Rapid Transit, Labor Market, and Business Growth: The case of São Paulo, Brazil[1]

Arthur Bazolli Alvarenga[2 3]

Admir Antonio Betarelli Junior[2]

Ricardo da Silva Freguglia[2]

June 2024

Abstract

The São Paulo Metropolitan Region (RMSP) underwent a series of rapid transit investments since the mid-2000s that increased the region's rail network by around 20 percent. We examine the impact of this expansion on wages, employment, and the emergence of new firms using unique geolocated employer-employee data from an annual administrative survey conducted by the Brazilian Ministry of Labor from 2006 to 2017. Our analysis employs a staggered difference-in-differences approach to assess these effects. Firms within a 30-minute walking distance to a new station are considered treated, while those between 31 and 60 minutes away serve as the control group. To account for spatial differences, we estimate the impact separately for each transit line. Our results indicate an 11.3 percent increase in employment and a 9 percent rise in new firms, with no significant change in wages. The average number of workers per firm remained constant, suggesting that employment gains stemmed from new companies rather than the expansion of existing ones. The effects varied between transit lines, with the most substantial impacts observed in the Line 9 region. Beyond providing unique empirical evidence for São Paulo, this study contributes a framework for applying the novel staggered Difference-in-Differences (DiD) method in the context of transit policy, emphasizing spatial characteristics.

Keywords: Agglomeration economies, labor market, rapid transit.

JEL Codes: R41, J61, O18.

---

[1] We thank the Brazilian agencies FAPEMIG, CAPES, and CNPq for financial support, the Brazilian Ministry of Labor for providing us access to the database, and data technical support of ECONS—Economics Research Laboratory from UFJF.

[2]Department of Economics, Universidade Federal de Juiz de Fora (PPGE/UFJF).

[3] Corresponding author, e-mail: baarthur0@outkook.com

1 Introduction

Cities have long been investigated as powerhouses of economic growth, giving rise to theories on agglomeration economies—increasing returns associated with urbanization and density (Duranton and Puga, 2004; Krugman, 1991)—but they can only grow as far as transport infrastructure catches up; therefore, transport lies at the core of agglomeration economies: Local labor markets are fragmented when workers cannot reach employment centers due to long commutes or when job searching is too expensive (Duranton, 1998; Gobillon, Selod and Zenou, 2007; White, 1999). Contemporary works hypothesize clear causal paths from agglomeration mechanisms to wages, output, and welfare through microfounded models, often unifying labor and real estate markets. Duranton and Puga (2004) divide Marshall's (1890) three broad agglomeration sources—sharing, matching, and learning—into separate mechanisms that can also take place in different stances. They can all relate to rapid transit improvements, but the effect is the clearest for labor sharing and firm-worker matching.

Consider a rapid transit investment that increases the number of workers that can reach a given location (that is, increases the labor pool). First, this leads to greater returns in labor-intensive intermediary input firms and an increase in the variety of such inputs, a concept similar to Jacobs' (1969) argument of greater product diversity in denser environments. These inputs are used in the final goods industry, which becomes more productive when they have more inputs and suppliers. Second, this accessibility improvement also increases the probability of workers finding firms that match their abilities (greater match probability), since both daily commute becomes feasible for a larger part of the population and the job search process becomes less costly.

At the same time, the quality of the match increases, e.g. when the number of workers versed in a very specific task becomes available with the increased pool. While the economic impact of transport infrastructure in the urban environment has accessibility gains as its main transmission channel, these investments also signal to agents which regions are prioritized by the government in terms of urban transformation; as a result, some economic growth (such as real estate development) stem from signaling rather than changes in accessibility (Credit, 2019). This is a purely external benefit (i.e., external to the firm), whereas agglomeration economies imply that the rest of the agents also enjoy accessibility-led gains (e.g., larger consumer market) in a connected urban system.

Empirical evidence backs up the theory: a meta-analysis of 189 studies found that a 10 percent increase in economic density is associated on average with annual *per capita* welfare gains of US$270 and losses of US$93—over a third in congestion costs, and an additional US$240 burden for individuals living under rent. While most estimates correspond to the urban reality of developed nations, the scarce quantitative literature on emerging economies points to higher negative externalities, a consequence of poor infrastructure such as irregular or limited transport networks (Ahlfeldt et al., 2018).

In the Brazilian context, investment in transport infrastructure has failed to keep pace with urbanization trends: Between 2011 and 2021, the country's largest metropolitan regions experienced a seven percent to 20 percent population growth. The motorization rate, in turn, increased between 22 percent and 57 percent. In all metropolitan regions in the country, no more than 20 percent of the population lives near medium- and high-capacity public transport stations. In the biggest cities, investments summing to R$ 454 billion (adjusted for inflation) are necessary for public transport to attend to commuting needs and reach decarbonization policies (Santos et al., 2015). Although the country's history of low investment in rapid transit results from fiscal restrictions and policies prioritizing the automobile, there have been efforts to include public transport in the federal budget since the 2000s, especially after Brazil was announced as the host of the 2014 World Cup. The most notable example is the São Paulo Metropolitan Region (RMSP), South America's most developed financial center and the world's seventh-largest urban area (United Nations, 2019). From 2006 to 2019, the region gained 31 new rapid transit stations in 62 kilometers, distributed in seven heavy rail lines and one monorail line.

Despite the relevance of São Paulo's local labor market, few studies have analyzed the economic impact of its transport network. Haddad et al. (2015) estimated the impact of the network on the GDP through a general equilibrium approach, whereas Boisjoly (2017) provided an analysis of the relationship between access to opportunities, informality, and employment likelihood. No study, however, analyzed the impact of specific rapid transit interventions on labor market outcomes. We contribute to closing this gap by analyzing the impact on wages, employment, and the number of firms of four interventions between 2006 and 2017: the eastbound expansion in the subway (Metrô) Line 2, the new subway Line 4, infill stations in regional metro (CPTM) Line 12, and the southbound expansion in CPTM Line 9. Although they were constrained to the limits of the capital, some of them are close to other municipalities in RMSP, which were also included.

Our data come from the official government employer-employee dataset, which covers formal job relationships and allows the construction of a counterfactual scenario. based on firms located farther away from transit stations. After geolocating addresses, our estimation strategy involves selecting firms for the treatment category if their walking distance to a new station is up to 30 minutes. Those between 31 and 60 minutes are part of the control group, and the remainder are not included. Walking times were calculated using the `r5r` routing algorithm, based on the paths provided by

OpenStreetMap. The analysis is conducted at two levels: at the firm and a spatial hexagonal grid with 10-hectare cells.

Two estimation techniques under the difference-in-differences (DiD) category are employed to recover the impacts: the standard two-way fixed effects (TWFE) in the static and dynamic (event study) version, for reference, and the novel staggered approach of Callaway and Sant'Anna (2021b). As Goodman-Bacon (2021) pointed out, the latter has the advantage of being free from cross-period contamination, an issue that arises in TWFE settings. This is possible since separate parameters are estimated over time and for each treatment cohort (defined by the time units get treated). Besides, parallel trends assumptions are less strict than using TWFE.

In large urban agglomerations, urban dynamics vary between different regions (White, 1999), which in turn affects local labor markets. To deal with spatial heterogeneity, we estimate five different models, one for each line and another for all of them. This allowed us to compare regions, in addition to the two levels of analysis (firm and hexagon). We summarize the group-time parameters in three aggregation schemes: an overall parameter for the whole period, average effects by cohort, and average effect by length of exposure regardless of cohort.

Our overall findings at the hexagon level resonate with the Latin American body of literature for increases in employment (estimated at 11.3 percent) and the number of firms (9 percent), whereas the lack of impacts on wages is more aligned with the European case. The biggest differences arise at the firm level and when lines are disaggregated: we found negative impacts for the average wage in firms near Line 2 (-1.98 percent) and the average number of workers in firms close to Line 12 (-7.23 percent). However, employment at the hexagon level did not change for the latter, suggesting a compositional change in the distribution of average workers per firm.

Our contribution is threefold. First, we provide evidence of how local transit policy performed in generating outcomes out of rapid transit expansion. Moreover, the case of São Paulo is worth analyzing not only for the city's importance in the local and global economies but also as it presents a unique case of commitment (albeit with shortcomings) in rapid transit expansion throughout the last five decades, especially in the Latin American context. Since the inauguration of the city's first subway line in 1974 and the restructuring of suburban rails in the 1990s-2000s period, the metropolitan area now enjoys a 372-kilometer network that carried over 7.9 million daily passengers, as of 2019 (CPTM, 2020), highlighting its role in integrating the region's labor market.

Second, we contribute to the literature on applied econometrics and place-based policies in urban and labor economics by employing the novel staggered Difference-in-Differences (DiD) framework developed by Callaway and Sant'Anna (2021b), utilizing rich geolocated data.

A final contribution regards the research design concerning treatment selection. While other works delimit treatment regions inside distance buffers, this study uses isochrones, which are calculated using real paths between a firm and a station, and therefore provides more precise delimitation of the study area, preventing incorrect selection of a firm for the treatment and control group.

2 Related literature

Interest in analyzing the impact on labor market outcomes of public transit provision has increased over the past decades, especially with the availability of geolocated microdata on job relationships (Credit, 2019; Vickerman, 2017), but with varying results. Following the DiD framework, insignificant impacts were found for Uppsala, Sweden (Åslund, Blind and Dahlberg, 2017), Charlotte, USA (Canales, Nilsson and Delmelle, 2019), and Los Angeles, USA (Severen, 2023); negative impacts following an exogenous service cut were identified in New York, USA (Tyndall, 2017) and positive results of service expansion are notably found in Latin America, such as BRT expansion in Lima, Peru (Scholl et al., 2018) and BRT, LRT, and Subway expansion in Rio de Janeiro, Brazil (Campos, 2019). Outside econometrics, structural models simulating integrated labor and real estate markets have shown significant welfare benefits related to rapid transit, such as the Ahlfeldt model applied by Tsivanidis (2019) for the BRT network of Bogotá, Colombia.

Two other studies are worth analyzing for analyzing the economic impacts of accessibility in the São Paulo Metropolitan Region, although with different approaches. Haddad et al. (2015) estimated general equilibrium impacts of the subway network at 1.7 percent of the capital's and 0.6 percent of the country's real GDP (around 19.3 billion reais), under the counterfactual scenario of replacing the subway network with bus corridors. Their structural approach provides insights into the ripple effects of accessibility in supply chains. This is done using commuting time and job accessibility elasticities relative to wages as proxies for productivity measures, establishing direct and separate links from accessibility and commuting times to productivity.

Our work can be thought of as an extension of theirs in three aspects. First, we look at market responses to rapid transit expansion through employment and business creation, in addition to wages. Second, geocoded microdata allows us to address the impact of different interventions at the intra-urban[4] scale, uncovering different urban dynamics between parts of the same city. Third, through the use of identified panel data, we set an identification strategy that provides a clearer path from subway expansion to wages through the DiD framework; such estimates can later be plugged into a more spatially refined spatial model (since they are not generalizable to the whole city) to provide a general equilibrium analysis.

Boisjoly et al. (2017) measured the relationship between accessibility and employment likelihood in RMSP, using distinct mixed logit models for workers below and above the minimum wage. They modeled accessibility for spatial units roughly equivalent to census tracts, using household data from the 2007 origin-destination survey, the 2010 census, and the 2015 public transportation network. Boisjoly et al (2017) found that a 1 percent increase in accessibility at the residence region is correlated with a 3 percent decrease in informality for workers below the minimum wage. In turn, public transport accessibility had no significance on the employment probability for better-paid informal workers, indicating a higher automobile usage by that income cohort.

These results are convergent with the spatial mismatch hypothesis, according to which underprivileged groups such as ethnic minorities face higher barriers to partake in the labor market due to higher commuting costs (in the form of higher reliance on a low-quality public transport network) and being spatially segregated in the housing market (Gobillon, Selod and Zenou, 2007). In that sense, despite not being directly comparable to our results, Boisjoly et al. (2017) provide a panorama of accessibility, land use, and segregation patterns in São Paulo that enriches the set of possible explanations for our results.

The great heterogeneity in the results of the surveyed studies indicates that regional characteristics do limit the extent to which transit improvements can foster agglomeration economies. Among other factors, the previous size of the network, their effectiveness in providing accessibility gains, and land use can be the reason behind a null effect, often combined.

