlled columns add age, age squared, gender, education, and race. Standard errors are clustered by UPP area and worker.

A.15 Customer-Facing Sector Classification

Table A12: Claude Classification of the 50 Most Common Five-Digit CNAE Sectors

CNAE	Sector	Class.	Hard	Firms	Share
52442	Retail trade of construction materials, hardware and hand tools, glass, mirrors and stained glass, paints, and wood	C-facing	No	148	5.1%
52132	Retail trade of general merchandise, mainly food products, in stores under 300 square meters, excluding convenience stores	C-facing	No	147	5.0%
52418	Retail trade of pharmaceuticals, medical and orthopedic goods, perfumes, and cosmetics	C-facing	No	143	4.9%
52493	Retail trade of other products not elsewhere classified	C-facing	No	112	3.8%
52329	Retail trade of clothing and accessories	C-facing	No	102	3.5%
74993	Other business services not elsewhere classified	Non-C	No	92	3.2%
60267	General road freight transport	Non-C	No	86	3.0%
52213	Retail trade of bakery products, dairy products, cold cuts, and preserves	C-facing	No	76	2.6%
55220	Snack bars and similar establishments	C-facing	No	76	2.6%
52434	Retail trade of furniture, lighting fixtures, and other household goods	C-facing	No	73	2.5%
52299	Retail trade of other food products not elsewhere classified and tobacco products	C-facing	No	63	2.2%
50300	Retail and wholesale trade of motor-vehicle parts and accessories	C-facing	Yes	62	2.1%
55212	Restaurants and full-service beverage establishments	C-facing	No	60	2.1%
50202	Maintenance and repair of motor vehicles	C-facing	No	53	1.8%
55247	Prepared food services	C-facing	No	44	1.5%
45500	Finishing construction works	Non-C	No	39	1.3%
52248	Retail trade of beverages	C-facing	No	37	1.3%
52450	Retail trade of office, computing, and communication equipment and supplies	C-facing	No	37	1.3%
45217	Building construction, residential, industrial, commercial, and services	Non-C	No	36	1.2%
52230	Retail trade of meat, butcher shops	C-facing	No	34	1.2%
51390	Wholesale trade of other food products not elsewhere classified	Non-C	No	27	0.9%
52469	Retail trade of books, newspapers, magazines, and stationery	C-facing	No	27	0.9%
52710	Repair and maintenance of machines and household appliances	C-facing	No	27	0.9%
52337	Retail trade of footwear, leather goods, and travel goods	C-facing	No	26	0.9%
51330	Wholesale trade of fruit, vegetables, and farm products	Non-C	No	25	0.9%
51535	Wholesale trade of wood, construction materials, hardware, and tools	Non-C	No	25	0.9%
93025	Hairdressing and other beauty treatments	C-facing	No	25	0.9%
18120	Manufacture of apparel, except underwear, blouses, shirts, and similar items	Non-C	No	24	0.8%
50504	Retail sale of fuels	C-facing	No	23	0.8%
74209	Architectural, engineering, and specialized technical advisory services	Non-C	No	23	0.8%
80152	Elementary education	C-facing	No	23	0.8%
15814	Manufacture of bakery, confectionery, and pastry products	Non-C	No	20	0.7%
51594	Wholesale trade of other non-agricultural intermediate products not elsewhere classified	Non-C	No	20	0.7%
52310	Retail trade of fabrics and haberdashery items	C-facing	No	20	0.7%
52221	Retail trade of candy, chocolate, and similar products	C-facing	No	19	0.7%
64122	Courier and delivery activities	Non-C	No	19	0.7%
85138	Outpatient care activities	C-facing	No	18	0.6%
45292	Other construction works	Non-C	No	17	0.6%
51918	Wholesale trade of general merchandise, non-specialized	Non-C	No	17	0.6%
51926	Specialized wholesale trade of merchandise not elsewhere classified	Non-C	No	17	0.6%
63215	Auxiliary activities for land transport	Non-C	No	17	0.6%
92622	Other leisure-related activities	C-facing	No	17	0.6%
51551	Wholesale trade of waste and scrap	Non-C	No	16	0.5%
51691	Wholesale trade of machinery, devices, and equipment for industrial, technical, professional, and other uses not elsewhere classified	Non-C	No	16	0.5%
52477	Retail trade of liquefied petroleum gas (LPG)	C-facing	No	16	0.5%
52795	Repair of other personal and household goods	C-facing	No	16	0.5%
74705	Immunization, sanitation, and cleaning activities in buildings and homes	Non-C	No	16	0.5%
93092	Other personal services not elsewhere classified	C-facing	No	16	0.5%
60259	Non-regular passenger road transport	Non-C	Yes	15	0.5%
52426	Retail trade of household and personal-use machines and appliances, records, and musical instruments	C-facing	No	14	0.5%