Accessibility gains may feature marginally decreasing returns; therefore, an addition to an already robust transit system provides fewer economic benefits than the first lines (Chatman and Noland, 2011; Deng, 2013). Even a completely new system may not enhance accessibility, therefore rendering no utility (Vickerman, 2008; Credit, 2019), e.g. in a highly car-dependent environment and in the absence of additional investments such as feeder bus routes.

[4] Following Villaça (2016), we append the "intra" suffix to empathize the focus on a very small scale rather than a regional approach .

Land use matters in the sense that mature portions of a city can benefit relatively less from transit improvements—as was the case for Charlotte (Canales, Nilsson and Delmelle, 2019) but not for Rio de Janeiro (Campos, 2019)—but at the same time, density and transport provision are highly associated, so it is difficult to separate those two aspects of urban development. Zoning might also prevent growth in any region that faces density constraints (Combes and Gobillon, 2015; Redding and Turner, 2015), so lack of growth in mature regions can be a result of lack of available space rather than lack of demand.

Research design choices also lead to different results, even for studies that share the same methodology. While the most recent ones delimit treatment and control groups, they use different distance thresholds, ranging from 400 meters (Canales, Nilsson and Delmelle, 2019) to 4.5 kilometers (Åslund, Blind and Dahlberg, 2017). More so, few of them are geolocated at the individual level, an issue also raised by Credit (2019): this can weaken identification since units are regarded as lying at the same distance to the stations, which is the region's centroid; thus, the bigger the region, the bigger the error potential. On the bright side, around half of the evidence related to labor market effects analyzes individuals at their residences, a better measure than looking at the workplace since it measures the impact of public transport more directly.

Time can also affect estimates in two ways: by length of exposure to treatment and by heterogeneities between groups treated in different periods. The first case is considered in some studies with the inclusion of time lags (only Scholl et al. (2018) regarding labor markets), but if the second heterogeneity source is present, those time dummies have a limited capability of recovering estimates due to the contamination issue.

Considering these research design and estimation issues, the strategy adopted in this thesis has three main differences from the surveyed literature. First, catchment areas are delimited using a routing algorithm through real paths—more precise than the straight-line distance—although the issue of an arbitrary threshold persists.

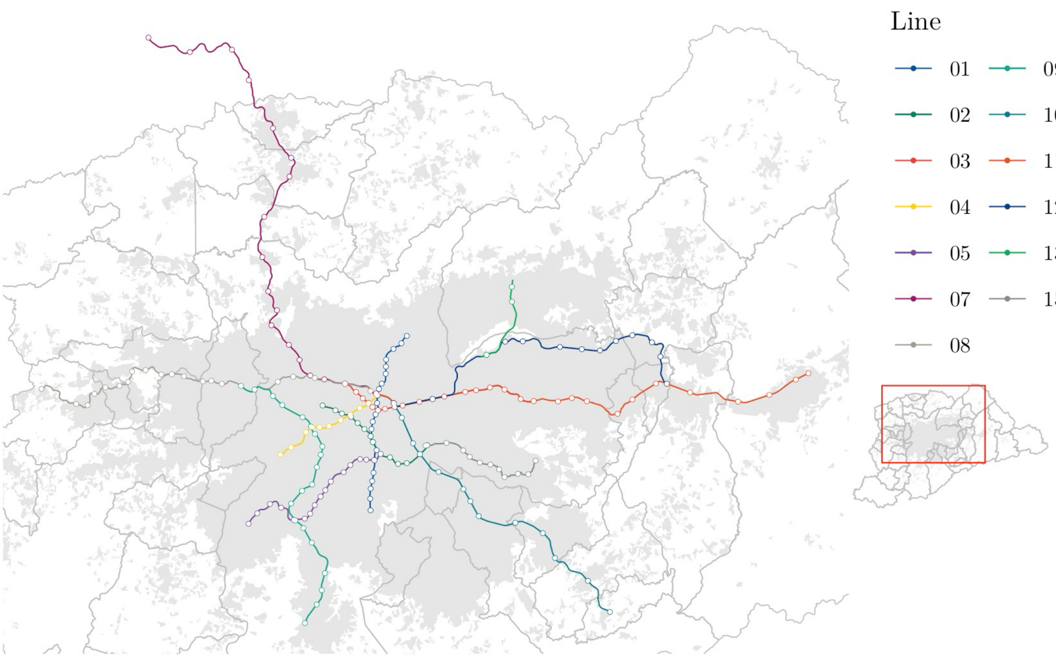
Second, identifying individuals at their residences would only be possible through the origin-destination survey, which has a decennial frequency and thus compromises the identifying assumptions, or using a household survey designed to be representative at the state level, compromising inference for the metropolitan level. In turn, the employer-employee dataset in use is an annual database containing the whole population of formal workers in the country. Therefore, its advantages related to inference and identification of the timing of the interventions overcome its limitations.

Third, the econometric strategy is where this thesis differs the most from the literature. Since treatment time is not equal for all units, this heterogeneity is modeled through the staggered setup of Callaway and Sant'Anna (2021b), which analyzes each treatment cohort separately and over time in a way that does not suffer from the contamination on TWFE lags. The underlying hypothesis is that groups treated earlier from rapid transit expansion since marginal accessibility gains are higher, and if the most demanded regions are attended first. To deal with spatial heterogeneities, each transit line is analyzed separately, similar to the distinction between trunk and feeder corridors made by Scholl et al. (2018).

3 Transit Policy in RMSP

São Paulo has a long history of railway investment that goes back to the second half of the 19th century with the first interurban services. There are four rail operators in the city. CMSP (*Companhia do Metropolitano de São Paulo*, mostly known as Metrô) was founded in 1968 and operates lines 1-3 and 15, a service commonly labeled as "metro", "underground", or "subway"—frequent headways (up to 90 seconds in rush hour), closer stops and inside a dense urban core—but its network is limited to the capital. Figure 1 locates the current rapid transit lines in the region, along with city borders and the urban footprint.

Figure 1: São Paulo Metropolitan Rail Network in 2023

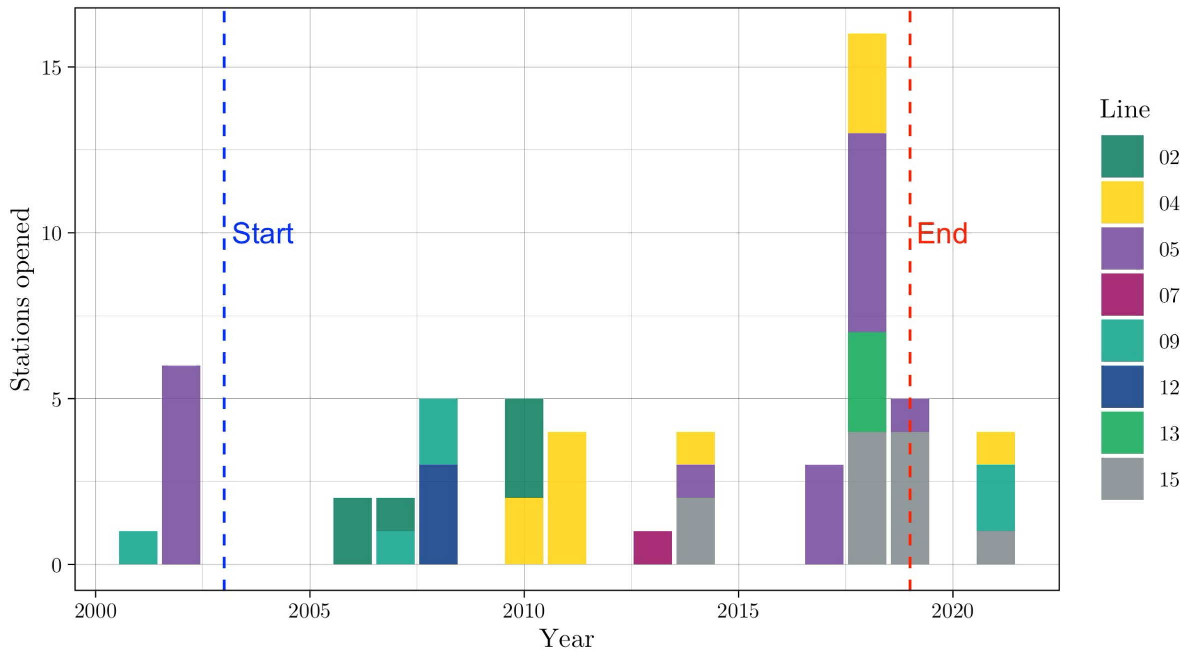


Source: Own elaboration.

CPTM (Companhia Paulista de Trens Metropolitanos) runs lines 7 and 10-13, connecting neighboring cities to São Paulo. It was founded in 1996 resulting from the merger of suburban lines operated by federal and state agencies[5]. Historically, urban development first began along this axis, which to this date concentrates on very dense neighborhoods surrounded by trade, services, and industrial activities (IBGE, 1954; Santos et al., 2011). CPTM is closer to the European regional metro concept than to North America's commuter rail service (Allen, 1998) since it features frequent service, electrified lines, and is almost fully grade-separated. Four other lines are operated under public-private partnership (PPP) regimes: Subway Line 4 is owned by Metrô but administered by Viaquatro since its inauguration in 2010, whereas Viamobilidade runs Metrô's Line 5 (since 2018) and CPTM's Lines 8 and 9 (since 2021).

Between 2003 and 2019, 52 new stations were inaugurated (a 31 percent increase), adding around 62 kilometers to the network, a 20 percent increase in length. Figure 2 plots the number of stations opened per year on each line. CPTM extended line 9 southbound, added infill stations in lines 7 and 12, and opened a new link to Guarulhos International Airport (line 13). Most of the expansion, however, was carried out by Metrô: first, an eastbound extension of Line 2, reaching CPTM line 10 in 2011. Second, the new Line 4 started its operations in 2010 in a roughly four-kilometer stretch linking Paulista Station (connection to Line 2), in Paulista Avenue, to the north end of Faria Lima Avenue. Both are dense avenues oriented towards retail, services, and offices.

Figure 2: Rapid transit expansion in RMSP between 2000 and 2022



[5] *Rede Ferroviária Federal S/A* (RFFSA, extinct), *Companhia Brasileira de Trens Urbanos* (CBTU, still operating in other states), and *Ferrovias Paulistas S/A* (FEPASA, extinct).

Source: Own elaboration.

While Paulista Avenue was the epicenter of economic dynamism during the 1980s—representing the first wave of migration from the historical city center to a new CBD—in the 2000s, Faria Lima took its role as the city's financial district (Lores, 2017); nevertheless, its north end was still less developed when it received a Line 4 station in 2010. In the following year, the line expanded north to Luz, at the core of the historical downtown, and southwest to Butantã, a neighborhood characterized by high-income single-family households but also institutions such as Universidade de São Paulo (USP), with almost 100 thousand alumni.

In the following years, three infill stations were inaugurated, in addition to a westbound expansion to Morumbi in 2018, a mostly single-family residential neighborhood, and the current terminus station Vila Sônia, in 2021. Two other significant openings in the period were Line 5 expansion and the new monorail Line 15; nevertheless, they are not analyzed in this work since most inaugurations in those lines are too close to the end of the analyzed period.

4 Data

The main data source for this study is RAIS[6], an employer-employee dataset of the Brazilian Ministry of Labor and Employment (*Ministério do Trabalho e Emprego* — MTE). Selected variables are the worker's average hourly wage and firm size, industry (both used as controls), address, and number of employees. The RAIS dataset was filtered to remove firms with less than two employees since they mostly consist of self-employed individuals under the different Brazilian variations of a single-member limited liability company. Our analysis begins in 2003 since some variables were not available before that and ends in 2017 to limit the study to the pre-pandemic period and avoid contamination from stations inaugurated in 2018 near Line 2.. Another adjustment concerns treatment timing: firms near stations that opened between July and December of year t begin treatment in year $t + 1$ since RAIS data is annualized.

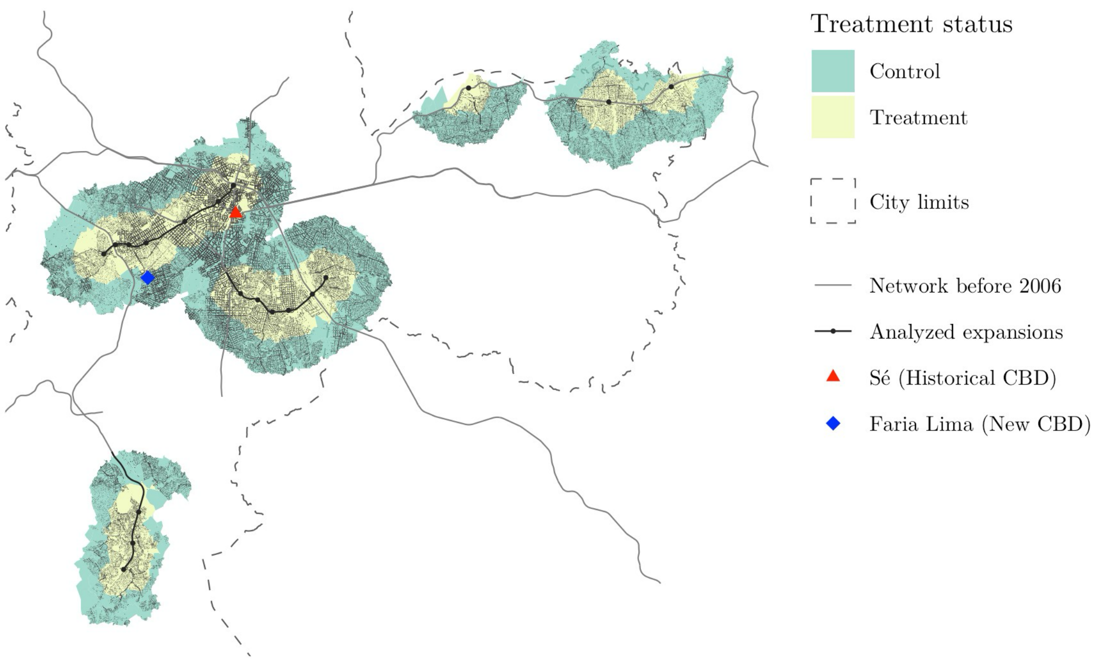
A prerequisite to correctly identifying the impact of transit expansion is to pay attention to the spatial extent of the effects, as economic agents closer to the treatment (i.e., the stations) tend to benefit more from it than those farther away. Applied works deal with this issue by distinguishing treatment and control groups in space so that treated units are close enough to the stations that some impact can still be recovered, and control units are far enough not to be contaminated by spillover effects (e.g.,

[6] *Relação anual de informações sociais*, lit. Annual Relation of Social Informations. The usage of this data was authorized by the univeristy's Committee in Research Ethics.

Campos (2019), Scholl et al. (2018), Åslund, Blind, and Dahlberg (2017)). We improve the proximity selection criterion by using isochrones—the maximum area that can be reached in a given time—instead of buffers to distinguish firms between treatment and control; hence, space heterogeneity, such as physical barriers and irregular streets, is considered. Isochrones are calculated using the street layout, hence they provide a better measure of accessibility to transit.

After running auxiliary econometric models with different isochrones ranging from 5 to 60 minutes of walking distance from a station, we defined the 30-minute isochrone as the treatment area and the 31-60-minute one as the control area, since impact on outcomes was detected up to the 30-minute threshold. Firms that change their address to any untreated area (or treated in a different period) were removed from this study since, despite being treated in the first place, moving to a different location implies accessibility changes, which alters the impact of transit expansion in the target outcomes.

Figure 3: Catchment areas for each line according to treatment status



Source: Own elaboration.

Using official data on rapid transit networks (São Paulo, 2018, 2019, 2022, 2023), an algorithm was created in R to define each station's catchment area, using the package `r5r` (Pereira et al., 2021). They are then subdivided according to their treatment status, using custom functions compiled by the authors in the package `spatialops` (Alvarenga, 2024). In our initial strategy, we dropped all firms that intersected with the 30-minute isochrone of stations that existed before 2003, since firms who receive additional treatment are impacted by transit expansion differently. Despite the robustness advantage of this strategy, it limits the number of observations in data. Hence, we have chosen the simpler approach of allowing additional treatment firms to belong to any group. The resulting study area comprises four different lines (2, 4, 9, and 12) with non-overlapping catchment areas depicted in Figure 3.

The single infill station inaugurated in Line 7 was dropped from the analysis since the number of surrounding firms is too small to provide adequate inference. As for Line 5, the single station opened in 2014 was discarded for the same reason, while the 2017-2018 expansion was dropped since not only are they too close to the end of the period, but the northernmost stations overlap with Line 2's expansion. São Paulo-Morumbi station in Line 4 was discarded since it opened in 2018, the last year selected for analysis. Finally, the time window was restricted to 2017, especially to prevent contamination from Line 5 into the results of Line 2.

Although there are 39 cities in the metropolitan region, only four of them intersect with the stations' catchment area in 60-minute isochrones: São Paulo, Guarulhos, Itaquaquecetuba, and São Caetano do Sul. In practice, only a minority of them are outside of the capital, where all the stations that opened in the period are. We geolocated the addresses using the ArcGis (ArcGIS, 2024) and HERE (HERE, 2024) APIs through the `R` package `tidygeocoder` (Cambon et al., 2021), retrieving latitude and longitude coordinates. The total number of locations was above 800 thousand addresses, which can represent more than one firm: not only do some companies move, but a single address can hold multiple firms (e.g., office towers). This process resulted in the catchment areas in Figure 3. Each geolocated firm is represented by a small point in the map: In denser regions, they are close enough to resemble the road layout.

We analyze four outcomes in this study in two aggregation schemes. At the firm level, employment and hourly wage; at a spatial level, the number of firms and employment. The first aggregation provides fine-grained information on the firms' labor market response to transit expansion at the demand side, e.g. by contracting more employees or increasing their salaries in response to increased productivity. In turn, zooming out of the firm via spatial aggregation helps to address impacts on a wider scale, especially by looking at the relative growth or decline in a region's number of firms after transit expansion. Analyzing the number of jobs at a regional scale also helps in understanding if there are significant employment gains near new stations even if, on average, firms do not change their number of workers (or even if the average decreases).

5 Identification strategy

Assuming that the established control group is an adequate counterfactual for the treated group in the analyzed period, the differences-in-differences framework (DiD) is a suitable option to recover the impact of transit expansion in labor market outcomes. The most common DiD applications are its canonical form, consisting of two groups and two periods (2x2 DiD), and the two-way fixed effects (TWFE), either static or dynamic (event study), where multiple periods can be used to recover an overall treatment effect (or by length of exposure, in the dynamic case).

TWFE models, nevertheless, can generate misleading interpretations if treatment effects are heterogeneous either between units treated at different periods or over time. (Borusyak, Jaravel and Spiess, 2022; Callaway and Sant'Anna, 2021b; De Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Marcus and Sant'Anna, 2021; Sun and Abraham, 2021). As the Bacon decomposition demonstrates, early-treatment units are part of the control group for later-treated ones (Goodman-Bacon, 2021). Therefore, if the treatment effect is not null for early-treated groups, their effects are subtracted from those of later-treated groups, leading to an underestimated overall ATT if they are positive and overestimated if they are negative. To circumvent this issue, we employ the staggered DiD method of Callaway and Sant'Anna (2021b), which deals with heterogeneity through a group-time average treatment effect on the treated, $ATT(g, t)$.

Besides the staggered DiD model, we also estimate static TWFE DiDs and ES-TWFE models for robustness. For all estimation techniques, the included covariates are the same but depend on the outcome. For the wage equation at the firm level, they are three dummies: one if the firm is classified as business-to-business service, and two other for micro and small companies, following the national classification provided by the Brazilian Service of Support to the Micro and Small Businesses (*Serviço Brasileiro de Apoio às Micro e Pequenas Empresas* — Sebrae), available in SEBRAE... (2013, p. 17). Firm-level employment also has the B2B binary variable but does not include size dummies, since they are potential colliders. The choice of the B2B industry is backed by the theoretical framework. In this sector, input and output transport play a minor role than in manufacturing and construction; hence, location decisions are more driven by access to consumer markets and workers than the ease of transporting goods. At the hexagon level, employment and number of firms have both industry and size variables, with the difference that they are proportions relative to the total number of firms in the hexagon.

Introducing the notation following Callaway and Sant'Anna (2021b), let Y_{it} be the outcome for the firm or hexagon i observed in t, D_{it} an indicator equal to one if i is treated in period t, X the vector of covariates for the baseline period, and G the period when a unit becomes treated. For never-treated units, $G = \infty$. G_g is equal to one for all units that begin treatment in period g and let $\bar{g}$ be the maximum G: since never-treated observations are used in this study, $\bar{g} = \infty$. Let $\mathcal{G}$ be the support of G excluding $\bar{g}$, i.e., all treatment periods but the last one. Finally, let the generalized propensity score for the probability, in time s, of a unit being treated since period g be $p_{g,s}(X) = P[Gg = 1|X, G_g + (1 - D_s)(1 - G_g) = 1]$. In addition to this probability being conditional on pre-treatment covariates, for the never-treated groups, $(1 - Ds)(1 - Gg) = 1$.

There are two parallel trends assumptions under Callaway and Sant'Anna (2021b). In our study, the not-yet-treated units are part of the control group along with the never-treated ones. In this case, the $ATT(g,t)$s are properly recovered when in addition to parallel trends after treatment, the observed outcomes between all eventually treated (except the first) and the never treated group is the same in the previous period:

$$\begin{aligned} &\mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, G_g = g] = \mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, D_s = 0, G_g = 0] \\ &\forall g \in \mathcal{G}, (s,t) \in \{2, \dots, T\} \times \{2, \dots, T\}, t \geq g - \delta, t + \delta \leq s < \bar{g}. \end{aligned} \tag{4.3}$$

Parallel trends in Callaway and Sant'Anna (2021b) can hold after conditioning on pre-treatment covariates. As Marcus and Sant'Anna (2021) hightlight, parallel trends assumptions (PTAs) in Callaway and Sant'Anna (2021b) are weaker. For instance, if one uses both never-treated and not-yet-treated units as controls, De Chaisemartin and D'Haultfœuille (2020) and Sun and Abraham (2021) require parallel trends to hold in the pre-treatment period between all eventually treated cohorts (in addition to the never-treated group), while in Callaway and Sant'Anna (2021b) they do not need to hold for the first treated group. Parallel trends still need to hold in the post period; however, the weaker assumptions in Callaway and Sant'Anna (2021b) can accommodate situations where PTA does not hold before treatment but then do hold after it, e.g. when economic conditions were different between groups before intervention (Marcus and Sant'Anna, 2021).

The group-time average treatment effect $ATT(g,t)$ can be nonparametrically identified through three methods: inverse probability weighting (*ipw*), outcome regression (*or*), or doubly robust (*dr*), a combination of the previous two. In short, *ipw* weighs outcome difference relative to the baseline period ($\Delta Y := Y_t - Y_{g-\delta-1}$) by the probability of an unit being treated at that given time and its covariates, relying on $p_{g,s}(X)$ being correctly modeled. In turn, *or* subtracts from ΔY the population outcome regression (conditional on X) for the control group, and thus depends on the outcome evolution for the control group being correctly modeled. According to Theorem 1 in Callaway and Sant'Anna (2021b), under the *dr* method only one of the two conditions above needs to be met, rendering more robust estimators; therefore, this is the procedure chosen in this study.

Let the population outcome regression when not-yet-treated (*ny*) units are part of the control group be $m^{ny}_{g,t,\delta}(X) := \mathbb{E}[Y_t - Y_{g-\delta-1}|X, D_{t+\delta} = 0, G_g = 0]$. The parameter of interest in the doubly-robust setting is

$$ATT_{ny}^{dr}(g,t;\delta) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_{g,t}(X)(1-D_{t+\delta})(1-G_g)}{1-p_{g,t+\delta}(X)}}{\mathbb{E}\left[\frac{p_{g,t+\delta}(X)(1-D_{t+\delta})(1-G_g)}{1-p_{g,t+\delta}(X)}\right]}\right) - \left(Y_t \; - \; Y_{g-t+\delta} - m_{g,t+\delta}(X)\right)\right]. \qquad (4.4)$$

The first step for obtaining $ATT_{ny}^{dr}(g,t;\delta)$ is estimating $p(\cdot)$ and $m(\cdot)$ according to the parametric estimators as demonstrated in Callaway and Sant'Anna (2021b). Next, these estimates are plugged into the sample analog of Equation 4.4. These procedures and the following aggregation are all available in the R package `did` (Callaway and Sant'Anna, 2021a).

In each analysis, there are $g \cdot t$ group-time average treatment effects, resulting in 544 total estimates since there are four outcomes and five analysis schemes (overall and each transit line separately). For better interpretation, they are aggregated in three different schemes: overall measures (compared with the static DiD), overall measures by treatment cohort, and effect by length of exposure (compared with ES-TWFE).