Notes: The table lists the most common five-digit CNAE classes. Customer-facing status was classified using Claude.

Table A13: Five-Digit CNAE Sectors Flagged as Hard to Classify by Claude

CNAE	Sector	Class.	Firms	Claude rationale
50300	Retail and wholesale trade of motor-vehicle parts and accessories	C-facing	62	Dealership-type trade spanning both retail showroom and wholesale/fleet sales
60259	Non-regular passenger road transport	Non-C	15	Passenger transport - service used in person, but usage-based rather than classic retail "footfall", not discretionary shopping footfall
91995	Other membership-organization activities not elsewhere classified	Non-C	12	Membership/trade association - dues-based, some in-person events but not footfall revenue
71404	Rental of personal and household goods	C-facing	9	Consumer rental counter - customers show up in person to rent/return
63304	Travel agency and tour-operator activities	C-facing	8	Travel agency - traditionally walk-in storefront, increasingly online
65226	Multiple banks with commercial portfolios	Non-C	8	Retail/commercial banking - branch network has some in-person reliance, but most revenue (interest/fees) is not footfall-driven
91910	Religious-organization activities	Non-C	6	Religious organization - in-person attendance matters but not commercial "revenue" in the usual sense
50105	Retail and wholesale trade of motor vehicles	C-facing	5	Dealership-type trade spanning both retail showroom and wholesale/fleet sales
60232	Regular urban passenger road transport	Non-C	5	Passenger transport - service used in person, but usage-based rather than classic retail "footfall", not discretionary shopping footfall
80993	Other education activities	Non-C	5	Postgrad/technical/other courses - often shorter, exam-prep, or partly remote-capable formats
74918	Photographic activities	C-facing	4	Photo studio - typically serves individual clients in person
60240	Regular non-urban passenger road transport	Non-C	3	Passenger transport - service used in person, but usage-based rather than classic retail "footfall", not discretionary shopping footfall
50415	Retail and wholesale trade of motorcycles, parts, and accessories	C-facing	2	Dealership-type trade spanning both retail showroom and wholesale/fleet sales
62103	Regular air transport	Non-C	2	Passenger transport - service used in person, but usage-based rather than classic retail "footfall", not discretionary shopping footfall
64114	National postal-service activities	Non-C	2	Postal service - walk-in counters exist, but much volume is not in-person
91200	Labor-union organization activities	Non-C	2	Membership/trade association - dues-based, some in-person events but not footfall revenue
65242	Credit cooperatives	Non-C	1	Retail/commercial banking - branch network has some in-person reliance, but most revenue (interest/fees) is not footfall-driven
70319	Real estate brokerage and appraisal	C-facing	1	Real estate brokerage - client-facing but not classic footfall (by appointment/showings)
71102	Automobile rental	C-facing	1	Consumer rental counter - customers show up in person to rent/return
80314	Higher education, undergraduate programs	C-facing	1	Undergraduate higher ed - substantial in-person campus reliance
80969	Technical vocational education	Non-C	1	Postgrad/technical/other courses - often shorter, exam-prep, or partly remote-capable formats
91120	Professional-organization activities	Non-C	1	Membership/trade association - dues-based, some in-person events but not footfall revenue
95001	Domestic services	Non-C	1	Domestic (household) employment - not a customer-revenue business at all

Notes: The table lists five-digit CNAE classes flagged as hard to classify when constructing the customer-facing indicator.