From the least to the most aggregated (the reverse order of presentation), the length of exposure parameters are summarized across all treatment groups. These estimates are balanced so that only units that have experienced treatment for at least six periods are used. As Callaway and Sant'Anna (2021b) demonstrate, the direct comparison of parameters in time is contaminated by differences in group composition and each group's weight in the event study aggregation; however, balancing can eliminate such nuisances. The second aggregation scheme is useful in understanding how treatment effect differs by group cohort. The parameter averages $ATT(g,t)$s separately for each g. The overall parameter is a simple average of the cohort parameters, which reduces the influence that length of exposure and group size have on the overall measure.

After estimation and aggregation, inference is done using a bootstrap procedure. Details are available in Callaway and Sant'Anna (2021b). In short, an influence function estimates the vector of $ATT(g,t)$s; then, for each iteration, the bootstrap algorithm calculates t-tests for the parameters. After B iterations, $\hat{c}_{1-\alpha}$ is constructed as the $(1-\alpha)$-quantile of the t-tests distribution. Standard errors are clustered at the firm or at the hexagon in their respective panels. In the case of event study plots, the same bootstrap procedure can be applied to pre-treatment estimates. Differently from the ES-TWFE case, these estimates do not contaminate post-treatment $ATT(g,t)$s nor are contaminated by then. Therefore, as suggested by Marcus and Sant'Anna (2021), pre-treatment t-tests can be used for placebo tests and as evidence against parallel trends, if they turn out to be significant.

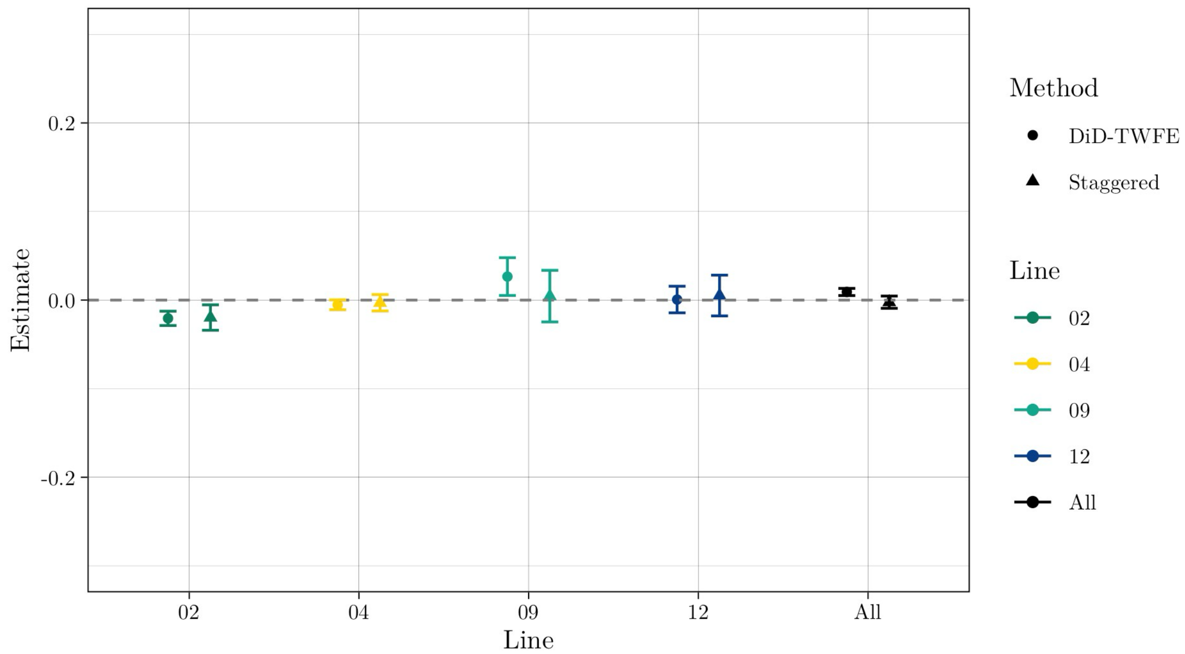
6 Results and Discussion

The next paragraphs explore results in detail and establish a connection with the theoretical grounds. For each outcome, a graph is presented showing the overall impact based on the DiD-TWFE and staggered DiD methods; next, overall ATTs for the staggered approach are dismembered by cohort. For conciseness, event study plots comparing ES-TWFE and staggered approaches are registered in Appendix A; besides ATTs, they also contain pre-treatment estimates. Confidence bands are reported for the 95% significance level; all results are presented as natural logarithms but converted to percentages when mentioned in the text by subtracting one from its exponential.

6.1 Average hourly wage

Figure 5 shows the overall impact on wages at the firm level. What strikes the most is the negative and significant result for Line 2, indicating a 1.98 percent reduction after the opening of subway stations in both the staggered and TWFE estimates. In contrast, results are positive for Line 9 (at 2.5 percent using TWFE); nonetheless, the staggered estimate fails to reject the null: despite both estimates having similar standard errors, the effect is closer to zero for the staggered one. Lines 4 and 12 are both nearly zero in both models.

Figure 4: Average impact of transit expansion on hourly wage at the firm level



Source: Own elaboration.

In the joint model ("All"), there is a marginal but significant effect in the TWFE case and a negative but insignificant estimate for the staggered setup; however, both are very close. Given the small confidence bands, there is not much variance in wage growth within regions, albeit they are about twice as large for Lines 9 and 12 (which have fewer units in any treatment group).

Table 2 uncovers differences between cohorts that are not visible in the overall parameters. Line 2's overall significance comes mostly from firms close to stations that opened in 2007 and 2010 since they are the only cohorts with relevant estimates, at respectively -5.16 percent and -3.44 percent. When lines are aggregated, $g = 2010$ has no significance and is softened—it even becomes positive, even though the (insignificant) coefficient for $g = 2010$ for Line 4 alone is also negative.

Table 1: Impact of transit expansion on average firm wage, by cohort

Cohort	All lines	Line 2	Line 4	Line 9	Line 12
Average	-0.002	-0.02*	-0.003	0.004	0.005
	[-0.007, 0.002]	[-0.031, -0.008]	[-0.011, 0.004]	[-0.026, 0.035]	[-0.016, 0.027]
2006	-0.014	-0.013	-	-	-
	[-0.035, 0.007]	[-0.033, 0.007]	-	-	-
2007	-0.049*	-0.053*	-	-	-
	[-0.092, -0.006]	[-0.093, -0.012]	-	-	-
2008	-0.014	-	-	0.004	0.004
	[-0.035, 0.007]	-	-	[-0.026, 0.035]	[-0.024, 0.033]
2009	-0.007	-	-	-	0.007
	[-0.063, 0.049]	-	-	-	[-0.037, 0.052]
2010	0.001	-0.035*	-0.009	-	-
	[-0.013, 0.015]	[-0.063, -0.007]	[-0.023, 0.006]	-	-
2011	-0.003	-0.011	0.01	-	-
	[-0.024, 0.018]	[-0.034, 0.012]	[-0.03, 0.05]	-	-
2012	0.002	-	0	-	-
	[-0.007, 0.012]	-	[-0.01, 0.01]	-	-
N	204867	61999	127019	9327	14197
Clusters	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm

Source: Own elaboration.
Notes: confidence interval reported in brackets; * = significant at 5%.

In contrast, $g = 2009$ changes sign (becomes negative) and thus has twice the magnitude just by changing the comparison group (since only Line 12 begins treatment in 2009), although still close to zero (from -0.7 percent to 0.7 percent). Pre-trends are all zero (figures 9–13, panel b of Annex A) except for $t = -5$ in Line 4. This can be thought of as a minor problem since outcomes are statistically equal on average between treatment and control for $t = \{-6, -4, -3, -2\}$; however, it can also indicate a placebo test failure. Line 4 opened in 2010 with two stations in a core section (Paulista and Faria Lima) followed by expansions towards downtown (Luz) and the west (Butantã) in the next year two years; therefore, it is possible that each time cohort follow different outcome paths due to their urban dynamics.

Looking at group-time average treatment effects for Line 4 in the lowest level of detail—the yearly $ATT(g, t)$s—pre-treatment parallel trends are violated for $g = 2012$ (Figure 14), which corresponds to firms and hexagons around stations República and Luz. Both stations, in addition to offering connections to several other lines in the network, are inserted in the historical CBD, a dense region with a high concentration of jobs that, despite losing part of its dominance to other business districts since the mid-20th century, still experienced relatively higher growth in comparison with never-treated and not-yet-treated regions in the Line 4 region, up until 2006 (firm employment) and 2007 (firm wage).

The result for Line 2 is not matched in the literature since the positive effects on wages are either nonexistent (e.g., Åslund, Blind and Dahlberg (2017)) or positive, particularly in Latin American contexts (Campos, 2019; Scholl et al., 2018; Tsivanidis, 2019). Looking for reasons for this result in the theory, one of the transmission channels from transit expansion into wages is through productivity, in which case wage reductions could indicate productivity loss and the lack of significance suggests no productivity gains or no pass-trough into wages, i.e., productivity being completely absorbed by the employer.

Alternatively (or in addition to this effect), employers might capture commute savings through smaller wages, while still resulting in a positive or null utility change for workers. A similar reasoning is found in Vickerman (2008) when describing the two-way road effect applied to wages on a regional scale: the increase in labor supply resulting from greater accessibility reduces wage pressure, while the contrary happens if firms elsewhere outbid local firms for their workers[7]. In the three cases, however, the sticky nature of wages—among other factors, in observance of the Brazilian labor legislation—suggests that the reduction perceived in Line 2 region stems not from wage cuts but rather from new employment relationships, that could either be a substitution of previous, better-paid workers, or result from new employers receiving less and thus bringing the average down.

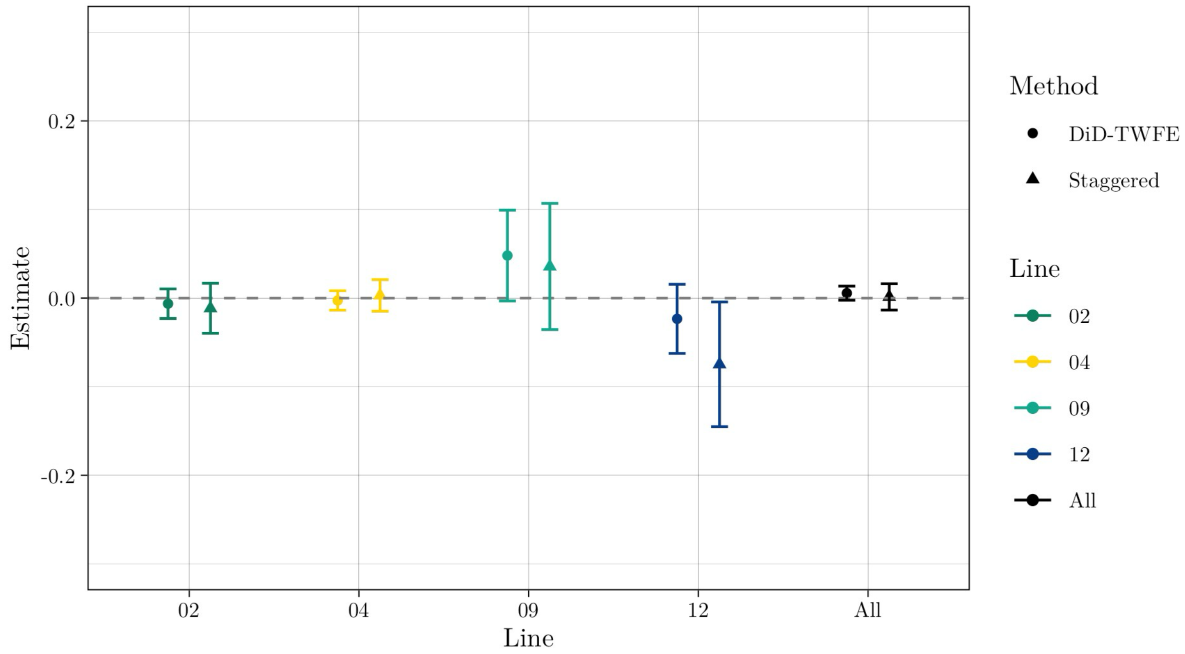
[7] Adapting to the intra-urban context, the latter argument only makes sense for the set of individuals that used to work near their houses, as mentioned previously in the Line 12 case.

6.2 Employment

At first glance, Figure 6 points to similar results for the two methods, but great variability between regions. The lowest values are registered for Line 12: impacts are negative under both methods, but for the staggered one, it reaches -7.23 percent and is significant. Similar to wages, results are the highest for Line 9 (at roughly 5 percent for TWFE and 4 percent for the staggered method), albeit none are significant; the same applies for Lines 2 and 4 and in aggregate.

Cohorts in Table 3 do not differ from the overall results: Group 2008 is significant for line 12 (-8.52 percent) and offsets Line 9's positive effect (3.67 percent but not significant) when all lines are considered, leading to a negative and significant impact of -5.26 percent for $g = 2008$.

Figure 5: Average impact of transit expansion on employment at the firm level



Source: Own elaboration.

Event study plots on panel (a) of figures 9–13 show virtually no fluctuation for all lines aggregated and for lines 2 and 4; in contrast, a crescent (but still nonsignificant) trend is perceived for Line 9 up to the fourth year when it breaks monotonicity and starts to fall, and the opposite movement is noted for Line 12. Pre-parallel trends are present in all scenarios but near significance for Line 12 in $t \leq -4$, albeit converging to zero.

The lack of meaningful results in most regions might suggest that transit investments did not translate into accessibility gains that increase the labor market pool or enable firms to be reached by more customers. This hypothesis needs to be considered with caution since accessibility is not modeled in this study, and available data look at individuals at their workplaces, not residences. Another possibility is that additional labor supply was absorbed by entrant firms rather than established ones: results for the number of firms at the hexagon level will help to investigate this hypothesis.

For lines 2 and 4, two other possible explanations are the high motorization rate in those regions (Boisjoly et al. , 2017), as was the case of Uppsala (Åslund, Blind and Dahlberg, 2017), and the fact that those regions were already important job centers prior to intervention, in the same line of Canales, Nilsson and Delmelle (2019).

Table 2: Impact of transit expansion on employment at the firm level, by cohort

Cohort	All lines	Line 2	Line 4	Line 9	Line 12
Average	0.001	-0.012	0.003	0.036	-0.075*
	[-0.009, 0.012]	[-0.036, 0.013]	[-0.012, 0.017]	[-0.034, 0.105]	[-0.138, -0.012]
2006	0.017	0.013	-	-	-
	[-0.023, 0.058]	[-0.031, 0.058]	-	-	-
2007	-0.089	-0.078	-	-	-
	[-0.18, 0.002]	[-0.167, 0.011]	-	-	-
2008	-0.054*	-	-	0.036	-0.089*
	[-0.106, -0.001]	-	-	[-0.039, 0.11]	[-0.165, -0.012]
2009	-0.026	-	-	-	-0.029
	[-0.161, 0.108]	-	-	-	[-0.156, 0.099]
2010	0.012	-0.013	0	-	-
	[-0.01, 0.034]	[-0.087, 0.062]	[-0.024, 0.025]	-	-
2011	-0.022	-0.03	-0.011	-	-
	[-0.068, 0.025]	[-0.086, 0.026]	[-0.093, 0.072]	-	-
2012	0.009	-	0.006	-	-
	[-0.013, 0.03]	-	[-0.017, 0.028]	-	-
N	204867	61999	127019	9327	14197
Clusters	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm

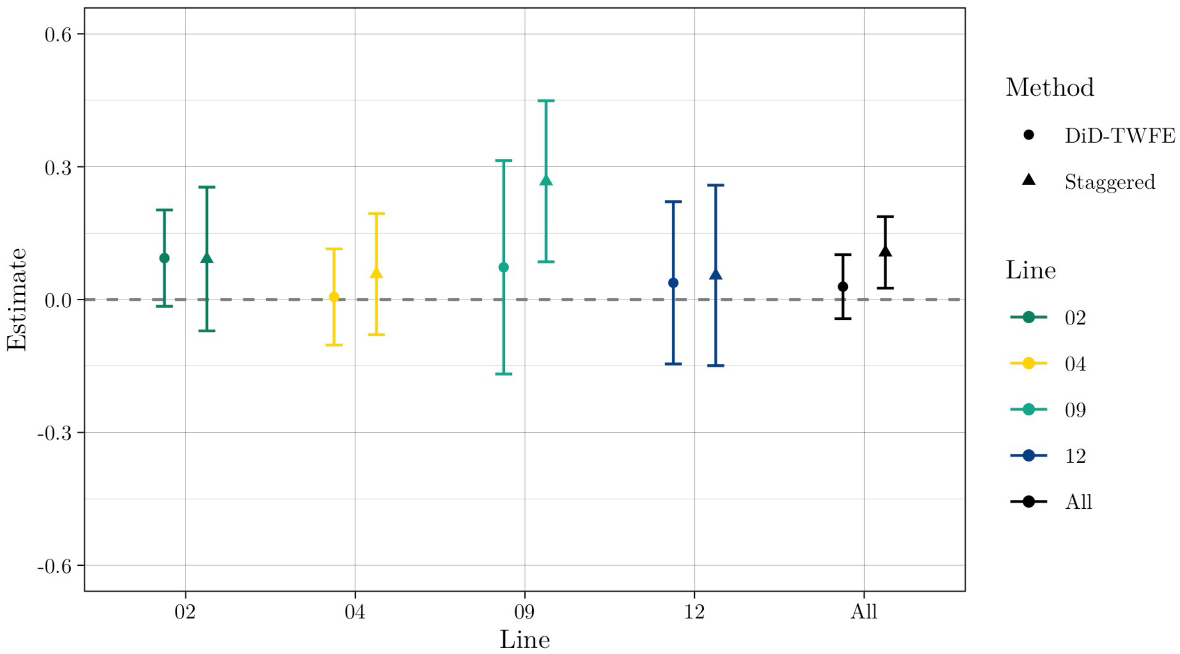
Source: Own elaboration.

Notes: confidence interval reported in brackets; * = significant at 5%.

As for the negative and significant impact estimated for the Line 12 region, how is it possible that new train stations decreased the average number of workers per firm? One can then think of the “two-way road” effect: the same accessibility gains that increase accessibility *to* a place also increase accessibility *from* it (SACTRA, 1999; Vickerman, 2008). In other words, if the three new infill stations in Line 12 increased the labor pool for the surrounding firms, residents nearby also gained access to a wider range of employment possibilities and thus might have exchanged from employment in a local business to other firms elsewhere along the network, which could also indicate a greater firm-worker match. Yet, a simpler explanation along the lines of the last paragraph is that new firms may employ fewer workers, bringing the average down. This is also intuitive assuming that new businesses are in an earlier development stage and, therefore, have fewer employees.

Zooming out to the hexagon level, overall results are positive and significant for Line 9, at 30.6 percent, and all lines jointly, at 11.3 percent (Figure 7). The biggest difference from firm-level results occurs at Line 12, where results are positive: this indicates that a reduction in firm-level average employment was compensated by business creation.

Figure 6: Average impact of transit expansion on employment at the hexagon level



Source: Own elaboration.

Group ATTs are all positive (Table 4), but only $g = 2008$ has significance. Despite some estimates suggesting strong employment effects—mostly positive and one negative, which is $g = 2009$ for Line 12)—, there is great variability. The estimated impact on hexagon employment for $g = 2011$ among all lines, the parameter is centered on 5.34 percent but goes from -16.14 percent to 32.18 percent; for Line 2 and $g = 2007$ it reaches 21.53 percent, but CI goes from -1.09 percent to 49,33 percent.

Employment evolution over time follows mostly positive and crescent paths (except for Line 12 as well). In special, estimates for the fifth and sixth year after treatment for Line 9 are significant, at roughly 40 percent job growth, but on the edge of significance. Overall results, though, are not different from zero at a yearly basis.

Table 3: Impact of transit expansion on employment at the hexagon level, by cohort

Cohort	All lines	Line 2	Line 4	Line 9	Line 12
Average	0.107*	0.092	0.057	0.267*	0.055
	[0.042, 0.171]	[-0.058, 0.241]	[-0.054, 0.169]	[0.09, 0.444]	[-0.172, 0.281]
2006	0.059	0.043	-	-	-
	[-0.106, 0.224]	[-0.132, 0.219]	-	-	-
2007	0.143	0.195	-	-	-
	[-0.07, 0.357]	[-0.011, 0.401]	-	-	-
2008	0.283*	-	-	0.267*	0.168
	[0.115, 0.451]	-	-	[0.071, 0.463]	[-0.159, 0.495]
2009	-0.127	-	-	-	-0.24
	[-0.458, 0.204]	-	-	-	[-0.532, 0.053]
2010	0.071	0.263	0.056	-	-
	[-0.077, 0.22]	[-0.686, 1.213]	[-0.094, 0.205]	-	-
2011	0.052	0.047	0.037	-	-
	[-0.176, 0.279]	[-0.297, 0.39]	[-0.322, 0.396]	-	-
2012	-0.017	-	0.072	-	-
	[-0.216, 0.183]	-	[-0.187, 0.331]	-	-
N	2552	768	837	367	603
Clusters	by: Hexagon	by: Hexagon	by: Hexagon	by: Hexagon	by: Hexagon

Source: Own elaboration.
Notes: confidence interval reported in brackets; * = significant at 5%.

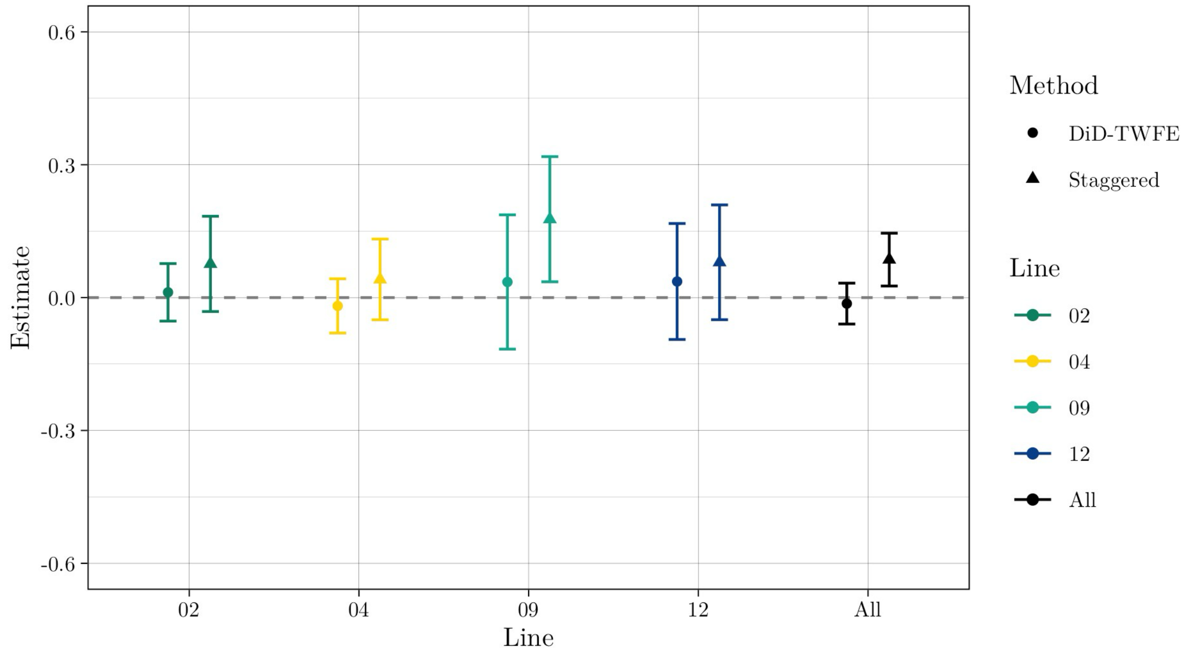
The contrast between firm-level and hexagon-level estimates of employment provides additional support to the hypothesis that the decrease observed in some regions at the firm level does not mean less employment in general, but rather a composition change resulting from new players employing fewer workers.

6.3 Business growth

In the most aggregated form, the 2006-2012 Metrô and CPTM expansion resulted in 8.98 percent more jobs for the treated hexagons (Figure 8). All individual overall estimates for the staggered approach are positive but significant only for Line, 9 at 19.36 percent. The aggregated results are in line with the findings for Rio de Janeiro (Campos, 2019) after the inauguration of subway stations for firms between 750 and

1000 meters from a station. BRT inauguration in Lima (Scholl et al., 2018) also caused business growth, at roughly 32 percent.

Figure 7: Average impact of transit expansion on firm growth at the hexagon level



Source: Own elaboration.