A.16 Alternative Wage Samples

Table A14: Treatment Effect on Log Wage: New Hires, Early Cohorts

	2004–2013 Sample		2007–2013 Sample	
	No Controls (1)	Controls (2)	No Controls (3)	Controls (4)
Treated	0.098***	0.061**	0.095***	0.054*
	(0.025)	(0.028)	(0.027)	(0.028)
Worker-years	23,737	23,737	18,144	18,144
Workers	23,035	23,035	17,730	17,730
Firms	1,744	1,744	1,459	1,459

Notes: The outcome is log wages for new hires at incumbent firms. Controlled columns add age, age squared, gender, education, and race. Column 4 also adds experience controls. Models include UPP-area, year, and UPP-population-quintile-by-year fixed effects. Standard errors are clustered by UPP area and worker.

Table A15: Treatment Effect on Log Wage: All Workers, Early Cohorts

	2004–2013 Sample		2007–2013 Sample	
	No Controls (1)	Controls (2)	No Controls (3)	Controls (4)
Treated	0.034**	0.017	0.034**	0.010
	(0.015)	(0.020)	(0.016)	(0.020)
Worker-years	78,143	78,143	57,084	57,084
Workers	34,693	34,693	27,338	27,338
Firms	2,719	2,719	2,187	2,187

Notes: The outcome is log wages for all workers at incumbent firms. Controlled columns add age, age squared, gender, education, and race. Column 4 also adds experience controls. Models include UPP-area, year, and UPP-population-quintile-by-year fixed effects. Standard errors are clustered by UPP area and worker.

Table A16: Worker-Level Wage Effects with RAIS Work-History Controls

	Area and Year FE		Area, Year, and Firm FE	
	Worker Controls (1)	+ RAIS History (2)	Worker Controls (3)	+ RAIS History (4)
Treated	0.039***	0.034***	−0.007	−0.009
	(0.010)	(0.010)	(0.009)	(0.009)
95% CI	[0.019, 0.060]	[0.014, 0.054]	[−0.026, 0.012]	[−0.027, 0.010]
Worker-years	13,816	13,816	13,816	13,816
Workers	6,409	6,409	6,409	6,409
Firms	1,222	1,222	1,222	1,222

Notes: The outcome is worker-level log wages. Worker controls include age, age squared, gender, education, and race. RAIS-history controls are prior-year formal employment and years observed in RAIS over the preceding four years. Models include UPP-area and year fixed effects. Columns 3 and 4 also include firm fixed effects. Parentheses show standard errors clustered by UPP area and worker. *** $p < 0.01$.

A.17 Derivation and Sensitivity of the Channel Decomposition

Let subscripts 0 and 1 denote the states before and after treatment. Define $W = \Delta \ln w^*$, $T = \Delta \ln(1-\tau)$, and $U = \Delta \ln S$. The sector-specific labor-demand conditions in Equation (6) imply

$$\Delta \ln n_{NCF} = \gamma(T - W), \qquad q \equiv \Delta \ln n_{CF} - \Delta \ln n_{NCF} = \gamma \Delta \ln D. \tag{12}$$

Thus, q identifies a shift in customer-facing demand relative to the other sector. It does not assume that the customer response is zero.

Write $H(c) = M_{NCF} + M_{CF} D(c)^{\gamma}$. If $s = M_{CF} D_0^{\gamma} / H_0$ is the baseline customer-facing employment share, the change in aggregate customer demand is

$$Q \equiv \Delta \ln H = \ln\left(\frac{M_{NCF} + M_{CF} D_1^{\gamma}}{M_{NCF} + M_{CF} D_0^{\gamma}}\right) = \ln(1 - s + s e^{q}). \tag{13}$$

Taking log differences of the equilibrium wage in Equation (8) gives

$$(\gamma + \varepsilon) W = \gamma T + Q - U. \tag{14}$$

Combining this equation with non-customer-facing labor demand recovers both shifts:

$$T = W + \frac{\Delta \ln n_{NCF}}{\gamma}, \tag{15}$$

$$U = \Delta \ln n_{NCF} - \varepsilon W + Q. \tag{16}$$

The calculation maintains a fixed set of incumbent firms, a common local wage, and the normalization that $D(c)$ is the relative customer-facing demand shifter.