By cohort, all parameters in Table 5 are positive with two exceptions: $g = 2009$ for Line 12 and $g = 2012$ for the joint analysis. Both cohorts belong to only one project (Line 12 and Line 4, respectively) and when analyzed separately, estimates are positive, just as happened for $g = 2009$ for the average wage. Line 2 presents large effects for all groups except $g = 2006$, but are only significant for $g = 2007$ at 18.77 percent. Once again, confidence intervals are too large, indicating a high variability of results among hexagons. Aggregating all lines, groups 2007 and 2008 remain significant.

The role played by length of exposure varies between lines, as indicated by panel (c) of the figures 9–13 of Appendix A. Just as in the case of hexagon employment, a crescent trend is noted for lines 2 and 9, although with no significance for individual $ATT(g,t)$s; for lines 4 and 12, this pattern is reverted respectively in the third and fourth years after treatment. Considering all lines together, the overall trend is positive, crescent, and significant from the third year onwards. On one hand, estimates are more moderate than those recovered for Line 9 and more aligned with the ones from Line 2; on the other hand, analyzing all lines at the same time provides more precision through smaller standard errors, leading to significant estimates.

Recalling the drivers of firm location decision, public transport attracts firms to a location if its accessibility gains allow firms to save labor and shipping costs, attract more customers (at the retail sector), enjoy a greater labor pool and employ better matched workers. At the same time, land value and go in the opposite direction, as regions with good accessibility tend to be more expensive, and so does technology, since they can make proximity less essential for a firm's activity.

Table 4: Impact of transit expansion on firm growth at the hexagon level, by cohort

Cohort	All lines	Line 2	Line 4	Line 9	Line 12
Average	0.086*	0.076	0.041	0.177*	0.08
	[0.043, 0.129]	[-0.018, 0.17]	[-0.039, 0.121]	[0.037, 0.317]	[-0.052, 0.211]
2006	0.073	0.04	-	-	-
	[-0.029, 0.176]	[-0.091, 0.171]	-	-	-
2007	0.162*	0.172*	-	-	-
	[0.031, 0.292]	[0.032, 0.311]	-	-	-
2008	0.201*	-	-	0.177*	0.122
	[0.095, 0.307]	-	-	[0.043, 0.311]	[-0.051, 0.295]
2009	0.035	-	-	-	-0.03
	[-0.193, 0.262]	-	-	-	[-0.232, 0.172]
2010	0.045	0.192	0.046	-	-
	[-0.066, 0.156]	[-0.333, 0.716]	[-0.067, 0.16]	-	-
2011	0.031	0.04	0.014	-	-
	[-0.115, 0.177]	[-0.168, 0.248]	[-0.223, 0.251]	-	-
2012	-0.017	-	0.047	-	-
	[-0.148, 0.114]	-	[-0.153, 0.248]	-	-
N	2552	768	837	367	603
SE Clusters	by: Hexagon	by: Hexagon	by: Hexagon	by: Hexagon	by: Hexagon

Source: Own elaboration

Notes: confidence interval reported in brackets; * = significant at 5%.

Applied in the context of this study, either transit expansion did not improve access to workers and consumers in the regions served by lines 2, 4, and 12, or land values offset these benefits. The latter is a plausible possibility, in particular, for Line 4. Its core section was already dense prior to intervention, and, therefore, less land is available for development.

Additional pressure can come from zoning restrictions; however, it exceeds the scope of this study.[8]. By the same logic, the two-digit growth in number of business

[8] In the 2014 zoning code, restrictions were eased for development around rapid transit stations, while increasing limits on other parts of the city (São Paulo, 2014)

experienced in the surroundings of Imigrantes station (Line 2, $g = 2007$) and in the southbound expansion of Line 9 can reflect not only increased agglomerative potential in those regions led by new stations, but also cheaper land, in the case of Line 9.

When these results are compared with firm-level outcomes, the positive estimates support the hypothesis that lower average wages and employment per establishment stem from entrant firms, but the lack of significance precisely in the regions where those firm-level results were observed (Line 12 and most of Line 2) go in the opposite direction.

7 Final remarks

Our findings portray different scenarios depending on the outcome, region, and treatment cohort, but some patterns arise in space and time. Most of the positive and significant impacts take place in the surroundings of Line 9's 2008 south expansion; in contrast, the central, denser, and wealthier Line 4 region saw no relevant effects. This goes in the opposite direction of recent empirical evidence for Latin America since the impact on labor market outcomes was greater for either wealthier regions or better-paying jobs.

Recovered estimates also differ between treatment cohorts, which points toward the relevance of the staggered design. One might suppose that groups treated earlier benefit more from the intervention if their units have more incentive to self-select for treatment. Despite differences in ATT between treatment cohorts, they do not follow a pattern. One possible explanation is assuming that the most important network links were those already existent before 2006, when the study began.

Rapid transit in São Paulo is far from covering the entirety of the metropolitan region. The two biggest expansions in the 2006-2015 period took place precisely in central places already served by rapid transit, which can be a reason for the insignificant effects found for Line 4 and most of Line 2. In contrast, the Line 9 expansion served the extreme south of the capital, which was underserved by rapid transit since the suppression of rail service in the late 1980s.

Two counterintuitive results found were the decrease in average workers per firm for Line 12 and in average hourly wage for parts of Line 2. In both cases, the simplest explanation is that entrant firms employ fewer individuals and pay less, thus bringing down the average. This idea is backed up by the growth in the number of firms and employment at the more aggregated hexagon level, albeit not always significant. Another possibility is that improved accessibility allowed residents to exchange workplaces from local firms to other companies along the network (the "two-way road" effect), thus decreasing local activity, but this hypothesis requires caution since there is no data on worker residence and it may be too strong to assume that the number of workers living close to their jobs was big enough to generate such negative and significant estimates.

More than answers, our study provokes two questions to aid future policymaking. First, is transit expansion merely catching up with urban development or promoting it? Since 2014, the zoning code of the capital explicitly ties density to transit corridors, in a 600-meter buffer around mass transit stations and 200-meter along low/medium capacity corridors, where double the floor-area ratio (FAR) is allowed (São Paulo, 2013, 2014). The effect of this legislation is probably not captured by this study, since the analyzed period ends in 2017, when few real estate ventures developed under the new legislation were inaugurated; instead, it mostly reflects the 2004 zoning code (São Paulo, 2004), which had more restrictive FARs.

Second, which regions should be prioritized for the next interventions? If a society wishes to reduce social exclusion—as appears to be the Brazilian case, given the social contract implied in the country's Constitution—transport planning should meet minimum accessibility standards, especially for disadvantaged communities, to mitigate inequalities. In addition to accessibility, this study proposes that the project's transformation potential on labor market outcomes should be analyzed as well. The fact that Line 9 south expansion—which serves a lower to middle-class portion of São Paulo and with the 8th worst HDIs in the city[9]—had such a large impact on business and employment growth suggests that rapid transit expansion has the potential to transform the scenario of underprivileged communities.

This study can be extended in several ways. To begin with, a migration analysis tracking individual workers and firms in space could provide additional robustness to distinguish growth from reorganization and give more insight into the proposed explanations. An intersection with the 2022 Census microdata (gradually being released as of May 2024) and previous editions would be helpful in conducting a welfare analysis into the interventions' potential to reduce social inequalities, given income and household characteristics. Inasmuch as the results obtained here are limited to a partial equilibrium context, a quantitative spatial model integrating land use, labor markets, and accessibility modeling such as the Ahlfeldt model (Ahlfeldt et al., 2015) could enhance welfare analysis in a general equilibrium context and help to integrate land use and transport planning.

[9] Data compiled by Rede Social de Cidades (2024) using data from the 2000 and 2010 IBGE Censuses and SABESP, the water and sewage company of São Paulo

References

AHLFELDT, G. *et al.* Demystifying compact urban growth. 2018.

ALLEN, J. G. From Commuter Rail to Regional Rail: Operating Practices for the 21st Century. **Transportation Research Record: Journal of the Transportation Research Board**, v. 1623, n. 1, p. 127–134, Jan. 1998.

ALVARENGA, A. B. **spatialops: Spatial operations that comes in handy**. , 2024.

Anuário do trabalho na micro e pequena empresa. . [s.l.] Serviço Brasileiro de Apoio às Micro e Pequenas Empresas; Departamento Intersindical de Estatística e Estudos Socioeconômicos, 2013. Disponível em: <https://sebrae.com.br/Sebrae/Portal%20Sebrae/Anexos/Anuario%20do%20Trabalho%20Na%20Micro%20e%20Pequena%20Empresa_2013.pdf>. Acesso em: 27 apr. 2024.

ARCGIS. **Geocoding service—ArcGIS REST APIs | ArcGIS Developers**. Esri, , 2024. Disponível em: <https://developers.arcgis.com/rest/geocode/api-reference/overview-world-geocoding-service.htm>. Acesso em: 22 apr. 2024

ÅSLUND, O.; BLIND, I.; DAHLBERG, M. All aboard? Commuter train access and labor market outcomes. **Regional Science and Urban Economics**, v. 67, p. 90–107, 1 Nov. 2017.

BERTAUD, A.; RICHARDSON, H. Transit and density: Atlanta, the united states and western europe. 2004.

BOISJOLY, G.; MORENO-MONROY, A. I.; EL-GENEIDY, A. Informality and accessibility to jobs by public transit: Evidence from the São Paulo Metropolitan Region. **Journal of Transport Geography**, v. 64, p. 89–96, Oct. 2017.

BORUSYAK, K.; JARAVEL, X.; SPIESS, J. **Revisiting Event Study Designs: Robust and Efficient Estimation**. Rochester, NY, 19 Apr. 2022. Disponível em: <https://papers.ssrn.com/abstract=2826228>. Acesso em: 25 apr. 2024

BRASIL. **Substituição da captação dos dados da RAIS pelo eSocial e aspectos metodológicos da RAIS 2019**. Brasília: Ministério da Economia, 26 Oct. 2020.

CALLAWAY, B.; SANT'ANNA, P. H. C. **did: Difference in differences**. , 2021a. Disponível em: <https://bcallaway11.github.io/did/>

CALLAWAY, B.; SANT'ANNA, P. H. C. Difference-in-Differences with multiple time periods. **Journal of Econometrics**, v. 225, n. 2, p. 200–230, Dec. 2021b.

CAMBON, J. *et al.* tidygeocoder: An R package for geocoding. **Journal of Open Source Software**, v. 6, n. 65, p. 3544, 2021.

CAMPOS, M. C. DE. **Urban Mobility, Inequality and Welfare in Developing Countries: Evidence from 2016 Olympics in Rio de Janeiro**. Tese de Doutorado—Rio de Janeiro, Brazil: Pontifícia Universidade Católica do Rio de Janeiro, 25 Feb. 2019.

CANALES, K. L.; NILSSON, I.; DELMELLE, E. Do light rail transit investments increase employment opportunities? The case of Charlotte, North Carolina. **Regional Science Policy & Practice**, v. 11, n. 1, p. 189–202, Mar. 2019.

CHATMAN, D. G.; NOLAND, R. B. Do Public Transport Improvements Increase Agglomeration Economies? A Review of Literature and an Agenda for Research. **Transport Reviews**, v. 31, n. 6, p. 725–742, 1 Nov. 2011.

COMBES, P.-P.; GOBILLON, L. The Empirics of Agglomeration Economies. In: **Handbook of Regional and Urban Economics**. [s.l.] Elsevier, 2015. v. 5p. 247–348.

CPTM. **Relatório Integrado da Administração 2019**. São Paulo: Companhia Paulista de Trens Metropolitanos, 2020. Disponível em: <https://www.cptm.sp.gov.br/esg-consciente/Governanca/Paginas/Relatorio-da-Administracao-RIA.aspx>. Acesso em: 1 nov. 2023.

CREDIT, K. Accessibility and agglomeration: A theoretical framework for understanding the connection between transportation modes, agglomeration benefits, and types of businesses. **Geography Compass**, v. 13, n. 4, p. e12425, Apr. 2019.

DE CHAISEMARTIN, C.; D'HAULTFŒUILLE, X. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. **American Economic Review**, v. 110, n. 9, p. 2964–2996, Sep. 2020.

DURANTON, G. Labor Specialization, Transport Costs, and City Size. **Journal of Regional Science**, v. 38, n. 4, p. 553–573, Nov. 1998.

DURANTON, G.; PUGA, D. Micro-foundations of urban agglomeration economies. In: HENDERSON, J. V.; THISSE, J. F. (Eds.). **Handbook of regional and urban economics**. Handbook of regional and urban economics. [s.l.] Elsevier, 2004. v. 4p. 2063–2117.

EBERTS, R. W.; MCMILLEN, D. P. Agglomeration economies and urban public infrastructure. In: **Handbook of Regional and Urban Economics**. Applied Urban Economics. [s.l.] Elsevier, 1999. v. 3p. 1455–1495.

GOBILLON, L.; SELOD, H.; ZENOU, Y. The Mechanisms of Spatial Mismatch. **Urban Studies**, v. 44, n. 12, p. 2401–2427, 1 Nov. 2007.

GOODMAN-BACON, A. Difference-in-differences with variation in treatment timing. **Journal of Econometrics**, v. 225, n. 2, p. 254–277, Dec. 2021.

HERE. **Geocoding and Search API v7 API Reference**. HERE Global B. V., , 2024. Disponível em: <https://www.here.com/docs/bundle/geocoding-and-search-api-v7-api-reference/page/index.html>. Acesso em: 22 apr. 2024

IBGE. **I Centenário das ferrovias brasileiras**. Rio de Janeiro: SERGRAF do IBGE, 1954.

KRUGMAN, P. Increasing returns and economic geography. **Journal of Political Economy**, v. 99, n. 3, p. 483–99, 1991.

MARCUS, M.; SANT'ANNA, P. H. C. The Role of Parallel Trends in Event Study Settings: An Application to Environmental Economics. **Journal of the Association of Environmental and Resource Economists**, v. 8, n. 2, p. 235–275, 1 Mar. 2021.

O'BRIEN, L. **h3jsr: Access uber's H3 library**. , 2023. Disponível em: <https://CRAN.R-project.org/package=h3jsr>

OPENSHAW, S. The Modifiable Areal Unit Problem. In: **Concepts and techniques in modern geography**. Norwich: Geo Books, 1983.

PÁEZ, A.; SCOTT, D. M.; MORENCY, C. Measuring accessibility: positive and normative implementations of various accessibility indicators. **Journal of Transport Geography**, v. 25, p. 141–153, Nov. 2012.

PEREIRA, R. H. M. *et al.* Distributional effects of transport policies on inequalities in access to opportunities in Rio de Janeiro. **Journal of Transport and Land Use**, v. 12, n. 1, 11 Oct. 2019.

PEREIRA, R. H. M. *et al.* r5r: Rapid realistic routing on multimodal transport networks with R5 in R. **Findings**, 2021.

REDDING, S. J.; TURNER, M. A. Transportation Costs and the Spatial Organization of Economic Activity. In: **Handbook of Regional and Urban Economics**. [s.l.] Elsevier, 2015. v. 5p. 1339–1398.

REDE SOCIAL DE CIDADES. **Rede de esgoto (domicílios sem ligação com a rede de esgoto) de São Paulo, SP**. Disponível em: <https://www.redesocialdecidades.org.br/br/SP/sao-paulo/regiao/capela-do-socorro/rede-de-esgoto-domicilios-sem-ligacao-com-a-rede-de-esgoto>. Acesso em: 17 may. 2024.

SACTRA. **Transport and the economy**. London: Standing Advisory Committee on Trunk Road Assessment, 1999. Disponível em: <https://www.ffue.org/wp-content/uploads/2016/08/SACTRA_Full-report.pdf>.

SANTOS, R. T. DOS *et al.* Demanda por investimentos em mobilidade urbana no Brasil. **BNDES Setorial**, n. 41, p. 179–134, Mar. 2015.

SANTOS, S. M. M. DOS *et al.* São Paulo: dinâmica urbano-regional do estado. In: **Dinâmica urbano-regional: rede urbana e suas interfaces**. 1. ed. Brasília: Ipea, 2011. p. 486.

SÃO PAULO. LEI Nº 13.885 DE 25 DE AGOSTO DE 2004. . 25 Aug. 2004.

SÃO PAULO. **Coeficiente de Aproveitamento**. Government webpage. Disponível em: <https://gestaourbana.prefeitura.sp.gov.br/novo-pde-coeficiente-de-aproveitamento/>. Acesso em: 17 may. 2024.

SÃO PAULO. 16.050. LEI Nº 16.050 DE 31 DE JULHO DE 2014. . 31 Jul. 2014.

SÃO PAULO. **Metrô - Estação**. Geosampa, , 30 Nov. 2018. Disponível em: <https://geosampa.prefeitura.sp.gov.br/PaginasPublicas/_SBC.aspx>. Acesso em: 27 apr. 2023

SÃO PAULO. **Trem Metropolitano - Estação**. Geosampa, , 7 Feb. 2019. Disponível em: <https://metadados.geosampa.prefeitura.sp.gov.br/geonetwork/srv/por/catalog.search;jsessionid=6A16949E35221CBF3E8B8D22F13CE067#/metadata/6ee9af1d-d343-4fd2-b2ab-18d4a6556aba>. Acesso em: 27 apr. 2023

SÃO PAULO. **Metrô Projeto - Estação**. Geosampa, , 2022. Disponível em: <https://geosampa.prefeitura.sp.gov.br/PaginasPublicas/_SBC.aspx>. Acesso em: 27 apr. 2023

SÃO PAULO. **Trem Metropolitano Projeto - Estação**. Geosampa, , 11 May 2023. Disponível em: <https://metadados.geosampa.prefeitura.sp.gov.br/geonetwork/srv/por/catalog.search;jsessionid=6A16949E35221CBF3E8B8D22F13CE067#/metadata/65df3022-5086-45f8-80b3-fcb92f861eaf>. Acesso em: 27 apr. 2023

SCHOLL, L. *et al.* **A Rapid Road to Employment? The Impacts of a Bus Rapid Transit System in Lima**: IDB Working Paper Series. [s.l.] Inter-American Development Bank, Dec. 2018.

SEVEREN, C. Commuting, Labor, and Housing Market Effects of Mass Transportation: Welfare and Identification. **The Review of Economics and Statistics**, v. 105, n. 5, p. 1073–1091, 13 Sep. 2023.

SUN, L.; ABRAHAM, S. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. **Journal of Econometrics**, v. 225, n. 2, p. 175–199, Dec. 2021.

TSIVANIDIS, N. The Aggregate and Distributional Effects of Urban Transit Infrastructure: Evidence from Bogotá's TransMilenio. Feb. 2019.

TYNDALL, J. Waiting for the R train: Public transportation and employment. **Urban Studies**, v. 54, n. 2, p. 520–537, Feb. 2017.

UNITED NATIONS, D. OF E. AND S. A., Population Division. **World urbanization prospects 2018: Highlights**. ST/ESA/SER.A/421 ed. New York: United Nations, 2019.

VICKERMAN, R. Transit investment and economic development. **Research in Transportation Economics**, v. 23, n. 1, p. 107–115, Jan. 2008.

VICKERMAN, R. Beyond cost-benefit analysis: the search for a comprehensive evaluation of transport investment. **Research in Transportation Economics**, v. 63, p. 5–12, Aug. 2017.

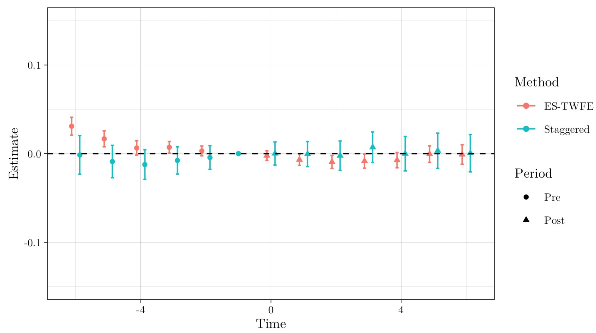
VILLAÇA, F. **Espaço intra-urbano no Brasil**. [s.l.] Studio Nobel, 2016.

WHITE, M. J. Urban areas with decentralized employment: Theory and empirical work. In: **Handbook of Regional and Urban Economics**. Applied Urban Economics. [s.l.] Elsevier, 1999. v. 3p. 1375–1412.

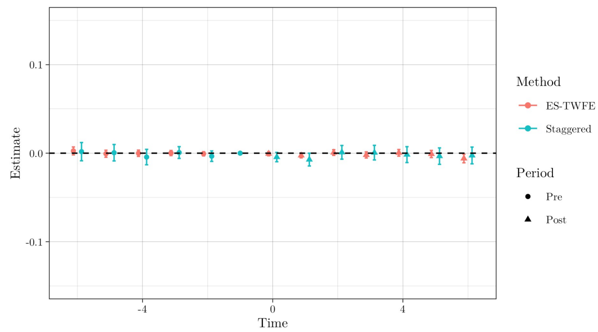
APPENDIX A – Additional estimates

Figure 8: Average effect on outcomes by length of exposure for all transit lines

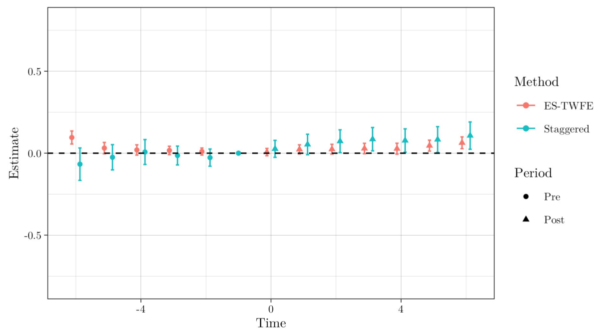
Panel (a): Number of jobs, firm level



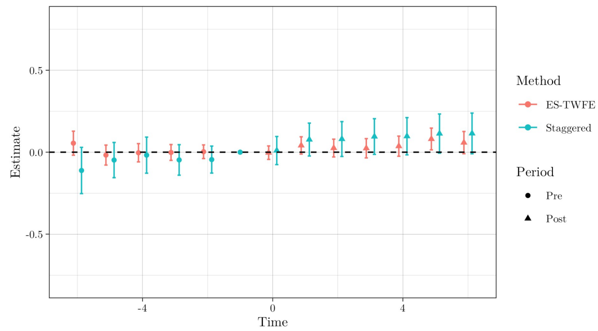
Panel (b): Avg. hourly wage, firm level



Panel (c): Number of firms, hexagon level



Panel (d): Number of jobs, hexagon level

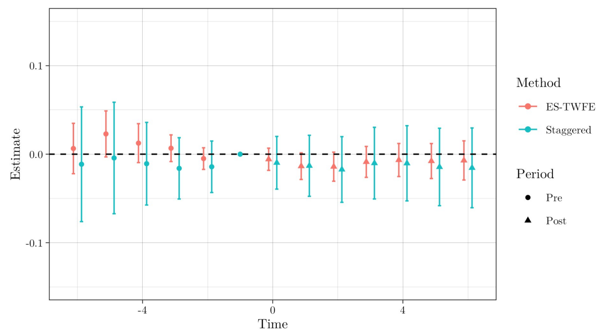


Source: Own elaboration.

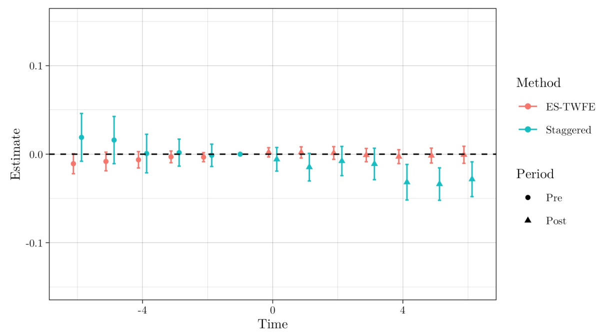
Notes: Time and estimates are relative to opening year ($t = 0$); events were balanced to keep only units submitted to at least six years of treatment.