Substituting Equation (14) into the sectoral labor-demand conditions decomposes employment growth:

$$\Delta \ln n_{NCF} = \underbrace{\frac{\gamma\varepsilon}{\gamma+\varepsilon} T}_{C_\tau} + \underbrace{\frac{\gamma}{\gamma+\varepsilon} U}_{C_S} - \underbrace{\frac{\gamma}{\gamma+\varepsilon} Q}_{C_{D,NCF}}, \tag{17}$$

$$\Delta \ln n_{CF} = C_\tau + C_S + \underbrace{\left(-\frac{\gamma}{\gamma+\varepsilon} Q + q\right)}_{C_{D,CF}}. \tag{18}$$

The $C_{D,NCF}$ term is an indirect wage-equilibrium effect of customer-facing demand on other firms. Their own demand shifter is normalized to one.

The benchmark uses the Main classification in Column 1 of Table 2: $\Delta \ln n_{NCF} = 0.14325$ and $q = -0.03786$, implying $\Delta \ln n_{CF} = 0.10539$. The pre-treatment customer-facing employment share in the sector-regression sample is $s = 0.58430$. The wage input is $W = -0.02546$ (standard error 0.01554, $p = 0.111$), estimated among 6,141 new hires at 1,037 small incumbent firms in a worker-level event-study regression with age, age-squared, gender, education, and race controls, UPP-area and year fixed effects, and firm fixed effects. This differs from the all-worker estimate in Table 3. Using it as a common equilibrium wage change is a calibration assumption. Unobserved changes in hire composition may also affect the estimate.

At $\gamma = 3$ and $\varepsilon = 2.7$, the inputs yield $Q = -0.02195$, $T = 0.02229$, and $U = 0.19005$. Table A17 reports the implied employment contributions. For customer-facing firms, the positive-share denominator is $C_\tau + C_S = 0.1317$ log points. The negative customer-demand contribution remains in the net total. Non-customer-facing shares divide by net growth.

Table A17: Model-Implied Employment Contributions Among Small Incumbents

	Non-Customer-Facing		Customer-Facing	
Channel	Log points	Net share	Log points	Positive share
Labor-supply shift	0.1000	70%	0.1000	76%
Crime-tax relief	0.0317	22%	0.0317	24%
Customer-demand shift	0.0116	8%	−0.0263	—
Total employment effect	0.1432	100%	0.1054	—

Notes: Contributions sum to each sector's estimated log-employment effect, apart from rounding. They are model-based functions of the estimates, baseline employment share, and calibrated elasticities.

The local labor-supply elasticity is not estimated in this paper. Table A18 varies ε while holding $\gamma = 3$ and the estimated inputs fixed. Tax relief $T = W + \Delta \ln n_{NCF}/\gamma$ does not change with ε, but the supply shift and contribution shares do.

Table A18: Sensitivity to the Labor-Supply Elasticity

ε	$\Delta \ln S$	Supply share of NCF net growth	Supply share of positive CF contributions
2.0	0.172	72%	79%
2.5	0.185	70%	77%
2.7	0.190	70%	76%
3.0	0.198	69%	75%
5.0	0.249	65%	69%
10.0	0.376	61%	63%

Table A19 varies γ at $\varepsilon = 2.7$. The recovered supply shift U remains 0.190, while tax relief and its employment contribution fall as labor demand becomes more responsive.

Table A19: Sensitivity to the Labor-Demand Parameter

γ	$\alpha = 1 - 1/\gamma$	$\Delta \ln(1-\tau)$	Supply share of NCF net growth	Supply share of positive CF contributions
2	0.50	0.0462	56%	60%
3	0.67	0.0223	70%	76%
4	0.75	0.0104	79%	87%
5	0.80	0.0032	86%	96%

These are calibrated point estimates rather than precisely estimated structural contributions. A paired, 34-UPP Rademacher wild-cluster bootstrap of the sector employment and new-hire wage regressions gives percentile 95 percent intervals of $[0.094, 0.277]$ for $\Delta \ln S$ and $[-0.018, 0.058]$ for $\Delta \ln(1-\tau)$ at the benchmark elasticities. The tax-relief interval includes zero.