Figure 9: Average effect on outcomes by length of exposure for Line 2

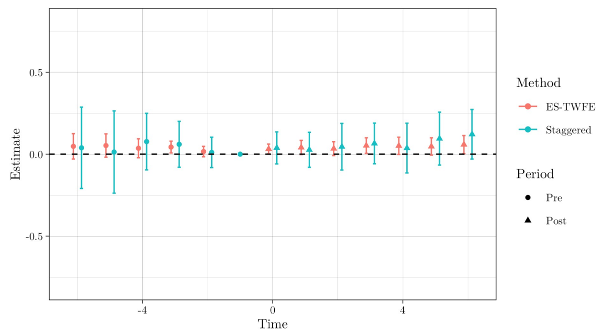
Panel (a): Number of jobs, firm level



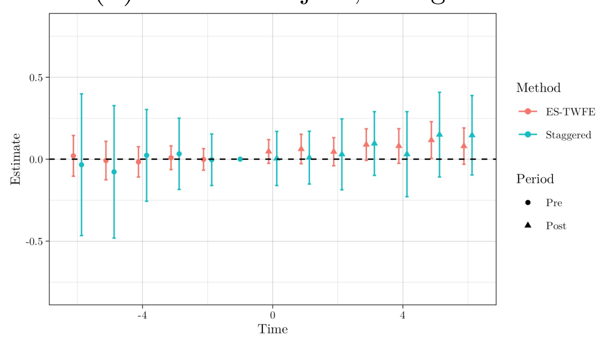
Panel (b): Avg. hourly wage, firm level



Panel (c): Number of firms, hexagon level



Panel (d): Number of jobs, hexagon level

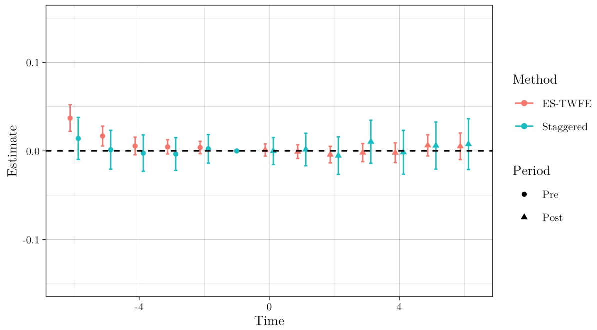


Source: Own elaboration.

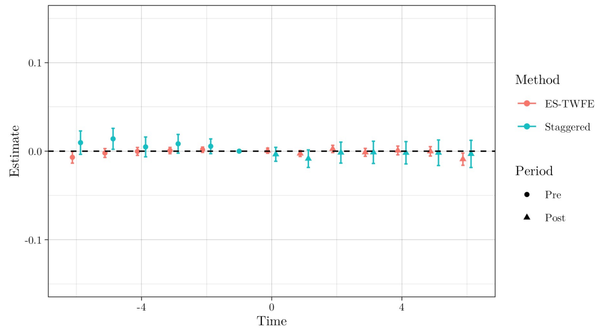
Notes: Time and estimates are relative to opening year ($t = 0$).

Figure 10: Average effect on outcomes by length of exposure for Line 4

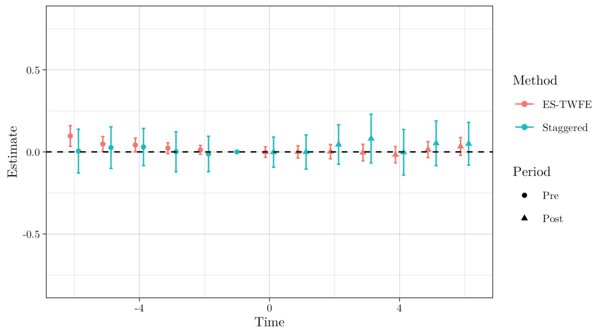
Panel (a): Number of jobs, firm level



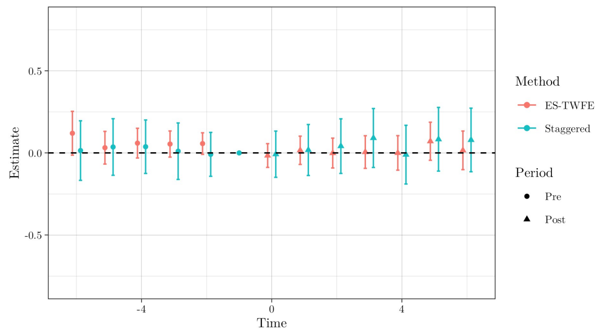
Panel (b): Avg. hourly wage, firm level



Panel (c): Number of firms, hexagon level



Panel (d): Number of jobs, hexagon level

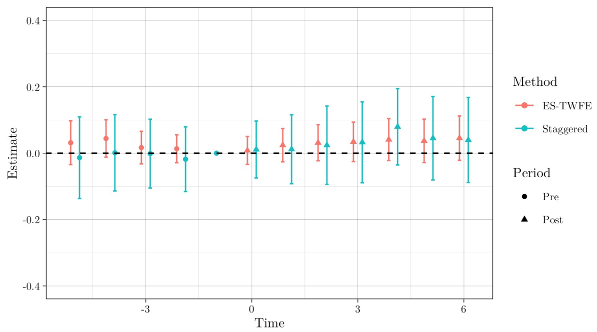


Source: Own elaboration.

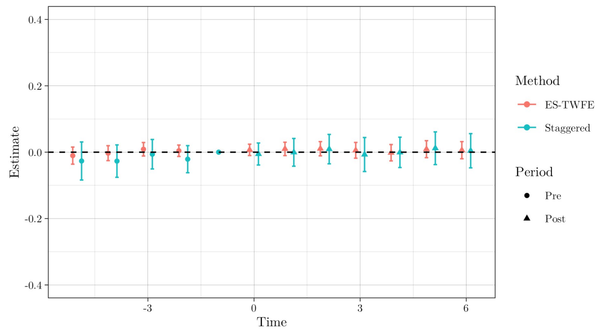
Notes: Time and estimates are relative to opening year ($t = 0$); events were balanced to keep only units submitted to at least six years of treatment.

Figure 11: Average effect on outcomes by length of exposure for Line 9

Panel (a): Number of jobs, firm level

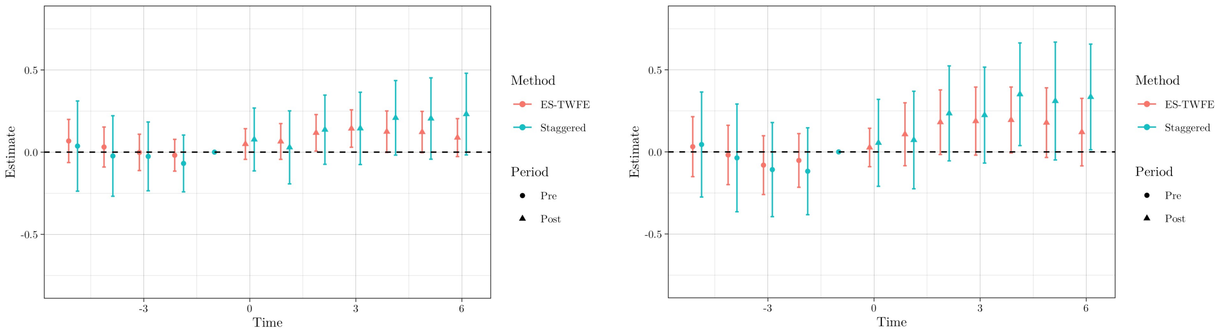


Panel (b): Avg. hourly wage, firm level



Panel (c): Number of firms, hexagon level

Panel (d): Number of jobs, hexagon level

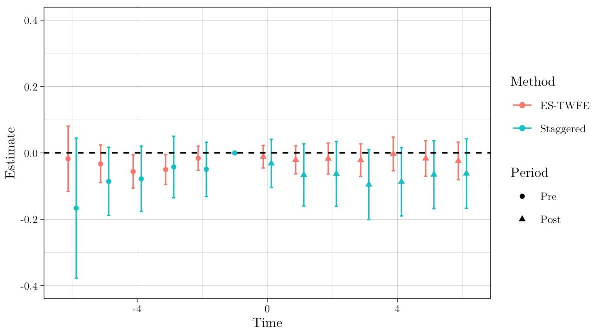


Source: Own elaboration.

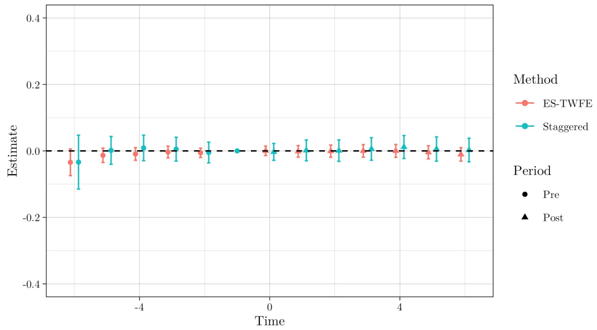
Notes: Time and estimates are relative to opening year ($t = 0$); events were balanced to keep only units submitted to at least six years of treatment.

Figure 12: Average effect on outcomes by length of exposure for Line 12

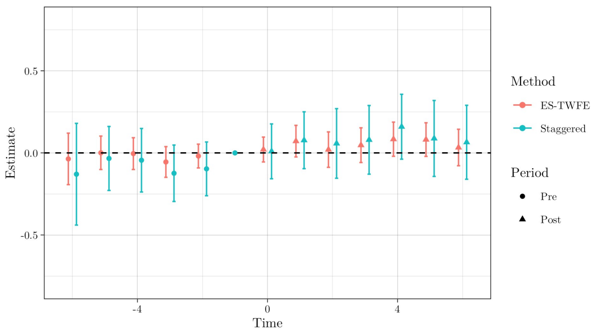
Panel (a): Number of jobs, firm level



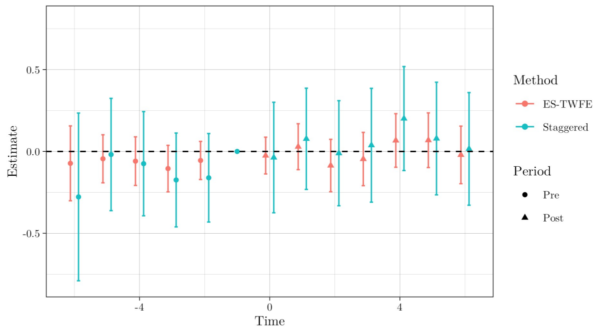
Panel (b): Avg. hourly wage, firm level



Panel (c): Number of firms, hexagon level



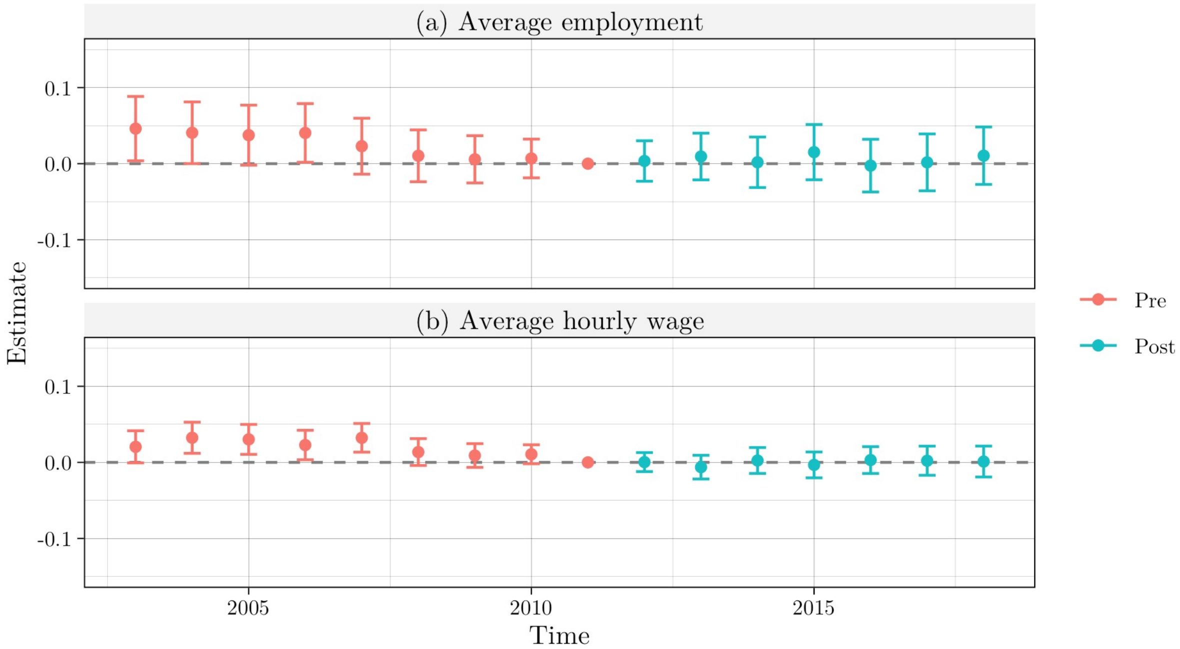
Panel (d): Number of jobs, hexagon level



Source: Own elaboration.

Notes: Time and estimates are relative to opening year ($t = 0$); events were balanced to keep only units submitted to at least six years of treatment.

Figure 13: Event study plots for units around Line 4 treated in 2012, firm-level outcomes



Source: Own elaboration.